Shocks and Rocks

Seismology in the Plate Tectonics Revolution

The story of earthquakes and the great earth science revolution of the 1960s

by

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PREFACE

During the decade of the 1960s, science of the solid earth underwent an astonishing and awesome upheaval. In just a few years, geoscientists constructed a new way of describing and understanding the dynamics of ever-changing earth, past and present, and so found a route to explanation for how most, if not all, of the great features of the earth's surface that have harbored and plagued and enchanted humans throughout their existence came to be. Continents, ocean basins, mountain ranges, deep sea trenches, earthquakes, and volcanoes suddenly became explicable as consequences of earth movements that, on a global scale, have a remarkably simple and readily understandable pattern. The long-sought key to the ponderous and agonizingly slow movements of earth that, over millennia, have deftly shaped our surroundings was found during that decade, or so most scientists think today, more than a quarter of a century later.

For those scientists studying the earth, and a few skeptics aside, the 1960s were a time of astonishment and discovery, of delight and euphoria, of pride, and of stimulation reinforced by success after success. Word of the achievements spread quickly throughout science and beyond. The upheaval would be called "the plate tectonics revolution," "plate tectonics" being the term invented to describe the essence of the new dynamics. It has been claimed by some that the discovery of plate tectonics is the greatest achievement ever of modern earth science, although those who hold brief in this regard for one or two other outstanding revelations such as recognition of the great age of the earth or the postulating of the theory of evolution might challenge that statement.

In any case, the achievements of the 1960s were outstanding and their story seems to merit telling, not once from the singular perspective of a particular science historian, but multiply from the diverse perspectives of scientists and others who witnessed and/or participated in that revolution. This book is one such attempt. It focuses on the many critical contributions of the field of seismology to the new dynamics during the early stages of the revolution and it is written not by an historian but by a participant. As such, it should complement the now numerous other histories of the discovery of plate tectonics, most of which report, but none of which is solely devoted to, the role of seismology.

In an effort to keep the text flowing smoothly, and to hold its length in check, I cite only key references and discuss non-seismological topics in rather generalized fashion. For those who would like additional reading on the subject as seen from a perspective different from mine or with emphasis different from mine, or who would like a more complete set of references, I include at the end of this section a list of some of the available literature. It is a measure of the breadth of interest in the subject to note that the following categories are represented in the list of authors: science writer, historian of science, philosopher of science, sociologist of science, earth scientist, non-earth scientist, participant in the revolution, and non-participant in the revolution. Their views of the revolution and their purposes are sometimes common, sometimes diverse, but they all share recognition and appreciation of a major happening in science. Some 25 years after the fact, it should be clear and obvious to the reader that the prime purpose of this book is not to add glory to the revolution and its participants, but rather to describe just how a science works when things are going well, in the hope of accelerating the pace toward great discoveries of the future.

As is the case in most, if not all, scientific revolutions, or paradigm shifts to use modern terminology, the essence of plate tectonics and the characteristics that distinguish it from previous theories on the nature of earth dynamics are remarkably simple. In contrast to the static earth models that dominated earlier geologic thinking, plate tectonics calls for horizontal movements of rock masses at or near the earth's surface through huge distances (perhaps thousands or tens of thousands of kilometers) over long intervals of time (perhaps tens or hundreds of millions of years). The corresponding speeds of a few cm/yr for a land mass such as a continent, although extremely slow in normal day to day context, are sufficiently high geologically speaking to startle those accustomed to thinking of rocks or land masses as immobile and fixed in global perspective.

The coming of plate tectonics, then, corresponded to a shift in emphasis of geologic thought from what has been termed the "fixist" school in which rocks are thought to remain throughout their existence near the place where they were formed, to the "mobilist" school in which rocks are thought to move laterally and geologically rapidly through significant fractions of earth's circumference. In this sense, plate tectonics is like its earlier but never widely accepted forerunner, the concept of continental drift. Plate tectonics is built upon, and incorporates some of the basic elements of, continental drift, but differs fundamentally from early or orthodox continental drift theory in that the prime entities that move as units are not defined by the borders of the continents but are the mostly larger and usually less obvious segments of the earth's surface that have come to be known as the "plates."

Taken together, fewer than a dozen large plates form a mosaic that covers virtually the entire surface of the earth. Obvious earth features such as continents, oceans, islands and mountains are, in the very simplest perspective, not the prime elements in the dynamic model; it is the less readily apparent plates that are the basic units, and it is their movements that can be described in simple geometrical fashion. The lesser units suffer more complex and intricate secondary effects that make the story of earth dynamics more comprehensive, more varied, more interesting, and often more confounding, but those effects now fall into place and can be understood as consequences of the simple primary motions of the basic plates.

The effects of the ponderous motions are awesome. Driven by heat escaping from the earth's deep interior, the elements of the mosaic, i.e. the plates, are continually in motion and continually modifying their shapes through addition of new surface material at some segments of plate boundaries and destruction of old at others. Huge land masses riding atop the plates may travel from one hemisphere to another, or from the tropics to the arctic, and may be split apart, or sheared, or deformed and piled atop one another in collision, thereby providing a setting for a variety of lesser geologic processes that build the features of rocks, terrains, and landscapes that humans find familiar and beautiful and essential to support of civilization. That so simple a basic model of earth was found to account for surroundings so complex and so perplexing as those we find in the real earth is a striking example of success in science through synthesis, and also of the beauty of explanation through simplicity that characterizes great advances in science and that merits attention well beyond the inner circles of earth science.

In fact, and as is the case in most revolutions in thought, the beautiful simplicity of the plate tectonics concept is a source not only of pride and elation for the earth science community, but also of mild embarrassment and consequent humility. Why, indeed, didn't we, and couldn't we, earth scientists arrive at that simple solution earlier? How had we misdirected and immobilized our thinking so that something that in retrospect seems so obvious remained hidden for so long? What might we have done to make this revolution occur at an earlier time and in less abrupt and more predictable fashion? Such questions suggest that some introspection on just how the new order came about is appropriate and may well be instructive.

Well, how, indeed, did we manage to achieve this new level of understanding of earth? Was such discovery the product of genius, a consequence of a flash of inspiration in an exceptional mind secluded and isolated from the mainstream of geologic thought which, it would turn out, was in need of reorientation? Well, not exactly. The plate tectonics revolution had its share of clever and inspired ideas by innovative and creative individuals of course, and I do not wish to detract from deserved credit to the scientists who had those ideas. But something as grandiose and as far-reaching as the plate tectonics revolution is rooted in backgrounds of knowledge and prolonged series of major and minor events that go well beyond the brilliance of a few individuals and the isolated incidents that come to be known in history as "the discoveries". For such a broadly-based and far-reaching advance to be achieved, there first had to come a long, difficult and agonizing period of sweat and toil, of clever strategic and tactical thinking, of accumulation of observation, and of failed attempts at synthesis that would set the stage for the climactic and successful developments to follow. This book therefore is not solely about the role of seismology during the plate tectonics revolution of the 1960s, for it also touches on and summarizes events before, and to a lesser degree after, that decade.

