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The Development of Plate Tectonics: a Personal Perspective. A Reply to a Series of Questions from Professor H. Frankel

Author

Sclater, John G

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The Development of Plate Tectonics:

A Personal Perspective

A reply to a series of questions from Professor H. Frankel

by

John G. Sclater

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The Development of Plate Tectonics: A Personal Perspective

John G. Sclater

Geological Research Division-0215, Scripps Institution of Oceanography,
the University of California, San Diego, La Jolla California 92093-0215

A reply to a series of questions

from Professor H. Frankel

Department of Philosophy, the University of Missouri

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OVERVIEW

I originally prepared these comments between January 8 and January 25, 1991, in Austin, Texas. I revised them between July 20 and July 31, 1992 in La Jolla, California. These comments are replies to written questions addressed to me by Professor Hank Frankel of the Department of Philosophy at the University of Missouri.

I begin with a copy of Dr. Frankel's letter to me, my covering letter and then follow with my responses to each of his questions.

I have also added my vitae and list of publications and a copy of the manuscript that Jean Francheteau and I submitted as a Citation Classic commentary for *Current Contents*, which is published by The Institute for Scientific Information.

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Questions for John Sclater
Institute for Geophysics
University of Texas, POB 7456
Austin, TX 78712

Feb. 6, 1990

From Hank Frankel
Department of Philosophy
220 CH Hall
University of Missouri
Kansas City, MO 64110
816-276-2818

Some of these questions might not be in the correct order. I don't know enough about you to begin putting them in logical order. The order reflects my worry about not including questions that came out of our brief conversation.

1. Would you please send me the following: (a) a copy of your CV and a list of your publications. It would be extremely helpful if you were to star those publications which you think are more significant, and briefly explain why. This will help me when I turn to my next project, the proliferation of plate tectonics and how it changed the face of the earth sciences. (b) The address of J. E. Everett. I have interviewed A. G. Smith.

2. Could you briefly describe your undergraduate training? Where and what did you study as an undergraduate? From whom did you take courses? Were there any teachers who particularly influenced you? When and why did you decide to go into geophysics? When and why did you decide to enter the Department of Geodesy and Geophysics at Cambridge? Had you heard about continental drift while an undergraduate? Did you have any opinion about it? When, if ever, did you read Wegener, Argand, du Toit and Holmes on continental drift? What about Carey? (I realize that you might want to answer these questions while discussing your graduate work.)

3. When did you become a graduate student in the Department of Geodesy and Geophysics? You mentioned how Hill was your first supervisor. Was that by choice or were you simply assigned to him? After Hill's suicide, you ended up working with Teddy Bullard. Was that by choice? Was it related to your thesis topic? What was your thesis topic? Did you switch topics after Hill's suicide but before you began working with Bullard? Or, did you switch topics once you were assigned to Bullard? (Of course, the last two questions assume that you switched topics.)

4. You mentioned, I believe, how you were a member of the oceanographic expedition to the Indian Ocean that included Fred's and Drum's gathering of the data over the Carlsburg ridge and adjacent seamount which they used in the development and initial defense of their hypothesis. Would you expand upon (a) what happened during the voyage, (b) Drum's role in deciding what to survey, and (c) the nature of your own project? I take it that you were then working with Hill. Is that correct?

5. Did Fred Vine discuss his hypothesis with you? Was it even a topic of conversation around the department? You mentioned the importance of Bullard with regard to, I believe, the development of the Vine-Matthews hypothesis. Would you please expand? Did you know Fred while he was an undergraduate geology major?

6. Harry Hess came to Cambridge in January of 1962, and presented a talk on sea floor spreading. Vine heard the talk, and he was impressed. Dan McKenzie also heard the talk and was struck equally by Hess' ideas and the fact that people like Bullard took them seriously. Can you remember if you were at the talk? If so, can you remember your reaction?

7. Vine gave a talk (his presidential address) before the Sedgwick Club on 10 May 1962. Can you remember if you were there? If so, would you please expand?

8. Bullard switched to mobilism around 1962. Did you ever discuss continental drift, sea floor spreading, the paleomagnetic work of Irving, Creer, Runcorn, Clegg and Blackett with Bullard? How about the Vine-Matthews hypothesis?

9. I take it that you overlapped with Parker and McKenzie at the Department of Geodesy and Geophysics. Is that correct? They also worked with Bullard. (In retrospect, Bullard had three pretty good students at about the same time!)

10. Did you go to the 1965 Royal Society meeting on continental drift which was organized by Blackett, Bullard and Runcorn? If so, would you please describe what happened? What, if anything impressed you? And, any ensuing conversations you had with other graduate students about the meeting? (Runcorn told me that the reason Vine and Matthews were not asked to speak about their hypothesis is that he didn't know about it while organizing the meeting – as the junior of Blackett and Bullard, Runcorn actually did most of the organizing.)

10'. Both Hess and Wilson spent time at Cambridge in 1965. It was during this period that Wilson came up with his idea of transform faults, and he Fred (and Hess with the Gorda Ridge) "found" the Juan de Fuca on the Mason-Raff survey. Did you discuss any of this with them? Did you get to know Hess or Wilson during this time?

11. When did you become familiar with Carey's ideas? Were they discussed while you were at Cambridge? What did you think of Carey's expanding earth view while you were an undergraduate? Any comments about Carey's continued espousal of earth expansion?

12. When did you take your orals for your Ph.D.? Who was on your committee? Would you please discuss how your work on your Ph.D. led, if it did, to your eventual work on heat flow, relation between temperature of sea floor and depth of sea floor, relation between depth of sea floor and age of sea floor. I have not been through those papers, and I have no ideas as to the various contributions by you, Dan, Bob Parker and others. If you could give me a start, I would appreciate it.

13. You mentioned your tremendous respect for Drum Matthews, and said how he was involved in two major developments: The Vine-Matthews hypothesis and Drum's work in the North Atlantic. Would you please give me a reference to Drum's work in the North Atlantic, and briefly explain its importance?

14. When did you arrive at Scripps? Did you go there directly after finishing up at Cambridge? Were you a post-doctoral student at Scripps or a junior professor?

15. What work were you involved with at Scripps?

16. Could you please discuss your role in obtaining Morgan's paper? Were you at Morgan's first talk on plate tectonics at the Spring, 1967 AGU meeting? Did you give Menard a copy of the paper? You told me that Menard told you how he was reviewing Jason's paper for JGR. Would you please expand? Can you date when this occurred?

17. Would you please recount how (a) you did not go to Woods Hole to hear Le Pichon's talk, (b) what you did do (your going to the IUGG meeting, going to MIT, and being at sea)?

18. Bill Menard, in his Ocean of Truth, has said about you, his reviewing of Jason's paper, and/or your relation with Dan the following:

> Jason Morgan sent me a preprint of his manuscript in its early draft, probably in the late spring of 1967. I believe I also received the paper for an editor. In any event the manuscript certainly circulated among my students, and we discussed it. The original draft, however, was difficult for me to fathom, and it did not have the impact of the final publication. Moreover, I had other things on my mind. I spent July through September on the Nova expedition in the southwestern Pacific. Teddy Bullard was among the old hands to

participate, and there were many students including those who knew about Morgan's paper (p. 290-291).

John Sclater was at sea on Nova with me until 12 September, and he was out again from 29 October to 19 November. Meanwhile, two of the five geophysicists who received doctorages at Cambridge in 1967 had joined Scripps. Bob Parker was on the permanent staff, and Dan McKenzie was a visitor for six months beginning in June. Dan and John Sclater shared at apartment a few blocks from Scripps when John was ashore. (p. 291)

In November, John Sclater recalls that

> Dan had just received a tough review of one of his theoretical papers on the internal structure of the earth. As light relaxation he started thinking about plates and in two days produced the North Pacific plate tectonics paper. (p. 292)
> This passage is from a letter you sent to Cox in 1972.)

In light of these passages and the previous question, could you develop your own chronology and deal with the following questions. I add that the McKenzie-Parker paper was received by Nature on Nov. 14, 1967, and I believe you told me that Dan gave a talk at Scripps in October.

In your chronology would you discuss:

(a) Whether Dan talked to you about his own developing version of plate tectonics. Did you speak to him about Jason's ideas? More importantly, if you did talk about his and Jason's ideas, can you remember what you spoke about? Did you understand what he was talking about? Did you discuss any of these ideas with Bill Menard, Teddy Bullard, Bob Parker and Tanya Atwater? Did you have a number of discussions with Dan, and if so, can you recall anything about the development of his ideas?

(b) Can you recall Dan's talking about his ideas, before the Morgan paper arrived? Dan has said that he came up with the idea in June after he had arrived at Scripps.

(c) I've asked Dan what paper it was that he had received a tough review about. Do you know?

(d) Can you recall the reaction to Dan's talk in October? Was Teddy at the talk.

(e) Do you know when Parker got involved? Would you discuss his contribution and its importance. (I spoke with Parker yesterday, and I'm sending him a set of questions.)

19. Would you please discuss?

a. The difference in the approach and papers by Dan and Jason.

b. The importance of Le Pichon's contribution with his sea floor spreading and continental drift.

c. The importance of the Isacks, Oliver and Sykes article, "Seismology and New Global Tectonics." How much of that article was already in Sykes' JGR article, "Mechanism of Earthquakes and Nature of Faulting on the Mid-Oceanic Ridges" (April '67), and Oliver Isacks "Deep Earthquake Zones, Anomalous Structures in the Upper Mantle, and the Lithosphere" (Aug. '67).

20. Would you please describe when and why you decided that sea floor spreading (Vine-Matthews and Wilson's idea of transform faults) was probably correct? You mentioned how it had to do with some of your own work. Would you please describe what reservations you had until you saw that it (or was plate tectonics itself?) nicely explained your own data?

21. Would you please elaborate upon Bullard's contributions in the overall controversy over continental drift, the development of the Vine-Matthews hypothesis, his role, if any, in the development of plate tectonics, and his importance at Cambridge while you were there?

22. What have I missed asking you about that is relevant to the development of Dan's ideas (and Jason's for the matter)?

23. I am enclosing Le Pichon's "The Birth of Plate Tectonics", some of my reprints to give you an idea about what I do, and a draft of a paper I've done on the development of plate tectonics by Jason and Dan. The draft is based upon a brief interview with Morgan and some correspondence with McKenzie. However, their published articles served as the major basis for the paper. I suspect that the paper will be somewhat boring for you. Van Andel suggested I publish it in TERRA. Regardless of whether I do, it will serve as the beginning of a much more extensive account, an account in which I'll integrate your answers, and it will serve as the last historical chapter of a book I'm finishing on the controversy over continental drift. Once the book is finished, I would like to turn to the way in which plate tectonics spread to different areas of the earth sciences, and to what extent it has changed the earth sciences.

Anyway, I would appreciate any comments you might have about the preprint as well as any of the other articles.

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SCRIPPS INSTITUTION OF OCEANOGRAPHY
GEOLOGICAL RESEARCH DIVISION

9500 GILMAN DRIVE
LA JOLLA, CALIFORNIA 92093

Professor Hank Frankel
Department of Philosophy
220 CH Hall
University of Missouri
Kansas City, MO 64110

8/3/92

Dear Dr. Frankel:

I apologize for the long delay in my reply to your questions regarding my participation in the development of the theory of plate tectonics. I started this reply in Austin in January of 1991 and only completed in La Jolla in July of this year. I moved from UT, Austin, to Scripps Institution of Oceanography (SIO) June 1 last year.

For my own benefit I have structured my comments as formal replies to your specific questions. I plan to enter these comments in the SIO archives under the title "The development of plate tectonics: a personal perspective." As the material will be in the public domain you can cite anything you want from it.

I reread the articles by Le Pichon and yourself. I liked both.

