U.S. GEOLOGICAL SURVEY CIRCULAR 961

Conference on Continental Margin Mass Wasting and Pleistocene Sea-Level Changes, August 13–15, 1980

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Edited by David W. Folger *and*
John C. Hathaway

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Edited by David W. Folger *and* John C. Hathaway

ABSTRACT

A conference on Continental Margin Mass Wasting and Pleistocene Sea-Level Changes was held in Woods Hole, Mass., August 13–15, 1980. Forty-seven participants, representing many government, academic, and industrial organizations, discussed the current state of knowledge of the features of marine mass wasting and of the interrelations of factors influencing them. These factors include sediment source, composition, textures, sedimentation rates, climatic and sea-level changes, gas and gas hydrate (clathrate) contents of sediments, geotechnical characteristics, oceanographic and morphological factors, ground-water processes, and seismic events. The part played by these factors in the processes and features of mass movement and the engineering considerations imposed by the emplacement of manmade structures on the sea floor were considered vital to the evaluation of hazards involved in offshore exploration and development. The conference concluded with a call for bold programs to establish the probability of occurrence and the quantitative importance of these factors and to devise more reliable means of measurement, particularly in place, of the characteristics of the sediment and features involved.

INTRODUCTION

A conference entitled Continental Margin Mass Wasting and Pleistocene Sea-Level Changes held at Woods Hole, Mass., August 13–15, 1980, examined the state of knowledge in the United States of the subject of mass-wasting phenomena on the sea floor and the possible relation of these phenomena to sea-level changes. The 47 participants, representing a variety of disciplines, presented the results of either current or past studies or contributed to discussions of how these or future studies might interrelate to help answer questions on the causes and effects of marine mass movements.

The keynote considerations were presented in introductory remarks by H. William Menard, Director of the U.S. Geological Survey (USGS). He pointed out that the impetus for interest in the subject was a lack of sufficient, readily available oil and gas resources in the United States and that the best chances for finding new large fields lie in areas as yet unexplored, such as the continental margins. Safe development of these areas requires knowledge of possible hazards from mass movements, and although advances have been made in such knowledge, much remains to be learned. The aim of the conference was to increase understanding of the geologic evolution of the offshore regions and thereby to determine if development can be undertaken safely—if, indeed, oil and gas are found there.

The 3-day conference was divided into eight parts: (1) Mass-Wasting Features and Processes, (2) Sediment Sources and Current Regimes, (3) Climatic Changes, (4) Sea-Level Changes, (5) Gas Content and Clathrates, (6) Geotechnical Factors, (7) Ground-Water Processes, and (8) Engineering Considerations and Study Techniques.

We asked the participants to edit the transcripts of their presentations and to add a few illustrations and a limited bibliography for publication. Because of the long delay in assembling the proceedings for publication, about one-third of the participants complied with our request. A few expanded their presentations into more formal papers, and one well-organized individual (Robert G. Bea) came to the meeting with a fully prepared manuscript. Another one-third of the participants preferred to include excerpts of work published after the meeting and representative of their presentations. We have, therefore, reprinted a number of abstracts, introductions, or, in one case, a short paper.[1] We prepared short summaries of the remaining presentations because the transcripts were either inappropriate for publication or would have taken more time to reconstruct and illustrate than participants could afford. We included edited transcripts of the discussions that ensued after most presentations; these were particularly valuable because the discussions often emphasized important points and problems. Although this mixture of formats may be disconcerting to the reader, the compilation still conveys the content and flavor of the meeting and, perhaps more importantly, highlights controversial issues that need additional attention and research.

ACKNOWLEDGMENTS

We thank Hartley Hoskins, Don Sousa, Russell Brown, Del Smith, John Deacon, and Marie Sorbera (Woods Hole Oceanographic Institution), and Elizabeth Winget, Sally Needell, Maggie Goud, Mary Lou Pina, and Joan Williams (U.S. Geological Survey), who helped coordinate and carry out the conference.

We are indebted to a number of our colleagues, including Jim Booth and Hal Olsen, who assisted us in editing the tapes of the conference. We also offer special thanks to Peggy Hempenius, who assembled the tapes and transcribed them single-handedly. She and Susan Peterson typed the final draft. Patricia Forrestel and her staff drafted many of the illustrations used in the volume. Elazar Uchupi and Jim Booth reviewed the manuscript.

[1]These have been taken from a wide variety of published sources; most, therefore, have not been edited in accordance with USGS editorial standards. In a few cases, if the author did not include a reference list, the reader will have to refer to the original article for information on cited references.

Welcome and Introduction of Participants

David W. Folger, Chief,
Branch of Atlantic-Gulf of Mexico Geology,
U.S. Geological Survey

I want to welcome you all to this conference on behalf of Jack Hathaway and myself. This is Bill Menard's second conference here at Woods Hole; it is sponsored jointly by the U.S. Geological Survey and the Woods Hole Oceanographic Institution. The first conference, last August, concerned mainly the structure of the continental margins, estimates of their resources, and problems associated with making those estimates. We had more of an in-house group then—mostly Geological Survey plus a few participants from industry and academia. This year we've expanded greatly; the majority of you today are from private institutions and industry as well. It's a much more diverse conference than the first one, and we hope that it will be as stimulating; that is, stimulating in a different way. Last year the President got into the act during the meeting, as many of you remember. His request for the Director to present some of the material we were discussing at an imminent major national briefing on energy spurred us all into a flurry of unanticipated activity.

There's a big group from Washington here—I would be very happy if we don't see any organization charts! We also have a rather large group from Texas and the Deep South, and I would be very happy if we didn't hear any Aggie jokes either!

Now, I would like to have everyone introduce himself or herself, and then Bill [Menard] will start the ship underway. Terry [Edgar], would you open, please?

Terry Edgar, Chief, Office of Marine Geology, Reston, Va.

H. William Menard, Director, U.S. Geological Survey, marine geologist, Reston, Va.

Dallas Peck, Chief Geologist, USGS, Reston, Va.

Jack Hathaway, USGS, Woods Hole, Mass.

Jim Robb, USGS, Woods Hole, Mass.

Paul Teleki, USGS, Office of Marine Geology, Reston, Va.

Harley Knebel, USGS, Woods Hole, Mass.

Dave Aubrey, Geology and Geophysics Department, Woods Hole Oceanographic Institution, Woods Hole, Mass.

Dave Twichell, USGS, Woods Hole, Mass.
Robert Embley, National Ocean Survey, NOAA, Rockville, Md.
Art Maxwell, Woods Hole Oceanographic Institution, Woods Hole, Mass.
Bill Doyle, Shell Development Co., Houston, Tex.
Dave Prior, Louisiana State University, Baton Rouge, La.
Art Bloom, Cornell University, Ithaca, N.Y.
Armand Silva, University of Rhode Island, R.I.
Wayne Dunlap, Texas A & M University, College Station, Tex.
Jim Coleman, Louisiana State University, Baton Rouge, La.
Monty Hampton, USGS, Menlo Park, Calif.
Dwight Sangrey, Carnegie-Mellon Institute, Pittsburg, Pa.
Dick Giangerelli, USGS, Reston, Va.
George Smith, USGS, Menlo Park, Calif.
Tom Cronin, USGS, Reston, Va.
Bruce Hanshaw, USGS, Reston, Va.
Ray Wallace, USGS, National Space Technology Laboratories, Bay St. Louis, Miss.
Bob Bea, Woodward-Clyde Consultants, Houston, Tex.
Homa Lee, Pacific-Arctic Branch, USGS, Menlo Park, Calif.
Arnold Bouma, USGS, Corpus Christi, Tex.
Hal Olsen, Engineering Geology Branch, USGS, Denver, Colo.
Bill Dillon, USGS, Woods Hole, Mass.
Bonnie McGregor, USGS, Miami, Fla.
George Carpenter, USGS, Washington, D.C.
Bob Miller, USGS, Reston, Va.
Jim Booth, USGS, Woods Hole, Mass.
Bob McNeill, Sandia Laboratories, Albuquerque, N. Mex.
Dot Marks, USGS, Woods Hole, Mass.
C.C. Woo, USGS, Woods Hole, Mass.
Yang Zho Seng, Institute of Marine Geology, Qingdao, People's Republic of China
John Lees, USGS, Washington, D.C.
Bob Rioux, USGS, Reston, Va.
Eric Sundquist, Water Resources Division, USGS, Reston, Va.
Elazar Uchupi, Woods Hole Oceanographic Institution, Woods Hole, Mass.
Nick Shackleton, Cambridge University, Cambridge, United Kingdom
Warren Prell, Brown University, Providence, R.I.
Joe Liddicoat, Lamont-Doherty Geological Observatory, Palisades, N.Y.
John Schlee, USGS, Woods Hole, Mass.
Richard Clingan, USGS, Hyannis, Mass.
Brad Butman, USGS, Woods Hole, Mass.
Gordon Burton, USGS, Reston, Va.
Bud Danenberger, USGS, Hyannis, Mass.
K.O. Emery, Woods Hole Oceanographic Institution, Woods Hole, Mass.
Lou Garrison, USGS, Corpus Christi, Tex.
Dave Ross, Woods Hole Oceanographic Institution, Woods Hole, Mass.
Charlie Hollister, Woods Hole Oceanographic Institution, Woods Hole, Mass.

Introduction to the Conference

H. William Menard, Director,
U.S. Geological Survey

I think it's obvious to everyone that the principal problem in this country is that we don't have enough in the way of readily available energy resources, and this means oil and gas, mainly. Last year, we had a rather interesting conference here trying to identify potential sources of deep oil and gas. Those of you who read the *New York Times* must have noticed in the last few days that drilling is up to a new high in this country—almost the highest it's ever been. The *Times* didn't mention it, but the finding rate for new fields is the highest it's ever been; that is, the chance that you will hit a new field "wildcat" is the highest that it's ever been. Of course, you could say, "What's the problem?" The problem is that the fields you hit with a new field wildcat are hardly worth hitting—except that the price of oil has gone up so much from what it used to be. Multiply the price of oil by 10, and a 100,000-barrel field becomes as valuable as a million-barrel field used to be.

So we're finding an enormous number of little fields. But that's not the way you solve the energy problem. The energy problem is solved by finding big fields, the way it always has been. And the big fields are not in the 48 contiguous States on land. Nothing that they would even call a minor field in the Middle East is going to be found in these 48 States on land for oil. Gas? Who knows. You have to go to an area that hasn't been thoroughly explored, and that means, in the United States, either Alaska or the parts of the continental margin that haven't been opened for development and out into the deep sea.

Last year we investigated the interesting reef trend off the east coast, learned a great deal about it, and it was a lot of fun. One of the reasons it was a lot of fun was because we had a range of people from quite different units of the Geological Survey. Well, it turned out last year that there was a good deal to be learned from having these various, sometimes autonomous, units talk to each other, and we thought, as this seminar developed, that we would highlight the things to be done by having a wider range of people present.

One of the things that came up last year was in connection with the lease sale in Baltimore Canyon Trough. It developed that, because of the uncertainty of whether or not there were large submarine landslides or scars that might be reactivated off the area, we were unable to tell the Bureau of Land Management that it was safe to go ahead and develop. And so a number of the most promising tracts, as far as oil companies were concerned, were simply eliminated because we didn't know whether it was safe to develop them. Fortunately, there have been great advances [in our knowledge] about what is safe to develop and what isn't safe to develop. But we still have an enormous amount to learn—that's what we're here for. We have the hope that, by inviting people with a wide variety of backgrounds, who look at the environment in different ways and at the oil development problem in different ways, and by having people who are concerned with development talk to people who understand the effects of such phenomena as sea-level fluctuations, erosion, ground-water motion, sediment instabilities, and sediment depositon, we may be able to interpret the geologic record adequately to develop the continental margin safely.

So, from the way this meeting has been organized, we hope that it will lead from trying to understand a different environment to trying to find out whether it's safe to operate in it and then to go ahead and work in it. There's a great difference between being sure enough to talk to a group of scientists about whether something is so and being sure enough to tell the Bureau of Land Management that they can lease an area. Scientists behave differently from most people. You don't lose your reputation if you get up and say, "The records look like so-and-so-and-so," and then 6 months later you say, "Well, I got some new records, and it wasn't that way at all." Scientists will say, "That's fine; you got new information," but if, meanwhile, the Bureau of Land Management has leased the whole area for $2 billion, you've got a mess on your hands!

What we're trying to do is avoid that mess. We'll also have a lot of fun, and I hope we don't get involved with having to go directly out into the media, as we did last time. I hope you all enjoy it.

MASS-WASTING FEATURES AND PROCESSES

Gulf of Mexico Margin

[This abstract and introduction summarize the contents of two presentations. The first, given by James M. Coleman, was entitled "Distribution of Mass-wasting Features in the Mississippi Delta Area," and the second, given by David B. Prior, was entitled "Mechanisms of Mass Wasting in the Mississippi Delta Area." The discussions that followed the presentations are included.]

Active Slides and Flows in Underconsolidated Marine Sediments on the Slopes of the Mississippi Delta

David B. Prior and James M. Coleman

ABSTRACT[1]

On the continental shelves off large deltas, rapid progradation and deposition result in highly underconsolidated marine sediments. These deposits, which are often also rich in interstitial methane gas, can be subject to widespread and active mass movement downslope. For example, the submarine slopes of the Mississippi River Delta are affected by a variety of sediment instability processes. Geologic and geophysical surveys using sidescan sonar, subbottom profilers, and precision depth recorders have been completed for the entire subaqueous delta. Survey lines were spaced at 240-m intervals, and water depths ranged from 5 m to 20 m. Bottom morphology, including sediment deformations indicative of instability, has been mapped at a scale of 1:12,000, and large-area, scale-corrected sonar mosaics have been constructed. The features identified include collapse depressions, bottleneck slides, shallow rotational slides, mudflow gullies, overlapping mudflow lobes, and a wide variety of faults. The slides and mudflows are extremely active, and movement rates of several hundred meters per year have been recorded. Damage to offshore oil and gas pipelines and platforms has occurred. Also, the concept of slow, continuous deltaic progradation must be modified to include the effects of these processes. For example, on the shelf, normal settling of suspended clays averages only a few millimeters per year, whereas at the front of the delta slope more than 30 m of sediment has been deposited by mudflows and slides since 1875.

These deltaic processes are the result of complex temporal and spatial combinations of different factors, including the generation of excess pore pressures by rapid sedimentation, methane gas within the sediments, wave-induced stresses, and localized slope steepening. These conditions are not unique to the Mississippi Delta, and indeed similar processes, which affect geologic deposition models and provide design constraints for offshore engineering, appear to be common in many deltaic environments.

[1] 1982, *in* Saxov, S., and Nieuwenhuis, J.K., eds., NATO Conference Series, Series IV: Marine Sciences, v. 6, p. 21–49.

INTRODUCTION

Recent detailed investigations on continental shelves and shelf slopes have revealed that subaqueous gravity-induced mass movements of sediment, whether active or relict, are extremely common phenomena and should be considered integral components of normal shelf and shelf slope transport processes. In some shelf environments, especially those seaward or downdrift of large river discharges, sediment transport and deposition by subaqueous mass movement account for a large proportion of the shelf deposits. Continental shelves such as those bordering the north-central Gulf of Mexico, northern coasts of South America (Magdalena River, Esmeraldas River, Orinoco River, Surinam, the Guianas, and the Amazon River), Alaskan shelves (Yukon, McKenzie, and Copper Rivers), and other deltaic areas such as the Niger, Congo, Orange, Ganges-Brahmaputra, Indus, Nile, Yangtse, Red, and Hwang Ho Rivers all display sediment and sea-floor morphology characteristic of downslope mass movement and slope failure. The following generalizations can be made about instabilities in these regions: (1) Instability can occur on very low angle slopes (generally less than 1°); (2) Large quantities of sediment are transported from shallow water depths into deeper water offshore along well-defined landslide channels; (3) Individual failures, although variable in size, generally possess three morphological components: a source area with subsidence and rotational slumping, a central transport zone, often defined by a channel or chute, and a composite depositional area composed of overlapping lobes of remolded debris; (4) Although movement areas are not generally known, it appears that displacements of sediment can accompany the initiation of new features on a previously stable part of the shelf or reactivation of previously existing unstable areas.

The Mississippi River Delta and adjacent shelf region have been investigated for many decades, but recently there have been many improvements in the systematic utilization of various techniques for underwater exploration and sea-floor mapping. The application of sidescan-sonar and high-resolution seismic techniques has led to substantial progress in the documentation and mapping of the subaqueous region of the delta. These techniques, aided by the history of problems encountered in foundation design for offshore oil and gas structures and pipelines, have permitted the identification of a variety of active slope and sediment movements.

Discussion following Coleman's talk:

Menard: You made the general comment that, with 1-mi spacing, you were able to do geologic interpretation. What did you mean by that?

Coleman: With mile-wide spaced lines, I can begin to correlate features. I can't come in and find small-scale features. When you really get down to detailed mapping like this, you need line spacing in keeping with the feature you're mapping. For a channel that is a major geological feature, I can use mile spacing, but if you say you want to know about the stability of a specific site, then you must have closer line spacing.

Menard: Twenty years ago, 1-mi spacing was incredible detail. Do you think that, with what you've learned here, you could go into another area? What sort of spacing would you need in another area, now that you've done the details in one like this? A comparable area?

Coleman: I think I can start out with 1-mi spacing, if I'm not asked for a point on the ground. For general assessment, I think in some other parts of the Gulf of Mexico, you need 2 mi or even 3 mi. Two mi, I'm a little more confident; one mi, very confident. There are some other people I'm sure that might disagree with that in the room, but that's the feeling I get.

Menard: What's the relief on the channel?

Coleman: The relief between the smooth sea floor and the adjacent lobes varies from 2 to 3 m to about 30 m maximum. The widths vary from 50 or so m to the maximum of about 1200 m. Heights of these mudflows are on the order of 20–30 m.

Edgar: Jim, you've mentioned that in the constructional phase this whole system moves seaward. You've stated first then that you should be having a number of these mass-wasting features buried. Are you seeing these on the high resolution [seismic data]?

Coleman: We don't see them on the high resolution because of the biochemical production of gas. High resolution does not work in the shallower areas. But if I drilled and used things like carbon dating for time, or faunal data, deeper water versus displaced fauna—[evidence] from a boring—we might see three and four of these units stacked up and then date them back to the early 1800's, for example. So you do see this progradational situation that is a phenomenon that has been going on at least since the 1800's.

Discussion following Prior's talk:

Bloom: David, have you seen any blocks that started to move, then acquired a sediment load on the surface, and continued moving? That is, accretion simultaneous with movement?

Prior: No, I don't think we have.

Ross: Your data show much upslope, downslope, alongslope types of movement. Do you think it's appropriate to use the infinite slope hypothesis to work out something?

Prior: A quick and obvious answer is no. We face some severe differences in analysis of these subaqueous slides, and I think when you get to the geotechnical discussions, there are problems of what are the appropriate models to use. That model—the infinite slope model—is a simplistic approach: Can I use it, and what do I need to do so? The model shows you the need for some high pore-water pressures. If you watch the kind of high pore-water pressures you measure in the field, then I think you'll begin to understand. And we did just that, specifically because everybody said these can't be mass-movement features because of the small slope angle.

Sangrey: Just to make a comment, David, I disagree with your "no." The answer should be "yes." The most sophisticated mechanism compared with the infinite slope is 10-percent difference at most, and we don't know anything out there with 10-percent accuracy.

Prior: Yes, I think that's right. Let's back off; even with so much slush and so many problems in defining the parameters you want to put into these equations, crude though as they are, they have considerable utility.

Bea: There's also the sequel to that. It's not difficult to tell if a material with 50-lb/ft^2 shear strength is going to fail or not, and that's the break-back shear strength, so an infinite slope may work. But the problem is more complex.

Menard: Are you sure that all the slumps stop? If they don't, what happens? We've heard denigrations of turbidity currents, but I can't help but wonder what form all that stuff takes out of the Gulf of Mexico, on the flat abyssal plains, and so on.

Prior: That's a fascinating question. Now, if I can briefly just say something in answer to that. Remember our delta distributaries are moving around here a great deal. In the latter part of the 19th century, the major pass was called Grand Pass. South Pass was not particularly active, so our locus of sedimentation was around Grand Pass. What we finally deciphered is that there are some buried features that are very similar to the mudslide groups that we described. But out here in 1874, there was a large lobe with debris, apparently fed by a system emanating from old Grand Pass. By 1940, that mudslide lobe over 100 ft [30 m] thick had disappeared. As crude as the bathymetric comparisons are, the data are too consistent. There are large areas of major sediment loss. We aren't sure where it went, but the obvious direction is off the shelf edge—under what conditions and due to what mechanisms, we really don't know. But I think it's an important question because there is a link clearly to deeper water.

Menard: So you may be looking at a very small scale phenomenon that is superimposed on a giant one, for which you don't have many historical records.

Prior: That's right.

Teleki: In some of the chutes, the movements of these blocks are apparently episodic, or at least most people think in those terms. But what time scale are we talking about? 10 years? 50 years? Is there some sort of a number that you can put on as the rate of movement even though it occurs as individual events?

Prior: No, I think we've tried to get at the time-scale problem. I think you do have some ideal rates of sedimentation which are indicative of activity and movement, and I mentioned that. I don't think we can say how much material moves down the chute, or the actual transport budget downshoot over a particular period of time. What we do know, however, on the longer time scale than these repetitive surveys, is that the occurrence of new ones has a remarkable correlation with major floods in the river. In 1973, for example, we identified from repeated bathymetric surveys that new ones occurred then. Many also occurred in 1921, I believe. There are periods when we don't get any major development and others when we do get major periods of gully growth that seem to correlate with flood activity.

Teleki: Let me put it another way. Is 300 m in a day an excessive figure for a mass of sediment to move in a gully?

Prior: Three hundred depth or distance?

Teleki: Distance.

Prior: I don't know that, because we haven't got data on a daily basis. But I think the indications are, and the platform that was lost shows, that you can move several hundreds of meters in a relatively short period of time. But we don't tie it down to say that the maximum rate of movement of this stuff is such and such. We've got a number of time scales. And the indications are that you're going to have a spread of movement rates; that is, slow progradation in some lobes but occasionally, when a particular load is put on, a major thrust forward.

Coleman: Now, Paul [Teleki], if I can add one thing. Every once in a while we get lucky, from a scientific standpoint. A pipeline breaks out there and—to answer your question—the day before, the pipeline was there and a break occurs. By the time you mobilize a boat and go out, it's 2,000 ft downslope. Now, that's over a few hours. Those are rare, fortunately, but every once in a while you do get an indication that this can happen very fast.

Menard: Do you mean the data are rare or the phenomenon is rare?

Coleman: Both. First of all, when you really look at this region, there are a lot of lease blocks, but there are not a lot of wells or platforms. If you look at mudslide resistance, in older designs there are only a few (Bob Bea, correct me if I'm wrong) such as right on the dome, with good foundations. There are three that were there that are no longer there, that failed. And then there's one out in Texaco 54 that is called "old shakey." This is a mudslide-resistant platform sitting right in the middle of the mudslide. So right now, at our state of the art, we don't have a long record of pipeline breaks to get that kind of history.

Bea: A couple of things that may help Paul and his answer: Analytical modeling and the resurveying that we've done in some of these areas indicate that given an episode of movements [during, for example, a hurricane] we can look for 3,000 ft [915 m] of seaward migration [that] could be as great as 30,000 ft [9150 m]. Secondly, in doing hindcasting, [we] set up analytical models and calibrate them with geologic, geotechnical, and oceanographic data. To go back to the 1900's, it indicates that in the intermediate water depths of, say, 100 m, we could look for massive movements once every 5 years. So it is a fairly high repetition event that a platform or a pipeline could look forward to withstanding during its lifetime.

Prior: That kind of thing fits with the evidence of our resurveys, very much so.

Sangrey: A comment that you might find helpful only because it's an opinion: One of the values of the kind of schematic illustrations that you put up is that they give us all a better feel for what is happening; each of us does his own thing with it. My gut feeling is that if you were to take a very detailed look, with borings and so forth, at the head of your retrogressive slide, you would find that there is a much smaller amount of what we might call remolded material and a larger block size than there is further down[slope]. And I base that opinion partly on this kind of work, but also partly on the work that I've done in Norway, Sweden, and Canada on the quick-clay slides where we have had the luxury of being able to set up a rig and drill through them. What we find is that the head of the classic bottleneck slide is about 99 percent block and 1 percent remolded materials—essentially big blocks slipping around the very weak material. And it's only as you get down and dissipate the energy [that is] further breaking up those blocks, remolding them as they go through the chute, [that] you might have 80 percent intact material and 20 percent remolded material. Conceptually, that might help us in evolving our thinking about this—to think of more solid material at the head end than blocks floating in a paste.

Prior: Yes, I think that's right. One of our problems with the subbottom profiles is gas. We don't get that sort of subbottom information, and while, as Jim mentioned, there are a lot of boreholes out there, we are now at a point with these detailed maps and with our capability [where we ought] to go back to specific areas to start from. [There, with] shallow coring maybe only a couple of hundred feet [deep, we can] address specifically the geology of the subsurface horizons of a particular feature. What does it look like when you go through a block into the subblock material? What does it look like in and out of the channel? I think that's one of the next steps, which is going to be interesting.

Sedimentary Characteristics of the Northwestern Gulf of Mexico Margin

Arnold H. Bouma

Jim Coleman and Dave Prior discussed one area of the Gulf of Mexico on the U.S. side. Basically, we can divide the Gulf into three major areas: the broad central and western continental margin, the central abyssal plain, and the carbonate province on the east and south. I will concentrate on the northwestern slope, where I'll show a number of phenomena that will give you an idea about the progress we have made. It is an extensive area, and, although a lot of work has been done, the density of lines is insufficient to solve many of the detailed problems. Very few areas in this region greater in size than this building are horizontal. The simplified bathymetry does not indicate this. The bathymetry is hummocky and contains a number of canyons. I have a feeling we are talking about a quasi-permanence of canyons. Once they form, they more or less stay in that area and become modified during their lifetime by salt diapirs. But overall, once there is a system formed like the Alaminos Canyon, it stays in the same area for its lifetime. That means that there likely is a restricted number of submarine canyons, and those may be where the only sand bodies exist in the area. Figure 1, from Ray Martin, shows a plot of the salt diapirs that underlie this area. The plot is based on high resolution seismic profiles and common depth point (CDP) data. You see a high concentration of different sizes and different shapes, which really influences the entire geological evolution of this area. In the south, the diapirs are more massive, whereas, to the north, they are smaller. Note the lip that overhangs at the Sigsbee Escarpment. We don't know how far it continues to the north because of the difficulty of penetrating the salt where it has become too thick, but at least we can see Miocene layers butting against the overlying salt. Berryhill mapped the shelf, shelf break, and specifically the Flower Garden Bank area. Over the salt dome, he mapped with high resolution a few delta systems that more or less stop at the shelf-break area and from there feed into a submarine canyon system. He has not continued his mapping offshore yet. During low sea level, we have a buildup of deltas in the outer shelf area from where the canyons are fed by several mechanisms. The progradational facies very often contain a growth fault or several growth faults. Diapirs often have gas seeps around them. You can see angular unconformities cutting the steeply dipping layers; most of these are shales, overlain or capped and surrounded by older Tertiary shales. Salt domes can be covered, abraded, and re-covered—which means that they have been exposed during the latest low sea-level stand. Very often these domes are capped by rather thin reefs along the shelf edge.

Folger: Do you want to rerun that last one. You show what looked like a concavity; is that an artifact, or is that due to solution of the salt?

Bouma: No, this is collapse.

Folger: So there is probably salt solution.

Bouma: Very likely salt solution and then collapse. And the next one I have to show has the same feature; that's why I didn't mention it.

Here, in figure 2, is a nice collapse feature on the upper Continental Slope, with several growth faults. These shallow basins are very irregularly shaped and elongate. The sediment fill is fine grained, but there is no relation or no connection to the shallow water on the shelf. These features are quite common all over the area. The area of the shelf break near the Mississippi Delta has a lot of gas that makes seismic interpretation difficult; going into deeper water, you end up with some sliding, slumping, or folding mechanisms. We still don't understand some of the newer records that very often show unconformities in waters as deep as 300 m. This means we may deal with combinations of subsidence and erosion at depths that were originally shallower. Gas seeps again are very common all over the place. Any CDP record will show a little about the overall features on the Continental Slope and of the salt, outlined by Martin (1980) (fig. 1); often the base is not visible. Very seldom can we correlate the salt confidently. Figure 3 shows how a depression between the salt diapirs is filling in. You can see small side meanders and side canyons that flow into the major canyon. This means that the majority of sand will be found in the center, with some smaller amounts of sand on the local slope. We can also see major alternations of sand and shale, probably related to sea-level variations. This is visible in the Alaminos Canyon even on very old records. It gives you the idea that in the major canyons, some activity is still going on. These filled side canyon, diapirs, and the scattered reflectors you see are typical for the shales surrounding the salt itself. Look at the high-resolution record (fig. 4); slopes are steep—up to about 8° occasionally—and show slumps. Most of them are rotational slumps. Many are about the same size. I would not be amazed if there has been a previous deposit that, because of diagenetic alteration before or after deposition, formed a rather uniform slide zone that, combined with certain limiting angles and overlying sediment thicknesses, caused all these slumps to be roughly of the same size.

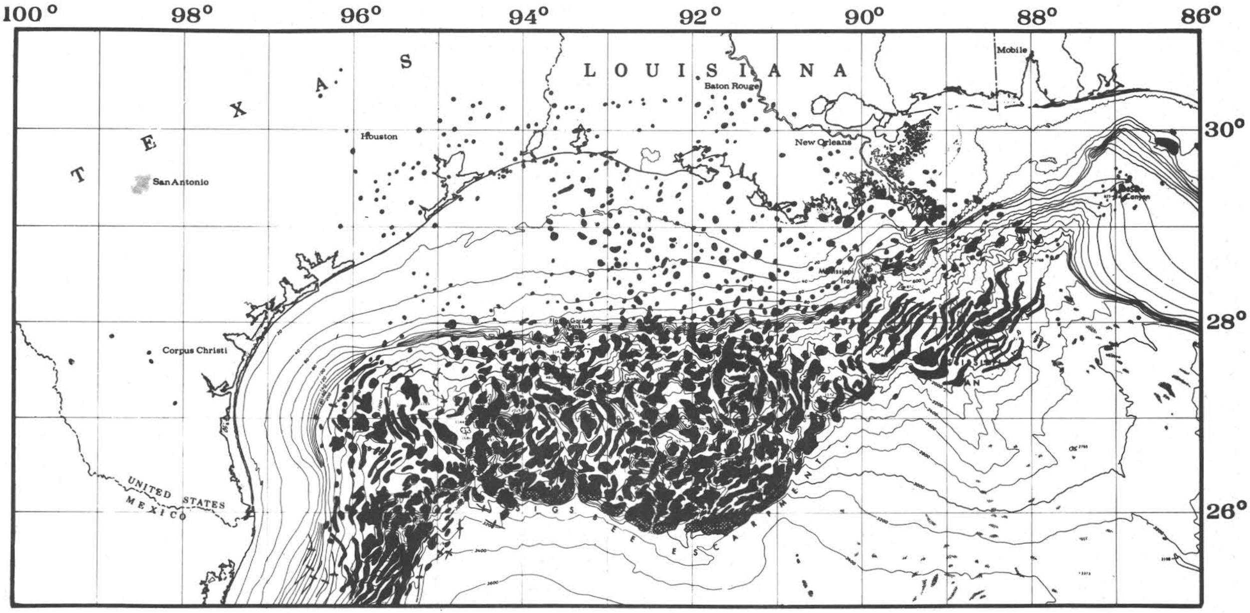

FIGURE 1.—Distribution of salt structures in the northern Gulf of Mexico region. Cross-hatched pattern along the Sigsbee Escarpment denotes extent of salt tongues. From Martin (1980).

Some correlation occasionally is possible on the upper Continental Slope; there, based on coiling direction of foraminifera, warm periods may be correlated with slump periods. Otherwise, seismic correlations are very often impossible because of the diapirs. I would like to show a few details. In figure 3, we have the Gyre Basin, which I consider as part of the former or ancestral Alaminos Canyon area; then another basin I want to show is the Orca Basin,which is characterized by its saline bottom water (fig. 5). We think that during high sea level, no sand is being transported. I am not talking about turbidity currents eroding this but about sand causing a scouring action and thereby maintaining a thalweg in spite of diapirs that are moving up more or less continually. So when we have high sea level and sand is not being transported, the continuous diapir growth may block a canyon and consequently form a basin. And we do find, below a Holocene pteropod ooze, sands underlain by clays. Very close, to the southeast, Shell drilled one of its many holes on top of the diapir; it had 150 m of shale underlain by 50 m of sand, what they call turbidite sand, and then salt. Very few of the cores—it was cored intermittently, unfortunately—are useful for dating. What they did get, reportedly, was late Pleistocene. This sand lies about 17 mi from the center of the Gyre Basin and is presently 400 m higher than the sands in the Gyre Basin. Making a simple calculation and a lot of assumptions that this sand is similar in age, similar in size, and similar in deposition mechanisms, and assuming its age may be around 8,000–12,000 years, and assuming a uniform upward movement rate of the diapir, this diapir moved up with an average rate of 4–5 cm a year. This causes considerable slope steepening in a short time. These phenomena obviously are geologically important, but, unfortunately, dating is difficult, and boreholes are insufficient to make realistic models.

In figure 5, you can see a seismic cross section over Orca Basin. There are two differences with the previous example: (1) The Orca Basin is formed between a number of upward-moving diapirs, and it is not related, as far as we can figure out, to any canyon system. (2) This basin is exceptional; no other basin has been found so far that has in its lower 200 m a very saline, anoxic brine. The brine has a salinity about 8 times that of seawater. The picture shows faulting and steep slopes with slumping; the interface of the brine with overlying seawater is visible on the airgun records very nicely.

In figure 6, the interface can be traced around the basin, and, interestingly, we found that it oscillates with a period of about 6 hours. Thus, there's some influence of tides observable. The

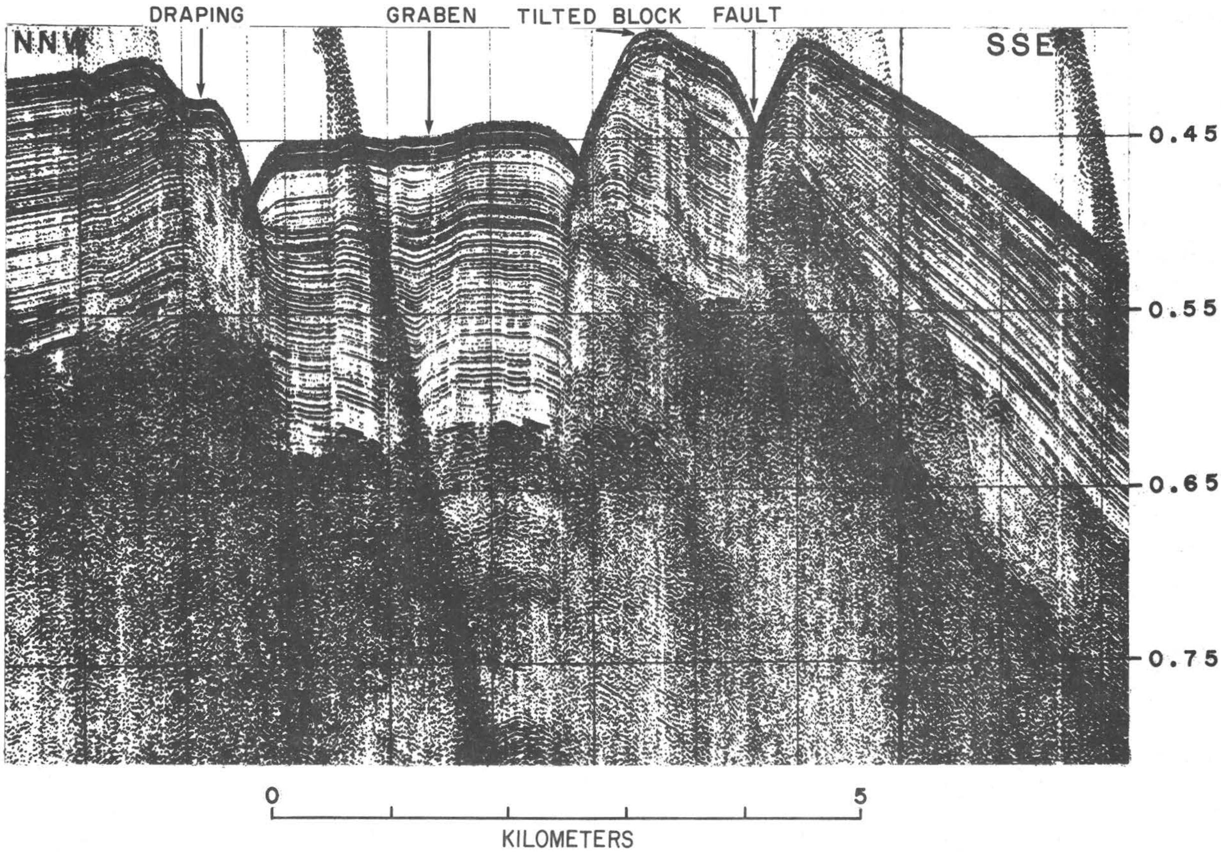

FIGURE 2.—Seismic-reflection profile (minisparker) over a collapse basin (East Breaks Basin at about 27°45'N and 94°50'W), showing a graben, faulting, tilting of blocks, and draping sediments. From Bouma (1982).

salinity of the brine/seawater contact suddenly increases rapidly, but it decreases in the sediments. Temperature goes up about a degree and a half, and the oxygen declines rapidly. So it is a nice area to study sediments because there is no bioturbation. We have the feeling that, on at least the eastern side where there is a steep slope, the diapir may be exposed occasionally because of slumping and that the salt dissolves, forming the brine which flows down like a density current. Similar very small brine flows have been observed on the Flower Garden Bank from submersibles. The brine fills part of the basin; collapse of the formed cavity then follows. The process can be repeated, not necessarily from the same point, as a result of repeated slumping by the upward-moving diapir. Just the right geologic conditions have to be present to form the brine. In cores, you can see the sedimentary layers about 3 to 4 mm thick with colors that alternate between yellow and gray.

Discussion following Bouma's talk:

Coleman: Are those cores from beneath the brine?

Bouma: Yes, that's correct. Above the brine you see a lot of bioturbation; you don't see any sedimentary structures.

McNeil: Arnold, would I have seen wormholes in that slide or is that something else?

Bouma: No, I'm not sure what it is. The chemists say that those are sulphide concretions. I've never seen this core; it's a picture I got from [Texas] A & M years ago.

Apparently we don't see any influence of diapirs on the deep-sea fan proper (fig. 7*A*). The situation differs from that on the west coast where the sand/clay ratio of the input material is very high. If we deal with salt ridges or shale ridges, coming up occasionally, they may block a canyon and starve the downflow area (fig. 7*B*). Thus, material may run through the saddles, leaving material

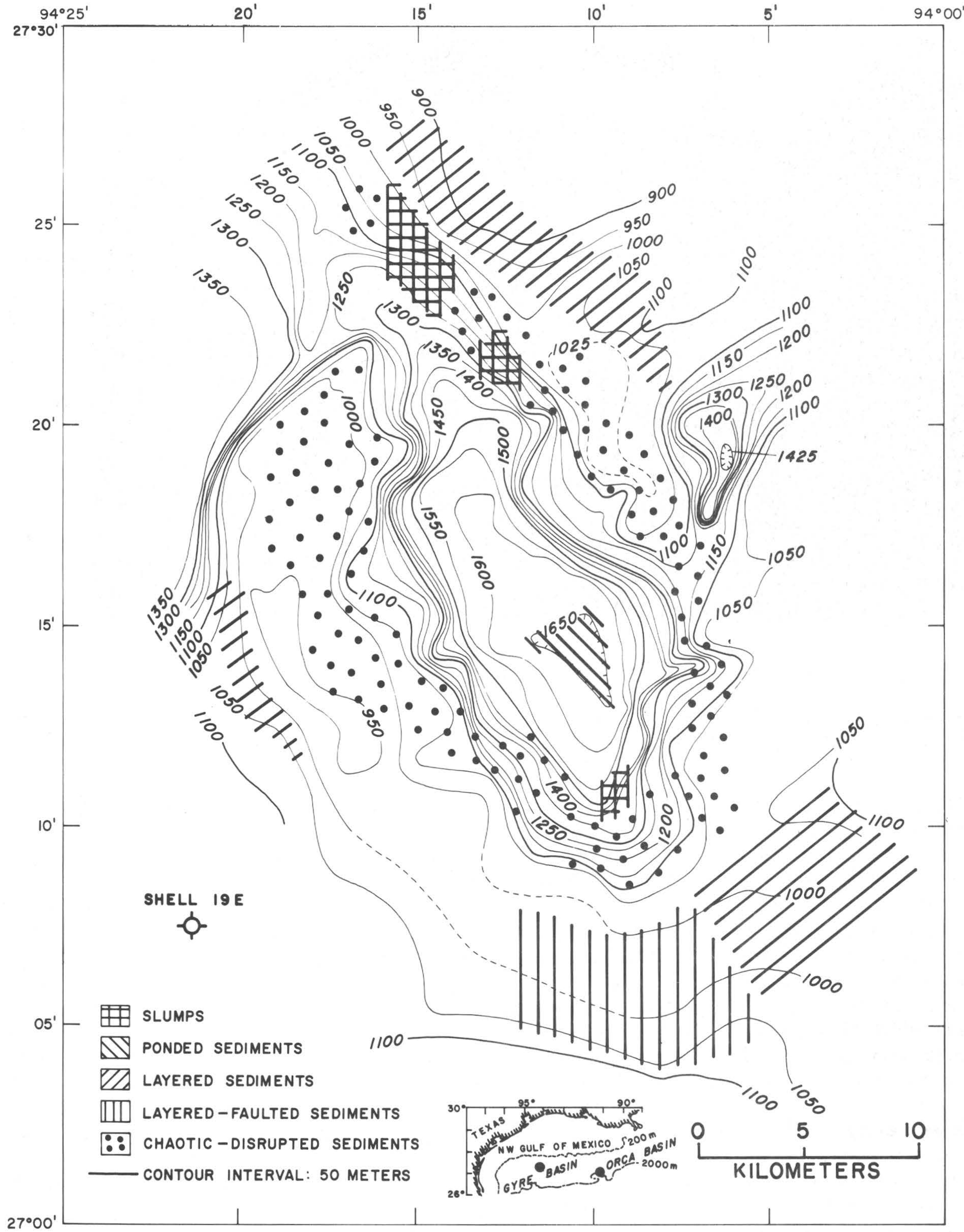

FIGURE 3.—Bathymetric chart of Gyre Basin with an overprint of 3.5 kHz-derived seismic facies. From Bouma and others (1978).

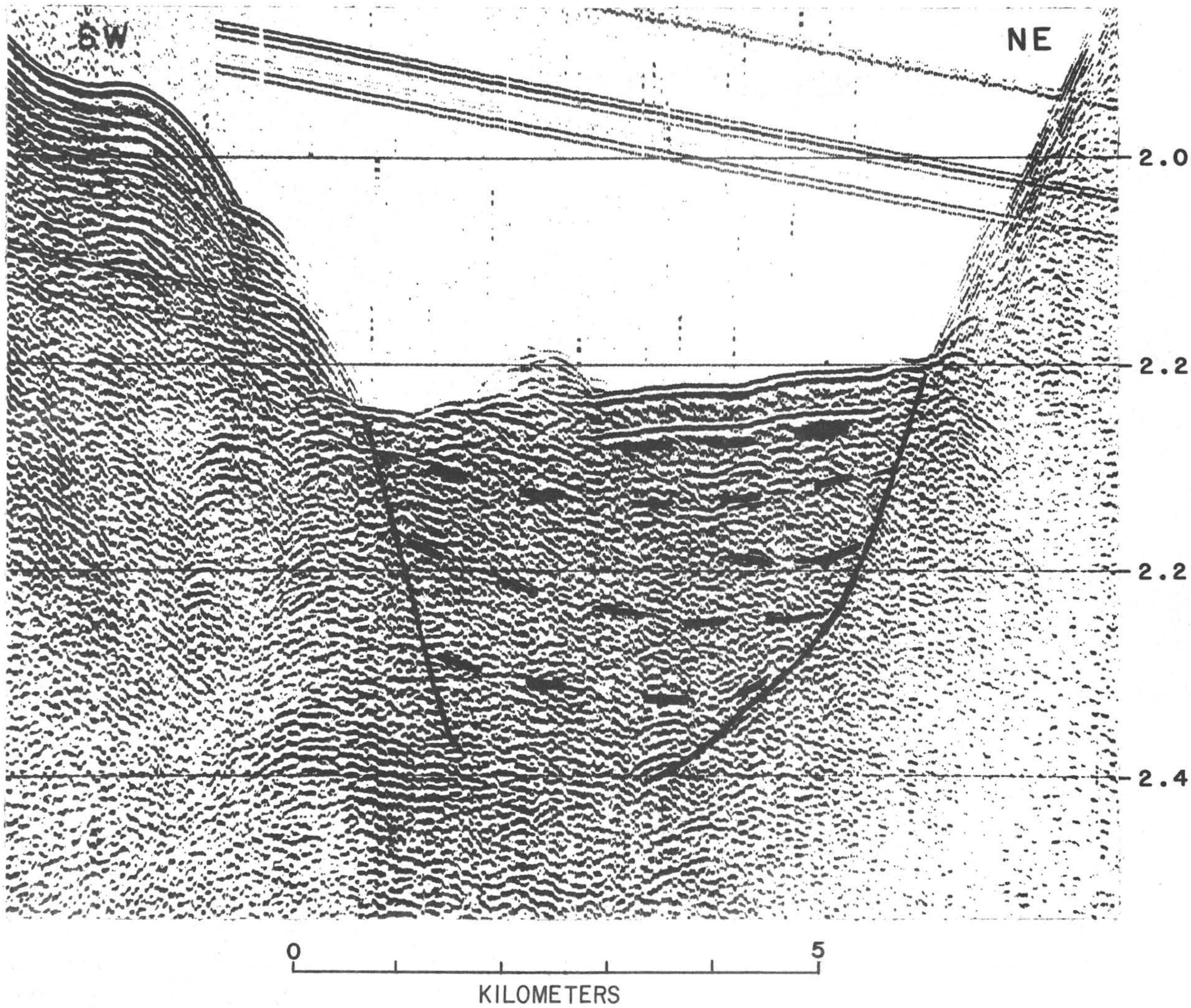

FIGURE 4.—Seismic-reflection profile (airgun) over Gyre Basin showing onlap of acoustic reflectors and sets. From Bouma (1982).

behind in the canyon as lenses of sands and clay. If we have an individual salt diapir, the picture is more complicated (fig. 7*C*). It can break off a canyon, producing starved areas downcurrent and sand bodies upcurrent. The last example is very complicated (fig. 7*D*). We have several little sand bodies whose origins are uncertain. What is the model for this type of setting, the type of source material, and the influence of the diapirism? And where do these pieces belong in the model of the deep-sea fan? How is this related to sea-level fluctuations, what are the modifications by salt diapirs, and what are the additional modifications by slumping? Thank you.

Coleman: What cores did you cut?

Bouma: We didn't get any cores yet.

Uchupi: I have a question. What can you interpret of the morphology you have? Because if those basins were always isolated, they were not part of a channel system, and the turbidites are local turbidites derived from surrounding topographic highs. My point is, that the whole thing had to be connected. But, as a matter of fact, according to the topographic information you have, the area consists of highs and lows, and it's very difficult to establish topographic continuity; the depths of individual lows do not even appear to be the same. It's just as logical to assume that the sources of the turbidites were local, due to the slumping caused by the upward-moving diapirs and deposition in the topographic lows.

Bouma: That's a possibility. Your assumption,

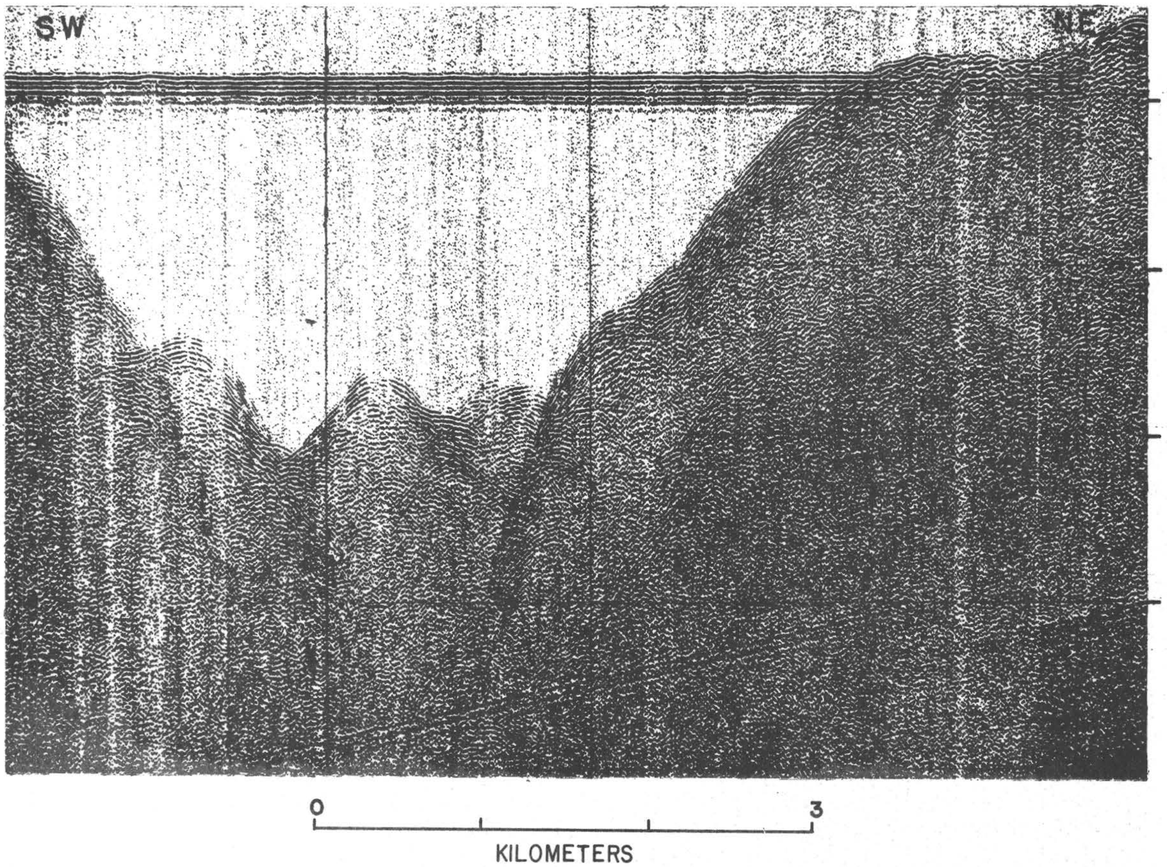

FIGURE 5.—Seismic-reflection profile (minisparker) over Orca Basin. Note the distinct horizontal reflector set at the top denoting the interface between the high-salinity brine and the overlying seawater. Seismic patterns are difficult to observe; slump structures are common. From Bouma (1982).

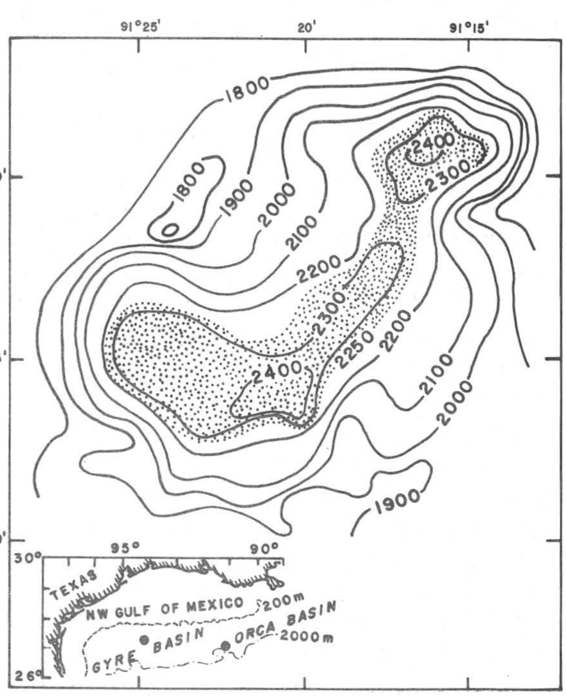

FIGURE 6.—Bathymetry of Orca Basin, in meters. The dotted pattern shows the approximate distribution of the brine. From Trabant and Presley (1978).

of course, is that you have sands. In addition, you look at the present topography. You should consider it a dynamic area.

Uchupi: You can go one step further and say that, given enough time, if the deposition of the turbidites is faster than the movement of the blocks surrounding them, the turbidites will advance in a landward direction and connect with the delta on the shelf; then you establish continuity.

Bouma: I'll keep it in mind, Al; thanks.

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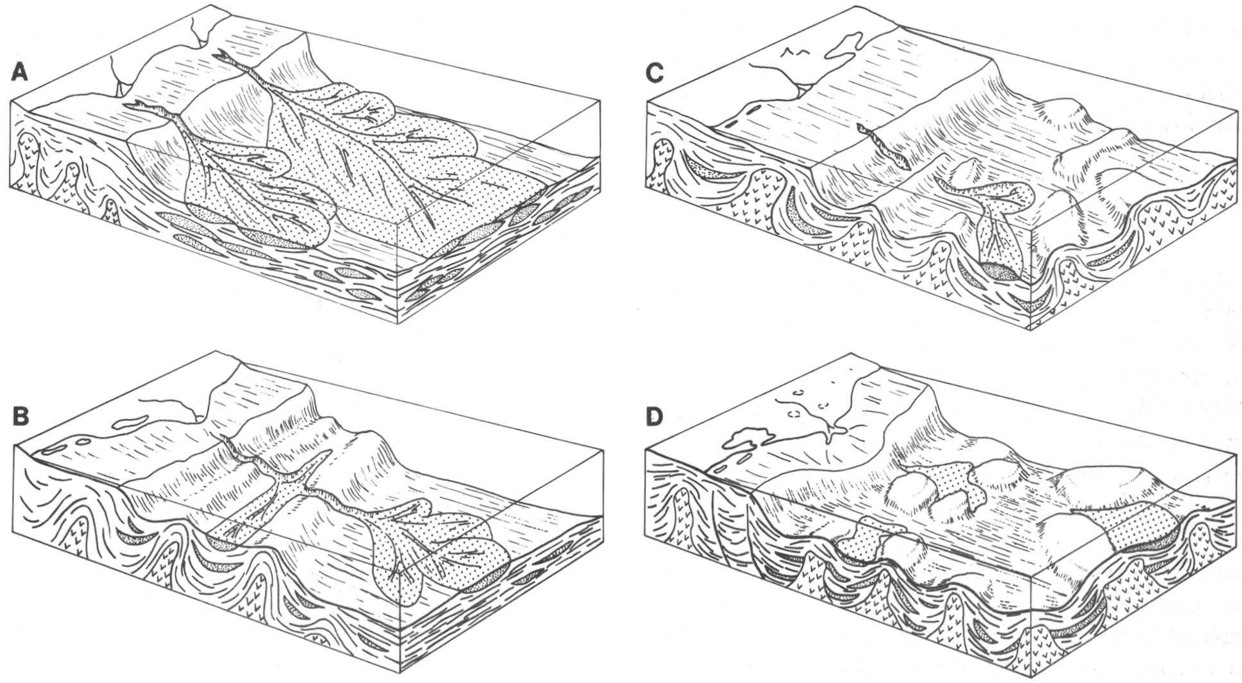

FIGURE 7.—Schematic diagrams showing: *A*: Classical coalescing fans on a lower Continental Slope and its adjacent Rise; *B*: Diapiric ridge with spill point. The ridge blocks downslope transport, resulting in lateral fill of a trough; *C*: Breaking apart of a canyon-fan system because of active diapirism; *D*: Breaking apart of a fan in individual segments by active diapirism. From Bouma (1982).

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Atlantic Margin

Geomorphic Map of the U.S. Atlantic Continental Margin and Upper Rise Between Hudson and Baltimore Canyons

David C. Twichell

INTRODUCTION

In 1979, the U.S. Geological Survey and the Institute of Oceanographic Sciences, U.K., conducted a cooperative study to map the morphology of the U.S. Atlantic Continental Slope and upper rise by long-range sidescan-sonar. This study summarizes results of the 220×50 km portion of the survey conducted between Hudson Canyon and Baltimore Canyon offshore of the Middle Atlantic States (fig. 1). The survey area extended from the shelf edge to approximately 2,500-m water depth on the upper rise. Interest in petroleum exploration on the Continental Slope in this area has required a detailed understanding of the morphology of the slope for platform and pipeline stability studies. Several detailed studies of small segments of the slope by medium range sidescan sonar (McGregor and others, 1982; Robb and others, 1981a,b,c; Ryan, 1982; Farre and others, 1983), detailed bathymetric and geologic mapping (Robb and others, 1981a,b,c; Kirby and others, 1982; Hampson and others, 1982), and submersible observations (Stubblefield and others, 1982; Robb and others, 1983) provide valuable insights to processes active in these areas. The long-range sonographs, although not able to resolve some of the features seen in the detailed studies, provide a regional overview and as such permit extending features beyond the limits of the detailed study areas. Furthermore, the large area surveyed permits insight to the regional variability in the morphology of the slope that cannot be obtained from the detailed studies.

METHODS

GLORIA (geologic long range asdic) is a low-frequency (6.5–6.7 kHz) long-range sidescan sonar system that was developed at the Institute of Oceanographic Sciences, U.K. (Somers and others, 1978). The system was set to scan 15 km to each side of the towed vehicle in this area, and track lines were run close enough that most of the area was insonified from two viewing directions (fig. 2*A*). Sonographs are not slant-range corrected, but they have been adjusted anamorphically to correct for along-track variations in ship's speed. The images from the different tracklines were compiled in a mosaic at a scale of 1:250,000. The base map is a mercator projection based on the international spheroid with a reference latitude of 46° north (see fig. 3).

Seismic-reflection profiles were collected by the USGS along GLORIA survey lines (fig. 2*B*). Additional, previously collected seismic profiles aided in the identification and mapping of geomorphic features on the GLORIA images (fig. 2*A*). Features such as submarine canyons, scarps, levees, and exposures of pre-Pleistocene substrate were identified on the widely spaced seismic profiles, and their courses or extent then could be accurately mapped on the sonographs (fig. 3).

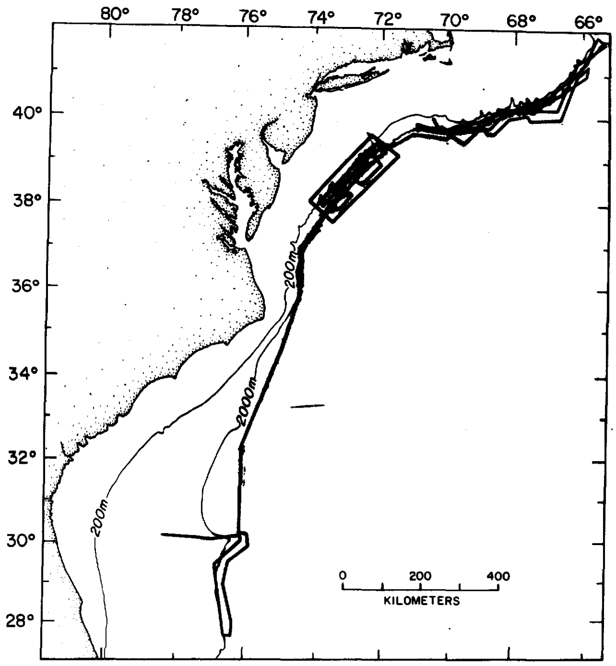

FIGURE 1.—GLORIA coverage of the eastern United States continental margin. Solid line is the track of the R/V *Starella*, and shaded area represents GLORIA coverage. Box outlines part of survey discussed here.

DISCUSSION

Submarine canyons are the major feature shaping this segment of the Continental Slope (Twichell and Roberts, 1982), although they are not uniformly distributed. Between Hendrickson and Hudson Canyons, they are absent (figs. 3, 4*A*), yet, around Spencer Canyon, they are spaced 2–4 km apart (figs. 3, 5). The canyon axes form a parallel drainage pattern on the slope, with the heads of some indenting the shelf edge while most are initiated on the upper slope. Canyons extend across the slope, some having sinuous axes (fig. 4*B*) and others straight axes (fig. 3). Most canyons are connected with channels that extend onto the rise, but a few stop at the base of the slope (fig. 3).

A network of closely spaced, nearly parallel, shallow gullies cut the canyon walls on the upper slope (shallower than approximately 1,000 m). These gullies feed into the canyon axes at high angles, giving the upper parts of the canyons a herringbone appearance (figs. 3, 4*B*, 6). Along much of the upper slope, the gullies interfinger at the crests of the intercanyon divides. With the gullies included as part of the canyon systems, little of the upper slope in this area is not part of a canyon system. The absence of gullies on the canyon walls in water depths greater than 1,000 m probably is at least in part because the canyon walls have gentler gradients on the lower slope than on the upper slope (Twichell and Roberts, 1982).

Mass wasting undoubtedly has contributed to shaping the Continental Slope, but most of the evidence suggests that individual slides are relatively small and associated with the submarine canyons. The gullies along the canyon walls are interpreted to be slide scars. The absence of mass wasting in the intercanyon areas is probably due to the scarcity of intercanyon areas and to the gradients of these areas being considerably gentler than the canyon walls.

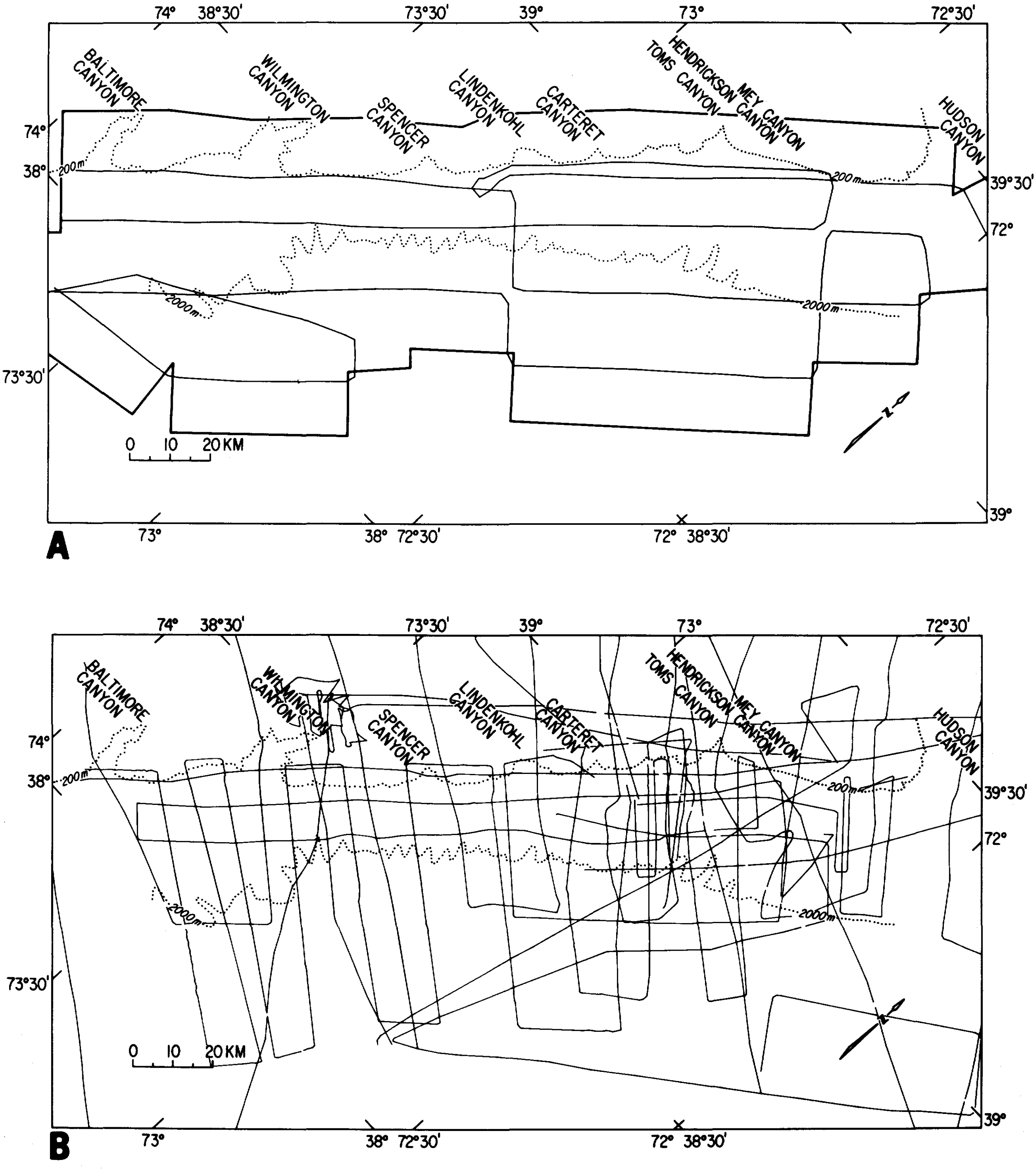

FIGURE 2.—Track lines along which (*A*) GLORIA sonographs were collected, and (*B*) single-channel seismic profiles were collected by the USGS.

The only probable large slides in this area are the two sedimentary blocks that trend diagonally across the middle and lower slope in the Baltimore to Wilmington Canyon area. The full extent of these two blocks and their influence on the canyon systems can be mapped on the GLORIA images (figs. 3, 7). Seismic-reflection profiles indicate gentle deformation of the reflectors within the blocks

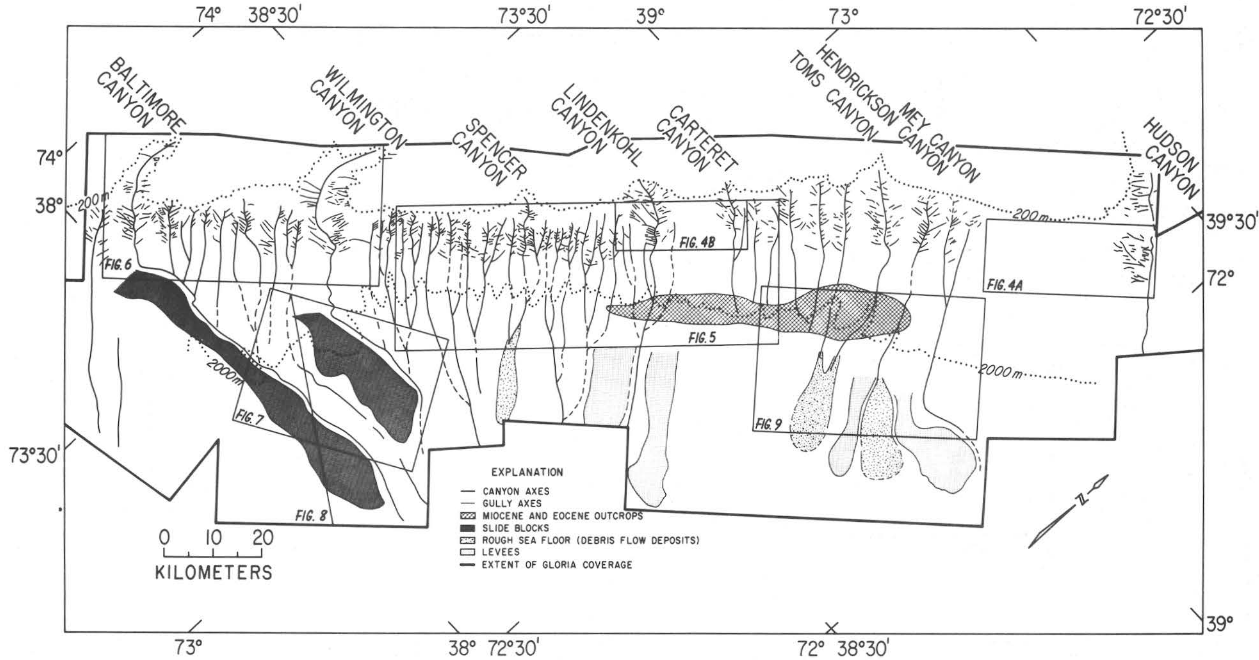

FIGURE 3.—Map of geomorphic features identified on GLORIA sonographs.