The writing of the book was stimulated by a request in 1993 from the Committee on History of the American Geophysical Union for a monograph on this subject. I presume that I was chosen for this task at least partly because I am an author, together with Bryan Isacks and Lynn Sykes, of a paper entitled "Seismology and the New Global Tectonics" published in the AGU's *Journal of Geophysical Research*, 15 September 1968. That paper, which I shall call the NGT paper, was timely, widely-read and hence very well-known at that time. Perhaps more than any other, it brought the seismological component into, and in the process made fundamental contributions to, the nascent subject of plate tectonics. It attempted a comprehensive review of all available evidence from the field of earthquake seismology that bore on the then new and rapidly developing topic of what is now known as "plate tectonics," but that term was not in use at the time, and we referred to the topic as "the new global tectonics."

The NGT paper contributed some important fundamental and new ideas to what would become the plate tectonics concept, cited abundant supporting observations, and communicated the information reasonably well through figures and a text that minimized seismological jargon. The paper thus became influential, partly among those who were already caught up in the early phases of the plate tectonics euphoria from their studies of geomagnetism or other phenomena and who hence appreciated and welcomed confirming and supplementary evidence from seismology, and partly among those in other specialties of earth science who were in the process of becoming acquainted with the concept of plate tectonics for the first time.

The NGT paper was described by some as a milestone, bringing seismology firmly into the subject of plate tectonics, and plate tectonics firmly into seismology for what, it seemed then and still seems at present, was for once and for all. Among other things, that popular paper left its three authors labeled, perhaps over-generously, as authorities in the field. This monograph is therefore partly about the events that led up to and resulted in the NGT paper, including earlier publications on related subjects by the three authors.

The NGT paper in original form is included as an appendix. References to it in the text are in the form: (NGT, Fig. n).

The history of seismology and plate tectonics, however, is hardly the story of one particular paper, no matter how timely or well-received. Science is more than just a few isolated events that happen fortuitously at the right time and place. Other seismological papers of various types and about the same or earlier vintage also contributed information that would be important to the development of the concept of plate tectonics. Furthermore, and of tremendous yet often overlooked importance, for many decades prior to the 1960s seismologists engaged in activities that provided a background of information on the earth and its earthquakes that was critical to the development of models of the dynamic earth and its moving surficial plates. I hope this book is as much a tribute to them as it is to those of us who were fortunate enough to be in the right place at the right time when seismology offered the opportunity for contributions to the revolution. And, furthermore, in the period since the revelation of the '60s, seismologists have continued to contribute to the refinement of those early models as they labor now in a seismological world with plate tectonics as the ruling paradigm.

In this monograph, therefore, I try to sketch and trace certain streams of seismological efforts, and other activities of related nature, through an interval of history much longer than that decade of the '60s. Sometimes the story begins well back into the 19th century, sometimes even earlier. Those early activities, it would turn out, led to a level of understanding of the earth's interior and its processes that provided a fertile foundation for the exciting new ideas about the dynamics of the earth that arose in the 1960s.

I also discuss the events of the '60s in some detail of course, and summarize and comment briefly upon post 1960s research on this subject. With the advantage and clairvoyance of the historical perspective, I draw attention not only to the triumphs of seismology but also to oversights and missed opportunities along the way. My purpose in doing so is not to denigrate in any way the efforts of seismologists of the past, for I admire and respect all of them. Rather I cite what now seem obvious fumblings or oversights of the past in order to stimulate rising young scientists of today to think and reason in ways that will help them avoid such missteps in the future. Any history, it seems, should have enlightenment, encouragement, and assistance for succeeding generations as a prime goal, and this book tries to do its share on this score.

As the reader will quickly recognize, this book is based on my personal perspective of the events and activities of the time. It is not a scholarly tome based on endless library research and including innumerable citations. It has factual material, opinion and selected anecdotes. In places it seems autobiographical. A history written by, and based on the personal perspective of, a participant or observer will, of course, nearly always result in a subjective and somewhat biased view of the historical events. At least, however, in this type of history it is obvious to the reader that such bias is likely. The other extreme of perspective, that of the historian who comes along later to produce a scholarly, carefully-researched, and annotated record of what happened may seem more objective at first glance. However, my experience with such histories when they involve events that I know from personal association suggests that they always end up with a view of what happened that is at least as distorted as the various views of the participants. Try as we might, in other words, and regardless of our backgrounds, we never manage the perfect and complete history of anything.

For whatever it is worth then, what follows is my current view, based on memory, long past and recent discussion with many colleagues, and some literature search, on what happened as seismology joined with other earth science disciplines to produce the concept of plate tectonics, a concept that some have described as the greatest advance ever in human understanding of the earth. For those who experienced the revolution and who may feel that I have not always reported things as they remember them, I can only respond that (a) I have tried to give as fair and accurate a depiction as I can, subject to limitations of space and memory, and (b) I encourage them to make different views known as appropriate.

My purpose in writing this book is, of course, to tell the story of seismology's role in the coming of plate tectonics, at least as I saw it, for all who care to read it. I hope that the audience will include not only seismologists and scientists in related disciplines, but also readers, scientist or non-scientist, with a general interest in how one branch of science operated to produce one of those anomalous periods when the ups overwhelmed the downs for a time and things were going very well indeed. To facilitate reading by those with limited familiarity with seismology, I have tried to include enough introductory material to make the book readable by non-specialists.

The organization is straightforward and largely, though not strictly, chronological. The early pages are about the pre-1960s when techniques for quantitative observation of earth's vibrations were being invented and developed, and when styles for handling and analyzing the observations in ways that would turn out to be important during the discovery of plate tectonics were being formulated and established. The relevant results of these pre-1960 studies of earthquakes are summarized. Also included is a description of the evolution, and the results, of seismic studies using artificial sources that range in size from the pops of compressed air bubbles to the wallops of huge nuclear explosions. Such studies contributed important information on the earth that bore, directly or indirectly although perhaps not so charismatically as earthquake studies, on the development of plate tectonics.

For the critical and exciting years of the 1960s I focus on the earthquake studies that played a strong role in the plate tectonics story but also try to draw attention and give credit to less obvious and lesser known seismological results of other types. For the post-1960s period, I discuss some of the seismological studies that followed immediately after the formulation of the concept of plate tectonics but make only a cursory effort to summarize current activity in earthquake seismology. This is a history book, not a review of a branch of current science.