However, I have two difficulties with Xavier's article. First, I am pretty certain I did not go to his talk in Woods Hole. I was only in Massachusetts once that year right after the IUGG and I am not sure when Xavier gave his talk. Even if I had gone I do not think I would have understood its significance. Second, in his mention of the institutions and people who were important in the development of sea floor spreading and plate tectonics, he overlooked SIO and Wilson. I am surprised by both. For me, Wilson's paper was seminal in the development of the theory, and there were many important, indeed key, contributions from SIO in the late 50's and early 60's. I know that Maurice Ewing, for whom Xavier worked, was not a fan and was very scathing about the lack of organization of the data that was being collected at SIO. But to omit the papers by Von Herzen and Uyeda on the heat flow across the East Pacific Rise that was so important to Hess in his paper on sea floor spreading and the shipboard seismic work of Fisher, Raitt and Shor that demonstrated that oceanic crust was being subducted surprises me. Fisher and Hess wrote a major paper on trenches for The Sea volume 3 at the time Hess's Buddington volume paper was published. In his 1963 paper there is practically no mention of trenches. It is only when one reads the paper by Fisher and Hess that one gets the picture of the consumption of ocean floor at trenches which is clearly implied by the diagrams in the papers by Fisher, Raitt and Shor. This point is well made in Menard's book where he reviews the development of the ideas of consumption of ocean floor at trenches. Particularly telling are Figures 16 F through H from Menard's book. Before I checked it out carefully I always attributed Figure 16 G either to the Hess or Fisher and Hess paper, not to an independent paper by Fisher published in 1962.

I am not particularly happy as to how history has dealt with Raitt, Fisher and Raff. Mason was elected to the Royal Society. Vacquier has received the Fleming medal but Fisher, Raitt and Raff have received no honors at all.

Your article I found very clear, especially the science. As you can see from my replies to your questions I am sure that Dan and Jason developed the quantitative aspects of the theory of plate tectonics independent of each other.

One last point – on rereading my answers to your questions, it seemed that the development of the heat flow/age and depth/age relations would make a good story. The relations lead to (a) the discovery of black smokers at the spreading centers, (b) our realizing that the surface of the earth, the plates, are the cooling portion of a thermal convecting system, (c) a framework for understanding how continental shelves and basins are formed and the development of a new quantitative method in the research for hydrocarbons, and (4) a realization that Lord Kelvin got the age of the earth wrong not because he did not know about radioactivity, which is the conventional wisdom, but because he ignored the possible effects of convection, about which he should have known.

The relations and concepts developed quickly but in a very haphazard way, with everyone making a mistake before we all finally understood what was going on. It would make good history, especially as the biggest errors were made by Le Pichon, McKenzie and myself! One of the good things about a fast-moving field in science is that it moves so fast that there is no time for anyone to remember the errors.

I trust this letter and my replies are useful. I would be interested in hearing about your reaction to my suggestion for a depth/age story.

Yours sincerely,

John G. Sclater

P. S. A sad postscript, my first wife Freddie, who features significantly in my personal history because our romance overlapped almost exactly my direct involvement in the development of plate tectonics, suffered a catastrophic brain hemorrhage in 1982 from which she never recovered. I have since remarried.

Question 1

Would you please send me the following: (a) a copy of your CV and a list of your publications. It would be extremely helpful if you were to star those publications which you think are the most significant, and briefly explain why. This will help me when I turn to my next project, the proliferation of plate tectonics and how it changed the face of earth sciences. (b) The address of J. E. Everett. I have interviewed A. G. Smith.

I have included a copy of my vitae and a list of my publications. The home address of Jim Everett is Dr. J. Everett, 49 Goldsmith Road, Nedlands, Perth, Western Australia, Australia. Jim is a senior lecturer in the business school at the University of Western Australia.

Brief comments on the papers I consider the most important follow:

5 Sclater, J. G., and H. W. Menard (1967)

Though an early paper in my career and no longer much cited, this paper was extremely important in my development as a scientist. In the summer of 1967 I went out to the southwestern Pacific on the NOVA expedition described by H. William (Bill) Menard in his book *Anatomy of an Expedition.* On the first leg of this cruise Jean Francheteau and I surveyed a massive seamount, which we named Dixon Seamount after the marine technician Fred Dixon, who helped take most of the Scripps heat-flow measurements at sea. On the second leg of this expedition I joined Bill Menard on R/V *Horizon*; we took about thirty heat-flow measurements on the elevated topography of the Fiji Plateau. (This part of the expedition is discussed at length in Bill's book.) Sir Edward (Teddy) Bullard joined us for part of the cruise but left the ship in Fiji. The weather was terrible, and *Horizon* was a small, ocean-going tug. Conditions were very unpleasant. I gained a high regard for Bill from the way that he handled the cruise. He did not attempt to fight the elements, but rather went with the flow of the weather. On a small boat this leaves one in good shape and rested so that one can work very hard when the weather improves.

The results from the cruise were exciting. We found high values on the elevated topography that lay west of the Tonga Trench system and low to normal values on the ocean floor east of the trench. Bill and I realized that this correlation held for all the elevated topographic regions immediately to the west of the major trenches in the western

Pacific. Bill made a deal with me: if I were willing to write the paper at sea when we were unable to work, he would type it. I wrote the paper, he typed it, and we submitted it to *Nature* within a month of getting back from sea. It was published without revision later that year.

I am very grateful to Bill. He showed me that going to sea and doing good science did not have to be a drag; in fact, it could be great fun. Further, this experience taught me that if one had a good idea on a particular problem, it was very valuable to check how well the concept fit all the current data available and not just the limited data set one had collected. This experience taught me the value of always, if possible, going from the particular to the general in the earth sciences. This cruise also taught me something personal that was equally important. I had graduated somewhat in awe of my contemporaries. (In retrospect this awe was justified: two were to get Nobel Prizes and many were to be elected to the Royal Society.) Many of them were extremely able physicists and had a tremendous amount of mathematical sophistication. I came out of Cambridge with the belief that only those with this particular skill could make a significant contribution. Being at sea with Bill showed me that knowing how to pick an important problem and solving it with relevant data was more important for understanding the earth than the sophistication of the analysis. The cruise and the writing of the paper gave me confidence to tackle a whole suite of other problems and was the real start of my scientific career.

#12 McKenzie, D. P., and J. G. Sclater (1969)

This was the first serious attempt to apply the plate model, developed by Dan McKenzie (1967) to explain the heat-flow data, to the seafloor topography. We underestimated the actual topography by about one-third. Our error was to leave out the loading effect of the water. If included, this effect increases the actual subsidence by about one-third. I presented this paper at the IUGG in Geneva, Switzerland, in 1967. Gene Simmons of MIT was the convenor of the symposium. He requested that on my way back to the States I return via MIT and give a talk there. I presented the paper again. Norman Sleep was in the audience and I believe asked some questions that I had a hard time answering. In addition, Gene Simmons let Norman review the paper when it was submitted to the journal that was publishing the conference proceedings. I suspect that the approach that Dan and I took in our paper stimulated Norm Sleep's important 1969 paper on the topography of the Mid-Atlantic Ridge. Norm's model for the spreading center does not balance isostatically at the ridge axis. This is an easy error to make if one is following someone else's approach

and does not fully think through the implications of the changes one has made to the original model. This was confirmed somewhat later when I arrived at MIT and Norman told me that he had not referenced our paper because he did not want to bring out the fact that we had made a mistake by omitting the effect of the water. He pointed out, correctly, that both the original paper on the subject by Langseth, Le Pichon, and Ewing (1966), which tried to use the plate model to show that plate tectonics didn't work, and the 1967 paper by McKenzie neglected the effect of the water. He claimed that Vogt and Ostenso in their 1967 paper in *Nature* addressed this error and showed for the first time that the plate model actually fit the topography remarkably well. I have gone over the Vogt and Ostenso paper carefully at least twice but can't find any reference to including the effect of the water. Though the Vogt and Ostenso paper was not directly important in the development of my thinking about the relationship between depth and age, it is still a seminal paper and has received much less credit than it deserves. That two scientists from the University of Wisconsin, one a new graduate student (Vogt) and the other an assistant professor (Ostenso), were able to write such a paper is remarkable. At this time neither had any obvious access ahead of publication to the papers applying the theory to the oceans. In addition, Wisconsin was totally out of the mainstream.

Even though this was not a significant paper scientifically, it had great importance for my development. My inclination and training is as an observational geophysicist. In this paper I worked closely with an able theoretician, Dan McKenzie. For the first time I deliberately tried to understand fully what he was doing and then to apply my understanding directly to the observations that I had collected. I attribute much of my later success to my willingness to work closely with theoreticians. I think the key is that I concentrate on presenting the observations as well as possible and I do not have my ego tied up in the theory. Thus, I do not try to force the data to fit the theory; rather I let the data talk for themselves.

#20 Sclater, J. G., and J. Francheteau (1970)

This paper has been heavily cited. In it, Jean Francheteau and I reconcile the continental and oceanic heat-flow data with plate tectonics and also show that a simple, self-consistent model could explain both the decrease in heat flow with age and the subsidence of the ocean floor with age. We also resolved – with help from Robert Parker, a colleague at Scripps – the problems of mass balance in the model developed by Norman Sleep. (Included in this publication is a review Jean and I wrote for the Institute for Scientific Inforamtion to describe how this paper (#20) came about.)

Even though this is probably the best known of my papers, I am not overly proud of it. I now believe that we misapplied the Kolmogorov/Smirnov statistics we used to analyze the data and that we downplayed the most important part of the paper – the oceanic model, which presented the first half-way decent fit of any model to both the decrease in heat flow with age and the subsidence of the ocean floor with age. The only point that concerned us a little was that the heat-flow averages on young ocean floor lay below the values predicted by the model that best fit the subsidence with age.

#26 Sclater, J. G., R. N. Anderson, and M. L. Bell (1971)

This is the paper of which I am the most proud. Both the data analysis and the theory are correct, and all of the work was done by me or my two students: Roger Anderson, a graduate student, and Miller Lee Bell, an undergraduate. The paper had an inauspicious beginning.

In late 1969, I went to sea with Tom Hanks, a graduate student at Caltech, to measure the heat flow in the vicinity of the Mathematician Seamount Chain west of the East Pacific Rise at about 20°N. I was chief scientist. During the cruise a crew member became very ill, and we had to cancel our original plans and stay within 500 miles of the coast of Mexico in case the crew member had to be taken back to port.

We had to find some useful science to do near Mexico. I had just completed the topographic models for the paper with Jean Francheteau and was most impressed by the fit of the observations and the theory. Tom and I had noticed a sharp ramp in the topography 200 kilometers west of the East Pacific Rise on our westerly transect out of Acapulco. Both of us realized that such a ramp was exactly what was to be expected if the spreading center that created the East Pacific Rise had jumped eastwards to its current position some 8 million years previously. We decided to run an additional three closely spaced topographic profiles within 500 miles of the coast of Mexico over the East Pacific Rise north of our first profile. The ramp showed up at exactly the same place on all four crossings. Both of us recognized that this couldn't be by chance and that both the subsidence of the ocean floor and the ramp had to be the result of a young spreading center intruding into old ocean floor.

The problem was to develop the theory to see if our idea about the ridge jump were correct and to test whether or not the observational data for the rest of the world agreed with what

we had found on the East Pacific Rise. Previous treatments had ignored the the flow of heat in the horizontal. We had to include this to account for the ramp. In early 1970, I tried many attempts at a solution. All were unsuccessful. I went to see Bob Parker and John Miles, who are both excellent applied mathematicians at the Institute for Geophysics and Planetary Physics (IGPP) here at Scripps. Neither could see an easy way to do the inverse Laplace transform required to solve the problem. I knew it was important, and I was really frustrated that we couldn't arrive at an analytical solution. Also, I knew Dan McKenzie was working on the same problem, and I was concerned at having to wait for his solution before I could analyze any more data.