(fig. 8). Submersible observations note steeply upturned beds downslope of one of these blocks and gravel and a tree root cast exposed on the downslope scarp of the block (Stubblefield and others, 1982). The age of the slide blocks is uncertain. They rest on an inferred Pliocene unconformity (McGregor and others, 1982), but the sediment onlapping the toe of the block (fig. 8) indicates that its movement was not recent. Also, the absence of a scarp upslope from these blocks probably is due to its having been buried and the overlying sediments subsequently shaped by canyon building (fig. 3).

The exposed Eocene and Miocene substrate that Robb and others (1981a,b,c) and Hampson and others (1982) mapped on the lower slope in their study area between Lindenkohl and South Toms Canyons has a distinctive signature on the GLORIA images; it can be traced from Hendrickson Canyon to just south of Lindenkohl Canyon (figs. 3, 5). The exposure of these strata is shaped by numerous scarps and depressions (Robb and others, 1981c) that show as discontinuous light and dark lines and patches on the GLORIA images. This is the only area off New Jersey where this signature is seen; the rest of the lower slope (other than localized patches in canyon floors) is acoustically uniform, presumably reflecting a smooth Pleistocene and Holocene sedimentary cover.

On the upper rise, levees have been built on the sides of some channels and debris flow fields at the ends of others (figs. 3, 9). Low broad ridges having a weak acoustic signature on the sonographs are interpreted to be levees. The levees have formed beside some channels on the rise but are absent from others. The relation of the levees to the ridges that separate the canyons on the slope cannot be resolved by these data. Debris flow fields, which have been observed from submersibles and on midrange sidescan sonar images (Robb and others, 1981a) show on the GLORIA images as a strong acoustic signal (fig. 9). These debris fields occur on the upper rise at the mouths of some canyons and can be traced seaward as much as 20 km.

In summary, the long-range sidescan sonographs in concert with seismic-reflection profiles show that submarine canyons play a major, although not exclusive, role in the shape of this part of the Continental Slope and upper rise. Canyon wall gullies suggest that, in addition to sediment supplied to canyons off the shelf, sediment also is derived from the slope by erosion of the canyon walls. Downslope sediment transport presently is through the canyons because, in the few intercanyon areas that do exist, the sea floor is smooth and shows no evidence of mass wasting. The debris flows and levees on the upper rise are aligned with the seaward

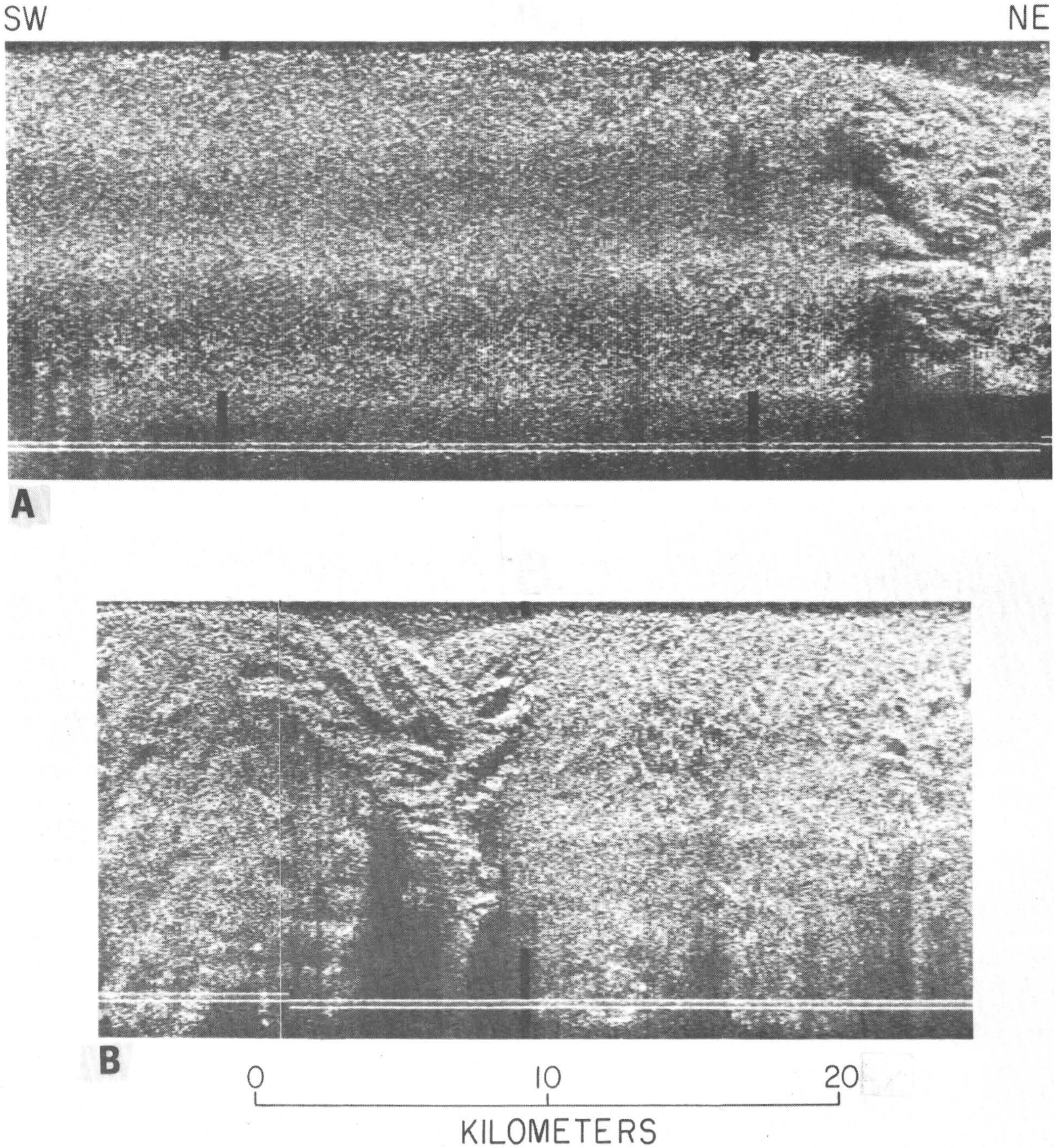

FIGURE 4.—*A*. Sonograph showing gullies on the southwestern wall of Hudson Canyon and the smooth upper slope south of Hudson Canyon. *B*. Sonograph of Lindenkohl Canyon showing the sinuous canyon floor and the gullies on the canyon walls.

few extensions of canyons and further support the importance of canyons as the avenues of transport across the slope. The slide blocks off of Wilmington and Baltimore Canyons suggest that, in the past, large-scale mass wasting also contributed to slope degradation.

Discussion following Twichell's talk:

Embley: The gullies only go down to a certain depth, right?

Twichell: That's right; they're limited to the upper half of the slope.

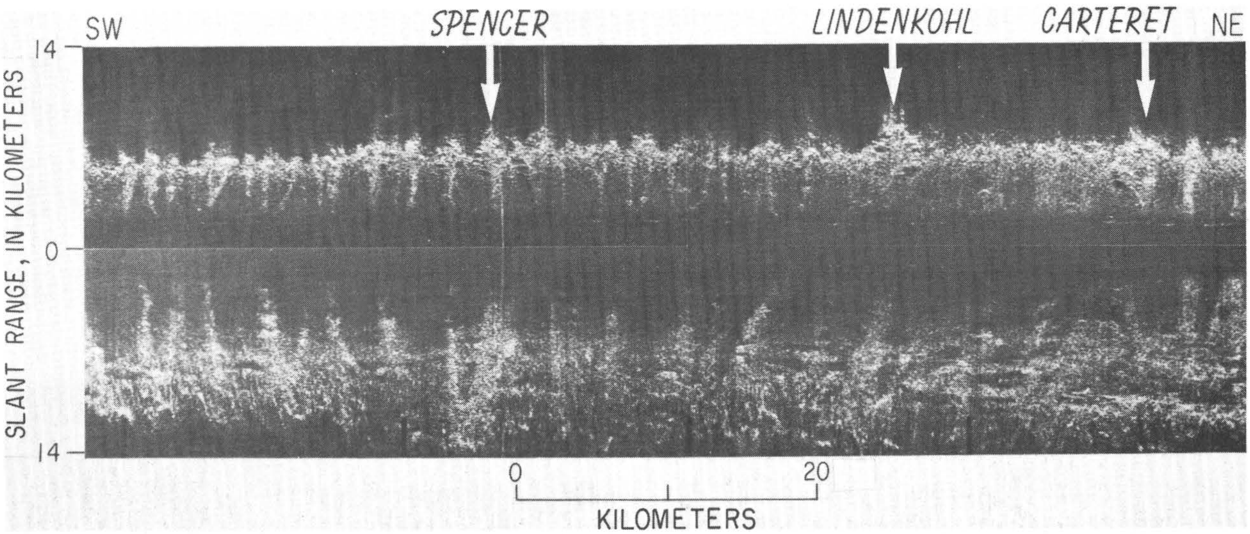

FIGURE 5.—Sonograph of the middle and lower slope from Carteret Canyon to south of Spencer Canyon. Canyon axes have a bright acoustic signature. Note how they merge in a downslope direction. Lineations perpendicular to the canyon axes are outcrops of Eocene strata on the lower slope.

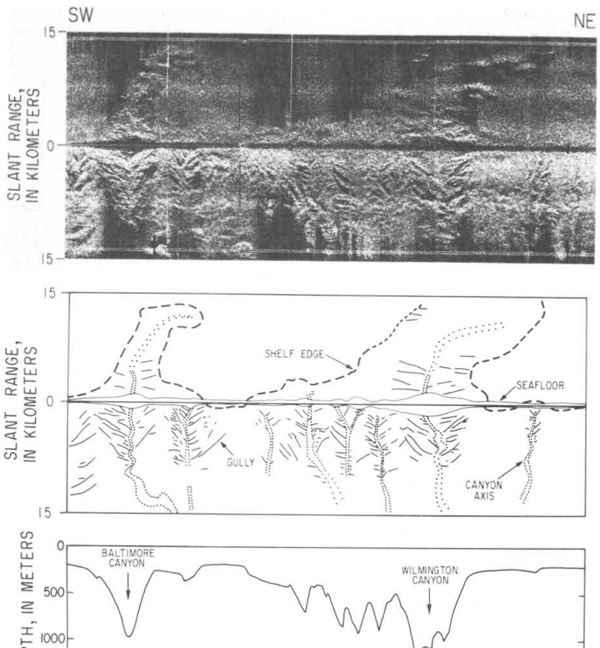

FIGURE 6.—The upper Continental Slope between Baltimore and Wilmington Canyons. Upper panel shows the sonograph, middle panel an interpretive map, and the lower panel the water depth along the ship's track.

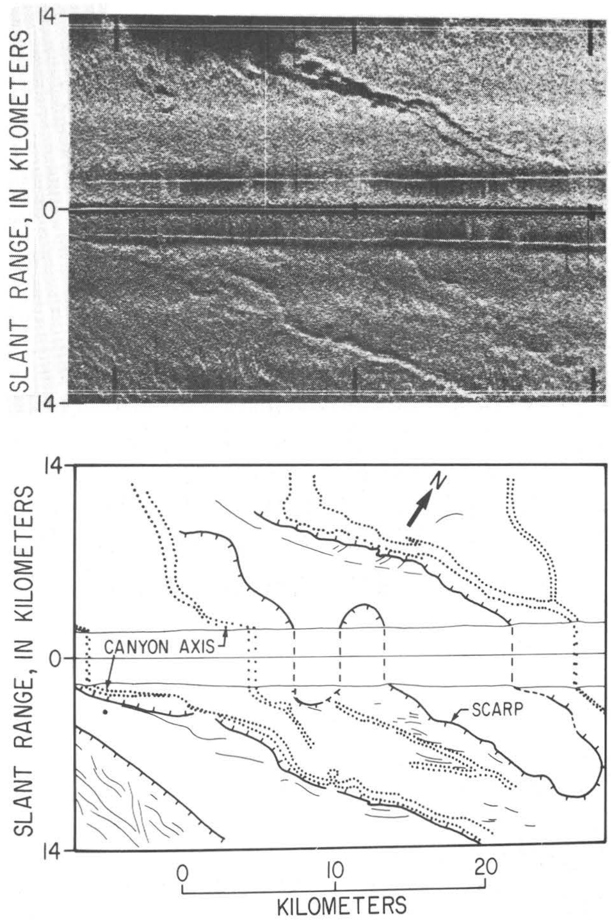

FIGURE 7.—Sonograph and interpretive map of a portion of the lower slope and upper rise seaward of Wilmington Canyon, showing the axes of Wilmington and Baltimore Canyons and the possible slide block separating them.

Embley: So could they be due to more direct runoff at the time of lowered sea level brought in from the sides?

Twichell: I think the problem with that, as K.O. [Emery] mentioned this morning, is that a lot of

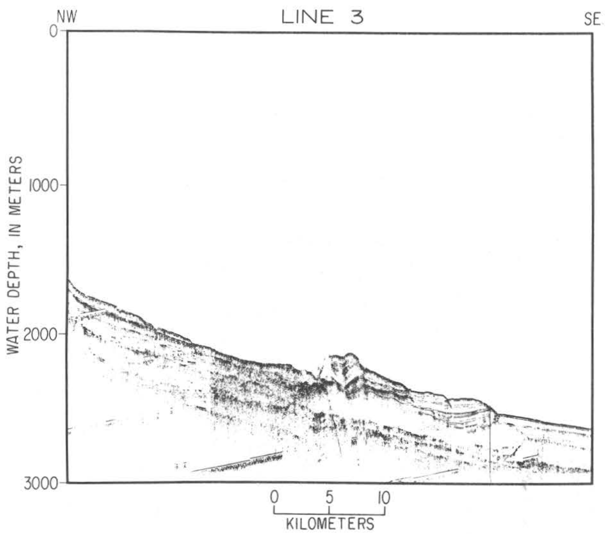

FIGURE 8.—Seismic-reflection profile across one of the possible slide blocks on the lower slope seaward of Wilmington Canyon.

these gullies come up on the ridges well downslope from the shelf break so that they're still down in several hundred meters of water.

Embley: From the shape of the contours, it seems as if there is a lot of opportunity for runoff to come directly to some of those indentations. They seem to be water-depth limited. Are they also slope limited?

Twichell: The canyon walls are steeper and higher on the slope than they are downslope in most cases. You also have Pleistocene cover upslope that is less, or nonexistent, on the lower slope. I'm not exactly sure why they're limited to the upper slope. This is something that we want to look into.

Ross: A point we discussed before. I think that by labelling all those ridge crests red, you make the area look much more treacherous than I think your data give the appearance of.

Menard: I agree it looks twice as busy by having the highs and the lows. As you say, there's no place for those slumps to have come from relatively recently. They don't look all that different to me in the cross section I saw from stuff I can remember mapping in the western Mediterranean that were obviously modern turbidites. The position is just where they'd be if they were levee deposits. Had you entertained and ruled out for some reason the possibility that these were just levees?

Twichell: We debated that issue and my knowledge of levees is probably not as good as it should be, but my feeling was that a large canyon like Wilmington ought to have a levee on both sides of it.

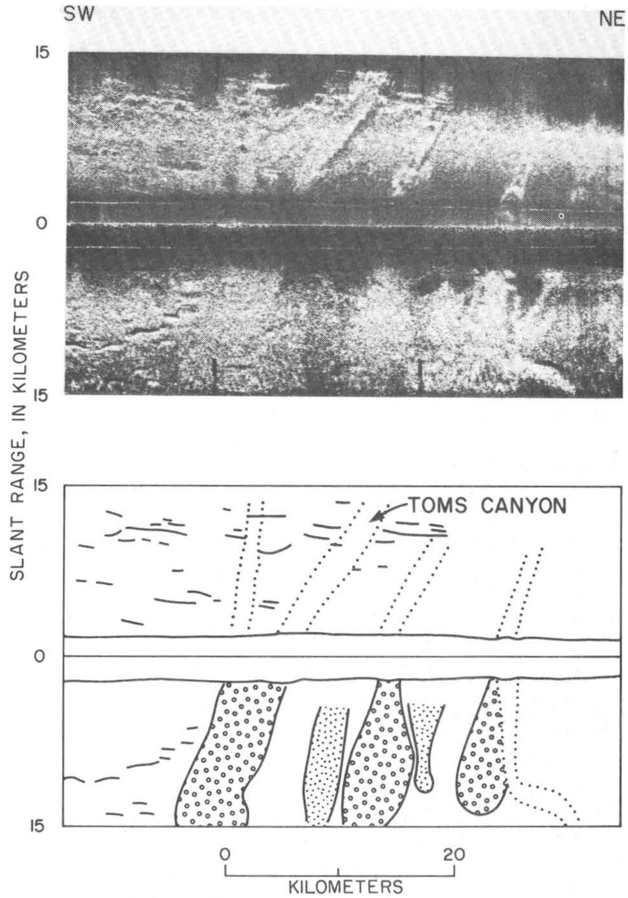

FIGURE 9.—Lower slope seaward of Toms Canyon showing paths of canyons across Eocene exposures on the lower slope, debris fields (open circle pattern) on the upper rise at the mouths of canyons, and possible levees (dotted pattern) to the sides of the debris fields.

Menard: You've got two canyons where you could feed in a lot of sediment by longshore drift, and those are where you've got your long ridges or what-have-you below.

McGregor: Can I make a comment here, Dave? I have some other profiles down in that area, and another argument against levees is that the dip is the wrong way for Wilmington Canyon, primarily that block that flanks Wilmington. It appears that the whole block is rotated up to the south so it's dipping back toward the canyon.

Menard: That's a good argument.

McGregor: I would think the deposits would be thickest at the canyon and slope away, but this is just the reverse.

Menard: Sure, true.

Teleki: You could alternately think of it as a feature that has been incised by the canyons coming through a depositonal surface, therefore, an erosional feature rather than mass-wasting feature. It seems rather obvious that you have one large feature cut through by a canyon.

Twichell: That could be. O.K. Thank you.

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Diversity of Processes and Morphology on the U.S. Atlantic Continental Slope and Rise

Bonnie A. McGregor

I would like to continue this discussion on the mid-Atlantic Slope and Rise with another data set. Dave Twichell and I have come to the same conclusion that slumping has caused the ridges on the rise near Wilmington Canyon, but the evidence is not complete. I'd be happier if I could find a scar, for instance, and I don't see it. I think maybe, as Dave said, we're looking at a modified slope; that the slump block is an older feature and deposition has covered the original scar and perhaps this material, in turn, has been eroded.

I'd like to show you (fig. 1) a long profile run parallel to the slope at 1,500 meters, basically in the same area where Dave Twichell showed the GLORIA coverage. Morphologic features along the profile include the slope adjacent to Hudson Canyon on the northeast, Lindenkohl Canyon, Spencer Canyon, and Wilmington Canyon; as you continue south, seaward of Delaware Bay, Norfolk and Washington Canyons; and seaward of Chesapeake Bay, the slope seaward of Albemarle Sound. The profile ends seaward of Cape Hatteras. I'd like to call your attention to the fact that the slope along strike is obviously very different. Between Hudson and Wilmington Canyons is an area where there has been extensive erosion. There are numerous valleys and ridges all along the slope. When you get south of Wilmington Canyon, the depositional style appears to have changed; in the area around Baltimore Canyon, there are fewer valleys. They're larger and more widely spaced. The ridges themselves are much broader, and we're able to define their internal structure. This sort of environment seems to hold all the way to Washington Canyon; then, as you get to the vicinity of Norfolk Canyon and farther south to Hatteras, it changes again and seems almost to be intermediate in the sense that there is much

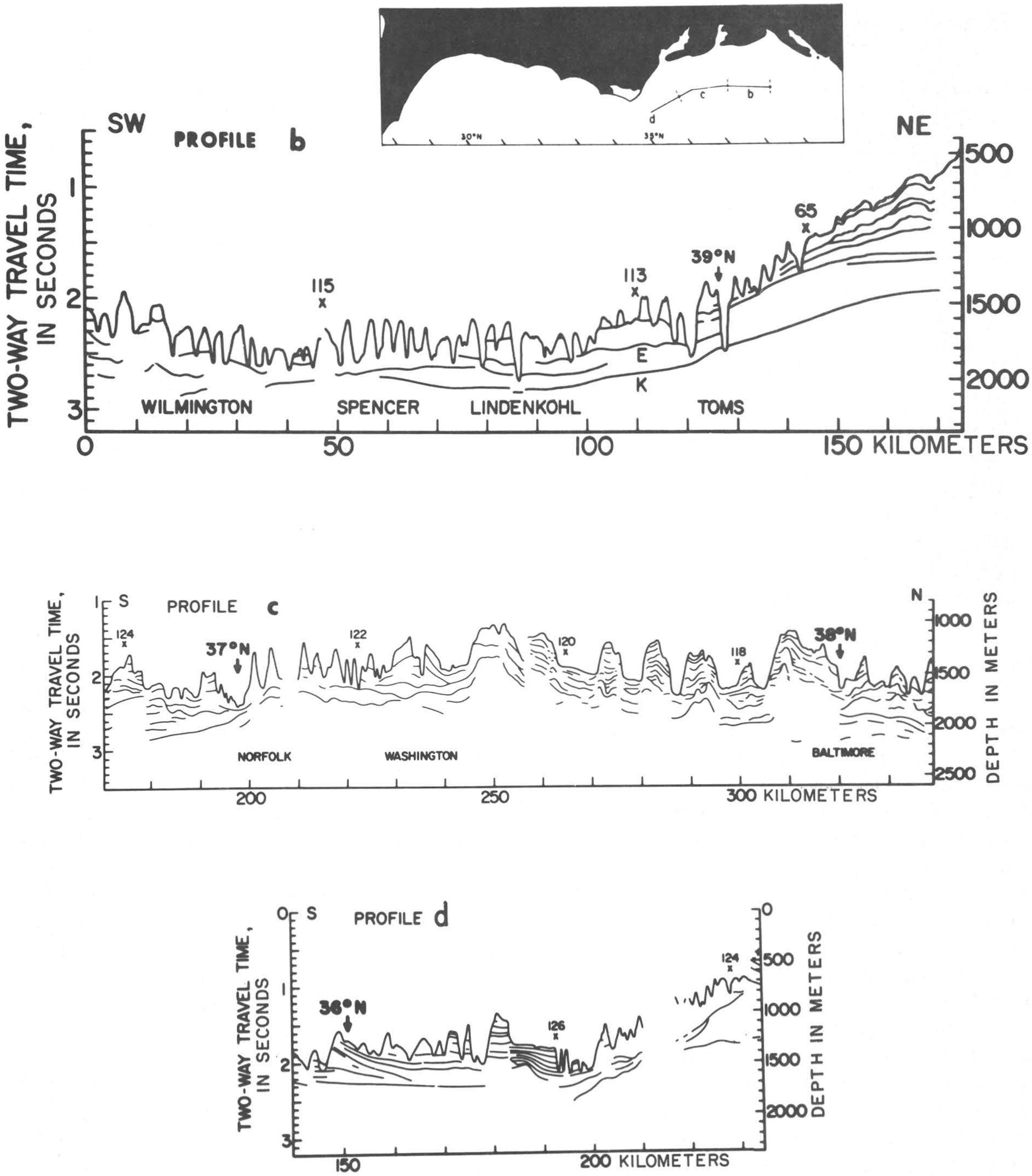

FIGURE 1.—Line drawing of an airgun profile from Hudson Canyon to Cape Hatteras. The profile is oriented parallel to the shelf edge along the mid-slope, approximately 1,500-m water depth.

more erosion and valley dissection of the slope but not as much dissection as on the slope north of Wilmington Canyon. I call your attention near cross profile X 126 (fig. 1) to a flat portion of the slope and a ridge that is reminiscent of what we saw in Baltimore Canyon. For your geographic location, we're looking at the outlined region on the slope seaward of Albemarle Sound (fig. 2). I call your attention to the distance or spacing between the 1,000- and 2,000-m contour (fig. 2); as

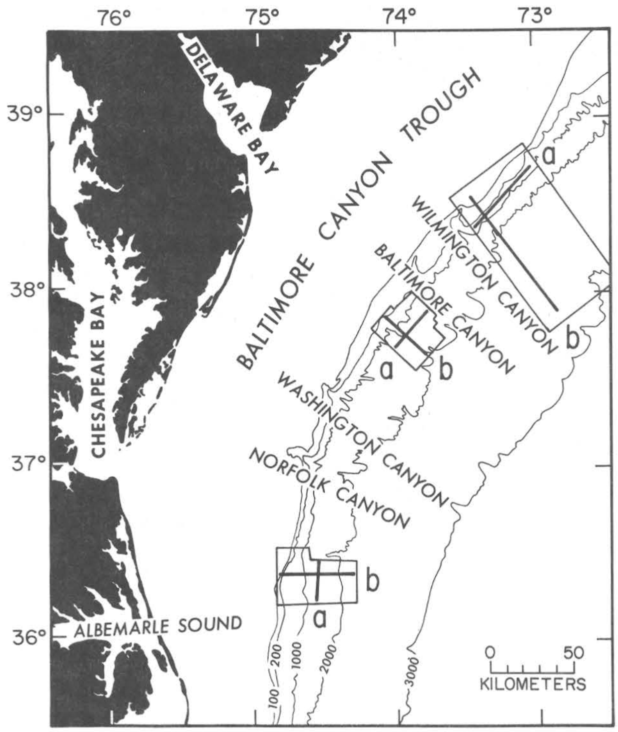

FIGURE 2.—Index map showing the location of three study areas and representative profiles *a* and *b* in each area. Contours are in meters.

I said, the area north of Wilmington Canyon looks very erosional with a narrow contour spacing. At Baltimore Canyon, the contour spacing increases and this looks like an area where deposition may be a dominant process. Seaward of Albemarle Sound, the bathymetry on the slope is poor, so this may not be quite a true picture, but it seems to be intermediate in contour spacing to either of the other two areas.

Now, for this discussion, we're going to look at the degree to which the slope has been eroded, both by valleys in the erosional sense and also by mass wasting or slumping. I'd like to deal primarily with large block movement. We're talking of blocks on the order of 100 m or more. That's not to say that small features on the 3.5-kHz records were not seen in these areas; we saw them but, for this discussion, I'm not going to try and trace them. We're going to look primarily at what you see on a 40-in.^{3} airgun single-channel record. The three areas we're going to look at in some detail are shown in figure 2 to see if we can get a handle on how the processes have changed along the margin to shape the morphology we see today.

This is the data coverage in the area north of Wilmington Canyon (fig. 3). Wilmington Canyon is in the southwest part of the survey area; Spencer Canyon in the middle, and Lindenkohl is in the northeast part (see fig. 1, profile b). Jim Robb is going to describe the abutting area from Lindenkohl Canyon northeast to Toms Canyon in some detail as well (Robb and others, 1981). Every fifth strike line is shown, so we really have data coverage that is five times greater than what you see (fig. 3). Basically, the data coverage comprises 1-km line spacing on the strike lines and 2.5-km spacing on dip lines. We have constructed a bathymetric map at a scale of 1:40,000 (fig. 4).

We're going to look at a series of four dip lines that are spaced equidistant along the slope to the northeast, and then we're going to look at a series of strike-oriented lines on the slope and rise (figs. 5 and 6).

I'm going to show you some blowups of these in figures 7 and 8. Profile 910 is closest to Wilmington Canyon (fig. 5). The numbers get smaller toward the northeast. What I want to show you is the steepness of the slope. On all of the profiles, the Continental Slope is about 7 to 10 degrees; when you look at the rise, you see that the bathymetry changes very markedly in character. Profile 910 is located at Wilmington Canyon; this is actually a crossing over the block that Dave Twichell showed. Profile 830 shows another crossing across the nose of the block; then, as you get off to the northeast, profiles 750 and 670, you see the topography of the rise become much more subdued (fig. 5). What I would like also to point out is that in figure 5 we see a very definitive horizon that can be traced along the rise up onto the slope on all of these profiles. A close-up of the large block on the rise adjacent to Wilmington Canyon is shown in figure 7. The horizon underneath it is continuous and can be traced onto the slope and rise. I believe that we are looking at a slump block and not at a levee. Look at the internal faulting (fig. 7). An acoustically transparent layer above the unconformity or hard reflector seems to deform plastically up into the faults within the block, almost like fingers of material extending up into the block itself. We have no drill information out here at all, which is something that we really need. It's my belief that this unconformity (figs. 5 and 7) may represent a Pliocene unconformity, which we also can see on the slope and trace along the slope from north to south. It can be traced on the rise in this area to Deep Sea Drilling Project

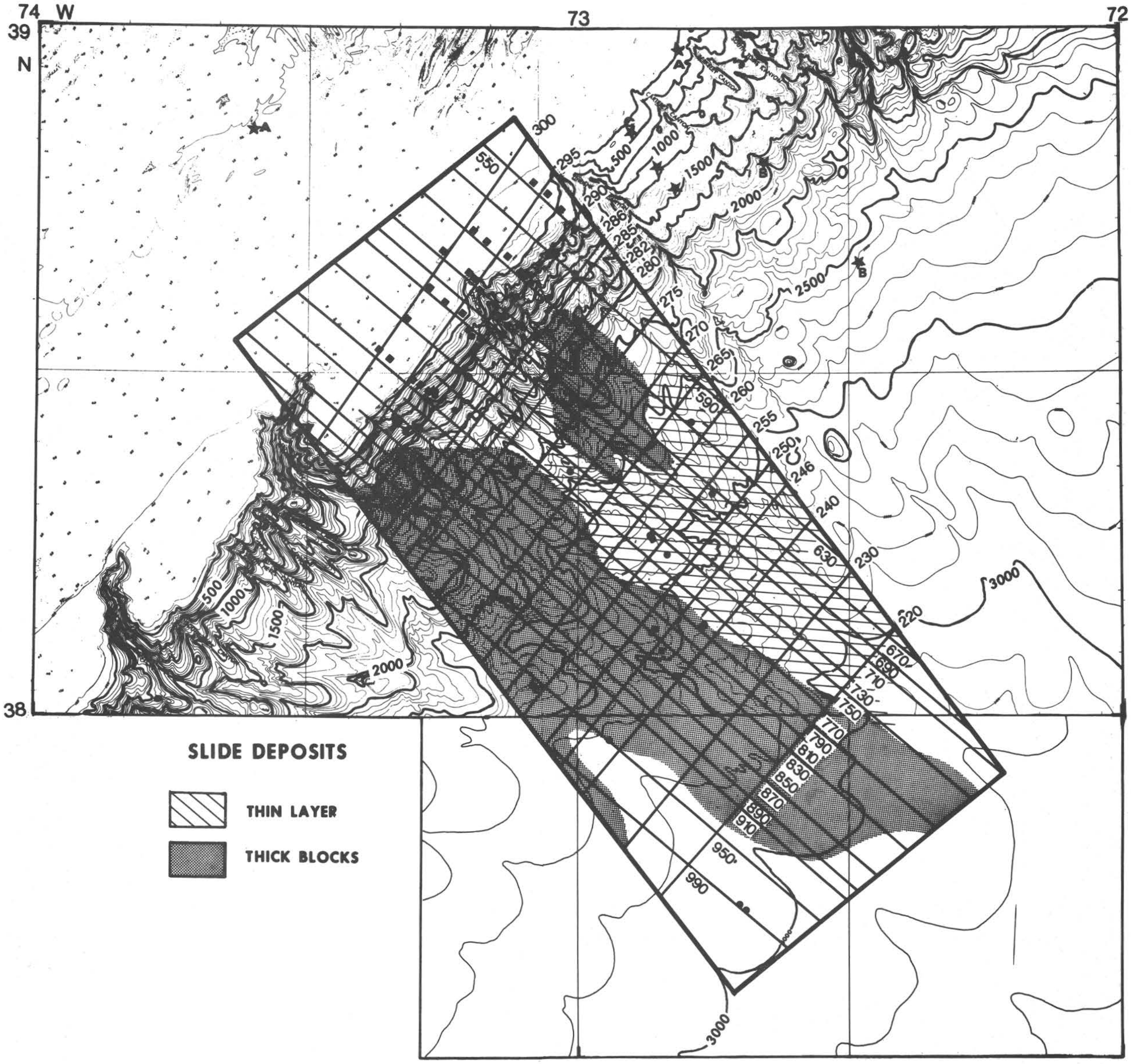

FIGURE 3.—Coverage of geophysical data between Wilmington and Lindenkohl Canyons is superimposed on the bathymetry in meters. In general, only every 5th trackline with a strike orientation is shown. Stars denote location of drilled cores, dots the location of piston cores, and boxes the location of grab samples.

(DSDP) hole site 106 to the northeast, which is a long way away. What we're looking at here may be the Pliocene unconformity that Vail and others (1977) believe is global. We're looking at Pleistocene material deposited on top of that surface (figs. 5 and 7). Further seaward, a crossing over the block shows a feature with internal structure, the nose of which is a series of rotated upturned beds (fig. 8). The bathymetry shows a pull-apart zone behind this feature; so, it looks as if the block deformed the material in front of it, and then, subsequently, we had an erosional period when some of the deformed sediment on the side was removed (fig. 7 at kilometer 10). All of the flat-lying onlap beds that Dave Twichell showed on a profile actually represent postdeposition in a valley adjacent to the block. I'm not saying sediment failure is so old that we have thick blanketing of sediments, but it certainly occurred sometime in the Pleistocene. I do not believe sediment failure occurred in the late Wisconsinan, I think it occurred sometime before that because we had to

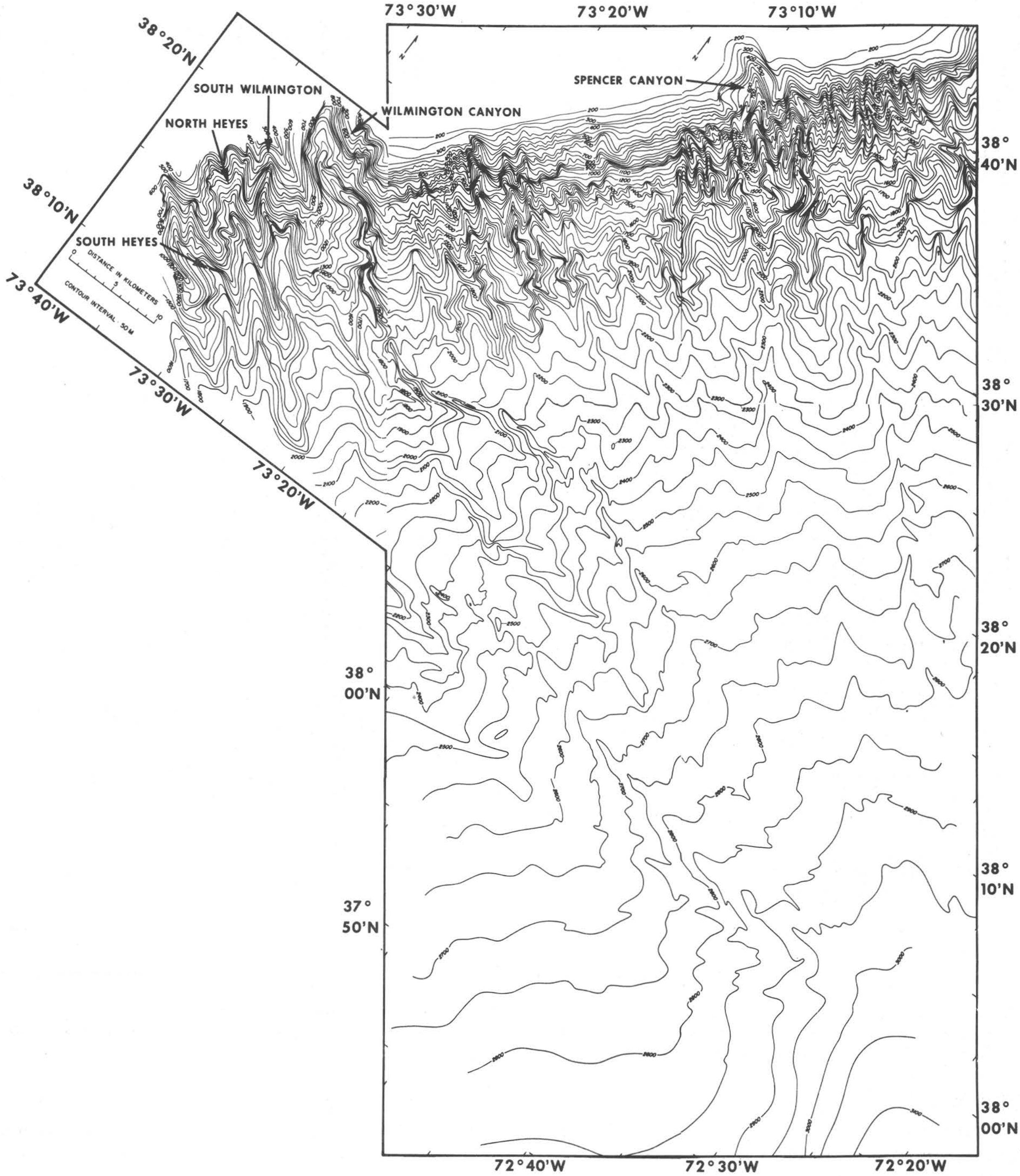

FIGURE 4.—Bathymetric map of the Continental Slope and Rise in the Wilmington Canyon area. Contour interval is 50 m.

have a major period of erosion after failure to cut the valley and remove some of the material that should have been deformed on the rise.

On strike profiles, you can't see any difference between Spencer and Wilmington Canyons, at 1,500 m, and between any of the valleys adjacent to them; they appear to cut down to approximately the same level (fig. 6). Profile 265 crosses a portion of the slump block on the rise. Again you can see the acoustically transparent material. This

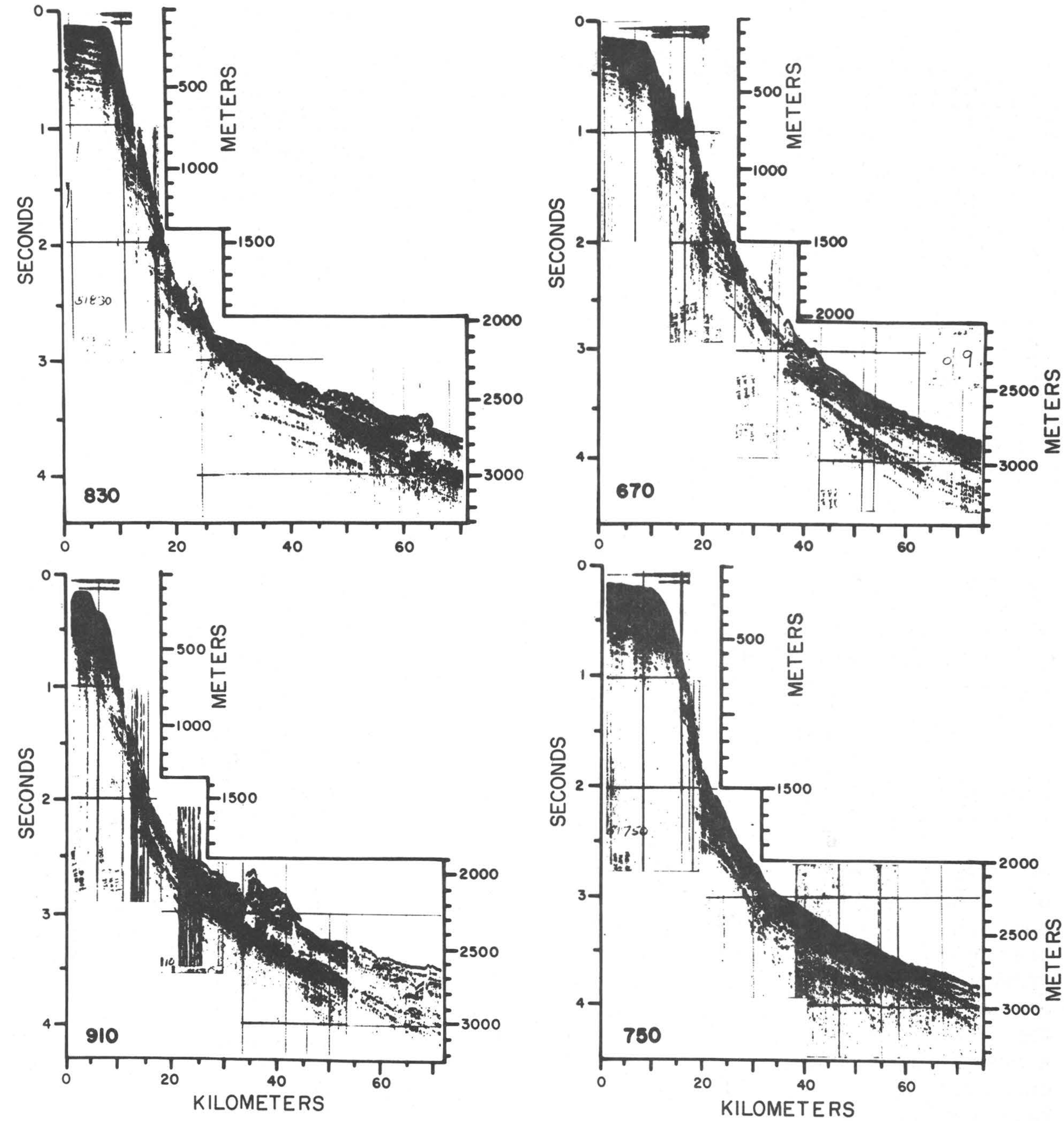

FIGURE 5.—Photographs of single-channel seismic reflection profiles oriented perpendicular to the margin. See figure 3 for profile location. Profile 910 is adjacent to Wilmington Canyon, and Profile 670 is adjacent to Lindenkohl Canyon. Vertical exaggeration = 28x.

summer, we hope to core near an area where this acoustically transparent material comes to the surface. On the 3.5-kHz profiles, there does not appear to be a sediment cover; however, I'm sure there is at least 10 ft [3 m] of olive gray silty clay. Profiles 275 and 255 show the continuous horizon that I believe we can trace all along the rise in this area and up on to the slope.

I constructed a generalized isopach map (fig. 9) by taking the values at each track-line crossing for the depth to the continuous horizon. Areas in gray have a sediment thickness greater than 200 m. The

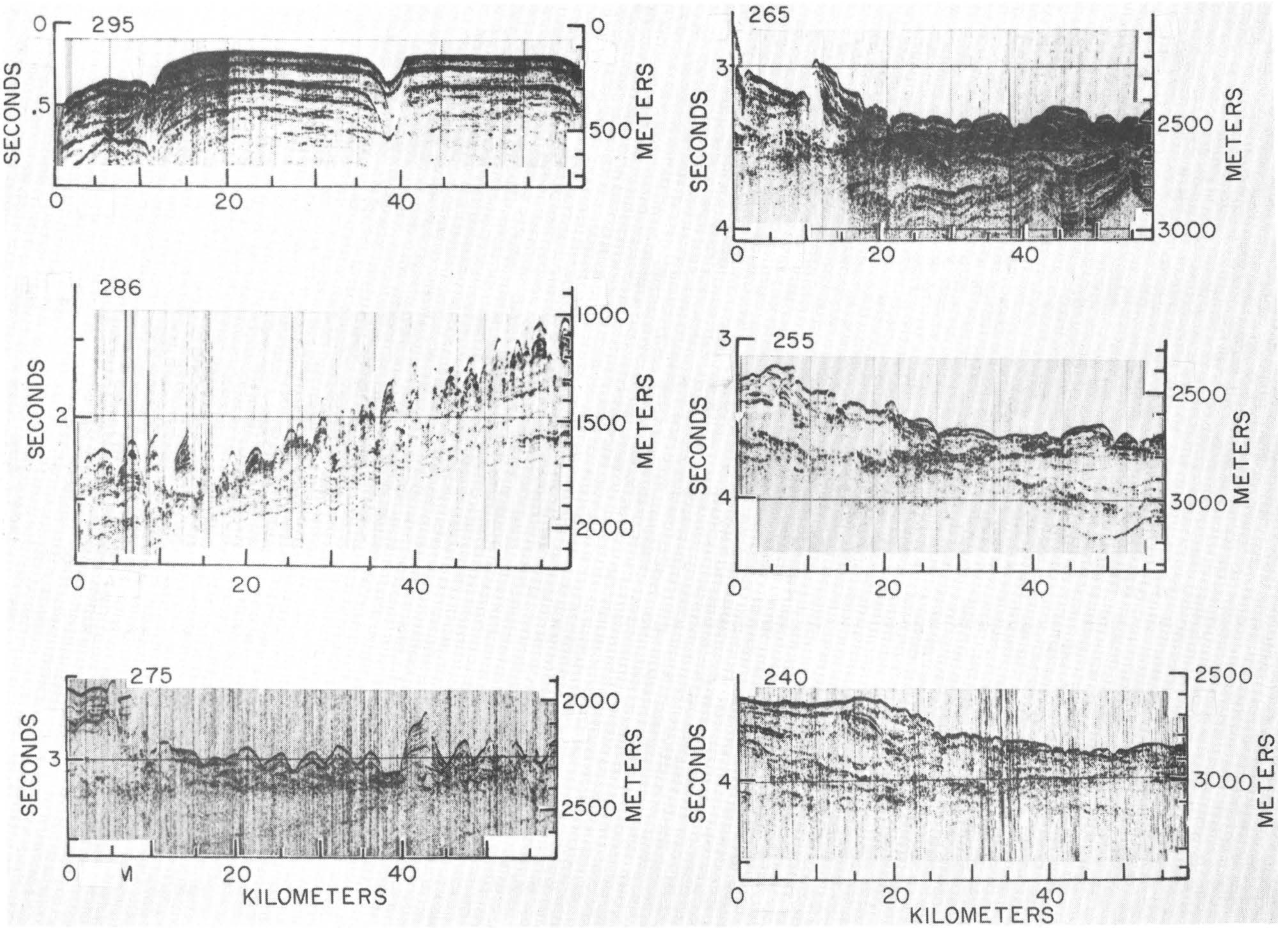

FIGURE 6.—Photographs of single-channel seismic reflection profiles oriented parallel to the shelf edge. Profile 295 is located near the Shelf edge, and Profile 240 is along the rise. See figure 3 for profile location. Vertical exaggeration = 28x.

dashed line is the location of the topographic axes of the canyons today. Spencer Canyon has developed two small levees on either side of it. Sediments are very thick south of the present axis of Wilmington Canyon; however, it has no paired levee on its north side (fig. 9). Also, on some of the profiles near the base of the slope, I can trace a buried valley that I believe represents the ancestral Wilmington [Canyon], which originally had a trend straight down the slope. The canyon channel was diverted by the block and forced to flow to the east. In summary, this area can be characterized as having a highly eroded and dissected slope. Pleistocene material lies above an unconformity on the slope, but the greatest thickness of material appears to be on the rise, which I believe is mainly Pleistocene and late Pliocene in age. In general, the reflecting horizons are not continuous with the upper slope above the unconformity. Below the unconformity, horizons can be traced from the slope to the rise. The fact that we see faulting within the block on the rise and not in the underlying horizons makes me believe that the block is a large slump and not a constructed levee-type feature.

We'll now look at the Baltimore Canyon area, to the south, where we've had outbuilding of the slope (fig. 2). Figure 10 is a bathymetric map contoured at a scale of 1:40,000, on the basis of a 1-km line spacing of strike lines and 5-km spacing of dip lines. The slope has prograded in a series of ridges. An erosional zone is present on the upper slope in the northeast portion of the survey (fig. 10).

We now will look at a series of dip lines from south (bottom left) to north (top right) (fig. 11). The northern profiles show somewhat steeper slopes. Profile B is down one of the ridges; I call your attention to the continuity of horizons that can be traced all the way down the slope to the base of the rise. The profile to the immediate south (fig.11)

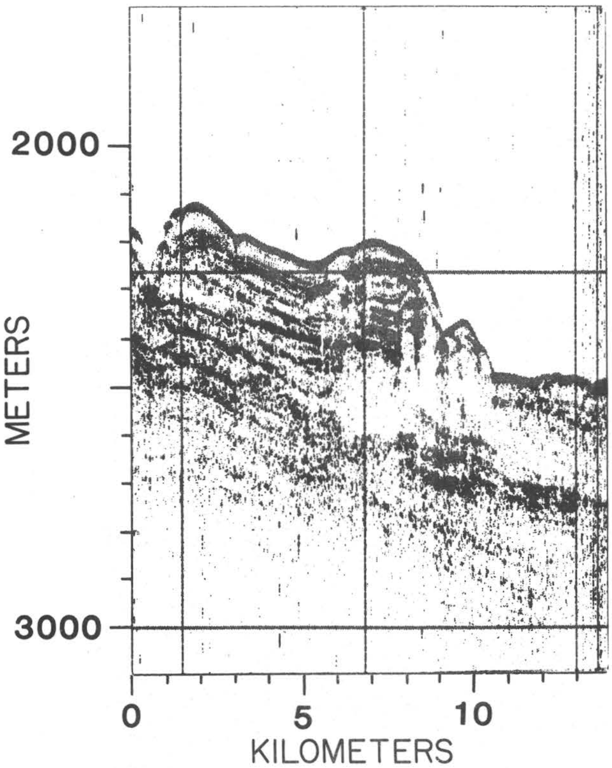

FIGURE 7.—Photograph of a portion of the ridge that flanks Wilmington Canyon on the rise. Acoustically transparent sediment appears to have intruded in diapiric-like fashion into the overlying stratified material. Vertical exaggeration = 14x.

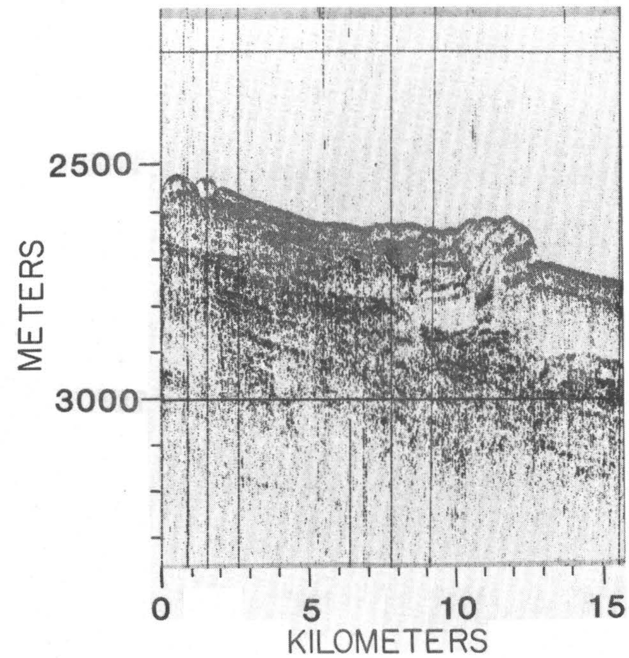

FIGURE 8.—Photograph of a portion of profile 830 (fig. 5) showing upturned beds on the rise. Vertical exaggeration = 16x.

is through one of the valleys and you can see similarly that the valleys are well stratified and the horizons are continuous down the slope. Looking at a strike profile (fig. 12), you can see that the ridges contain a series of disconformities that can be traced from one ridge to the next. I suggest that, in this particular area, the valleys have been migrating back and forth with time and the whole slope has been prograded seaward and built up. We see in this particular area no evidence of major slump blocks, although it is the area where Embley and Jacobi (1977) have described debris flows. I believe that some slumping of material off the sides of the ridges does occur, but there has not been massive block movement in this particular area.

Figure 12 shows a profile over one of the ridges. You can see some crinkling and distortion of the sediments that may be due to some sort of creep but, in general, the horizons are continuous. It would be very nice if we had an acoustic horizon that we could trace into this area. The unconformable horizon that is present at Wilmington Canyon is not found here. As soon as you cross over to the south side of Wilmington Canyon, it vanishes. Perhaps it may be buried too deeply to see with our high-resolution gear. More likely, in this particular area, we've had such an extensive sediment supply that the erosion surface never developed.

Now we will look at the last area (fig. 2), seaward of Albemarle Sound, where we thought we could see a combination of processes occurring. A bathymetric map, contoured at a scale of 1:40,000, was constructed with a 2-km track-line spacing on the strike lines and a 9-km spacing on the dip lines (fig. 13). A large, featureless, relatively smooth seaward-dipping area is present on the midslope. On five strike profiles extending all the way from Cape Cod to Cape Canaveral, Fla., this is the only location where a smooth and featureless slope occurred. A ridge transverse to the slope is also present and is similar in internal structure to the ridges near Baltimore Canyon. The smooth midslope region is flanked by a steep scarp on the west side, the landward side, as well as on the seaward side (figs. 13 and 14). It's an interesting question as to why there's no expression of valleys crossing this area, nor is there evidence of buried valleys in this area. In fact, there is a continuous horizon traceable through this area (fig. 14).

As we saw in Baltimore Canyon, the ridge here

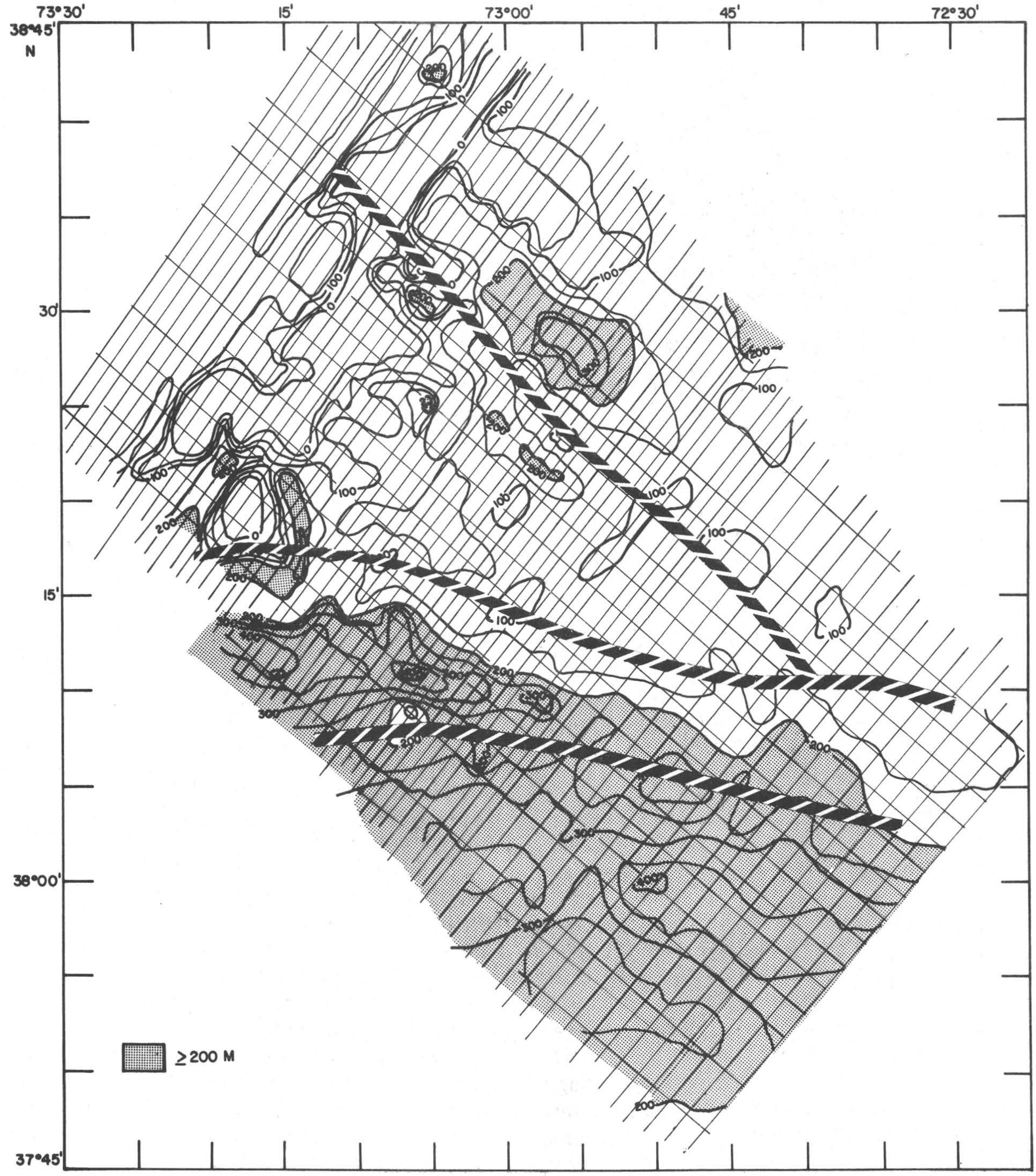

FIGURE 9.—Isopach map of sediment thickness above an interpreted Pliocene unconformity. Values were determined at each trackline intersection. Shaded areas have a sediment thickness 200 m or greater. Dashed lines depict the canyon axes of Spencer, Wilmington, and merged valleys south of Wilmington.

has internal stratification which is continuous in the downslope direction. It has been eroded on the west side by a valley which, over on the ridge, results in a feature that looks like a slump block on a single profile but, on the basis of the bathymetry, we can see that, indeed, it's not.

A dip profile over the flat portion of the slope (fig. 14) shows two steep seaward-facing scarps

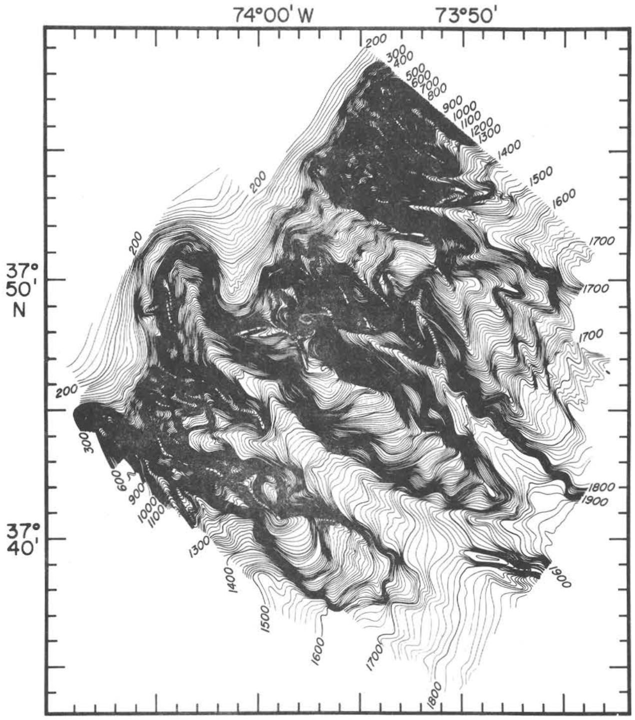

FIGURE 10.—Bathymetric map of the Continental Slope south of Baltimore Canyon. Contour interval is 10 m. See figure 2 for location of the area.

with a 12° gradient, whereas the flat region in between has a 4° gradient. As erosion proceeds, you can see that the slope is dissected and steeper to the northeast (fig. 13). Valleys dissect the upper scarp and lower scarp (fig. 13). Two valleys are present on the upper scarp and may have played a role in isolating a portion of the slope.

On strike and dip profiles (fig. 14), you can see that the horizons are continuous down and along the slope. They are also relatively flat and extend into the ridge shown on profile A (fig. 14). The morphology is peculiar, and the only way I can explain it is to suggest that a block of material has been removed from the slope in this area. The block would have been the same size as the feature we saw on the rise at Wilmington Canyon, so it isn't unrealistic to assume that such a large block was removed. That would account for the steep wall on the head of the scarp, also the valleys on the wall, but no transverse valleys cutting across the midslope region. We also have a core at the top of the seaward scarp, near the arrow marked "Slip Surface" on profile B (fig. 14). We believe that we have cored a horizon that we can trace on the 3.5 kHz over this midslope area. The recovery of lower Pleistocene sediments in this core implies that the upper part of the Pleistocene sequence has been removed. Unfortunately, we don't have any material dated in the adjacent ridge to see if we have any more of the Pleistocene sequence. Ideally, that's what one should do. It also may be that another block was removed from the lower slope. We have a very steep scarp developed on the lower slope, which valleys are now dissecting. The problem with the model, that is, that the morphology resulted from slumping, is that we don't see the block. The picture is incomplete if you don't see where the material went.

I was going to say also that Al Uchupi has some pertinent information in the published literature, although I'm not sure he'd agree that a slump block is present seaward of this area after his discussion this morning; but certainly, the literature suggests that the material may be seaward of this area (Embley and Jacobi, 1977; Emery and others, 1970).

The points I'd like to make from these data sets are that the morphology on the U.S. east coast margin is extremely different from area to area; therefore, you can't use one particular location to characterize the whole slope. The Continental Slope in the Wilmington Canyon area certainly appeared erosional with the extensive dissection by valleys; the slope near Baltimore Canyon appeared [to be] depositional with evidence of progradation, and the area seaward of Albemarle Sound looks as if it might have been intermediate between the two. Because of the focus of this conference, I think we can say that during Pleistocene low stands of sea level we had sediments deposited directly onto the slope. These sediments were subsequently modified by valley dissection, some of it very striking, that removed the material from the slope and transported it out onto the rise. There also are numerous buried valleys at the shelf break along the Baltimore Canyon Trough region that can be correlated with present day topographic valleys on the slope. I hope I have presented evidence that suggests that we have had, in at least a couple of areas, large block sediment failure with material deposited out on the rise. This is not to say the whole slope has extensively undergone large block sediment failure, because I don't believe it has. But I believe it occurred at Wilmington Canyon and possibly on the slope seaward of Albemarle Sound.

The problem on the east coast is to identify the mechanism or mechanisms that trigger these large

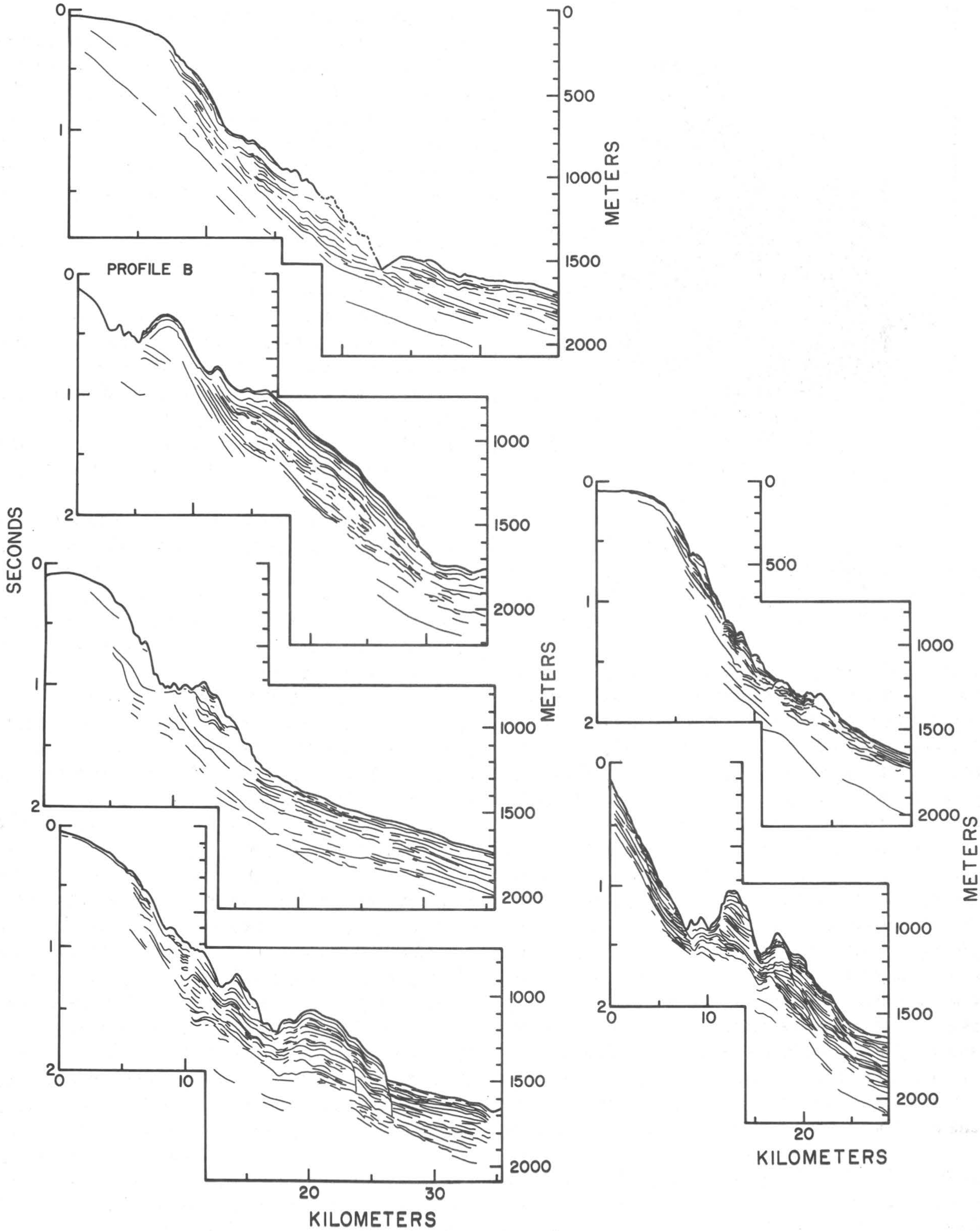

FIGURE 11.—Line drawings of seismic reflection profiles oriented perpendicular to the margin in the study area south of Baltimore Canyon. In general, the slope has prograded seaward. Vertical exaggeration = 12x to 18x. For location of profile B see fig. 2. Other profiles are spaced about 20 km apart adjacent to profile B.