Near the end, however, and with the dreams characteristic of an aging scientist, I could not resist the temptation to pass on a few philosophical remarks that might inspire some hard-driving young scientist somewhere to bigger and better discovery. I point out a few areas which, in my opinion, hold exceptional potential for seismology of the future, and some strategies and tactics for realizing that potential.

As the book closes, I hope it will be no less than blatantly obvious that I am optimistic about the future of earth science, and that I encourage those who would study the earth to push ahead at full steam and with the conviction that if they do so they can readily eclipse the accomplishments of earlier eras, even of the very special and exciting era of discovery of plate tectonics described here, and now some quarter of a century past.

Each limerick's a bit of tomfoolery
And hardly a triumph scholaroolery
But limericks pulled from the air
And applied here and there
Are a kind of syntactical jewelry.

SOME HISTORIES OF THE PLATE TECTONICS REVOLUTION

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Benjamin Howell Jr., Charles Drake, Bryan Isacks, Lynn Sykes, Muawia Barazangi, and George Hade read an early draft and made helpful comments and suggestions, most of which resulted in revisions. However, I, not they, bear full responsibility for the final content. Cornell University provided logistical support during the writing; Sue Peterson typed the manuscript and innumerable revisions.

Writing a book on the history of a facet of science, perhaps more so than normal day to day activity in science, inevitably leaves one awed by realization of the scale of the collective efforts of large numbers of those scientists who contributed almost endlessly and tirelessly and often almost anonymously to progress in that branch of science. No historian or history could possibly do full-justice to those contributions. I can only acknowledge in a general way the collective efforts and effects of such scientists and regret that they can not all be cited individually.

Various institutions, in addition to Columbia and others mentioned in the text, supported the efforts of scientists described here as did various government and private funding agencies. Particularly prominent were National Science Foundation, Air Force Cambridge Research Laboratories, Air Force Office of Scientific Research, Office of Naval Research, and Advanced Research Projects Agency, all, except for NSF, parts of the Department of Defense that supported basic research in earth science.

Finally, during that period of the 1960's that brought the scientific excitement, upheaval and euphoria on which this book is based, I met and married my wife, Gay, and she produced and cared for our two daughters, Nelly and Amy. In appreciation and gratitude, I dedicate this book to her and to them.

1

Observing the Shaking

A magnitude eight every time
Makes P, S, and Rayleigh sublime
Then those undulations
Form earth's free oscillations
For a grand seismological chime.

All modern science is basically empirical, in essence merely the organization and comprehension of reliable observations. No matter how sophisticated the theory, how intricate the reasoning, how ingenious the schemes for organization, or how contorted the philosophical analysis of the scientific process, the ultimate test of science is in the observations. Any branch of science, seismology of course included, is therefore rooted in, controlled by, and often driven in new directions by, its observations. What can be, and is, observed and then communicated appropriately ultimately determines the course of science. To understand the history and the evolution of seismology then, we must have a general understanding of the development and growth of its observational capacity, and of the observations themselves.

Unlike some natural phenomena, such as cosmic rays or radioactivity, which require the use of sophisticated instrumentation before the phenomenon can even be recognized, earthquakes have probably never been unknown, ignored or overlooked, at least since humans began living in active seismic areas. The largest earthquakes are readily felt by all those near the center of the disturbance, and accounts of earthquake-induced destruction and casualties and fear appear early in recorded history. Earth scientists, ancient and recent, have sought to relate non-instrumentally determined "macroscopic" evidence, such as shaking, faulting, elevation change and geographical location, to other kinds of geological observations in an effort to understand the earthquake phenomena.

The early Greek philosophers noted and commented upon earthquakes; so did the Chinese and Arabs; so have Western scholars. As just one example, Darwin, during his travels aboard the Beagle, studied shaking, shoreline changes, and volcanic eruptions correlated with the great Chilean earthquake of 1835, and, somewhat incredibly, at that early date proposed a mechanism to explain Andean earthquakes that bears some similarity to, although it falls short of, modern ideas about plate tectonics. Neither Darwin's perceptive thoughts nor those of others that were also based on macroseismic effects played a role of great significance in the development of plate tectonics during the 1960s, however. Seismology's contributions were to arise instead not so much from the macroscopic data as from the microscopic evidence that would come from the operation of global or regional networks of sensitive seismographs capable of detecting extremely minute motions of the earth's surface at large distances from the center of the disturbances. No human, unaided by instruments, can detect such motions.

The development of global networks and the sensitive individual seismographs that are the components of those networks is an inspiring tale of dedicated individuals, clever and imaginative ideas, perseverance and hard work, and technological evolution both within and outside the discipline of seismology (See Howell, 1990 for a comprehensive review). What follows is a brief sketch of the principles and certain of the problems involved, particularly as they relate to the plate tectonics story.

The goal of recording, or measuring, or just detecting through some device, the ground motion associated with an earthquake has been around for many centuries. Some early attempts to build seismographs were based on such phenomena as steel balls falling from the mouths of sculptured dragons (!), ripples on a pool of mercury disturbed by earth motion, and fallen rectangular or cylindrical blocks once stood on end. Such attempts seem primitive or incongruous when contrasted with the best of modern instruments. Nevertheless they emphasize an important and reasonably obvious point, namely the influence of the state of contemporary technology on the activity in a particular branch of science. As we shall see, and as in most sciences, the development of seismological instrumentation follows, with only a modest lag in time, development of new technologies that can be incorporated into seismological instrumentation. For the visionary, that dependency raises the issue of what modern and near-future technology holds in store for near-future seismology; certainly the electronics revolution is in the process of making current instrumentation seem as cumbersome and antiquated as some of the early seismographs we are about to discuss. For the historian, it raises the question of just when seismology was capable of producing the observations that would enable it to contribute importantly to the development of plate tectonics.

The earliest crude seismic instruments, such as those mentioned above, were designed to detect that an earthquake had happened and perhaps provide a crude measure of the intensity and direction of the shaking. That was not enough. Soon seismologists recognized that great value lay in recording in some detail the complex motion of the earth as it changed with time. Thus beginning in the late 19th century the goal was to build an instrument that could obtain a record of the ground motion at one point some distance from the center of an earthquake as a function of time. Once that goal was to a degree attained, instruments capable of doing so became common and widely distributed at locations around the earth, and seismology became the prime means for learning about the earth's deep interior.