My frustrations were obvious to all around me. One Friday I left some of my unsuccessful efforts on one of the tables in the lab. Our undergraduate lab helper, Miller Lee Bell, a physics major at UCSD, was taking a course in Laplace transforms at the time. He thought he might be able to help. I let him look at the problem but was not at all hopeful that he would contribute anything after the difficulties that two able mathematicians from IGPP had had. Imagine my surprise to come back after the weekend and discover the solution on my desk. It was not fully correct, but he had had the insight that enabled both of us to work through the solution completely. As soon as I had this solution in hand I set to work on applying the model to the data and to analyzing as broad a range of data as I could easily lay my hands on.

Having convinced myself that the subsidence of the ocean floor was due to the creation of oceanic plate, I decided to tackle other oceans. First, Lee Bell and I plotted out three profiles each from the South Atlantic, the Indian Ocean, and the East Pacific Rise. Bill Menard (1969) had attempted to do the same thing earlier. However, he picked profiles in areas of anomalous depth and obtained very scattered results when he plotted the depth against age. When we examined profiles from areas that we thought better represented the overall depth, we obtained remarkably consistent results.

With the data and theory completed, we submitted the paper to the *Journal of Geophysical Research* (JGR) in 1970. To our surprise it was not received with the great acclaim that we thought it deserved. In particular, Ron Oxburgh (one of the reviewers) was not very convinced by our use of only nine profiles. I was upset and used my anger to look at all the topographic profiles that I could lay my hands on. Roger Anderson had just joined me as a graduate student, and he helped me collect and plot the data. At Scripps, through the good graces of Bob Fisher, I was able to gather the data from the Indian Ocean. For the

Pacific, I was able to collect the data from the compilation made by Tom Chase, who worked for Bill Menard. Earlier that year I had approached Bill Menard (who was in Cambridge on a sabbatical) about doing a joint project. Having not heard back from Bill, I decided to complete the project with just my own students. Denny Hayes of Lamont – even though he did not agree with me – was most helpful in acquiring topographic data from the Eltanin cruises in the South Pacific.

Roger Anderson and I plotted up all the data. Lee Bell had left by this time for graduate school at Stanford. We obtained exactly the same relation with 81 profiles that we had with 9, except that – as expected – the scatter was much reduced. We submitted the revised paper to JGR, where it was immediately accepted. I think that Fred Vine was the associate editor.

In retrospect I am very grateful to Ron Oxburgh for his review. It made me look at so much more data. It is the overwhelming agreement of large quantities of data from different oceans that convinced everyone of the validity of the depth/age relation.

#29 McKenzie, D. P., and J. G. Sclater (1971)

This paper on the Indian Ocean missed being the entire issue of the *Geophysical Journal of the Royal Astronomical Society* by just one short paper. In 1968, I was scheduled to go to sea to help Albert E. J. (Al) Engel, a petrology professor at Scripps, to dredge the Ninetyeast Ridge in the Indian Ocean. Bob Fisher, who had sponsored my coming to Scripps in 1964, had organized the cruise. When Al Engel dropped out, Bob asked me to take over the cruise and to dredge the Ninetyeast Ridge. The cruise was about a month long and was scheduled to go from Colombo in Ceylon to Mauritius. I was excited to be given the ship time but did not know quite what to do. I asked Dan McKenzie to come to sea with me, and he agreed. The papers of the Lamont group on matching magnetic anomalies in the various oceans had just been published, and we decided to test some of the ideas we had gained by reading the Heirtzler et al. paper on the Indian Ocean. We decided to concentrate on the flat ocean floor south of Ceylon and omitted all but five dredges on the Ninetyeast Ridge in favor of two long magnetic lines running north-south parallel to a Vema line on which magnetic anomalies 21 through 30 had been recognized.

We observed the same anomalies on our lines and were able to get a direction for the lineations. We had computers at sea, and Dan wrote a program to generate synthetic

anomalies. Shiri Shrivistava from Bedford Institute and I put together a program to process the data. On my way back through New York I stopped off at Lamont and persuaded Ellen Herron to allow me to duplicate all the Lamont data from the Indian Ocean and take it back to Scripps. Interrupted only by my marriage in Hawaii and a honeymoon in Scotland, I returned to Scripps to develop the processing routines with Wesley (Wes) Hilton, a programmer at Scripps, and then to digitize all of the Scripps magnetic data in the Indian Ocean. Dan joined me in Scripps late in the same year, and we interpreted as much of the data as we could. Dan returned to Cambridge and wrote a whole series of programs for reconstructing the past position of the continents. I continued to process the data and sorted out the enigma of the Arabian Sea. In late 1969, Dan and I tackled with Bob Fisher the Central Indian Ridge; once we had resolved this problem we were ready to complete the reconstructions.

Prior to this paper it had not been clear that the plate tectonic concept worked for reconstructions of the Indian Ocean. The significance of this paper is that it showed that having a lot of data, rather than making things more difficult, made the interpretation easier. An additional point was that we presented the first truly quantitative method for reconstructing the past position of the continents.

#25 Fisher, R. L., J. G. Sclater, and D. P. McKenzie (1971)

In this paper we first used topographic contours and magnetic anomalies to sort out the tectonic history of a complex area of the ocean floor. Some scientists had used the wavy trends of the Central Indian Ridge to argue against a plate tectonic model. In contrast, we showed with this paper that the current pole for Africa/India obtained from the transform faults, earthquakes, and magnetic anomalies allowed us to fit the Chagos Laccadive Ridge snugly into the Mascarene Plateau about 40 million years ago. The resulting feature was a long and linear ridge parallel to the Ninetyeast Ridge. The Chagos Laccadive Ridge and the Mascarene Plateau clearly represented excessive basaltic vulcanism on the Indian Plate and marked the motion of India northwards with respect to Antarctica. Understanding the tectonic history of the Central Indian Ridge over the past 40 million years was crucial to determining the evolution of the whole Indian Ocean from the Jurassic to the present.

I started on this paper after the one mentioned above with Dan McKenzie. But this paper with Bob Fisher was published first because it dealt with only a portion of the Indian Ocean and hence was much shorter.

#53 Williams, D. L., R. P. Von Herzen, J. G. Sclater, and R. N. Anderson (1974)

Clive Lister had proposed that the variation in heat-flow values near a ridge axis was due to hydrothermal circulation. On a big leap of faith Richard P. (Dick) Von Herzen and I decided to test this idea by building a vertical array of thermistors that we could tow across a spreading center. We choose the Galápagos Spreading Center because I already had funding to examine the variation of heat flow with age. Neither Dick nor I really believed Clive, because we were influenced by the petrologists, who argued that, because of the freshness of the basalts dredged at the ridge axis, if hydrothermal circulation did occur, it had to be very minimal. We carried out four or five tows across the spreading center, and initially David L. (Dave) Williams, who had built the instrumentation and ran the experiments, thought that we had not found anything. However, on analyzing the data back in the lab, Dave found that for a short section of the record, two of the three thermistors had gone off scale (>1°C range) and the other showed a clear increase and decrease. The only believable interpretation was that we had gone over a plume of very hot water. This was the first documented evidence of a hot plume on the ocean floor. Only Jack Corliss of Oregon State really believed us, and he immediately mounted a diving expedition on the Deep Submersible *Alvin* to the area. In the following year he was the first to observe hydrothermal vents on the ocean floor.

#62 Sclater, J. G., C. Bowin, R. Hey, H Hoskins, J. Pierce, J. Phillips, and C. Tapscott (1976)

In September 1972 I left Scripps to join the faculty at MIT. My first big cruise on a Woods Hole Oceanographic Institution (WHOI) vessel was in the austral summer of 1974-75 to the very inhospitable howling 50's to study the Bouvet Triple Junction. Carl Bowin ran the general surveys of the area on the first leg of this project, and I ran the detailed surveys over the three limbs of the spreading centers on the second leg. I had a great deal of help from Richard (Dick) Hey on this cruise, and it was he and I who rescued the project from disaster. Even as late as 1974, the rigidity of plates and their effect on triple junctions was not fully understood by most seagoing scientists. The major result of this cruise was to show that the Bouvet Triple Junction was a ridge-transform-transform type. It was stable and – to the accuracy of our measurements (i.e., 10 km) – the plates appeared to act as rigid bodies. It was a nice observational confirmation of the theory and strongly supported

Dick's somewhat speculative paper on the Galápagos Triple Junction that had been published some few years before in *Nature*.

#64 Sclater, J. G., J. Crowe, and R. N. Anderson (1976)

This is another observational paper of which I am particularly proud. The results completely convinced me of the correctness of the simple intrusion model we had developed for the heat loss and subsidence with age of the ocean floor. Clive Lister had proposed that hydrothermal circulation at a ridge axis would perturb the heat flow. These perturbations would have two effects. First, the data would be highly scattered, and second, if the circulating water could interact with the seawater, heat would be lost by hydrothermal circulation, and we would not be able to measure the heat loss. Thus the averages we calculated from measurements with conventional techniques would be an underestimate of the actual loss of heat. We proposed, in this paper, that if a 20-by-20-km area of the seafloor were covered by more than 200 m of impermeable sediments then no heat would be lost by hydrothermal circulation and the average value would represent the true value. We predicted, from the models that matched the subsidence, that on 36-million-year-old ocean floor (anomaly 13) the heat flow would be 2.0 mcal/cm^{2}s. Anomaly 13 shows up very clearly on the flanks of the Rekjanes Ridge in the North Atlantic south of Iceland. The area is also covered by a thick blanket of sediment. We decided to test our hypotheses there. We obtained exactly the value predicted and ended up with as simple a relation between heat flow and age as we had developed for that between depth and age. The only condition on the heat-flow measurements was that they had to come from areas that were well covered by sediment.

#73 Parsons, B., and J. G. Sclater (1977)

This paper is still widely cited because Barry Parsons, who was a postdoctoral fellow with me at MIT, and I did a careful job of looking at the relationship between depth and age on ocean floor more than 100 million years old. In our original paper, we had just examined that data for ocean floor younger than 80 Ma because older anomalies had not been identified at the time the initial analysis was completed. Bob Parker, Doug Oldenburg, Clive Lister, and Earl Davis had shown not only that a simple plate model could account for the subsidence of the younger ocean floor, but also that a cooling half-space model would do equally well. Their work implied that the subsidence of the ocean floor was simply due to the intrusion process and that there was no necessity to assume a plate of constant thickness. Barry and I did a careful analysis of all the data to see if we could differentiate

the two models. It was well known that if the half-space model were to be correct then the depth of the ocean floor would continue to increase as the square root of the time even on very old ocean floor. We analyzed the depths in the deepest portions of the ocean floor and showed that the average depth flattened exponentially on 100-million-year-old and older ocean floor, strongly supporting some form of plate model. We found that the heat flow, which had originally been used to construct the plate model, actually provided little or no constraint. We inverted the depth data to estimate the temperature at the bottom of the plate and the thermal expansion coefficient. This was the first attempt to use data on heat flow and depth to compare and contrast the two models. We found that for the first 100 million years the ocean floor behaves like a cooling half-space. But thereafter, extra heat appears to be brought to the base of the oceanic plate, and the data are much better fit by a plate model. Then the big problem – still unresolved – becomes, Why do the depths flatten in this way?

#84 Sclater, J. G., C. Jaupart, and D. Galson (1980)

This was a redo of Sclater and Francheteau on a worldwide basis using all the available data. The objective of this paper was to analyze all the heat-flow data within the framework of plate tectonics and to compute a value for the heat loss of the earth. We showed that if one takes account of the amount of heat loss due to hydrothermal circulation at the ridge axis, the heat loss for the earth is about 50% higher than previous estimates. Also, we found that the heat loss through the continents was about half that through the oceans.

#98 Sclater, J. G., B. Parsons, and C. Jaupart (1981)

This paper used the information from Sclater, Jaupart, and Galson to examine similarities and differences between models of continental and oceanic heat flow. Teddy Bullard, then based in La Jolla, was dying of cancer, and Art Maxwell, Chris Harrison, and I – all of whom had been influenced very much by Teddy – decided to run a symposium in his honor at Scripps. This paper was presented at the symposium and then published in the *Journal of Geophysical Research.* It is well cited because we published a colored map of the ocean floor and the continents as a function of age. We argued in this paper that the plate structure does not differ much under oceans and under continents. However, seismic data seem to show that the lower portion of the continental lithosphere may be quite complex and that our inferences for similarity may be less justified than we thought. But,

our basic point that the earth is principally losing heat by creating oceanic plate is generally accepted.