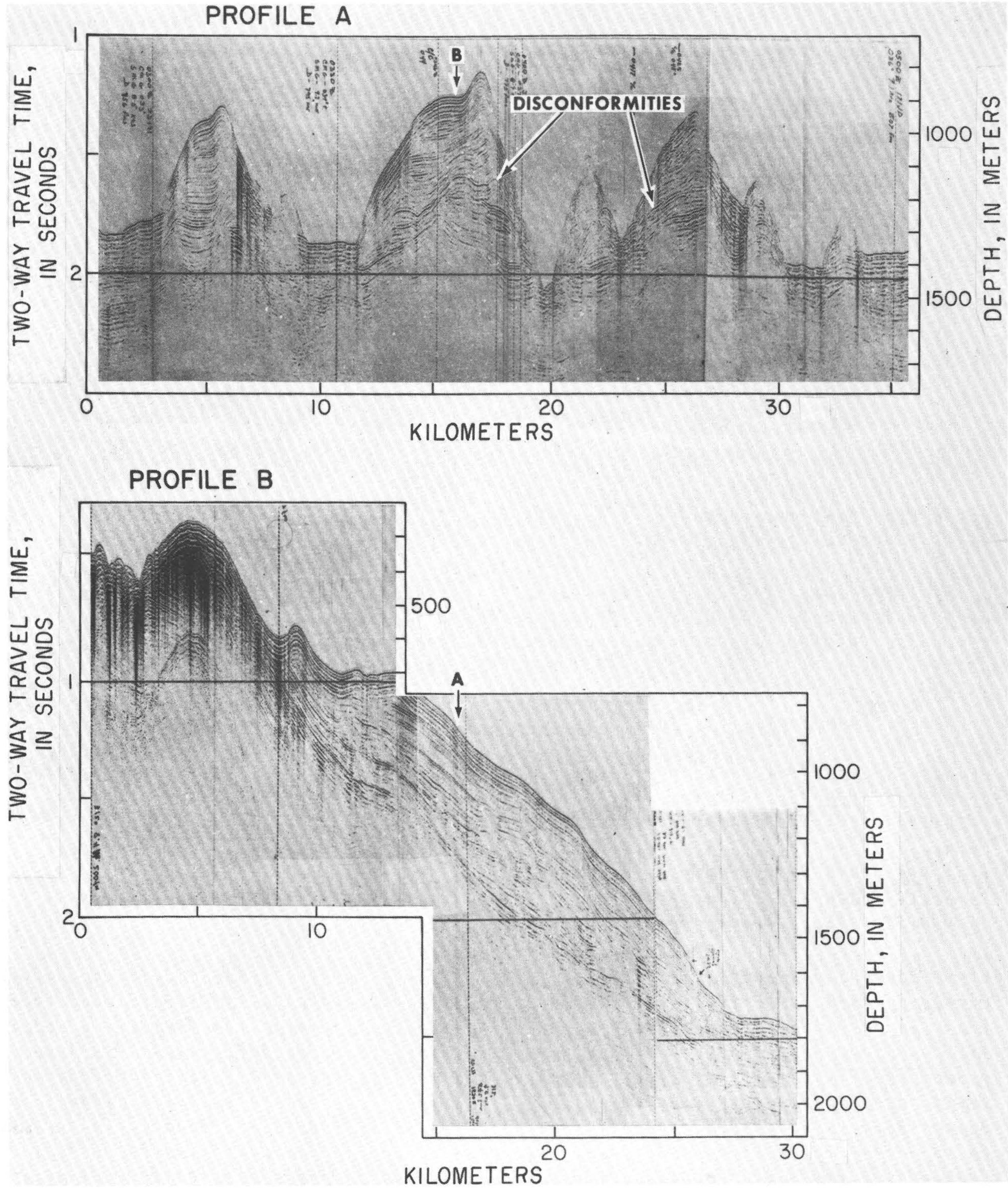

FIGURE 12.—Photographs of original seismic reflection profile data from Baltimore Canyon. Ridges have numerous disconformities (A) and the slope has built seaward (B). See figure 2 for profile location. Vertical exaggeration = 14x. Arrows noted with A and B are where the two profiles cross.

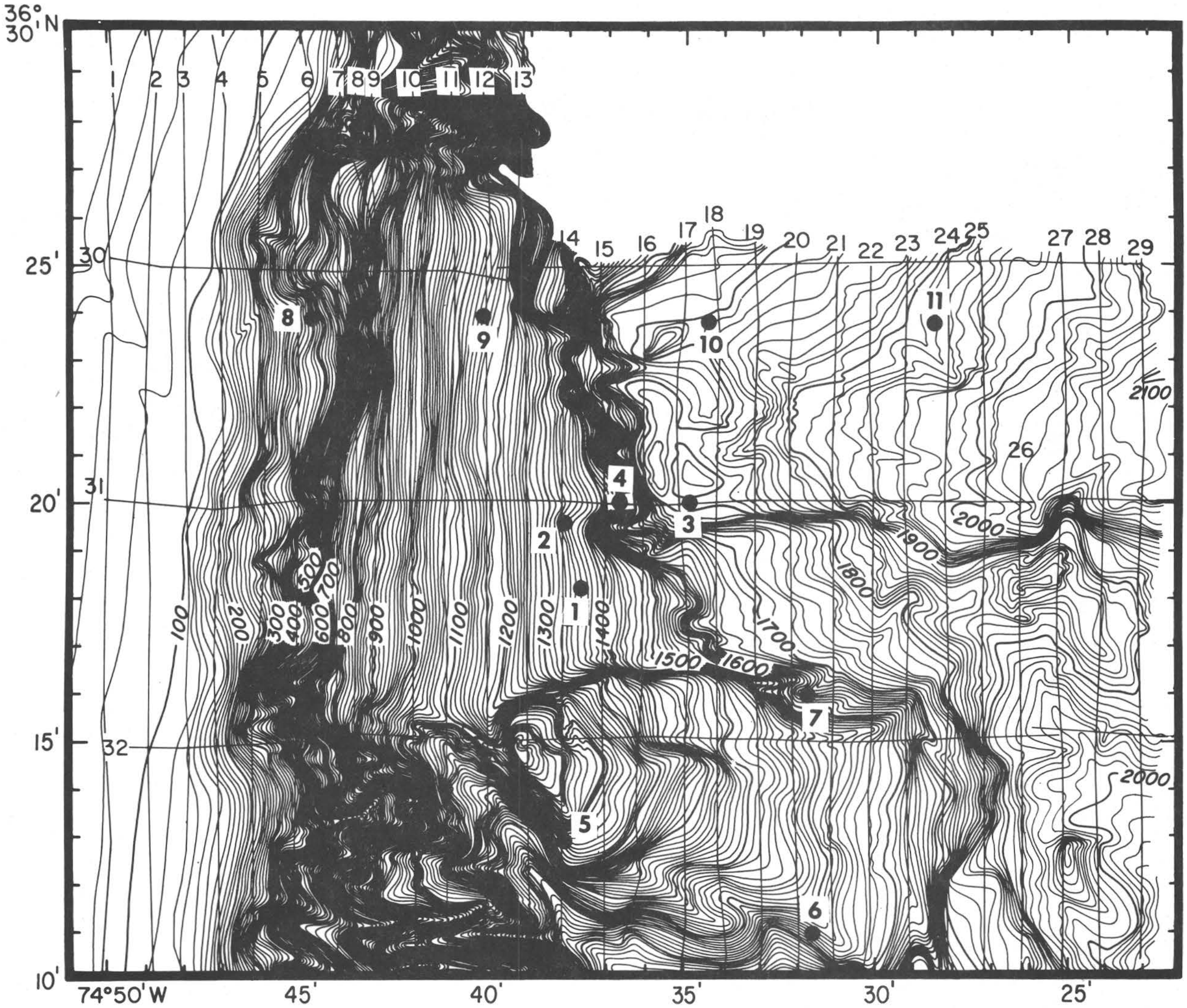

FIGURE 13.—Bathymetric map of the Continental Slope seaward of Albemarle Sound. See figure 2 for location of the study area. Contour interval is 10 m. Numbered dots show core locations. Core data help define the location of the slip surface shown in fig. 14.

block failures. Rapid deposition of sediments directly on the upper slope during lowered sea level certainly might contribute to sediment failure. Of course, during lowered sea level, surface gravity waves as well as internal waves impinged directly onto the shelf break and the upper slope. Maybe this was a sufficient mechanism to cause these blocks to fail during the Pleistocene. To understand these mechanisms on the east coast slope, I think it's very important that we also look at the rise. The rise is the repository of all the material that has come from the slope, and, as we've learned here, if you don't see where the deposits go, you're not sure of what has happened. My final pitch is that we'd love to have some drilled cores from the upper rise. As the three small area surveys of Dave Twichell showed, it is essential that we obtain detailed bathymetry out here. We are working at 900-m line spacing, which is what Jim Coleman used for his basic geologic background information. That's the best we feel we can do with the navigation constraints we presently have. I'm not sure that using this line spacing is going to be sufficient. We're going to have to go to something more detailed to really understand what we're looking at out on the Continental Slope and Rise. Thank you.

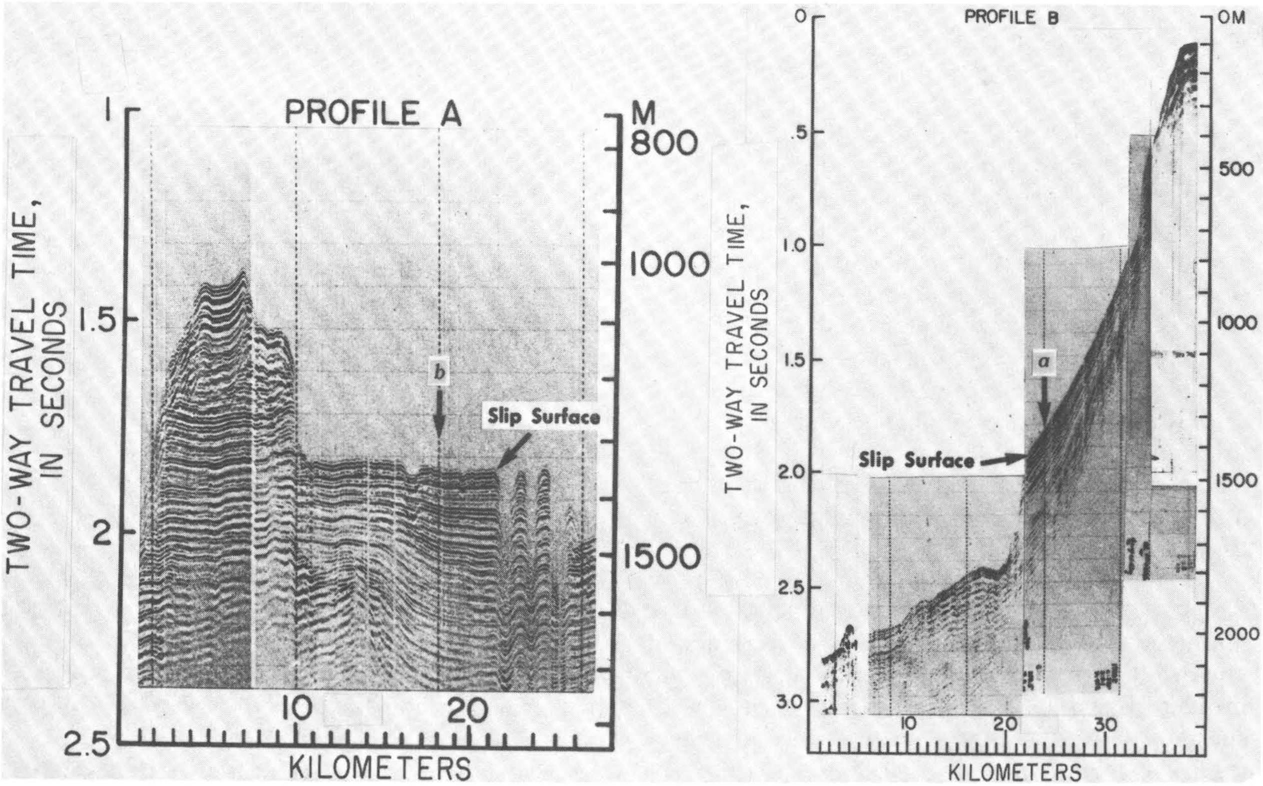

FIGURE 14.—Photographs of original seismic reflection profile data in Albemarle Sound area. See figure 2 for profile location. Vertical exaggeration = 30x. Arrows labeled with *a* and *b* are locations where the profiles cross.

Discussion after McGregor's talk:

Wallace: Do you have any figures on the total ship miles of data you collected?

McGregor: Well, the first survey at Wilmington Canyon was 3,000 nautical miles. The others are probably of the order of 1,500 or 2,000 apiece, so I'd say around 7,000 nautical miles in those detailed surveys alone.

Ross: Sea Beam data would be useful in that region.

McGregor: That's right. That's the problem we've been talking about. Sea Beam data would be a great improvement over the NOAA narrow beam echo sounder. With it, I would have much greater confidence that the geometry of the valleys we are seeing is correct.

Coleman: Could we take a second and enlarge on your comment about valley dissection?

McGregor: I think Jim Robb may have some data that bears on this as well. Unfortunately, the vertical exaggeration on a lot of our profiles was such that you don't have good discrimination on the slope portion which is obviously the critical part. But, as we discussed yesterday, I think what's happening out there is that, during a low stand of sea level, for instance, sediments accumulate both on the ridges and in the valleys in a uniform blanket. But, because there are differences in slope, the material tends to move downslope on the walls into the valleys and then is flushed out.

Coleman: Let's start right there. What do you mean by flushed out?

McGregor: Moved down those valleys by some mechanism. What it is, I don't know. Turbidity currents is what comes to mind although, in some of these valley floors, you can see blocks. In Wilmington Canyon, of course, the canyons are a little different from the valleys. There's a beautiful graded sequence out on the rise that's roughly about 42 cm thick that goes from gravel up to fine sand in the axis; that was obviously turbidity flow. There needs to be a lot more work done on the upper rise, coring in these valleys to see what you find. One of the problems, when you look at a slope map of this area, is that the valleys have the lowest gradient of anywhere on the sea floor. They are

1° and sometimes less. So, when I say everything is being flushed down these areas where the slope is lowest, it's sort of strange. Really, what we're saying is, "Maybe the material moves down these ridges."

Robb: In my area, we were looking for sediment moving down the valleys. The valley floors, the best we can tell from the data we have, seem to be fairly clean in most cases. There are a few blocks here and there, but it's basically clean valley floor.

McGregor: The problem is that when you're coring them, all you get back is gray silt and clay.

Robb: Either that or fluid—a couple of feet of fluid muck.

Sangrey: Bonnie, if we're talking about the slope in the axis of the main valley, what's the slope in the axis of the gullies?

McGregor: At Baltimore Canyon, it was 4°-5°, or somewhat steeper down the ridges. I think you really have to do a slope map to see what gradient you're looking at, because vertical exaggeration on profiles makes the slopes look like precipitous scarps. It turns out that the walls of the valleys can have slopes greater than 25° into those 1° slope valleys. The main axes of the ridges may have slopes on the order of 5° or 7°; they're greater than the valleys, but less than those on the valley walls.

Emery: Doesn't that mean, automatically, that the debris should not be going down the ridges because it would soon be diverted to the side by even steeper slopes?

McGregor: That's where Dave Twichell finds gullies, which is what you would expect.

Menard: Maybe it tells you that you've got thick turbidites or thick turbidity current flows that are going to flow because it isn't the bed slope that matters; it's the hydraulic slope.

McGregor: That's right.

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Detailed Bathymetry and Seismic Stratigraphy of the Atlantic Continental Margin Between Hudson and Baltimore Canyons

James M. Robb

This project started out a couple of years ago in response to the uncertainty about slope stability. We decided to map one area at large scale as a model. We have found a lot fewer slumps than we thought we would. The area that we're working in includes Hudson Canyon, Wilmington Canyon, and Baltimore Canyon (fig. 1). Bonnie McGregor's area is to the south. Our data, 2,250 km of track line, were collected in 1978 and 1979 aboard R/V *Gillis* and R/V *Iselin*. To acquire the profiles, we used 40-in^3 airguns with wave shaper, minisparker, and a 3.5-kHz echosounder. Here is one cross section (fig. 2), in an area where six wells have been drilled that gave us good stratigraphic information. This section (fig. 2) is pretty close to Atlantic margin coring (AMCOR) hole 6021 and the B-3 well. The surficial part of the slope is covered with Pleistocene sediments and, possibly, some Pliocene. Wylie Poag has found Pliocene in the B-3 well cuttings. Shown here is the Miocene, Oligocene, Eocene, and Cretaceous. The Cretaceous does not crop out on the Continental Slope. The Eocene dives below the onlapping Pleistocene sediments on the rise.

We have done the detailed bathymetry in the area from our data (fig. 3). It can be directly

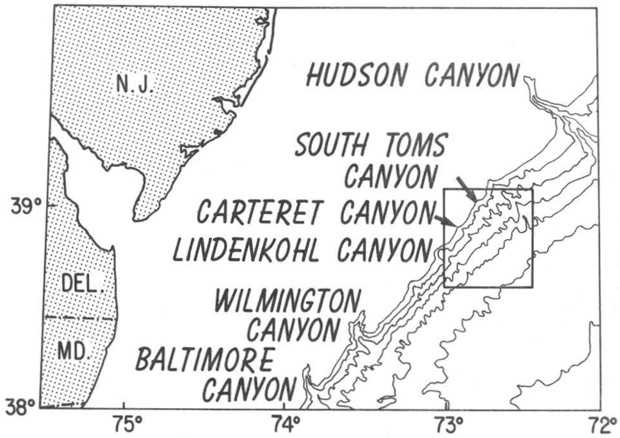

FIGURE 1.—Index map showing location of study area and names of major submarine canyons on Continental Slope.

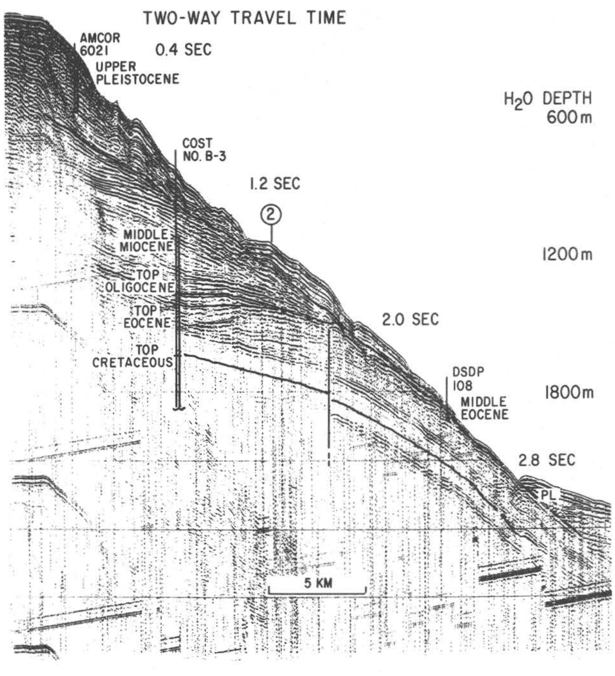

FIGURE 2.—Single-channel seismic-reflection profile across Continental Slope. This profile is located between Berkeley and Carteret Canyons and shows Pleistocene sediments overlying truncated, slightly seaward-dipping Tertiary strata. Because this is a time section, it is slightly distorted due to variations in sound speed through water and sediments and is vertically exaggerated about 11 times.

compared with maps from the old, 1937, data of Veatch and Smith (1939), which are still used. We have spent some time making this bathymetric map, and this talk could become a pitch to encourage acquisition of good bathymetry along the whole coast. We feel that the National Ocean Survey (NOS) bathymetry is really too general at 1:250,000 scale to be useful. Accurate, detailed bathymetry is really necessary for profile interpretation of surficial features.

The geologic map (fig. 4) was constructed by using a fair amount of stratigraphic control. We have three Atlantic Slope Project (ASP) wells, AMCOR hole 6021, the B-3, and two DSDP holes. The solid circles locate USGS piston cores. They penetrate only Pleistocene. It turns out that the Pleistocene in these piston cores contain either upper slope or, in some cases, shelf *Foraminifera*. The sediment can be termed hemipelagic; however, it is difficult to draw the line on profiles such as ours between turbidite and hemipelagic sediment. The geologic map shows the Pleistocene and outcrops of the Miocene, Oligocene, and Eocene strata. The Pleistocene is 450 m thick at the top of the slope and comes downslope in fingers or in ridges between canyons.

Figure 5 shows a midslope profile; you can see how we've traced the pre-Pleistocene unconformity. The ridges in between canyons are really quite complex and have a varied geologic history, as you can see. A problem with our data, and with any data taken from the surface at these water depths, is that we're dealing with diffractions in these submarine canyons, and we really can't see valley floors. Figure 6 shows a deeply towed hydrophone profile. Note the flat floor in this valley on the middle slope. Deeply towed profiles like this also show that some areas that look chaotic in our surface-towed airgun records are really quite evenly bedded and that surface chaos in some places is mainly a topographic effect from a combination of localized deposition and erosion.

In addition to the geologic outcrops on the map, we mapped areas of truncation in the valleys. Pleistocene strata, shown on the map, do not show much truncation on the middle and lower slope. They are either draped or conformably bedded. We mapped buried valleys or recut valley fills that exist along the sides of submarine canyons on the upper slope (fig. 7). We find these on the left side (looking downcanyon) in every major canyon in this area. Our objective is to evaluate the area that is unstable or slumped, and often, when we thought we had a slump on the side of the canyon, we actually were looking at a buried canyon.

Along the bottom of the slope (fig. 8), we have the levees or pieces of the ridge. These are fingers of Pleistocene material coming down the slope on top of an eroded surface. And, on the Continental

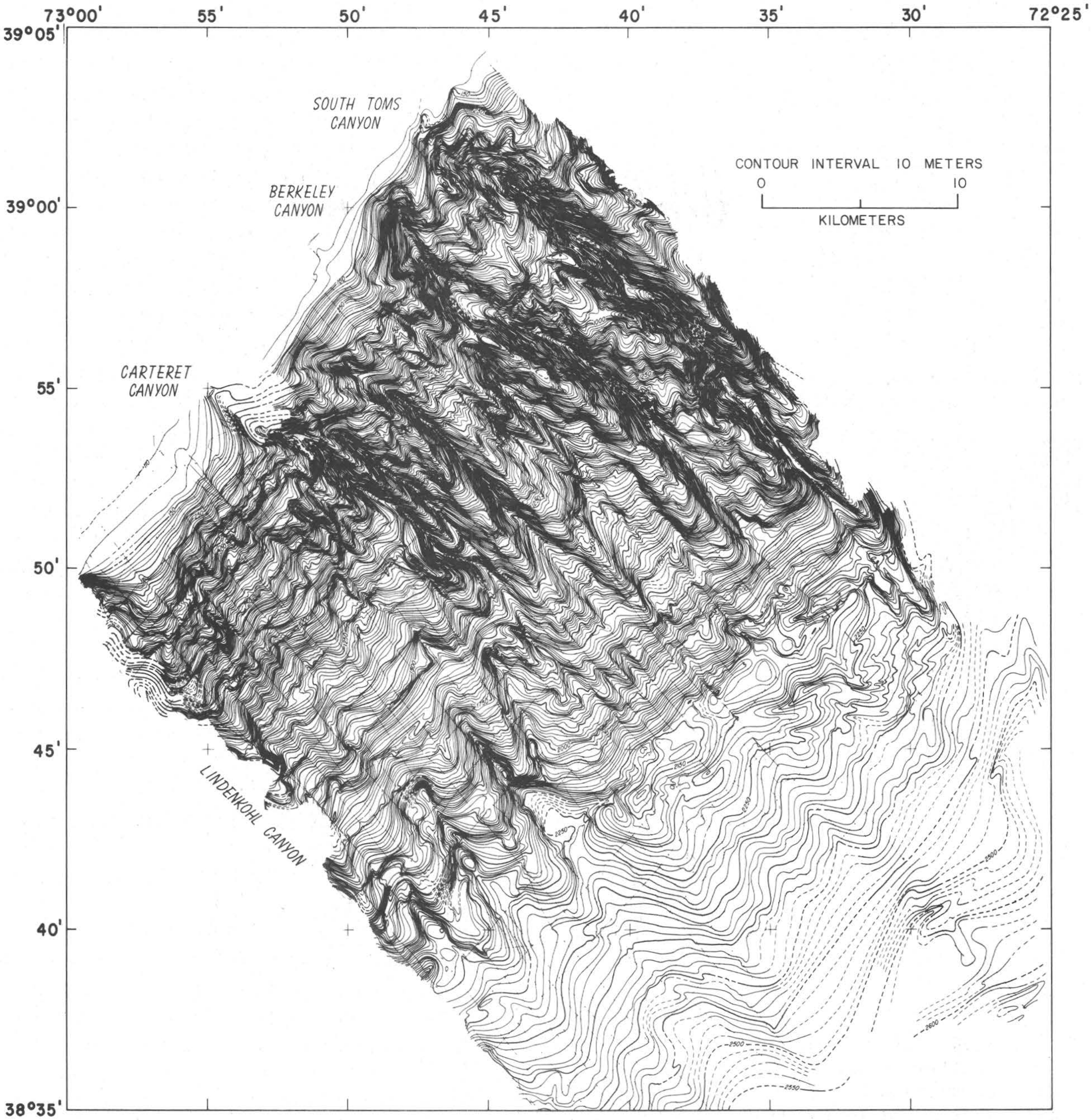

FIGURE 3.—Bathymetric map of Continental Slope study area. The contour interval is 10 meters.

Rise, here is another strike line that crosses some of the accumulated material (fig. 9). You can see an unconformity here. There are structures that do not have the appearance of gravity-flow structures, but rather, that of cut and fill on the Eocene surface.

We have mapped the thickness of Pleistocene sediments (fig. 10), and we have mapped the base of the Pleistocene. Figure 11 is the pre-Pleistocene unconformity. In general, we feel comfortable with these interpretations; where they are dotted, we didn't have any tie lines that we could make much sense of. Note that the Pleistocene does lie on a previously eroded surface.

In conclusion, I want to give credit to Jack Hampson and Jack Kirby who are responsible for much of the work that I have presented.

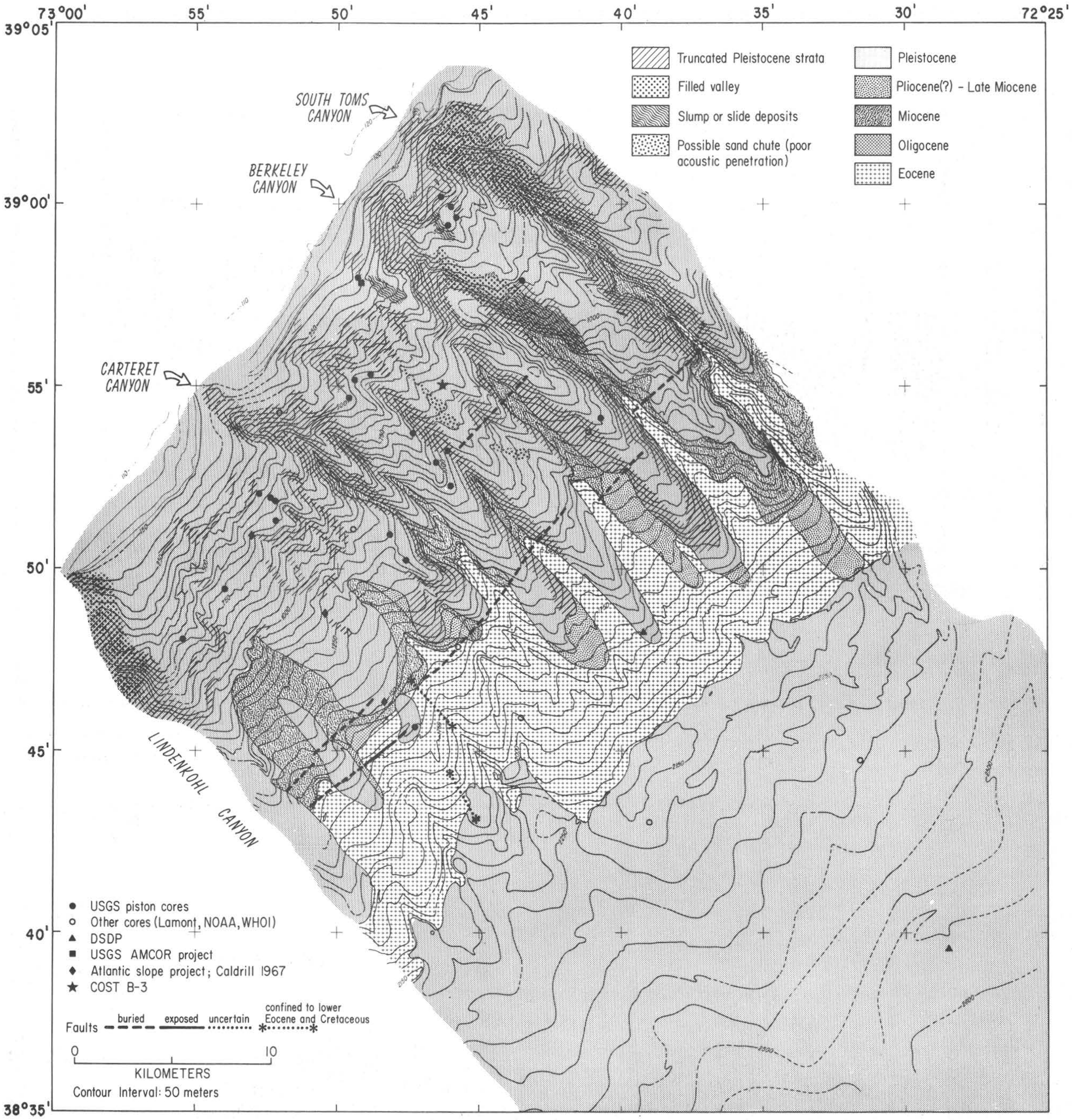

FIGURE 4.—Geologic map of Continental Slope study area between Lindenkohl and South Toms Canyons.

Discussions following Robb's talk:

Coleman: Jim, do you have any idea what the thickness of the Pleistocene is there back on the shelf area?

Robb: I think that when we estimated 450 m, it was based on the 6021 well that went all the way through Pleistocene, extending stratigraphically the section in ASP 13 which is near the top of the slope. We feel that 450 m is as thick as it gets.

Coleman: It doesn't thicken back up?

Robb: It doesn't thicken back up; it thins. We have also been trying to discover where the sediment might have come from and how it got there,

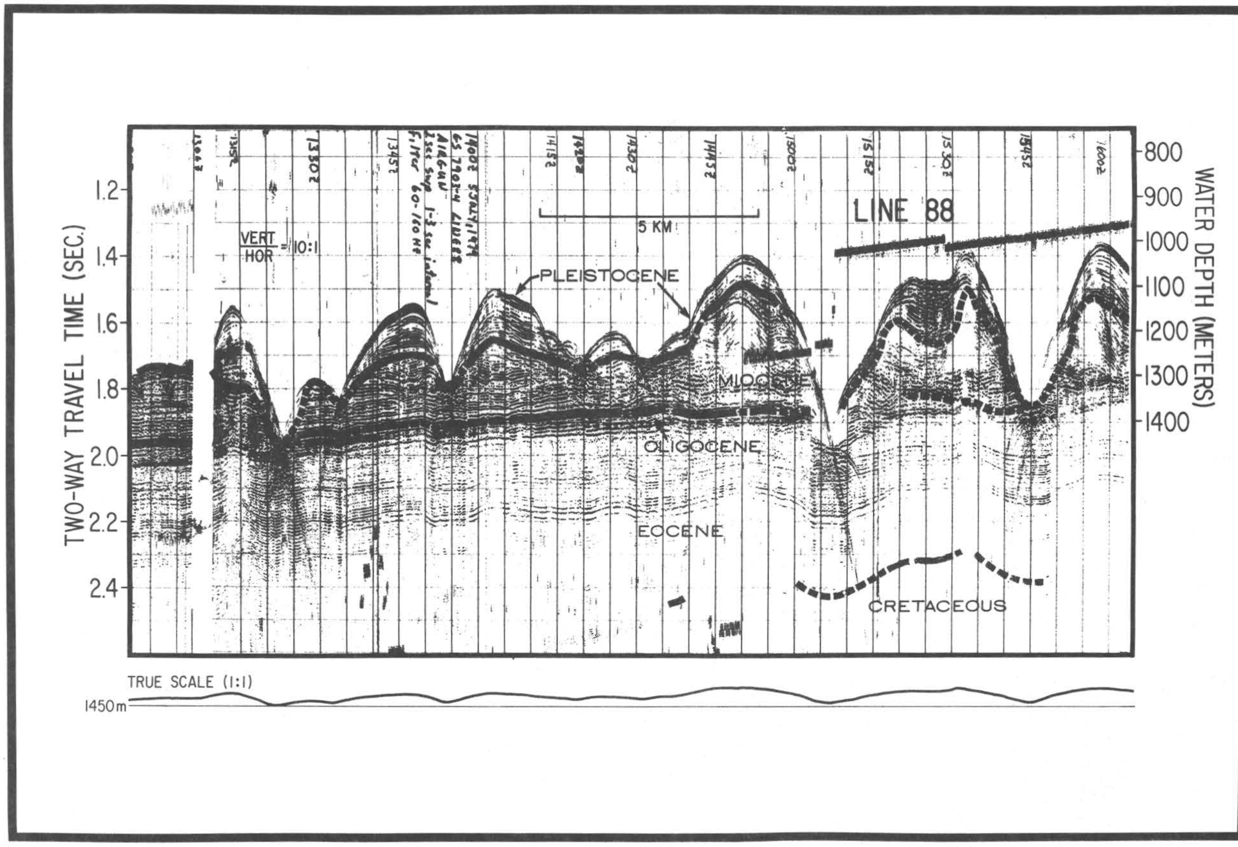

FIGURE 5.—Interpreted single-channel seismic-reflection profile along the Continental Slope at about 1,200-m depth showing the constructional and erosional nature of the intercanyon lobes covered with Pleistocene sediment, and the downslope-trending canyons and valleys.

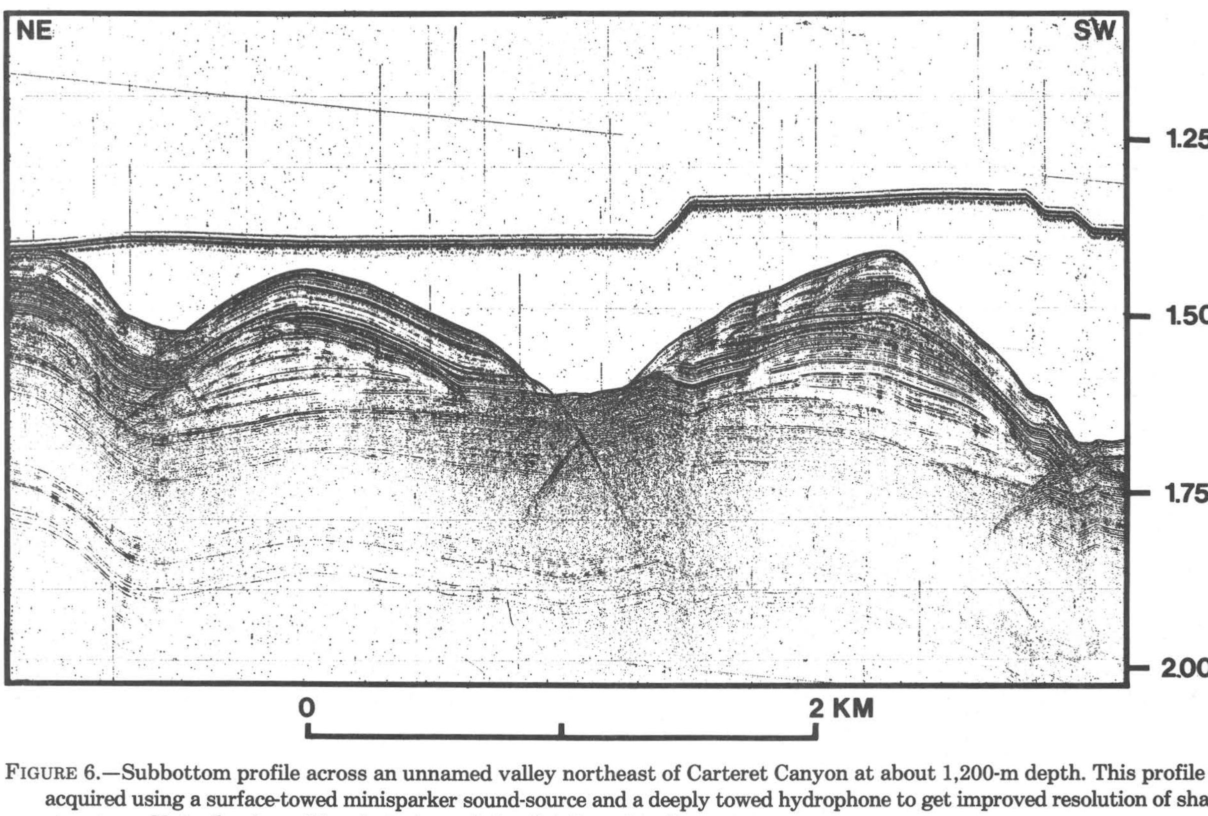

FIGURE 6.—Subbottom profile across an unnamed valley northeast of Carteret Canyon at about 1,200-m depth. This profile was acquired using a surface-towed minisparker sound-source and a deeply towed hydrophone to get improved resolution of shallow structure. Note the depositional strata and the flat-floored valley.

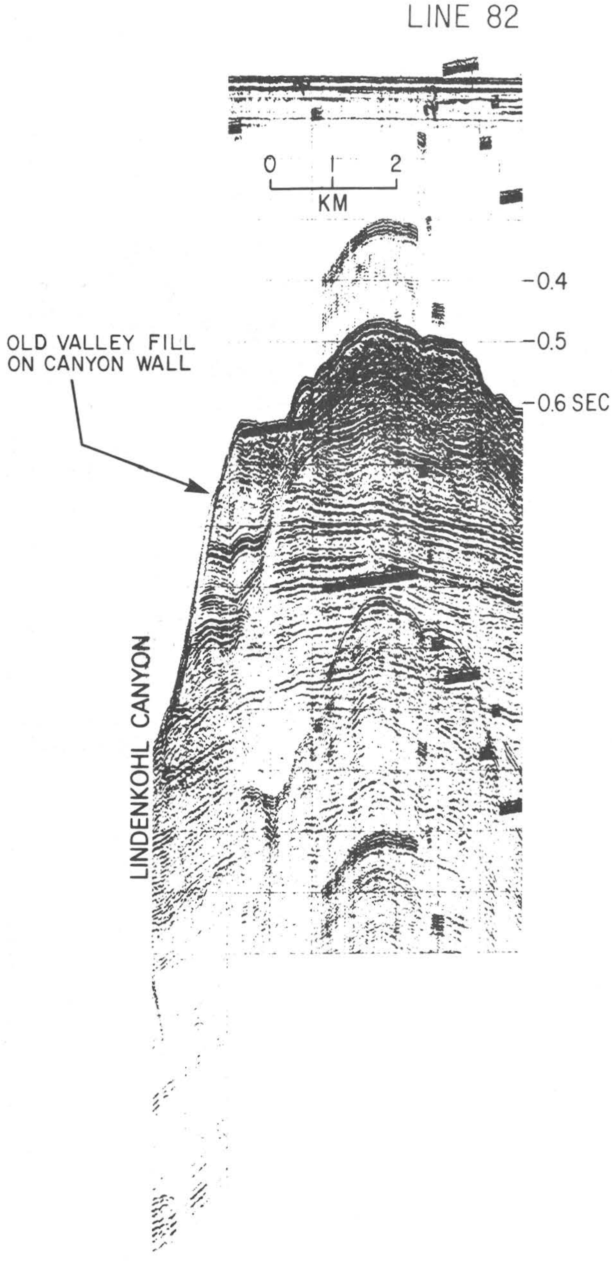

FIGURE 7.—Seismic-reflection profile across northeast wall of Lindenkohl Canyon at about 450-m depth. Note that the canyon wall is eroded into an older, filled canyon.

but we have not. We have a fellow from the University of Rhode Island who's working on a large amount of CD [Conservation Division, USGS] data looking for just this. He's not having much luck. I can't see anything in the data either, so it may be a dead end.

Schlee: What we see on CDP records is a wedge of sediment that's thickened right at the very shelf edge to about one-half of a kilometer, which isn't far from your 450 m; it then thins as you go shoreward and it's internally prograded. It thins as you get into the upper part of the slope.

Robb: Speaking from the applied point of view, it seems as though the slumping danger is limited to the Pleistocene sediments and that it is limited to areas of steep slopes on the sides of canyons. I think that there is an awful lot of interesting science concerning these levees. I don't know if you want to call them levees because they're on a slope; most levees are on the rise or on fans at much lower gradients. The material is very fine and Bill Normark points out that fans off the West Coast that consist of fine sediments have the greatest possibilities of having levees around the distributary channels. I don't know quite what to make of that yet, but we're still at work trying to find out these things.

Prell: Would you comment on the pre-Pleistocene unconformity that you're doing this isopaching on top of. What is the nature of the unconformity?

Robb: All we say really is that Pleistocene materials overlie Miocene, and, more recently, Wylie Poag tells me that some Pliocene is present. So, one presumes that it's a late Pliocene unconformity, and, since that ties in so nicely with Bonnie's inference from her work down south, I'll stick with that analysis.

Edgar: Dave Twichell was showing on the GLORIA records what he thought was outcropping Pleistocene. Is that in this area?

Robb: Yes, that's in this area. Now, this summer we're going out there. In fact, the ship is out there right at this moment, I hope, taking more seismic records in this area to expand our coverage and to fill in some lines. On August 26th, we're leaving on the *Gyre* again to conduct a mid-range sidescan sonar survey with 5-km track width. We intend to start tying together some of the finer scale details which we can't map with our half-mile line spacing. Our line spacing, I don't know if I mentioned it, is 900 by 1,700 m.

Folger: Incidentally, the reason that Bill Ryan is not here is that he's out using that midrange sidescan sonar to look for the *Titanic*. We're going to put it to more scientific purposes.

Robb: He reports that the device is working very well. He said there are a lot of targets the same size as the *Titanic*. They mapped 1,000 km² in 5 days. They see clearly beveled outcrops and can trace individual horizons in their outcrops with this device. They are seeing a lot of interesting geology, but, at that time, they hadn't found their target yet.

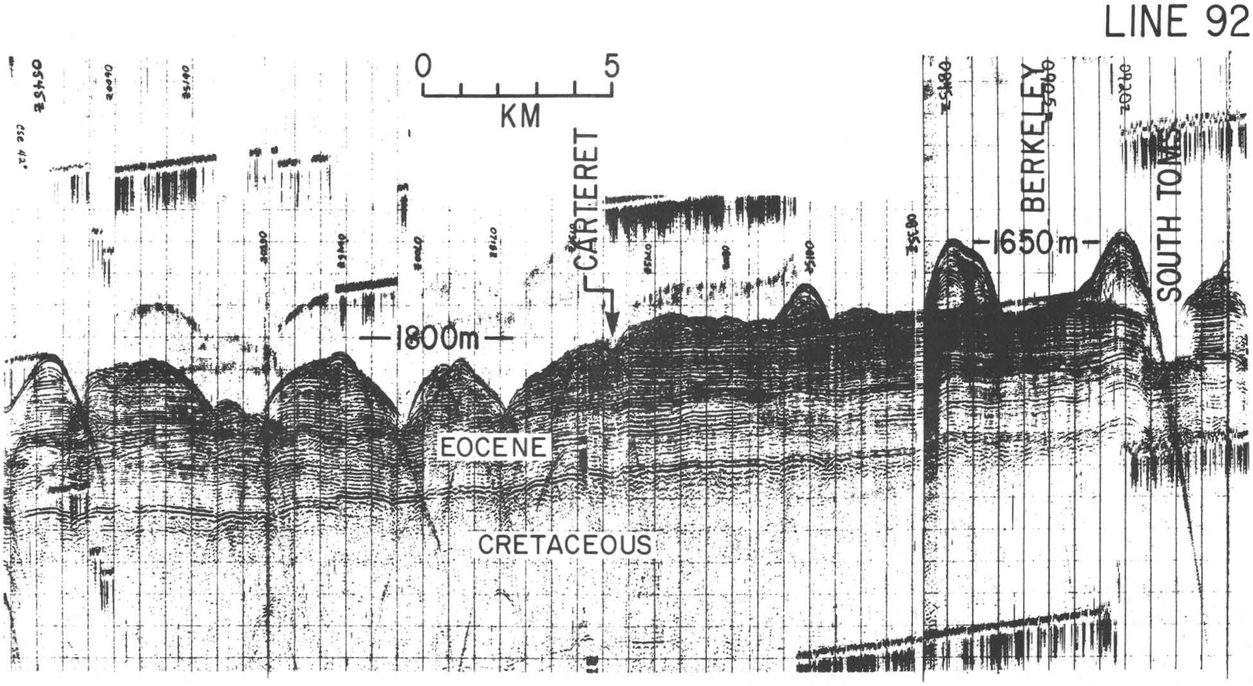

FIGURE 8.—Seismic-reflection profile across lower slope near 1,800-m water depth. Note the downslope ends of intercanyon lobes near Berkeley and South Toms Canyons. These are Pleistocene sediments unconformably deposited over Eocene rocks. Carteret Canyon and the valleys to the southwest (left) are cut directly into the Eocene rocks.

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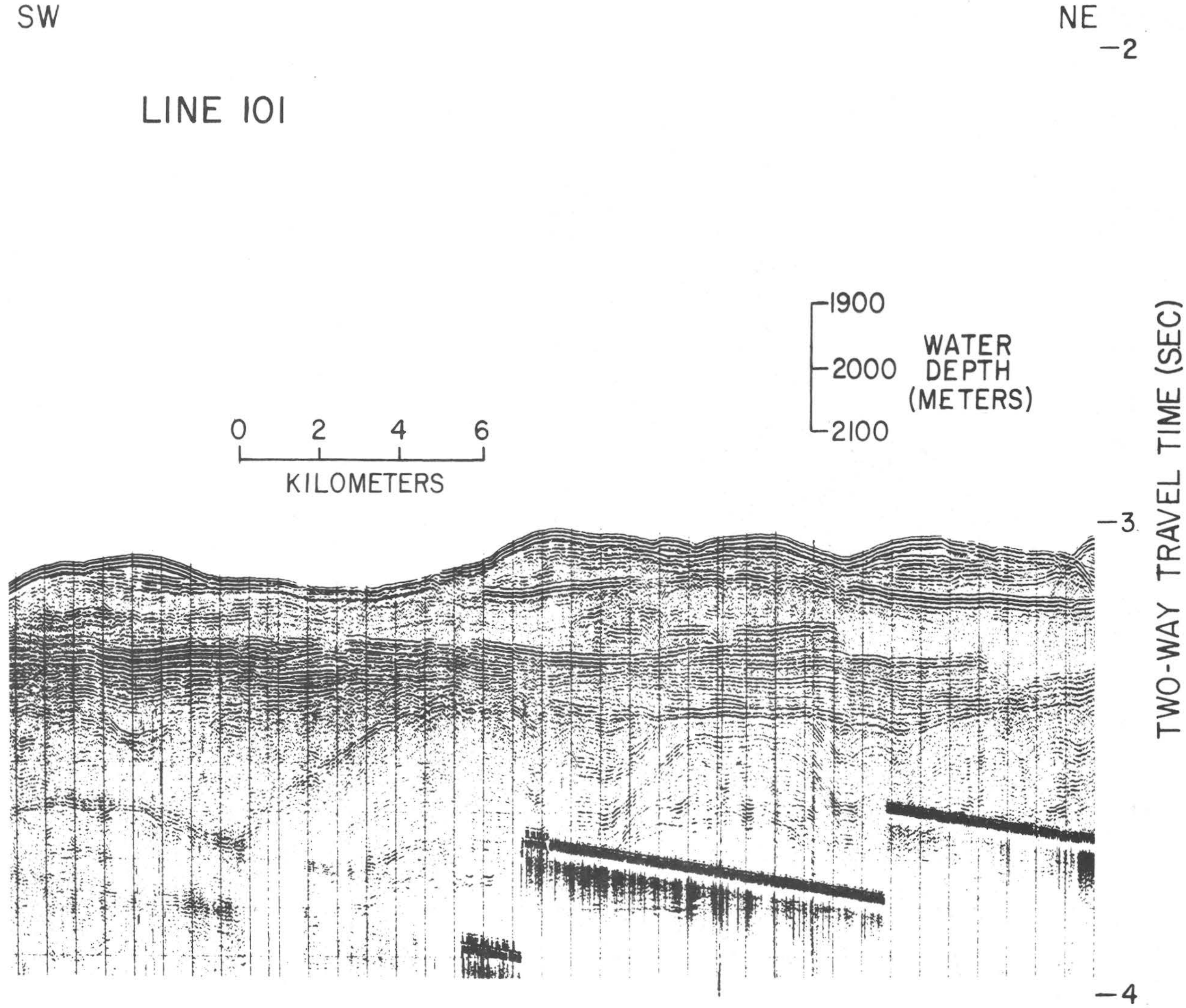

FIGURE 9.—Seismic-reflection profile along upper rise near the base of the Continental Slope. Note low-relief surface with smooth but discontinuous subbottom reflectors. Lower part of profile shows filled valleys cut into Eocene rocks.

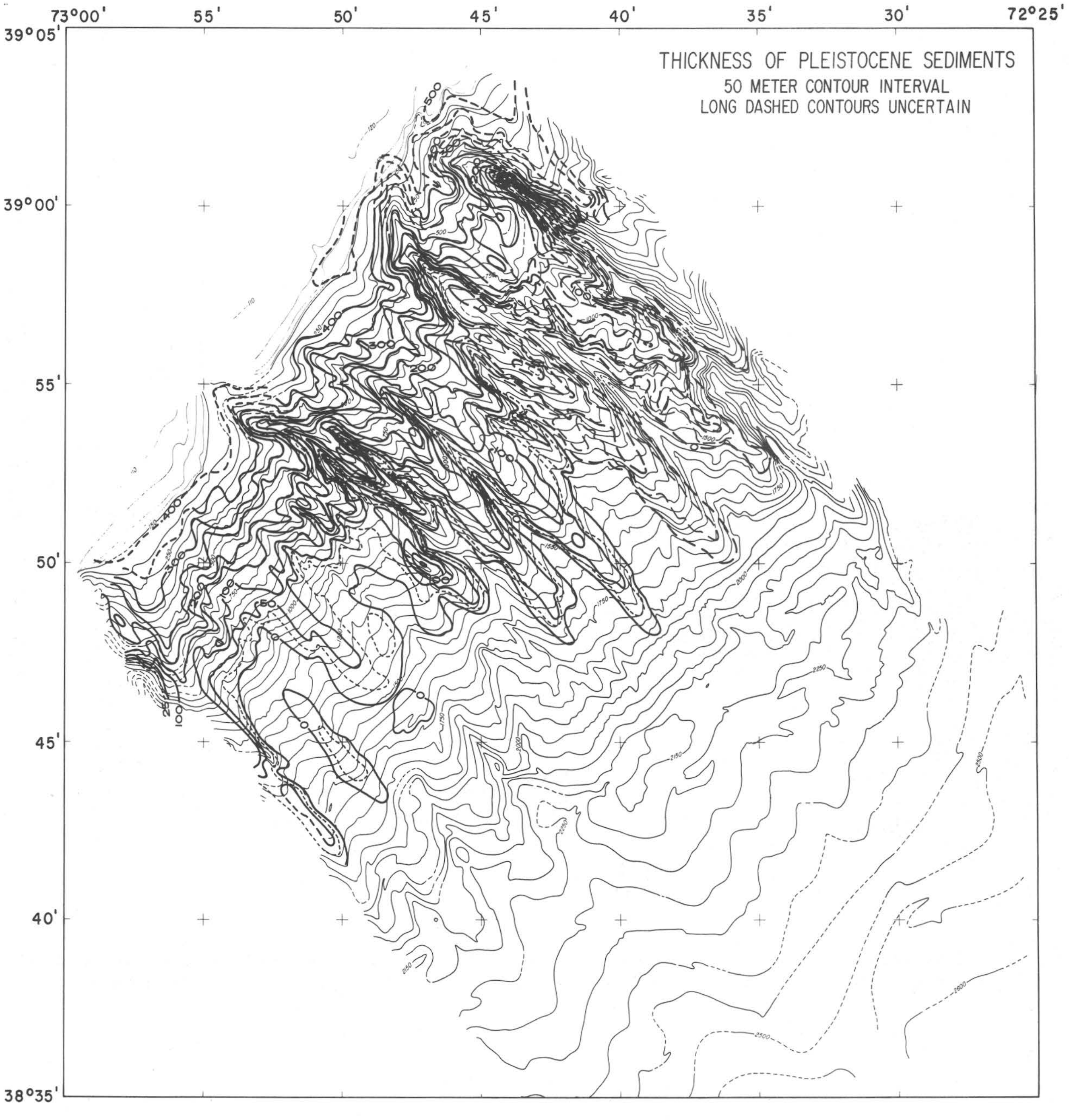

FIGURE 10.—Map showing the thickness of Pleistocene sediments on the Continental Slope. Thicknesses of Pleistocene and Holocene sediments to about 2 or 3 meters are not mapped because they could not be identified within the resolution of our seismic-reflection date.

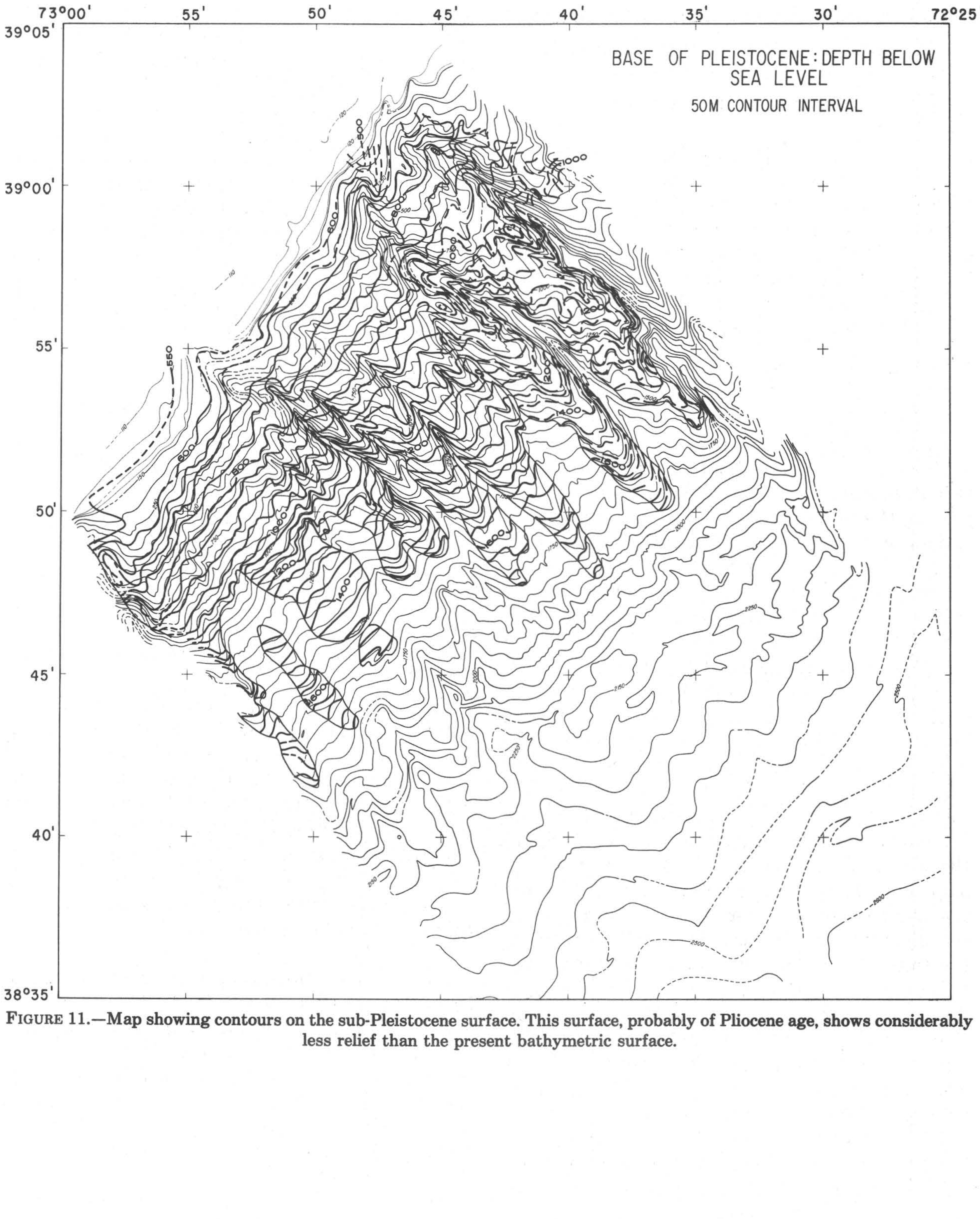

FIGURE 11.—Map showing contours on the sub-Pleistocene surface. This surface, probably of Pliocene age, shows considerably less relief than the present bathymetric surface.

A Nomogram for Interpreting Slope Stability of Fine-grained Deposits in Modern and Ancient Marine Environments

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ABSTRACT[1]

A nomogram [fig. 1] has been constructed for analyzing slope stability in fine-grained sedimentary environments. It was designed to aid in interpreting the causes of mass movement in modern and ancient settings, to provide a basis for evaluating and predicting slope stability under given conditions, and to further the understanding of the relationships among the several key factors that control slope stability.

Design of the nomogram is based on effective stress and combines consolidation theory as applicable to depositional environments with the infinite-slope model of slope-stability analysis. The link between the two combined theories is a term representing the effective overburden stress, which may be predicted from consolidation theory and a knowledge of sedimentation rate, time, and the coefficient of consolidation. In turn, if infinite-slope conditions are assumed to exist, the effective overburden stress can be used to derive a factor of safety against static slope failure by using the angle of internal friction and the slope angle. Values of the variables may be determined directly from measurement, or, depending on the objectives or limitations of the application, they may be specified or estimated. Information supplied with the nomogram is intended to assist in estimating values where necessary.

The nomogram applies to depositional settings in which fine-grained sediment has accumulated at a relatively constant rate upon a base that is essentially impermeable. The model further assumes that the lateral extent of sediment affected by any mass movement will be great compared to its thickness and that no outside agents (for example, cements, gas) are influencing the section. The nomogram is applicable to static conditions (inherent stability of the slope) and certain dynamic conditions (such as earthquakes). It may be used to investigate mass movements in the geologic past as well as those in modern environments.

Although the nomogram was not designed to be more than an interpretive aid and was not intended for solving slope-stability problems of any complexity or where precision is required, it does provide a basis for interpretation and an adequate first approximation in most cases.

[1]1985, Journal of Sedimentary Petrology, vol. 55, no. 1, p. 0029–0036.

Discussion following Booth's talk:

Teleki: Were there any specific weight parameters that went into that wave loading?

Booth: That's just the maximum it could get.

Teleki: The maximum is a function of some weight?

Booth: In other words, the maximum excess pore pressures you could generate by loading would be that point.

Coleman: Jim, how do you handle normal gravitational forces?

Booth: Well, that's no problem. The slope angle is taken into account. There are a lot of triggering mechanisms. Many of them can be incorporated and you can just as easily do the equation at that point, but you could have some tectonic factors, too, or you could have undercutting. All these things change the angle that, in turn, [could tell you] where you would change the figure you plugged into here anyway.

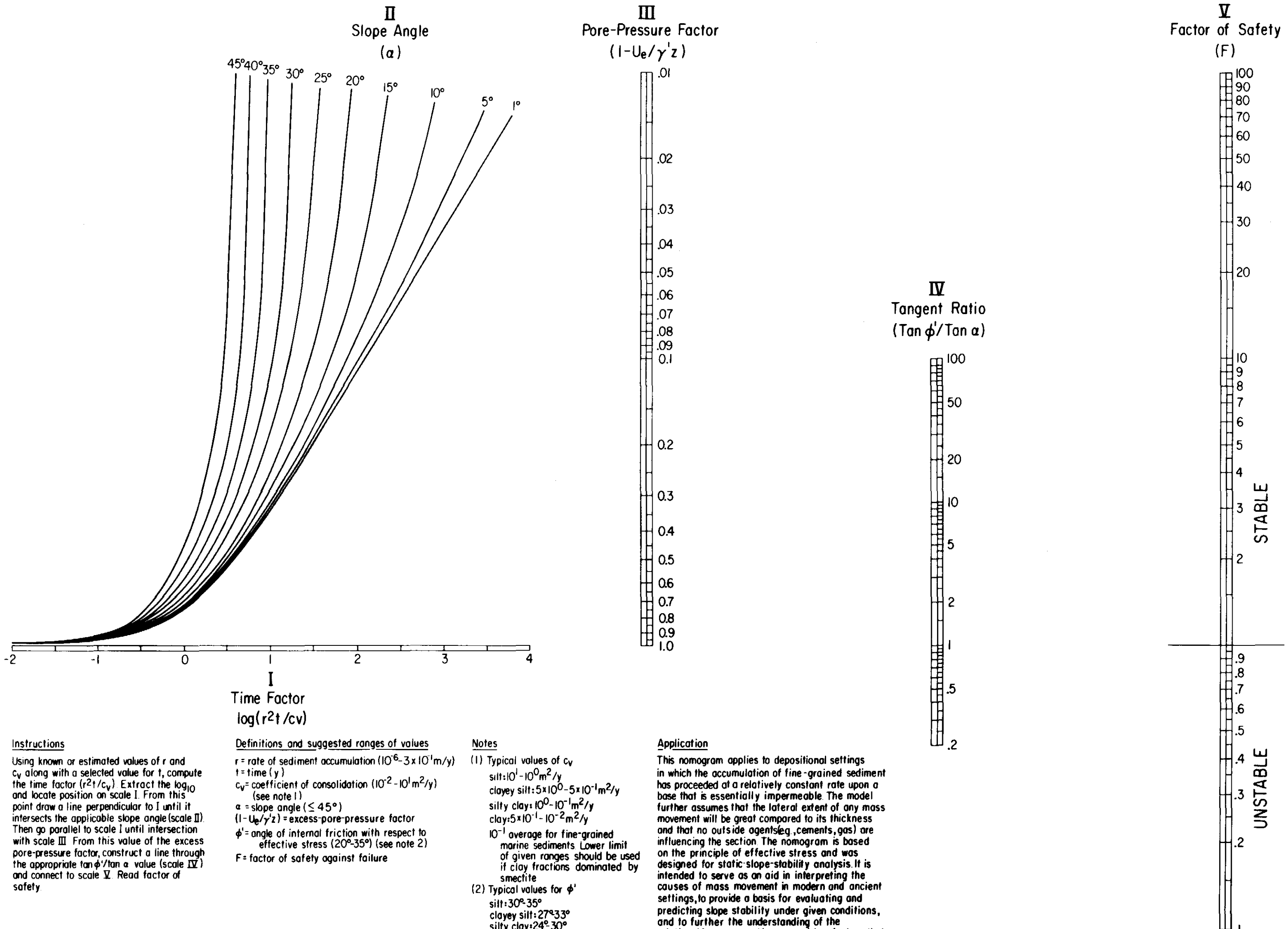
Emery: The model assumes no lateral transport parallel to bedding plane, does it?

Booth: Yes.

Emery: And if there is, that increases the instability or decreases the stability?

Booth: I didn't want you to get the impression that this is an analytical tool. It just gives a general idea of the factors that are involved and how they bear on each other, if you can allow for different loading effects and so on. I look at it as a sort of desk-top type of tool that can be used to look at different situations.

Coleman: May I ask you another question about the East Coast? That sedimentation rate, that time factor, when you use it out there today, we're talking about sedimentation rates that are fairly



Instructions

Using known or estimated values of r and c_v along with a selected value for t, compute the time factor (r^2t/c_v). Extract the $\log_{10}$ and locate position on scale I. From this point draw a line perpendicular to I until it intersects the applicable slope angle (scale II). Then go parallel to scale I until intersection with scale III. From this value of the excess pore-pressure factor, construct a line through the appropriate $\tan\phi'/\tan\alpha$ value (scale IV) and connect to scale V. Read factor of safety.

Definitions and suggested ranges of values

r = rate of sediment accumulation (10^{-6}–3×10^{-1} m/y)
t = time (y)
c_v = coefficient of consolidation (10^{-2}–10^{1} m²/y) (see note 1)
α = slope angle (≤ 45°)
$(1-U_e/\gamma'z)$ = excess-pore-pressure factor
φ' = angle of internal friction with respect to effective stress (20°–35°) (see note 2)
F = factor of safety against failure

Notes

(1) Typical values of c_v
silt: 10^{1}–10^{0} m²/y
clayey silt: 5×10^{0}–5×10^{-1} m²/y
silty clay: 10^{0}–10^{-1} m²/y
clay: 5×10^{-1}–10^{-2} m²/y
10^{-1} average for fine-grained marine sediments. Lower limit of given ranges should be used if clay fractions dominated by smectite

(2) Typical values for φ'
silt: 30°–35°
clayey silt: 27°–33°
silty clay: 24°–30°
clay: 20°–28°
28° average for fine-grained marine sediment. Lower limit of given ranges should be used if clay is dominated by smectite.

Application

This nomogram applies to depositional settings in which the accumulation of fine-grained sediment has proceeded at a relatively constant rate upon a base that is essentially impermeable. The model further assumes that the lateral extent of any mass movement will be great compared to its thickness and that no outside agents (eg., cements, gas) are influencing the section. The nomogram is based on the principle of effective stress and was designed for static-slope-stability analysis. It is intended to serve as an aid in interpreting the causes of mass movement in modern and ancient settings, to provide a basis for evaluating and predicting slope stability under given conditions, and to further the understanding of the relationships among the several key factors that control slope stability. It is not intended for use where accuracy or precision is required: it will give only approximate solutions.

FIGURE 1.—Marine-slope-stability nomogram.

uniform. But, when you get back to the Pleistocene, it was indeed rapid deposition. How do you estimate that?

Booth: A fellow down at Corpus Christi, Jim Fugate, is modifying the Gibson model to take into account variations in sedimentation rate so we can eliminate that assumption.

McGregor: Jim, what would happen in the model at the end that Dwight showed when you have a rising sea level? Suppose sea level came up very, very quickly. Would that effectively change the pore water pressure in the sediments if you had a rapid rise over a slow rise, or can it be equilibrated quickly?

Booth: Sea-level rise itself no, unless it's jumped all the way down.

McNeill: If it's mostly pore water with nothing compressible in it, then, in principle, it should be in this continuous response. On the other hand, if you have substantial gas, I'm not sure I understand precisely what will happen, but I think it would take a while to respond.

Homa Lee: One thing that's always bothered me: Gibson's equation shows that material should be most underconsolidated at the surface and should become progressively less underconsolidated the deeper you go. But yet, when you see something like your plot of strength vs. depth relative to what you'd expect for normally consolidated material, just the opposite occurs. And I'm wondering if that is some problem with Gibson's equation.

McNeill: But the consolidation pressure is different at each depth also. He's just saying what the rate of change is.

Homa Lee: No, Gibson shows that the degree of underconsolidation is the lowest right at the surface.

Dunlap: One thing that's a problem with Gibson's consolidation theory, as you're well aware, [is that it] does not assume the change of properties. Get down to the bottom, obviously it's compressed more [and has] greater densities. A lot of people have been working on this and Bob Schiffman has developed a very nice theory which takes into account change in properties as consolidation proceeds and shows exactly the same thing Gibson does. So, so much for theories.

Gravitational Tectonics in the Atlantic

Elazar Uchupi and K.O. Emery

The speakers summarized many studies concerning sedimentary mass movement on the Atlantic margin as well as in the regions of active spreading where rock slides and pelagic sediment slumping are common.