At first it seems that detecting and recording the ground motion of an earthquake that shakes down buildings and causes nausea in nearby observers should be a simple matter, and indeed that particular task is not difficult. But earthquakes of interest to scientists vary greatly in size, through a range of at least ten orders of magnitude, and the amplitudes of their waves also vary with distance. Detecting an earthquake whose source is beneath one's feet is far different from detecting that same earthquake at a point halfway around the world. Thus the amplitudes of seismic transients that a seismologist might wish to detect, record and study may range from many feet to a fraction of the minute distance between two adjacent atoms in a solid! And the frequencies of interest vary from as high as 100 cycles per second or so to as low as a single cycle of oscillation in nearly an hour! The challenge of developing instruments to monitor this wide range of seismic motions is thus a substantial one that, even today after more than a century of effort, continues to evolve.

When a seismic wave passes a particular location, everything in the vicinity — buildings, rocks, seismologists and instruments — moves more or less in unison. Except in the case of "shaky" structures or catastrophic failures, there is little relative motion to be measured. Detecting earthquake-generated ground motion would be easier if one had a fixed platform whose position was unaffected by the earthquake. Then the seismic motion of a moving point on the ground could be compared to a fixed point on the platform. But there is, of course, no such fixed platform. In this respect, the seismologist experiences a dilemma somewhat like that of Archimedes who claimed that given a lever long enough and a place to stand, he could move the earth.

Seismologists resolve the matter principally through the use of an inertial mass that is not firmly fastened but, rather, loosely coupled to the earth. To illustrate how this technique works in principle, consider the simple case of a mass suspended from the lower end of a coiled spring whose upper end is fixed to the earth. If constrained to move freely only in the vertical direction, the mass will have a simple natural period of oscillation. Furthermore its oscillations can be damped so that it does not vibrate wildly.

Now if, as a consequence of an earthquake (or any other source of ground motion), the earth at the point of attachment of the spring oscillates with a period less than that of the free mass-spring system, the mass will tend to remain fixed in space. With the earth moving and the mass more nearly stationary, one need only measure and record the distance between the two in order to obtain a registration of earth motion, in short a seismogram. In fact, even for periods of oscillation much longer than that of the mass-spring system, a relative motion corresponding not to displacement but to acceleration of the earth can be measured. This principle is the basis of nearly all seismographs and they are termed inertial seismographs. Another type of seismograph based on measurement of the distance between two well-separated points in the earth is called a strain seismograph but such devices are far less common.

The simple inertial seismograph described above detects only the vertical component of ground motion. But to specify ground motion completely, two other orthogonal components must be recorded simultaneously. In order to measure a horizontal component, a modified sensor is required. Such a sensor can be readily constructed by suspending the inertial mass on what amounts to a sort of swinging gate, a boom supported by hinges rotating about a near-vertical axis, rather than a spring. The free period of such a sensor, or horizontal pendulum, can be adjusted simply by tilting slightly that axis of rotation for the hinges. The free period of a vertical sensor can be adjusted by varying the spring constant or the geometry of the suspension. In practice, it is not overly difficult to build horizontal and vertically operating sensors with free periods as great as 15 to 30 seconds. Such sensors can detect much, though not all, of the spectrum of ground motion generated by a large earthquake.

Once the sensors are in hand, the next task is to convert the minute relative motion between earth and mass into a form that allows it to be recorded as a function of time. In the early days of seismological instrumentation, the late nineteenth and early twentieth century, the relative motion of the inertial mass was commonly amplified by a system of mechanical levers. In systems with considerable amplification, significant friction had to be overcome, so sometimes huge masses, some as large as a small room, were employed. The mass drove a lever system that culminated in a needle that scratched a track on smoked paper covering a continuously rotating and translating drum. Earth motions appeared as deflections of what would otherwise have been a straight line on a sheet of paper that, once removed from the drum, was flat and rectilinear, easy to store, and easy to read. Dipping the smoked paper in shellac preserved the record indefinitely.

In a later advance, in order to cut down friction or improve gain, small mirrors were attached to the mass, or somewhere in the mechanical system, so that an optical lever could supplement or replace the mechanical one. A spot of light then wrote a photographic record similar in format to the smoked paper record described above. Still later, sensitive electromagnetic methods for detecting relative motion between the inertial mass and earth were devised. Motion between a coil mounted on the mass and a magnet on the earth generated current in a galvanometer connected to the coil, and so moved a light spot that could be recorded photographically. This technique had many advantages and so became more or less standard for a considerable interval of time. Almost all of the data on earthquakes that helped to produce the plate tectonics revolution were obtained from seismographs using this technique, or a slight variation of it based on variable reluctance.

Almost all modern seismographs continue to rely on the inertial detector. The electronic age, however, has brought amplifiers, filters and other devices, and particularly digital recording techniques that are clearly the wave of the present and future. Digital methods were beginning to take root in seismology during the 1960s because of the ease and versatility of digital data processing and because of the enhanced dynamic range of the recording system, but they were not an important factor in seismology's contribution to the plate tectonics revolution during that decade.

To emphasize the point, referred to earlier, that seismological instrumentation follows technological development in general, one might note here the parallels between seismograph development and the evolution of the phonograph. Early phonographs, of course, were also largely mechanical, transmitting irregularities of a groove scratched in wax and then sensed by a needle through a mechanical lever system to a mechanical speaker. Later, mechanical to electrical transducers were added as were electronic amplifiers and electromagnetic speakers and such systems prevailed for a time. During the 1960s, seismologists celebrating the coming of plate tectonics at a party listened to music from a device analogous to the seismographs of the day. Now phonographs, like seismographs, are dominated by digital techniques and digital recording systems.

The preceding paragraphs describe briefly and in very generalized fashion the evolution of the seismograph. No such evolution ever actually occurs simply or unidirectionally in practice of course, at least in its earlier stages, because the humans involved have different perspectives, different goals, different backgrounds and different ideas on how to proceed. To unify such an effort requires a common purpose or a central funding source. Early seismologists differed somewhat on the former and certainly did not have the latter, for the field of seismology as a whole had very limited funding during the early 20th century and before. The sparse funds that were available came from diverse sources, perhaps the organization that employed the scientist, perhaps the scientist himself or, rarely, an organization concerned with the local or regional earthquake hazard. Thus, in those early days, each seismologist operated more or less independently and with strong financial constraint, and the diversity and decided lack of standardization of instruments that the science produced showed it.

For example, during roughly the first half of the 20th century, some seismograph stations measured only one component of ground motion, perhaps the vertical. Others measured only two orthogonal horizontal components. If three components of ground motion were measured, the frequency response of the vertical rarely matched that of the horizontals and usually was so different as to inhibit inter-comparison almost entirely.

Designers of sensitive seismographs also had to contend with noise. In the spectrum of seismic background noise in the earth there is a sharp and persistent peak in the period range of about four to nine seconds due to microseisms generated primarily by gravity waves of twice that period on large bodies of water. The earliest arriving, compressional seismic waves from a distant earthquake often have abundant energy in periods shorter than that of the noise peak. Furthermore, the energy of later arriving seismic body waves, and most seismic surface waves, is predominantly in periods greater than that of the peak. Instrument designers, therefore, tried to avoid the peak and built both "short-period" and "long-period" seismographs. Often, and unfortunately, the vertical and horizontal components differed in frequency response as a consequence.