#83 Royden, L., J. G. Sclater, and R. P. Von Herzen (1980)

Leigh (Wiki) Royden was a very bright physics undergraduate from Harvard who worked with Dick Von Herzen and me on models for determining how continental crust could be thinned and how hydrocarbons in the thick sediments of the continental shelves would mature through time. We started this project as a consulting job for people in the oil industry who wanted to know whether the new discoveries in plate tectonics could help in hydrocarbon exploration. We proposed a series of models to account for the exponential subsidence of the continental shelves that had been shown by Norman Sleep to be similar to that for the observed exponential subsidence of old ocean floor. One of our models was similar to the simple stretching model put forward by Dan McKenzie in 1978 to account for the high heat flow in the Aegean Sea. Our work was completed at the same time as that by Dan but took ages to publish because the American Association of Petroleum Geologists is leery of having equations in their publications. The most important part of this paper was the quantification of the index of thermal maturation.

#91 Sclater, J. G., and P. A. F. Christie (1980)

I spent a year back in Cambridge, England, from September 1979 till August 1980. I had just completed the paper with Wiki and Dick and was keen to apply these ideas. The North Sea was a major area of oil exploration, and I wanted to see how well the ideas of Dan and ourselves would work. Phil Christie, who had an undergraduate degree in physics from Oxford, had worked for Schlumberger before he came back to graduate school at Cambridge. He knew a great deal about analyzing well logs, and he kept the interpretation very close to the real data. To obtain data from the oil companies with holdings in the North Sea I had to do something useful for them. As a consequence I developed in this paper a simple method of removing sediments through time to look at the dynamic subsidence, which is what one actually compares with the models. The portion on methodology is probably the most useful part of the paper. However, we found that to explain the subsidence data we had to propose 150 km of stretching in the North Sea. This was much more than oil company scientists who examined the throw on faults from multichannel seismic lines were willing to permit.

#123 Sclater, J. G., and M. Shorey (1989)

Our North Sea paper became quite controversial in industry because of the large amount of stretching we proposed. However, it was not possible for academic scientists to get access to and publish really good seismic data from the area because of problems with confidentiality. Eventually, I got access to a line that was shot in 1984 across the Central Graben of the North Sea from a Norwegian spec-shooting group called NOPEC. By combining theoretical analysis done by Nickolas (Nicki) White and Dan McKenzie with our observations and reinterpretation of the data we were able to show that the oil industry geologists were substantially underestimating the amount of extension. I still don't know whether or not our ideas are accepted by industry. (The BBC did a feature on this in which I appeared in 1986.)

QUESTION 2

Could you briefly describe your undergraduate training? Where and what did you study as an undergraduate? From whom did you take courses? Were there any teachers who particularly influenced you? When and why did you decide to go into geophysics? When and why did you decide to enter the Department of Geodesy and Geophysics at Cambridge? Had you heard about continental drift as an undergraduate? Did you have any opinion about it? When, if ever, did you read Wegener, Argand, du Toit, and Holmes on continental drift? What about Carey? (I realize that you might want to answer these questions while discussing your graduate work.)

My father was a prominent Edinburgh physician who was a Fellow of the Royal College of Physicians in Edinburgh. Also, he taught at Edinburgh University Medical School in the undergraduate and graduate programs. I was sent to boarding school at the age of 5 because of an agreement by my mother, who was a Roman Catholic, and my father, who was an agnostic, that I be brought up as a Catholic. From 5 through 13 I was educated by Benedictine monks at Carlekemp Priory School in North Berwick, which is 25 miles from Edinburgh. From 13 through 18 I was educated by the Jesuits at Stonyhurst College in Lancashire, England.

I did well but not outstandingly at school except in history. However, there are no jobs in history, and when I was 15 I surprised myself by doing well in a chemistry practical. As a

result of this success, after I passed my O levels at 15, I decided to become a scientist and to concentrate on maths, physics, and chemistry for my A levels (the college entry exams in the U.K. at that time). I had hoped to be funded by Stonyhurst College to return for an extra term to take the Oxford scholarships, but was disappointed to find out that I wasn't considered enough of a certainty for the college to part with its money. My father, who was very Scots, was unwilling to pay for another year of tuition at school, especially if it meant that I would end up going to an English university, even one as prestigious as Oxford. He brought me home and I went to Edinburgh University. I must be one of the few people who did not grow up with his parents but then spent his college years at home. After the rigors of Jesuit boarding school I really enjoyed home life!

I was particularly influenced by two masters: the physics master Fr. Worthy, who ran the Stonyhurst Observatory, which is part of the worldwide geomagnetic network, and my math teacher, whose name I have forgotten. Our math teacher was studying to be a Jesuit. He was not very good at math, but he loved to take us on long walks in the beautiful Fell country of Lancashire around Stonyhurst. I liked being outdoors and used to talk to him on these trips about his college career. I discovered that he had been trained as a geologist and that there was a career called geology that involved lots of walking around outdoors looking at rocks and fossils. I was hooked and decided that I wanted to be a geologist. I even asked the master why he wanted to give up something like geology that seemed like so much fun to become a Jesuit, which seemed so dull. I became even more enchanted with geology when I discovered that if one worked for an oil company one could get well paid, travel all over the world, and even live in countries like Saudi Arabia where the sun shone all the time! (The sun never shines in Lancashire, and my memory of school is of it raining almost every day and being so damp that the stone walls of the school were always weeping.)

My father obviously enjoyed his work as a doctor. I was determined to be a professional like him. He told me that if I wanted to be well paid as a professional I needed to have a Ph.D. One of my father's patients was Dr. Donald Duff, who was the undergraduate science advisor for the geology department. As soon as I decided to go to Edinburgh University I contacted Dr. Duff. I felt confident I could handle an undergraduate geology degree but was not so sure about the maths and physics. I wanted to do geophysics, but that was not an option. Dr. Duff persuaded me to take two years of physics and then to transfer and do four years of geology. This would take me five years, and I would start with physics.

I started at Edinburgh doing a pure physics major, taking physics, applied maths, and maths. I did much better than expected and came third in my year in physics. I did geology in my second year but did not find it as challenging as the physics, so half-way through the year I decided to change majors from geology to physics (most people go in the opposite direction). Dr. Duff pointed out two of the advantages to changing: I would finish in four years, and job opportunities in physics at that time were much greater than in geology. The change was pretty traumatic because I had to try to learn one year of honors maths in half a year. Somehow I made it, and by the third year I was enrolled for a degree in physics. I am very grateful for Donald Duff's support. The physics department at Edinburgh was not a great research center, but it ran then and still runs a superb undergraduate teaching program in experimental physics. I gained a first-class education in experimental physics that was to stand me in very good stead later on. Also, I was fortunate in going through Edinburgh with a group of extremely able fellow students who kept the level of the class very high. About one-quarter of the class went to Cambridge to do graduate work. Some 50 percent have Ph.D.'s, and one-third teach at major universities around the world.

Though I did physics as an undergraduate I always intended to go to graduate school in geophysics, though having done well in physics I did flirt with the idea of doing biophysics. Being able to do fieldwork out of doors tipped the scales in favor of geophysics. When it came time for me to apply to graduate school I was influenced by Dr. Marion Ross, who was reader in physics at Edinburgh. She had worked on magnetic mines with Sir Edward Bullard during the war, and she persuaded me to apply to Cambridge. After initially being turned down, I was accepted late and entered the Department of Geodesy and Geophysics in the fall of 1962.

Holmes had retired as professor of geology at Edinburgh just before I started as a student. I saw him a couple of times in the library but never actually conversed with him. If my memory serves me correctly, the Edinburgh department was pretty much in favor of continental drift. For our first-year classes we all used Holmes's book *Principles of Physical Geology*, which advocates drift. I read about du Toit and Wegener in Holmes's book, but continental drift as an exciting research topic did not register with me. I have never read du Toit or Argand and only read Wegener when one of my students at MIT got a copy.

QUESTION 3

When did you become a graduate student in the Department of Geodesy and Geophysics? You mentioned how Hill was your first supervisor. Was that by choice or were you simply assigned to him? After Hill's suicide, you ended up working with Teddy Bullard. Was that by choice? Was it related to your thesis topic? Did you switch topics after Hill's suicide but before you began working with Bullard? Or did you switch topics once you were assigned to Bullard? (Of course, the last two questions assume that you switched topics.)

When I went to Cambridge in September 1962, it was clear to them that my background in experimental physics could be put to good use. I had built a proton magnetometer for my senior project in Edinburgh. Maurice Hill ran the seagoing program at Cambridge and knew that I wanted to do experimental work and was more than willing to go to sea, especially if it meant I could travel. Sir Edward Bullard supervised the theoretical students, so there was no chance that he would be my supervisor. As a graduate student I did switch topics, but both were under Maurice Hill. My original project was to build a low-frequency ocean-bottom seismometer to be used on the 1963 RRS *Discovery* cruise to the Indian Ocean, in which all the seagoing students would participate. Because good, stable very-low-frequency transistors did not exist at that time, I had to use vacuum tubes for the input section. In retrospect, my task was very hard, but all the seagoing students were expected to build a new piece of equipment, and I learned a great deal from the experience. Because of my success in constructing this equipment, I was asked to learn how to use the heat-flow equipment that had been built by my immediate predecessor in the department, Clive Lister.

On the 1963 Indian Ocean cruise I was expected to bring along the low-frequency seismometer I had built and to run the heat-flow equipment. The low-frequency equipment flooded the first time I used it, and I never got it to work again. The heat-flow equipment rolled off the bench and disintegrated before I ever used it. Also I was terribly seasick for the first four days at sea. I rebuilt the heat-flow equipment at sea and got it to work. In the Somali Basin we took measurements, which were average to low, and we obtained high values in the Gulf of Aden. The main thrust of my work on the cruise was to be in the Red Sea. However, the very hard sediments of the southern part of this sea ripped the thermistors and outriggers used in our equipment off the core barrels. We were hoping for many measurements but recovered no useful data. I and the senior technician had a fierce

fight with Maurice Hill about whether to continue or to move on to another area. He was for changing position; we were for staying where we were. Because of his drinking problem, which had become more and more noticeable as the cruise progressed, we did not respect his advice and continued with our own plans. In retrospect, it turned out that we would have done much better had we followed his suggestion. Maurice had a wonderful feel for what was and was not possible at sea.

On returning to Cambridge, I worked up the heat-flow data, which ended up as a paper (#2) published in 1966 by the Proceedings of the Royal Society dedicated to the results of the Indian Ocean expedition. During my working up of the data at Cambridge, Teddy Bullard showed much interest in my results. He was most concerned about the variability of the local heat-flow measurements and suggested I do some experiments using an electrical analogue to thermal conductivity variations to examine the heat-flow around structures of varying thermal conductivities. I was successful in building the experimental apparatus, and with help from fellow students Jim Everett, Bob Parker, and Dan McKenzie I was able to develop a good theoretical understanding of my results.

Bob Fisher had come to Cambridge in September 1963 for a 12-month sabbatical. He had worked extensively in the Indian Ocean and was most interested in the results from our recent trip to the same area. Scripps had an expedition going to the Indian Ocean in the fall of that year, and Bob asked me if I would like to participate. With support from the U.K. for travel and from the National Science Foundation for per diem, I took off in September for Mauritius to join R/V *Argo* on the cruise for which Victor Vacquier was to be chief scientist and on which Dick Von Herzen was to participate. On the first leg we took lots of heat-flow measurements on a series of diagonal crossings of the Central Indian Ridge to test for high values near the ridge crest. The second leg involved taking heat-flow measurements in the north central Indian Ocean between Colombo and Singapore. The results from the first leg were somewhat equivocal – high but scattered values near the crest, lower values elsewhere – but nothing spectacularly conclusive. However, the general trend shown by Von Herzen and Seiya Uyeda over the East Pacific Rise appeared to be borne out. Surprisingly enough, there was little talk of Hess's or Vine's work or papers on this cruise.