They pointed out that although most data for studying slumping come from seismic profiles, other techniques might also be useful for detecting active slide movement. One such technique is the use of acoustic records collected by such means as sonobuoys, the SOFAR net, or devices set out to record animal noises. For example, on one occasion, whale sounds recorded by members of the Woods Hole Oceanographic Institution contained an unidentified background noise lasting 100 seconds. The speakers attributed the noise to a slide on the Continental Slope. If their interpretation is correct, a net of hydrophones set in an area where instability is common, such as the Mississippi Delta, might yield much information on timing and duration of slides.

Based on examination of thousands of kilometers of seismic records between 60 °N and 60 °S in the Atlantic, Uchupi has become convinced that many features previously attributed to gravity tectonics could equally well be explained by turbidity flows and bottom currents. This is, of course, not true of areas such as the Mississippi, Amazon, Niger, or Orange Deltas, in areas of Neogene volcanism, or in areas near plate boundaries.

A detailed account was presented concerning the stratigraphy and the sediment stability off South Africa. Gravity tectonics in the area were divided into three types: (1) those affecting sediments of Late Cretaceous age between seismic horizon Davie (Paleocene-Eocene age) and horizon AII (Cretaceous age), (2) those of Neogene age affecting sediments above horizon Davie, and (3) those of Pleistocene age affecting the upper few tens of meters. The discussion below focused on the African margin and then expanded to various aspects of mass-movement features.

Discussion following Uchupi/Emery talk:

Edgar: Do you know the age of Horizon Davie?

Uchupi: Horizon Davie is Paleocene/Eocene age; the sediments above that are Oligocene and younger. Horizon Davie is somewhat older than horizon Au of the east coast of the United States.

Dillon: Al, are these all a single slump structure, or is it an episode of slumping all along the margin?

Uchupi: The structure below Davie appears to be a single massive slump at the apex of the

Orange fan. The structure that affects all the sediments above Davie occurs off Capetown, and the small slumps affecting the upper tens of meters are found throughout the margin.

The features that we originally interpreted as possible slump structures in the Niger fan in the Gulf of Guinea appear to be thrust faults [occurring] as a result of the seaward flow of mud due to overloading by the Niger Delta.

Dillon: Al, some of the oil companies were talking about rollover anticlines.

Uchupi: Yes, in addition to the thrust faults, you also have rollover structures, but these are located beneath the delta. Off northwest Africa, there also are debris flows, relatively superficial features of Pleistocene age. One of the areas where you have massive gravitational structures is in the region of the Canary Islands where a Miocene volcanic debris flow is found southeast of the island chain. The other regions of northwest Africa which have structures comparable to those in South Africa are along the plate boundary between Eurasia and North Africa and seaward of the Western High Atlas.

Thus, the only areas where massive structures occur are South Africa, seaward of the Western High Atlas, north of Agadir Canyon, and on the plate boundary between Eurasia and Africa. Most of the other features that were originally interpreted as massive slump structures can also be explained as turbidity current features. Until you have detailed information, I think you would be very hard put to distinguish between one structure and another.

When I first began work on the east coast of the United States, I interpreted much of the deep seafloor morphology as a product of gravitational tectonics. Today, I believe that most of these structures probably are due to turbidity current activity. On the whole east and gulf coasts, the only area where there's clear-cut evidence of slumping is on the Mississippi Delta and in other fans, areas of massive deposition leading to instability and to the gravitational structures described.

Schlee: How about the area south of Rhode Island?

Uchupi: I'm not saying there are not slump structures present at the base of the slope. I'm just saying there are a lot of features that have been called slumps that may have other origins.

Schlee: Are those still slumps?

Uchupi: I still think they are.

Emery: There, of course, is the mud hole south of Rhode Island, if that's what you're talking about.

Schlee: No, I'm not. I'm talking about the ones he showed.

Uchupi: The ones that John means are also slump structures. Such structures probably are the result of rapid sediment loading south of the mud hole. There also are slump structures associated with diapirs; for example, off Angola and Brazil. Similar structures also occur in the diapiric field in the Gulf of Mexico. The Mexican ridges also are a massive gravitational slide. However, many of the features that have been interpreted in the past as slumps may have a different origin.

Embley: Off South Africa, you see these large slumps; off northwest Africa, you don't see any. I know that Francis Watkins published a paper on Miocene there, where he showed some fairly large ones.

Uchupi: The structures described by Watkins are either associated with Miocene volcanic activity in the region, tectonic activity along the Eurasian-African plate boundary, or uplift of the Western High Atlas. Other structures in the region are surficial features and are related to Pleistocene events.

Folger: Al, off southwest Africa, you show a lot of slumping, that is, real slumping. Why is it there?

Uchupi: Primarily due to rapid deposition off the Orange River (Orange Cone) and subsequent instability. I'm not talking about the very surficial features; I think those are associated with Pleistocene events—debris flows, turbidity currents, that sort of thing.

Emery: It may also be that the general subsidence of the continental margins is a factor. If subsidence takes place mostly toward the ocean, then that should tend to steepen the slope—if this will tend to support the general argument in favor of the possibility of slides on the slope.

Robb: Dave Prior touched on the way we use the word slump, and we use it to include everything from pseudotectonic features to several centimeters of movement in submarine canyon heads. Would you care to comment about the probable period of time over which that accumulation takes place?

Uchupi: Disturbance occurred?

Robb: Disturbance occurred, and what was the scale of mechanisms and the size?

Uchupi: In regard to the Atlantic (for the moment let's forget the Gulf of Mexico and the Mississippi), most of the features are relict and not related to the present processes, so I don't think the slope is unstable at the present time. Period of time? I have no idea of the period.

Robb: We've been talking about slumps as big slumps or little slumps, and you have shown on some of your records massive features. I presume that these do not catastrophically occur in a very short period of time as, say, the delta is now.

Uchupi: The only area that I know that may be catastrophic is the Laurentian fan, as a result of the 1929 earthquake. That slumping occurred there in a matter of minutes is indicated by the cutting of the cables. Other areas where such catastrophic events took place include the Magdalena Cone in the Caribbean, and Congo and Cayar Canyon, all regions of massive sedimentation. When does a sediment pile become unstable? I don't know. One way to find out is by periodic surveys.

Prior: I'm interested in your alternative possible origins of some of these phenomena. But do I understand, from your references to the western Europe area, that all the papers that have been published on the shelf edge off western Britain, for example, are not to be interpreted as slumps.

Uchupi: I would want to see detailed surveys made of the region before I would interpret such features as slumps. Until data are available, such features can be interpreted as erosional or structural in origin. Farther south, there is no doubt that a massive olistostrome was emplaced along the Eurasian-African plate boundary during the late Miocene Alpine orogeny. The surface of that slide is acoustic basement; you can't see into it by using acoustic techniques. It affects material down into the Triassic, and it is intruded by diapiric structures. You can trace the olistostrome onto southern Spain and north Africa.

Emery: Let's go a little farther north in the Bay of Biscay, the north side of the Bay of Biscay. There have been some very nice sidescan records made there. They show the main submarine canyons, but lots of tributaries coming to the interfluve. There's no way that those can have access to sediments on the Continental Shelf to make turbidity currents because they don't go to the Continental Shelf. Presumably, those are lateral gullies made by slides, mass movements. Whether they are slumps or not is something else. So, Al did not mean to say that all landslides are not landslides.

Bloom: You get the clear implication that they're not, unless they're proved they are.

Uchupi: Correct.

Emery: You have to look at them carefully, sure.

Robb: K.O., do you two agree or disagree?

Uchupi: I would say that he tends to see more of such features than I do. It's funny the way our ideas have changed. I first believed that such structures were common on the slope and rise. I remember the long discussions with Joe Curray and Dave Ross who believed that they were formed by turbidity currents. It is not until the last few years, after having examined thousands of kilometers of seismic profiles, that I have tended to question my initial interpretation. I don't question the idea that surficial gravitational structures probably occur on slopes. However, I believe that many of the features that have been interpreted as massive slumps and slides may have other origins. From widely spaced observations, you cannot tell whether you are looking at channels, divides, or slumps. Only when detailed control is available, can you distinguish between massive gravitational structures and features due to turbidity and bottom currents. I wish someone would explain to me how those so-called massive gravitational structures that are hundreds of meters thick and tens of kilometers wide are displaced downslope and their internal stratification is hardly disturbed. How is this possible with sediments whose compressive velocities would indicate [that they] are unconsolidated?

Pacific Margin

Mass-Movement Problems off the Pacific Margin of the United States

Monty A. Hampton

Hampton presented an overview of U.S. Geological Survey activity on mass-movement problems in the offshore Alaska and Pacific coast areas, a wide variety of geologic settings.

In the Beaufort Sea, the Continental Shelf and Slope show three zones generally paralleling the shelf break; these zones contain increasing degrees of deformation in a seaward direction. The landward zone, termed "sag" terrain, has incipient motion only and gives way to a zone showing pull-apart features, such as deep crevasses, and then to the Continental Slope, where large rotational slumps as thick as 230 m occur. Seismicity is the current explanation for these movements.

On the Kodiak Shelf, little evidence of instability exists for the shelf itself, but seaward of the shelf break, large rotational slumps and shallow block glides occur. Evidence here indicates tectonic deformation as the causative factor through slope steepening and removal of support at the base of slopes by faulting.

In the eastern Gulf of Alaska, an abundant sediment supply from rivers and glaciers creates instability on the shelf as well as on the Continental Slope. Causative factors for mass movement here include underconsolidation, wave motion, earthquakes, and possible gas charging.

The continental margin off the northern California coast consists of (1) a narrow shelf, (2) a sloping area where instability features are observed, and (3) a broad plateau which gives way to the Continental Slope farther seaward. The plateau is being uplifted by Pliocene shale diapirs, and retrogressive slumping occurs on the slopes produced by these diapirs. Seismic activity may serve to trigger movements.

Off central California, slope instability exists in several large areas a few hundred kilometers square. Here, sources of sediment are absent, so mass movements are probably caused by seismic activity or possibly gas charging.

The Southern California Borderland consists of a narrow Continental Shelf and several basins and ridges that provide abundant steep slopes and abundant examples of slope instability, ranging from major slumps thicker than 100 m, through medium-scale features, to small-scale movements observed by submersible, TV, and camera studies. These small-scale movements probably lead to the large-scale by progressively loading an area beyond its factor of safety.

Eastern Mediterranean Margin

Sedimentary Characteristics of the Nile Delta and the Persian Gulf

David A. Ross

The speaker presented studies of the Mediterranean, particularly the Nile Delta, and the Persian Gulf areas. The data were collected to determine general structure and evolution of the regions, so the line spacings are much broader (about 60 km) than those used for surveys specifically designed to study mass movement.

The shelf of the Nile Delta is narrow on the west and broader to the east where most of the sediment has been carried. Offshore, the main part of the delta consists of the Nile Cone and the Levant Platform, which is higher than the Nile Cone and which shows the effects of evaporite flow and solution. In the main part of the delta, the sedimentation rate is about 37 cm/1,000 yr; 1.8 km of post-Miocene sediment has accumulated. An evaporite sequence (reflector M), visible in seismic profiles, indicates much flow. In some places, the evaporite crops out on the sea floor and, in one location, has produced a salt ridge 160 km long.

The Nile cone is relatively smooth and shows less deformation than the Levant Platform. Although previous work has postulated intricate fault patterns in this platform area, Ross interprets the arrays of such features as the result of solution of evaporites and collapse. On parts of the shelf and outer slope, slumping is common; farther offshore, faulting is common, with a few slumps.

Channels are notably lacking in the whole region. The principal agents that produce these structures are the high sedimentation rate, the flow of salt, and compression from the Mediterranean Ridge.

Ross continued with a presentation of some of the features of the Persian Gulf that were observed on a recent cruise. Although existing topographic information would suggest that the region is smooth and relatively featureless, the 3.5-kHz records revealed a large variety of structures such as faults, slumps, and ponding of sediments. Considerable discussion followed this presentation; the discussion focused principally on the contrast between interpretations of sidescan-sonar data collected by the U.S. Navy in the Nile Delta region and interpretations based on the more widely spaced geophysical profiles. The need for detailed surveys and sampling was reemphasized.

Discussion following Ross' talk:

Coleman: Dave, you made a comment about the smoothness of the bottom out on the lower parts of the rise off the Nile Delta. You know of the work that the Navy did over there. They covered much of the shelf with overlapping sidescan. The thing that amazes me is that you found smoothness as far up the rise as you did. When you go from the shelf the other way, you get just exactly the opposite situation. Narrow blocks can be traced in 2,000–3,000 ft [610–915 m] of water. They're 8 km long, and you can move them back up 4 mi [7.5 km] and fit them perfectly into the next sidescan.

Ross: Right at the delta?

Coleman: All off the delta. The Navy ran the whole delta region. All to about 2000 ft [610 m] of water. I think if you had their line spacing, you'd see a lot of contortion.

Ross: I suspect it's quite similar to what you have in the Gulf of Mexico, but on this scale the topography is pretty simple. I am surprised at the lack of channels, even at this scale; I thought we would have seen more. We didn't happen to pick up many of those slump blocks.

Robb: Did you have any strike lines, Dave? Parallel to the bathymetry?

Ross: No; mainly, there were no shallow-water or turn lines at the ends. You know, if you're going into an area for the first time, it makes more sense to go in [shoreward]; we really didn't have much previous structure to go on. There have been some detailed studies along small parts of the Mediterranean Ridge before we went there. I found some nice salt patch structures.

Menard: Dave, it's been a long time since I looked at the records but, as I recall, the telegraph cables run parallel to the contours in that area, and they haven't been broached. Maybe my memory is wrong. Those are old telegraph cables.

Ross: Could be.

Menard: Once again, not much goes on.

Uchupi: We looked at those records and, as a matter of fact, they are included in that information.

Ross: There's one canyon, Alexandria Canyon.

Uchupi: The interesting thing about that fan, as Dave pointed out, is that you'd think you would see channels.

Ross: Even though there are supposedly two fans, we found they really didn't seem to exist, even off the ancient Nile Delta.

Folger: Bill Ryan was to be here. He would have some comments on Mediterranean sediment stability. He did some very detailed work on a different scale from this, I guess off Italy. Have you talked to him at all about that, Dave?

Ross: He's used some of our data and feels the same way.

Folger: From detailed piston coring, he finds evidence of sheet flow, very thin layers shed at one time that accumulated at the base of the slope.

Ross: It could be the other way. People have looked at our cores and Lamont cores and correlated sapropels over hundreds of miles. Personally, I think they're wrong. I don't think you can really do that, but nevertheless they do; and again, they indicate some sort of stability out there.

Coleman: Dave, if I could make another comment right there, getting back to the point of this data on spacing. I made reference to the Navy work that was done. They ran 400-m-spaced lines over a 5-year period, starting in the Central Nile, all the way over to the other area. It was something like 19,000 mi [35,000 km] of sidescan sonar, 100 percent overlap. They went down to about 2,000 ft [620 m] of water, and by that time their cable played out in shallow water. We had an opportunity to map all of that. What was interesting to me is that I saw a lot of things I did not understand; in fact, I found more things I didn't understand than I understood. But take the same Nile outer shelf-shelf area; there have been several cruises across that shelf, among them, the Woods Hole cruise and the Russian cruise. When you overlay all that data, it really shocked me; how different the picture when you have a lot of data. And I keep raising the point that I think, if we're going to start answering some of these questions we're asking, we've got to have some data. I mean really close-spaced data.

Ross: I completely support what you're saying.

Coleman: Everywhere we've worked, the same thing has happened.

Ross: From the first time you said it in your talk, I couldn't agree with you more. Of course, you know we're talking about a $15 to $20 million project that far away.

Uchupi: Another thing that makes that area (Nile shelf) so distinct from the Mississippi is that in the Mississippi during the Pleistocene a canyon, fan, and a turbidity current regime of channels, levees, and so on, was established. If you look at the Nile shelf, a lot of the forms that Dave showed you on the profiles are Pleistocene carbonate buildups constructed at the time when the Mississippi was dumping a lot of sediment on the cone. Here you have a major river (Nile), and, right at the mouth, you have carbonate deposition taking place. You don't have those conditions in the Mississippi.

Coleman: We have data on a whole bunch of those carbonates that all dated older than about 800 years. And another point I'd make is that without good radiocarbon dates, you might make an assumption that all of that sediment is residual out there. It has not accumulated simultaneously, and, once more, it just comes down to detail you have to have to make a complete interpretation of an area.

Uchupi: The thing that makes it all so complicated is that, if you remember, Dave showed you the eastern part—the Levant Platform—is disrupted by diapir structures which may be composed of salt. These structures are supposedly intruded into post Miocene sediments. In other words, the Nile fan was much wider in the past. The question is why is it that deformation stops so sharply at the western boundary of the platform? What Dave and I suggested is that on one side you had a late Miocene basin where salt was deposited, and on the other side you had a carbonate platform. The undeformed segment of the Pliocene-Pleistocene fan is sitting on top of the carbonates, and the deformed section is sitting on top of the salt. That's why there is a drastic difference in east-west morphology.

Coleman: Has that been borne out by industry drilling out there?

Uchupi: No, but you do have continuity of seismic stratigraphy from the rest of the eastern Mediterranean.

Schlee: My question is directed to Jim [Coleman]. Does the Navy data look like the Gulf of Mexico-Mississippi area? If it doesn't, how does it differ?

Coleman: We don't see a lot of these delta-type features; they are absent.

Schlee: What do you see?

Coleman: You see a large number of isolated blocks. Now don't ask me how they got there, what they are. They didn't sample. But you see a lot of areas with actively growing carbonate reefs, and there are just literally thousands of little mud volcanoes spewing out methane gas. I've never seen that many anywhere but in the Gulf of Mexico.

Schlee: What about off active mouths?

Coleman: Smooth; not a feature on the sea floor, except for the wrecks and airplane debris and all that. It wasn't until you got out to the shelf edge where you really start seeing the blocks. When they moved, I don't know.

Schlee: Is there a channel connecting them back?

Coleman: On a single line, yes. It looks like a beautiful channel. If I slide the block back up a few kilometers, I can close it right back in. Basically, what you see is a smooth area for the sea floor except for these little carbonate mounds. Then, you hit that shelf edge and sometimes on the profiles—400 m lines—you can see two little scars merge into one; that's how intricate they are out there. It's hard for me to visualize something that fine. And, if it's old, could it have maintained such freshness?

Smith: I suspect it's a mistake to try to expect the mouth of the Nile and the Mississippi to look the same, in that the times of sea-level lowering would be times of maximum flow in the Mississippi, but probably, judging from the fluvial lakes, would be minimal for Africa or, for that matter, Mexico. So you have sort of an opposite situation for the two areas.

Prior: We've also been doing some work with sidescan-sonar on other smaller delta systems. We've gone to some of the gravity-prograding arcuate ones, and sometimes we see things that are analogous to the Mississippi, where there are apparent channels running down the slopes and slump block chaos on the steep prograding slopes. Other places, you don't see any channels at all and what you have is a totally rumpled front of the delta. I think we don't want to suggest that we only have one model for a delta-front instability system; there are going to be a whole bunch of them.

Schlee: But can you crank in a bunch of factors to explain the difference? In other words, obviously the Mississippi is not the same as the Nile.

Prior: I think that you can start doing some qualitative stuff, but I think that verges on arm waving. You can say what is different here and there, but I think there has got to be a lot more geotechnical analysis of properties of materials and proper stability evaluations to see what the kind of ranges the differences are. I think it's probably going to tell us a lot of things we don't yet know.

Coleman: Take, for example, the Navy data. Just by adding one coring tube to that ship that obtained four years of good subbottom profiles would have added an enormous amount of data.

Ross: The nearshore data, you and I both know, has tremendous political implications because the erosion rate is spectacular along the coast; the

Egyptians admit the erosion rate is high and is causing damage to harbors.

Coleman: Is the sediment going to Israel?

Ross: The Israelis are getting upset too, because the sand is not coming at the same rate as it used to.

Coleman: That's right.

Woo: Is there gas seepage in the delta? The Nile Delta?

Ross: Not that I'm aware of, but we heard of some here. The drilling, the best I know, on the Nile Delta proper has not been too successful. It has been successful in the Gulf of Suez region. I'm not aware of any based on my stuff, but Jim said he saw some.

Coleman: Do you mean thermocatalytic gas, or methane?

Woo: Methane.

Coleman: There are structures that we saw that have vents and cone buildups. Elsewhere, we've seen mud volcanoes that are leaking methane gas. No samples were taken of the gas to show what it was, but there are a lot of structures out there that had that feature. They're muds, fine-grained muds, but we have no direct measurements. They are mostly along the outer shelf.

Hathaway: Thank you, Dave. We still have time in our general discussion period if anyone has questions for any of the previous authors. Brad?

Butman: What is the sedimentation rate at the mouth of the Mississippi?

Coleman: You have to ask that over some time period—take a year. Over one year in some areas, it may range from something like 15 ft [5 m]. It may be as high as 15 ft in 3 months during floods. In other areas, it may only be a foot per year, or sometimes over a 2- to 3-month period. We've taken 1974 and 1977 bathymetric maps and compared depths at various locations. We took the difference and divided by the elapsed time. But a certain percent of that sediment is not there; it's moved downslope. Many people were using 1 ft of sediment per year, or even lower values than that. You say, "Well, I can't build up any such pore pressures in that." But you lay down 16 ft [5 m] in 3 months, and pore pressures could be high.

Butman: Was that one-size distribution or were the sediments mixed silts and clays?

Coleman: Right near the mouth, you get some fine sands, but the rest of it is essentially silt and clay. And by the time you get way offshore, except for the occasional slump block, most of it, I would say, is silt.

Emery: Jim, with reference to those figures for the bottom of the Mississippi trough: the thickness you gave amounts to an average of 10 to 15 cm/yr.

Coleman: O.K., let's see if I can figure it out. Actually, we're going to need a hole through these sediments to get a radiocarbon date. We will then go back to the data that's been run already, and take two intervals that can be traced seismically. What we may see is that some of those sediments may have accumulated in less than 1,000 years, sediments as thick as 500 ft [152.5 m]. When the Mississippi was filling, it filled in fast; then moved somewhere else and filled another channel or hole. I think we're going to be able to answer that because there are some borings that are going down in it.

Sangrey: Jim, what's the sediment makeup of the Nile, say 100 years ago? In terms of grain size and in terms of mineralogy, what was the source rock for most of the sediment, particularly the fine-grained material?

Coleman: Dave can answer some of the questions on the mineralogy. The grain sizes that we ran on the shelf were fine-to-medium sands in the sandy areas. There is a minimum amount of fine silt in our samples, and it wasn't until we got way out to the shelf edge that we could really see any good clays deposited, except for right against the shoreline behind the levees of the main distributaries. There's a lot of reefal fragments in the cemented slabs in the area. Much of the delta is in active migration. The 1922 British Admiralty map was really well sounded, when you compare it with the 1970's map that was made by the Navy. Now, in water depths of 60-70 m, there would be as much as 5–8 m of change over that time, when you measure at the same locations. Now, if you could isopach those changes with extensive age control, you could make a dynamic interpretation.

Sangrey: Dave, do you have any mineralogy on the fine fraction? What are the source rocks in Africa?

Ross: Well, it drains about 1/6 of Africa; it's got almost everything. I can get you the information. I don't have it here, but we did look at it and the Egyptians did work on it. It's pretty easy, looking at the clays, to know what's Nile stuff and what isn't.

Coleman: You could distinguish between the Nile source by the clay minerals?

Ross: No problem; it was done 20 years ago.

SEDIMENT SOURCES AND CURRENT REGIMES

Deep-Water Circulation along the Margin of Eastern North America

Charles D. Hollister

The speaker discussed potential mechanisms that cause sediment to move in the deep sea and problems associated with monitoring and measuring those movements. Are, for example, ripples in bottom sediments deflation patterns or depositional patterns? What parameters should and can we measure to answer such questions? Before discussing, in detail, elements of the High Energy Benthic Boundary Layer Experiment (HEBBLE) program, which is being designed to measure a broad spectrum of parameters at the deep-sea bottom, Hollister discussed some of the background of studies concerned with deep-sea sediment motion.

Much information in the early 60's was derived from bottom photographs. For example, Antarctic bottom flow was reflected by rippled microtopography in the Bellinghausen and Argentine Basins. The bottom of the western Atlantic is similarly affected by flow of the same water, whereas the bottom of the eastern Atlantic, which is protected from Antarctic bottom-water flow by the mid-Atlantic Ridge, reflects sluggish circulation. In contrast, along the western Atlantic margin southward-flowing, contour-following currents that impinge on the Continental Rise not only create bed forms on the bottom but also actively transport sediment.

The HEBBLE project is aimed at integrating a number of sensors and collectors on one tripod or lander to relate and quantify forces that produce bottom-sediment motion. The sensors include current meters, conductivity-temperature-depth (CTD), turbidometers, and sediment traps. Early deployment of components of these devices revealed that some southward-flowing Antarctic bottom water is present at the base of the continental margin off Nova Scotia.

Application of these instruments to waste-disposal problems is obvious. For example, within the 200-mi zone, dumping in waters over 3,600 m deep will be in much more vigorous current regimes than at shallower depths. Turbidity and sediment trap measurements as part of HEBBLE show that high sediment concentrations near the bottom in deepest waters increase greatly where mud waves and crag and tail structures are most common on the bottom.

Experiments with various components of the lander are still underway. "The whole idea is to look within the high-energy benthic areas where [effects of] chemical and biological signals seem to be relatively low and the [effects of] physical signals relatively high on resuspension and deposition."

Discussion following Hollister's talk:

Bloom: What's the story about the Antarctic bottom water coming back around, heading south on the western edge of the basin? Where has it been, and where is it going, and how long has it been there?

Hollister: Along the Atlantic coast? Oh, that's a real question. I don't know the answer to that.

Bloom: What sort of travel times in a trip of that length?

Hollister: From the Antarctic up to here? Well, you can figure it out. Assume a half a knot. It's the number of years I suspect.

Bloom: All old water then?

Hollister: Yeah, old. It may have gone up one side of Bermuda and been entrained by some other flow and brought south. This is a question we don't know the answer to.

Peck: How do you know if that's Antarctic water rather than Arctic water?

Hollister: Well, Arctic water has a much lower silica content, and it's much saltier. That's basically the reason.

McGregor: Any evidence that it's gotten around the Corner Seamounts and that they're acting as a deflector to channel it westward?

Hollister: It very well might be coming through the axis of maximum depth to the west of the Corner Seamounts going up to the Laurentian fan and bending around to the left. I think the Corner Seamounts may have a major impact.

Teleki: How do you get bedforms parallel to the current?

Hollister: How do you get bedforms in cohesive clay?

Edgar: Do you have any cores that indicated whether or not there has been net erosion?

Hollister: Well, if it's been net erosion over any length of time, we wouldn't have any Continental

Rise off Nova Scotia. I mean it would be gone—there'd be holes in it. So it looks, if you want a wild guess, as though for the sediments in the Labrador and Irminger Seas it's just picking up gobs of mud there and up along the Labrador margin, and it's moving right through an area where you have some deposition and some erosion; all of that material is being dumped down on the Antilles, Caicos, and Blake-Bahama Ridge. So you have a huge resuspension process, a pass-through area like this, and the depositional area down off Puerto Rico. But that's nothing more than the wildest of all guesses.

Edgar: Are you saying that any time we photograph features like those on the ocean floor, they have to be made within 2 weeks of photographing?

Hollister: I would say they're not Pleistocene. The objective is to find out how long it takes to form one of those features, and I don't know. We're planning 6-months deployment of the whole master lander in this type of field first and that type of field second. We think, obviously, the larger the bed form, the longer it takes to produce it. Big furrows might take hundreds of years to develop. I don't know. Some might take weeks to months and others might take hours.

Folger: Have you looked at Brad's [Butman] results on the shelf?

Hollister: I talked to Stefanie and Brad. I've been peering over various shoulders trying to learn from what they're doing, and I think we seem to be both doing roughly the same sorts of things in two different environments. I'm hoping we'll drag them into deeper water, and I think he and Stefanie are trying to drag me into shallower water. But I'm hoping that we'll overlap.

Folger: I'd be interested to compare the cost of your lander with the cost of our lander. We have nine landers, you know.

Hollister: Actually, I tried to get one of those from Brad. We had one problem; he has them deployed all the time. I don't have a lander yet. But I believe the spin-out is something like $60,000 for one.

Shackleton: Did you catch any benthic forams?

Hollister: Oh yes. We actually got big benthic [forams] and other forams. We got some coated with iron oxide; we got mineral grains that are 200 microns across. Greg tells a story that goes like this: If resuspension is taking place so locally, that is, within a kilometer or two, then that's where the sediment is coming from. It isn't coming from Labrador. There is a lot of resuspended material in those traps. We had grams of sediment in a month's deployment.

Observations of Bottom Currents and Sediment Movement Along the U.S. East Coast Continental Shelf During Winter

Bradford Butman and John A. Moody

ABSTRACT[1]

Near-bottom current and sediment-movement observations were made from January to May 1978 at two locations on the southern flank of Georges Bank in 64- and 85-m water depth and at two locations in the Middle Atlantic Bight, both at 60-m water depth. The observations were made by a bottom tripod system that measured bottom current, temperature, light transmission, and bottom pressure and photographed the bottom. During nonstorm periods, the observations show continuous reworking of the upper 1–2 cm of the surficial sediments on Georges Bank caused by the strong semidiurnal tidal current (especially at the 64-m station) and generally tranquil conditions in the Middle Atlantic Bight. Several major winter storms occurred during the first month of the observation period. During these storms, the surficial sediments along the entire Continental Shelf were reworked and resuspended. Net sediment movement over the entire observation period was primarily longshelf toward the southwest, but movement during storms both to the northeast and southwest was observed. The spatial and temporal variability of the near-bottom suspended-sediment concentration, as determined by the transmission observations, was complex; a major cause of increased suspended-sediment concentrations was local resuspension, but advection of horizontal and vertical suspended-sediment concentration gradients past the tripod system, as

[1]1983, *in* McGregor, B.A., ed., Environmental Geologic Studies on the United States Mid- and North Atlantic Outer Continental Shelf, 1980-1982, Vol. III, North Atlantic Region: U.S. Geological Survey, Final Report to the U.S. Bureau of Land Management under Memorandum of Understanding AA851-MUO-18 and Interagency Agreements AA851-IA1-17 and AA851-IA2-26, Chapter 7, p. 7-1 to 7-60.

well as changes in the composition of the suspended material, also could have contributed to the variability. Bottom stress was computed by using the model of Grant and Madsen (1979), which incorporates the effect of oscillatory currents associated with surface waves on bottom stress. Near-bottom stress during storms was enhanced significantly by waves; bottom stress computed using a quadratic drag law and a constant drag coefficient based on the observed physical roughness underestimated the stress in the wave boundary layer computed using Grant and Madsen by a factor of 2–30 during storm periods. Thus any estimates of bottom stress or sediment movement on the Continental Shelf must include the effect of waves.

Frequency and direction of sediment movement was estimated by using a simple model of sediment transport. Transport of 0.125-mm-sized sediment particles occurred less than 10 percent of the time and only during storms. Transport of 0.063-mm-sized sediment occurred between 75 and 100 percent of the time on Georges Bank and less than 50 percent of the time in the Middle Atlantic Bight. Less than 50 percent of the transport occurred during storms. Suspension of 0.031-mm-sized sediment was almost continuous and thus was transported with the mean flow. The data suggest a net westward movement of sediment from the southern flank of Georges Bank toward the Middle Atlantic Bight.

Drill muds and cuttings discharged by OCS activity on the southern flank of Georges Bank will be gradually reworked and redistributed, primarily by winter storms, but also by the tidal currents in the shallower water. Because much of the sediment resuspension is caused by wave-associated bottom currents, which decrease with water depth, resuspension caused by storms should be more intense in shallow water and less intense in deeper water.

Lydonia Canyon Dynamics Experiment: Preliminary Results

Bradford Butman, M.A. Noble, J.A. Moody, and M.H. Bothner

ABSTRACT[1]

A field program was conducted to study the circulation and sediment dynamics in Lydonia Canyon, located on the southern flank of Georges Bank, and on the adjacent shelf and slope. The program included (1) measurements by an array of moored current meters, bottom tripods, and sediment traps maintained between November 1980 and November 1982; (2) synoptic observations of the hydrography and suspended sediments; (3) sidescan and high-resolution profiles; (4) samples of the surficial sediments; and (5) direct observations of the sea floor from the submersible *ALVIN*.

The surficial sediment distribution and the high-resolution profiles (Twichell, 1983) suggest that very fine sand and silts and clays accumulate in the head of the canyon and on an area of the adjacent shelf. However, the moored current measurements show that the surficial sediments are reworked and resuspended along the canyon axis to a depth of at least 600 m. Thus, although fine sediments may be accumulating, the axis is not tranquil. Maximum hour-averaged current speeds 5 m above bottom (mab) were 50 cm/s at 282 and 600 m in the canyon axis. No evidence of sediment movement was observed at 1,380 m. Further analysis is required to determine the net transport of suspended sediment transport in the axis.

The mean Eulerian current on the shelf adjacent to Lydonia Canyon and above the level of the canyon rim was southwestward, consistent with previous studies of the mean circulation on Georges Bank. On the Continental Slope, the mean flow was strongly influenced by Gulf Stream eddies. Several eddies passed to the south of Lydonia Canyon during the observation period. The strong clockwise flow around the eddies caused eastward flow along the edge of the shelf as strong as 80 cm/s. On the slope the influence of the eddies in the water column extended to at least 250 m but not to 500 m. The influence of the Gulf Stream eddies did not extend onto the Continental Shelf to water depths of 125 m. There was a persistent off-shelf and downslope component of flow near the bottom of a few cm/s.

Within the canyon, the mean Eulerian current pattern was complex. At 282 m at the head of the canyon, net flow 5 mab was downcanyon at about 3 cm/s, and upcanyon at about 2 cm/s 50 mab. At 600 m in the canyon axis, net near-bottom flow was weak or upcanyon at a few cm/s, but downcanyon

[1]1983, *in* McGregor, B.A., ed., Environmental Geologic Studies on the United States Mid and North Atlantic Outer Continental Shelf, 1980–1982, Vol. III, North Atlantic Region: U.S. Geological Survey, Final Report to the U.S. Bureau of Land Management under Memorandum of Understanding AA851-MUO-18 and Interagency Agreements AA851-IA1-17 and AA851-IA2-26, Chapter 8, p. 8-1 to 8-93.

100 mab. These observations suggest a convergence of the mean Eulerian flow between 300 and 600 m and possibly several cells of recirculation along the canyon axis. However, because of the energetic nonlinear high-frequency motion observed in the canyon and the small spatial scales, the mean Eulerian current may not indicate the actual Lagrangian water particle motion. Further analysis is required to determine the Lagrangian circulation pattern. Measurements made on the eastern rim of the canyon at about 200 m show westward flow directly across the canyon axis. Measurements on the eastern wall of the canyon just a few kilometers away at comparable depths show northward inflow along the eastern wall. Measurements on the western wall show southward outflow. The mean Eulerian currents in the canyon thus suggest a complex vertical Eulerian circulation along the axis and horizontal exchange along the canyon walls.

The subtidal current (periods between 33 and 768 hours) can be separated into currents on the shelf, slope, and in the canyon. On both the shelf and slope, the subtidal currents were oriented primarily along isobaths and were vertically and horizontally coherent over the separations measured. On the shelf, the subtidal currents were 25–40 percent wind driven. On the slope, the currents were not wind driven, except in the surface (10 km) Ekman layer. Although the currents on the slope were not wind driven, the currents on the shelf and slope were somewhat coupled. Within the canyon axis, the subtidal currents were much weaker than on the adjacent shelf or slope and were oriented along the canyon axis. Analysis to date suggests that they were not very coherent vertically or horizontally over the spatial scales measured. The subtidal currents in the canyon were not strongly coupled to the currents on the shelf or slope and were not strongly driven by wind stress.

The currents on the shelf and slope and in the canyon were dominated by strong tidal currents. Within the canyon, the high-frequency currents (periods faster than 12 hours) increase in amplitude toward the canyon head, and indicate highly nonlinear processes within the canyon. Large high-frequency currents were not observed on the shelf or slope.

A small moored array experiment conducted in Oceanographer Canyon suggests that the current and sediment dynamics in Oceanographer are somewhat different than in Lydonia Canyon. Near-bottom currents at comparable depths in the canyon axis were larger in Oceanographer Canyon than in Lydonia Canyon. In addition, the mean Eulerian current was downcanyon at both 300 and 600 m in Oceanographer Canyon, in contrast to Lydonia Canyon where the net Eulerian current was downcanyon at 300 m, but upcanyon at 600 m. The current observations, the surficial sediment texture, and large bed forms observed in the canyon axis (Valentine and others, 1981, 1983; Twichell, 1983) suggest that Oceanographer Canyon is more energetic than Lydonia Canyon and that fine-grained sediments may not accumulate in the head of Oceanographer Canyon.

Discussion following Butman's talk:

Teleki: A couple of things that are perhaps worth looking at. You have eddies from the Gulf Stream that come up on the shelf, and I think satellite data have shown very interesting new views of the ocean. The fronts are oriented off-slope, and there's mass transport associated with it. The other thing is that, especially along the mid-Atlantic Bight and as far as Georges Bank, internal waves may be an important factor in moving sediment.

Butman: I should mention that we commonly see in our data large-scale internal waves. In the summertime, they are competent to resuspend sediments. There's another experiment in the works for the summer that looks at the amplitudes of the internal waves as a function of area and a function of distance along the shelf. You're right; that's an important point. Internal waves will act to resuspend sediment. I was surprised that our slope record didn't show much large-scale internal-wave activity, but we may have been too far from the front.

Teleki: Now, one other thing. You say [you have] storm-generated currents; are these basically wind-stress generated, such as from northeasterlies?

Butman: Right. But the surface-wave part of those storms is really what gets the sediments up off the bottom, and the mean currents associated with those storms are what moves them along. The storm-generated currents themselves typically are only 20-30 cm/sec, and that's not strong enough to resuspend the bottom sediments without the surface waves.

Edgar: What happens to internal waves when they get funneled into one of the canyons?

Butman: Good question. We're going to be conducting a major experiment in Lydonia Canyon next fall to answer that question. But we've done some preliminary hydrographic work in the canyons, and they do show evidence of increasing concentrations but not very much: 0.5 mg/L vs. 0.2 mg/L in the middle of the water column. We expect to see some major resuspension where they break on the shelf.

Teleki: You should also see some refraction between the canyon walls, and you should see frequency composition to some extent.

Butman: There's no question. There's a lot to do.

Mineralogic Evidence of Quaternary Current Regimes on the Atlantic Continental Margin

John C. Hathaway

I would like to discuss the evidence that mineralogy has to offer about what currents may have done during the Pleistocene and the Holocene and how mineralogy can serve as an indicator of the overall effect of both sediment sources and currents at depth. Can it give us some clues as to what has been occurring? What is the present situation in regard to possible sediment sources? I've chosen to show the distributions of clay minerals and also of carbonates of clay size as possibly the best indicators to show us what has happened.

A number of years ago, as part of the Emery project, we analyzed a large number of samples that were collected. Of these, I selected about 400 that were representative of those samples that showed more than 0.1 percent clay in the size analysis. In areas like Georges Bank and most of the Continental Shelf, the sediments contain sand; there is almost no clay. However, clay is common in the estuaries of the U.S. east coast and on the Continental Slope and Rise. Diagrams of clay mineral distributions in these areas are given in Hathaway (1972) and the tabulated data in Hathaway (1971). Of various clay minerals that seem to show distinct distribution geographically, illite is quite widespread. It shows a very strong dominance in the north, declines slightly off the shelf edge, slope, and rise, and declines to trace levels south of Cape Hatteras. Samples of flood-plain deposits represent the materials carried by present-day rivers; they are the material left by floods when runoff decreased. Northern areas contain high concentrations of illite; in the south, only traces occur in the rivers.

T. Edgar: Are the mineral amounts computed from peak heights in x-ray diffractograms or from areas under the diffraction curves?

Hathaway: Both are needed to make quantitative estimates.

The same type of distribution shown by illite is illustrated by chlorite, where the northern influence is reflected by relatively large amounts in the Gulf of Maine that extend as far as Cape Hatteras. Thus, chlorite is even more of an indicator of this northern type of assemblage. I should point out at this time that both the illite and the chlorite are particularly abundant just off Chesapeake Bay and extend southward to Cape Hatteras. Also, high values occur in Pamlico Sound, yet there is no apparent contribution of chlorite from the rivers. This distribution strongly supports the idea that material is brought in from the shelf and deposited in the estuaries. The lower parts of Chesapeake Bay contain large amounts of chlorite, but the rivers leading into Chesapeake Bay carry relatively small amounts.

Another indicator of the northern-type assemblage is feldspar. It is present in much smaller amounts, usually 10 percent or less. Traces of it show up all through the northern area, through the mud patch on the Continental Shelf south of Martha's Vineyard, and on the Continental Slope, but only spotty occurrences exist to the south. Another indicator, and this is just a qualitative not a quantitative one, is the presence of hornblende in the samples. Hornblende, usually in traces or one or two percent, is concentrated in the northern area, the Gulf of Maine, the mud patch, and the slope as far south as Cape Hatteras. The four minerals (illite, chlorite, feldspar, and hornblende) seem to be indicative of a northern influence.

Now let us look at some of the other materials, such as the fine-grained carbonates. Calcite has a much wider spread than the mineral assemblage described above. Large amounts of calcite occur near the Bahamas, with moderate amounts extending toward Cape Hatteras; just to the north are smaller amounts, and then calcite increases again. Is this northern calcite really part of the same distribution as that to the south of Cape Hatteras, or is it something different? Notice that there is very little carbonate, none really in the Gulf of

Maine or the mud patch, or in the estuaries along the shore. The only place along the shore that carbonates show up is right off Florida. So to resolve the question as to what this southern vs. northern situation might be, let us look at another fine-grained carbonate, namely, magnesium calcite. We find that magnesian calcite is clearly restricted to the area south of Cape Hatteras. No detectable amounts exist north of this area. Aragonite gives a similar distribution, which is evidence that the distribution of calcite is not the result of transportation by currents carrying the calcite from the Blake Plateau area and depositing it along the slope to the north. If that were so, the magnesian calcite and aragonite (less than 2 microns in these cases) would also have been carried and deposited. Neither the magnesian calcite nor the aragonite, which occur in significant amounts in the regions to the south, show up north of Cape Hatteras. The conclusion I reach is that the northern calcite is from an altogether different source, probably planktonic foraminifera that have settled out from the water column in the northern area, whereas, in the south, these carbonates are largely from benthic contributions.

What is the role of clay minerals in the southern areas? Kaolinite is an important component in the sediments of the southern rivers. The major northern rivers, the Santee, the Savannah, the Altamaha, carried (at least, they did before some of the existing major dams were constructed) much larger amounts of sediment than the rivers of the northeast. In fact, very little sediment is carried by northern rivers at all. Only the southern rivers have probably carried much suspended material in Holocene times. On the sea floor, kaolinite is abundant nearshore but is less common out in the Blake Plateau or on the Florida-Hatteras slope. Kaolinite is also common in the Gulf of Maine, in the mud patch, along the shelf edge, and on the slope. Also, it is rather common around Martha's Vineyard, which gives us some clues as to a possible source. Cretaceous sediments are exposed in the southwestern part of Martha's Vineyard; here they are very, very high in kaolinite. Perhaps the larger amounts of kaolinite on the south side of Georges Bank than on the north side are derived from similar material that was scraped up or eroded by glaciation and allowed to mix with the clays deposited on the south side.

Another of the southern assemblage of clays is montmorillonite, or smectite as it is called these days. Here again, we find a strong southern distribution in estuaries and on the sea floor but relatively little in the rivers because the rivers draining the Piedmont carry a lot of kaolinite. The montmorillonite that occurs in the estuaries was probably derived from older sediments of the Coastal Plain. These have also been eroded on the shelf area and deposited along the Florida-Hatteras slope and on the Blake Plateau.

What we really need at this point is some kind of indicator of material that is in the present-day soils and that would suggest how much of these materials is being contributed to the shelf. We do have one that seems to be forming all along the Atlantic seaboard, and that is material called dioctahedral vermiculite. Some investigators have called it soil chlorite; other such names have been proposed for it. At any rate, it is quite common in most of the rivers, both north and south. If these rivers are contributing very much to the shelf, we ought to see it out there. It has been proposed in the literature that this material is degraded in seawater by diagenesis. Tests, however, that have been made on it show that it really does not degrade; but it does fade out in estuaries. The cause of such apparent loss probably is not really diagenesis at all but simply dilution by other minerals brought in from the seaward direction. Anyway, if we assume that the latter process is the effective one, that dioctahedral vermiculite is not removed by diagenesis, then we ought to see it as an indicator of material carried out from the various rivers. The only place we get it at all is in traces right off the Middle Atlantic area. In other words, although its influence is strong in the rivers, it is very, very weak on the slope. This is another possible piece of evidence that the contribution of present-day river sediment is not important on the Continental Slope of the U.S. east coast. In other regions, of course, it may be but not in this area.

As a summary of what may have gone on during the Pleistocene, figure 1 shows the probable location of the ice front. Drainage from it as it melted would have passed across the shelf and accumulated right at the edge due to the currents that Butman has spoken of. I've shown the currents in arrows perhaps too big and strong. They may not have been that dominant, but at least the general trend would probably have been along the shelf and down towards the Cape Hatteras area.

The Gulf Stream itself does not appear to be a large transporter of materials from the southern

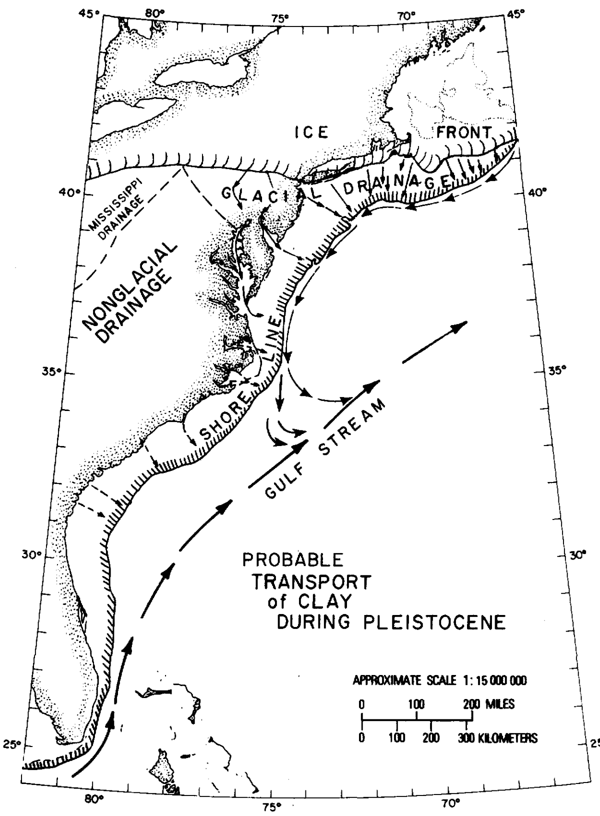


FIGURE 1.—Probable transport of clay during the Pleistocene along the Atlantic continental margin.

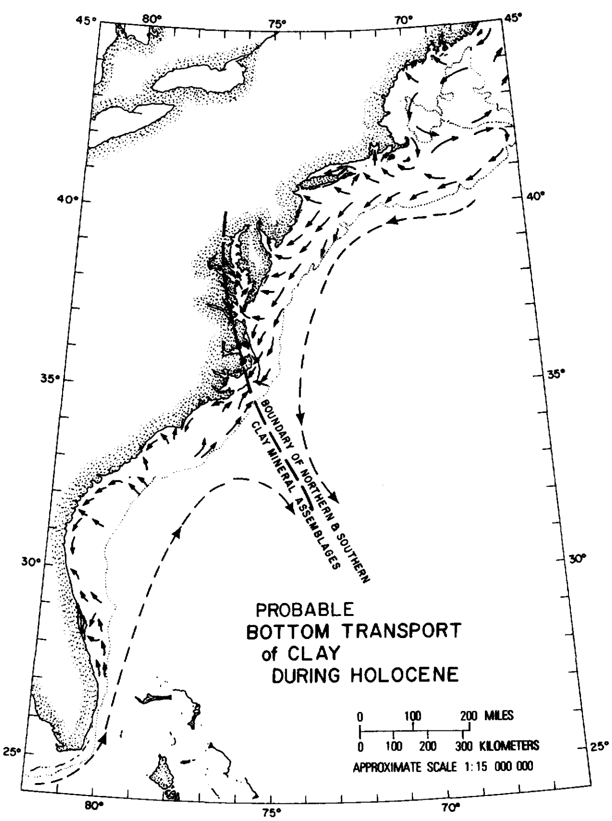


FIGURE 2.—Probable bottom transport of clay shown by small arrows within the dotted 200-m isobath. Dashed line with arrows shows direction of offshore current flow. Northern mineral assemblage is characterized by illite, chlorite, and hornblende. Southern assemblage is characterized by kaolinite, smectite, magnesium calcite, and aragonite.

area because, if it were, we ought to see plenty of the carbonates, including the magnesium calcite, in the northern region. So I don't think the Gulf Stream is really important in terms of transporting these sediments.

Figure 2 shows speculatively what may have been occurring during the Holocene after sea level rose. Here we have the circulation around Georges Bank that Butman pointed out. Also shown are arrows going toward shore that represent the bottom shoreward movement into the estuaries, which has been proposed by me and by others (Meade, 1969). So, the Holocene situation is that anything winnowed from a place like Georges Bank is carried along the shelf and perhaps relatively small amounts fall off the shelf edge occasionally. There is a distinct boundary between a northern assemblage of illite, chlorite, feldspar, and hornblende and a southern assemblage of kaolinite, montmorillonite, and various forms of carbonate.

Thank You

Discussion following Hathaway's talk:

Edgar: Some years ago, Dave Needham did some studies of some cores in the upper rise, I guess it was, studying the distribution of sediments that were characteristically red, I think derived from the Permian up in the St. Lawrence. He traced them all the way down and around the Blake Bahama area, even south of that line.

Hathaway: Yes, that is true. If I had samples from deeper water, we might have shown that influence going around the edge. But we have only one or two fairly deep water samples.

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CLIMATIC CHANGES

Sea-Level Changes and Mass Wasting

Warren Prell

I would like to address the question of how climatically induced sea level changes are related to mass wasting. From the perspective of what we learned yesterday, mass wasting occurs on a tremendous variety of time and space scales. I was struck by the fact that we have considered a time span from monthly changes in deltas to Cretaceous slump blocks. In general, these features range from historical time scales to geologic time scales. If historic changes are of interest, then sea level and climate are relatively constant. If geologic time scales are important, then boundary conditions change. The question is: "Which of these time scales are really responsible for various mass-wasting processes that occur on the sea floor?"

Sea-level and climate changes can be considered as boundary conditions, or in some cases, even forcing functions for various types of mass wasting. As one cause of mass wasting, seismicity is always mentioned, but, as far as I can determine, seismicity is pretty much a random process with respect to the individual mass-wasting events. If you wanted to develop a predictive model, it would be discouraging to depend on something like that as the primary forcing mechanism. The climatic sea-level changes, however, are not random, and they can affect mass wasting in a variety of ways. First, we have enhanced erosion during low sea-level stands, increased gradients of rivers, exposed unstable shelf sediments, and a variety of changes in energy patterns on the Continental Shelf. Second, processes dependent on sea-level and climatic changes will affect the actual depositional patterns of sediments. Where will fine-grained sediments be deposited? On the shelf? On the slope? This is a possible precondition for mass wasting—where is the depositional center? Third, climatic sea-level changes also load and unload the margin itself. It struck me that this might, in fact, give us a source of seismicity that we can predict, that is, rebound seismicity as a function of loading and unloading.

Given all this, how do we determine the importance of climatic and sea-level changes in your record of mass wasting? I think that Bonnie McGregor had the real key yesterday; that is, you've got to correlate the depositional sequences to good records of climatic and sea-level changes and not the unconformities. What we really want to know is the timing of these events and their frequency. When you look at the slump scar, you are looking at what is left; so you really know something only about gross boundaries of when a particular event occurred. So, the real key is to look at the depositional sequences.

What do we need to know? From a climate standpoint, we want to know the amplitude and the frequency of sea-level changes through some period of time—the longer, the better. The problem is "What do we use as a yardstick?"—"What are we going to compare to?" That will be part of what I hope to speak to today, with some new data that are now becoming available.

For those of us in paleoceanography, the *Glomar Challenger* has been our dream for obtaining long, undisturbed cores of deep-sea sediment so that we could resolve past climatic events. Unfortunately, our dream has often been a nightmare. A typical soft-sediment core from the *Challenger* collection is usually badly disturbed by the rotary drilling process (fig. 1*A*). In fact, it is absolutely useless to resolve detailed climatic changes. If you want to talk about sea-level or climatic changes on a tens-of-thousands-year basis, which we know occur, this is not a useful record. Fortunately, though, the DSDP has come up with a new device called a hydraulic piston core (HPC), which is a down-hole tool that has recovered long sequences of relatively undisturbed deep-sea cores. The HPC effectively works like a standard piston core and has a piston to provide back-pressure and prevent the sediment from squirting up the tube like toothpaste, which is what it often does with the *Challenger's* rotary drill. We used the HPC in the Pacific and the Caribbean about this time last year and recovered a couple of extremely interesting cores (Prell and

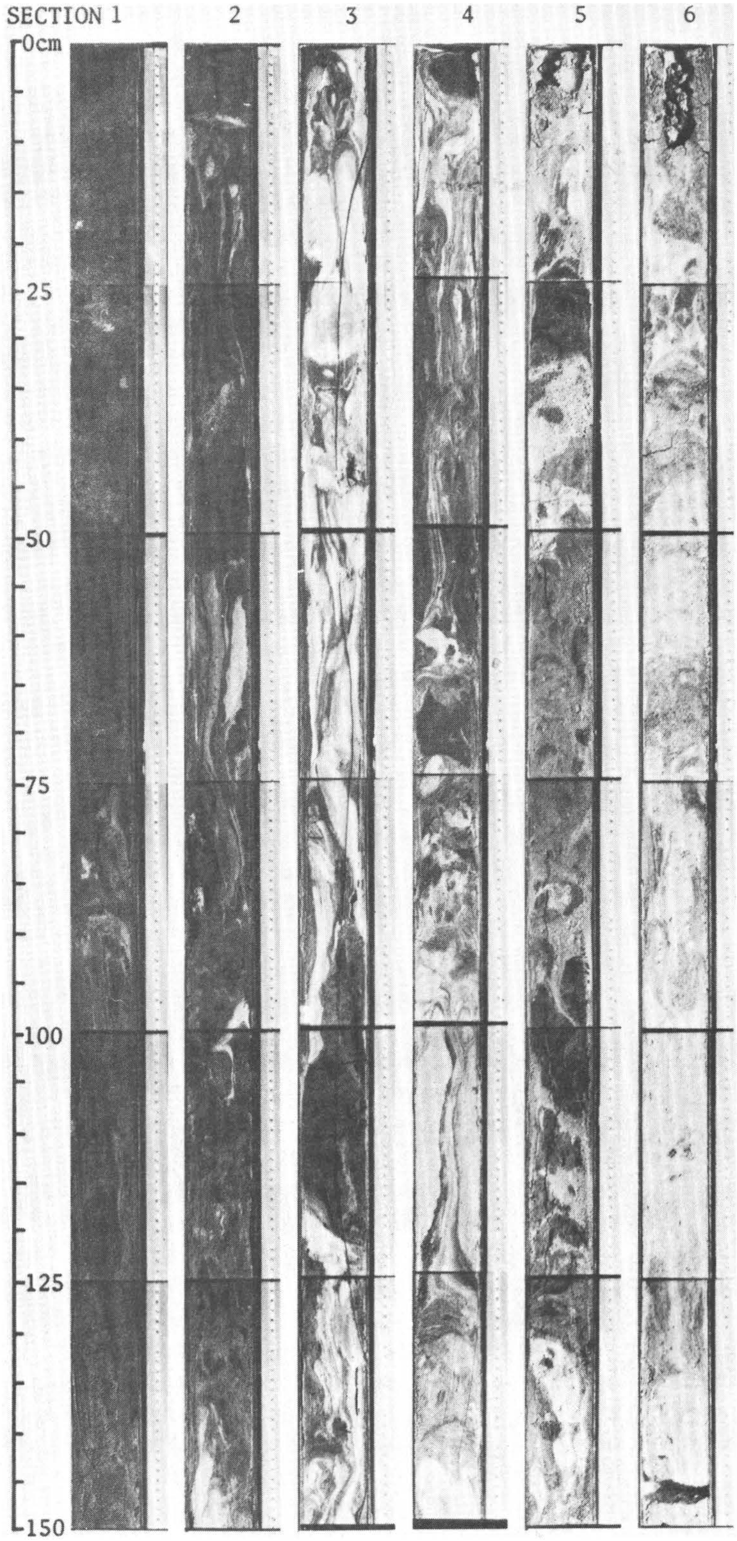

FIGURE 1.—*A*. Rotary drill cores: A distorted record. These cores of deep-sea sediments were obtained by conventional rotary drilling at Pacific site 83. These cores represent the same interval illustrated in figure 1*B* and should show the same features. The drilling technique, however, has grossly disturbed the sediment, as shown by near-vertical layering, flow patterns, and distorted burrows.

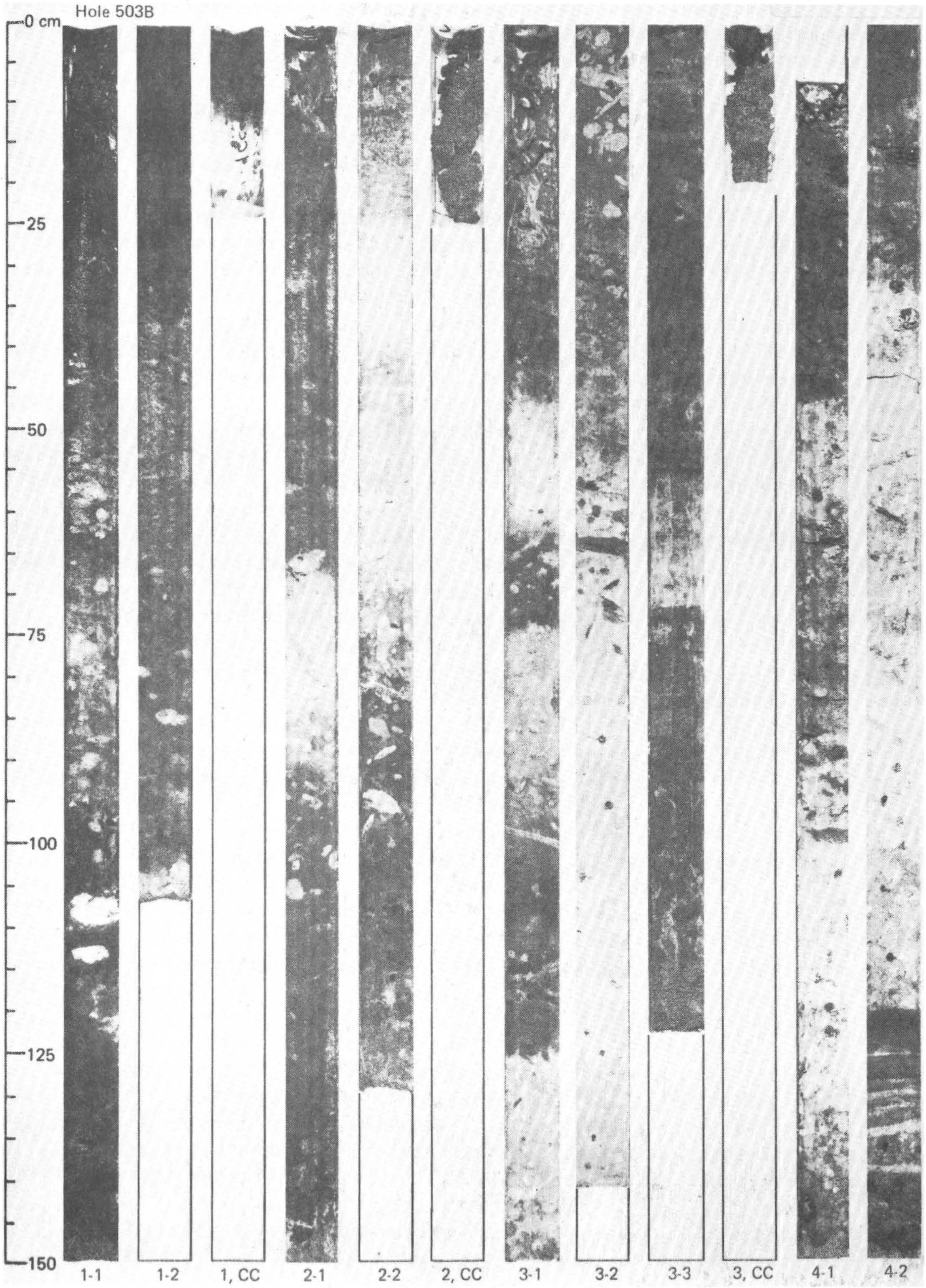

FIGURE 1.—*B*. Hydraulic piston cores: An undistorted record. These cores of deep-sea sediments were obtained by hydraulic piston coring (HPC) at Pacific site 503B. Note the discrete, cylindrical worm burrows and the horizontal contacts between different layers—features that indicate that the samples contain an undisturbed sedimentary record.

Gardner, 1982). As examples, figure 1 shows a core we took from the Pacific. We reoccupied old site 83, near the equator about 95° east. You can see that the *Challenger* rotary drill core was totally disturbed (fig. 1*A*). You see all the color units, but they are mixed, whereas, in the upper core, taken with the HPC, all of the structures are preserved (fig. 1*B*). In fact, we even recovered open burrows in this core. So, the HPC is an extremely successful tool that will allow us to use these cores to construct a detailed climatic record. What is the frequency and amplitude of various climatic sea-level changes? I am going to give just a few results.

One record I want to look at briefly is from the Caribbean site (for details of site 502, see Prell and Gardner, 1982). I should also note that, because these cores are undisturbed, we were able to do the paleomagnetics right on the ship with a long core spinner (Kent and Spariosu, 1982). What I have plotted here in figure 2 is the magnetostratigraphy and percent total carbonate for site 502. In the late Pleistocene of the Atlantic Ocean, this curve mimics the oxygen isotope curve; so, in many cases, it can be used as a proxy curve for glacial eustatic sea-level variation. It is not clear whether this relation is true throughout the whole sequence. In fact, it is probably not true. But the pattern that struck us, looking at the curve (fig. 2), is that a tremendous amount of variation occurs throughout this entire record. We are talking about 150 m worth of record here that goes back about 6 million years. If this record is related to sea level, and it certainly is a record of our depositional system, there is a lot of action going on judging by the high-frequency fluctuations. The pattern does not necessarily change in the places where we thought that it would. A variety of people have made a case that the initiation of northern hemisphere glaciation took place somewhere around 3 million years ago. So, you would expect the depositional system to respond, but this particular record does not. We plan to do some detailed oxygen and carbon isotope studies throughout this core, and hopefully, we will see a corresponding pattern.

One thing that we can do is look at the amplitude and frequency of some of the variations. What is going on in this Pliocene section (fig. 2)? It is somewhat different in character from even the late Pleistocene. We took an early Pliocene carbonate record, which is virtually continuous for about 30 m, and used the paleomagnetic events to construct an age model so that we could examine the time series of carbonate variation. We sampled this record at about 5,000-year intervals and did a spectral analysis of the carbonate time series (fig. 3). We compared this spectrum of Pliocene carbonate variations to two records (^{18}0 and carbonate) from another Caribbean core—V12-122—in which the age structure is well documented. In the upper Pleistocene, both the ^{18}0 record (fig. 3*A*) and carbonate record (3*B*) reveal the 100 kyr period that is the familiar ice age cycle that Broecker, Imbrie and many other people have talked about. We also see a very strong concentration of variance at 41 kyr which is the tilt periodicity. Several studies (for example, Hays and others, 1976) have documented that the orbital frequencies do occur in deep-sea sediments. The interesting observation in this Pliocene section is that we see an apparent orbital frequency that tells us that the tilt cycle may go all the way back through at least 4 million years. We do not really see that much variance at 100 kyr where we might expect it. If you look at the record closely, it is obvious that the 100-kyr cycle is not there, but a lower frequency of around 250 kyr does occur. So what we are seeing here is a suggestion, at least in this carbonate record, that the climate and therefore the sea level is not always in the same mode. The climate system may be nonstationary so that, at certain times, you see one mode of variation and, at other times, you see another mode of variation. To the degree that these modes translate into sea level, then climatic modes determine the importance of sea level to depositional models of the continental margin.

A similar example occurs in the Pleistocene section of hole 502 (fig. 4). Figure 4 shows the oxygen isotope record for *Globigerinoides sacculifer*, a surface-dwelling planktic foraminifera, a record of the total amount of forams in the core (essentially the fraction >62 microns), the total carbonate and a measure of foraminifer fragmentation (for details of these data, see Prell, 1982). The absolute values are not really so important here. I would like to focus on the overall amount of variability that you see in these records. What is striking about the ^{18}0 record is that you can see fairly high amplitude and long-period changes throughout the Brunhes, whereas, in the lower part of the record, you do not. The actual mean value is somewhat different; the amplitude

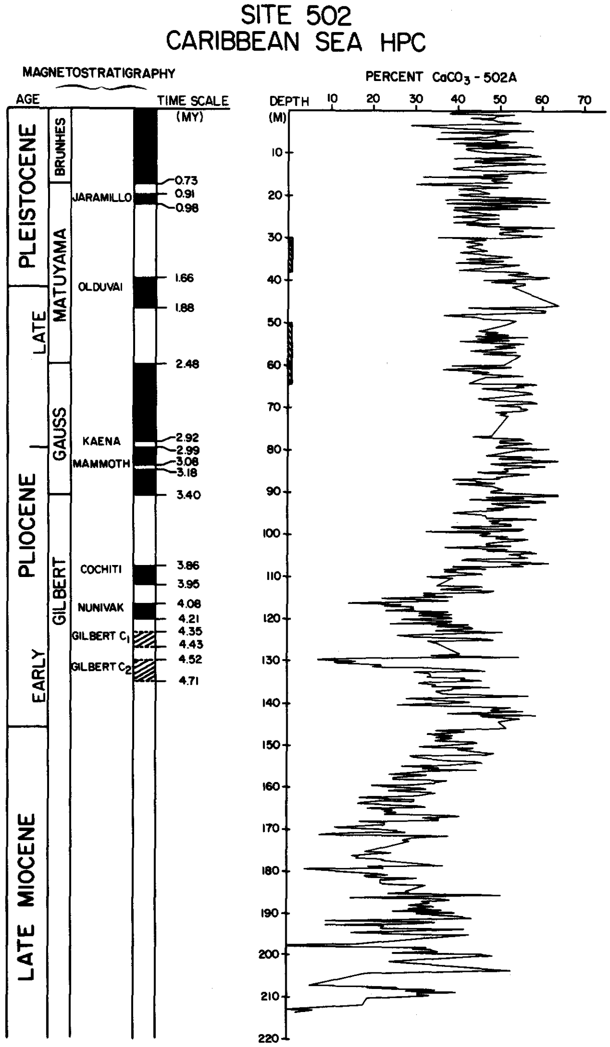

FIGURE 2.—The magnetostratigraphy and carbonate stratigraphy for Deep Sea Drilling Project (DSDP) site 502.