Other factors contributed to the diversity of seismographs. Seismologists living far from the earthquake-prone areas tried to build instruments of sufficient sensitivity to detect background noise in the earth and hence many teleseismic events. Seismologists living in active areas built less sensitive instruments that would not be thrown off scale or made inoperative by a nearby shock. Seismologists such as Milne, Wiechert, Galitzin, Benioff, Wood, Anderson, Press, and Ewing focused a part of their efforts on instrument design and produced seismographs with special characteristics that became widely used and that bear their names. As was true for many physicists in the early days of that subject, a seismologist was often a machinist, engineer and technological innovator as well as a scientist. Hugo Benioff, for example, a versatile and talented earth scientist, was also a consultant to a maker of musical instruments, an occupation where certain parallels with seismographic instrumentation may be found by those who understand the basic physics of both kinds of devices.

Adding to the difficulties that came from lack of standardization of seismographs was limited capacity for calibration. Most instruments of that era were not regularly calibrated, but should have been for the performances of some instruments were sensitive to external environmental effects and seismographs of the era did not normally operate in controlled environments as they do today. The location of many early instruments was often in the basement of a science building of the scientist's university, and hence in whatever surroundings the campus of that institution might be — metropolitan, suburban or rural; coastal, island, or continental interior. Arrays of seismographs were sometimes especially installed in active seismic areas to measure local earthquakes but usually at places selected for convenience of operation rather than favorable environment.

One short story might illustrate the modus operandi of early seismologists. From one scientific meeting I can recall a paper whose title announced a newly-developed seismograph with an abrupt right-angled bend in the boom that supported the mass. I attended the talk, eagerly expecting to hear of some unexpected quirk of instrumental dynamics that produced an ingenious new way to observe seismic waves, only to learn that the bend was only there so that the instrument would fit into the available, but also very confined, space open to use by that investigator. As most seismometers occupy an area of less than 3 or 4 square feet, it was obvious that space was very tight indeed in the quarters of that science department!

Another factor affecting the location and operation of seismograph stations was the need for accurate timing of the seismic recordings. Seismologists quickly recognized that, in order to compare optimally the seismic data from one station with that of another elsewhere in the world, accurate timing, preferably to a small fraction of a second was needed. During the first half of the century at least, radio time signals were not available for much of the world as they are now. Therefore, some seismographs were located at astronomical observatories where their pendulum clocks could be frequently calibrated against the stars. Other seismographs were located at remote places that were the sites of other kinds of scientific observatories, or religious missions, or military outposts, or whatever. Sometimes recorded time at such sites was accurate, but often it was not, and timing depended on a pendulum clock checked only sporadically against fortuitous short wave radio signals. After WWII, radio stations capable of broadcasting standard time signals throughout much of the world began to operate, and seismologists benefited accordingly, but prior to that development timing was sometimes so bad that earthquake locations in certain parts of the earth might be in error by hundreds of kilometers, perhaps two orders of magnitude worse than the standard of today. As we shall see, improved precision and accuracy of location of earthquakes throughout the world during the 1950s and 1960s was a factor in the development of plate tectonics.

The end product of the diverse efforts and effects described above, as carried out or experienced by a heterogeneous group of scientists working with little funding, scattered throughout the world, and having little in common except for an intense interest in how the earth shakes, was, of course, correspondingly diverse. What evolved was a global array of various kinds of instruments operating only on land at locations that otherwise seemed selected almost at random. There was a variety of recording formats; there was initially no center for data archiving. Readings were communicated by mail, often months after the event. Observations in the form of seismograms were circulated from seismologist to seismologist rarely and through the courtesy of individuals. From the viewpoint of one indoctrinated in order and organization, the situation in early seismology must have seemed chaotic. But, in spite of growing pains it was not all bad. Much of it was good, and the early efforts were the basis for the improved system that would follow.

As early seismographic observations on teleseisms accumulated, and it quickly became apparent that, for sufficiently large earthquakes, stations throughout the world recorded data that were complementary and had to be studied together, seismologists felt the need to amass and analyze data at a central place. They began to set up organizations and communication modes to accomplish this task. Interestingly, one of the very first groups to recognize the value of a globally coordinated seismograph network was made up of members of the religious order of the Society of Jesus. The Jesuits had some talented scientists, a strong interest in learning just how the earth worked, and a unique network of globally-distributed missions. They established seismographs at as many as possible of those missions, sent the data to a central point, and from there published locations and other information on earthquakes throughout the world. They were an important factor in the early development of the science of seismology.

Two international centers sprang up in Europe, one the Bureau Centrale Internationale de Seismologie in France and the other, the International Seismological Summary, or Center, in England. Both published locations of events and the ISC in particular published extensive readings, mostly arrival times of seismic phases, for all stations reporting on a particular earthquake. The ISC became a kind of final archive for most such data. Other organizations, usually the centers for local or regional or national networks also sprang up. Some examples are the Dominion Observatory in Canada (where seismology co-existed with astronomy), the Japanese Meteorological Agency (likewise with meteorology), and the U.S. Coast and Geodetic Survey (seismology with geodesy and surveying). The USC and GS performed a valuable service by providing relatively quick preliminary locations of earthquake epicenters and distributing them by postcard. "Quick" in the 1950s meant within a few weeks whereas the ISC "final" locations often took many months or years as the center waited for communication of data that sometimes arrived only in the form of station bulletins prepared and distributed annually. In contrast, using data from today's global and electronically-connected network, hypocentral locations are available within minutes or hours of the time of the shock.

Not all of the early networks were government operated. The Seismological Laboratory of CalTech and the University of California at Berkeley, for example, both operated local networks primarily for study of earthquakes in southern and northern California, respectively.

For all of these services, local, regional or global, emphasis was on the time of arrival of seismic phases. As we shall see in the next chapter the arrival times were a very important source of information on the earth's interior. However, there is much additional information in the form of amplitudes, frequencies, wave character, etc. The seismologist who wished to study such features had to request copies of the original seismograms separately from each of the individual stations. For a global study that was a chore, often an exercise in language and communication. And there was often a long delay while seismograms were being copied (no handy Xerox copier then), and errors or gaps in information transmittal often crept in along the way. Some further standardization and organization was clearly needed. It would become possible as new funds for support of the science of seismology became available because of government interest in a nuclear test ban treaty.