I returned to the U.K. through Scripps, where I stayed for about a month to work with Vic on the results of the cruise. Vic asked me to look at a computer method that Charles (Chip) Cox was developing to examine how differences in the thermal conductivity of sediments

and basement affected heat-flow measurements. The problem was that closely spaced heat-flow measurements often differed by an order of magnitude. Teddy Bullard often complained that he had never had anything named after him, so Maurice Hill coined Bullard's Law: "Never repeat a heat-flow measurement in rough topography because the value could differ by an order of magnitude." The problem was serious because until we understood the high scatter in the data no one was going to take heat-flow measurements seriously. I decided for my thesis to investigate how variations in thermal conductivity affected the surface heat flow – one possible explanation of the high scatter.

Chip had given the task of writing the code to a programmer. Because of my background with electrical analogue models, I saw a small flaw in Chip's approach. I asked to be able to work on the project for a couple of weeks to see what I could do. I worked really hard and got the code working, sorted out the bug, and tested the program against the predictions of some simple structures for which I already knew the analytical solution.

On returning to cold and damp England in late 1964, quite depressed because I had so much enjoyed the beach and sun during my six weeks in California, I asked Teddy if I could use the work I had done at Scripps as part of my thesis. Teddy was very supportive and arranged for me to get free time to develop the program on the IBM computer at Imperial College in London. (Computer facilities at Cambridge at that time were not adequate for my needs.)

I went to sea again with Maurice Hill in March and April of 1965 to take heat-flow measurements. This was a badly-thought-out cruise to an area of only minimal interest where the weather was likely to be very rough. It was rougher than expected. The wind speed averaged 28 kts for 30 days, with two gales thrown in. I stuck it out but came back pretty depressed about having wasted two months at sea and having few measurements to show for it. However, in my absence, Vic Vacquier had written Teddy asking if he would recommend me for a Postgraduate Research Assistantship at Scripps. He had. On my return from sea I found that Teddy had accepted the position for me. I was pleased, but it also meant I had to write up and get out fast.

Maurice Hill was having real trouble with his drinking after the last cruise and went off to a sanatorium to recover. David (Dai) Davies took over ostensibly as my supervisor, but all the useful advice came from Teddy or Chip Cox. My thesis consisted of two parts – a write-up of the heat-flow data from the Indian Ocean cruise and an examination of how

near-sea-bottom variations in thermal conductivity affected local heat-flow variability. Chip Cox was wonderful help on the second part. Together we developed the whole quantitative background for a finite difference approach to the problem.

I finished in October of 1965, in just three years, including eight months at sea, and defended in November. After sorting out problems with my visa, I left for Scripps in mid-December 1965.

Although my thesis was actually finished by 1965, my college was unwilling to put me up for my degree until I signed that I had spent 59 nights in college for each of the nine terms I had been at Cambridge. As this was an obvious lie, I had refused to sign. But finally, to get my degree, I signed their form and graduated in June of 1966.

Maurice Hill committed suicide early in the New Year in 1966. This was a serious personal loss to all of those who had been close to him. Later, we learned that he had a serious neurological condition that may have been hereditary; his mother had Parkinson's disease. What I most admired about Maurice were his love of the sea and his enthusiasm for his and others' observational research. This, coupled with his obvious talents and intellectual modesty, made him a wonderful foil to Teddy Bullard.

QUESTION 4

You mentioned, I believe, how you were a member of the oceanographic expedition to the Indian Ocean that included Fred's and Drum's gathering of the data over the Carlsburg Ridge and adjacent seamount which they used in the development and initial defense of their hypothesis. Would you expand upon (a) what happened during the voyage, (b) Drum's role in deciding what to survey, and (c) the nature of your own project? I take it that you were then working with Hill. Is that correct?

There were two major expeditions to the Indian Ocean in which the Cambridge group participated: one in 1962 aboard HMS *Owen*, in which neither Fred Vine nor I participated but Drummond Matthews did, and a second in late 1963 on which Fred and I were cabin mates for three months.

If my memory serves me correctly, the data used by Drum and Fred were collected on the first cruise, in which only Drum participated. (I am not absolutely sure Fred did not go on this cruise, but he was an undergraduate at the time, so I do not see how he could have participated.)

The expedition on which I participated was led by Maurice Hill and concentrated on two major seismological experiments. The objectives were two-fold: one, involving Tim Francis, to determine the crustal thickness between the African mainland and the Seychelles, and the other to determine the crustal thickness of the Seychelles Plateau. There was little time for heat-flow work and even less for the magnetic anomaly profiles and seamount surveys that Fred Vine wanted to run. I certainly felt that Fred did not get enough ship time on this cruise. I had not bothered to study his paper with Drummond Matthews on seafloor spreading that had been submitted to *Nature*, but I felt he should at least get a chance to do some seamount surveys. My arguments were based on fairness, not science. I was not popular for challenging my seniors (Hill and Anthony Laughton) but Fred got his ship time. Actually the captain of RRS *Discovery* supported us. He was a decorated war hero, and Maurice had a lot of respect for his opinion. In retrospect Hill, Laughton, and Matthews treated me as an equal, and though it was hard for a 23-year-old student to take their criticism, they really respected those who stood up and argued with them. Also, they were sufficiently open-minded that if they felt we had a good case, they would change their cruise plan.

In retrospect, the surprise on this whole cruise was how little effort was made to check out the paper by Drum and Fred and how little Drum and Fred fought to do this. If my memory is correct, I was the only one who argued for more time for magnetics, and I think even Drum initially argued against me! Fred was disappointed that we all didn't take his paper more seriously, but was too much of a gentleman ever to let us know this in public. I only picked it up because I was rooming with him. (Also Fred had just become engaged to the lady who would become his wife, and I suspect he spent more time thinking about her than about science during this cruise. If the positions had been reversed, I know I would have done the same!)

QUESTION 5

Did Fred Vine discuss his hypothesis with you? Was it even a topic of conversation around the department? You mentioned the importance of Bullard with regard to, I believe, the development of the Vine-Matthews hypothesis. Would you please expand? Did you know Fred while he was an undergraduate geology major?

Though we spent three and a half months on the late 1963 cruise on RRS *Discovery* in the Indian Ocean, I never directly discussed the seafloor spreading hypothesis with Fred. However, we did have some discussions in the laboratory of what one could do with seamount surveys and how they would support the concepts. I did not participate too much in the science discussions, because the two instruments I was responsible for were almost always close to breaking down. My memory of the cruise is of long hours in the laboratory fixing the equipment and a similar number of hours in the bar recovering.

At Cambridge I was most impressed by Drummond Matthews. In his office in a barn ("the Drummery") at Madingley Rise he had all the published crossings of the mid-ocean ridges. They had been blown up and made into cut out profiles. I can remember that even then I was impressed by the size of the central magnetic anomaly.

I don't know for sure how important Teddy Bullard was in the development of the seafloor spreading theory, but I remember either Drum or Fred telling me that Teddy had the idea of assuming that the process of intrusion could be symmetric. It is the tradition at Cambridge that this discussion was in a nearby pub – the Plough and Harrow – that the students and junior staff often went to on their way home from the department.

I did not know Fred Vine as an undergraduate geology major at Cambridge. I was a senior in physics at Edinburgh when he was a senior at Cambridge in 1962.

After Fred and Drum came up with their hypothesis of seafloor spreading it was often a topic of conversation at coffee (at 11 a.m.) or tea (at 4 p.m.) at the Department of Geodesy and Geophysics at Madingley Rise. Most of the comments about the theory were humorous. Most of us still believed that the continents were fixed, and we were all surprised that *Nature* published what we considered idle speculation. We gave both Drum and Fred a lot of friendly grief over their success at conning *Nature*.

Maurice Hill, who was perhaps the major critic, did not believe it at all. He discussed Vine's thesis alone with me one Saturday morning in 1965 just after he read it. He felt that the whole theory for Harry Hess's idea through Vine and Matthews was unsubstantiated speculation.

During our discussions at coffee or tea, Teddy Bullard was always coming up with ideas about the earth. Maurice Hill was openly critical of most of these ideas. In many ways this was wonderful for us students to hear. Maurice was always ready to ask Teddy to justify his speculations, and particularly emphasized the importance of observations for testing hypotheses. Students were actively encouraged to participate and were treated as equals when we did. For those of us who liked to talk and were not too self-conscious, it was an invigorating intellectual environment. The discussions were wide-ranging and challenging, covering geology, geophysics, physics, applied maths, politics (the Profumo scandal, which Teddy found out about from Harold Wilson – then head of the opposition party – months ahead of the British press), and the campaign for nuclear disarmament (P. M. S. Blackett, a friend of Teddy's, was a supporter of Campaign for Nuclear Disarmament – Teddy was an active opponent of CND), which was a popular issue in the U.K. in the early sixties.

Maurice Hill's comments about Fred Vine's thesis had an effect upon me. They made me reconsider the whole hypothesis and certainly made it harder for me to accept the concept. At this time I was critical of all theoretical ideas. All I wanted to do was to obtain reliable heat-flow observations from the ocean floor that I could write up for my thesis, and to understand the variability of the values I had obtained. My horizons were very limited.

QUESTION 6

Harry Hess came to Cambridge in January of 1962, and presented a talk on seafloor spreading. Vine heard the talk, and he was impressed. Dan McKenzie also heard the talk and was struck equally by Hess's ideas and the fact that people like Bullard took them seriously. Can you remember if you were at the talk? If so, can you remember your reaction?

I did not arrive in Cambridge until September 1962. I did not attend the lecture by Hess. Also I cannot remember talking to Hess in 1962. I first learned of his ideas through discussion of the Vine-Matthews hypothesis at coffee or tea in 1963.

QUESTION 7

Vine gave a talk (his presidential address) before the Sedgwick Club on 10 May 1962. Can you remember if you were there? If so, would you please expand?

Again, I was not in Cambridge at this time and did not listen to Fred's talk.

QUESTION 8

Bullard switched to mobilism around 1962. Did you ever discuss continental drift, seafloor spreading, the paleomagnetic work of Irving, Creer, Runcorn, Clegg, and Blackett with Bullard? How about the Vine-Matthews hypothesis?

I was a close friend of Jim Everett's in Cambridge. He worked on building a magnetometer for use on land, and with Teddy on the paper that became Bullard, Everett, and Smith. Teddy discussed this paper with Jim and me before asking Jim to work on it with him. Jim was a better theoretician than I was, and Teddy was very impressed that Jim had spent a year at MIT before coming back to Cambridge as a graduate student.

Teddy was impressed by the work of Sam Carey on reconstructing Africa and South America on a globe. He wanted to test this idea using a computer technique. In the resulting paper, he and Jim developed a computer method for determining the pole and angle that best described the rotation necessary to superimpose the east coast of South America on the west coast of Africa. They were the first to apply Euler's theorem to the rotation of plates.

The only people with whom I discussed Carey's ideas were Jim and Teddy. However, Irving's work on paleomagnetism and that by Creer was highly respected by Teddy, and he often mentioned their papers. In fact, one of the first things Teddy did when he returned to Cambridge as reader and head of department was to get Ted Irving a D.Sc. Ted was not given a Ph.D. because of a personality battle between Keith Runcorn and Ben Browne,

who succeeded Teddy as head of department in the late fifties. (I'm not exactly sure of this, but Keith probably knows the answer better.)