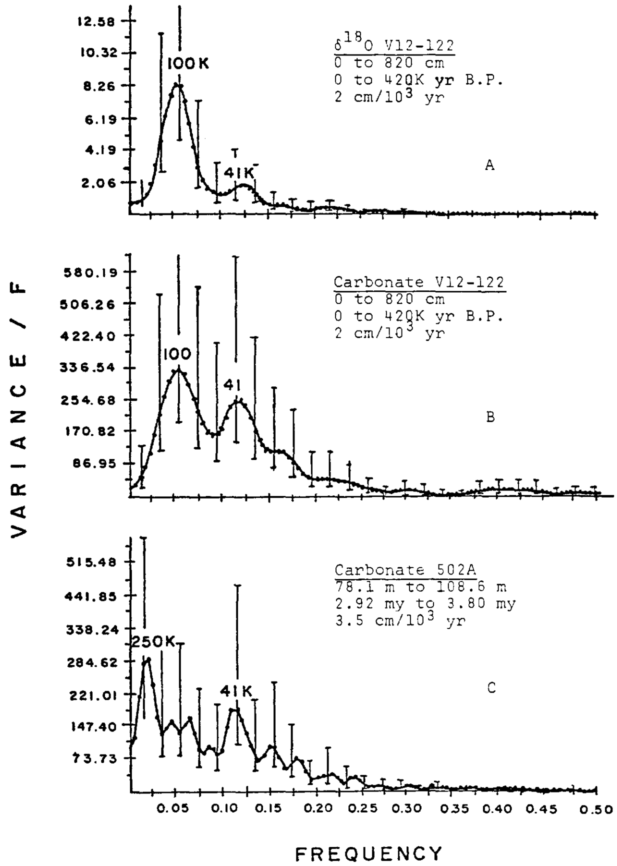

FIGURE 3.—Variance spectra for Caribbean oxygen isotope and carbonate variations. *A*. Oxygen isotopes for 0 to 420 kyr in V12-122, *B*. Carbonate for 0 to 420 kyr in V12-V122, and *C*. Carbonate for 2.92 my to 3.80 kyr in DSDP 502A.

is lower, and frequencies appear to be higher. Now this pattern was initially observed in a record documented by Shackleton and Opdyke (1976) from the Pacific; however, that record was fairly slow in accumulation rate. This record is more than twice the sedimentation rate of the Pacific core, so that we really are very sure that this difference in variability exists.

Menard: Before you get away from that one, there was not uniform sampling along the core if those black dots represent the sampling points. Do they?

Prell: Yes, they do.

Menard: So that flat-top long gap right about. . . .

Prell: Well, there are a couple places here where they represent gaps in our coring which were not filled yet.

Menard: So the heart of the absence of high frequencies in the upper part may be due to that sampling.

Prell: No, there's not that much of a gap.

To compare the records, I divided the section into late Pleistocene, which was the Brunhes Epoch, and early Pleistocene from the Jaramillo down to the Olduvai and looked at the means and standard deviations of the various components. The mean of the early Pleistocene oxygen isotope record is lighter; it is more depleted, and the standard deviation is approximately half of that for the late Pleistocene. However, other records, such as the coarse fraction and the carbonate, show the opposite sense. The means are not that much different, and the standard deviations in the older section, if

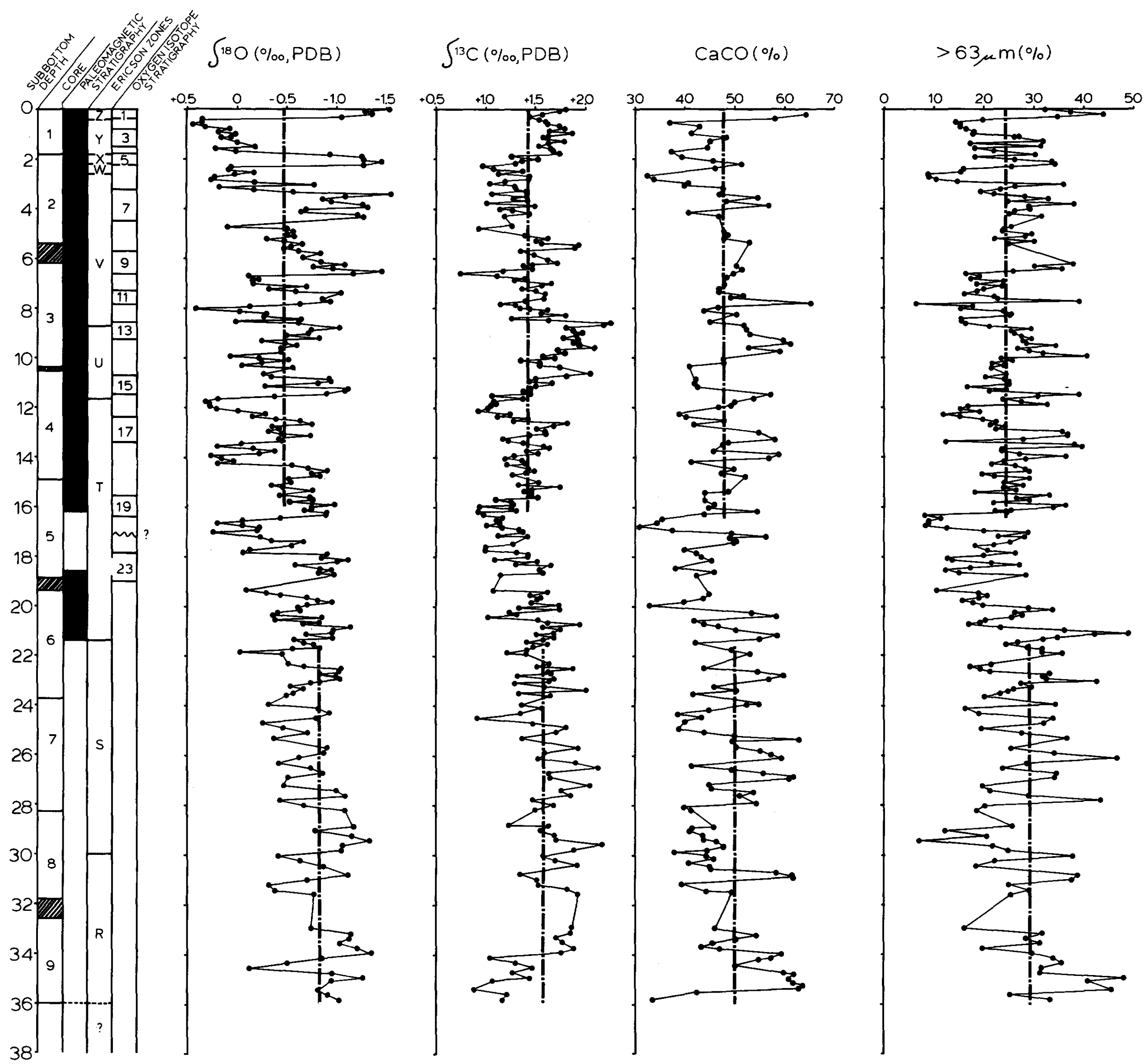

FIGURE 4.—Comparison of oxygen isotope stratigraphy with measures of carbonate dissolution and productivity. The ratio of test fragments to whole foraminiferal tests reflects carbonate dissolution, whereas, the percent of calcium carbonate and coarse fraction (>63 microns) reflects productivity, dissolution, and terrigenous dilution. All three measures exhibit different patterns in the early Quaternary and the late Quaternary. However, the fragment ratio displays two clear modes of variation. All variables change their relation (phase) with the isotope stratigraphy between early and late Pleistocene.

anything, are larger than they are in the upper section. Fragmentation is both higher and more variable in the early Pleistocene. These data suggest that the Pleistocene contains two different modes of oxygen isotope variation and carbonate preservation (fragmentation) but that modes of other sedimentary parameters, also related to sea level, are more subtle. I looked at the other cores, and not very many are comparable, to see whether this mode of ^{18}O variation stands up. This comparison shows that the available Atlantic (V16-205) and Pacific (V26-239) cores show the same pattern as site 502B. So this two-mode pattern is real.

What implications do these modes have for mass wasting or depositional models on the Continental Shelf? One way to think about it is to translate the oxygen isotope curve into an estimated sea-level variation, which is what I've done in figure 5.

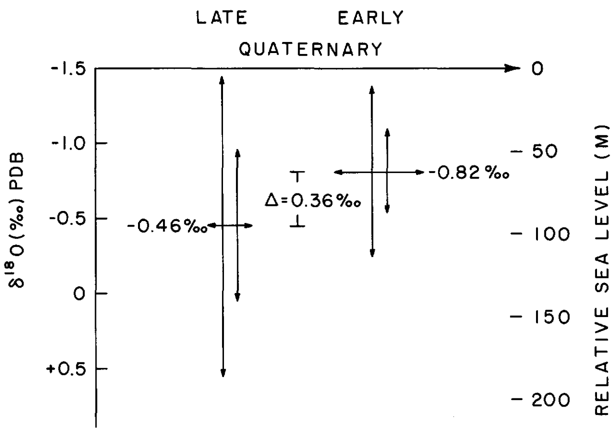

FIGURE 5.—The envelope of sea-level variation in the early and late Quaternary as estimated from isotopically derived sea-level changes. The calibration of 0.11 $\delta^{18}O$ per each 10 meters of sea-level change is from Fairbanks and Matthews (1978). One and two standard deviations around the mean for each mode are shown. The difference in means between early and late Quaternary is equivalent to 33 meters sea-level change.

The mean isotopic values for the late Pleistocene and for the early Pleistocene are shown scaled to a value of -1.5 per mil, which is the value we get for modern Caribbean surface sediments. I have actually zeroed everything to the modern ^{18}O value for *G. sacculifer* and used Fairbanks and Matthews (1978) calibration for the relation of ^{18}O to sea level; at this point, I am not taking temperature effects into account. So, you could probably reduce the total sea-level variation by maybe 20 percent for Caribbean cores. Not much temperature change occurs between glacial and interglacial times in the Caribbean—a couple of degrees centigrade. Comparison of the envelope (two standard deviations) of estimated sea-level change (^{18}O) reveals a range of about 182 in the late Pleistocene and 102 in the early Pleistocene. The difference in mean eustatic sea level in the early Pleistocene (-62) versus the late Pleistocene (-94) indicates that the shelf was subjected to different patterns of marine transgressions and regressions during these two intervals. Overall, transgressions and regressions in the early Pleistocene were limited to mid- and inner shelf, while those of the late Pleistocene covered the entire shelf, including the shelf break in many areas. These patterns of sea-level variation, in large part, determine the location and character of sediments that are susceptible to mass wasting. Again, these estimates are relative values that do not reflect isostatic rebound or loading of the margin. But the values are what we would interpret from deep-sea records as ice volume changes; therefore, they are eustatic sea-level changes. The clear implication of these data is that, as you go back through time, different modes of sea-level variations exist. Further, each mode will have a characteristic pattern of erosion and deposition that forms one of the preconditions for sediments subject to mass wasting. These records, for hydraulic piston cores, allow us to think about the real frequency and amplitude of the sea-level variations rather than just waving our arms and talking about high levels and low levels. Eventually, we'll do the detailed isotope analyses for the whole core and look at the actual spectral content of these cores. Then we will be able to say something quantitative about the amplitude and frequency of the different modes of sea level variations.

Results reported here were funded by National Science Foundation Grants ATM 78-25629 and ATM 80-18897 (SPECMAP) through the Climate Dynamics Section, Division of Atmospheric Sciences; The Seabed Assessment Program, International Decade of Ocean Exploration, Division of Ocean Sciences; and the Division of Polar Programs. Any questions?

Discussion following Prell's talk:

Coleman: After listening to Charlie Hollister and several other people yesterday, I've been going through the literature and finding out that, in the deep sea, we do not have an extremely tranquil type of setting. That is, there is obviously resuspension and movement and so forth. I think that's becoming clearer every day. A lot of work being done on the deep-sea cores makes the assumption that sedimentation rates are constant through time. How does that affect some of your computations when you then start trying to put time into that sequence? I mean everything is always linear and, if we go down the deep-sea cores. . . .

Prell: Well, linear accumulation is really the great step that we have to make to talk about the spectrum. What you do not realize is that when people talk about constant accumulation rate, they are talking about probably less than 1 percent of all of the cores that we have in our collections. I've personally gone through the entire Indian Ocean collection at Lamont, which is about 1,500 cores. Out of all those cores, probably fewer than 100 fall in the constant accumulation category. We are now developing, through the oxygen-isotope data, a stratigraphy with enough time datums to estimate an actual depth-versus-time plot to assess whether or not it is constant. But you cannot take any core and assume constant accumulation. That has to be proven. That should never be assumed.

Menard: It seems to me that Erickson said for the first 1,000 piston cores in the Atlantic, there was one in the Caribbean that he thought probably was a continuous record.

Prell: Well, that was the great irony of the whole coring effort at Lamont. When they first started, they thought they'd go out and take a couple of dozen piston cores and solve the whole Quaternary question. And 20 years later, Dave Erickson was still working away as hard as he could on the question.

Menard: I think you've given earthquakes a bum rap as far as not being predictable. They may not be the kind of quake we're concerned with, but in California now we are developing methods whereby you can get the recurrence intervals with some reliability.

Prell: Perhaps, but it depends on what you want to predict. I do not think I would want to predict the slumps on the continental margin on the basis of those yet.

Folger: But, certainly, frequencies on a broad basis you could. In the Alaskan situation, we looked for seismics as being a definite forcing function and one that you could count on quite regularly.

Prell: You can count on them regularly, but the point I was making is that if we can take some of these curves that I showed as proxy sea-level curves, then you know both the timing and the amplitude of when sea level is going to be a certain place. So you can say that I have got a certain type of depositional system on the shelf or on the slope. With an earthquake, you just—Boom!—you've got that one shock and that is it. Maybe you can predict that it will be in a 10,000-year or 5,000-year or even shorter time span.

Menard: Oh, no; you're predicting that it's something like 40 years plus or minus 30. No, it's probably a much better predictor than you've got with any of these methods.

Coleman: In your oxygen analysis also, what is the minimum level? Comparing oxygen to sea-level change, what is the minimum resolution? In other words, how much does the level of the sea have to change?

Prell: The calibrations between sea level and the oxygen isotopes have been done with isotopes on terrace corals where people know the elevation changes. For example, in Barbados and the New Hebrides. The calibration is about 10 m of sea-level change per 0.1 ^{18}O response. In the records that we look at, the reproducability of 0.1 per mil between replicate samples is about what most labs would expect to get. Sometimes, with some samples, you go a little bit better. But I would never want to talk about sea-level changes of 10 m.

Coleman: So what would you accept?

Prell: Well, I think between 20 and 30, especially in benthic foraminifera records. The benthics are subject to a much more constant environment than the planktonics. When you start to see variations of 0.15 to 0.2 per mil, which would be 15–20 m of sea-level change, that are reproducible from core to core in different oceans, you have confidence that the variations are significant.

Question: But do not Fairbanks and Matthews put a ± 20 on some of their own estimates from which you're calibrating? In their isotope paper, where they calibrate from the Barbados terraces to the deep sea, they put in, I think, a footnote of ± 20. Not on the 6-m 5E terrace, but on the other ones.

Prell: Yes, they include a possible error of 20 m due to possible temperature variation. This source of variability must be estimated independently. Also, the effect of possible floating ice shelves must be evaluated.

McGregor: How about the work that's being done now, using the satellite data, that's showing significant changes in the geoid height of sea level—on the order of 150 to 180-m changes over what appears to be very short scales. Is this going to mess up your 20 to 30-m changes in sea level?

Prell: Our forams do not know what the geoid is doing. What we are talking about is those changes reflecting essentially the total isotopic composition of the ocean which is, to a first degree, a function of the amount of terrestrial ice volume and a partial function of temperature. We have to be concerned about the global signal and not the local signal. I am talking about how much water is effectively in the ocean, or whether it is partitioned between ice and the ocean, and that's what they measure. They are not measuring sea level with respect to any mark.

McGregor: Physical thing. . . .

Prell: Right.

Bloom: You have to be careful the way you phrase that, with respect to the geoid changes. They are not showing short-term changes in time, but spatial changes. Most geophysicists would say that the geoid has bumps and hollows that are closely reflected by sea level but, unless you can move large masses around in the interior of the Earth, it is not likely that those are going to migrate. Now, there are people who invoke that as a mechanism for changing sea level, and they could be right, but there is no mechanisms to do it in time.

Prell: After listening to some of the talks yesterday, it struck me that its really important to look at the depositional system on the slope and rise. Not many traditional piston cores can be used to do high-resolution studies on the slope/rise sediments because few penetrate a full glacial/interglacial sequence due to the high accumulation rates. So we cannot even model what the depositional system is doing over 150,000 years, or something like that. So a series of hydraulic piston cores across the lower rise and slope would be extremely valuable to model the transport from the shelf and the continent to the continental margin.

Schlee: Has anybody gotten one of these cores across the Oligocene part of the Tertiary because it's supposed to be a major dip in sea level, according to Vail's curve?

Prell: Ours was the first leg that really used this [Vail's curve] extensively. Last year at this time, a core was collected in the South Atlantic on the Rio Grande Rise that we think should be a good section in this interval. I don't recall the details of the result.

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Geologic Analogs: Their Value and Limitations in Carbon Dioxide Research

Eric T. Sundquist

ABSTRACT[1]

Geologists have recently suggested that atmospheric CO_2 concentrations may have been higher or lower during several periods in the geologic past. These suggestions are based on data from sediments, as well as ice cores. They tend to confirm a general association between high CO_2 and warm climates. However, great caution is required in interpreting these periods as "geologic analogs" of the climatic effects of anthropogenic CO_2.

The time scales considered by geologists are generally much longer than those considered by current CO_2-climate models. Because sediments are deposited episodically, and are often stirred after they are deposited, the sedimentary record is usually inherently averaged or integrated over thousands of years. The time resolution of regional or global reconstructions is further limited by the accuracy of dating and correlating sediment sequences. Therefore, geologic evidence bears most directly on the long-term geochemical and climatic effects of anthropogenic CO_2.

From a consideration of the present distribution of carbon on the Earth's surface, and from calculations based on present relations among the

[1]1986, *in* Reichle, D.E., and Trabalka, J.R., eds., The changing carbon cycle—A global analysis: New York, Springer-Verlag, p. 371-402.

atmosphere, the oceans, and marine sediments, it is possible to approximate the magnitude of natural CO_2 perturbations characteristic of various time scales. With the possible exception of mass mortality events (such as might result from meteorite impacts), the only natural processes capable of generating large (2-fold or greater) atmospheric CO_2 excursions are those which operate on time scales of about 10^5 years or longer. From a CO_2 model expanded to include interactions with marine sediments, it can be shown that the global geochemical response to anthropogenic CO_2 may extend to a time scale of 10^4 years or more. Thus, the geologic record of long-term events is important not only because it may contain evidence pertaining to large CO_2 excursions, but also because the anthropogenic CO_2 perturbation itself may have significant long-term consequences.

Discussion following Sundquist's talk:

Embley: I just have a comment. Some large masses of sediment on some of the midocean rises are slumping. Magnuson and others, some years ago, published a paper suggesting that the solution of limestone in the deep sea caused failure of overlying material.

Sundquist: Yes, undercutting. And if what we're seeing in this model is correct, that sort of thing might be expected to increase over the next 500 years.

Edgar: What's the general assumption on the exchange rate of CO_2 in the atmosphere with the ocean?

Sundquist: Well, the gap exchange rate is such that either the surface ocean-mixed layer or the atmosphere CO_2 has a residence time of about 5 years with respect to that exchange. The reservoirs are about the same size, so that residence time here would be about the same.

Edgar: It's rapid.

Sundquist: Yes, it's very rapid, and, for a first approximation, you can say that as long as there's wind, the surface of the ocean is going to behave with respect to atmospheric CO_2 very much as if it were in equilibrium with it. And you've just seen me demonstrate it, by using all the models that I have. Any other questions?

Shackleton: How does this affect the lisocline, solution of carbonates, and accumulation rates?

Sundquist: In terms of dissolution as a reflection of the average carbonate-ion concentration of the deep sea, I think this model has something to say to us—that dissolution in the deep sea is certainly very sensitive, over perids of 500 to 5,000 years, to the addition or loss of CO_2. I think that the sudden increase in dissolution is an artifact both of the peculiar mix of foram species and their solution susceptibility, and it's a function of our being used to looking at the most conspicuous dissolution—calcite compensation, which is a sudden effect. But it's not difficult to show that what we see in the sediments is really just removal of the last few percent of the carbonate that was available.

Menard: Well, Keeling's curve for Moana Loa is magnificent as far as CO_2 increase is concerned, but temperature increases around the Earth did not show a corresponding one-for-one agreement; so part of this lack of agreement is attributed to increase in particulate matter in the atmosphere. Do you think there may not be balancing effects that would cause these calculations to be more uncertain than it appears?

Sundquist: Well, I think that's quite right, that there can be balancing effects, particularly due to the temperature effect. In fact, in this week's "Science", the lead article concerns whether we can detect at high latitudes, in the temperature record, a temperature increase that would correspond to the already known CO_2 increase. In fact, the monthly mean temperatures at 60°N., according to this paper, have decreased a couple of tenths of degrees during the last 70 years. The points made by the article are that, first of all, our predictions for the temperature increase don't take into account the oceans as a determinant, and, certainly, they are important. This is the next step in trying to predict mean low temperature increases and trying to do with the heat flux what Ewing has done with the carbon flux in terms of mixing in the deep layers of the ocean. Secondly, that there may be natural or anthropgenic influences on temperature, besides CO_2, that are offsetting the CO_2 influence.

Paleohydrologic Regimes in the Southwestern Great Basin, 0–3.2 M.Y. Ago, Compared with Other Long Records of "Global" Climate

George I. Smith

ABSTRACT[1]

Nine distinct paleohydrologic regimes in the southwestern Great Basin over the last 3.2 m.y. are recorded by the lacustrine deposits in KM-3, a 930-m core from Searles Lake, Calif. These are characterized as being "wet," "intermediate," or "dry" (like today). Excepting the present incomplete regime, each lasted 0.12 m.y. to 0.76 m.y. Major regime changes 0.01, 0.13, 0.6, and 2.5 m.y. ago appear to coincide with recognized changes in global ice-sheet histories as represented by the ^{18}O and other records from marine sediments, but comparable changes 0.3, 1.0, 1.3, and 2.0 m.y. ago do not appear to coincide closely with comparable perturbations in ice-sheet histories. However, all regime boundaries (during the last 1.75 m.y.) coincide closely in time with changes in sea-surface temperatures in the tropical Atlantic, and many coincide with other deep-sea and continental paleoclimatic boundaries. The average duration of these paleohydrologic regimes was about 0.4 m.y. (standard deviation, 0.2 m.y. or less, depending on assumptions), and it is suggested that the regime boundaries reflect times of change in global(?) sea-surface temperatures, possibly controlled in part by the Earth's 413,000-yr orbital eccentricity cycle.

During the wettest and driest regimes in the Searles Lake area, lake levels were not sufficiently affected by the 23,000-, 42,000-, or 100,000-yr climate cycles related to high-latitude ice-sheet fluctuations to produce changes in the lacustrine sediment character. During intermediate regimes, however, when lacustrine sedimentation in this area was more sensitive to climate, the sediments in KM-3 record lake fluctuations with average frequencies near those of the ice-sheets. This seems to indicate that the high-latitude ice-sheet fluctuations caused local climatic perturbations but did not dominate the hydrologic component of climate in this area.

Other lacustrine deposits in the southwestern Great Basin of California and Nevada have ages comparable in part to those of the wet to intermediate regimes indicated by KM-3, and they may all be products of finite periods when lake expansion, alluvial fan growth, increased spring discharge, and fluvial deposition were promoted in this area by widespread wet climates. Glacier expansion in the Sierra Nevada may also have been primarily an expression of, and in phase with, these wet regimes.

[1]1984, Quaternary Research, vol. 22, no. 1, p. 1–17.

Discussion following Smith's talk:

Peck: How much could tectonic uplift of the Sierra or disruption of the drainage system on the east side of the Sierra have an effect on that curve to make a difference?

Smith: A slight effect. Some work that's recently been done makes it look as if, at about 3 m.y. ago, the middle of the Sierra Nevada was about 1,000 m lower than it is at present; so that would decrease the size of the rain shadow east of the Sierras. And by taking a look at a lower-level meteorologic chart and looking at how much lower moisture might sneak over the top, it looks as if it might be about twice as much. But if the river drainage areas were anything like what they are now, which is a big if, we would need something on the order of five to ten times more moisture, rather than just two times more moisture to account for it. Now, on top of that, you could probably bring the baseline up and make sort of a sloping curve, but I don't see how you could really attribute either gradual uplifting of mountain ranges or things like stream capture to cause short-term events like this.

Edgar: Instead of comparing this with world ice volume, did you do any studies on the configuration of the North American ice sheet?

Smith: How much of a record do we have of the North American ice sheet? I don't think we really have much. We really have very little paleoclimatic data that goes back more than about 200,000 or 300,000 years. There's a huge blank spot.

Coleman: In an enclosed lake like that, the core you discussed had a continuous sedimentation rate. In conditions of changed runoff, how can you maintain a continuous sedimentation rate?

Smith: In a deep lake, sedimentation goes on more or less year after year as conditions change.

In a shallow lake or a salt lake, you first of all have very rapid sedimentation as the salt layer forms, possibly as much as a half meter a year. But after that, you get a continued accretion as all of the salt that is left high-and-dry around the basin continues to flush out with each rain and add to the salt layer. It's a feast-or-famine situation. If we knew the details of each of these ages, I'm sure you'd get something with salt in it.

Coleman: You went from a playa essentially to deep-water lake deposits, and I still can't see how, from a single core, you could have a constant sedimentation rate.

Liddicoat: What George is basing it on is the paleomagnetics. We don't have a slide to show it, but it does turn out to be a very linear sedimentation rate. It's striking, really.

Coleman: For example, if I put a sediment trap in a playa and also at the bottom of some of these deep lakes, would I get the same sedimentation rate?

Liddicoat: Maybe. Well, you probably wouldn't; depends on how much time you have.

Prell: I was not as pessimistic as you were about your comparison to the deep sea. What we showed with this long record is what Shackleton and Opdike showed—and there are some papers in press now—that you shouldn't look for a 100,000-year record beyond 700,000 years; it's not there in the deep-sea record, but the 40,000-year record is.

Smith: That's good to know, because I gave a talk to our people in marine geology at Menlo to the same effect. We're finding more of a recurrence interval on the order of 200,000 to 300,000 in some of the cycles. So it may be that, once I take off my 100,000-year glasses and take on a new sort of freshness, maybe there'll be more similarity. I can't conceive of this history being decoupled from the global picture; they must somehow be speaking to each other. In this kind of a record, here, you would pick up any temporary aberrations because if you have a dry lake, you can always flood it, but, if you have a flooded lake, you can't dry it.

Shackleton: Is that an indication of three soils, or something else?

Smith: There were three zones that had quite a few zones of glauberite in sort of patchy concentrations of what looks like a soil, maybe capillary water bringing it to the surface. This is more diagrammatic than anything else.

SEA-LEVEL CHANGES

Late Quaternary Sea-Level Change on South Pacific Coasts: A Study in Tectonic Diversity

Arthur L. Bloom

ABSTRACT[1]

Figure 1 and table 1 show only a selection of radiometric dates from a variety of coasts in the coral-reef zone of the southwest Pacific Ocean. For some sets of dates, sea-level curves have been drawn (Micronesia, from Bloom, 1970b, is similar to Florida, USA, from Scholl and colleagues, 1969; New Caledonia curve from Baltzer, 1970). Other data points only suggest trends. The Micronesian curve may be considered as a reference standard, based on islands of mid-Cenozoic volcanic rock far from any tectonic plate boundaries.

Western Samoa has subsided several meters relative to Micronesia, based on a series of radiocarbon dates from basal mangrove peat. The subsidence is consistent with historic effusive basalt volcanism and drowned archeologic sites in Western Samoa.

The area of the North Queensland coast in Australia, represented by a tentative curve, has been demonstrated by Hopley [1971] to be experiencing tectonic uplift. Western New Caledonia is also emerging. Some Holocene uranium-series dates (these are not converted to radiocarbon years) from Santo and Efate in the New Hebrides support many other lines of evidence that those islands are rising rapidly; rates of 2 m/1,000 years are common, and 1 m of local uplift during a single earthquake in 1965 is documented. New Ireland is violently seismic, but vertical uplift of mid-Pleistocene reef terraces is slight, and the Holocene dates are similar to many others. Tonga is also a seismic region, but Holocene emerged reefs were found only on Tongatapu and Eua. Other islands in Tonga have been stable or possibly subsided in the Holocene.

The array of data points, ranging from +12 m to -8 m in the last 7,500 years, should convince us that local tectonic history is the primary factor

[1]1980, *in* Mörner, Nils-Axel, ed., Earth Rheology, Isotasy and Eustasy: New York, John Wiley, p. 505–516.

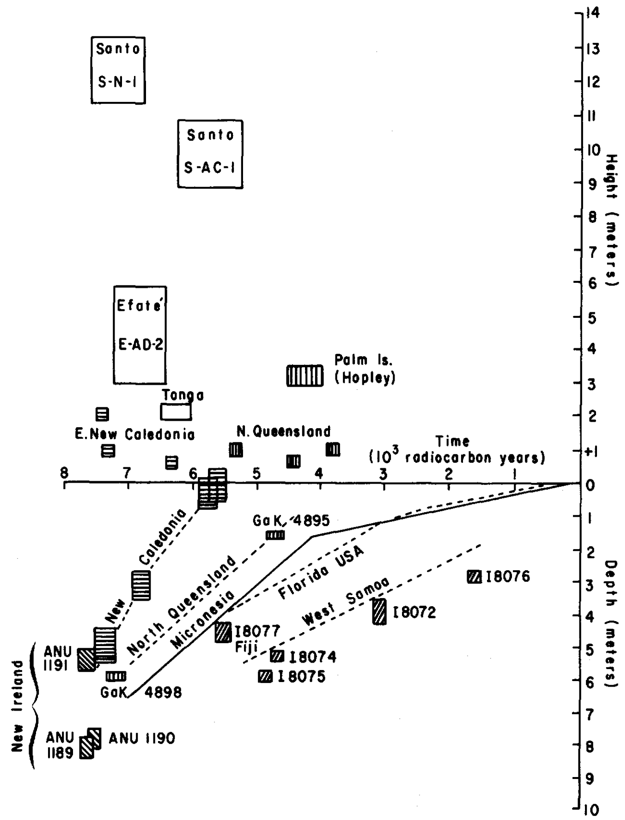

FIGURE 1.—Holocene sea-level changes on a variety of tropical Pacific coasts. Previously unpublished data are listed in table 1. Some dates are subject to minor corrections before final release by the dating laboratories.

in Holocene shoreline displacement of Pacific islands. It would be absolutely wrong to assemble a regional synthesis of sea-level history by connecting the array of data points shown by the figure.

INTRODUCTION

The coral-reef zone of the South Pacific has been the focus for a continuing controversy about Holocene sea levels (see Russell, 1961) with current opinions no longer polarized into two or three "schools of thought" but spread across the full range of possibilities. Because the controversy about Holocene sea-level change concerns a vertical range of no more than 3 m from present sea level, the resolution is easily confused by shore-zone deposits of storm and tsunami debris, especially on coral coasts. Local tectonic movements in island arcs are also likely to confound the record, as are possible changes in tidal amplitudes or geoid perturbations caused by as yet unknown processes deep within the Earth (Morner, 1976).

A constructive new approach to Holocene sea-level studies in the Pacific has been provided recently by Clark, Farrell, and Peltiet (1978) and Clark (1980). These authors devised a model of the deformation of the geoid and the Earth's surface in the last 16,000 years as a result of viscoelastic isostatic responses to water and ice loads. They predicted a zone of slight emergence in the South Pacific as a result of global mass transfer and isostatic adjustment without any change in the budget of ocean water and continental ice. Their theoretical model is sure to be quoted by field workers to support new findings about the Holocene sea-level history of individual islands. However, the best theoretical model cannot replace accurate field observations and reproducible radiometric dates. We can hope that the day has passed when a discrete event on an oscillatory eustatic sea level curve is inferred from a single radiocarbon date from a single island, as has been done in the past. The fundamental scientific test of reproducibility must be applied.

In the arguments about the history of eustatic sea-level changes, the tectonic history of various islands has either been ignored or assumed to be negligible in the few millennia of the Holocene. The purpose of this paper is to demonstrate that a selection of South Pacific coastal segments have each had a distinct late Quaternary tectonic history, and the resulting position of sea level during the past 7,000 years may range from −9 m to +4 m, a range five times greater than the usual predictions of eustatic change. Some of the regions to be described are obviously tectonic, and others would be candidates for "stable" coasts. Even in the obviously tectonic regions, the Holocene record of sea-level change is not what might have been expected. The lesson to be learned is that before a global or ocean-wide eustatic sea-level curve can be constructed, even one that includes viscoelastic isostatic compensations and corrections for deformations of the geoid, the local tectonic environment of individual islands and coastal segments must be understood and appropriate corrections made.

Discussion following Bloom's talk:

Cronin: In relation to your ideas on hydroisostasy and Walcott's calculations for possible uplift for

TABLE 1.—*Radiometric ages and sample data used to compile figure 1*

[Some of the dates may be changed slightly by the dating laboratories prior to final release]

Locality	Sample no.	Method	Age	Depth (m) or altitude (m)
Micronesia	see Bloom (1970a)			
Florida	see Scholl, Craighead, and Stuider (1969)			
Western Samoa	I-8072	C^{14}	3060±95	3.6–4.3
	I-8074	C^{14}	4655±95	5.2–5.5
	I-8075	C^{14}	4845±95	5.8–6.1
	I-8076	C^{14}	1595±85	2.7–3.0
Fiji	I-8077	C^{14}	5500±110	4.3–4.9
New Ireland	ANU-1189	C^{14}	7580±100	7.8–8.4
	ANU-1190	C^{14}	7490–±90	7.6–8.2
	ANU-1191	C^{14}	7600±160	5.1–5.8
N. Queensland, including Palm Island	GAK 4895	C^{14}	4680±135	1.5–1.7
	GAK 4896	C^{14}	2180±90	2.4–2.6
	GAK 4897	C^{14}	1350±80	3.6–3.8
	GAK 4898	C^{14}	7130±150	5.9–6.1
	see also Hopley (1971; 1974)			
New Caledonia	see Baltzer (1970); Coudray and Delibrias (1972); Launay and Recy (1972)			
Tonga	see Taylor and Bloom (1977)			
New Hebrides	Santo S-N-1	Th^{230}/U^{234}	7100±400	11.5–13.5
	Santo S-AC-1	Th^{230}/U^{234}	5700±500	9.0–11.0
	Efate E-AD-2	Th^{230}/U^{234}	6800±400	3.0–6.0

deglaciation—would you expect remnants of some of these interglacials onshore, maybe where they had not been removed, since really there's a possibility of it being preserved there where it hasn't been swept out by the last glacial?

Bloom: I think that, as powerful a tool as the idea is of isostatic compensation to water loads, I think it's a really great idea, and it's been extremely useful. But remember that it's a response to a load that is added and removed and added and removed. That is, it will not create any permanent displacement between last interglacial and present, because we supposedly returned to equilibrium at the last interglacial; then we unloaded the shelf (maybe it responded; it rose). Then we melted the ice shield; we put water on it, it sunk back. But it always goes back to the same position; there's no net gain or loss from that process.

Cronin: Because of relaxation.

Bloom: It goes to completion, yes. Like any isostatic response to a load, it finishes so that you shouldn't expect to find any successively older interglacials at higher levels due to that process. If you do, it's certainly due to other processes. It's certainly due to general, long-term post-Miocene uplift.

Cronin: Yes, but given the subsidence rates of 2–4 cm offshore, as you move inland, they should decrease. But should they hang up?

Bloom: Well, they might even inflect because the engineers will argue about a flex-beam model that will give uplift in them. But it's only returning to where it was before the load was put on; it's not going to add up inland. But if, during a full glacial, you unload the shelf, it simply goes back down and melts the ice sheets and comes back up. . . .right where it was before.

Cronin: Does that depend on the view of the core-mantle interface? Or is that. . .

Bloom: No, it's supposed to be an upper mantle phenomenon.

Emery: Your curves and many others suggest that the time of the lowest sea level was late Wisconsinan (that's 19,000 years), and always it's based on the trees knocked over by glacial advance 19,000 years ago. I have not seen similar data for the maximum extent of ice sheets elsewhere in the world except in, say, Greenland. We really don't know, unless you know of some data of trees knocked down in Europe and other places. If you don't, I kind of hesitate to accept too strongly that 19,000-year date.

Bloom: 15,000 or 20,000 would make me happy, though in one long review paper I wrote for the Flint volume, I added up the areas, put gridded paper over the areas of all the ice sheets, and summed them all up. And I got 15,000 too, because the whole western part of the ice sheet is much younger. No, I think it's a number like 15,000—but it's not critical to the argument.

Emery: No, but I think we have to look critically in as many ways as we can for this time of lowest sea level. It's hard to get it from the shelf, because oysters didn't live at that time—at this latitude anyway; too cold for them—and it wasn't a big area.

Bloom: But a date in that time range is going to have about a 1,000-year rut around it anyway. I agree with you. I usually say "15,000 to 18,000 years ago" because that's better than saying "15,000 to 20,000." I probably said 18,000 because that's the CLIMAP number. That's what the rest of these people relate to.

Uchupi: With respect to the Pleistocene stratigraphy on the shelf, can you say there's no pre-Wisconsinan material there?

Bloom: I came here to find that out. I haven't heard it yet, but I'm listening.

Uchupi: I remember going back to the old high-resolution data; there you can recognize five to six depositional sequences, thickness in all of several hundred meters. I can't believe that that represents all of the Wisconsinan.

Bloom: I wouldn't either. Where was that?

Uchupi: That's around the area of Hudson Canyon. And that's one little area. I'm sure since then other areas have been looked at where you see the sort of detail where you could talk about pre-Wisconsinan material.

Bloom: Dot [Marks], what was the argument about—was it COST-2? Which well did you look at?

Marks: It was USGS Atlantic margin coring (AMCOR) hole 6021, and there just isn't a date in there at all.

Bloom: How thick is the section?

Marks: It's 300 m; it's being called Pleistocene.

Hathaway: It's Pleistocene at the bottom of the hole, but the whole section hasn't been looked at in detail yet.

Bloom: The fact that it looks like it might be unstable, or overpressured at depth as implied from what we've had from the engineering talks here. That's young, rapid sedimentation, very recent.

Uchupi: One thing you can do is to look at the coastal plain, see the distribution of the glacial debris on top of the coastal plain deposits. And in deep water: if you were to drill a hole, and it just happens that it's located right where the coastal plain is exposed; and then, from there, you would extrapolate that there's no glacial interval on the coastal plain. The Pleistocene stratigraphy on the shelf is probably very complex, and we only got a couple of places where it has been looked at.

Bloom: On the gulf coast, the Prairie surface is a real marker of that last interglacial. It's consolidated; it's weathered; it's a tough yellow zone that they can map. And I haven't seen any clue that there is such a surface on the Atlantic. I suspect that the construction people would like to know there was such a surface on the Pleistocene sediments on the outer Atlantic shelf, but I haven't seen any indication yet that anybody has found it.

Hathaway: Is Joe Liddicoat here? Joe, can you talk about anything in 6021?

Liddicoat: This is the one that's supposed to be Pleistocene, and what I did was sample it all the way down; it is normally magnetized. So that just means it's less than 700,000 years.

Menard: Before you get away—you didn't talk about earlier into Pleistocene or earlier sea level. What do you think was the maximum drop in sea level in the last several hundred thousand years?

Bloom: In that respect, I'm really impressed with the oxygen isotope record, that all of those ice volume changes were of a comparable magnitude, and I can live with 100 $\pm$30 m, because that covers about everybody's estimate. And I think, considering what we know about it, that's not bad. That's what my graph showed here and, at least for a working model, I find that's the best I can do. I think it would be unrealistic [to do otherwise now]. I don't like the older idea of a general fall of late Cenozoic sea level with the oscillation superimposed. I don't think there's any justification in the oxygen isotope record, or in the record of glaciers on land, of any progressive growth of ice volumes throughout the late Pliocene and Pleistocene. The big Antarctic ice sheet was there by Oligocene or Miocene, and it's been essentially impassive since. And as Eric's ice volume showed, Greenland is only 6 or 7 m, West Antarctic 4 to 6 m of sea-level equivalent. Those are the things that have been fluctuating, plus the Northern Hemisphere ice sheets.

Menard: You'd have to appeal to another mechanism, like spreading.

Bloom: Yes, and there we get into Vail's type of argument, or Pitman's. Yes, those are interesting arguments. They are much longer time scales. Nobody is going to change plate motions on a scale of 100,000 years.

Cronin: They estimate about a 300-m drop since the Cretaceous high, which gives a relatively small component.

Bloom: Right. We wouldn't even see it on the 100,000-yr time scale. I can still stick with my tectonic rates, appearing to be constant on the time scale in which I'm working.

Emery: Of course, that implies that the average depth of the shelf may have significance.

Bloom: Yes, I think it must have. [It averages] 130 m and that, for me, up in this part of the world, is a good magic number.

Emery: The only problem is it varies from 0 to about 400.

Bloom: Well, it's interesting that in New Guinea, at least, Chapelle had a drowned reef. The large drowned reef and lagoon complex offshore, right off those terraces, is at 135-m depth, and Chapelle made quite a point of that. It looks like it's the low sea-level reef from that time, so that's almost a coincidence; but it's in the record.

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Oxygen Isotope Methods and Sea-Level Changes

Nicholas Shackleton

Shackleton's talk mainly concerned the applications of the oxygen-isotope method to determine sea-level curves. He listed and explained the factors that allow a conversion of the isotopic composition of sea water to the volume of ice involved in glacial maxima.

Most detail and larger extremes in values are found in cores collected in areas of higher sedimentation rates. Thus, the reason that the 100,000-yr cycle appears to be more prominent could be because bioturbation has removed the variation of the 20,000-year cycle and they appear smaller than the 100,000-year periodicity.

The hydrostatic piston corer now being used aboard the *Glomar Challenger* has permitted much better detail to be worked out in the comparatively undisturbed sediment.

In a piston core taken from the Panama Basin, Carnegie Ridge, a 7,000-year record was recovered. The two different species of foraminifers that were analyzed gave different absolute values;

nevertheless, relative changes in temperature were similar.

In high-sedimentation-rate areas, the time scale is based on an assumption of sedimentation rate. The amplitude of change in $\delta^{18}O$ is 1.6–1.8/mL. Highest variability occurs in times of glacial maxima. For example, the Greenland ice cores show greatest variability in ice volume during the glacial maximum, with greatest extremes just before the final melting (25–15,000 years ago).

Shackleton then discussed the isotopic composition of materials in reefs of Australia, New Guinea, and Barbados. Mollusks or coral from reef crests show differences from values obtained from the deep sea with $\delta^{18}O$ offsets of 0.4 per mil. This may be due to the storing up of ice in the Arctic where the displacement of sea by ice would maintain sea level or by a change in the sea-water temperature.

Rapid Sea-Level and Climate Change: Evidence from Continental and Island Margins

Thomas M. Cronin

ABSTRACT[1]

Evidence for Quaternary sea-level fluctuations from emerged coral reefs, continental shelves, upper continental slopes and deep-sea oxygen isotope studies indicate that sea level has risen and fallen at rates of 1 to 3 cm/year and probably faster during periods of most rapid climatic change. Sea level during the glacial maximum (about 18,000 years ago) and during the initial period of deglaciation (18,000–13,000 years) is poorly known, but this may have been a time of relatively rapid sea-level rise. Most sea-level studies, however, lack paleoclimatic data, and thus the relationship between sea level and climate for intervals of 10^4 to 10^3 years remains unclear.

Marine deposits and terraces from the last interglacial complex 140,000–70,000 years ago occur above present sea level to about -20 m below sea level along many island and continental margins. Nevertheless, a eustatic curve cannot be constructed due to vertical crustal movements and dating uncertainty. Sea level probably fluctuated near its present level for much of this interval and, on several occasions, briefly dropping below -20 meters. Four peaks in sea level occurred during this interval but each may not be recognized on a particular coast. Future sea-level studies should focus on stratigraphic and paleoclimatic studies of key intervals of ice growth and decay using marine deposits along continental and island margins. Because sea level itself cannot be used as a monitor of paleoclimate, these studies should emphasize sediments containing both relative sea-level indicators and paleoclimatic data such as pollen and marine microfossils.

[1]1983, Quaternary Science Reviews, v. 1, p. 177–214.

Rates and Possible Causes of Neotectonic Vertical Crustal Movements of the Emerged Southeastern United States Atlantic Coastal Plain

Thomas M. Cronin

ABSTRACT[1]

Emerged Pliocene and Pleistocene shorelines and associated marine deposits were used to determine the magnitude and rate of vertical crustal movement during the past 3 m.y. in the United States Atlantic Coastal Plain of South and North Carolina. On the basis of a new regional ostracode assemblage zonation, planktic biostatigraphic data, and radiometric data, emerged marine deposits were determined to be primarily interglacial and can tentatively be correlated with hemispheric warm intervals in evidence from deep-sea data.

The paleontologic evidence indicates a primary glacio-eustatic component to the local sea-level record and a secondary tectonic component. Net vertical uplift rates averaging 1 to 3 cm/1,000 yr, but perhaps as high as 5 to 10 cm/1,000 yr, are in evidence for the emerged coastal plain. Although details of the timing of regional rheological events remain obscure, the trend of net uplift contrasts with general subsidence rates of about 2 to 4 cm/1,000 yr since the Cretaceous in submerged parts of the continental margin near the subsiding sedimentary troughs. Hydroisostatic crustal

[1]1981, Geological Society of America Bulletin, Part I, v. 92, p. 812–833, 11 figs., 2 tables.

response to multiple deglaciation events may have periodically uplifted the coast, but long-term lithospheric flexural upwarping in response to sediment loading offshore is a more plausible mechanism to explain the present positions of shorelines above present sea level. A eustatic sea-level model is proposed for interglacial high stands of the past 3.0 m.y.

Quaternary Climates and Sea Levels of the U.S. Atlantic Coastal Plain

Thomas M. Cronin, Barney J. Szabo, Thomas A. Ager, Joseph E. Hazel, and James P. Owens

ABSTRACT[1]

Uranium-series dating of corals from marine deposits of the U.S Atlantic Coastal Plain coupled with paleoclimatic reconstructions based on ostracode (marine) and pollen (continent) data document at least five relatively warm intervals during the last 500,000 years. On the basis of multiple paleoenvironmental criteria, we determined that relative sea-level positions during the warm intervals, relative to present mean sea level, were 7±5 m at 188,000 years ago, 7.5±1.5 m at 120,000 years ago, 6.5±3.5 m at 94,000 years ago, and 7±3 m at 72,000 years ago. The composite sea-level chronology for the Atlantic Coastal Plain is inconsistent with independent estimates of eustatic sea-level positions during interglacial intervals of the last 200,000 years. Hydroisostatic adjustment from glacial-interglacial sea-level fluctuations, lithospheric flexure, and isostatic uplift from sediment unloading due to erosion provide possible mechanisms to account for the discrepancies. Alternatively, current eustatic sea-level estimates for the middle and late Quaternary may require revision.

[1]1981, Science, v. 211, p. 233–240.

Late Quaternary Sea-Level Curve: Reinterpretation Based on Glaciotectonic Influence[1]

William P. Dillon and Robert N. Oldale

ABSTRACT

High-resolution seismic-reflection profiles obtained between Chesapeake Bay and Long Island along 2,500 km of trackline appear to show tilting of a large lithospheric block within the United States eastern continental margin. This warping probably was related to glacial loading and unloading. It has resulted in postglacial changes in depth at locations where radiocarbon-dated samples have been obtained and thus has affected the eustatic sea-level curves deduced from these samples. Corrected values indicate significantly shallower depths in the older (deeper) parts of the United States east coast curve and thus a revised estimate of global sea-level lowering during Wisconsinan time.

SUBMERGED SHORES OF UNITED STATES EASTERN CONTINENTAL SHELF

Several low escarpments on the Continental Shelf near Hudson Canyon were identified by Veatch and Smith (1939) and named the Nicholls, Franklin, and Fortune shores. These features were subsequently observed and extended by several authors (Ewing and others, 1960; Donn and others, 1962), and a fourth shore, the Block Island shore, was identified by McMaster and Garrison (1967) on the inner shelf off Rhode Island. Emery and Uchupi (1972, fig. 21) attempted to extend these four shores southward using bathymetric charts. However, their extension of the Block Island shore appears to be incorrect, as observed in our profiles (fig. 1), because depths to this shore south of Long Island are consistently deeper than at the "type locality" nearby, south of Block Island. Therefore, we propose that the name Block Island shore be restricted to the feature described by McMaster and Garrison (1967) and that the landwardmost shore shown in figure 1 be called the Altantis shore after the R/V *Atlantis II*.

[1]1978, Geology, v. 6, p. 56–60.

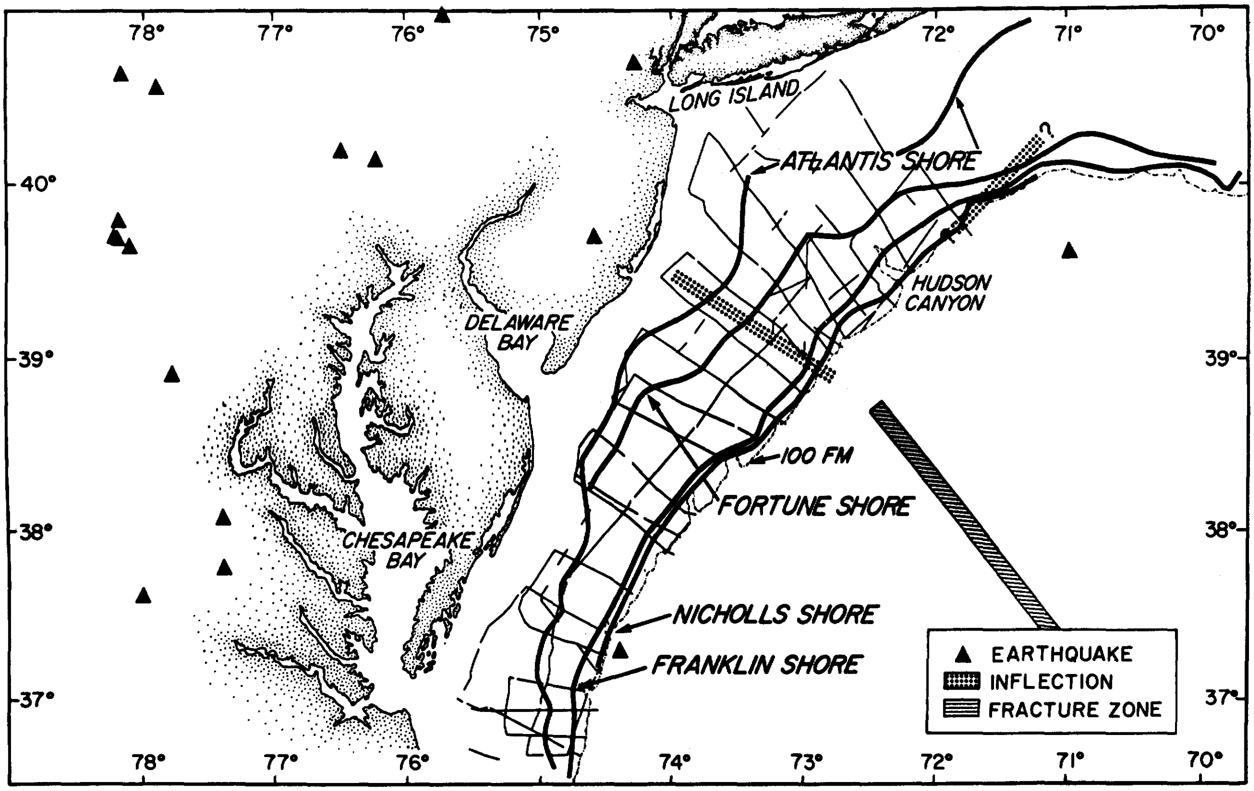


FIGURE 1.—Locations of tracks of U.S. Geological Survey high-resolution profiling survey and shores identified on those tracks. Eastward extensions of shores are based on data from University of Rhode Island Graduate School of Oceanography and Woods Hole Oceanographic Institution. Earthquake epicenters are for period of January 1961 to June 1974, obtained from National Geophysical and Solar-Terrestrial Data Center, Boulder, Colo.

The shores are both erosional and depositional, the results of wave activity on unconsolidated Pleistocene deposits during the last rise of the sea as the Wisconsinan glaciers waned (Garrison and McMaster, 1966; Knott and Hoskins, 1968; Emery and Uchupi, 1972). They probably are fragile enough to have been erased, had they been formed during a prior sea-level fluctuation. The bases of the scarps probably were formed at an approximately constant depth of a few meters below sea level, dependent on effective wave base. A similar change in slope is observable near the present shoreline. Formation of the shores probably relates to still stands in the eustatic rise in sea level, that in turn may be related to changes in the rates or retreat of the Wisconsinan ice.

WARPING OF CONTINENTAL SHELF

The discussion above implies that a shore represented an approximately level line when it was produced, and that present variations in depth must have resulted from subsequent warping of the Continental Shelf. Therefore, depths of the scarps may be rather sensitive indicators of tectonic warping. In order to examine such effects, depths at the bases of each shore were measured at each occurrence on the seismic profiles and the values projected to a line paralleling the shelf trend (026 °T) along the midshelf. Depths to the shores along this line of projection are shown in figure 2. In general, the shores are approximately horizontal south of an inflection zone off central New Jersey, whereas north of this zone they appear to slope uniformly downward north-northeastward. Location of the inflection zone is shown in figure 1.

North of the inflection, the shores dip more steeply toward the northeast, and dips are steeper on the deeper and, therefore, older shores. Thus, tilting was occurring during the time that the shores were being cut and continued up through the cutting of the shallowest shores. Approximate ages of the shores were estimated by comparison

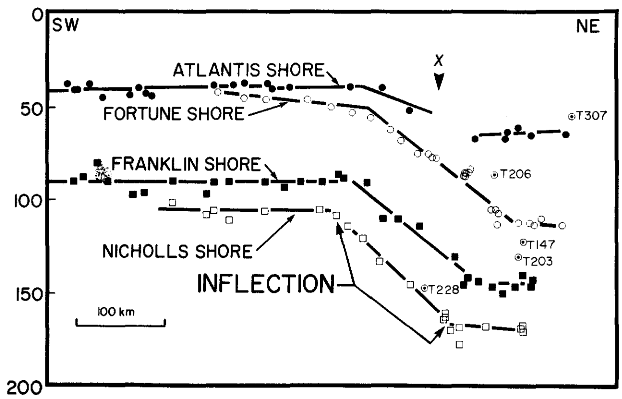


FIGURE 2.—Depths of shores (m) projected to line paralleling continental margin (026°T). Projected locations of radiocarbon-dated samples are shown by numbered encircled dots.

of their depths with radiocarbon dates of samples assumed to have been formed near sea level on the shelf nearby (Emery and Garrison, 1967). Ages of the Nicholls, Franklin, and Atlantis shores are estimated at about 15,000, 13,000 and 9,000 years, respectively. We assumed that the level part of the shores, south of the inflection zone, represented undisturbed shelf, although it probably has moved vertically somewhat owing to loading by sea water as sea level rose (Bloom, 1967; Walcott, 1972a). Average tectonic subsidence rates due to tilting toward the north, eliminating eustatic effects, and overall subsidence or uplift effects due to water loading were calculated at the location marked X in figure 2 for the time periods between formation of the shores. The approximate average tectonic subsidence rates at location X were 10.5 mm/yr between 13,000 and 15,000 years ago, 3.8 mm/yr from 9,000 to 13,000 years ago, and 1.7 mm/yr from 0 to 9,000 years ago. Of course, points north of X subsided at greater rates.

CAUSE OF SUBSIDENCE

The downwarping is apparently the subsidence of a Wisconsinan age forebulge that had bowed up in compensation for glacial loading of the crust to the north. Evidence for concurrent subsidence of such a peripheral bulge around an area of glacial rebound has been presented for Fennoscandia (Flint, 1971, fig. 13-2), Great Britain (Valentin, 1953), and eastern North America (Fairbridge and Newman, 1968; Walcott, 1972b). The suggestion that the shelf south of New England has subsided in response to glacial rebound to the north has previously been made by Garrison (1967), Emery and Garrison (1967), and Emery and Uchupi (1972). The deceleration in subsidence since the beginning of glacial retreat, as noted above, also suggests a relation to ice deloading. We do not believe that the inflection is due to any long-standing tectonic process, one suggestion of Belknap and Kraft (1977), because a seismic-reflection profile across the inflection showed no significant warping or breaking of reflectors inferred to be as old as Cretaceous, to depths exceeding 1 km, and because the subsided area (fig. 2) is not located at the zone of major geosynclinal subsidence of New Jersey (the Baltimore Canyon Trough).

STRUCTURAL IMPLICATIONS OF INFLECTIONS

The theoretical analyses of crustal loading (McGinnis, 1968; Walcott, 1970) show that the deflected crust of a forebulge will join undeflected crust by a smooth curve, if an unfractured elastic or viscoelastic lithosphere is assumed. Therefore, we believe that a zone of weakness must exist to produce the distinct inflections observed in figure 2. Several other factors also suggest lithosphere weakness at these locations. First, few earthquake epicenters occur on the westward extension of the zone of inflection crossing New Jersey (see fig. 1). Second, in the same region, acoustic basement abruptly becomes deeper, as observed on a 48-channel, longshelf seismic profile (J. Schlee, 1977, oral commun.) Finally a fracture zone, which was identified by offsets of magnetic anomalies at sea, intersects the continental margin at this zone of inflection (fig. 1; Klitgord, 1977, oral commun.). Finally, a fracture zone, which was identified by offsets of magnetic anomalies at sea, intersects the continental margin at this zone of inflection (fig 1; Klitgord, 1977, oral commun.).

The tilt of the Fortune shore south of the inflection point off New Jersey may represent the effect of partial coupling across the zone of weakness in the midshelf region, whereas the zone of weakness seems to represent a zone of total decoupling beneath the inner shelf (Atlantis shore) and outer shelf (Nicholls and Franklin shores).

The plot in figure 2 also shows a second inflection northeast of the first, at which the dip of the shores decreases to nearly horizontal. This inflection has been found to trend northeast north of Hudson Canyon (fig. 1). It continues and extends the trend of the shelf edge south of the canyon (100-fm curve in fig. 1), although the shelf edge in this northeastern region begins to curve abruptly eastward. East of this second inflection, the shores are nearly level to at least long 70°W. A series of northeast-trending basement horsts and grabens is interpreted to exist in this area, east of Long Island, on the basis of magnetics (K. Klitgord, oral commun.). We infer that one of the boundary faults associated with these was reactivated to create the inflection. Thus, it appears that the two inflection zones may define the southwestern and southeastern borders of a block of the Continental Shelf south of New England that moved somewhat independently under stresses associated with glacial-ice loading and unloading.

ADJUSTMENT OF EAST COAST SEA-LEVEL CURVE

The demonstrated differential subsidence of part of the Continental Shelf south of Long Island has a profound effect on the previously accepted eustatic sea-level curve for the United States east coast. Figure 3 shows the locations of samples dated by carbon-14 and the sea-level rise curve derived from them by Emery and Garrison (1967). It is apparent that the sample points that control the deepest part of the curve all come from locations within the area of shelf subsidence. Emery and Garrison (1967) and Garrison (1967) concluded that this area of shelf may have subsided, but later this was contradicted by Milliman and Emery (1968), who concluded that the data could be used to obtain a eustatic sea-level rise curve. The latter conclusion was based on two pieces of information. First, other radiocarbon-dated samples from elsewhere on the east coast were found to fit the ages and depths of the samples taken off Long Island. Second, the curves derived for the east coast fit fairly well to ages and depths of radiocarbon-dated samples from much of the rest of the world.

In regard to the first point, a recent paper by Macintyre and others (1978) discussed the samples that were dated to obtain the deep points which reinforced the Emery and Garrison (1967) curve. Macintyre and others concluded that at least four of the data points that were thought to be in agreement with the points off Long Island were associated with erroneously deep depths. These samples, obtained off the southeastern United States, were originally interpreted to be beachrock and near-surface algal deposits, but actually they probably represent material that formed at intermediate subtidal depths. Therefore, as indicated in figure 4A, all samples used to construct the curve for Holocene sea-level rise on the United States east coast that represent water depths greater than about 85 m apparently have been related to erroneously great depths.

The plot of shore depths (fig. 2) gives us a means of correcting the depths associated with the Emery and Garrison (1967) data points for subsidence of the forebulge. For example, if a dated sample plots at one-third of the vertical distance from the Nicholls to Franklin shores at point X in figure 2 (a depth of about 150 m), then its depth should be corrected to one-third of the distance from the Nicholls to Franklin shores to the south of the inflection zone (a depth of about 100 m). In applying this method, the assumptions are (1) the level parts of the Atlantis, Nicholls, and Franklin shores are tectonically undisturbed and (2) the rate of shelf subsidence due to tilt was constant from one formerly horizontal reference line (a shore) to the next. In fact, it is apparent that the rate of tilt was decelerating during the Holocene eustatic sea-level rise; however, a graphical analysis of this inaccuracy shows that it creates a maximum error of only a few meters in corrected depth and, thus, need not be considered. Corrections applied to some of the samples according to this technique are shown in table 1 and by arrows in figures 3 and 4.

We have replotted (fig. 5) depth and age values reported by Emery and Garrison (1967), Milliman and Emery (1968), and Macintyre and others (1978), but we have used our corrected values and have eliminated the four samples that Macintyre and others (1978) have indicated to be erroneously deep. This procedure has eliminated all points representing depths greater than about 80 to 85 m and has reduced considerably the scatter of points reported by Emery and Garrison (1967) and Milliman and Emery (1968). The points have been classified as fixed or mobile, following the concept of Macintyre and others (1978), who demonstrated that shells on the Continental Shelf commonly are transported shoreward, and thus depth associated with a shell sample is likely to be less than the depth of formation. Therefore, we have sketched our curve near the bottom of the scatter of mobile points. Only one deep value is available on the United States east coast between 16,000 and

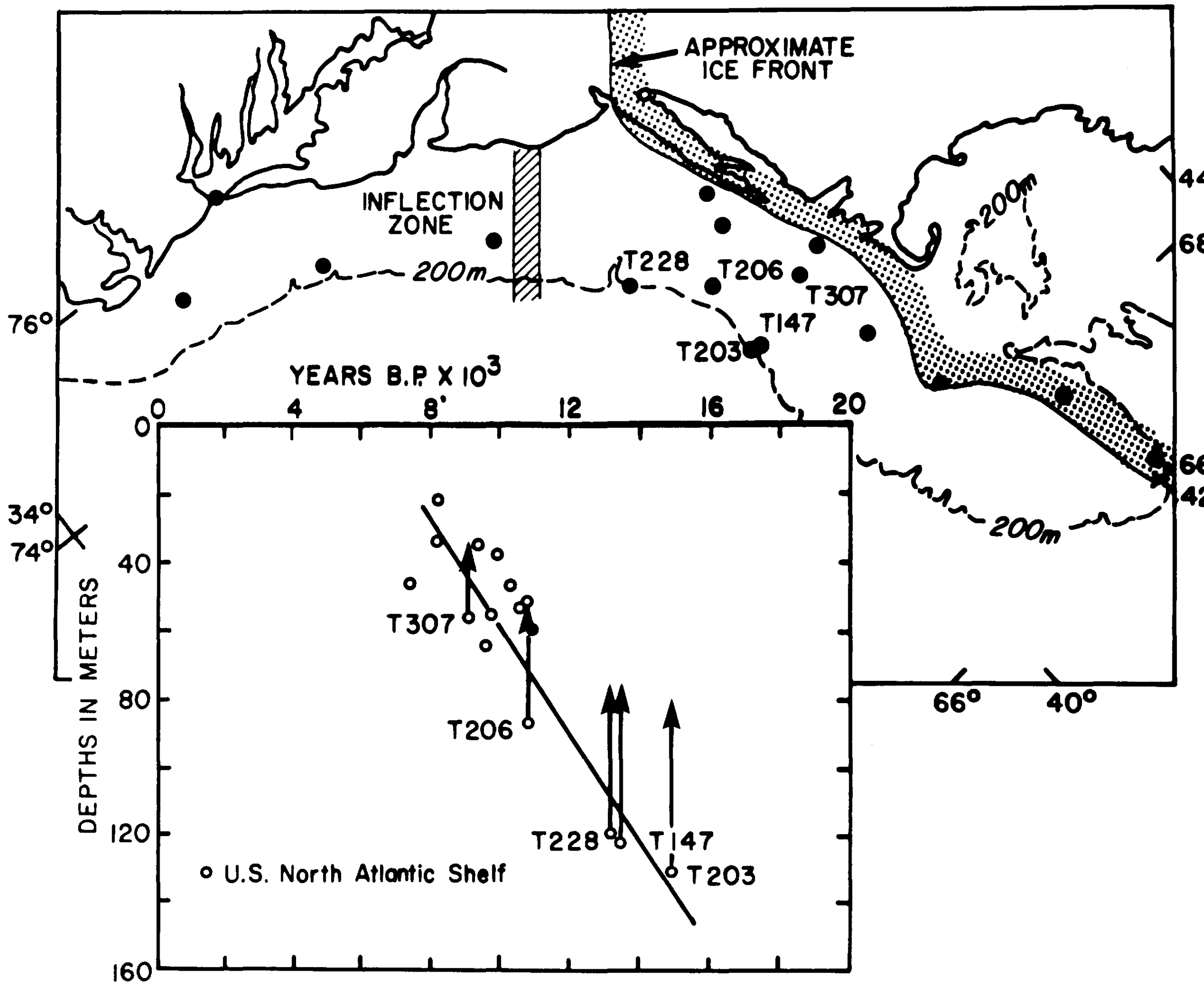
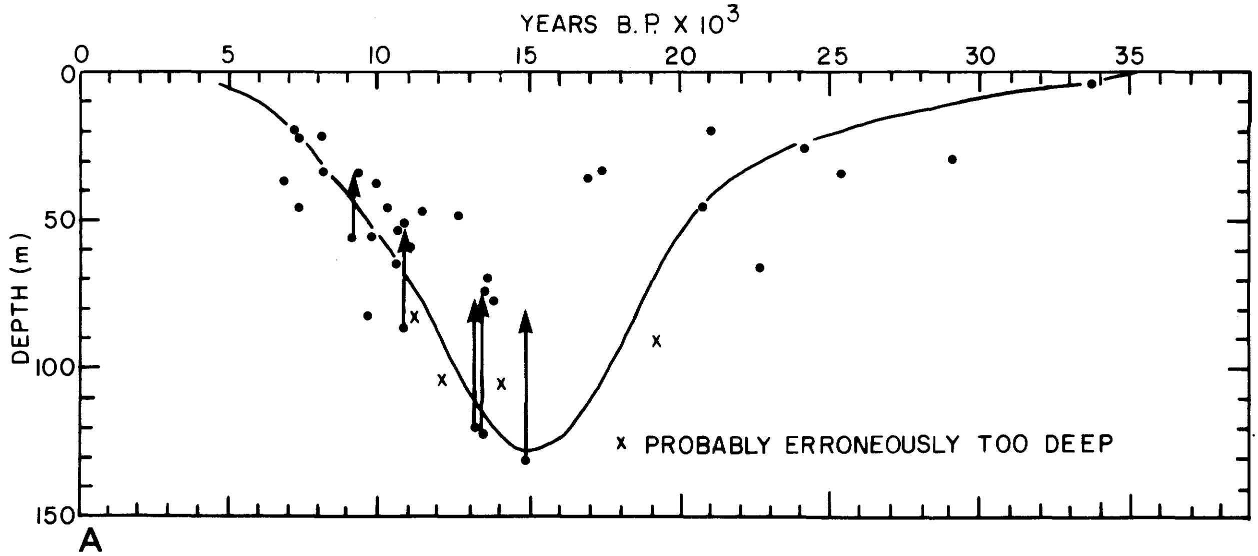


FIGURE 3.—Locations of dated samples relative to Wisconsinan ice front (Emery and Uchupi, 1972, fig. 12) and inflection zone off New Jersey. Inset plot shows depth versus age of samples; vertical arrows show amount of upward correction that we propose to apply to samples from subsided area. Diagram adapted from Emery and Garrison (1967).