With the start of the atomic age in the mid 1940s, a new era of the subject of seismology began, although it was some time before seismologists, or anyone, recognized how large the impact would be. The very first atomic explosion in New Mexico during WWII was recorded by seismographs. In the post WWII years as the Cold War flourished, larger and larger nuclear explosions were tested, some so large that they could readily be detected by seismographs throughout the world. Networks of seismographs designed and operated to monitor earthquakes recorded these events, as did clandestine national networks of seismographs designed to monitor the test explosions of other nuclear powers. Eventually nuclear testing and the Cold War itself caused such high levels of concern that a means for curtailing nuclear testing was sought by the political powers. In 1958, seismologists suddenly and unexpectedly found themselves members of political delegations to international negotiations on a nuclear test ban treaty. They were the technical experts on underground test detection. For most, it was a strange leap from the musty basement of the laboratory where earthquake instruments were maintained to the stately and protocolized halls of Geneva where seismology became both hopelessly and hopefully intermingled with international politics, from that date to at least the present.

The scientific-political history of the role of seismology in nuclear test ban negotiations is well beyond the scope of this book but a consequence of that political initiative is not. As a result of the attention focused on seismology as the principal means for detecting distant underground nuclear explosions, it became evident to governments, as it had long been to seismologists, that seismology could, and because of hope for a treaty should, be improved, that given additional effort, there was potential for major advance in seismology, and that the field of seismology, compared to many sciences was underfunded. In 1959, a panel of seismologists (of which I was one) and other scientists under the chairmanship of Lloyd Berkner made a number of recommendations for strengthening the U.S. capability in seismology. As a consequence new funding flowed into the field and activity accelerated. Bright new students and scientists and engineers from related areas were attracted into the subject and seismology began to flourish in the early 1960s as it never had before. Coincidentally it was also the time when the forerunners to what would become plate tectonics began to appear.

One recommendation of the Berkner Panel, authored by David Griggs and Frank Press, deserves special mention here because it had an effect especially relevant to the plate tectonics revolution. During the International Geophysical Year 1957-1958, Press and Maurice Ewing had established a global network of matched three-component long period seismographs at ten widely-dispersed stations. It now seems a modest attempt to build a standardized long-period global network, but it was a major step at the time. All the seismograms were archived at one place (Lamont Geological Observatory). It was an effort designed to facilitate research and, in a modest way, it was successful in doing so. The Berkner Panel built on this experience and the Griggs-Press recommendation called for 100 such stations, each with three matched short-period and three matched long-period components, distributed around the world. All data, in the form of analog records on photographic paper, were to be sent to one center where they would be microfilmed for distribution to researchers as requested.

The US Coast and Geodetic Survey undertook the task of installing the network and operating it and the data facility. Most of the instruments were installed at existing stations where they had the care of interested seismologists. It was a huge success. For the first time there was a network of numerous standardized seismographs throughout the world (it was called the World Wide Standardized Seismograph Network) and it transformed much of the activity in seismology to a higher level, particularly, as we shall see in a later chapter, the effort related to plate tectonics.

At this point, and on that note, it would seem reasonable to end this chapter on observations and move on to the next on the analysis of same. However, I am reluctant to do so abruptly and thereby leave the impression that seismology would necessarily have been better off had early seismologists gotten together and standardized their observations earlier, foregoing in the process the less-coordinated, instrument-developing activities. Certainly standardization was a great boon. Furthermore the WWSSN has been followed by still greater and more elegant standardization based on digital recording and modern computing techniques, all much to the benefit of the science of seismology. I support this trend and dread to think of how backward seismology might be without it. However, I also support a certain level of less regimented, less fettered activity in a science.

The free-wheeling, unorganized activities of early seismology were accompanied by a spirit of wonder and exploration and innovation. I am now concerned that, with the highly-organized systems of today, seismologists of the present and future may lose the freshness and the versatility and the stimulus of new ideas that pervade a field because many of its participants do things that are different in every way from those of their colleagues. It is easy to fall into the traps set by routine and convention and it requires special effort on the part of the individual to break free of those traps. A science thrives on freshness, innovation and vision. I hope this point will become even more clear as we proceed to following chapters.

2

Learning About the Earth and About Earthquakes

The early seismological players
Though found wanting at times as soothsayers
Job performed quite superiorly
As they probed earth interiorly
To reveal multi-concentrical layers.

The development of reliable seismographs and operation of them at many locations around the earth as described in the last chapter, and the earth's continuing seismic activity that typically produces annually more than a hundred shocks large enough to be recorded worldwide, eventually generated a huge quantity of information on seismic motion of many points on the earth's surface following earthquakes at a wide variety of hypocentral locations. The data were somewhat heterogeneous to be sure, sometimes lacking in standardization and timing accuracy and calibration, and collected only from the land-covered portion of the earth's surface, but they would turn out to be a treasurehouse of information on the nature of earthquakes and on the structure and composition of the earth's interior.

Determining just how to analyze those complex observations so that the information they contained could be best organized, comprehended, and made useful to others as a part of science was no simple task, however. But early seismologists with inspiration, insight, perseverance, diligence, and good fortune led the way and eventually there evolved an understanding of both earthquakes and the interior that became a foundation for the concept of plate tectonics and, indeed, for most of our understanding about the earth's interior today. Let us sketch briefly the manner in which that knowledge evolved.

Virtually all of the data were recorded in analog form, so-called wiggly-line records in which the trace described one component of ground motion as a function of time. Early in the game, one point was obvious. Near the source, most of the earth motion sensed by observers, or seismographs, was of short duration, perhaps only a few seconds at most. At distant seismograph stations, the motion, though lower in amplitude, lasted much longer, perhaps tens of minutes to an hour or more. The message was unavoidable. As is the case when a flash of lightning generates a long, low acoustic rumble of thunder, complex propagation of seismic waves as they travelled through the earth was turning a near-impulsive seismic disturbance into a prolonged wave-train at distance. In fact, the seismogram for a distant shock, or teleseism, typically began with a series of distinct near-impulsive events. The series lasted many minutes and the events were superimposed on a lower level of unrest also generated by the earthquake. The impulsive events were followed by a long-drawn out, oscillatory wave train of large amplitudes and sometimes a half hour or more in duration. Recognizing that there were messages about the deep interior hidden in those impulses and wave trains, seismologists set out to make sense of the complexity. One focus of attention was the very first wave to arrive.

The seismic record often began abruptly, a consequence, it turned out, of the compressional, or P, wave, the fastest wave traveling through the interior. Partly because, as the first arrival, it could be so easily identified and its arrival time measured so precisely, the P wave became a prime source of very useful information. For one thing, and particularly after its velocity became known, data on P waves from a number of stations could be used to locate the initial earthquake disturbance accurately in both space and time. Successive approximations and some bootstrapping were required, but eventually this technique became highly refined and it remains the best method for locating natural seismic disturbances today. Finding the velocity of the P wave, or to state the matter more precisely, the travel-time of P to any distant location, took some time and effort, but once in hand, P-wave travel times also became an invaluable source of information on the earth's interior.