Runcorn's work was controversial. Though Teddy had a high regard for Keith's work on paleomagnetism, there was much concern about Keith's approach to fluid dynamics and convection. However, it is my belief that it was Keith who first understood the importance of Euler's theorem and the concept of poles of rotation. They were called pivot points in the paleomagnetism literature and were used well before Bullard, Everett, and Smith introduced poles of rotation in the paper on the fit of the Atlantic continents. I suspect Teddy or Jim may have got the idea of using poles of rotation from this work. I suggest you ask Jim Everett.

QUESTION 9

I *take it that you overlapped with Parker and McKenzie at the Department of Geodesy and Geophysics. Is that correct? They also worked with Bullard. (In retrospect, Bullard had three pretty good students at about the same time!)*

I overlapped with many very able students at the Department of Geodesy and Geophysics at Cambridge. I inherited the heat-flow program from Clive Lister, who proposed hydrothermal circulation at the ridge axis to explain heat-flow variability. For one year, Chris Harrison, who was the first to show that the sedimentary record would reveal the geomagnetic time scale, overlapped with me. I went to sea with Tim Francis, a well-known marine seismologist, who is now chief technical officer for the Ocean Drilling Program. My year at the department included Fred Vine, with whom I spent three and a half months at sea, and Jim Everett. The year after me included Bob Parker and Dan McKenzie. To give you some idea of the quality of the students, the head TA when I TA'd practical physics at Cambridge was Brian Josephson, who was to win the Nobel Prize for the Josephson junction.

At the department Teddy Bullard supervised the theoreticians, such as Everett, Parker, and McKenzie; Maurice Hill supervised the experimentalists, such as Lister, Francis, Fred Vine, and myself. One of the great strengths of the department was that we all worked closely together, talked about our research at tea and coffee daily, and are still close friends.

Maurice Hill was important for my development because he did not let either Teddy or the other theoreticians intimidate experimentalists.

QUESTION 10

Did you go to the 1965 Royal Society meeting on continental drift which was organized by Blackett, Bullard, and Runcorn? If so, would you please describe what happened? What, if anything impressed you? And, any ensuing conversations you had with other graduate students about the meeting? (Runcorn told me that the reason Vine and Matthews were not asked to speak about their hypothesis is that he didn't know about it while organizing the meeting – as the junior of Blackett and Bullard, Runcorn actually did most of the organizing.)

Yes, I went to the meeting on continental drift. I knew about the paper by Vine and Matthews but somehow did not make the connection between their paper and continental drift. If my memory serves me correctly, the meeting was held in 1964. I took my then girlfriend to the meeting, and I remember her commenting on the poor quality of the presentations. Had Keith known of the importance of Vine and Matthews, I am sure he would have invited them, especially if P. M. S. Blackett and Teddy did not think too highly of their work. He loved to needle both of them.

The fact that Teddy did not invite either Fred Vine or Drum Matthews to talk reflects my memory of the situation at the department regarding their paper. It was considered highly speculative and worthy only of barroom gossip. It was only after Harry Hess and Tuzo Wilson spent time in the department in 1964 and 1965 that the importance of Vine and Matthews was realized.

QUESTION 10'

Both Hess and Wilson spent time at Cambridge in 1965. It was during this period that Wilson came up with his idea of transform faults, and he and Fred (and Hess with the Gorda Ridge) "found" the Juan de Fuca on the Mason-Raff survey. Did you discuss any of this with them? Did you get to know Hess or Wilson during this time?

Wilson and Hess were together at Cambridge in the winter of 1964-65.

In both years the heating in the place where I was living was poor. The department was centrally heated, and I used to spend Saturdays keeping warm and working. Both Harry Hess and Tuzo Wilson would work on Saturdays, so I spent much time with them.

My impression was that Tuzo Wilson had already sorted out most of his ideas on plate tectonics before he came to Cambridge, and he only spent the time there adding the finishing touches. Both Fred Vine and I spent time with Tuzo. In fact, I think Tuzo approached me in the library about the Vema-16 magnetic profile before he approached Fred. I remember copying the profile, enlarging it, and rotating one side and superimposing the other on it in an attempt to persuade Tuzo that the anomalies were not symmetric. This profile was later to be used by James (Jim) Heirtzler and others as the base profile for the magnetic time scale!

I discussed plate tectonics a lot with Tuzo. He never treated the idea defensively but more as a lot of fun. For his talk on the subject at Cambridge I took a globe, cut out the continents, and fitted South America against Europe and northern Russia. I placed this globe right behind him as he gave the talk.

In my own defense, I was still thinking as an experimental physicist and not as a physical geologist. I was confused by geological information and as a consequence did not appreciate people who tried to find the basic physical law behind all the noise. I much preferred to deal with hard experimental data and to try and interpret it within a very limited framework. Not until I started to work with Bill Menard and Dan McKenzie did I realize the limitations of such an approach.

QUESTION 11

When did you become familiar with Carey's ideas? Were they discussed while you were at Cambridge? What did you think of Carey's expanding earth view while you were an undergraduate? Any comments about Carey's continued espousal of earth expansion?

I was aware of Carey's ideas right from the beginning. Had Jim Everett not been given the opportunity to work with Teddy Bullard on the computer fit of the continents, Teddy

would have asked me. Teddy correctly believed Jim Everett was a better applied mathematician than I, and therefore asked Jim to work with him on the problem. Teddy discussed it openly one morning over coffee with the two of us. Neither of us had any idea how important the use of Euler's theorem in this paper would become. I consider the methodology employed by Teddy or Jim a major contribution. I do not know which of them knew about Euler's theorem. I suggest you ask Jim. Jim has definitely not gotten the credit he deserves for this paper.

QUESTION 12

When did you take your orals for your Ph.D.? Who was on your committee? Would you please discuss how your work on your Ph.D. led, if it did, to your eventual work on heat flow, relation between temperature of seafloor and depth of seafloor, relation between depth of seafloor and age of seafloor. I have not been through those papers, and I have no idea as to the various contributions by you, Dan, Bob Parker and others. If you could give me a start, I would appreciate it.

There are no orals for a Ph.D. in Cambridge, just an oral defense of a thesis. Maurice Hill was technically my advisor but he became ill, and Dai Davies, who had just graduated, did my final supervision. However, most of the advice for my thesis came from either Teddy Bullard or Chip Cox from Scripps. I gave a talk the day before my thesis defense. It went well, and my actual defense before Teddy Bullard and Dick Von Herzen took less than an hour.

I have answered some of the questions about depth and age and heat flow and age in my answer to your first question. I think of it as a great story because everybody who did anything got something badly wrong before doing something that was spectacularly correct.

QUESTION 13

You mentioned your tremendous respect for Drum Matthews, and said how he was involved in two major developments: the Vine-Matthews hypothesis and Drum's work in

the North Atlantic. Would you please give me a reference to Drum's work in the North Atlantic, and briefly explain its importance?

I have great respect for Drummond Matthews. It was he, alone at Cambridge, who realized the significance of examining the large magnetic anomalies across mid-ocean ridges. I remember seeing in his lab cutout profiles of every magnetic profile across a ridge axis that Drum could get his hands on. It was not by chance that he was involved in the seafloor spreading interpretation of these anomalies. Fred is very bright and numerically competent, but to my mind it was Drum who was the observational driving force behind the paper. After this point Fred teamed up with Tuzo Wilson, and then Harry Hess hired him at Princeton.

Drum's other major contribution has been to develop the concept of looking at deep reflections from the upper mantle using conventional multichannel seismic data. He developed the British Institute for Reflection Profiling Services (BIRPS) program at Cambridge that paid for and organized the collection of multichannel seismic data taken on cruises around the United Kingdom. Normal oil company data is taken only to 7-1/2 seconds. These data showed clear reflections from the Moho around Britain, as well as some reflections that are probably the trace at depth of major thrust sheets. These results have revolutionized how we look at the continents.

QUESTION 14

When did you arrive at Scripps? Did you go there directly after finishing up at Cambridge? Were you a postdoctoral student at Scripps or a junior professor?

I visited Scripps as a graduate student in 1964 for six weeks in November and December and worked with Chip Cox on a computer program to examine how variations in thermal conductivity affect surface heat flow. I returned to Scripps in December of 1965 as a postgraduate research assistant to work with Victor Vacquier on the Scripps heat-flow program. In 1967 I was promoted to a junior research position, associate research scientist, at Scripps. I wasn't particularly worried about my lowly position, because I was just 25 when I finished my Ph.D. I had been offered a position at Lamont by Joe Worzel, who had been on sabbatical at Cambridge in 1964, but I turned the position down in favor of Scripps. I was more interested in sun and the beach than in science at the time. I left

Scripps for MIT in 1972 when I was offered an associate professorship at MIT and Scripps was unwilling to offer me the assistant professorship that would have kept me. I returned to Scripps as a full professor last year.

QUESTION 15

What work were you involved with at Scripps?

I came to Scripps in 1965 to run the heat-flow program under Victor Vacquier. I spent three to four months at sea each year. We took measurements on old Pacific Ocean floor off the coast of Japan, around Hawaii, and near the Galápagos spreading center in 1966. Also Vic was involved with paleomagnetic measurements in sediments, magnetic induction work on land, and measuring the movement across the Gulf of California. I participated in all of these projects and helped write up many of them.

I loved to travel and to gather observational data at sea. Also I have a personal compulsion in feeling that I can't let go of anything until it is written up to my satisfaction. As a consequence, I got a tremendous amount accomplished and written up during these years.

The work in the Indian Ocean started through my association with Bob Fisher, who led many geological and geophysical expeditions at Scripps. He was planning a series of cruises to the Indian Ocean, and knew I would be willing to participate. By the time I was 30 I had been chief scientist on more than 10 expeditions.

QUESTIONS 16 and 17

Would you please recount how (a) you did not go to Woods Hole to heart Le Pichon's talk, (b) what you did (your going to the IUGG meeting, going to MIT, and being at sea)?

Could you please discuss your role in obtaining Morgan's paper? Were you at Morgan's first talk on plate tectonics at the spring 1967 AGU [American Geophysical Union] meeting? Did you give Menard a copy of the paper? You told me that Menard told you how he was reviewing Jason's paper for JGR. Would you please expand? Can you date when this occurred?

Though I think I went to AGU in 1967, I did not go to Morgan's talk. Jason was known to be an incomprehensible speaker – he totally confused everyone the first time he talked about the dynamic effect at trenches – and I was too busy worrying about my own talk, the subject of which I cannot remember.

If I did go to AGU in April, immediately thereafter I left to go to sea on two legs of Scripps's NOVA Expedition. I joined the ship in Hawaii in June; Harmon Craig was chief scientist. We examined the chemistry of the bottom water on a north-south section across the equator close to 180°E. In addition, Jean Francheteau and I surveyed two seamounts to examine the possible northward shift of the Pacific, and I took a series of heat-flow measurements.

When we arrived in Suva, Fiji, at the end of the cruise, I left R/V *Argo* to join Menard on R/V *Horizon*. Teddy Bullard was on the cruise at the time. Bill devoted the entire cruise to surveying the topography of the Fiji Plateau and to heat-flow measurements. The plateau had a shallow depth of around 3000 m, and the heat flow was high. Bill and I knew we were onto something, and his infectious enthusiasm carried me along. As I mentioned earlier, we had a completed manuscript by the end of the cruise.

On my way back from Fiji, I stayed with my wife-to-be and her parents in Hawaii for the month of August. I then roomed with Dan McKenzie at Scripps, because the house I rented in Del Mar with Bill Farrell and Bob Parker had to be given up for the summer. It is at this point that I started to really think about seafloor spreading and plate tectonics.

I caught the bug of scientific discovery with Bill Menard. It was also obvious to me that my personal life was going to change, as my relationship with Freddie was rapidly deepening. I finally had both the enthusiasm and the mental space to tackle serious science. Having the energy had never been a problem for me.

I had started to work earlier in the year on some heat-flow data and topographic profiles I had collected in the previous year across the Galápagos spreading center. Dan, after the 1967 paper in which he tried to use a plate model to account for the relation between heat flow and age, was interested in applying the same approach to the topography. We forgot the effect of the water, but we produced a useful initial paper which compared both the heat flow and topography across the Galápagos spreading center with the predictions from the

plate model. I wrote up this work, made four or five slides, and set off for the IUGG in Switzerland.