21,000 years, a sample obtained at 90 m and dated at 19,200 years (fig. 4; Milliman and Emery, 1968). This value is erroneously deep, according to Macintyre and others (1978). Therefore, this sample is associated with a maximum possible depth, and our curve has been carried above this point.

This evidence suggests that the previously accepted late Wisconsinan and Holocene sea-level curve for the United States east coast is erroneously deep in its deeper part. However, the second piece of evidence that caused Milliman and Emery (1968) to conclude (reasonably) that their curve represented global (eustatic) sea-level change was its agreement with worldwide age and depth values, as shown in the lower part of figure 4. It is apparent that, in general, deep values, which appear to corroborate the east coast curve of Milliman and Emery, are associated with unstable continental margins and that stable margins show shallow values. Two presumably stable margins do not follow this pattern: the Nigerian and Australian margins. The Nigerian values were obtained from species for which the depth range is questionable (Emery and Garrison, 1967; K.O. Emery, 1977, personal commun.). One of the Australian samples is of unidentified mollusk fragments that might have been transported (Hubbs and Bien, 1967), and the other two, dated at 17,000 years, cannot be identified in this reference.

Another generally accepted "eustatic" curve, based mainly on gulf coast data, is that of Curray



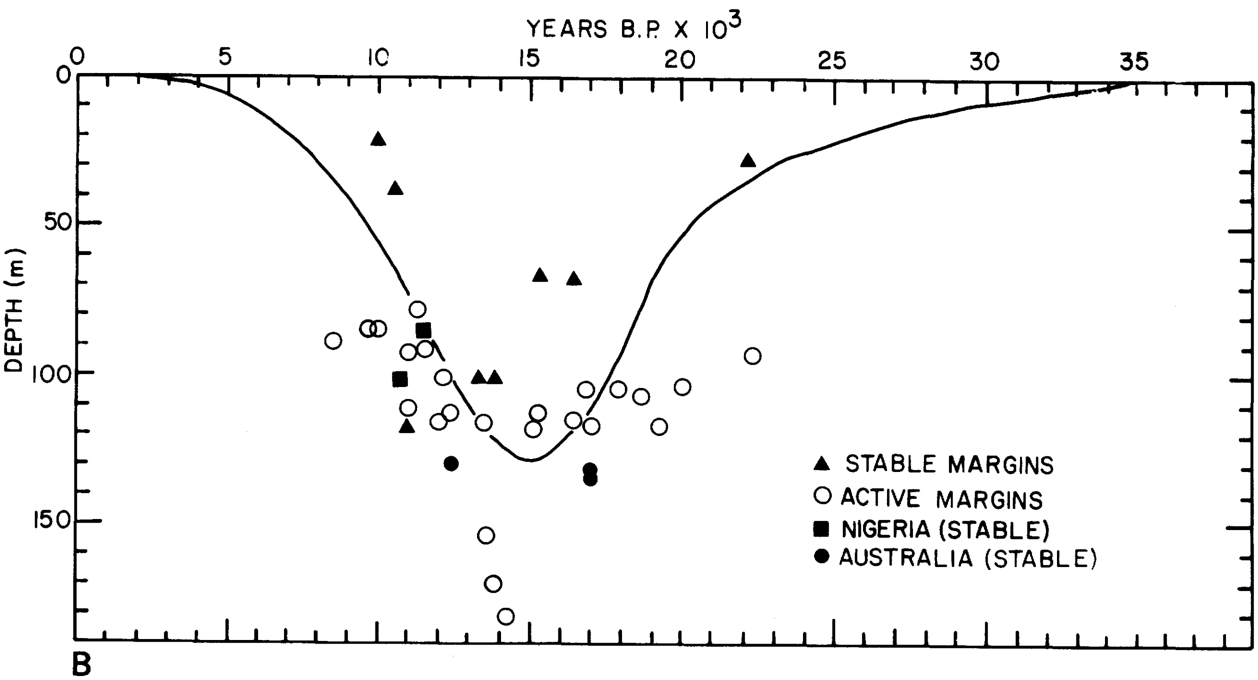


FIGURE 4.—*A* (adapted from Milliman and Emery, 1968). Arrows show depth corrections we have applied, and samples that are considered to be associated with erroneously deep depths are identified. Curve represents Milliman and Emery's proposed eustatic curve for United States east coast. *B* (also adapted from Milliman and Emery, 1968). Curve obtained from upper diagram plotted with radiocarbon-dated samples from elsewhere in the world. Margins that we considered to be stable are Campeche Bank, Argentina, Bahama Islands, and west coast of Florida. Those we inferred to be tectonically active are west coast of Mexico, southeastern Caribbean Sea, Gulf of Panama, East China Sea, and southern California.

(1960, 1961, 1965; curve shown in fig. 5). However, the dated samples from the gulf coast used to construct this curve were obtained from depths only to a maximum of 88 m. Four of the 12 samples, those controlling the curve in the 12,000- to 13,000-year range, were obtained from banks that are

TABLE 1.—*Depths for some radiocarbon-dated samples, United States east coast*

[Samples reported in Emery and Garrison (1967). Locations are shown in figure 3]

Sample no.	Depth collected (m)	Corrected depth[1] (m)
T307	55	35
T206	86	53
T147	122	76
T203	130	81
T228	147 (120[2])	104 (77[2])

[1]Based on adjustment for postglacial warping of shelf.

[2]Represents correction originally applied by Emery and Garrison (1967) on basis of depth range of organism.

topographic expressions of salt domes (Poag, 1973). Thus, if the dome has risen, the Curray curve may be too shallow in this age range. The curve has been extended to greater depths than allowed by the gulf coast dates by employing data from the California margin (J. Curray, 1977, personal commun.), an active margin. Our adjusted points appear to indicate that the Curray (1965) curve is slightly too shallow from the present to 15,000 years ago.

It probably is absurd to conclude that all active continental margins have subsided; indeed, some may have risen. It is possible that the entire United States east coast margin may have risen owing to broad-scale tectonic forces or eustatic loading of the ocean basin (Walcott, 1972a) and thus may present a "eustatic" curve shallower than the rest of the world. However, the processes of tectonic movement, transport of samples, and submarine formation clearly have combined to introduce errors into some generally accepted "eustatic" sea-level curves. We question the validity of some curves and encourage a critical re-study of the problem.

SUMMARY AND CONCLUSIONS

Submerged shorelines of late Pleistocene and early Holocene age on the continental shelf between Chesapeake Bay and Long Island are horizontal south of an inflection zone off central New Jersey. The shores dip northward north of this zone and again become more nearly horizontal east of a second inflection zone off Long Island. These abrupt changes in attitude imply the presence of a lithospheric block bounded by zones of weakness. The dipping shores within the block indicate a tilting of the block in response to crustal rebound in the glaciated area to the north.

The shores, representing formerly horizontal reference lines, provide a means of correcting for

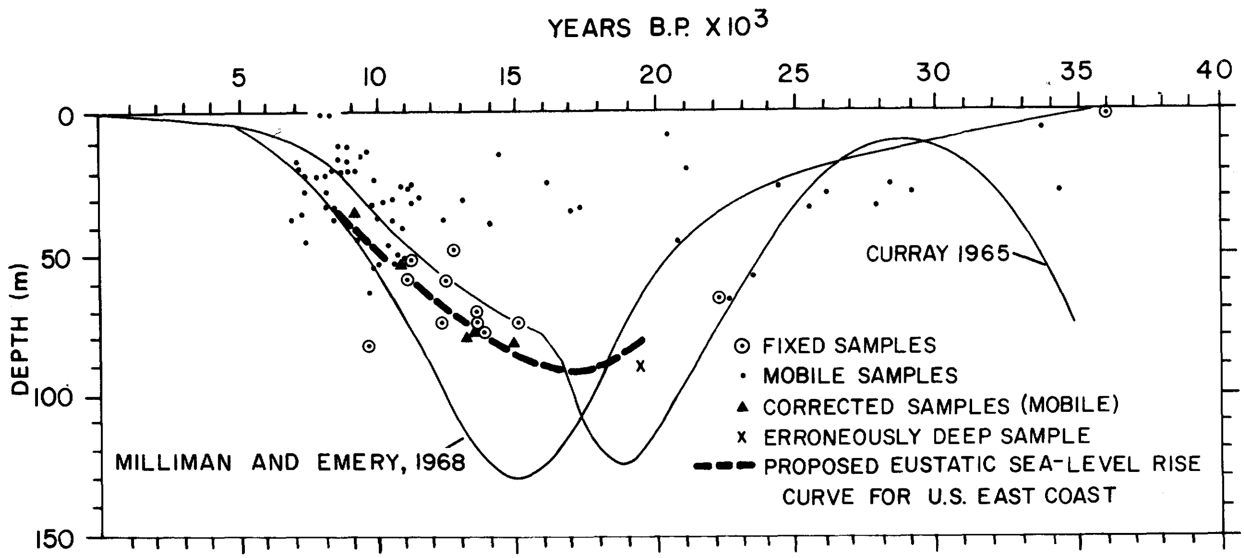


FIGURE 5.—Age and depth relations of samples presented by Macintyre and others (1978), including data previously published by Emery and Garrison (1967) and Milliman and Emery (1968). Concept of fixed and mobile samples is that of Macintyre and others (1978). Curves proposed as eustatic by Milliman and Emery (1968) and Curray (1965), as well as shallower curve suggested here, are shown.

tectonic effects the depths of samples previously used to construct sea-level rise curves. Doing so produces a sea-level curve that never falls below 100 m, whereas previously suggested curves exceed 120 m. We suggest that tectonic movement of continental margins, landward transport of shells dated for sample points, and erroneous interpretations of the depth of formation of samples may have led to an overestimate of maximum sea-level lowering during the past 25,000 years.

NOTE ADDED FOR SYMPOSIUM VOLUME

This paper demonstrated that regional loading by ice was significant in causing crustal warping that could have major impact on local sea-level-rise curves. Previously, such effects due to water loading had been discussed (cf. Bloom, 1967). Shortly after our paper was published, several excellent papers by Clark (cf. Clark and others, 1978; Clark and Bloom, 1979; Clark and Lingle, 1979) indicated that the redistribution of ice and water on the Earth's surface actually had effects that were worldwide, rather than just regional. Loading and unloading of ice in high latitude regions and addition and removal of water from the irregularly distributed ocean basins were shown to cause a change in the shape of the Earth and its geoid. Because water flows freely and crustal rocks do not, local sea-level curves (measured relative to the continent) will vary radically at different locations on the Earth. The relation of sea level to changes in shape of the Earth also had been considered by Morner (Morner, 1976, 1980). Therefore, the analysis of a regional sea-level curve, in the manner proposed in our paper, is a correct approach, and, in this case, the New Jersey-southern New England curve should be evaluated in this way. However, due to reshaping of the Earth and the geoid by redistribution of ice and water masses, this curve is applicable only at the locality where it was measured. Elsewhere, local sea-level curves measured relative to the continent will appear quite different.

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GAS CONTENT AND CLATHRATES

Clathrates and Sea-Level Changes

Robert E. Miller

Before going into the relation between clathrates and sea level, the speaker addressed a number of problems concerning pressure, clathrate formation, and sediment instability. What, for example, is the role of free gas in the development of bubble coalescence that reduces sediment cohesiveness and shear strength? What is the role of authigenic cement such as aragonite, ankerite, or siderite that may be precipitated by outgassing of methane? To answer such questions a key element is in knowing the phase and the in-place concentration of the gas.

The speaker discussed in some detail the characteristics of gas recovered in core samples at three sites in the Atlantic during the Atlantic Margin Coring (AMCOR) Program. Concentrations of methane in these samples approached or, in a few cases, exceeded 300,000 ppm. Some of the gas may have been in the free phase, but the origin of the gas was not clear. Was the gas due to migration from a deeper reservoir, or was it related to outgassing during a previous lower sea level? These questions are difficult to resolve if much of the gas was lost during its ascent from the bottom in the core barrel.

The speaker defined the character and evolution of clathrates and pointed out that penetration of a clathrate with a drill may result not only in an increase in gas pressure but also of fluid pressure. To develop a better understanding of the kinetics of clathrates, we need a temperature-controlled pressure core barrel in which to recover the clathrates. Perhaps then we can resolve whether clathrates can form seals and what the role of authigenic minerals is, both in relation to seals and to the interpretation of reflectors in seismic sections associated with clathrates.

Discussion following Miller's talk:

Emery: Your slides show the variation of methane with depth. You didn't mention whether that variation is controlled by the grain size.

Miller: In hole 6021C, those sediments are consistent in grain size, all up and down the hole. So I don't think it's grain size.

McGregor: You mention mineralization occurring along the base of the clathrate. What is that mineralization? Was it found in the DSDP [deep sea drilling project] cores in the trench?

Miller: The answer is what was found—ankerite, siderite and aragonite; the possibility for this occurring as aragonite is [found in] some of the work that Jack Hathaway and Egon Degens published in 1969. They found very anomalous light aragonite (-60 per mil). What's the origin of it? Could it have been an outgassing of methane as a result of lowering sea level? Methagenic bacteria take over, go through their biochemical processes, change the pH. As a result, there's a precipitation of the carbonate that is anomalous in its isotope values.

McGregor: Was that found in hole 6021? Did you see that?

Miller: I looked for it, but I haven't found it yet.

Dillon: Bonnie, that stuff was found in leg 11, but, based on the velocities we think we see, it should have been above the gas hydrate, the base of the gas hydrate at that level.

McGregor: In the Middle America trench, they went through clathrate in a hole. I don't know if they went through it, but they recovered ice on the deck.

Dillon: They didn't go through it. Did they get any ankerite?

McGregor: I don't know; they got siderite.

Bea: Would you explain the statement that you made earlier that there is the potential, if one drills through clathrates, to experience a blowout? Explain pressures and volumes?

Miller: I can give you an example of what happened up in Alaska, as a firsthand problem. The Glomar people were up there drilling at 328 ft (100 m) in the Bering Sea; they hit a clathrate. As a result of that, they had lots of problems maintaining the stability of the ship because tremendous volumes of gas were released. As they cut through the clathrate, there was diffusion and a phase change, and, consequently, gas was evolved. The problem is the gas was trapped underneath. The clathrate itself started venting.

Bea: Through the sea column? It's shallow.

Miller: To answer your question on the exact depth of the water, I can't give you that information. But they hit a cap, what they interpreted as a cap, and something released a tremendous amount of gas that had been trapped under the seal.

Bea: I can, of course, see it in shallow gas pockets, but in the clathrate form that you refer to, it's difficult for me to understand.

Dillon: I am suggesting—not that the gas hydrate is converting to gas, but that there's a gas invasion below the gas hydrate. In fact, the gas hydrate is a seal with the invasion below it.

Miller: That's the geologic hazard.

Bea: Where's this?

Dillon: The Blake outer ridge, just off North Carolina.

McNeill: There's two points to be made here. Number one, I believe, in the case you're referring to, [was] the hydrate was under permafrost?

Miller: No, it was not. This was offshore.

McNeill: Notwithstanding though, you could hold at shallow depth, 1,000 or 2,000 ft, on the order of six times as much methane in a hydrate as you can hold in sea water saturated with methane at that same depth. So then, by using drilling mud or sea water circulating through it, you can very easily warm it up enough to get it to release six times what would otherwise be in saturation and get a geysering effect.

Miller: I can give you an example of what the Soviets are doing. They're producing clathrates in a field in northern Siberia. This is the tundra, in permafrost. Their technique is to pump down methanol and glycol and, as a result, change the phase and release the gas. They have increased their production by 100 percent, if you can believe their numbers.

Emery: There was a report, a Russian-type report, that the Russians were going through the permafrost and hit clathrates. The clathrates expanded and made a hole 100 m or something in diameter, and their well rig and the crew disappeared. This was duly reported in Moscow, and they were told to drill ten more of them. Do you have any information about this?

Miller: No, I hadn't heard that one.

Dillon: The gas hydrate thing is full of rumors. I heard a rumor that the Russians blew out an entire field, that as they produced the warm gas from underneath, the pipe got warmed up and eventually broke down the gas hydrate that was acting as a seal. So the whole field blew-out around the pipes and they lost the whole thing.

Grow: I've heard from people working in Canada that they've had some problems with blow-outs under conditions where you have permafrost and possible gas hydrate problems.

Miller: Right. Dick McIver, who had been with Exxon[1] for a number of years and a very close friend of mine, has some proprietary information on certain areas, but his experience has been, from information conveyed to him, that there's been one hell of a problem with potential blow-outs. And this

[1]Any use of trade names and trademarks in this publication is for descriptive purposes only and does not constitute endorsement by the U.S. Geological Survey.

is why they get so spooked when they start suspecting they're getting into this kind of problem.

McNeill: It's not just in exploration, it's also in production.

Miller: That's right.

McNeill: There's the experimental data for enclosed bombs that the pressure can whistle up to 100,000 psi very fast, very fast.

Menard: Are there any problems at Prudoe Bay with the exploratory drilling up there?

Miller: There are clathrates in that area, but they have not had any problems drilling. Exxon has two cores—sealed up, pressurized core barrels. They're not telling anybody any information on them. They're kept in cold storage.

Menard: I don't recall any problems at all with our NPRA [National Petroleum Reserve—Alaska] drilling.

Miller: They've not had a problem. It may be due to the technology too.

The Relation of Clathrates and Sediment Stability

George Carpenter

I'm going to talk about the effect that clathrates and their associated free gas may have on ediment stability. In several places on the Continental Shelf of the United States, particularly in the Southeast Georgia Embayment, we've seen examples of what I interpret as a slump overlying clathrate. I'm going to discuss why I feel this is a slump, why I feel this is a clathrate, and speculate on the relation between the two.

Figure 1 shows the location of the slump-clathrate feature. The feature is on the Continental Slope, about 100 mi south of Cape Lookout. Figure 2 shows dip and strike profiles of the feature. The layering is truncated, and you can see some disturbance in the rotated block. I did a migration calculation on these data to collapse the diffraction due to the edge effect, and I find that it's a fresh-appearing, concave-outward slump scarp with nicely truncated reflectors. Again, some disturbance is evident in the failed block.

Figure 3 is a physiographic diagram of part of the slump, showing the scarp, the crumpled material at the toe, and the failed block. Overall, it is about 12 km by 4 km, as shown in figure 4.

What I attempted to do in this area was to map out what I thought was a clathrate. The clathrate is depicted by a reversely dipping sequence of short, "bright spot" reflectors that are capped at some consistent point along their upper limits. These reflectors corresponds to the base of the clathrate, below which is free gas, which is possibly geopressured. The base lies on the curve developed by Shipley, Charlie Paull, Bill Dillon, and others who've done this sort of thing. This point lies on the permissible subbottom-depth/water-depth curve for the clathrate, so it works in that sense. The relation between the slump and the clathrate is somewhat speculative. It may be that free gas derived from under the clathrate somehow seeps through the clathrate cementation zone and contaminates the surface sediments with high concentrations of free gas, and weakens them. Most people would agree, I think, that there is a causal relation between gas content in sediments and their strength; the relation remains to be quantified, but I think it's real. Or the clathrate doesn't reach the surface, and there's either a zone of free gas in this general area of near-surface sediments or a kind of near-surface clathrate slush. No one really knows if the clathrate itself has any topography or anything like that. But it is probable that gas, from whatever source, is contaminating the sediments and weakening them.

A couple of possible mechanisms suggest themselves. I've seen two other systems like this, where a slump overlies a clathrate, and they're all in the Southeast Georgia Embayment but all at different depths and in different places. So I developed a curve from those three points. I plugged in a generous Pleistocene sea-level change of 200 m. It turns out that the eustatic change only moves the base of clathrate up about 25 m, which would have to be enough to destabilize the sort of system I'm talking about. I was hoping the sea-level change would drive the base of the clathrate right out of the section and that all this free gas would be available to cause sediments to fail. That didn't work out. I talked to Charlie Paull about this; he has a much better curve with many more data points on it, and he only gets a 17-m rise.

Discussion after Carpenter's talk:

Edgar: Could not the fact that you've now got a shift in the location of that phase change itself cause a plane of weakness that could fail at that depth?

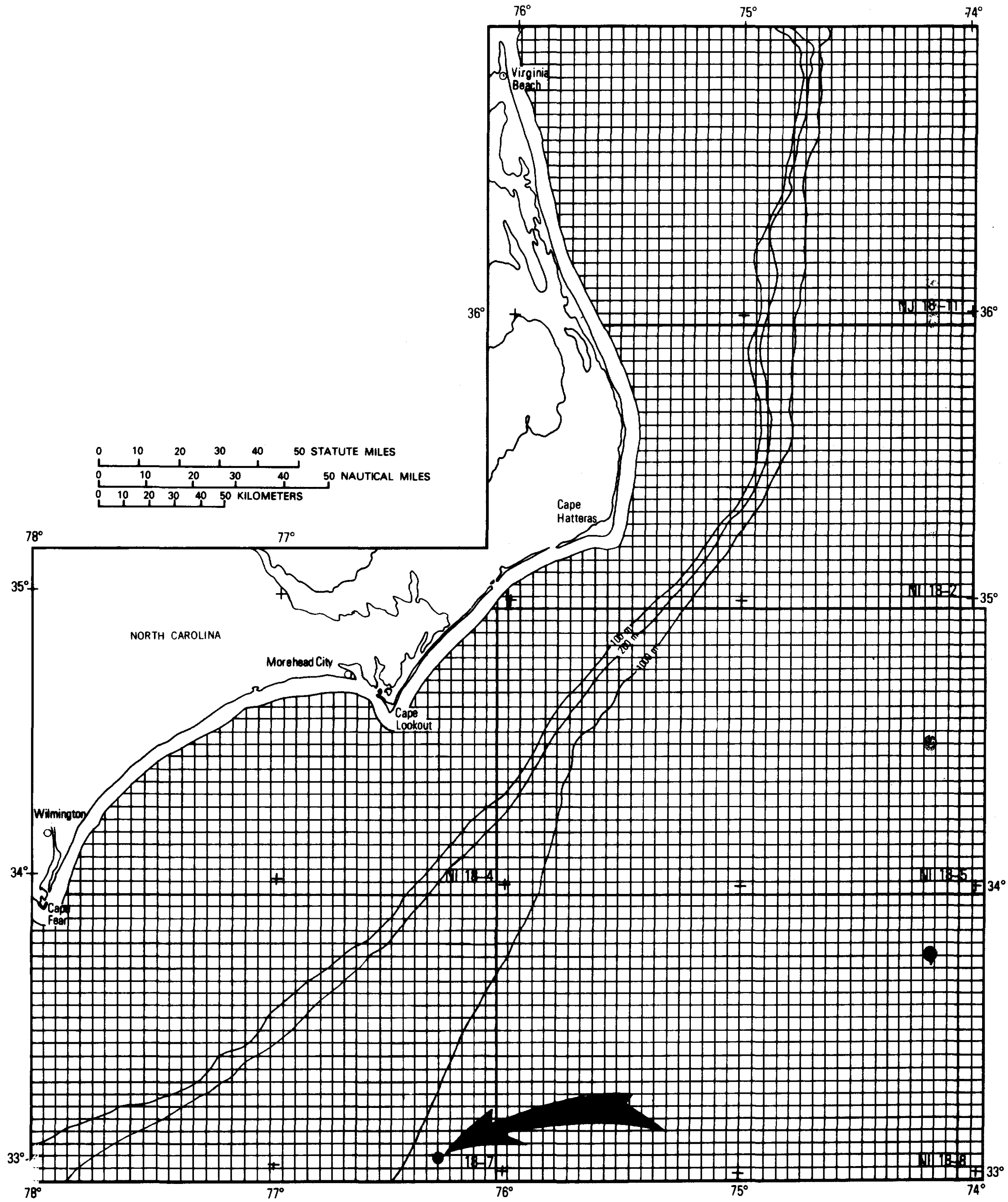

FIGURE 1.—Location of the slump-clathrate complex.

Carpenter: Bob Miller and I have discussed that possibility. It may be that he will have a program developed next year to decide whether or not there is any surface topography on those things and what happens there.

Dillon: Terry, the plane that you're talking about

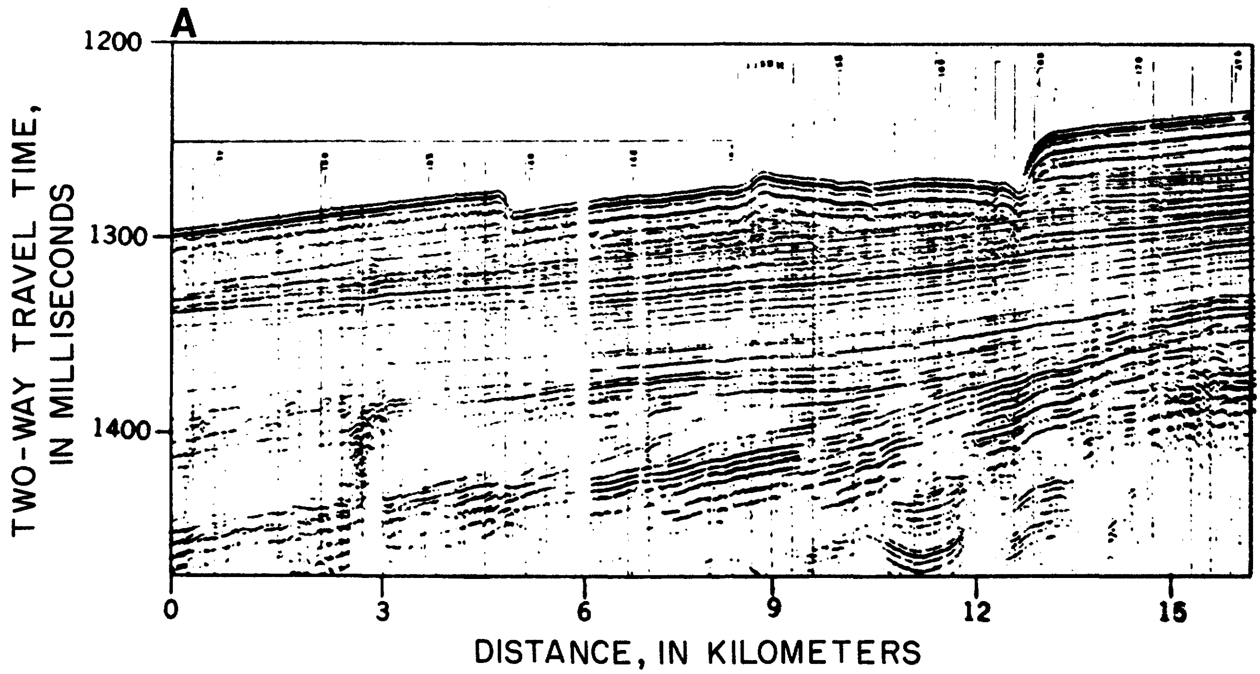

FIGURE 2.—*A*. Strike profile across the slump-clathrate feature.

will be the base of the gas hydrate and would be several hundred meters below the sediment surface.

Edgar: That's right.

Paull: The plot that George was just talking about. . .the depth at which the base of the bottom simulating reflector (BSR) occurs in relation to the water depth, is here. I guess what George is pointing out is that if you were to change sea level 100 m, that would be equivalent to moving 100 m farther down that curve. So we would expect to see, with a 100-m sea-level change, a rising in the clathrate of about 17 m. Since the shallowest that the clathrates have been known to exist now is 200 m, perhaps a minimum number, I wouldn't expect the clathrate to move up to less than 180 m. So if the slumping is related to the change in sea level and migration of the hydrate, I think you would expect to see the failure going down to the plane at the base of the hydrate.

Carpenter: That's certainly the case here.

Paull: But I think your argument that it can be related to seepage has some merit. In terms of a major one, you would have to talk in terms of 200 m. In fact, the hydrate is more typically found 500 or 600 m down into the sediment.

McNeill: Can we discuss that diagram a little bit more? I'm afraid I don't understand something there. First of all, the pressure at a given temperature, pressure that controls the formation of the stability of the hydrate, is the pressure in the ambient water donating the host molecules that form the basket. Is that right?

Paull: Yes. Bob Miller is probably better at this.

McNeill: Therefore, what does the fact that you are either above or below sediment have to do with that?

Paull: Well, this plot is constructed by looking at the depth of the BSR. [Some transcription lost here due to tape change.] So what we're plotting really is the base of the hydrate. And we're looking for the depth the hydrate exists in the sediment.

McNeill: Okay, so there's some geothermal gradient noise on your data here.

Paull: Yes, or gas composition. If you're looking at an ideal system, that's going to be a perfectly smooth top, and you should have a perfect fit to it. And I think the noise on that is just the sloppiness of the system we're dealing with.

Grow: I'm just curious. George, I missed your first slide; was that scarp that you have very near the top of the Blake outer ridge?

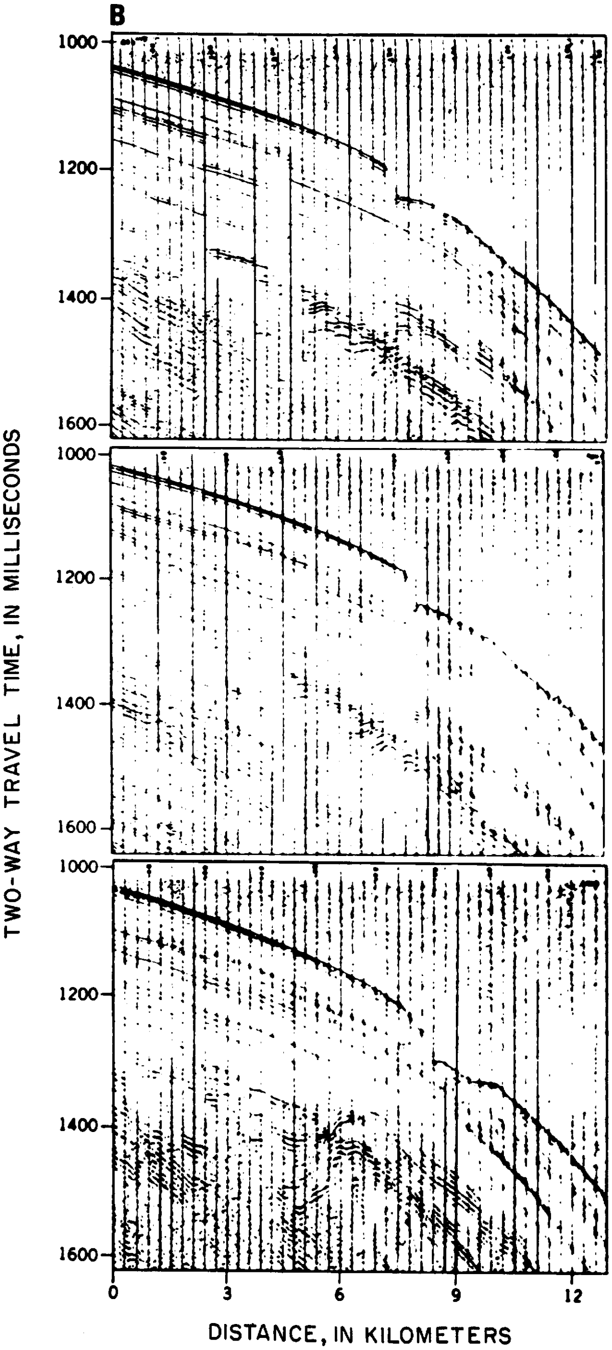

FIGURE 2.—*B*. Dip profiles across the slump-clathrate feature.

Carpenter: Well, yes; it's on the slope at about the 1,000-m level.

Grow: For example, in that slide that Bill Dillon showed, with what we were calling the "bright spots," if you follow that particular profile up to about the 1,200-m mark, you would see that the gas-hydrate boundary does shallow, just as Charlie's slide shows. We have not been shooting

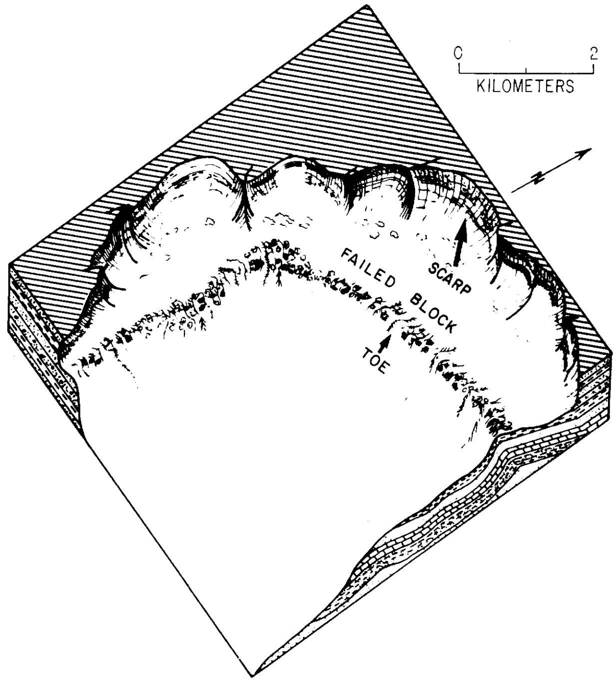

FIGURE 3.—Interpreted physiographic diagram of the slump-clathrate complex. The diagram was constructed from bathymetric data and seismic profiles.

our seismic data with enough true amplitude data. If you have a conformable reflector and you have a lot of automatic gain control in your seismic record, you can't even see it. And, starting with our airgun data, when the reflector is less than 200 to 300 milliseconds down, we can't see it anyway. And I was just curious whether you have any true amplitude displays for your high-resolution data and whether you could follow it in onto the Plateau.

Carpenter: Well, this is a relative amplitude plot, and that's the shallowest hydrate we see in any of our data—and ours goes right up onto the shelf.

Grow: And the line that we have, the one that Bill showed, as it went up onto the Blake Plateau, has a strong reflector that looked as if it was getting tangled up in some unconformities at that point. But we didn't have the resolution with the equipment we were using at that particular time. I think if one could use a higher resolution system, and get both automatic gain control and true amplitude displays, you could really sort this out in a much cleaner way than we've done in the past.

Carpenter: Yes, I agree. This is half-millisecond data; we have a quarter-millisecond, too.

Grow: You really need the true amplitudes types of display, I think, to see these things, unless there is a tremendous discordance between the gas hydrates and the sediments.

Carpenter: Well, one of the things you want to look at is the free gas below the base of the hydrate, which is a "bright spot," so that sort of processing technique shows it very well.

Grow: The other conclusion we came to, and I think illustrated in the slide that Bill showed, was that sometimes the BSR would be very weak in impedance contrast, if there's no free gas underneath it. But if there's free gas underneath it, you get a whopping signature from the thing. That may have a lot to do with the local supply of gas, and it may have a lot to do with the local character of the sediments underneath the gas hydrates.

Dillon: You could get a fairly big impedance contrast anyway. Because the velocity of the hydrate is so high, the water-saturated sediment below it should give you a pretty good reflection.

Garrison: George, did I understand you to say that you thought that one reason this thing may have failed is that you had seepage of the gas through the hydrate zones?

Carpenter: It's got to be some sort of mechanism like that, Lou. Apparently, if hydrates include this interval, well, of course, they'd be very tightly cemented and very resistant to any sort of separation at all. Somehow. . .I really don't know, but free gas has got to get from here to there, for this sort of system to work.

Garrison: You'd have to assume that the gas couldn't pass through that zone without being. . . .

Carpenter: Yes. Dillon has shown, in a paper a couple of years ago, that it's a very efficient cap over this stuff. So how does it get from here to there.

Dillon: The only way you could get gas through that zone is to totally saturate the water, use up all the water to form the gas hydrate, which seems difficult because it takes an awful lot of gas. At the last DSDP Safety Panel meeting, George Claypool talked about that as a hazard—creating gas pockets within the gas hydrate zone. But even he admitted he didn't think it was very likely. The hydrate can absorb so much gas, it's very difficult to believe there might be an excess.

Carpenter: Yes, that's the problem with this. Is this a hazard, or is it production? It represents a significant accumulation of gas.

McGregor: Do all three slump features in that area have the same height of scarp? Are you moving the same thickness of sediment, or does it change from area to area?

Carpenter: It changes from area to area because the depth varies in each of these systems. Of course, the base of the clathrate is deeper in the deeper slides.

Paull: Do you think that's an area where you'd find more slumps? Or do you think, if you compared it with the rest of the margin at similar depths, do you find a similar number of slumps? Do you think [that in] the area where the hydrates are developed, statistically there'd be more slumps?

Carpenter: On the basis of our data, that we collected to look for this sort of thing, slumping was very rare on the slope in the Southeast Georgia Embayment.

Embley: Right downslope is one of the megaslides. There's a zone of slides all the way down to the Hatteras Abyssal Plain.

Carpenter: That's only on the rise, probably.

Embley: Yes, but I mean it points up in that direction.

Paull: By the time you get to the megaslides, you probably don't have the hydrates developed.

Dillon: How thick are the slide blocks?

Embley: It's a series of blocks over a large area.

Dillon: How thick were the individual blocks? The point is, if you want to get to the base of the hydrate as a lubricating zone or a weak zone, with free gas, you need to talk about something that's about a hundred meters thick.

McNeill: Since we have all the clathrate experts here, and I'm going to talk on the subject. . .and I'm the first one to admit I don't know much about it. I'm going to ask a question that could be very important to the geotechnical engineers who are here. It takes three things for a clathrate to form—proper temperature, proper pressure, and an adequate supply of the captured gas, an excess supply. I don't really look upon the generation of the soil deposit or the generation of methane within it as a quality-control process, and I'd be very happy to stick to the fact that, a thousand meters down, there are going to be different (perhaps slightly different) concentrations of gas. Therefore, we have a discontinuous system in which volume changes have occurred that could grossly change the permeability over the classical concepts of a 10^{-8} [coefficient of permeability] clay or a zero hydrate, into something that is much more permeable to a gas phase.

Miller: One of the things that has to be enumerated is that the clathrate is slush on the top, and

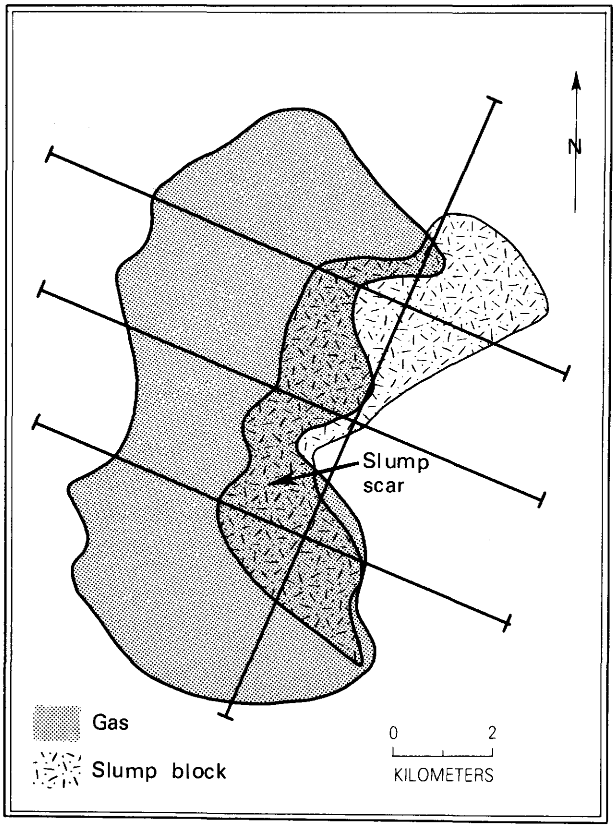

FIGURE 4.—Map, drawn from seismic profiles, showing both the areal extent of the failed material and the clathrate accumulation. Note that the slump scar nearly defines the long axis of the clathrate deposit.

we don't really get a reflector on the hard stuff until we get to the bottom. So it's that phase change in between the hard stuff and the slush that is the problem.

McNeill: You expect the seals to be vertical and not very likely to be horizontal.

Dillon: At the top, I think we can see longitudinal variations in the degree of gas hydrate formation. Is that what you mean?

McNeill: Horizontal variations?

Dillon: Yes, you get to the limits of gas hydrate formation, particularly toward the deep end when you're running out of gas. Theoretically, you can form gas hydrates anywhere in the ocean, but when you get down to about 4,000 m or so, you start running out of enough gas to make it, and then it gets patchy. You might have a fairly solid BSR, but, above it, in the blanked-out sedimentary layers that also seem to be characteristically sort of wavey, you appear to get a patchy development of gas hydrates.

McNeill: So what I hypothesized might be a possibility? That is, this whole situation could depart from the classical concepts of permeablity and therefore would pass gas.

Dillon: I don't think it would pass gas? Is that what you're saying?

Grow: Bill, what you're saying is that the amplitude of the reflection off the bottom of the gas hydrate does vary quite a bit along our seismic profiles.

Dillon: Yes, that's true, but also I'm not sure. . . .

Grow: I think I would agree with that, and a lot of that has to do with the way in which the sedimentary beds are coming up, moving up and down with respect to the gas-hydrate boundary.

Carpenter: Wouldn't it also depend on the size of the free gas reservoir?

Grow: Some horizons are trapping the gas, and you've got more gas moving up through sedimentary pockets. You've got a lot of gas coming up in that pocket when you hit a good bright spot.

Dillon: What I was talking about—the slide is not the clearest—the BSR is coming through here, and you seem to get these blanked-out zones above the BSR; if it's really well developed, you'll get essentially no reflectors from here up to the surface. But here, you see, we've got patchy, [or what] would appear to me to be patchy, distribution of gas hydrate within the sediment. The base is set by the physiochemical limit; the top and the lateral limits are probably set by the quantity of gas available when you get down to these depths. See, that's 4½ seconds of water depth.

Carpenter: I see horizontal discontinuity in each of those horizons. Am I seeing it correctly?

Dillon: I'm not sure what you're looking at. The BSR is coming this way, and these are sedimentary reflectors going through that way. Okay? Then these black patches, the zones in which you get no reflectors through, probably are cemented patches of gas hydrates, I think. Charlie, why don't you put the other slide on? I think Charlie has something to say.

Here's an example (Charlie passed me this one) in which we see very strong [reflections]. Notice the difference in reflectivity of the layer reflections between here and here. Presumably, the gas hydrate here is cementing things up and cutting down the amplitude of the impedance variations between layers.

Grow: Depending on how that was processed, depending on what time scale they used for automatic gain control, that can fool you a little bit.

Dillon: True, but on that same record, you can go longitudinally and see variations.

Carpenter: Then you think cementing extends all the way to the surface?

Dillon: Yes. It looks like it would come up to, maybe, here—if my argument is correct. Here, you see, you've got less of an effect of that where the BSR is less. There's a little hill here that may be trapping gas.

Grow: The point I want to make, Bill, though, is when we process those records, that very strong event on the automatic gain control record will dominate your amplitudes. And so far, unless we go into some very careful processing, which we've never had an opportunity to do, you can get fooled.

Dillon: Yes, but there's some kind of a window to the thing, and it's not blanking-out the stuff below.

Grow: That can be an artifact of the processing, and that's the point I want to make.

Folger: But Bill's point is that, laterally, you can go along and see that change.

Grow: Yes, when you go out to the side and where the gas underneath the boundary is not so strong, then that isn't such a big reflector on the gas hydrates, and then that doesn't overdrive your processing, your scaling in your processing.

Dillon: Well, you're more familiar with this sort of processing than I am. I assumed that the automatic gain control (AGC) affected what was below you more...

Grow: I would tend to agree with what you're saying, but we really have never had a chance to tear that kind of data apart properly.

Dillon: Yes, we really ought to.

Grow: I think it does come back to trying to work with your amplitude data very carefully.

Dillon: That's a true amplitude. This is without AGC, by the way, down below, a true amp section. You can see the same effect, to a certain extent, down here too. These reflectors are stronger. The internal reflectors in here are stronger than they are up here. Presumably, that has no AGC, but you always wonder what happened in the black boxes.

McGregor: Bill, I'm not sure I understand something. Initially, we were saying that there was gas trapped underneath the bottom of the BSR, and yet, that's a zone where you have strong stratification; I thought that would be a zone where you would then get areas with no stratification because the gas content had increased in the sediment.

Dillon: I'm going to preface this by saying I'm sure I don't understand a lot of things. You said you weren't sure you understood; I'm sure I don't. However, what we're proposing here, and this is also based a lot on the velocity structure we seem to see, is that this gas-hydrate zone has just cemented things up so well with high-velocity material that you're just not seeing any laminations.

McGregor: Why don't you see the top of it?

Dillon: The top of the gas hydrate?

McGregor: Yes; it's so well cemented.

Dillon: Well, as Bob says, it probably turns into a slush and a zone of diffusion of gas at the top. The bottom is the big acoustic impedance mismatch.

Carpenter: I know the velocity of clathrates is very high. Why don't you get an apparent thinning along the edge?

Coleman: Or a pull-down.

Carpenter: Yes, because of the lateral velocity discontinuity.

Coleman: See all your beds below it. You can't have too much of velocity shift at 5 seconds or 4 seconds.

Dillon: There doesn't seem to be any such effect.

Robb: In fact, that looks like a somewhat damped structure compared to the surface, and your bottom-simulating reflector is indeed bottom simulating; if it is higher velocity, you should be enhancing the bottom-simulating reflector. You should have a greater amplitude.

Dillon: I don't think you can tell from that what it's doing with respect to the bottom. The problem is it's not following the bottom; it's following a pressure-temperature level, and the temperature in particular is what's controlling it most—more than pressure.

Robb: One more question? John said that you had worked with it more than he. Have you seen evidence of the clathrate in your velocity scans, in your velocity analyses?

Dillon: Yes, all right. I just happen to have another slide! Charlie, you want to talk about this? This is yours.

Paull: Well, you're asking if you see evidence in velocity. This is a velocity analysis in two areas—one where the clathrate is very well developed, where the BSR is well developed, and another area up here on the right where it's not. And here we see a normal increase of root mean square (RMS) velocity with depth, as one would expect but going to the area where the BSR is well developed, the velocity jumps to a high value quite close to the sediment-water interface. It remains somewhat high for RMS velocity until you get down to the depth of the BSR, and then there's actual inversion that goes on beneath the BSR. The implication here, if these velocity analyses are correct, since we see this pattern on all of them in this area, is that, in fact, we're seeing a very high velocity down to the BSR—velocities probably in excess of 2,400 m/sec—and then inversion at the BSR with velocities that are 1,500 m/sec, or perhaps even less. These velocities indicate that the velocity structures are consistent with the area down to the BSR, being progressively more solidly cemented, and then, at the BSR, you have this phase transition going into free gas.

Dillon: Our calculated velocities show huge differences in interval velocities, because to get an inversion in RMS velocity when you're down at a couple of seconds of water, you've got to have a very large change in velocity to have a velocity inversion because you're averaging from the surface down.

McNeill: Where would you have those 3.5 km/sec?

Dillon: Yes. I don't believe those. Those are qualitative numbers. Those are geologist's numbers! But that's what, if you really follow what an inversion like that tells you, you get those kinds of interval velocities. The interval velocities below it, as you see, are below water velocity, and suggest you have to have gas bubbles in there. It's about the only way you can.

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In-situ Measurements of Pore Pressures

Robert L. McNeill and E.W. Reece

ABSTRACT[1]

As engineering moves into deeper waters, measurements of soil properties in the laboratory may be highly suspect if the soils are gassy. Strength and pore-pressure data are presented and discussed. A suggestion is made for development of pore-pressure gauges to detect the presence of clathrates.

INTRODUCTION

Conventional methods of sample recovery and laboratory testing have possibly severe limitation in deepwater geotechnical engineering because of the apparently widespread existence of gases in marine soils. The gases expand because the pressure decreases as the sample is raised to the surface. The result is usually a sample expanded into a platey macrostructure like vermiculite. For example, such behavior was observed at hole 6021 of the USGS Atlantic margin coring (AMCOR) project in 1976 (Hathaway and others, 1979), where gas concentrations in excess of 400,000 ppm were measured in samples extracted from the voids between the expanded pieces of soil in the sample tube. The water depth was 1,000 ft, and the boring penetration was an additional 1,000 ft. The shipboard and laboratory nonremolded vane-shear strengths are shown in figure 1. Also shown are the results of three consolidated-undrained (CU) triaxial tests and the range of likely strengths to be expected if the soil were normally consolidated (NC). If those measured shipboard and laboratory vane-shear strengths are correct, the area should be actively sliding on a plane at a sediment depth of 200 to 250 ft if the slope inclination is greater than about 1 or 2 degrees. The overall slope of the area is about 10 degrees, with local

[1]International Conference on Recent Advances in Geotechnical Earthquake Engineering and Soil Dynamics, April 26 to May 3, 1981, Rolla, Missouri, University of Missouri-Rolla.

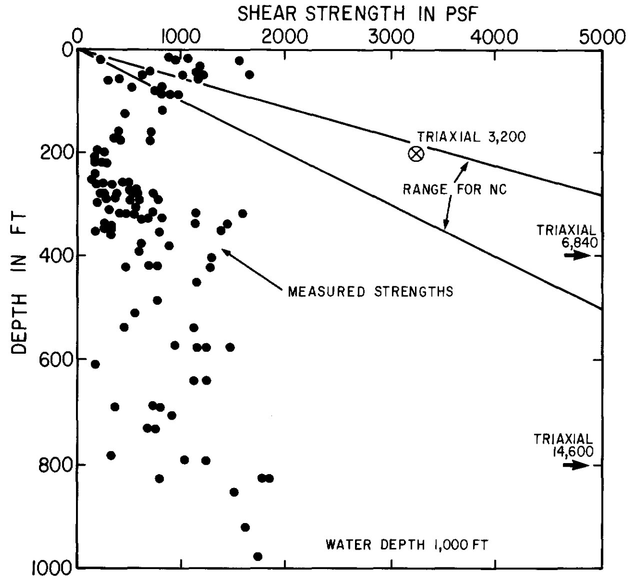

FIGURE 1.—Strength data from Atlantic Margin Coring Project hole 6021. Solid dots show values measured by laboratory miniature vane and torvane (Richards, 1977). Open circle with cross shows value based on consolidated, undrained (CU), triaxial measurements (Swanson and others, 1977). NC = normally consolidated sediment. PSF = pounds per square foot.

slopes as steep as 25 to 30 degrees. Thus, it appears that the measured strengths are too low, and by a substantial margin.

It is likely that, for gassy soils in very deep waters, the soil strengths measured on shipboard or in the laboratory will be controlled by the effects of expanding gases rather than by the *in-situ* strength. Such situations are particularly critical to earthquake engineering and soil dynamics because it is not generally known whether or not the effects of gas expansion on measured properties are on the conservative side for dynamic response analyses. Therefore, as earthquake engineering moves into deeper water, it appears that better methods will have to be developed to determine soil properties.

One way to decrease the uncertainties of the situation is to determine the soil properties *in situ*, acquiring samples only for water content and index testing. Mitchell and others (1978) have published an excellent summary of the present state-of-the-art of *in-situ* testing. The principal differences between *in-situ* testing in deeper waters, as opposed to shallow water or onshore, are (1) the high-pressure environment and (2) the gases present. The high pressures impose obvious instrumentation problems—for example, measuring an excess pore pressure of perhaps 10 psi at the bottom of AMCOR 6021, figure 1, where the ambient pressure is about 1,000 psi. It is assumed that those problems will be solved by instrumentation people. The purpose of this paper is to discuss the nature and effects of the gases on the pore pressures and soil strengths.

Discussion following McNeill's talk:

Teleki: In hole 6021C, at 1,000 ft (305 m), is the gas included still before the core was extracted and it fell apart? What would have been the shear strength in place.

McNeill: You're asking me that?

Teleki: In other words, what you're really saying here is that gas expansion and perhaps loss are affecting the shear strength to the point that you're down in the 100–200 lbs/ft^2 range, or would the data in place follow the curve? I'm looking for an answer somewhere between the actual onboard measurement and those data points, the theoretical curve you have the two lines on. Where would the in place shear strength value be, if you were coring?

McNeill: For these liquid-phase and gas-phase conditions, I know of no process that could make the data measured on shipboard be too high; that is, higher than what the in place strength was. There are conditions, but they're not these.

Teleki: It's much lower.

McNeill: I would expect this to be much lower than the true value. Now the effects of the gas can be twofold. One is, it's gassy, and it may be saturated, no bubbles. But I bring it to the surface, and it comes out of saturation; therefore, the strength is all botched up. The other case would be that, there's gas there but not enough to make a hydrate, only half as much, perhaps. Then I have gas in free bubble form and, therefore, higher pore pressures and a weaker material, compared to what I expected I would get.

Teleki: Let me rephrase that. Can you go back from the shipboard measurements to the strength that you would have measured if you had had a shear vane at the end of a drill pipe?

McNeill: Only if you're willing to assume that the soil is normally consolidated, with no gas-bubble phase in it. Those are very big assumptions. In the past, those assumptions have been made for the design of routine towers in benign environments like only a few hundred feet of water, storms but not earthquakes, and so on. The structures were known to be highly overdesigned, but the cost of the whole structure was so small compared to the resource, that nobody cared. I think, in the future, as we go to multihundreds, even few thousands of feet of water with structures, and we put them under the effects of storm waves—just because they're longer—they're going to be worse. In areas that may have earthquakes, we can't afford the luxury of just saying, "OK, we're ignorant, but we'll overdesign." Those days, I believe, are past.

Emery: Can I ask Bob Miller whether grain sizes had been run on that hole 6021 core? He said that it was uniform, but I'm still not sure about whether you actually ran grain size or not.

McNeill: The answer is yes, and I don't have the data, but as an engineering answer to your question, his answer was correct. There are variations, but it's still silty clay.

Emery: The grain-size variations don't correspond with the variations in shear strength that you measured then?

McNeill: There's no statistical significance that I can pull out of these data.

Lee: Do you have any kind of consolidation test data?

McNeill: Yes, C_v's indicate that it's probably a 10^{-7} to 10^{-8} material. That is not pervious by any means.

Lee: How about maximum pore pressures?

McNeill: There're there, but they're affected by the gas situation.

Shackleton: I didn't say anything whatever about clathrates, but I realize that I seriously misled you this morning in an important way, in that most of the data I showed you was with respect to the Pacific. In the Atlantic, the temperature of the deep water certainly changed between the last glacial and now, by at least a degree and probably more in this other group that's under consideration at this moment. That temperature variability, I think, is a much more significant factor in determining the changes affecting clathrates than typical changes in sea level.

McNeill: That's interesting. That's a good possibility.

Paull: The bottom water changes 1°?

Shackleton: In the North Atlantic deep water, because we're talking or looking at anywhere from 2° C up to 7° C in some of the areas. The temperature change was at least 2°; that's something to think about.

McNeill: We're looking at a range of 0° to 7°, or 0° to 14°, depending on the depth.

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GEOTECHNICAL FACTORS

Hindcasting Analysis of Slope Stability

Dwight A. Sangrey

The speaker's objectives in this talk were to "present and discuss the methodology of hindcasting as a tool for use in understanding better the offshore [sedimentary] processes and to illustrate this with an application to the mid-Atlantic Slope." Much of the work presented was the product of a collaborative study carried out by Dorothy Marks and the speaker.

Four elements to the process of slope stability hindcasting were outlined. These include setting, resistance, loading, and mechanics. Setting includes such parameters as stratigraphy and topography, much of which is resolved with high-resolution seismic data; resistance is the shearing resistance of sediment; loading includes such factors as gravity (a static factor), ocean waves, and earthquakes (dynamic factors); and mechanics concerns the set of equations developed to describe the situation.

By applying these elements, the speaker and his collaborator conducted an analysis of Atlantic margin coring (AMCOR) sediments collected on the Atlantic slope off the mid-Atlantic States. They then modeled the loading and resistance to figure out what combinations would produce failures or instabilities in these materials.

Methods of determining shearing resistance were then covered. From the results determined, the speaker concluded that some values obtained from AMCOR hole 6021 were dramatically low. He elaborated on the possible reasons for this, finally rejecting the conclusion that conditions off the mid-Atlantic coast were similar to those on the gulf coast, even during Pleistocene time. This conclusion was based on a series of parameters such as the relation of the water content within the limits of its liquid and its plastic limit.

The resistance for typical Atlantic Slope sediments apparently ranges from a C/P (Cohesion/ Overburden Pressure or Strength/Overburden) of 0.1 to 0.3 with 0.22 as representative.

The main thrust of Ms. Marks' work described by the speaker was the development of models that predicted slope stability safety factors for various combinations of strengths, slope angles, and pore pressures. This represented an extensive number-crunching effort.

The talk then went into a discussion of earthquakes as a loading mechanism. Various accelerations, slope angles, and strengths were combined that revealed for sediments on the slope off the mid-Atlantic States that an acceleration of 0.01 g is sufficient to initiate movement downslope.

After presenting similar analyses for different wave loadings both under present and lowered sea level, the speaker concluded that neither waves nor earthquakes have ever caused significant slumping in the area. Therefore, what has caused some of the slumping that is evident on seismic records? The speaker concluded that oversteepening due to erosion is a very important mechanism causing instability, especially on canyon walls.

Discussion following Sangrey's talk:

Folger: You compared 6021 to all the rest of those cores along that line of the section drawn on the shelf.

Sangrey: On the diagram, B-2 was on the shelf. But I don't think that makes any difference from the standpoint of a salt particle. It doesn't really care whether it fell on a 1° slope or a 6° slope; it doesn't feel anything different.

Folger: Except the shelf is reworked a lot more than the slope, and therefore the concerns would be different.

Sangrey: I don't see it, but maybe so. By the way, the clathrate people (and I don't want to talk about clathrates)... I think one element of this hindcasting analysis, of course, is to carry it to its extreme. I've got to explain how you get 6021. I couldn't get it from excess pore pressures due to the sedimentation, although, really, I think only two mechanisms are the mechanisms to produce the effect that we see in 6021. Mechanism number one is there has to be gas and gaseous phase, which is there now and has been there ever since it was deposited. I think that is an untenable argument myself; I don't think it shows up in the geophysics. Number two is that this deposit has in fact been laid down on an artesian pressure source, a geopressured source, that has since, in spite of deposition, been subjected to an upward gradient of excess pore pressure. That is my personal opinion of why we have 6021. I think it is a classic illustration of it. I think there is enough evidence from other places in the world to set that as a mechanism, and that's why I think we have 6021. It is, in fact, pressures developed by some geopressured, probably gas, source, and I think the fact that there are petrogenic gases in solution is very consistent with that. I think that those gases were in solution in water that comes from 5 or 10 or 15,000 ft—wherever it is—and they've worked their way up in response to that pressure gradient that has been in existence for 100,000 years.

Folger: Actually, I think I might address this more to the Pleistocene geologists. Certainly, we're not into tectonic zoning here, but I just wonder what happens when the ice builds up and then the ice melts along the marginal area. Bill Dillon has shown a flexure, a tectonic flexure, associated with the forebulge. Certainly, if you stress this area, either by the ice or by the change in sea level, then faulting might take place at that time and therefore at the time of maximum ice advance. You might have a triggering situation by those faults that were clustered along the Gulf and may be associated then with a maximum amount of sediment loading.

Sangrey: That would be a very consistent hindcast analysis scenario. You would say, "Let's take a look at what's the maximum relaxation rate of the hinge line that we assume is associated with the greatest earthquake intensity. Let's see what that condition of loading would do if we also estimated the shearing resistance, and so on." And you would certainly come up with a less stable situation, considerably less stable situation than the present. My comment, when I said I don't see any evidence, is, in our interpretation, I don't see any evidence that there'd been much earthquake shaking on the present slope. Another person might not necessarily conclude that. I just don't think we know.

Butman: Of the two holes drilled on the slope, COST B-2 and 6021, one seems to be close to unstable and one is very stable.

Sangrey: I think 6021 is a very little local feature.

Questioner: You left the impression that you think it is stable.

Sangrey: No. I think that the slopes, by and large, are stable. I think that 6021 is an anomalous feature. I think it's very clear. 6021 can't apply generally. One might argue, if you believe that the slump block that Bonnie described in 1976 or 1977, if you really believe that that's a slump block, that there is no way at all you can move that block unless you put 6021 or a petrogenic gas source underneath it. That's the way you'll move it. You can't shake it down. You can't knock it down with waves. It will only move if you have such a dramatically different strength profile as represented by the AMCOR 0.1 [C/P value] that I'd put on before.

Butman: Why do you think 6021 is so anomalous?

Marks: I think what he's driving at is the point that, if there's a slope that's 12°, you can't have those strengths anyway. I think it would fail. And that's the whole point.

Butman: What if you have a small earthquake?

Sangrey: No, no, no. They're stable under static conditions.

Marks: These are just sitting there; we have a lot of slopes that are very stable. If you go down to those 0.1 strengths. . . .

Miller: How do you explain the profile of methane? Down to about 200 ft [61 m], we have low concentrations; then, at about 200 ft [61 m], they increase to 3-odd thousand or better, and the texture up and down that hole is essentially uniform.

Sangrey: By the way, I've heard several people say that that ain't true.

Miller: What did you find?

Sangrey: It's really very clear when we look at the 6021 data, which I had nothing to do with. It is considerably more permeable in the top 60 or 70 m. Who said it's uniform? It is very uniform, but it's not that uniform. There's a significant statistical shift in the grain size just at about the place where the gas has kept going. Now, I'm not a gas expert, and I'm not going to say that there cannot be gas in solution, gas in some bubble phase, in 6021. It may well be. That would be a very nice explanation for the shearing resistance that I have here. I have some problems in having that gas in solution for all the time since the sediment's been there, which is necessary. And I really think this other mechanism is much more volatile.

Grow: Is there anything in the high-resolution data around 6021 that shows any faulting?

Sangrey: I don't know. When I saw Jim Robb's colored slide of the Cenozoic profile, in the section through there, I was excited to see something in the Miocene underneath and I immediately went over to him and said, "Jim, what was that?" He informed me it was a multiple...so I'm back where I started. I think that what you speak of is worth looking for; I think somebody ought to give a serious look to what kind of structures are there below 6021. Are there faults that are conduits for gas from some deep source, up into the Miocene or above? Because I'm personally convinced that that's the only way you can get 6021, which I personally also think is a real set of data.

Robb: I don't think that the high-resolution data allow us to see that well down there in 6021 to distinguish anything. Along the top of the shelf, there is a zone of acoustic turbidity where reflectors have been blanked out; this has been attributed to gas.

Schlee: On common depth point (CDP) line 2, which we published in 1977, Bob Mattick modeled the break in slope at about half-a-kilometer depth, which he thought was a gas accumulation of methane. You might want to look at the record. It was halfway across the shelf; it wasn't out close to where you are, but you might want to look at the acoustic signature. We don't know whether he's right or not because nobody has ever drilled it.

Grow: Let's just say we do have some multichannel lines that show normal faults going well down into Jurassic in the general vicinity, but I don't really know how close they are to 6021.

Robb: The band that I was talking about is right along the edge of the shelf. I would have ignored it, but it was cited in the Sale 49 Hazards Report as an area where sediment strength might be reduced due to gas.

Geotechnical Analyses of Submarine Landslides in Glacial Marine Sediment, Northeast Gulf of Alaska

William C. Schwab and Homa J. Lee

ABSTRACT[1]

Glaciation is the most important process contributing sediment to the northeast Gulf of Alaska. Large sediment failures within the Holocene glacial-marine sediment of the Continental Shelf have been identified on slopes as gentle as 0.5°. The major offshore processes responsible for sediment failure in the Gulf of Alaska are earthquake and storm-wave loading coupled with cyclic shear strength degradation. A normalized soil parameter (NSP) approach can yield shear strength parameters that are somewhat independent of coring disturbance by normalizing these parameters by appropriate consolidation stresses. The NSP approach also appears capable of aiding in the extrapolation of surficial sediment properties to the subsurface. Laboratory tests using the NSP approach, supplemented with in-place vane shear data, reveal that for these glacial-marine sediments, clayey silt with a natural water content between 35 percent and 45 percent is most susceptible to cylic loading. Cores that contain more of this susceptible clayey silt roughly correlate with locations of sediment failure features. A simplified analysis shows that, in water depths shallower than 35 m, maximum storm waves would produce shearing stresses greater than stresses induced by maximum earthquakes. In deeper water, earthquakes would produce greater stresses. Differences in failure morphology are difficult to relate to advanced geotechnical parameters but likely relate to observed variations in plasticity, slope, angle, water depth, or variations in consolidation state.

[1]1983, *in* Molnia, Bruce F., ed., Glacial-Marine Sedimentation: New York, Plenum Publishing Corporation, p. 145-184.

Monitoring Sediment Instability on the Mississippi Delta: Project SEASWAB

Wayne Dunlap

The speaker reviewed the setting of Mississippi Delta sedimentation and contrasted the high hydrostatic pressures developed there with areas where slow sedimentation (less than 2 m/kyr) takes place and geostatic pressure is dominant. In the delta area, shear-strength profiles remain almost vertical (very slight strength increase with depth), which is typical of underconsolidated materials. To investigate this phenomenon, piezometers to measure pore pressure were set near an oil rig in South Pass Block 28. This experiment was known as Project SEASWAB (Shallow Experiment to Assess Storm Waves Affecting Bottom). Instruments were deployed in two areas, one inside a collapse depression and one outside. The deepest piezometer set in the collapse depression was 51 ft [15.5 m] below the mudline. It, and others, showed that at some depths the sediment wasn't just underconsolidated but that excess pore pressure was so high that the pressure was equivalent to the weight of overlying material; that is, at 15 m below the mudline there was no discernible effective overburden weight. Other measurements confirmed the low consolidation state of the sediments both within and outside of the failure features. If wave pressure pervades sediment like this, no slope is needed for it to move.

The speaker then outlined the effects of waves crossing a fixed point over a sloping bottom. Most deformation would occur underneath the crest of the water wave, and least deformation under the trough. He summarized the analysis by treating the soils as a nonlinear viscoelastic material. For the case cited, the shear strength is about zero at the bottom and increases to a maximum at 27–28 ft [8.2–8.5 m] and drops to a low value [about 2 lbs/ft2] immediately below that depth. With further increase in depth, the strength starts to increase again. This is called the crust and cutback zone.