Early in the game seismologists discovered what some must have anticipated but to others was a complete and astonishing surprise. To the level of precision prevailing at that time, the travel-time of P waves between two points on the earth's surface was the same as that between any two other points anywhere on earth, so long as each was the same distance from the earthquake as its counterpart in the other pair! This simple but important observation meant that the earth at depth was spherically symmetric, that there is no large lateral variation at any given depth level. The velocity varies with depth but it does not vary much laterally. And the precision of the result was very high for that time, much better than 1%.

This result seemed natural enough to one group of scientists, those who commonly dealt with simple physical or mathematical models of earth or the planets. To them, mostly physicists and geophysicists, this observation was yet another step in their game of making nature simple, and readily amenable to study through mathematical modeling. They liked what was effectively a layered sphere that could be easily represented mathematically. To others, particularly those steeped in observations of the geology and geography of the earth's surface, simple spherical symmetry was a surprise. Except for the fact that the surface is nearly a perfect sphere geometrically (neglecting ellipticity for a moment), the features of the surface such as continents, oceans, mountains, islands, etc. show no sign of spherical symmetry whatsoever! The simple seismic P-wave travel times therefore seemed a little incongruous to those, mostly geologically-oriented, observers.

These two different perspectives, one a view of earth by the modellers as relatively simple, the other a view of earth by the field observers as rather complex, prevail today and this schism will receive more attention as we progress in this story of plate tectonics. Among students of the interior, the view of the earth as spherically symmetrical held the day for many decades. But as we shall see, one of seismology's early important contributions to plate tectonics is based on the observation of departures from spherical symmetry in certain parts of the interior. One might say that this study broke a psychological barrier that had been imposed by long acceptance and reliance upon the spherically symmetric, simple model. And this trend is continuing at present as modern seismologists try to map tiny velocity anomalies, i.e. departures from spherical symmetry throughout the earth. Nevertheless, spherical symmetry remains a fine first approximation and early work on this topic was well done and important and should not be denigrated, even though the result may have created that psychological barrier that had to be overcome in the 1960s.

The technique of studying the travel time of distinct impulsive events on the seismogram was soon extended from the first-arriving P wave to include all other such events including shear, or S, waves and various combinations of P and S waves that had left the source as one type and reflected, refracted or converted along the way. Eventually these "travel-time studies" provided a complete set of travel-time curves for a wide variety of phases propagating in the earth. And those travel-time curves became the basis for deducing most of what is known of the structure of the deep interior. The outer core, for example, was found and measured on the basis of travel times and gross amplitude anomalies. The inner core, the various regions of the mantle, even the mantle itself, were found largely through study of seismic wave travel times.

Many seismologists participated in such studies; two groups eventually led the way. Gutenberg and Richter at Cal Tech produced a set of papers that documented the travel times of most seismic body wave phases. Their work was based heavily on thorough observations and analysis of those observations made by them through tireless effort and perseverance and was a magnificent contribution to science that has clearly stood the test of time. More or less concurrently, Jeffreys and Bullen at Cambridge independently developed complete seismic body wave travel times in similar but slightly different style, relying heavily on assembled readings by others as collected at international centers and on sound and careful statistical analysis. At the completion of the individual efforts, the differences between the results of the two groups were minor, and earth scientists had reliable travel time curves that could be used with confidence for locating earthquakes and deducing the structure of the earth's interior (for the spherically symmetrical earth!). The determination of near-complete travel time tables for all principal seismic body waves is surely one of the most important scientific studies of earth ever made.

Some insight into how science works, insight that may be instructive for young scientists, can be gleaned from the sequel to this story. Gutenberg and Richter published their results in the normal form of scientific publication, that is as a series of articles in scientific journals. Different segments of their work appeared at different times and all were not readily available as a compact, well-organized unit. Jeffreys and Bullen published in journals but also published a complete set of travel times in convenient form in a handy booklet that could be readily obtained. Consequently, the Jeffreys-Bullen tables (or JB tables as seismologists call them) become widely-known and were used as the world standard for many years. The lesson here is that good communication of results, not just routine publication of them, is often what make a piece of scientific work influential.

The intensive study of body waves that led to the travel time tables also brought some evidence of other kinds to light. For the core, or most if it, P waves were observed but not S waves, strong evidence, indeed, that the outer core is liquid. For the mantle however, the vast portion of earth between core and crust, the travel times of P and S waves traveling along the same path seemed completely compatible and reasonable, i.e. the P wave traveled consistently with velocity slightly less than twice that of S, about the ratio expected for body waves in an elastic solid such as rock. Thus it seemed from seismology that the mantle, with the possible exception of isolated magma bodies here and there near volcanoes, was solid.

Meanwhile studies were being made of glacial rebound, the recovery over thousands of years of the continental crust following removal of the ice load of the Pleistocene. Particularly for Scandinavia where the observations were exceptionally good, such studies indicated that at least a part of the upper mantle was flowing, behaving like a liquid of high viscosity and not the purely elastic solid seismologists had deduced. Eventually the conflicting seismic and rebound results were reconciled by assigning the mantle properties somewhat like those of "silly putty," which responds on the long term like a viscous liquid but on the short term like a solid, propagating both compressional and shear waves. The mantle was indeed solid, but at least some of it could also flow very slowly.

Then Gutenberg, in a study of seismic wave travel times and amplitudes at moderate distances from the source, found in the upper mantle a "low velocity layer," i.e. a layer in which the seismic velocity, at least for S, decreases with increasing depth rather than increasing with depth as it normally does for the rest of the mantle and then remains lower than the velocities of the overlying layer for a certain range of depths. These results, which among other things were subtle and demonstrated Gutenberg's remarkable intuitive sense of seismic phenomena and their causes, were corroborated by others. Although not interpreted as such at the time, the concept of a "low velocity layer" in the upper mantle became important during the plate tectonics revolution as a possible sign of weakness and ability to flow and hence an indication of "asthenosphere," a weak layer beneath the "lithospheric" (or non-flowing) plates above. Gutenberg's discovery is a prime example of a soundly-based pre-plate tectonics observational result that could be reinterpreted to fit and support the plate tectonics model as it was being conceived.

In spite of the powerful evidence from the travel time curves that the earth was, to a good first approximation, spherically symmetric and hence generally lacking in lateral variation, the obvious observations of surface relief and near-surface geology that fail to fit this simple pattern, could, of course, not be ignored. Consequently seismologists sought lateral differences, especially in shallow layers of the earth, the crust and the bounding uppermost mantle. Body wave studies based on near-earthquakes data provided some information in a few places near local earthquakes. Teleseismic body waves reflected from the surface also provided some information on the crust near the point of reflection, but early on such evidence was neither reliable nor definitive.