Before I left, Dan and I had some talks about the application of the simple thermal model he had developed in reply to the Langseth et al. 1966 paper. Just before I left for the IUGG – possibly the morning I left – Dan was rereading Bullard, Everett, and Smith, 1965. He mentioned to me that he had seen the light and that if Wilson's theory of plate tectonics was correct, then the perpendiculars to the slip vectors from the first motion studies on earthquakes should determine a unique pole of rotation. I was not very interested in what he was saying as my romance with my wife-to-be was progressing at a great rate, and I was much more concerned about where this relationship was going than about science.

Gene Simmons was the organizer of the symposium at which I talked at the IUGG. He asked me to return via MIT and to give a talk there. I repeated my AGU talk; it was not well received at MIT. One of my major critics, I believe, was Norman Sleep.

I do not remember going to Woods Hole on this trip, though I well could have. Even if I did, I am reasonably sure I did not go to Xavier Le Pichon's talk, and I know I didn't discuss it with Dan when I returned. By this time I had determined that I was very much in love, and my only interest was drumming up the courage to propose marriage to Freddie, my wife-to-be. I don't think a scientific thought crossed my mind at this time. Freddie picked me up at the airport, and I proposed to her in front of the garbage cans at the back of the house before I went in to see Dan. Freddie told me, in retrospect, that she had been determined to keep me out all night to get the proposal, because she knew that as soon as I got inside the house with Dan we would do nothing but talk science. As I told Dan about my trip and asked where he was scientifically, I became impressed by how much he had done in the two weeks I had been away. I was starting to see that, when formulated quantitatively, the theory really could explain both the direction of the slip vectors and the direction of faults such as the San Andreas. However, my concern was not with what Dan was doing but with the criticism the people at MIT had given me on the model I was using to account for the heat flow and depth profiles across the Galápagos spreading center.

If my memory serves me correctly, Dan did not know that Jason Morgan was working on the same ideas before I left for the IUGG. However, sometime after I returned from AGU, he had found out that Morgan was working on the same ideas. Also it is my recollection that Bill Menard gave me a copy of Morgan's paper to take to Dan sometime after Bill

returned from sea in mid-September of 1967 and I returned from AGU. If this is correct, then it was clear that Dan did not hear from me about Morgan's work probably until mid-October, well after he had started on these ideas on his own. My impression is that Dan declined to review Jason's paper when I carried it to him from Menard because he was working on the same idea.

As I have mentioned previously, in late November or early December, Dan gave a seminar at IGPP at Scripps on his paper with Bob Parker. Jean Francheteau, who participated on most of the legs of the NOVA expedition, had returned by then. He and I were most impressed by Dan's talk and wanted to apply the idea to the world. In January we found out that Morgan's paper was already in JGR and that Xavier Le Pichon was well into what we wanted to do.

After the presentation of Dan's ideas and the publication of the Morgan and Le Pichon papers, nobody at Scripps could talk of anything else but plate tectonics. Tanya Atwater, Bill Menard, Jean Francheteau, Dan Karig, myself, Bob Parker and a bunch of others were all involved in trying to see how we could best apply the concept to the data we had been working on individually.

QUESTION 18

Bill Menard, in his Ocean of Truth, has said about you, his reviewing of Jason's paper, and/or your relation with Dan the following:

> *Jason Morgan sent me a preprint of his manuscript in its early draft, probably in the late spring of 1967. I believe I also received the paper for an editor. In any event the manuscript certainly circulated among my students, and we discussed it. The original draft, however, was difficult for me to fathom, and it did not have the impact of the final publication. Moreover, I had other things on my mind. I spent July through September on the NOVA expedition in the southwestern Pacific. Teddy Bullard was among the old hands to participate, and there were many students including those who knew about Morgan's paper. (p. 290-291)*
>
> *John Sclater was at sea on NOVA with me until 12 September, and he was out again from 29 October to 19 November. Meanwhile, two of the five geophysicists*

who received doctorates at Cambridge in 1967 had joined Scripps. Bob Parker was on the permanent staff, and Dan McKenzie was a visitor for six months beginning in June. Dan and John shared an apartment a few blocks from Scripps when John was ashore. (p. 291)

In November, John Sclater recalls

"Dan had just received a tough review of one of his theoretical papers on the internal structure of the earth. As light relaxation he started thinking about plates and in two days produced the North Pacific plate tectonics paper." (p. 292)

(This passage is from a letter you sent Cox in 1972.)

In light of these passages and the previous question, could you develop your own chronology and deal with the following questions. I add that the McKenzie-Parker paper was received by Nature on Nov. 14, 1967, and I believe you told me that Dan gave a talk at Scripps in October.

In your chronology would you discuss:

(a) Whether Dan talked to you about his own developing version of plate tectonics. Did you speak to him about Jason's ideas? More importantly, if you did talk about his and Jason's ideas, can you remember what you spoke about? Did you understand what he was talking about? Did you discuss any of these ideas with Bill Menard, Teddy Bullard, Bob Parker and Tanya Atwater? Did you have a number of discussions with Dan, and if so, can you recall anything about the development of his ideas?

(b) Can you recall Dan's talking about his ideas, before the Morgan paper arrived? Dan has said that he came up with the idea in June after he had arrived at Scripps.

(c) I've asked Dan what paper it was that he had received a tough review about. Do you know?

(d) Can you recall the reaction to Dan's talk in October? Was Teddy at the talk?

(e) Do you know when Parker got involved? Would you discus his contribution and its importance. (I spoke with Parker yesterday, and I'm sending him a set of questions.)

(a) I hope my own chronology has answered these questions. I had a series of talks with Dan about plate tectonics both before and after my trip to the IUGG, but they were all one-sided because I didn't really understand what he had done. I cannot remember discussing any of these ideas on the NOVA trip or before going to the IUGG. However, after Dan's talk in December at Scripps, they were all we could discuss.

(b) My recollection was that Dan came up with the idea in August or September when I was rooming with him. However, we had a house together for June through December and I didn't leave for sea until mid-June. So he could have had the idea before I left for sea in mid-June, but I don't remember it that way. I remember the development as very fast and occurring around the time of my trip to the IUGG in August or September of 1967.

(c) I don't know which paper – just that the change in Dan from frustration with a review with which he did not agree to elation over a new idea that seemed to work was so obvious.

(d) Teddy was not at Dan's talk. All I can remember is the reaction of Jean and myself. We saw science history being made, we understood what was happening, but the two of us had contributed nothing. We were both excited and depressed.

(e) Bob Parker got involved as Dan was discussing his ideas. Bob is a very clear thinker, and I am sure he helped clarify Dan's thinking. However, once he became involved, he worked hard on the paper to improve the presentation and make the writing more lucid. (I have heard both of them say that they computed the first pole using a globe and a spherical template. This was surprisingly crude for two very quantitative physicists.) Bob's major independent contribution came much later in his paper with Doug Oldenburg presenting the simple thermal boundary layer model for the relations between depth and age and heat flow and age of the ocean floor.

QUESTION 19a

Would you please discuss the difference in the approach and papers by Dan and Jason.

Dan's approach used the slip vectors of earthquakes around Alaska and the direction of the San Andreas Fault to show that Pacific/North America motion would be represented as a rotation about a single pole in Hudson's Bay. It was not easy to understand unless you were a seismologist who understood projection of slip vectors onto the horizontal.

Jason's paper was much simpler for geologists to understand. He examined active transform faults that showed up as fracture zones, the slip vectors for earthquakes on these fracture zones, and magnetic anomaly information. He chose the Atlantic and the motion between the American and African plates. His approach was developed more slowly than Dan's and was more easily understood by geologists. (I suspect this was the result of Menard's review.) Jason presented clear step-by-step diagrams. I use Jason's paper and approach in class when presenting the quantitative aspects of plate tectonics.

It is interesting to note that Jason does not give very good talks, yet his papers are masterpieces of clarity. He wrote a very good one on depth and age in a volume on plate tectonics and petroleum that clarified for me the theoretical difference between the boundary layer and plate model for the cooling of the oceanic lithosphere.

QUESTION 19b

Would you please discuss the importance of LePichon's contribution with his seafloor spreading and continental drift.

Once McKenzie and Parker (1967) and Morgan (1968) had presented the ideas, the next question was, Did they work globally? Xavier's paper demonstrated how well the concept worked on a global scale. The three papers, taken together with that of Wilson (1965), are a monumental contribution to improving our understanding of how the earth works.

QUESTION 19c

Would you please discuss the importance of the Isacks, Oliver, and Sykes article, "Seismology and New Global Tectonics." How much of that article was already in Sykes's JGR article, "Mechanism of Earthquakes and Nature of Faulting on the Mid-Oceanic Ridges" (April 1967) and Oliver and Isacks's "Deep Earthquake Zones, Anomalous Structures in the Upper Mantle, and the Lithosphere" (August 1967).

It is my impression that most of the ideas of Isacks, Oliver, and Sykes are in the Sykes 1966 article. However, what was important about this paper was that the earthquake data were compatible globally with the creation, destruction, and motion past each other of lithosphere plates about 100 km thick.

QUESTION 20

Would you please describe when and why you decided that seafloor spreading (Vine-Matthews and Wilson's idea of transform faults) was probably correct? You mentioned how it had to do with some of your own work. Would you please describe what reservations you had until you saw that it (or was plate tectonics itself?) nicely explained your own data?

In late 1966, I participated with Scripp's Art Raff in a suite of north-south lines run over the east-west Galápagos spreading center. We obtained symmetric magnetic anomalies on either side of a very large central magnetic anomaly. This cruise convinced me that the ocean floor had to be created by the seafloor spreading process.

My reservations prior to this were based on the criticism of Fred Vine's work by his supervisor Maurice Hill and my own reluctance to accept something I hadn't done myself. However, after the cruise with Raff and discussions with Dan McKenzie, who spent the early part of 1967 at Caltech, I was totally convinced about the value of plate tectonics before I ever started to analyze the heat-flow data on a global scale.

QUESTION 21

Would you please elaborate upon Bullard's contributions in the overall controversy over continental drift, the development of the Vine-Matthews hypothesis, his role, if any, in the development of plate tectonics, and his importance at Cambridge while you were there?

I trust that my previous comments have elaborated on Bullard's role. He was truly seminal because he was a very competent physicist who was willing to apply himself to answering messy geological problems. Teddy always had a clear idea of what was important. Also, he had very high intellectual standards and required the same from his students. Finally, it never seemed to occur to either him or Maurice Hill that we students were not as bright or as deserving to be heard as they were. In retrospect, the interactions were electrifying.

I think that the Bullard, Everett, and Smith paper was very important in that it demonstrated the application of Euler's theorem to the globe. It was seminal in Dan McKenzie's quantitative development of plate tectonics with Bob Parker.

QUESTION 22

What have I missed asking you about this relevant to the development of Dan's ideas (and Jason's for the matter)?

I have tried to fill in as much as I could in answer to your questions. I have been quite elaborate and hope I covered everything.

QUESTION 23

I am enclosing Le Pichon's "The Birth of Plate Tectonics," some of my reprints to give you an idea about what I do, and a draft of a paper I've done on the development of plate tectonics by Jason and Dan. The draft is based upon a brief interview with Morgan and some correspondence with McKenzie. However, their published articles served as the major basis for the paper. I suspect that the paper will be somewhat boring for you. Van Andel suggested I publish it in TERRA. Regardless of whether I do, it will serve as the beginning of a much more extensive account, an account in which I'll integrate your

answers, and it will serve as the last historical chapter of a book I'm finishing on the controversy over continental drift. Once the book is finished, I would like to turn to the way in which plate tectonics spread to different areas of the earth sciences, and to what extent it has changed the earth sciences.

Anyway, I would appreciate any comments you might have about the preprint as well as any of the other articles.