According to the speaker's analyses, if this sediment is subjected to the passage of a 50-ft-high [15.2 m] wave with a period of about 9.5 seconds in water 220 ft [67 m] deep, it will move about a foot [0.3 m] at the bottom. At 40 ft [12.2 m] where the minimum shear strength is located, it will move 1.6 ft [0.5 m]. The maximum excursion of a sediment particle then is about 3 ft [1 m] from the passage of a wave crest to the passage of a wave trough. As the pore pressure goes up, the shear strength goes down. On a 0.2-percent slope and shear strength of 60 lbs/ft^2, the speaker's analysis predicts downslope movement of about .001 ft/sec. If a storm lasted 6 hours, net downslope displacement would be 25 ft [7.6 m]. With weaker soils, downslope motion increases dramatically. These estimates of shear strength may, however, be too low.

During passage of Hurricane Anita and soon after Hurricane Babe, the piezometers indeed did show an increase in pore pressure that declined between and after the storms. Some accelerometers on the device actually went offscale during the storm passages.

The shear strength in a sediment may decrease with depth depending on the size of the waves. The maximum shear stress lies at a depth of 0.16 times the wave length of the water wave. Thus, for a 50-ft-high [15.2 m] wave [wavelength 450–500 ft (137–152 m)], maximum stress and movement will occur at about 75 ft [23 m]. A change in wave height of as much as 15 ft [4.6 m] does not make too much difference, but a change in sea level of a magnitude associated with a glacial advance would clearly produce much greater downslope movement in deeper waters.

Discussion following Dunlap's talk:

Prior: You start with the shear-strength profile that does not have a cutback in it, your regular model. Can you actually produce a cutback or reduction in strength at a particular depth for a particular wave height?

Dunlap: Yes. If you start out with a uniform shear-strength deposit, and slosh a wave back and forth on top of it, you'll find that you build up pore pressures at a depth of about 0.16 times the wave length. In our particular case there, that was about 75 ft [23 m]. The pore pressures will build up so that after the storm is finished, you should have a zone of high pore pressures at about that depth that would create a zone of decreased shear strength. When the next storm comes along, you start with a lower shear strength to work on in that zone, and you'll probably get more and more hazard potential. So yes, you can produce that cutback just with waves.

Booth: I wonder if you'd comment on how well the accelerometer record corresponded with the predicted horizontal movement.

Dunlap: What little we have looked at in the horizontal deformation is that the accelerometer measurements that we made show that the displacement was about what we expected, but we've only looked at the horizontal direction, not at any permanent downslope movement.

Prior: Would you care to comment about these extremely high excess pore pressures measured—this concept of a kind of a zero effective stress or negative effective stress? Is that physically possible? Or is there an instrumental problem here? How do you feel about that?

Dunlap: Well, at first, I thought the instruments were bad; I thought our pore-pressure measurements were incorrect. As you know, I pushed heavily for SEASWAB II to let us go back and make some correct measurements. But I think the pore-pressure measurements are probably right. There are a couple of things that can be influencing them. One of them is the gas, and you know we've argued this one out. How important is the gas in pumping up the pore pressure? But one that does seem to be very important, and I don't think we've paid enough attention to, is a storm wave. We've got all sorts of storm waves, from 50-ft-high [15.2-m] waves to 10-ft-high [3-m] waves; so the depth of maximum shear stress goes anyplace from just below the surface down to maybe 100 ft [30.5 m] below the surface for a killer wave. So as the waves move back and forth across the bottom and over a several-year period when they have the full effect of the storms, I think that that in itself can churn up these sediments, build up the high pore-water pressures that we've been seeing, and make them much higher than those that we would be predicting by a Henkel model or anybody else's model for just deposition alone. [There are] at least two ways, gas and generation of pore-water pressure by movement, that cause that to happen.

Bea: Would the third wave be the device itself, moving within the medium, that has inertia and much greater stiffness, noncompliance with the medium, and if they begin to move the bottom and the device does its own thing, isn't it reasonable to expect that it will generate locally the kind of pore pressures adjacent to the measurement?

Dunlap: Yes, I think it's possible that you can have the sediment-structure interaction effect. The only thing is that after the storm has left, say even several days afterward, we still have high porewater pressure. Not quite as high as during the storm, but it is significantly higher, and by that time, that effect should have died out. So it may have influenced our peak measurements during the storm, but I don't think it would have influenced our long-term measurements of, say, several days after the storm.

Bea: How many days did it take the device originally, when you placed it, to assume the ambient levels?

Dunlap: Depends on which device and which level. Actually, it took about 30 days to get the ambient at the lower level. But after the February storms, for example, the thing stayed up there for about 90 days. So it had plenty of opportunity to dissipate. There is one thing that we can't figure out, because what it looks like is that, instead of being a self-healing situation, it's just the other way around. We're talking about an area in which active deposition has not occurred for a hundred or more years, I guess. You would think that porewater pressures ought to be just fading off and that the area ought to be getting better and better, but during our measurements, the area got worse and worse.

Bea: Looks like, if you carried that to its ridiculous conclusion, that all 500 platforms in the area would now be seaward.

Geotechnical Characteristics and Instability of Deep-Sea Sediments

Armand Silva

This talk included properties of deep-sea clays, the stress system of the sediment column relating overconsolidation and underconsolidation of the sediments, slope stability analyses, and long-term creep of deep-sea sediments.

Before going into the details of the elements listed above, the speaker outlined the areas where geotechnical studies should be carried out and reviewed some of the techniques needed to study them. For example, conduct more detailed studies of existing slides and unstable areas, investigate areas where potentially unstable materials are accumulating, and, finally, initiate sediment movement by explosions or trenching. Techniques to apply to these situations include acoustics, photography, improved sampling, in place measurements, and more sophisticated laboratory studies and computer modelling.

The speaker then turned to a detailed description of deep-sea sediments. Though smectites may have in-place water contents of 240 percent, many of the physical properties of deep-sea sediments are similar to those of clays studied on land. However, this does not include the compressibility index, which the author pointed out was much lower for deep-sea sediments than for those on land. In addition, the undrained shear strength of deep-sea sediment in the upper 2–3 m is anomalously high. The resulting high overconsolidation ratios in the upper few meters may be due to high interconsolidation bonding stresses or cementation that isn't broken down until 3–4 m of sediment have accumulated allowing normal consolidation to proceed. In contrast, some deep-sea cores show the opposite. A giant piston core from the Bermuda Rise shows underconsolidation in the upper 6 m. It may be due to high rates of sedimentation—about 200 cm/1,000 yrs—in the area.

The speaker then went on to discuss analyses of a suite of cores collected near the Bermuda Rise. The water content of the material draped over the upper slope was highly variable (20–200 percent). The highest values were in an area of high sedimentation rate and resulted in low shear strength in the upper 10 m. On the slope, shear strength increased linearly with depth. However, in the hummocky region at the base of the slope or on the upper rise, low-strength material was again present. There, based on infinite slope analysis, failure (slumping) could be initiated on a 10-degree slope that was loaded with 2.5 m of sediment.

Finally, the talk turned to the analytical techniques being used at the speaker's laboratory to study creep behavior in deep-sea sediments. The upshot of this work indicates that creep rupture occurs at stress levels that are considerably lower than those for terrestrial clays. Thus, creep may be responsible for large-scale vertical and lateral displacements of sediment on scarps or slopes in the deep sea.

Discussion following Silva's talk:

Booth: I wondered, when you were hypothesizing about whether there was any powerful binding cements in the Pacific clays, if the samples showed any abnormal peak in shear strength.

Silva: They were not unusual. The sensitivities of the illites of the upper few meters were higher than the smectites. I was a little surprised at that. But there was not anything unusual there. The pore-pressure parameters, for example, were what you would expect.

Bloom(?): If you wanted to introduce slope failure into that stuff, would you poke water into it? Is that a feasible method of producing explosion? Physically, is there a way you do that by injecting water?

Silva: I think it would be difficult to inject the water uniformly within a given zone. You have a point source. It may be difficult to do it.

Sangrey: You showed two of your series of tests that went to the stage of creep rupture. You showed a specific illustration of one that amounted to shorter time, something less than a day. How were you sure that that was a drained rather than an undrained phenomenon?

Silva: We measured pore pressures for them, and we were running undrained also.

Questioner: Undrained creep I can understand, but drained creep is a real phenomenon?

Silva: That's a good question. I really don't know. All we know is that we did get creep rupture at that stress level. Whether or not we had pore pressures generated there is a good question. I would expect that, over a day, most of those pore pressures would have dissipated. From our experience, we can see that it did.

Geotechnical Properties at Sites Having Over- and Underconsolidated Sediment

Harold W. Olsen

ABSTRACT

Piston cores and high-resolution seismic-reflection data were collected on the upper Continental Slope in the Baltimore Canyon OCS Lease Sale 59 area by the U.S. Geological Survey during September 1979 to obtain information on the stability of near-surface sediments in a variety of locations on valley axes, valley walls, and intervalley ridges in water depths ranging from 328 to 1,342 m.

The stability at 31 sites was examined in terms of the infinite-slope stability model by using geotechnical profiles of the sites, together with sea-floor gradients interpreted from geophysical and bathymetric data. The geotechnical profiles include information obtained from the piston cores on the physical properties of the materials, their consolidation states, and their drained and undrained shear strengths in terms of normalized soil parameters.

The majority of the sites contained overconsolidated materials, and their safety factors generally exceeded 2.0 for both drained and undrained conditions. Underconsolidated materials were found in about one-third of the sites. Among these, low safety factors for undrained conditions (on the order of 1.0 or less) were obtained at five sites where sea-floor gradients were on the order of 15° or greater. Four of these sites were on valley walls, and one was on an intervalley ridge.

Compared with previously reported geotechnical information on near surface Atlantic Slope sediments, the materials in this study have a limited range of plasticity characteristics but a similar range of consolidation states and shear strengths. Moreover, variability in the latter occurs on a more local scale than variations in the morphology of the Continental Slope. (from Olsen and others, 1982).

The materials exhibit normalized behavior, and their index property correlations with consolidation and strength parameters are reasonably consistent with well-known correlations in the literature. Disturbance effects appear to have been effectively minimized in the consolidation and strength parameter determinations by using stresses and stress histories in excess of those in place. However, disturbance effects were substantial in the consolidation-state determinations, as revealed by a comparison of consolidation-state values derived from the consolidation data by using the Casagrande method, and also with values derived from unconsolidated-undrained laboratory-vane data and consolidated-undrained triaxial data by using the SHANSEP (Stress History And Normalized Soil Engineering Properties) method. This procedure evaluates normalized strength parameters for the soil as a function of over-consolidation ratio and stress system. These are applied to the stress history of the foundation to give a strength profile for use in design. The values from the triaxial data are high compared

with those from the laboratory-vane data, consistent with known disturbance effects. Furthermore, the values from the consolidation data are generally low compared with those from the laboratory-vane data. Because the latter values are probably low due to disturbance, it appears the values from the consolidation data are appreciably less than in-place values (from Olsen and others, in press).

The data supporting these findings are reported by Olsen and Rice, 1982.

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GROUND-WATER PROCESSES

Influence of Hydrothermal Tectonism on Shelf-Slope Stability, Northern Gulf of Mexico Basin

R.H. Wallace, Jr.

In a paper in 1974, John Jones and I made a statement that I don't think would surprise anyone here—that the northern Gulf of Mexico basin is certainly on no one's list of seismically active areas. And yet you see a great number of faults here. Because we've shown them onshore on this slide (fig. 1), don't be fooled; offshore, it's at least that complex, if not more so. What goes on out there is kind of a deep dark secret between industry and the Government. Actually, right now, growth faulting is occurring at the shelf-slope break.

In the 1974 paper, Jones and I discussed a new idea concerning the upflushing of waters from great depth and the part that this would play in faulting or [in] facilitating fault movement. We (Jones and others, 1976, p. 82) called this process hydrothermal tectonism. Jones (1975, p. 37) applied this to all aspects of subsidence, downwarp, normal faulting, diapirism in sediments resulting from the heating and thermal diagenesis of clay minerals, and the consequent release of bound and crystalline water, as well as deformation of deposits that might be caused by the influx of hot water.

Hydrothermal tectonism, then, is keyed to thermal diagenesis of montmorillonite as described by Powers (1967) first, then others. According to Jones (1975, p. 37), conversion of montmorillonite to illite and mixed-layer clays, wherever the temperature reaches or exceeds 212°F (100°C) causes the accompanying clay layer to begin to slurry. I'm not so sure it becomes a slurry, but, to say the least, there is a loss in load-bearing strength, and there is a definite change from the bound state to the free state of the water from about 1.4 g/cm^3 to 1 g/cm^3, with a concurrent increase in volume. So we have a mechanism here for increased pore pressure. Powers (1967, p. 1249) suggested that the conversion of montmorillonite to illite could generate sufficient pressure to support the total weight of the overlying rocks. There's some disagreement as to whether this is necessary, really; I don't think it is. What you create, then, is a state of flotation in the sediments similar to that described by Hubbert and Rubey in 1959.

The pressure buildup is a function of the rate of water formation, the conversion rate, and the rate of escape of the fluids from the fine-grained rocks. So you have to consider the rate of escape of the fluids. According to Burst (1969), the conversion of the clay and release of bound water is dependent on the sediment having attained the critical temperature of 200° to 230°F [93–110°C]. The important thing is that it is not depth dependent; it does not depend on depth at all. The conversion is nearly complete at a temperature of around 280°F, according to Burst. This may be true for Texas clay, but new information coming out seems to indicate that the range probably runs from about 212°F, [100°C], up to 280°F (137°C) for the Texas clays. But because clays are different over in the Mississippi Delta area, the range seems to be more like a beginning temperature of around 180°F (82°C) and going on up to a temperature of 300°F (149°C).

Concerning the fact that rapid loading alone can increase fluid pressures in the fine-grained rocks (we've seen that in a number of papers), I think

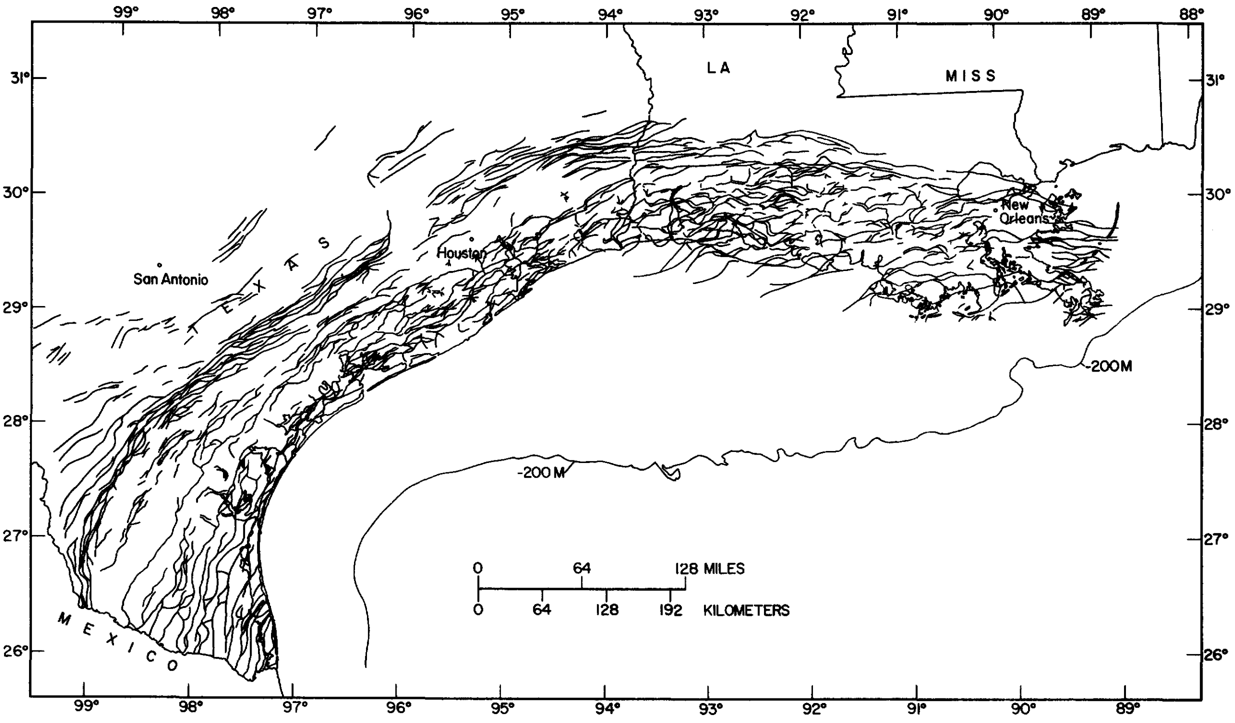

FIGURE 1.—Major onshore fault trends in the northern Gulf of Mexico basin.

that once you get this temperature of 212°F (100°C), or you get the conversions going on, you have an additive effect there in the geopressured zone. In other words, the greatest instability would be created where this loading effect, increasing the fluid pressure, would be coupled with the thermal change taking place to further increase or add to the already high pore pressures. And where does this occur? Of course, in the geopressured zone of the northern Gulf of Mexico basin.

This regional map (fig. 2) shows geopressure occurring at depths less than 6,000 ft [1829 m] (unpatterned area) to greater than 15,000 ft [4572 m] (dot pattern). Actually, it occurs at depths less than a couple of thousand feet to greater than 18,000 ft [5,486 m]. Figure 2 shows the depth of occurrence of, in this case, a fluid-pressure gradient of 0.5 psi/ft or, if you prefer, the top of the transition zone as opposed to the top of the super normal pressures or low-density shale zone that is pegged at about 0.7 psi/ft of depth.

The upwarps and downwarps of this surface are a function of the number and thickness of vertically interconnected sandstones in the sedimentary sequence. They are also a function of formation geometry and facies change, as well as geologic structure; in other words, faulting and diapirism.

Now, beneath this geopressured zone, we have a very thin crust (fig. 3) the Mohorovicic disconformity (MOHO) rising on the Outer Continental Slope area to as shallow as, say, 65,000–70,000 ft [19,812–21,336 m] beneath the Continental Slope. Heat is conducted upward to the base of the salt layer. You know salt is an excellent heat conductor and becomes a perfect plastic at around 572°F (300°C); I think it flows in one direction at about 302°F (150°C). This is supplying heat to the base of the sedimentary pile, producing the diapirism or what-have-you, as well as facilitating the faulting. What we're seeing here is the mechanism that brings heat into contact with the sediments in the basin and lowers the viscosity of the fluids therein.

Here's an example of hydrothermal tectonism from the offshore Texas area (fig. 4). While I have this slide (fig. 4) up here, I'll just point out the cross section (*A–A'*) that we'll look at later. The dot pattern shows where the structural geologic datum in this area occurs, at a depth of about 3,800 ft [1,158 m] or less, and the diagonal pattern shows where it is, at a depth of about 13,000 ft [3,962 m]

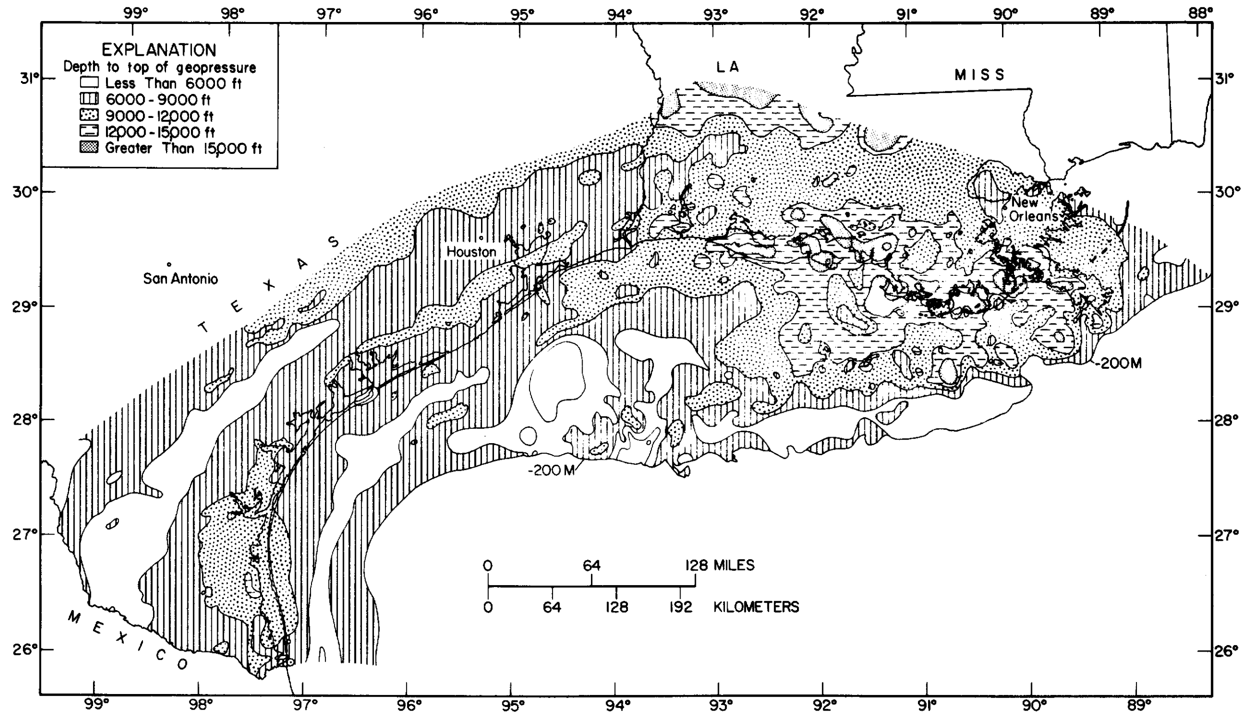

FIGURE 2.—Depth below mean sea level of the occurrence of the top of the geopressured zone in the northern Gulf of Mexico basin.

or more. What I'm showing here is that loading has occurred, with the salt moving out into the diapirs (cross-hatched area, lower right corner), and an upper Miocene depocenter has formed as a result of salt and shale deformation. See the faulting running across the encircled area here?

It's associated with what's called the Brazos Ridge or Anticline, a shale ridge. Here's a seismic line (fig. 5) across it. Shale mass B is the core of the anticline.

In this area (fig. 5), there are two types of complex faulting, differential compaction and gravity slide, associated with shale masses A and B. I want you to notice the decrease in the slope of the fault planes with depth, particularly along the southeast flank of shale mass B. This particular feature has been described by Bruce in a 1973 paper as being formed by stillstand-type deposition where a substrate of marine clays, overlain by sand and shales, was overridden but subsidence kept pace. In other words, seaward deposition was accommodated by subsidence. Then normal faulting with continuing deposition occurred. Later, sliding occurred, and a hypothetical gap was created. This resulted from a pressure differential I'll show later on. And then there was a collapse of sediment and development of these antithetic faults back into the main fault plane.

On a different, larger scale—this is the same sort of thing we were discussing in some of the papers yesterday—you see the scale there (fig. 5): 10 mi [18.5 km] across with about 10,000 ft [3,098 m] of vertical relief.

For compaction to occur at depth in the geopressured zone, and geopressured in this particular case is somewhere between 5,000 and 10,000 ft [1,524–3,048 m] or so, the water must get out. The most obvious means for the water getting out is up the fault planes, but there are many papers still where you read that faults in unconsolidated sediments do not act as conveyors of fluid. We'll see about that. You can see the faults in this particular area (fig. 6)—this is *A-A'*—come up and intersect pretty close [to] the uppermost Pleistocene surface, thus suggesting an active system. This (fig. 7) is not a stick diagram from the fault we saw earlier (fig. 5); we had to go to another area to pick this one up. This was published also by Clement Bruce (1972, p. 31). [It is from an area where we had] some fluid-pressure gradient data from wells that could be related to this type faulting. You see between wells 1 and 2 an angle of 60° on

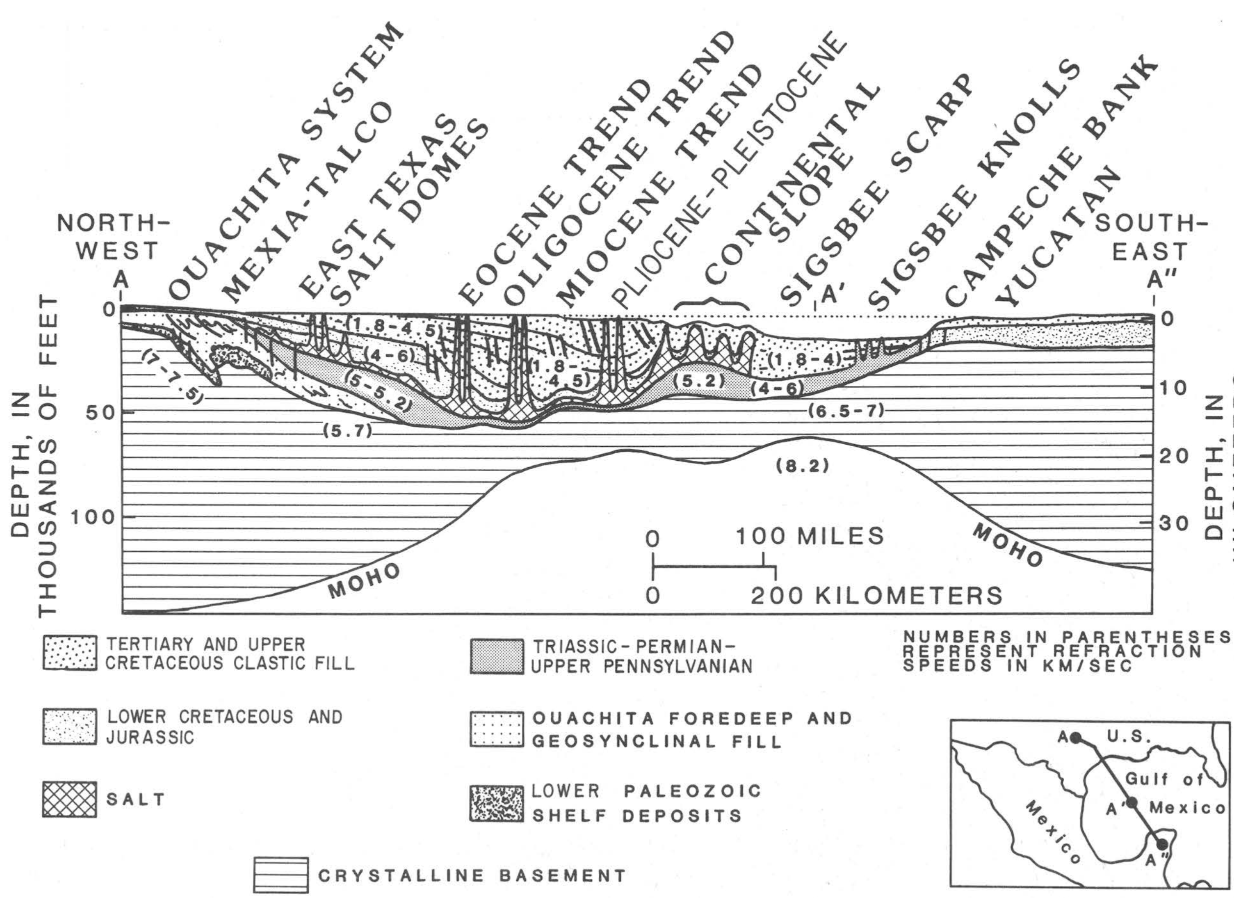

FIGURE 3.—Profile section through the Earth's crust showing the sedimentary fill of the gulf coast geosyncline, the rise of the Mohorovicic disconformity (MOHO) between the Oligocene Trend and the Sigsbee Knolls, and the effects of salt tectonics. Adapted from Lehner, 1969.

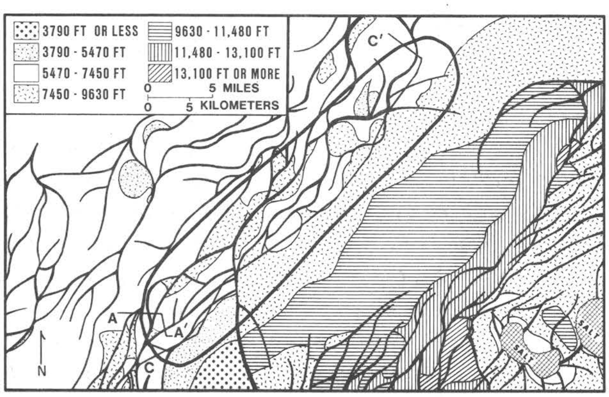

FIGURE 4.—Generalized geologic structure in the Brazos Anticline area (enclosed by heavy black line), offshore Texas. See fig. 6 for profile *A-A'* and fig. 10 for profile *C-C'*.

the fault. You see a fluid-pressure gradient of 0.702–0.811 psi/ ft, but in between wells 2 and 3, [where] the gradient rose from 0.811 to 0.952 psi/ft, you get a flattening of the fault plane to 15°. This flattening, sometimes becoming bedding-plane faulting, is in response to geopressure. Here's the base of normal pressure [and the] top of geopressure, denoted by the heavy black line.

Movement of fluid (fig. 7), because of the pressure differential, is going to be back away from the high to the low gradient and up and out of the system. Now, this (fig. 8) doesn't show it quite right; you should have a bowing on these beds that comes down adjacent to the fault zone. You are going to pull more and more of these sand and shale beds downward with movement past this critical 230 °F [110 °C] thermal boundary and initiate additional thermal diagenesis.

We're showing here (fig. 8) the migration of the fluids out of the shales into the sands, and landward and up the fault plane. It seems to me that these fault planes should certainly be more transmissive than the shale layers in that there are

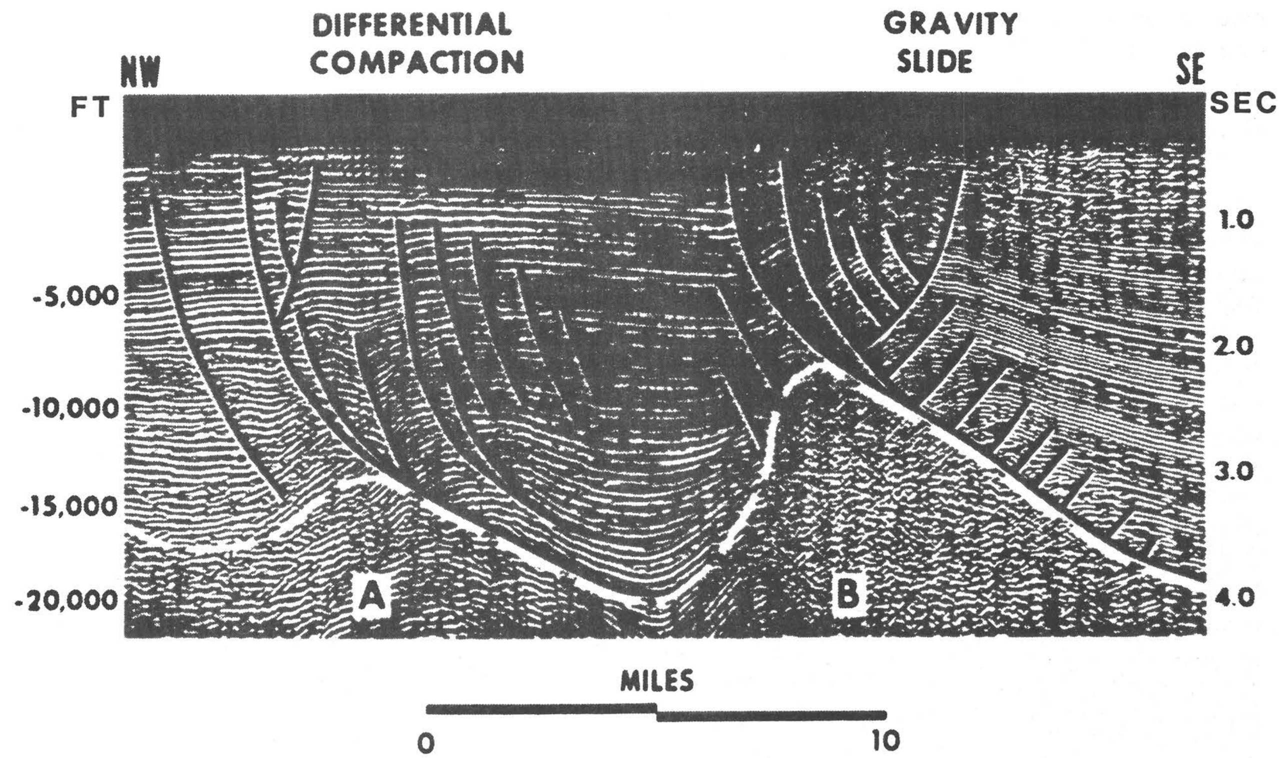

FIGURE 5.—Seismic section showing fault systems in relation to shale ridges. From Bruce, 1973, p. 881, fig. 4. A and B denote shale masses.

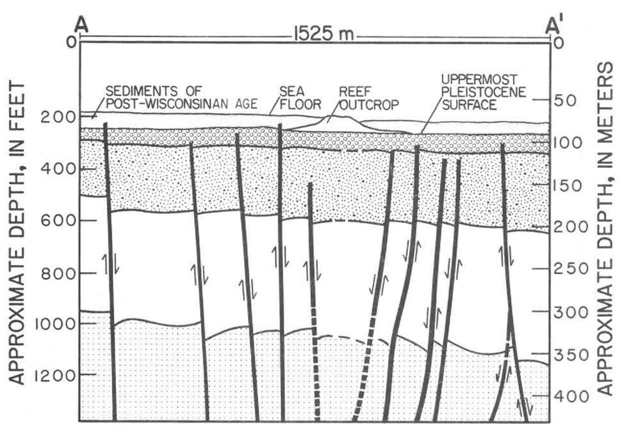

FIGURE 6.—Profile section (from Berryhill and Trippett, 1981) showing extension to near sea bottom of the fault system associated with shale mass B, figure 5. Profile location shown on fig. 4. After Berryhill and Trippett (1981).

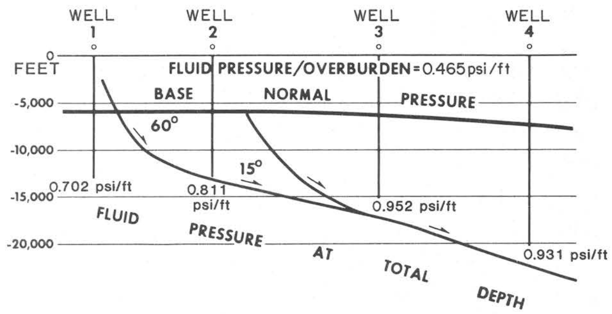

FIGURE 7.—Stick profile showing well locations with bottom-hole fluid-pressure gradients relating changes in fault angle to increasing pressure.

mashed sediments in there. The problem is getting measurements of some sort on just how this is occurring. We know in the Baton Rouge area and in the coastal terraces that we are getting movement on these faults (about 5 ft [1.5 m] in one case there on the Baton Rouge fault) within the last 15,000 to 30,000 years, but all this is extra-qualitative information.

In a 1977 paper, I tried a different technique on a Miocene occurrence that we'll see later on. It had to do with the thermal conductivity. The earlier papers all counted on conductive heat flow, the conduction of heat in the materials involved, for the heat transfer or the heat distribution. What we needed to show was a different ball game. I used a formula worked out by Guyod in 1946 for calculation of approximate absolute thermal conductivities of sediments from borehole-type information:

K (thermal conductivity, C.G.S. units) = 1/2 G_R (reciprocal gradient or vertical distance in feet for a temperature change of 1 °F) $\times 10^{-4}$.

If we assume conductive heat flow and calculate the thermal conductivity between the runs on this particular Brazos Anticline area well (fig. 9), we get, between the last two runs, a thermal conductivity of 0.154 Btu/ft/hr/°F. The thermal conductivity of water is 0.339; air, 0.12 Btu/ft/hr/°F; and the lower end of the range for shale (water-filled shale) is about 0.48 Btu/ft/hr/°F. In the next interval, we see 0.375 Btu/ft/hr/°F. This indicates to me that we're not dealing with conductive heat flow. We have to assume that we are dealing with forced-convective heat flow—a mass transfer of heat influenced by upflow of hotter fluids from greater depths than we are dealing with right here, say 12,000 to 14,000 ft [3,658–4,267 m]. I would expect those temperatures—we see 287 °F [142 °C] here, 337 °F [169 °C] at the bottom—out in this Miocene-Pliocene-Pleistocene environment to be found in some cases at depths at least 5,000 ft [1524 m] greater. Along this strike section (fig. 10), you can see all temperatures are quite high, ranging from 267 to 337 °F [131–169 °C]. Mud weights converted to gradient range from about 0.753 psi/ft up to 0.939 psi/ft, indicating quite high pressure, and there is evidence (higher-than-normal temperature) that we're getting convective heat flow in the sandstones at, or just above, the bottom of the hole there (well 9). To show the hydrogeologic setting, here is the 212 °F [100 °C] geotherm; this would be the critical temperature for diagenesis. And, of course, normally pressured sediments here above the top of geopressure line. The geopressured zone begins at depths between about 4,500 and 9,500 ft [1,372–2,896 m].

The other example (fig. 11), published in 1977 (Wallace and others), had to do with two wells down in the southern part of Padre Island, Tex.—well A to a total depth of 10,540 ft [3,215 m] and well B to a total depth of 10,021 ft [3,054 m].

Questioner: Was your transition to the geopressured zone very sharp?

Wallace: It can be a few hundred feet; it can be several thousand feet. The [thickness of the] transition zone itself depends on how much [interconnected] sand you've got in it [the interval].

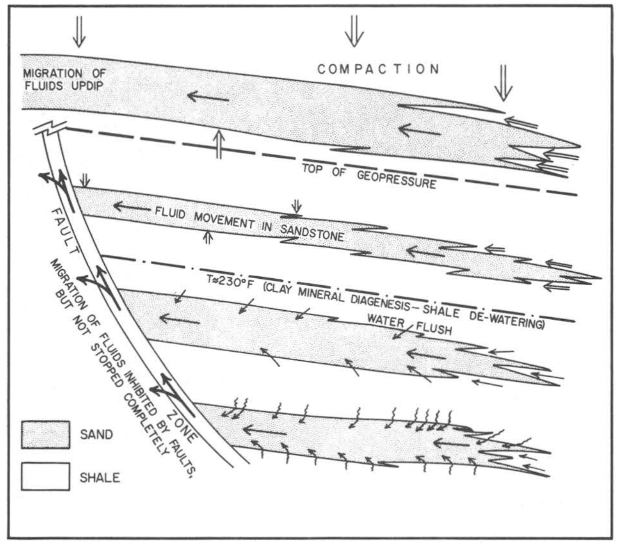

FIGURE 8.—Diagrammatic dip profile showing avenues of migration of fluids from geopressured sediments in relation to temperature of clay mineral diagenesis.

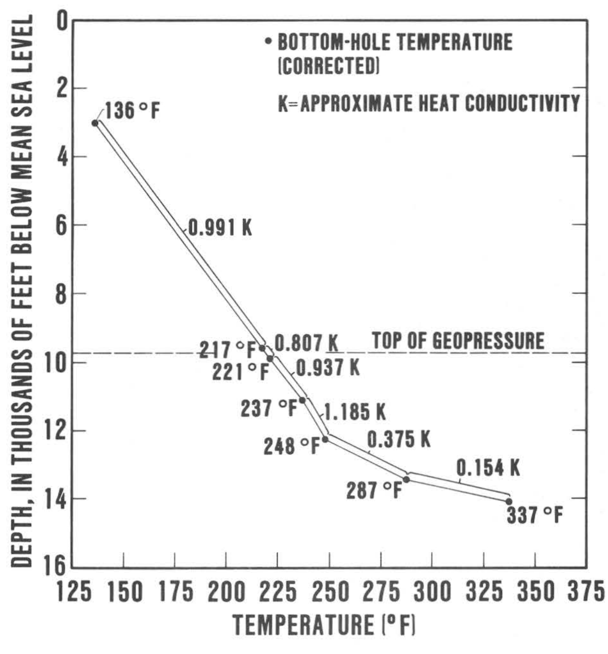

FIGURE 9.—Corrected bottom-hole temperatures and calculated approximate thermal conductivities in well 9 from the Brazos Anticline area.

In this particular instance (fig. 11), we have well A that was drilled away from this major fault, and well B that seems to have come right into the plane of it. The structure is dipping off to the southeast from about 9,500 ft [2,896 m] down to 11,500 ft

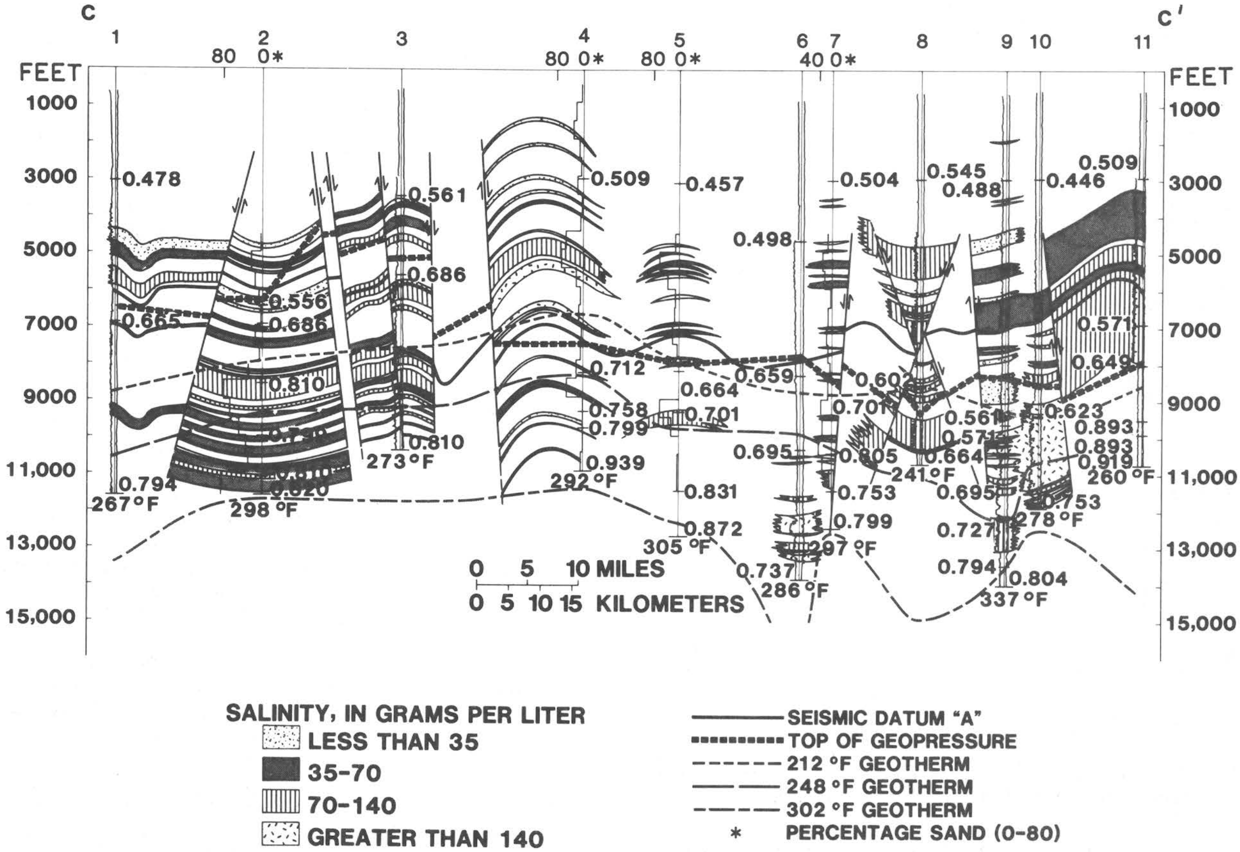

FIGURE 10.—Hydrogeologic cross section *C–C′* from the Brazos Anticline area, offshore Texas, showing variations in salinity, temperature, and pressure. Profile location shown in fig. 4.

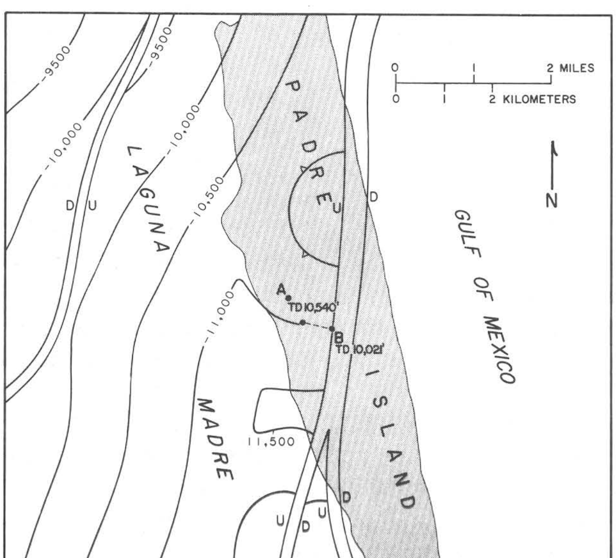

FIGURE 11.—Location of wells A and B in relation to geologic structure, Cameron County, Tex. Contours in feet below sea level. Upthrown (U) and downthrown (D) sides of fault surfaces shown.

[3,905 m] or so, and you can clearly see the component of separation on the easternmost fault. This indicates most probably that you have one of these curved fault planes that flattens with depth.

This is a stick diagram (fig. 12) showing the relation of faulting to the wells. Shown is well A, where the temperature is 238 °F [114 °C] at 10,536 ft [324 m]. If we calculate approximate heat conductivity between the geotherms in this area, we come up with reasonable numbers for thermal conductivity. When we calculate in well B thermal conductivity between a depth of 6,955 ft [2,120 m], where 180 °F [82 °C] was recorded, and 9,774 ft [2,979 m] (true vertical depth) where a temperature of 359 °F [182 °C] was recorded, we get the same sort of picture in the Miocene here in south Texas that we got in the Miocene-Pliocene-Pleistocene offshore—unreasonably low value of 0.19 Btu/ft/hr/°F, again indicating that you're getting mass transfer of fluids—heat in fluids moving from

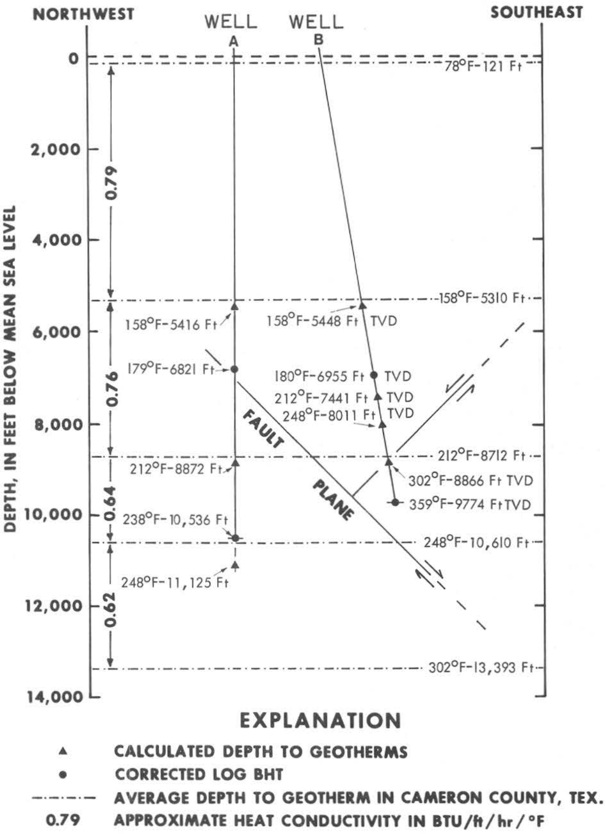

FIGURE 12.—Example of heat transport by conduction versus forced-convection in two adjacent wells (A and B) in Cameron County, Tex. TVD = true vertical depth.

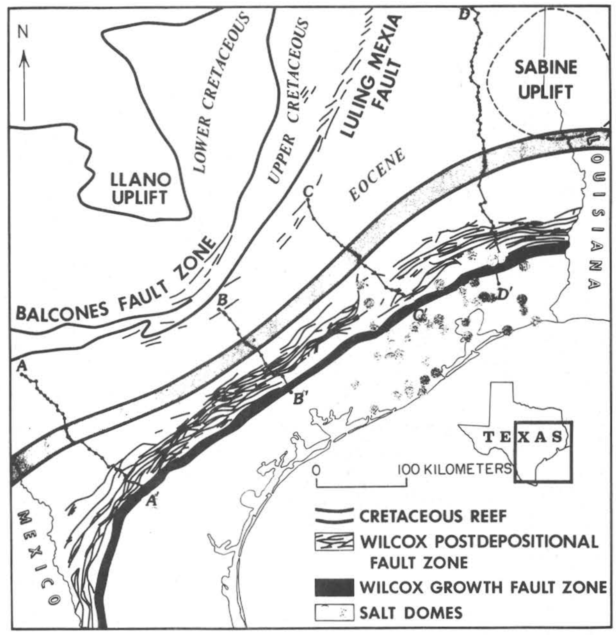

FIGURE 13.—Location of Wilcox (Eocene) cross section *B–B'* in relation to the Cretaceous reef, Wilcox postdepositional fault zone, and Wilcox growth fault zone. Other cross sections shown are not discussed in this paper.

greater depth up the plane of the fault. Now, the idea is that, as this heat moves up as a wave, it's going to cause instability by causing diagenesis in the shales at shallower and shallower depths.

One more example (fig. 13), this one from the Eocene. We can see here the trend of the Cretaceous reef. Gulfward and down beneath is the geopressured zone associated with the postdepositional Wilcox fault zone. Take a look at the location of the cross-section lines. This is *B–B'*. The sediments become shalier, of course, in the gulfward direction. Figure 14 shows the cross section, with growth faults; you can see that a wave of heat has come out of the unstable zone and moved upward and landward in the system. Note that the 200 °F [93 °C] geotherm is tracking along the top of the geopressured zone, then all of a sudden it abruptly rises 2,000 ft [610 m]. The 150 °F [66 °C] geotherm is also bowed upward. These postdepositional faults, I think, are adjustments resulting from the diagenesis of the shales in this downdip end.

I think that hydrothermal tectonism and thermal effects in general are an important consideration in any analysis of geologic hazards, particularly deep-seated normal faulting. Fault movement may be sustained or reactivated by this mechanism. Severity of consequences on manmade structures would depend on rate of movement and magnitude of offset.

Discussion following Wallace's talk:

Menard: If you follow the movement of some of these growths. . . Take some of those that crop out at the edge of the shelf now, out there, that we would refer to as present-day instabilities. Many of them continue on down to the growth faults that started, say, in Miocene times, and if you look at their activity (I could be saying Miocene)....a period of growth, and then a period of quiescence with no movement, and a repeated period. What you're saying is it takes these time periods to lubricate enough to cause them to move?

Wallace: Well, I hesitate to use the word "lubricate." We could get into an argument on that. There are all kinds of lubricants—round lubricants, thin lubricants, thick lubricants.

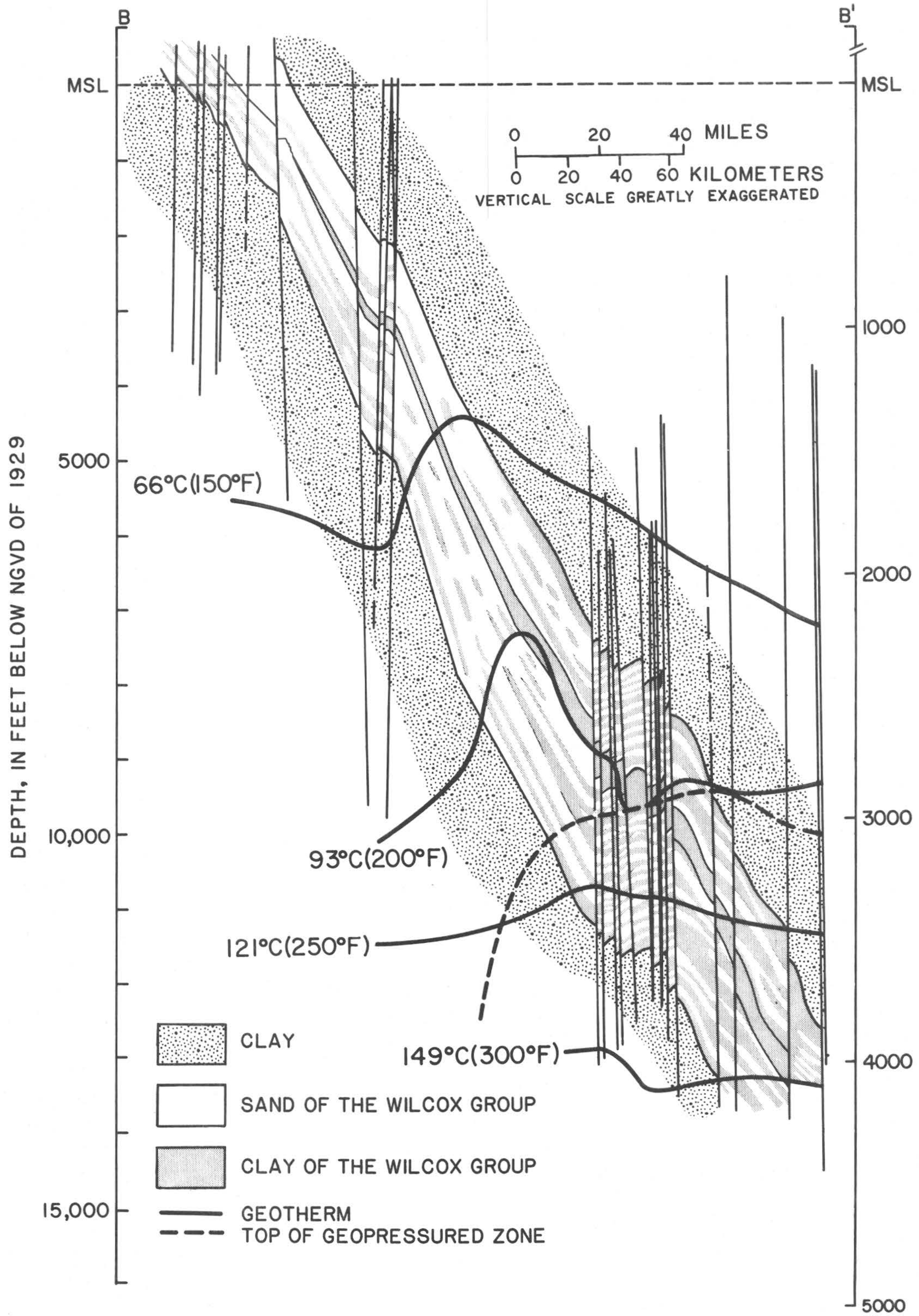

FIGURE 14.—Cross section *B–B'* showing sand and clay deposition in relation to geologic structure, temperature, and pressure in the Wilcox Group of the South Texas Coastal Plain. NGVD = National Geodetic Vertical Datum.

Maybe it is. I kind of like to think of it as putting a big block on rollerskates, sort of a hydroplane, if you want to call it that. But I think we could take a look at those critical geotherms, examine those critical geotherms to see how high they moved in the system. As close to the shelf break as one could get (there's an example here on the Brazos Ridge (fig. 5); you know how hard it is to get information out there). There's no reason, if the formation geometry is right, that we couldn't get some of these faults in the differential compaction time being helped along by the influx of heat coming out of the geopressured zone.

Menard: Is this something that people studying the Atlantic margin don't have to worry about, because their clays on the northern shelf are all illite to start with?

Wallace: I think that's a good assumption. Down here, we start off with about 80 percent montmorillonite, and I think [that] when you get over in the Mississippi Delta it's even higher than that. I think you start off with a younger type.

Menard: Like fire ants, you can keep it.

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Effect of Sea-Level Fluctuations on Porosity and Mineralogic Changes in Coastal Aquifers

William Back and Bruce B. Hanshaw

ABSTRACT[1]

The ocean is the ultimate base level for groundwater regimes. Climatic changes that affect the relationship between freshwater head and sea level can have pronounced effects on ground-water flow pattern, rate of ground-water discharge, position of the freshwater-saltwater interface in coastal aquifers, and amount of mixing within the zone of dispersion. Because part of the water discharged in coastal areas is brackish owing to mixing of freshwater with ocean water, discharged saltwater must be replenished from the ocean. This discharge generates a flow system within saltwater in the aquifer that is related to, but distinct from, the flow system in freshwater.

The zone of dispersive mixing is a highly reactive chemical system. This reactivity results from differences in significant chemical parameters such as temperature, pH, PCO_2, and ionic strengths of the two water bodies. For example, the nonlinearity of the activity coefficient γ_i as a function of ionic strength shows that mixing two waters will yield an activity coefficient less than what the value would be if the relationship were linear.

Because activity equals activity coefficient multiplied by molality, molality must increase and

[1]1983, *in* Cronin, T.F., Cannon, W.F., and Poore, R.Z., eds., Paleoclimate and mineral deposits: U.S. Geological Survey Circular 822, p. 6-7.

thereby cause additional solution of the mineral of interest in order to maintain an activity equal to that in either of the original solutions. This effect can be quite pronounced in carbonate aquifers, and it is possible for a water that is in equilibrium with calcite to be mixed with ocean water that is supersaturated with respect to calcite to generate a mixture that is undersaturated with calcite but supersaturated with respect to dolomite (Hanshaw and Back, 1980).

On the basis of these relationships, we developed a mixing-zone model (Hanshaw and Back, 1979) for the formation of dolomite in the zone of dispersion and hypothesized that the boulder zone of Florida that has conventionally been interpreted as a paleokarst feature may be undergoing dissolution and dolomitization at the present time. Badiozamani (1973) named this model the Dorag model and used it effectively to explain the origin of Ordovician dolomite in the midwestern part of the United States. Land and Epstein (1970) independently developed the same hypothesis to explain the dolomitization of Holocene deposits in Jamaica. The petrologic work of Folk and Land (1975) tended to substantiate the mineralogic changes in the mixing zone. Knauth (1979) and Land (1977) also developed a mixing-zone model to explain the occurrence of chert lenses and nodules from a biogenic opal precursor in carbonate rocks. Magaritz and others (1980) identified dolomite formed at the freshwater-saltwater interface in Israel. Sea-level fluctuations cause a zone of dispersion to oscillate through the carbonate rocks and thereby permit the rocks to be in the diagenetic environment repeatedly.

We investigated this phenomena in Xel Ha lagoon on the east coast of the Yucatan, where we observed a significant amount of freshwater discharging and mixing with ocean water in the lagoon. We hypothesized that this mixing caused pronounced solution features such as straight-walled and rectilinear channels along well-developed fractures. However, detailed mapping of the chemical character of the water and mass-transfer calculations indicated that outgassing of carbon dioxide from the discharged water was a more rapid chemical reaction than dissolution of limestone, and water therefore became supersaturated at the time of discharge. We then were able to demonstrate that dissolution was occurring within the aquifer before the ground water discharged into the lagoons and submarine springs (Back and others, 1979).

Our recent work in the Yucatan has demonstrated that lagoons such as Xel Ha and crescent-shaped beaches result from underground dissolution, which forms caves that later collapse (Back and others, 1981). In cores drilled for stratigraphic information in the Yucatan, we have seen the development of secondary porosity and the growth of dolomite rims on calcite crystals. Frank (1981) has described spectacular dolomite crystals zoned with calcite. He has concluded that these crystals grow in an environment where subtle changes in the chemistry of the water can cause pronounced differences in the resulting mineralogy. We believe that this type of dolomite would develop in a mixing-zone environment.

At Xcaret, a major cave system on the east coast of the Yucatan, scuba tanks permitted us to observe and photograph the differential dissolution of the limestone in the zone of dispersion. Above the water level, the walls of the cave are relatively smooth, but, in the zone of dispersion, tremendous dissolution causes the rock to look like Swiss cheese. This appearance is reported to be very similar to that of reservoir rock of the Golden Lane oil field in central Mexico and also typical of some reservoir rocks in Saudi Arabia. Not only does this dissolution cause a great increase in porosity, but the collapse of the cave roof also forms a precursor to solution breccia. Such solution features occur in other parts of the world but have not yet been determined to result from ground-water discharge. However, Paul van Beers (oral commun., 1982) of the Free University of Amsterdam suggested that many of the solution features in the Algarve of the southern coast of Portugal may be the result of ground-water mixing and discharge. Therefore, we believe it well documented that the zone of dispersion can increase porosity, is a suitable environment for dolomitization and chertification, and can provide conditions for formation of solution breccias.

By applying these ideas to Cretaceous sediments in Morocco, we can hypothesize that the manganese deposits occurring in Cretaceous dolomites were deposited in a mixing-zone environment. Cannon and Force (1983) show features such as zoned calcite and chert nodules, porosity, chert nodules zoned with pyrolusite, and fillings of pyrolusite in solution breccia. In association with the manganese ore are layers of insoluble residue that presumably resulted from dissolution of the original carbonate material.

The hypothesis is that ground water was discharging near the shore of an anoxic sea in which the manganous ion was mobilized in an organic-rich bottom layer. Ground-water discharge would set up a flow system whereby the saltwater moves into the aquifer, carrying with it the manganous ion that would then be oxidized to pyrolusite by the oxygen in the freshwater. Along with the precipitation of the manganese ore would be the processes of chertification, dolomitization, porosity development, and the concomitant incipient formation of solution breccia.

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ENGINEERING CONSIDERATIONS AND STUDY TECHNIQUES

Engineering Considerations of Continental-Margin Mass Wasting

Robert G. Bea

INTRODUCTION

Oil and gas reserves are being developed on the continental slopes. With this development has come concern for mass-wasting processes. This concern stems from the poorly known nature of the processes and their effect on structures, from scientific curiosity, and from confusion of objectives, motives, and methods for development of continental margin resources.

Engineering considerations associated with continental margin mass wasting are addressed in this paper. Potential effects on design, construction, and operation of structures for exploitation of oil and gas reserves are discussed. Design approaches are highlighted in relation to objectives, risks, structure systems, and strategies for accommodating continental margin mass-wasting processes. Three examples illustrate these approaches—design of a platform foundation in an earthquake area, design of a pipeline in a mudslide area, and the quest for the "shear strength" of a soil.

VIEWPOINT

The viewpoint expressed here is that of an offshore structures design engineer. The approach is pragmatic. There must be sufficient resources available to motivate developing solutions to continental margin mass-wasting problems. Given that there are sufficient quantities of oil and gas, and that the economics of development are attractive to industry, then engineering solutions to such problems can be developed (National Research Council, 1980). Such a viewpoint has been and is being proven in recent developments on portions of the continental shelves and slopes subject to mass-wasting problems (Bea and Audibert, 1980;

Ocean Industry, 1980; Sterling and others, 1979; Sybert and Gass, 1978).

Continental margin mass-wasting problems are very area- and site-specific, as are the engineering solutions. Thus, development motivations, research, and engineering are closely linked to particular areas and industrial objectives in development of resources in those areas.

OBJECTIVES

A primary objective of the design engineer is to configure a structure system so that it can perform its intended functions without undue expense or risk (Bea, 1979). The engineer is charged with developing structures that will permit timely, efficient, safe, and ecologically acceptable development of much needed resources, in spite of always present unknowns.

The research community (industrial, academic, government) are faced with the formidable task of providing the engineer with knowledge of the whens, wheres, hows, and whys of continental margin mass wasting. This knowledge must be provided at an appropriate time and in a manner that can be understood by the engineer (National Research Council, 1980). Rarely can this knowledge be provided so that there are no unknowns or uncertainties.

RISKS

Risks are an unavoidable fact of life for offshore structures. Primary problems are recognizing the risks, deciding how large or small they should be, and translating the decisions to reality (Bea, 1979, 1979a). Risks can be reduced only to the extent that time, knowledge, and money will allow them to be reduced. They can never be reduced to zero, for there will always be unknowns associated with loadings and performance of offshore structures.

Risk management focuses in the factors-of-safety incorporated into structures (increasing their initial costs while reducing long-term costs) and in damage mitigation measures. Decreasing impacts through damage mitigation equipment and procedures can be much more effective than attempting to prevent damage through increased factors-of-safety in the structures.

Past experience with structures on the continental shelves indicates that the largest risks we face are those associated with lack of knowledge. In most cases, a recognized danger can be engineered to manageable proportions. A danger not recognized is the critical flaw.

A primary initial objective in studying continental margin mass-wasting processes is to identify the general nature, locations, and extent of the dangers that might be present (National Research Council, 1980). This identification must not be such that development is impeded. Rather, it must be done so that development is encouraged with the proper safeguards and stimuli of warranted resource development.

STRUCTURE SYSTEMS

Three structure systems are used in drilling for and producing oil and gas:

(1) A system to support drilling and production equipment (tubulars, valves, pumps, chokes) placed below the sea floor. Generally, this is known as the well-conductor system.

(2) A system to protect and support the well-conductor system and to support and protect drilling and production personnel and equipment located above the sea floor. These are fixed and floating platforms, guyed or vertically moored platforms, islands, and sea-floor chambers. Generally, this is known as the platform system.

(3) A system to transport produced hydrocarbons and other solids, liquids, and gases produced or used in the course of exploiting the resources. These are the pipelines, barges, tankers and other forms of transporters. Generally, this is known as the transportation and supply system.

These systems generally have a very unique characteristic when compared with their onland counterparts—flexibility. Offshore systems can be designed to have large amounts of capacity for both temporary and permanent deformations (ductility) (Bea and others, 1979; Bea, 1979a). Adequate "give" in the system is essential to allow reduction and redistribution of loadings.

Large deformations of the structure system, including the soil component composing part of this system, does not constitute failure, provided that structural elements of this system have been designed to maintain their integrity under the imposed displacements. Unacceptable performance is developed only if such deformations cannot or are not adequately accommodated for in the design (Garrison and Bea, 1977).

ENGINEERING APPROACH

Design procedures for structures placed on the world's continental shelves have developed fundamentally on a base of empiricism—test, analyze, extrapolate, build, observe, and revise. This is not to say that theoretical or technical bases of this work have been low. To the contrary, they have been very advanced.

This approach has evolved from experience in attempting to understand complexities of the sea, its floor, and structures placed therein and on. Thus far, theory has always left the ocean engineer short of understanding reality. This experience, and the humility it brings, has resulted in heavy emphasis on field testing to give insights into reality, guide development of analytical models, and assist in the inevitable calibrations, extrapolations, and accommodation of uncertainties.

Herein lies a warning to those attempting to codify design of offshore structures. Codes and regulations must recognize the need for field testing, backed up with laboratory testing and competent analytical models placed in the hands of experienced engineers. Codes and regulations are necessary, but they must provide the encouragement for new developments and research, and they should provide encouragement of qualified judgment and innovations.

Coping (in an engineering sense) with mass-wasting problems on the continental shelves and slopes has centered in the field of offshore geotechnical engineering. Fundamentally, the geotechnical engineer has three categories of questions relating to mass-wasting problems:

(1) When, where, how and why will (and did) the sea floor move?
(2) What are (were and will be) the characteristics of sea-floor soils that may move and will be required to support the structure?
(3) What are the structural and operational characteristics of the elements to be embedded in and supported on the sea floor, how will they be fabricated and installed, what other loadings (other than soil) might the foundation be subject to, and what are the design constraints? Design constraints center on defining applicable procedures, codes, regulations, manpower, time, cost, and desired safety that are available or necessary to engineer the structure.

Some would define offshore geotechnical engineering as the art of taking bad samples of the sea floor, performing equally bad laboratory tests, or performing equally difficult to interpret in-place tests, and then proportioning foundations of structures to have acceptable performance, economy, and reliability. There is more truth than fiction in this definition. It has been fundamentally the empirical approach that has allowed in excess of 5,000 platforms and many thousands of miles of pipelines to be successfully sited in and on the sea floor of the world's continental shelves. Now, attention has been turned to the Continental Slopes and Rises.

In the author's opinion, this engineering approach should not be abandoned. It should be supplemented with intelligent field data, laboratory tests, and analytical models that relate to unique characteristics of mass-wasting processes on the continental slopes. This knowledge should be directed and developed to provide information with which to answer the three categories of questions cited.

FOUNDATION DESIGN

A foundation for a fixed, bottom-supported platform is to be designed for an area in which bottom movements (earthquakes) and storm waves are hazards (Bea, 1979; 1979a). An ideal foundation for storm wave forces is one strongly and firmly attached to the sea floor and having limited deformability.

An ideal foundation for earthquakes is one flexibly attached to the sea floor. A cable-stayed, floating platform that would allow the sea floor to move and not transmit the movements to the superstructure would be ideal. The more strongly and stiffly the foundation is attached to the sea floor, the more strongly the sea floor motions are transmitted to the superstructure.

The problem is illustrated in figure 1. As wave height (H) is increased, the maximum load imposed on the structure (Pmax) is increased. As ground motions (a) increase, the maximum loads induced in the structure increase and are very dependent on foundation stiffness and strength.

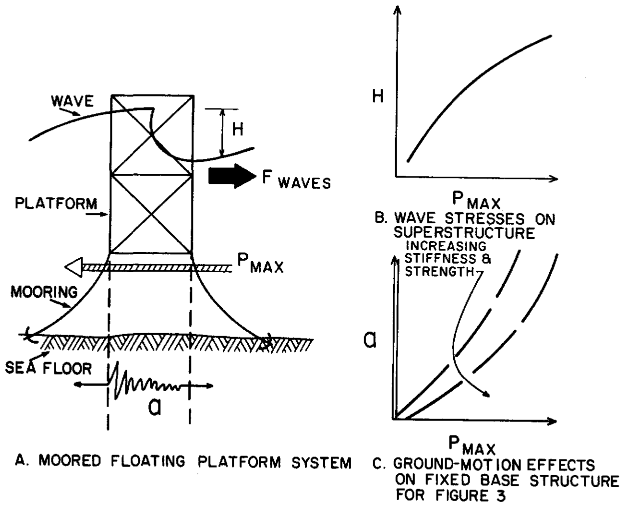


FIGURE 1.—*A*. A cable-stayed floating platform is minimally affected by ground motion due to earthquakes. *B*. As wave height increases, the maximum load on the structure increases. *C*. The more firmly the structure is attached to the bottom, the more sea-floor motion is transmitted to the structure. H = wave height; F = wave force; Pmax = maximum force on structure; a = ground motion.

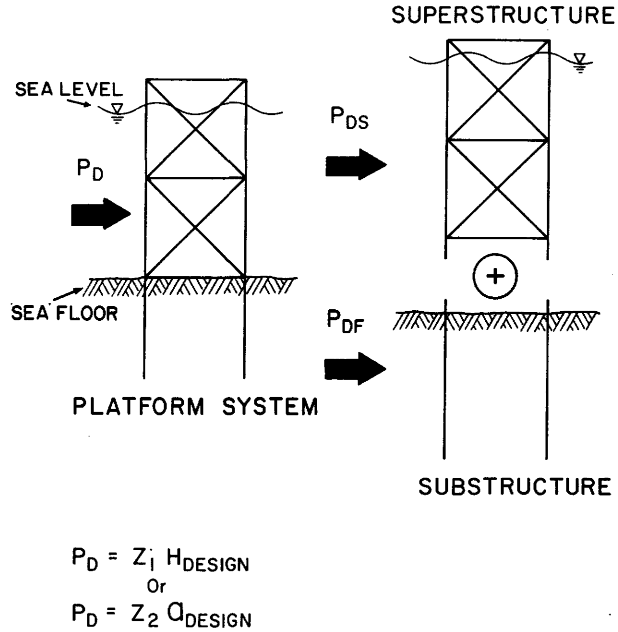


FIGURE 2.—Design loadings for platform superstructure and foundation. P_D = design force; P_{DS} = design force for superstructure; P_{DF} = design force for foundation; Z_1 = wave force transfer function; Z_2 = ground acceleration force transfer function; H = wave height; a = ground motion; ⊕ = two systems coupled; ∇ = free surface.

The engineer is faced with conflicting objectives. Figure 2 outlines the problem. It is one of finding the design force for the superstructure (P_{DS}) that will give it sufficient strength and, at the same time, finding the design force for the foundation (P_{DF}) that will give it sufficient strength without unduly increasing transmissibility of the foundation (Marshall and others, 1977; Marshall, 1978).

As shown in figure 3, a design wave height (H_D) can be chosen to develop the lateral loading (P_{DS}) used to size structural elements in the superstructure. The load-deformation (P_F - Δ_{ML}) or stiffness behavior of the foundation will be dependent on the foundation design load. As important, the ultimate strength of the foundation (P_{UF}) will be dependent on the foundation design load. As shown at the bottom of figure 3, this ultimate strength has important implications in limiting the amount of load that can be induced in the structure. Similarly, note the potentially important influence of factors-of-safety used in the design process.

The foundation design force concept is illustrated in figure 4 in terms of reliability (probability of satisfactory performance) of the structural system. This illustration assumes that the load used to design the superstructure exceeds that of the foundation ($P_{DS} \geqq P_{DF}$). For wave forces (imposed directly on structure), as the load used to design the foundation is increased, reliability is increased. This is due to the decrease in that portion of the probability of failure of the system contributed by potential for failure in the foundation (Marshall and Bea, 1976).

For quake forces (induced in structure by motions transmitted through foundation), as the load used to design the foundation is increased, the transmissibility and forces are increased, and reliability of the system decreased. The problem is to find the reliability and design loadings that will optimize the situation and give acceptable safety in the structure system.

As shown in figure 5, structural parameters and configurations can be used to assist in controlling response of the system to a two-component loading environment. As shown at the top of the figure, changing from batter piles (stiff) to vertical piles (less stiff) can assist in increasing reliability of the system. Similarly, as shown at the bottom of the figure, changing from a low redundancy structure to a high redundancy structure can have a similar influence. This is accomplished by providing alternative load-redistribution paths that can maintain structural stability in case of failure of one or more structural components.

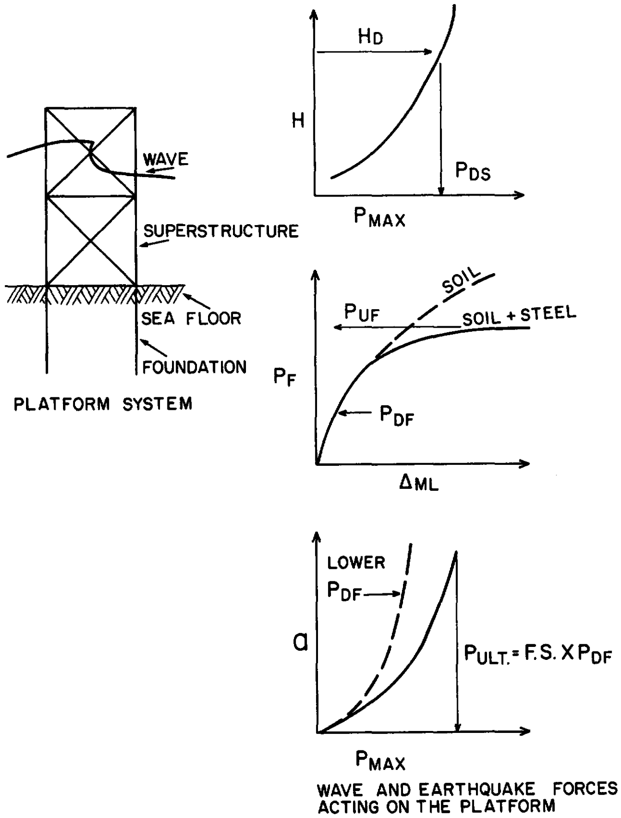


FIGURE 3.—Foundation behavior under design lateral loadings. H = wave height; H_D = design wave height; P_{MAX} = maximum force or load on structure; P_{DS} = design force for superstructure, that is, lateral loading; P_F = foundation force; P_{UF} = ultimate strength of foundation; P_{DF} = design force for foundation; Δ_{ML} = displacement of ground at mud line; a = ground motion; P_{ULT} = ultimate force; F.S. = factor of safety.

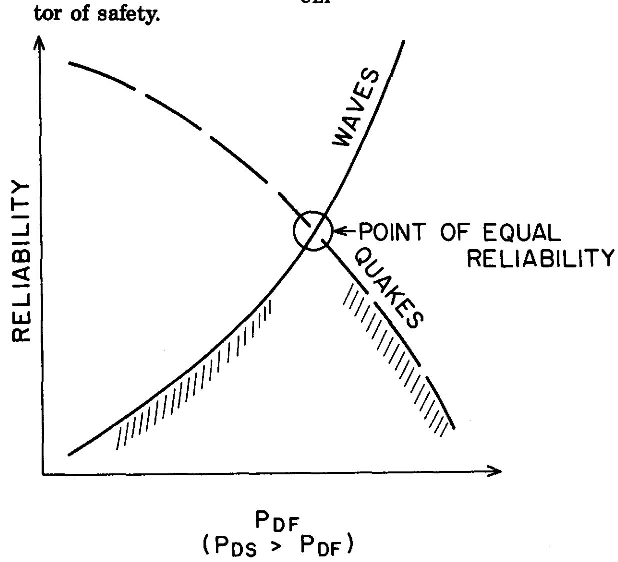


FIGURE 4.—Foundation reliability influenced by wave and earthquake environments. P_{DF} = design force for foundation; P_{DS} = design force for superstructure.

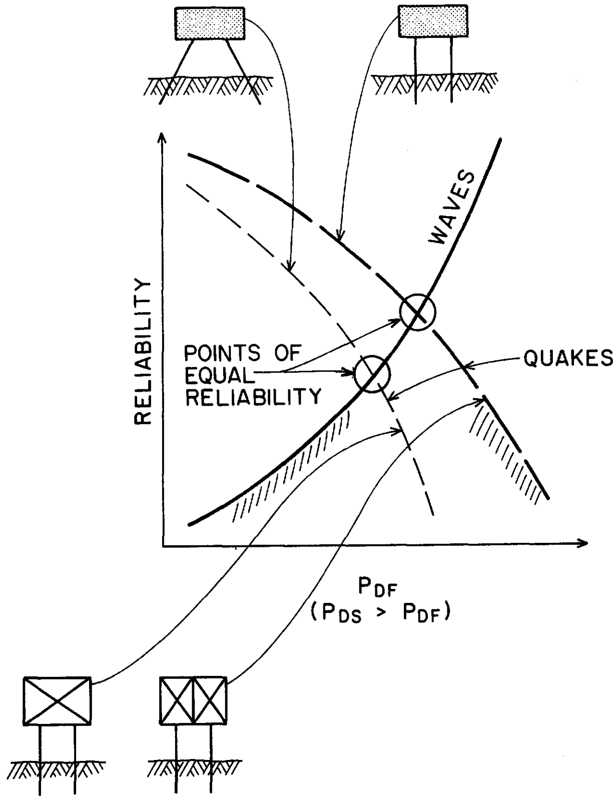


FIGURE 5.—Structural design strategies to improve reliability. P_{DF} = design force for foundation; P_{DS} = design force for superstructure.

Figure 6 illustrates how imperceptive design code development could have a negative influence on safety of a platform in a two-component loading environment such as discussed here. Imposing a design wave having a height of 100 ft (30.5 m) in a situation where strong quakes are potentially present could actually result in lowering reliability, in comparison to imposition of a 40-ft (120-m) sign wave (assuming that underdesign of the superstructure does not result).

This example illustrates that increased strength is not always a correct solution. Flexibility and deformation capacity built into the foundation can provide much more viable solutions (Bea and others, 1979; Garrison and Bea, 1977; Marshall, 1978).

PIPELINE DESIGN

A crude oil pipeline is to be installed in the Mississippi River Delta (Bea and Audibert, 1980). As shown in figure 7, there are two alternate tie-in points. One of the points is 18 mi (29 km) to the

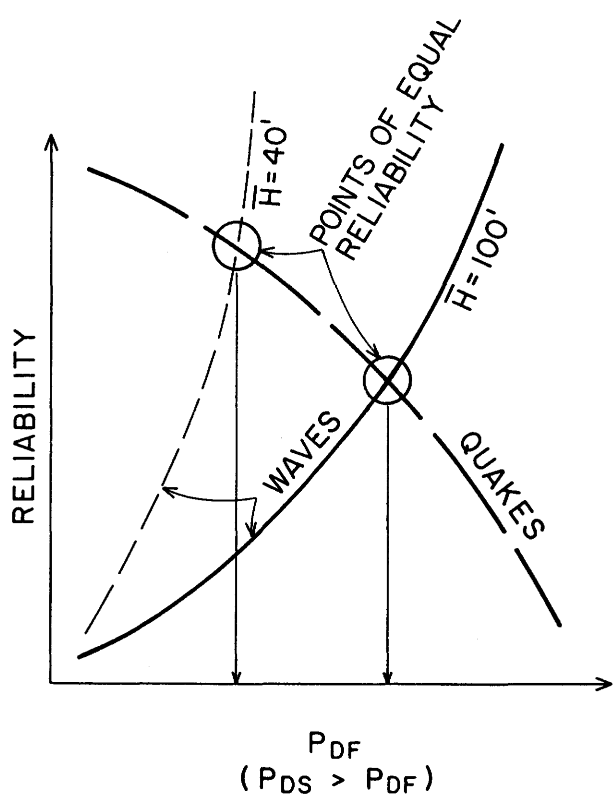


FIGURE 6.—Foundation reliability influenced by wave loading design criteria (1 ft = 0.305 m). H = design height; P_{DF} = design force for foundation; P_{DS} = design force for superstructure.

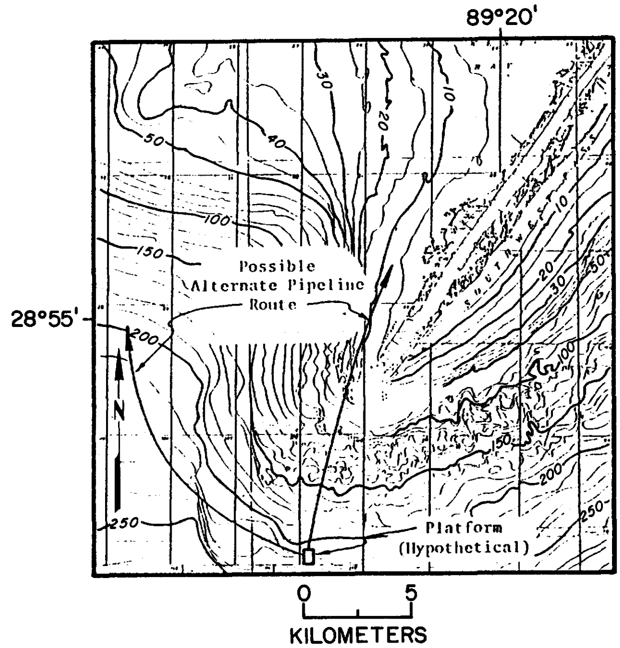


FIGURE 7.—Alternate pipeline routes near Southwest Passage in Mississippi Delta. Orthogonal lines are lease-block boundaries. Contour interval in feet.

west of the offshore platform location, and the other is 15 mi (24 km) to the north. The operating lifetime of the pipeline is 20 to 40 years.

It is desired to find the optimum route for the pipeline, determine the pipeline material and wall thickness, and the weighting of the pipeline. The objective of the design is to configure and locate the pipeline, its terminals, and its supports so it will reliably transport products during its lifetime at the lowest combination of initial, operational, and future repair costs.

A flow diagram for the design process is given in figure 8. There are four classes of design constraints. Operational constraints refer to the volume of crude oil to be transported, its pressure and temperature, and tie-in or terminal points that can accept the volumes to be produced. Natural environment constraints include the soils along the pipeline route, wave and current conditions, soil movement hazards, corrosion, and delta development projections. Construction constraints are defined by pipeline lay barges that are available, steels and welding to be used, and equipment that can be mobilized for repairs, should such be necessary. Design constraints have been defined previously.

Steps 5, 6, and 7 (fig. 8) in the design process focus on preliminary selection of the route and configuration of the pipeline on the basis of results of the field reconnaissance and characterization of design hazards. Principal tools in routing and hazard identification include geology, sidescan sonar, shallow geophysics, soil boring and in-place testing. Geologists must understand the sea floor conditions. Geotechnologists must understand the sea floor soils. Both must understand the present and future environmental constraints (Audibert and others, 1978; Bea and Audibert, 1980).

Figure 9 shows a geologically based picture of the rates of progradation of the pass of the Mississippi Delta influencing depositional and deformational conditions in the vicinity of the two proposed pipeline routes (Bea and Audibert, 1980; Coleman and others, 1980). Based on this history and what can be determined concerning man's interference in the future (Corps of Engineers diverting and directing flow of the river), a projected range in progradation is shown for the next 25 to 30 years. Several thousand feet of

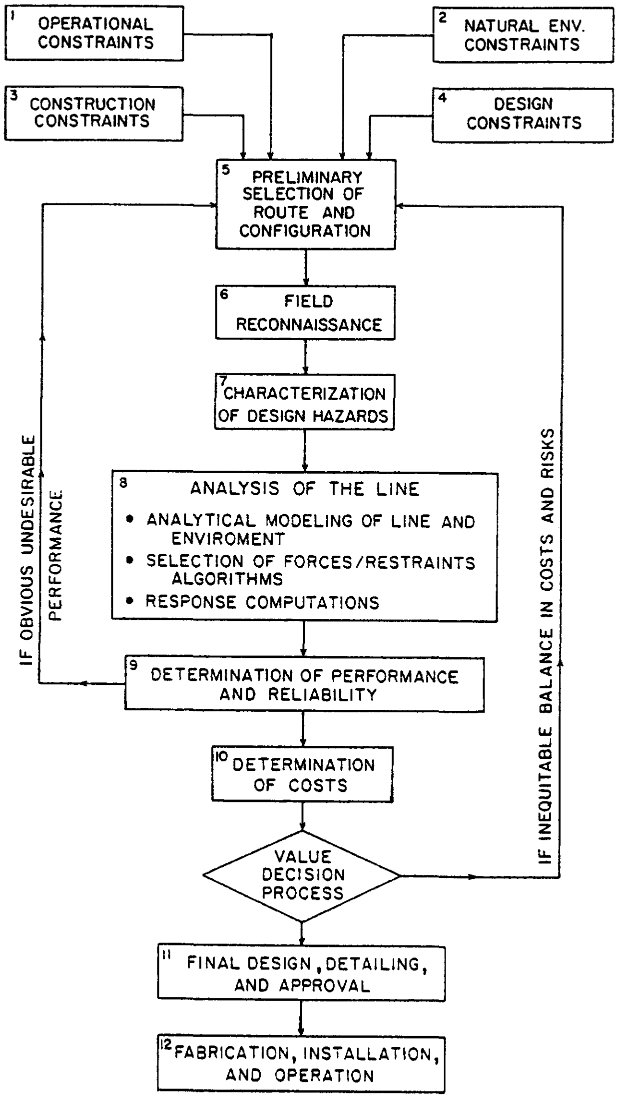


FIGURE 8.—Pipeline design flow diagram.

progradation seaward and changes in bottom elevation on the order of 20 to 40 ft (6 to 12 m) are indicated.

Bottom hazard conditions along the two alternative pipeline routes will be changing dramatically with time. Based on studies of the frequency of destructive motions that have occurred in the past in the principal depositional and deformational zones of the Mississippi Delta (Bea and others, 1975; Coleman and Garrison, 1977; Coleman and others, 1980), a projection of the estimated frequency of destructive motions along the two pipeline routes is summarized in table 1. While the northern corridor presently is subjected to more frequent motions (probability of 0.45 per year of experiencing destructive movements), in the future, the frequency will be decreased. This is due to the projected decreased deformational rates in the northern corridor associated with seaward progradation of the delta.

Production rates through the pipeline will be greatest during the 10- to 20-year portion of its life. There is a substantial initial cost savings in laying the pipeline in shallow water along the shorter northern route. The tie-in point at the end of the western route may be overloaded with crude to be transported in the 10th to 15th year of its life, thus favoring transport in the northern tie-in point that has much greater capacity. In addition, given a failure or rupture of the pipeline, ease and economy of repair are much greater in the shallow water of the northern route as compared with the deep water of the western route.

Consideration of the projected frequencies of destructive soil motions along the two routes, coupled with the operational, construction, and design constraints cited, indicates the northern route to be preferable. The problem now shifts to analytical, performance, and reliability aspects of the design procedure (fig. 8).

Figure 10 shows bathymetric details and identification of present bottom movement features along the northern corridor. The first problem is configuring the pipeline to perform acceptably in the present environment. Given that this can be done successfully, the next problem is configuring the pipeline to perform acceptably in the future environment, one that may have its own unique characteristics and impacts on design strategy.

Figure 11 shows the current mudslide gullies to be crossed by the route of the pipeline. Designing the pipeline to resist successfully the lateral and axial forces developed in crossing these rivers of mud is a first step. Characteristics of the design model used in such analyses are illustrated in figure 12 (Audibert and Wyman, 1977). The real soils and pipeline are idealized as discrete springs and beam column elements that are capable of mimicking reactions of the soils (or in some cases, more like viscous fluids) and the highly inelastic reactions of the pipeline steels (because large deformations are involved). While such an analytical model is capable of producing good answers, it is only capable of such if input parameters to describe soil loadings, restraints, and behavior of the pipeline in the soil are accurate. It is here that results of

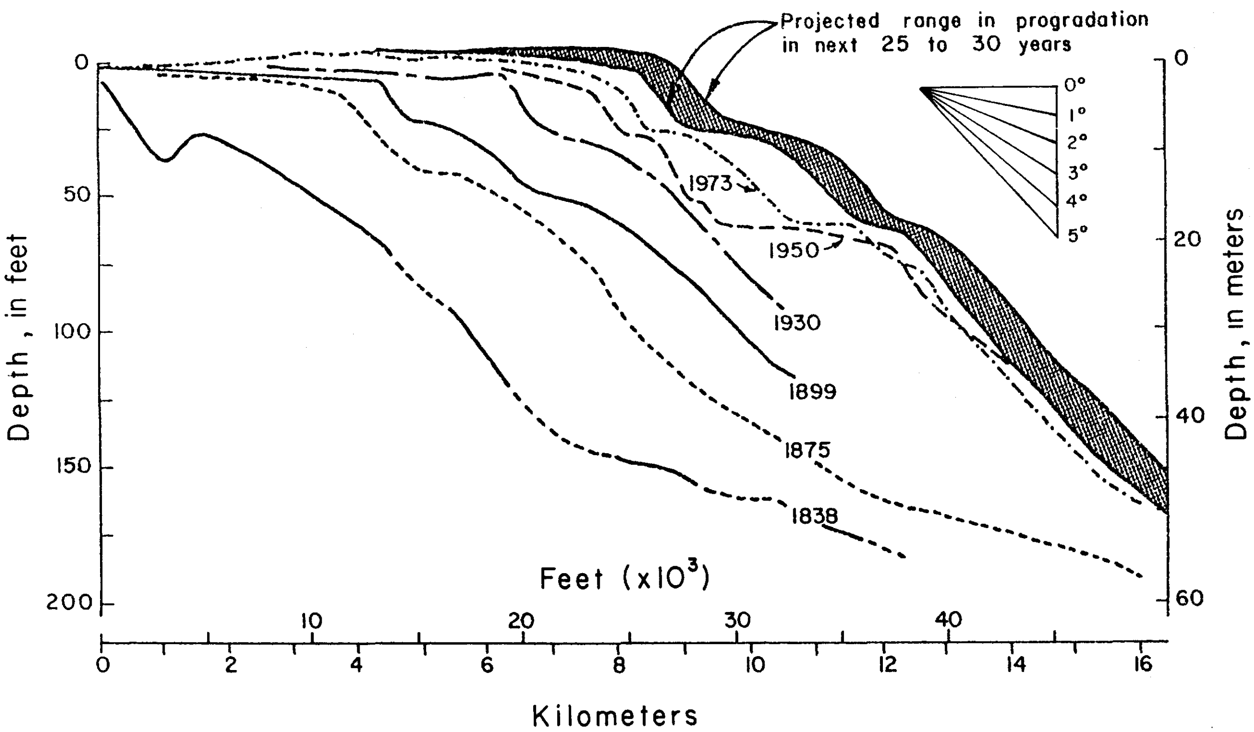


FIGURE 9.—Historic and projected progradation of Southwest Pass of Mississippi Delta.

field pipeline loading tests and experience with pipelines in mudslide areas become an essential ingredient in developing realistic results (Bea and Audibert, 1980; 1980a).

Results of applying such a calibrated analytical model to mudslide conditions along the northern route are summarized in table 2. The analytical model has been supplied with soils characterization data gathered in earlier parts of the pipeline design process (see fig. 8). These soil characteristics or indices were developed in the same manner as those used in calibrating the analytical model with field load tests and in hindcasting past failures of pipelines in mudslides. Due to the empirical nature of the process, mixing different types of soil tests or soil characterization processes without changing the overall analysis can result in inaccurate calibrations.

Table 2 shows for different widths of mudslides, and for the pipeline crossing the mudslides axially or laterally at different depths of cover, the upper and lower bound maximum stresses and sags. Due to the inherent uncertainties in the overall process of determining soil properties, loadings and restraints offered by the soils, and soil-pipeline interactions, wide ranges in answers result. The factors-of-safety used in sizing the line reflect these uncertainties.

TABLE 1.—*Comparison of estimated present and future frequency of destructive motions along the proposed two pipeline corridors*

Corridor	Estimated frequency (in percent) of destructive motions along entire line in "N" years hence				
	Present	10 yrs	20 yrs	30 yrs	40 yrs
Northern	45	44	39	23	19
Western	32	43	73	91	96

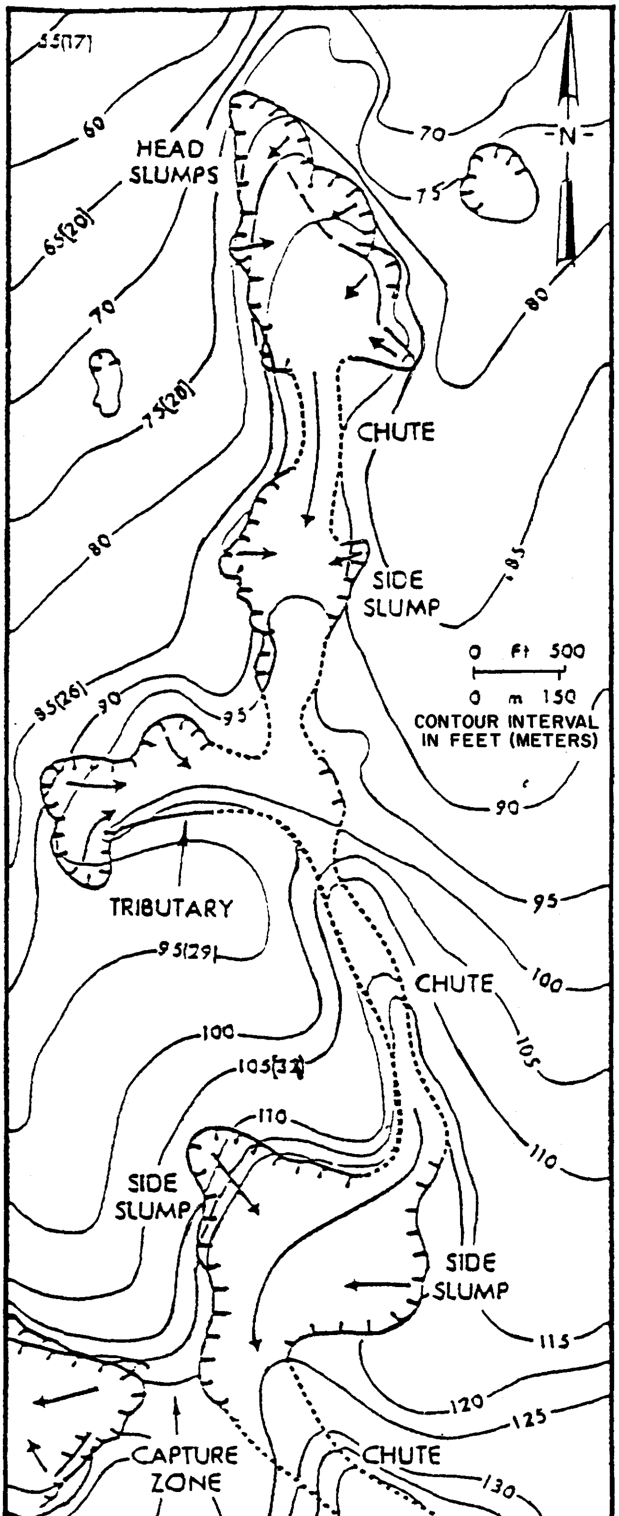


FIGURE 10.—Bathymetry and slide features along northern pipeline corridor.

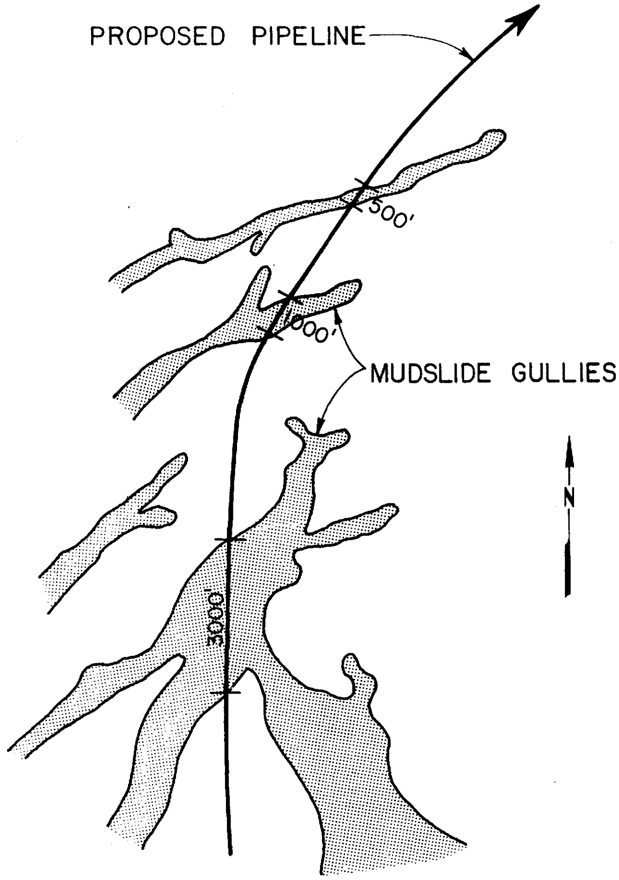
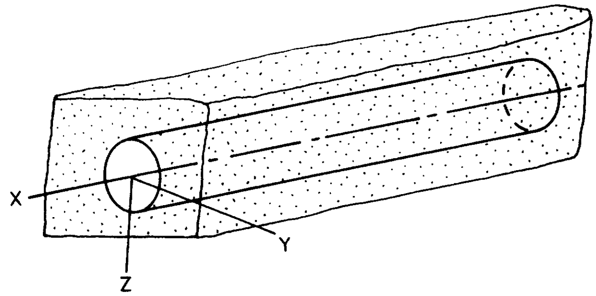


FIGURE 11.—Geometry of present mudslide gullies relative to proposed pipeline route (1 ft = 0.305 m).

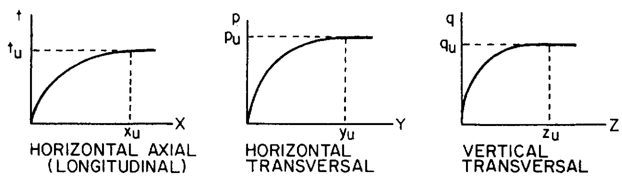
Note the importance of the depth of cover over the pipeline. This is a controllable design parameter, at least until one encounters governmental pipeline burial requirements. Keeping the pipeline in weaker surface sediments and at shallow depths where soil forces are reduced by surface effects is obviously attractive. For some of the conditions, reasonable yield strength steels and factors-of-safety may not be able to accommodate expected slide conditions. Yet, the pipeline must be constructed, and the best available route has been chosen.

The design strategy shifts from attempting to withstand the forces with strength and ductility to planning for failures. In this case, strategically placed breakaway couplings that are able to signal impending breaks and shut off the flow of crude at the intentional break points are placed in the line. These designed weak points permit warning and control and facilitate repair. Foam-filled buoys



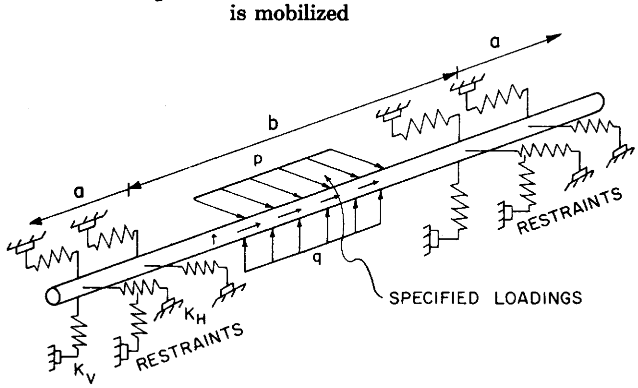
A. Pipe-soil interaction

- X = horizontal longitudinal axis
- Y = horizontal transversal axis
- Z = vertical transversal axis



B. Soil resistances

- t = axial resistance on pipeline
- t_u = ultimate axial resistance on pipeline
- x = displacement
- x_u = displacement at which ultimate resistance is mobilized
- p = horizontal or lateral resistance on pipeline
- p_u = ultimate horizontal resistance on pipeline
- y = displacement
- y_u = displacement at which ultimate resistance is mobilized
- q = vertical resistance on pipeline
- q_u = ultimate vertical resistance on pipeline
- z = displacement
- z_u = displacement at which ultimate resistance is mobilized



C. Pipeline with soil loadings and resistances

- a = restraints on pipe
- b = loading on pipe
- K_V = vertical stiffness
- K_H = horizontal stiffness
- p = unit values horizontal soil-bearing load
- q = unit values vertical soil-bearing load

FIGURE 12.—Analytical model for pipeline response. From Audibert and Wyman, 1977.

attached to the break points with cables are released to float to the sea surface and thus identify where the two ends of the pipeline are located. This is an illustration of damage mitigation strategy being employed where additional investments in steel to provide strength are less attractive and likely counterproductive. (Due to increased weight, pipeline can be forced to sink deeper into soils.)

SOIL STRENGTH

One of the Holy Grails of geomechanics, both onshore and offshore, has become the quest for the "the shear strength" of a soil. Crusades have been and are being conducted in the laboratory and in the field with in-place testing equipment to locate this ever-elusive quantity (Bea and others, 1975; Doyle and others, 1971; Ehlers and others, 1980; Emrich, 1970; Esrig and others, 1975; Gardner, 1977; Kraft and others, 1976; Ladd and Foott, 1974; Noorany and Bea, 1979; Sangrey, 1977).

What will we do with this quantity when we finally find it? Some of us are not sure. Some hope that it will provide the key to unlock the secrets of mass-wasting processes on the continental shelves, slopes, and rises. Some hope that it will provide the key to unlock the secrets of foundation behavior or the forces and restraints exerted by soils.

We hope it will be remembered that we need representative indices of soil strength and of other properties that control important elements of soil-structure behavior. Pore-water pressures, gas content, geostatic stresses, water content, and stress history are examples of these other important elements. Note two key words, "representative" and "indices."

Steel and concrete structures have been successfully designed and analyzed for many years by using mill tension test data on small specimens and compression tests on cylinders or cubes. These data do not give the true or in-place strength of steel or concrete in the structural forms that they are fabricated into. They are representative indices so that when they are coupled with competent analytical models, prototype test results, and performance history—the engineering process works.

Figure 13 shows data from a recent attempt to correlate in-place vane shear strengths with shear strengths determined on samples retrieved from beneath the sea floor (Ehlers and others, 1980; Emrich, 1970). When in-place vane shear results are

TABLE 2.—*Northern corridor pipeline—Summary of results*

[N/A, not applicable]

Case	Width of mudslide (ft)	Depth of cover (ft)	Upper bound soil loads and restraints: Max. stress (psi)	Upper bound soil loads and restraints: Max. sag (ft)	Lower bound soil loads and restraints: Max. stress (psi)	Lower bound soil loads and restraints: Max. sag (ft)
Crossing across mudslide	1,000	2	70,100	136	44,500	116
	1,000	1/2	62,000	126	37,000	98
	500	2	45,200	56	33,700	51
	500	1/2	43,800	53	29,300	45
Crossing mudslide axially	2,500	fully buried	43,500	N/A	22,000	N/A
	5,000	fully buried	87,000	N/A	43,500	N/A

multiplied by a factor of 0.75 and miniature vane shear results on retrieved samples are multiplied by a factor of 1.1, the data agree well. Much money and time have been invested by the industry in developing the in-place testing equipment used in this work (Doyle and others, 1971; Kraft and others, 1976; Noorany and Bea, 1979). Yet, when it comes time to apply the results, we search for modification factors that will make the results agree with what our judgment says should be "the shear strength." The modification factors are not unique, even though the logic used to derive them may be.

An alternative process for estimating in-place shear strength is illustrated in figure 14 (Bea and others, 1975; Esrig and others, 1975; Kraft and others, 1976). In this approach, samples retrieved from the sea floor are placed in triaxial chambers that recompress the samples to in-place conditions. Total pressures (to recompress gas) and backpressures are used to accomplish such states. Further, samples are compressed more highly than they were in place in an attempt to erase sampling disturbance effects. Effective consolidation pressure used to compress the samples is shown in figure 14 versus measured undrained shear strength of the sample. Data scatter is due to natural soil inhomogeneities and sampling and testing-induced variabilities. The mean trend through the data indicates a strength to effective stress ratio of 0.31.

This strength can be correlated directly with the natural water content and limit properties of the soils tested. This correlation generally is expressed with the liquidity index (I_L). The liquidity index is the natural water content of the sample (W) minus the plastic limit (W_p) divided by the liquid limit (W_L) minus the plastic limit. The plastic limit is the water content below which the soil behaves as a plastic. The liquid limit is the water content above which the soil behaves as a liquid. Water content properties are chosen for the correlation because they are the least prone to disturbance effects caused by sampling. However, the procedures for determining plastic and liquid limits are very inexact. Other alternatives (for example, fall cone) are being explored.

The correlation between the water content properties expressed by the liquidity index and undrained shear strength determined as discussed is illustrated in figure 15. The mean trend line for the data is indicated. Comparisons with other experimental results are shown in McClelland (1967), and in Skempton and Northey (1953).

The next step in the process is to return to the results of the soil boring itself. The retrieved samples are tested to determine their natural water contents and limit characteristics. Then the liquidity indices throughout the depth of the boring are determined. In-place shear strength can then be estimated from the information in figure 15. The results are given in figure 16. In-place shear strengths derived from the liquidity index correlation with total pressure triaxial test strengths are compared with results of in-place vane shear testing (Doyle and others, 1971). Excellent agreement is indicated. Similar results have been developed for more than 40 cases.

One could argue that the alternative method of estimating indices of in-place shear strength by using total pressure triaxial testing correlated through the water content properties of the soils (less susceptible to disturbance) makes as much sense as in-place testing with a wireline vane shear

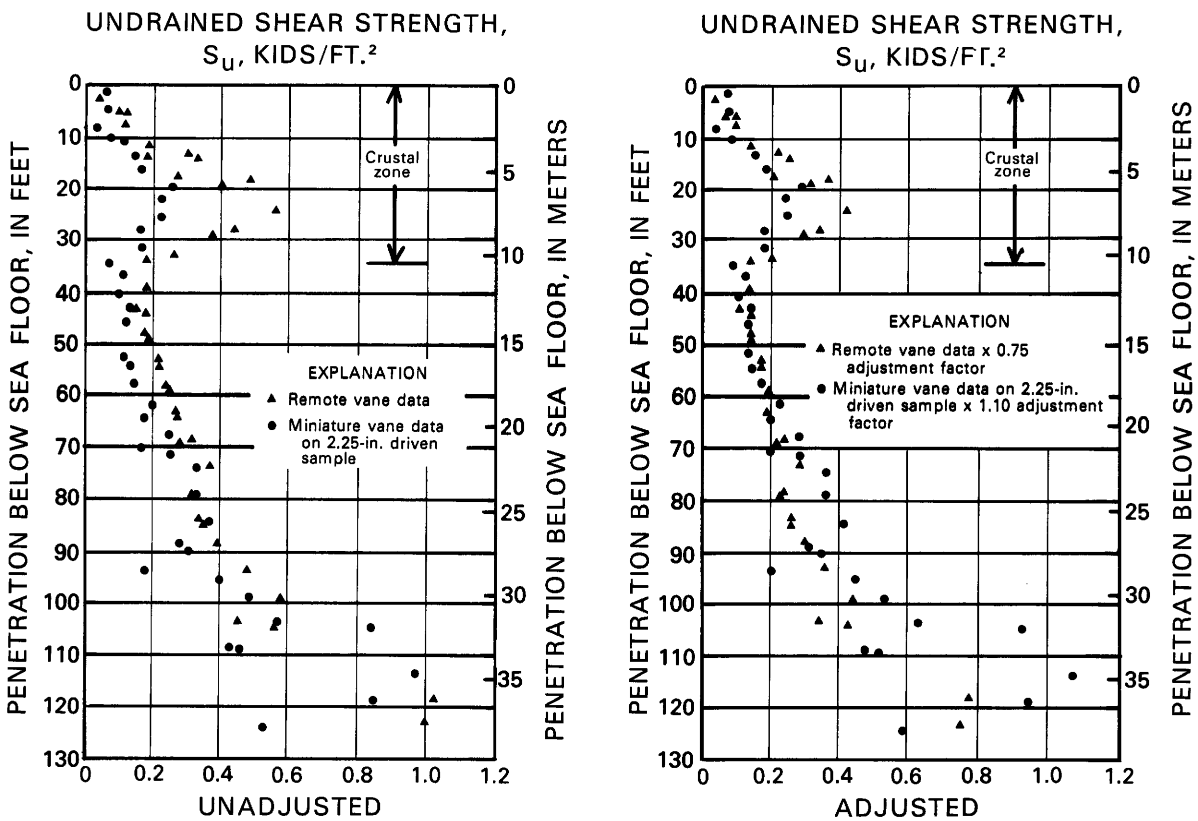


FIGURE 13.—Comparisons of unadjusted and adjusted remote vane and miniature vane shear strength data for Mississippi Delta soils.

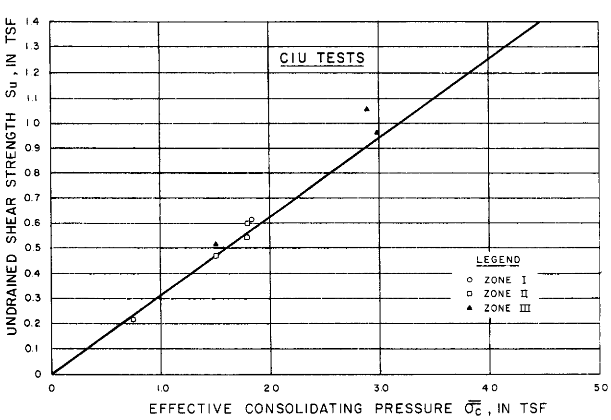


FIGURE 14.—Correlation of undrained shear strength and effective consolidation pressures used in total pressure triaxial tests on Mississippi Delta soils (1 ton = 8.9 kN; 1 ft = 0.305 m). CIU = consolidated isotropic, undrained (type of triaxial test). tsf = tons per square foot.

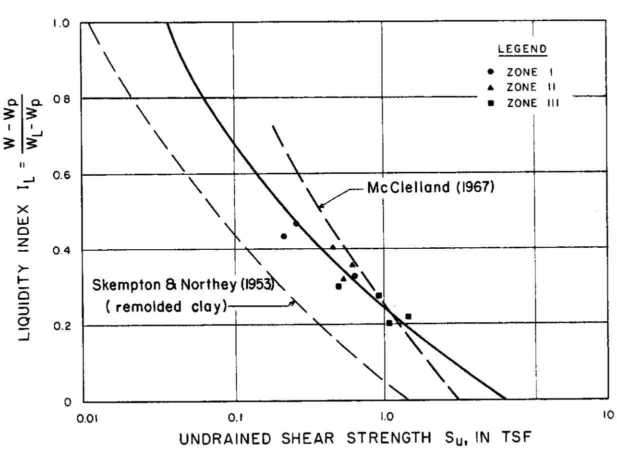


FIGURE 15.—Correlation of undrained shear strength to liquidity index. Zone I, crust; Zone II, failure; Zone III, transition. 1 ton = 8.0 kN; 1 ft = 0.305 m). W = water content; W_p = plastic limit; W_L = liquid limit; tsf = tons per square foot.

device. Neither gives the "shear strength". The laboratory triaxial approach has the advantage of allowing the sample to be subjected to a wide variety of conditions to simulate future environmental and foundation effects on the soils. Thus, the logic of extrapolating from current to future conditions becomes much easier to follow and analyze.

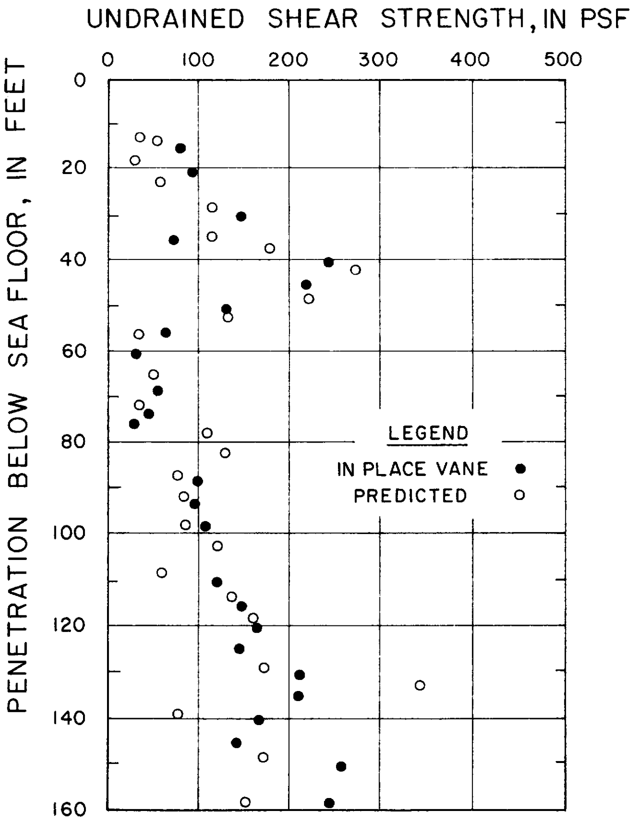


FIGURE 16.—Comparison of in-place vane with predicted undrained shear strengths. Predictions based on strength correlation with liquidity index (fig. 15). 1 ft = 0.305 m; 1 psf = 47.9 Pa. psf = pounds per square foot.

A key part of the laboratory approach is that connected with the previously cited term "representative." If the soils cannot be retrieved in a reasonably undisturbed condition (Sangrey, 1977), and if the laboratory techniques cannot reasonably eradicate and compensate for stress relief and sampling effects (Esrig and others, 1975; Kraft and others, 1976; Ladd and Foott, 1974), then the process can result in misleading indices. Similar misleading results can be developed by in- place testing equipment due to disturbance caused by drilling, instrument placement, and uncompensated heave of the vessel supporting the equipment (Noorany and Bea, 1979).

In any case, the fundamental state of affairs regarding soil strength is that we still have not developed instruments and procedures to define "the shear strength." We are still faced with using "a shear strength" as input to competent analytical models and then calibrating results with geologic observations and engineering observations made during field tests.

CONCLUSIONS

Four categories of engineering needs relating to continental margin mass-wasting processes have been highlighted.

(1) *Sensing instruments*—A recognized danger can be engineered to acceptable proportions. An unrecognized danger is the critical flaw. Unfortunately, tools for sensing characteristics and behavior of soils, for sensing performance of prototype foundation elements (piles, mats, pipelines) in these soils, and for generally observing what has and is happening to the sea floor are extremely limited. Data gathered with present tools frequently are misleading or not representative. Development of a wide range of reliable sensing instruments is needed.

(2) *Surveys and performance data*—Once the engineer understands the whens, wheres, hows, and whys of past and future continental margin mass-wasting processes and characteristics of the soils involved in such processes, then technology is available or can be developed to design successful structures. Instrumentation employed in surveys of the continental margins, long- and short-term observations of the margins and of candidate structure foundations to be placed on and embedded in the margins are needed.

(3) *Integration*—Answers to problems associated with continental margin mass wasting are composed of many parts requiring input from a variety of disciplines and sources. The sum of correct answers from each of the sources may not result in the best answer to the problem. The process and results of test, analyze, extrapolate, build, observe, and revise must be carefully integrated across the disciplines and information sources represented. Similarly, the needs and requirements of those ultimately responsible for the end products (resources spent for resources developed) must be understood as clearly as possible.

(4) *Reality*—Risks are an unavoidable fact of life for offshore structures. Uncertainty can never be reduced to zero in the problems associated with mass wasting of continental margins. Nothing is for sure, nothing is forever, nothing is free, and nothing worthwhile is easy or quick are key concepts of such reality. Engineers, researchers, scientists, and those responsible for industrial and governmental processes must come to grips with this reality. Uncertainty, variability, risk,

and reliability must be recognized in developing plans and strategies for coping with continental margin mass-wasting issues and in such a way that will allow timely, efficient, safe, and ecologically acceptable development of oil and gas resources beneath this margin.

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Communication Between Marine Geologists and Engineering Geologists

Dwight A. Sangrey

The speaker used shearing resistance, the shear strength of a soil, to show how important various levels of geologic information are to the engineering geologist and how to improve this information. The sequence that the speaker developed was as follows: (1) Knowing the age of a deposit will put some bounds on the shearing resistance though the uncertainty is large. (2) Measuring the Atterberg limits (the liquid limit, the plastic limit, and the natural water content) from a piece of core or a grab sample taken from the top meter or two of the sediment will considerably reduce the uncertainty in the value for the shearing resistance. (3) Acquiring deeper samples by dropping a piston or gravity core into the sediment and conducting torvane or labvane measurements, triaxial tests, and so on, on the sediment will narrow the range of uncertainty and will allow greater confidence in the extrapolation to greater depths. (4) Collecting samples to depths of about 150 m, the next major step, will permit more elaborate testing. (5) Finally, calibrating will most narrowly define the information about shearing resistance. Calibration is actually the hindcasting described in the speaker's previous talk (see p. 97). If one knows the geometry of a slide, and what the loading was, then the shearing resistance can be estimated. "...there's no substitute for having [studying] a process that is essentially the same as the process you are concerned with to use as the final and best indication of shearing resistance, or whatever other parameter [you wish to investigate]."

Thus, the discussion illustrates basically that knowing as much about the geology as possible usually, at reasonable expense, significantly improves the information needed to understand such parameters as shearing resistance.

Requirements for Effective Geotechnical Analysis

Robert L. McNeill

The speaker noted that shear strength has been discussed at length during the conference but that much more needs to be known. The information needed is in two categories: (1) "...a description of a soil in its present condition of which shear strength is one parameter..." and (2) more important, "...the parameters that are necessary to predict how a soil will change as a result of what he [the engineer] and his structure do in stressing the soil, and as a result of what nature might do over a period of time...." Related to and needed "for predicting future changes in shear strength, is the effective strength angle."

"We know that under stress systems, soils change in volume; therefore, we need to know something about their coefficient of volume compressibility. We know that waters flow through soils and have a profound effect upon them, so we also need to know the coefficient of permeability. In addition to that, and in order to be able to use those things in a predictive capacity, we have to know the pore pressures and, in fact, we have to know the pore pressures in terms of two components: the first—the one due to shear and the second, the one due to the formation itself and possibly its variations. And then, in addition, if there is any possibility of a clathrate [present] or any other problem that might involve mineral changes, we need to know the temperature of the material."

The speaker then went on to describe various pieces of geotechnical equipment that Sandia Laboratories is developing. The first is a wireline tool for measuring all the parameters listed above; the second, a device called GISP for measuring pore pressures at various depths over a total depth of 30 ft [9.2 m], and third, a marine sediment penetrometer. This last device is designed to be dropped from a vessel or an aircraft. As it penetrates the soil, it feels decelerations and radios the information back. Though the device is experimental, it appears to have worked in a variety of soil types.

Discussion following McNeill's talk:

Robb: Two things: First, will you tell us what you dropped that from? You've got 60 ft (18.1 m) of penetraton in dense sand?

McNeill: Sure. The first one I showed you, in fact, was the drop that you saw in the picture; it was hung over the side from an electromagnetic device and let go. The second one—the one in San Francisco Bay—was dropped from a helicopter. The third one was dropped from a fighter plane, but that was only a matter of convenience; you could do the same thing by renting a small cargo plane. I also have dropped them out of Cessna's.

Silva: Did you get a core at the location of the pore-pressure device [GISP]?

McNeill: On our next try, which will be a few months from now, we will take the drill rig with us to avoid the arm-up charges, and we will get those cores. We've got to have that answer, because right now it's just tantalizing speculation.

Prior: When you said tantalizing speculation, you almost stopped my question. You were pretty secure in the idea that you were right in gas pressures?

McNeill: No, I can give you the propositions. One is that you're going through a material that has a high pore-pressure shear reaction; you can put together the mechanics of a penetration event to indicate that it would be possible you were hydrofacturing, and then finally, there's gas. Those are the only three things I've been able to come up with.

Dunlap: How about rate changes in penetration during insertion?

McNeill: I would not expect the rate change to give that much of a response, but I have to hold it wide open. However, this thing went in at a reasonably uniform rate, that rate being as fast as the winch would unwind. And it was in very shallow water, so if it had changed I think we would have seen the cable react. That's a good point though.

Bea: How close to a platform were you?

McNeill: We were at the SEASWAB I site in the feature; the only reason for that is because there were data there from the previous experiments, and we felt that would be a good place.

Robb: To relate that thing to our research project—how much do they cost?

McNeill: First of all, the hardware cost is trivial. The cost is in the electronics. It depends on what you want to measure; if you want to measure the decelerations and go through the calculations like I'm talking about, then the nonrecoverable part of the electronics is a few thousand dollars. I would say $1,000 to $3,000. But if you finally satisfy yourself as to the validity of the theoretical approach involved, then you can use that theory to calculate calibration curve for a given penetrator, in terms of shear strength and velocity; then all you have to do is measure the depth, and you know the strength of the material. Now, I can do that for you for $50; but you have to believe the theory first.

Pressurized Core Barrel

Wayne Dunlap

The speaker described in detail the elements and operations of a pressure core barrel that was conceived by Lou Garrison, funded by the U.S. Geological Survey, and developed at Texas A&M University.

The barrel consists of a 19-ft [6-m] long, 2.5-in [6.35-cm] diameter stainless steel pipe. An umbilical cord connects the surface control system to the barrel, and a hydraulic pump supplies the fluid that operates the barrel. In operation, the barrel is used from a standard geotechnical-type drillship down a cased hole [23.98 in (7.56 cm)]. It is held in place at the bottom of the hole with expandable bladders. Upon command, a thin-walled [1.5-in (3.81-cm)] O.D. Shelby tube sampler moves out of the pressure-core barrel into the sediment below the bottom of the casing. After measuring the pressure at the bottom of the hole, the core barrel is retracted, and a ball valve is closed to trap the gas in the sample at downhole pressures. Once on the surface, the sample is transferred to a transportation chamber and thence to a van, where it is stored at 50°–55°F to prevent gas formation.

The sample is then transported to a hyperbaric chamber at Texas A&M. The transport chamber, a geotechnical engineer and an oceanographer are placed in the hyperbaric chamber, and pressure is increased to simulate the desired depth. The greatest equivalent depth attained at the time of the conference was 225 ft [69 m] of water.

In the chamber, a core is sampled and tested for such parameters as gas content, density, water content, degree of saturation, and so on.

The speaker described a few of the results obtained. One core taken off Eldorado, Southwest Pass (Mississippi Delta) showed abrupt large vertical variations in gas content. In two other cores recovered at localities 45 ft [13.7 m] apart at the same depth [15–17 ft (4.5–5.2 m)] below the mudline, methane content was 23 mL/L in one and only 0.2 mL/L in the other. These few samples indicate that vertical and lateral variability in shallow sediments is large. Vane shear tests showed great strength reduction upon decompression in some samples but less in others. Consolidation tests did not show a great difference between in place pressures and after decompression.

Discussion following Dunlap's talk:

McNeill: The gas you measured was methane?

Dunlap: We have tried to look at other gases but, when we took the sample out in the hyperbaric chamber under air pressure, we got nitrogen. So we now have a glove box that is washed out with helium, and we will take the sample out in a helium environment and then pass it outside. It will never have seen air, so we will start looking for nitrogen and for carbon dioxide in addition to methane. Right now, we're only testing in the gas chromatograph for methane.

McNeill: Pyrite indicates sulphide and that creates some problems with the production of methane.

Dunlap: No. It is a product of methane [generation].

Giant Piston-Core Development

Armand Silva

The speaker described in detail the state of development of the giant piston corer (GPC) that was conceived by Charlie Hollister and funded by USGS in 1969. A Long Coring Facility was set up to develop a corer that can reliably recover high-quality continuous sediment samples 50 m long in water depths up to 6,000 m from a number of oceanographic ships.

The assembly of a working group interested in developing the capabilities and applications of the GPC resulted from a workshop held in 1977 at Woods Hole mainly to design the 50-m capability. Initial funding came from the National Science Foundation and then Sandia Laboratories as part of the seabed disposal program.

The followup planning indicated that the biggest effort would be in systems analysis to develop a feasibility study and conceptual designs. The primary objective was to core stiff Pacific deep-sea clays that, on the basis of extrapolation from a 25-m core, would have a shear strength of 400 grams/cm^2 at 50 m. Extensive modeling of core-barrel thickness, taper configuration, free-fall height, and so on, was carried out; structural stability tests were run at Sandia.

Initial results suggested that a core of sufficient weight with a 2-in [5.1-cm] step-tapered configuration would not break as it penetrates. Also, placing most of the weight (30,000 lb) in the core barrel improved the structural stability and penetration. Pull-out forces are 80–90,000 lb for this configuration; cables, therefore, are a significant problem.

Because almost neutral buoyancy is necessary, a 2 3/4-in [7-cm] diameter polyester stable-grade cable was judged to be necessary. This gave a safety factor of 2. A traction winch obviously is necessary to handle cable of this diameter. The core weight will have to be variable to handle different types of sediment, and mechanical tripping must be eliminated. Piston motion has caused sediment disturbance, and various techniques are being tried to control it.

Instrumentation capability is being developed to monitor acceleration history, piston and core-weight position, and core rotation (during penetration) and tilt. A microprocessor will be mounted in the core weight to collect the data.

Among vessels identified to handle the corer are *R/V Melville*, *R/V Knorr*, and *R/V Hudson*.

Discussion following Silva's talk:

McNeill: Why do you have to let it [the corer] sit for a year on the bottom?

Silva: That has to do with this sub-seabed disposal program. We're planning on putting a heat source in the sediment, monitoring the temperature field around it, and measuring the geotechnical properties before and after the end of each year.

Dunlap: Is the giant coring device being designed as a geotechnical coring device?

Silva: I think that that is a little more than we can do with this kind of coring device, but we will go as far as we can. The idea is to smooth down the leading edge, the last 10 ft [3 m] or so, until it is as thin walled as we can make it, without breaking off big chips.

Robb: What kind of penetration do you think you might be able to achieve in slope sediments?

Silva: It's a function of shear strength, of course. We did apply this corer to the shear-strength profile on the Blake-Bahama Outer Ridge, GPC 7, which is a much lower shear strength than what we designed for, and, as I recall, the theoretical penetration there was something like 68 m, when using the same core weight and configuration. So we designed for fairly stiff sediment. With lighter versions, I'm sure we can get 50 m in softer sediments.

McNeill: You're not depending on hydrostatic head when you're pulling on the piston?

Silva: No. As a matter of fact, that works against you because you're getting a suction effect there. You have to provide an upward force to counteract that force.

McNeill: You could actually use the pressure in the withdrawal. .

Silva: We've tried not to get any more flowing. We've done that.

McNeill: I was thinking of a closed vessel on top that, for withdrawal, you could vent at the bottom to give some extra pull-out against the water pressure.

Silva: We looked at ways of reducing the side friction by using jets down the sides, and so forth; it gets very complicated.

Concluding Remarks

H. William Menard

It might be worthwhile to think a moment about whether we have realized the purpose of this meeting. To do that, we have to think what was the purpose of the meeting. I take it to be that our purpose was to meet collectively and try to exchange detailed information on the subjects of our own expertise, with the hope that we would inform each other in some way so we can broaden our viewpoints in that expertise. We didn't, for example, meet to try to solve a specific problem. We might want to do that in the future.

This is the second meeting we've had now. The first one was primarily concerned with trying to understand something about the potential resources to be developed on the continental margin; this time, we're trying to learn more about the factors that would be involved in the development, if there's something out there to develop. We might want to focus on some more specific subject in the future. If we did want to do that, rather than what we're trying to do here, there are a couple of ways you can go at it.

The main way you solve problems is to bring experts together—which we've done. Curiously enough, there are two groups of experts you can assemble to solve a specific problem—one, like those we had here—a group of experts who can communicate because they work more or less on the same subject, and it's related to the problem they're trying to solve. Two, you can also solve the problem by bringing together a group of experts who know each other intimately, who are exceedingly smart (but you have to pick the right field of experts), and who don't know anything at all about the subject. We could have had a group of people sitting here who didn't know anything about the subject but knew how to talk to each other on their own subject. They would have been informed about something new to help them solve the problem. That really works pretty well. What you have to have are people—in anything—who can communicate on something. Now, Wayne Dunlap said more or less that he didn't understand why a civil engineer was involved in hyperbaric experiments; I can say why. I went to a meeting in the early 1950's, when I was a member of the Navy's Panel on Underwater Swimmers, and sat through the meeting for 2 days, with people talking about physiology and biochemistry and a batch of stuff about which I didn't understand a word. Somewhere in the afternoon of the second day, Christian Lambertson of Penn State got up to talk about diving physiology in terms of flow, enclosed channels, air in the lungs, flow of blood, valves. As soon as he started talking about Reynolds Numbers, for the first time in 2 days I understood something. That's too low a rate—you've got to have people who can talk in some way to each other. And I think we've had that here. Now and then, I noticed a little stirring as the members of one group got a little too technical and talked to each other—but, mainly, we've have been talking to other groups. I think we can go from here and take advantage of what we've learned and try to broaden our approach in what we're trying to solve. At least you know people you didn't know before who are working on specific problems of interest to you and, hopefully, that you didn't even know were of interest to you before.

I think that I have certainly learned a great deal in this conference. I hope all of you have. Some things sound awfully familiar. We had a little discussion about doing experiments to generate instabilities on the sea floor. The first attempt to set up deliberately a turbidity current on the sea floor was done by Shepard in 1950, when geologists didn't believe in turbidity currents, at the same time when civil engineers were designing dams in the United States and South Africa to bypass the things. There were volumes of proceedings and transactions by the American Society of Civil Engineers on how to design to get rid of these terrible turbidity currents—and the geological literature was full of people saying "there ain't no such thing." Well, that's one way we can learn from each other.

I've been struck now and then in this meeting by statements to the effect that we might have done something else, but it would have cost money. We certainly don't want to consider doing things that are more expensive than the optimum solution, though when we're doing science we have to think in terms of engineering. An engineer can do something for a dollar that any idiot could do for ten. We have to think in terms of doing the right science that's related to the problem we're trying to solve. We should not limit our imaginations to what we ought to do on the ground that it might be too expensive. The issues are just too important. There isn't anything that we can conceive of doing that would cost too much, compared to the expense of the enterprise we've talked about. As an example, all of us here are trying to learn something about slumps, and, meanwhile, we seem to be carrying on a gigantic experiment in the Gulf of Mexico learning about the rates of slumping. We're doing that by planting platforms that cost $100 million, here and there across the shelf, and finding out how often they fail. There must have been some less expensive, more cost-efficient way to carry out this experiment. It's going to be far worse out on the Continental Slope. It costs too much to be ignorant for anybody not at least to give a good solid try at getting the resources to carry out the appropriate experiment. That doesn't mean you go around drilling holes to 10,000 ft (3,050 m) everywhere to find out physical properties of materials, if we learn that you don't necessarily get much more information from drilling holes.

Oddly enough, we are carrying out two activities. We're trying to develop resources, and we are also trying to develop resources safely. You didn't learn much more by drilling a deep hole about whether you could develop the resource safely, but you did learn a lot more (if the right people looked at the data) about whether there's any resource worth further drilling. So, if you all talk to each other, some things that might not appear to be feasible to carry out one experiment may be highly feasible if the experiment yields a variety of information you can use to solve a lot of problems.

We may not get the money to do these things, but I think we have to try. I hope we don't have to be so patient as Armand Silva was describing, in which a group of people got enthusiastic and, 4 years later the funds became available to begin to do something about it. We just don't have the time for that. That's one of the reasons for holding a meeting of this sort—so you can short circuit the time between when you get a good idea and when you can get the information to solve the problem. There isn't anything we can conceive of spending that's even equal to the interest charges that the U.S. Government loses because it hasn't had a chance to lease the area offshore, let alone the interest charges or the loss to the dollar of its overall value just because of the hemorrhage we've got to the Middle East for oil money.

I don't want to lead you to believe that we'll be able to get money for anything you can think of, but we sure will try. But to do that, you've got to compete with other people who have uses for money too. You have to have solidly based programs. I'm not going to say anything about organizational boxes, or even their absence . . . but I felt a sort of atmosphere less bold than I would hope. I would hope that you would aspire to programs on a scale that are really commensurate with the problems you're trying to solve. Consider sidescan sonar maps; Jim Coleman showed us absolutely fantastic stuff in the Gulf of Mexico. You can say that's so far ahead of anything you can conceive of elsewhere that that must be the end, but it isn't anywhere near the end. It's all very well to say that people 10 years ago were just arm wavers, but somewhere, just entering college at this point, are the people who will be getting their Ph.D.'s for actually working on the bottom. They will say, "People used to tow instruments above the bottom and try to understand what's going down here. . .they were waving their arms". We've got a long, long way to go before we've exhausted our resources in getting the kinds of information that will turn out to be what we need.

Well, I don't want to keep on talking or rambling. I think I've pretty well covered everything I wanted to, except for my principal responsibility for getting up, and that is, as one who is neither a speaker nor one of our hosts here in Woods Hole, I'm in a happy position to thank all of you who came here to give talks and to illuminate subjects we're all interested in, and also to thank our hosts for the effort they put on that enabled us to do that.

Thank you all.

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