My memory of what happened is not the same as Xavier's. I do not remember attending the talk he gave in Woods Hole, though I could well have been visiting Woods Hole when I went to MIT after the IUGG. I preferred visiting Woods Hole to MIT when I went to the Northeast, but I always stayed in Boston, where I had relatives. At the time, I was much more interested in whether or not my wife-to-be would accept the proposal of marriage that I planned to make as soon as I returned, than in science. I certainly did not want to distract myself with science when I had such a major decision on my mind.

It is my impression that Dan developed his ideas totally independent of Jason Morgan. However, he started to work on them seriously in August or September of 1967. On the other hand, Jason started much earlier in the same year. By a quirk of fate (Bill Menard's long trip to sea on the NOVA expedition), Jason's paper was held up. However, I suspect that as a result of Menard's review the published version of Jason's paper was much improved over the original.

Your article I found very clear, especially the science. As you can see from my replies to your questions I am sure that Dan and Jason developed the quantitative aspects of the theory of plate tectonics independent of each other.

One last point – on rereading my answers to your questions, it seemed that the development of the heat flow/age and depth/age relations would make a good story. The relations lead to (a) the discovery of black smokers at the spreading centers, (b) our realizing that the surface of the earth, the plates, are the cooling portion of a thermal convecting system, (c) a framework for understanding how continental shelves and basins are formed and the development of a new quantitative method in the research for hydrocarbons, and (4) a realization that Lord Kelvin got the age of the earth wrong not because he did not know about radioactivity, which is the conventional wisdom, but because he ignored the possible effects of convection, about which he should have known.

The relations and concepts developed quickly but in a very haphazard way, with everyone making a mistake before we all finally understood what was going on. It would make good history, especially as the biggest errors were made by Le Pichon, McKenzie and myself! One of the good things about a fast-moving field in science is that it moves so fast that there is no time for anyone to remember the errors.

DRAFT FINAL 7/19/92

Terrestrial heat flow and plate tectonics

by

John G. Sclater

Geological Research Division - 0215, Scripps Institution of Oceanography

University of California, San Diego, La Jolla, Ca 92037-0215

and

Jean Francheteau

Institute Physique du Globe, Universitee de Pierre and Marie Curie,

Paris 6 and 7, 4 Place Jussieu, 75230 Paris, France

Bibliographic Reference

Sclater J.G., and J.Francheteau, Implications of terrestrial heat flow observations on current tectonic and geochemical models of the crust and upper mantle of the earth, Geophys. J. R. Astron. Soc., 20, 509-542, 1970.

Abstract

The heat flow through both the continents and oceans decreases with age, though at a different rate, to a roughly constant value. These observations are compatible with the theory of plate tectonics. A single model of the oceanic lithosphere accounts for the decrease in heat flow and the subsidence of the ocean crust with increasing age.

Text

We wrote this paper when one of us (JS) was a post doctoral research scientist and the other (JF)was a graduate student at the Scripps Institution of Oceanography (SIO). We both studied under Victor Vacquier who ran the the SIO program to measure on a world wide basis the flow of heat through the floor of the oceans.

SIO was an exciting place to be in the late 60's with research time available for those willing to go to sea. We were both more than willing to go join the ships in exotic ports to participate in a program of world wide measurements. When not taking measurements, we spent the long days at sea trying to understand their meaning. One of us (JS) had been a graduate student at Cambridge in 1964 when Tuzo Wilson first presented the basic concept of plate tectonics[1]. However, the theory was controversial and, initially, we did not see how to apply it. In the fall of 1967, Dan McKenzie and Bob Parker, who were both at SIO, quantified the concept[2]. Immediately we realized how to tackle our observations. No one had systematically analyzed the heat flow data from a plate teconic framework. On the contrary, opponents had used the heat flow data to raise two objections to this theory. They argued, first, that the equality between the oceanic and continental means was difficult to reconcile with the concept of moving plates and, second, that no one had constructed a self consistent model that could explain both the subsidence of the ocean floor and the decrease in heat flow with age.

Using a technique initially applied by two Russian scientists[3], we showed that the important factor in the heat flow was not the equality of the means but the

decrease with age shown by both the oceanic and continental values. Further, we found that the oceanic values decreased away from a spreading center as predicted by the theory of plate tectonics[4]. By ignoring the lowermost values, most of which occured near the spreading centers, we found that this decrease lay close to that predicted by a model which also accounted for the subsidence of the ocean floor.

We believe that our paper has been widely cited because we showed that (a) the equality of the mean heat flows was not a significant physical parameter, (b) the heat flow through both continents and oceans was compatible with plate tectonics and (c) a single model explained both the subsidence of the ocean floor and the decrease of heat flow with age. Later, Clive Lister, of the University of Washington, demonstrated the existence of hydrothermal circulation in the porous basalt near the ridge axis which justified our removing the low values from the heat flow analysis[5].

Plate tectonics originally started as a kinematic explanation of how plates moved. Our analysis added a dynamic component by showing that the plates were really the rigid thermal boundary layer on top of a convecting upper mantle. Further work showed that the creation of this layer was the dominant way in which the earth loses heat[6]. Lately, this realization has led to a reevaluation of the approach Lord Kelvin took to determining the age of the earth. The conventional wisdom is that he was wrong by two orders of magnitude because he ignored radioactivity which had not yet been discovered. This is not correct. His mistake was to ignore the possible effects of heat loss by convection about which he should have been aware. However, even though he was off by two orders of magnitude for the age of the oldest

continents, Kelvin's approach and his insistence that the age was finite and measurable, forms the basis of all current geophysical fluid dynamic models of the earth[7].

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CURRICULUM VITAE

Updated 10/21/92

NAME: John G. Sclater

CITIZENSHIP: U.S., naturalized Sept. 22, 1986, Austin, Texas

ADDRESS: 10869 Portobelo Drive
San Diego, California 92124

PHONE: Office (619) 534-8653
Home (619) 467-1123

BIRTH DATE: June 17, 1940, Edinburgh, Scotland

EDUCATION: B.Sc. Physics, Edinburgh University, U.K., 1962
Ph.D. Geophysics, Cambridge University, U.K., 1966

PROFESSIONAL EXPERIENCE:

1965 - 1967. Postgraduate Research Assistant , Scripps Institution of Oceanography

1967 - 1972. Assistant Research Geophysicist, Scripps Institution of Oceanography

1972 - 1977. Associate Professor, Massachusetts Institute of Technology

1977 - 1983. Professor, Massachusetts Institute of Technology

1981 - 1983. MIT Director, Joint Program in Oceanography and Ocean Engineering with Woods Hole Oceanographic Institution

1983 - 1991. Associate Director, Sr. Research Scientist, the Institute for Geophysics, and Professor, Holder of the Shell Distinguished Chair in Geophysics, both at the University of Texas at Austin

1991 - present. Professor of Geophysics, Scripps Institution of Oceanography, University of California, San Diego

FAMILY STATUS: Married to Naila G. Sclater
Four sons (Iain, Fabio, Stuart and Felipe)

SCIENTIFIC EXPEDITIONS: Chief Scientist for numerous oceanographic scientific expeditions

EXPERIENCE & RESEARCH INTERESTS: Original experience was with marine geophysical data, principally taking heat-flow measurements at sea. Established a simple relation between heat flow, subsidence, and age for the ocean crust and showed that it could be accounted for by simple plate cooling models. In the immediate past, investigated the application of simple extensional models to the tectonic history of continental basins and shelves. Specifically these studies involve examining the subsidence of, the heat flow through, and the thermal maturation history of the sediments on the shelves. A problem of particular interest was the relation between the throw on the

faults observed on seismic sections across extensional basins and the amount of extension necessary to account for the subsidence, heat flow, and maturation of these sediments. All these studies involved interpretation of multichannel seismic sections. Areas of current interest include modelling of deformation by block faulting, the application of geoid anomalies to determine the tectonic history of the ocean floor, and the use of heat-flow measurements in the search for hydrocarbons on the continental shelf of the northern Gulf of Mexico.

Associate Editor: *Journal of Geophysical Research*, 1971-1974.

Member: Ocean Sciences Committee of the U.S. National Academy of Sciences, 1972-1976.

Ocean Studies Board of the U. S. National Academy of Sciences, 1985 - present; Chairman, 1988-1991.

Ocean Studies Board/Navy Panel, 1985 - present.

National Science Review Committee on Oceanography, 1974-1977.

Review Committee, International Decade of Ocean Exploration (IDOE), National Science Foundation, 1974.

International Phase of Ocean Drilling (IPOD): Heat Flow Panel, 1968-1974; Indian Ocean Panel, 1968-1974; Ocean Crustal Panel, 1974-1976.

Ocean Drilling Program: Lithosphere Panel, 1984-1986; Indian Ocean Panel, 1985-1988.

Committee on Engineering Considerations for Continuation of Deep Sea Drilling for Scientific Purposes Marine Board; Assembly of Engineering, 1977-1979.

Testified before Congressional Subcommittee on Science and Technology on the 1980/1981 National Science budget.

Office of Technology Assessment of the U.S. Congress Panel to Evaluate the Ocean Margin Drilling Program, 1980.

Courses Taught: MIT-12.01 Rocks, Structure and History, the incoming undergraduate earth sciences requirement (with J. Southard).

MIT-12.02 An Introduction to Marine Geophysics, senior requirement and graduate-level course (with S. Solomon).

MIT-12.56 Advanced Seminar in Plate Tectonics, graduate-level course.

UT- GEO385D - Geodynamics: Plate Tectonics and Marine Geophysics.

UT- GEO 391 - Advanced seminar in marine geophysics.

UT- GEO 312K - Geology for Engineers.

Students Supervised:	Ph.D. theses supervised and supported at UCSD: J. Francheteau, R. R. N. Anderson, and L. Lawver (with V. Vacquier); and R. Jarrard (with E. L. Winterer).
	Ph.D. theses supervised and supported at MIT: J. Pierce, K. Loudon, I. Norton (graduated through University of Witwatersrand, South Africa), C. Tapscott, J. Crowe, C. Jaupart, L. Royden, D. Sawyer, and B. Célérier.
	M.Sc. theses supervised at MIT: J. Nabalek, A.Shaughnessy, D. Goldberg, D. Galston, and J. Henderson.
	Ph.D. students supervised at UT: J. Dunbar (with D. Sawyer), S. Nagihara, D. Müller, and Tung-yi Lee (with L. Lawver).
	M.Sc. theses supervised at UT Austin: M. Renkin, S. Kautz, J. Giltner, M. Shorey, C. Mayes, L. Gahagan, M. Albertin, G. Zemlicka, S. Boerner, and L. Beckley.
Professional Societies:	Fellow, Geological Society of America
	Fellow, American Geophysical Union
	American Association of Petroleum Geologists
Awards:	Swiney Lecturer, Edinburgh University, 1975-1976
	Rosenstiel Award, 1979
	Shell Distinguished Professorship, 1983-1988
	S. Thomas Crough Memorial Lecturer, 1985
	Bucher Medal, American Geophysical Union, 1985
	American Association of Petroleum Geologists Distinguished Lecturer, 1987-1988
National Honors:	Fellow, The Royal Society, London, U.K.
	Member, National Academy of Sciences
Referees:	Dr. A. W. Bally, Geology Department, Rice University
	Dr. F. Press, President, National Academy of Sciences
	Dr. A. Maxwell, Director, The Institute for Geophysics, the University of Texas at Austin
	Dr. D. P. McKenzie, Department of Earth Sciences, Cambridge University, U.K.
	Dr. E. Frieman, Director, Scripps Institution of Oceanography, La Jolla, California

JOHN G. SCLATER
Publications

1. 1965 Sclater, J. G., Heat flux through the ocean floor, Ph.D. dissertation, Fitzwilliam House, Cambridge.

2. 1966 Sclater, J. G., Heat flow in the northwest Indian Ocean and Red Sea, Phil. Trans. Roy. Soc. London, 259, 240-261.

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