Eventually attention turned to the later-arriving train of waves (sometimes called the "coda" or tail) that followed the earlier-arriving and pulse-like body phases. They were surface waves, energy trapped near the surface because of the free surface and shallow near-surface layers of slow velocity. They were of two types, named after their discoverers, Rayleigh, and Love, respectively. The surface waves had large amplitudes, typically the largest on the seismogram of a particular shock, and were drawn out into long oscillatory, or dispersed, wave trains. The waves were dispersed, it turned out, in accordance with the complex rock velocity structure of the near-surface. Oceanic paths, for example, produced wave trains of completely different character than did continental paths, largely because of the water layer but also because of lateral differences in the underlying crustal rocks. Eventually such data, in conjunction with other information discussed in the next chapter, showed, unequivocally and contrary to some pending geological hypotheses, that the crustal rocks of the oceans were different from those of the continents. Hence the oceans were not simply submerged portions of old continents as some had suggested. This was an important advance during the early stages of the studies of the ocean floors that would lead eventually to the concept of sea floor spreading, the forerunner of plate tectonics.

Still longer surface waves provided information on the upper mantle that corroborated to a degree Gutenberg's evidence for a low velocity layer there. Eventually even longer waves, and the so-called "free oscillations" of earth they produced after multiple circumlocutions, would provide information on the deep interior complementary to that derived from body-wave travel times, but such information, though important otherwise, was not a key factor in the plate tectonics revolution.

At any rate, the studies outlined so briefly in the foregoing collectively produced an understanding of deep earth structure that would be especially useful during the plate tectonics revolution, particularly to those who sought to understand just how the earth could produce and maintain the moving system of plates that was the essence of the theory.

The attention of early seismologists was not exclusively directed to studies of the deep interior, of course. A significant fraction of their collective effort was devoted to attempts to understand earthquakes. One important question concerned the basic nature of the earthquake source. What happens to make the earth shake? Were earthquakes natural underground explosions? Or collapses? A shifting or rupturing or faulting of rock masses under stress? Was there a single common cause of earthquakes or a variety of causes? The great San Francisco earthquake of 1906, among others, provided part of the answer as geological observations of surface displacements coupled with seismological studies left little doubt that the earthquake had occurred because the San Andreas Fault had broken or ruptured, generating seismic waves in the process. The idea that the rupturing along a fault plane such as that in California was a consequence of tectonic stress was soon generalized and carried further by seismologists, and applied to almost all tectonic earthquakes.

In Japan, and in an important pioneering study, Honda showed that, if the directions of first motions of P waves at a number of seismograph stations were projected back to the focus of the earthquake, there was a complex but nevertheless consistent spatial pattern of waves radiated from the shock. Initial compressions went in some directions, initial rarefactions in others, and the radiation pattern was just that expected from a rupture along a segment of a plane with appropriate orientation in space and appropriate displacement of rocks on opposing sides of the fault. Eureka!! From this information, seismologists could not only deduce that the earth had ruptured but also determine the orientation of the fault plane in space and something about stresses deep in the earth. Honda's brilliant lead was followed by others. Byerly and his students at Berkeley, for example, developed the method further and applied it to selected large earthquakes, mostly those that had happened recently. Initially, however, only a small number of earthquakes was studied in this manner.

In the 1950s, John Hodgson of the Dominion Observatory in Ottawa and the son of Ernest Hodgson, a pioneer in seismology in Canada, developed a project that was visionary and of a style that would turn out to be very important in the plate tectonics story. But unfortunately, it had one serious problem; it was before its time. Hodgson set out to apply Honda's and Byerly's method on a global scale using data on many earthquakes distributed throughout the world. The objective was to seek consistencies in the global pattern of focal mechanisms that might reveal something fundamental about earth tectonics. The project had some limited success, but basically it ran aground and failed to produce the global results hoped for, simply because the raw data were not yet reliable enough. Lack of standardization in the seismographic network, plus the poor communication of data at that time, led to so many inconsistencies in the interpreted readings of the first motion of P that focal mechanisms were often unreliable and hence misleading. Inconsistencies were so common, in fact, that it was thought at first by some that the basic assumptions of Honda's method were incorrect, and some lost faith in it. When the World Wide Standardized Seismograph Network began to produce data however, it became clear that the method did work well much of the time, and it also became clear that the inconsistencies were artifacts and not reliable observations of earth. Thus Hodgson was vindicated and he deserves credit for developing a well-conceived and important new kind of project, but it was others who made a timely entrance to the field just as the WWSSN became productive who were able to make the critical studies that added to and supported plate tectonics in its early stages. Focal mechanism studies of the type developed by these pioneers continue to play a major role in seismotectonics.

Seismologists sought other kinds of information on earthquakes in addition to focal mechanisms, of course. As the capability for precise location of earthquakes in space and time and for crude measure of their size developed, it became possible to attack many questions about earthquakes. Where do they occur? When do they occur? How large are they? How often and in what sequence do they occur? Why do they occur? Most of these questions refer to a branch of seismology called seismicity, essentially the geography of earthquakes. It would turn out, particularly, that just where earthquakes occur was a most important piece of information during development of the plate tectonic theory.

Mapping and understanding of the pattern of the earth's seismicity developed gradually. Early in the game, of course, it became evident that earthquakes do not occur randomly over the earth. Rather they occur repeatedly in certain narrow and elongate zones, zones that often are, more or less, coincident with other readily observed tectonic features, such as volcanoes, mountains, island arcs, and trenches. Initially those zones were not recognized as interconnected on a global scale, but once a sufficient number of earthquakes had been located, a global pattern emerged. It was recognized that earthquakes tend to occur in narrow belts that encircle the earth. Later it would be recognized that such a coherent global pattern implied a driving mechanism of global scale.

In 1949 Gutenberg and Richter published an authoritative and comprehensive book that quickly became known informally as "the bible of seismicity." Through exceptional effort they had located or relocated most earthquakes throughout the earth that were sufficiently large to be well-recorded, and in that book they presented the information in text and on global and regional maps. By far the best effort of its kind at the time, their work produced much information on seismicity that is now considered basic. However, Gutenberg and Richter's work was based on data from the heterogeneous seismograph network of the first half of the century and on painstaking, but limited, calculations by hand calculator. Hence locations were not nearly so accurate nor the data nearly so complete on a particular shock as they became in the 1960s with the advent of the WWSSN and digital electronic computers (NGT, Fig. 15).

Nevertheless, the gross pattern of the major global seismic belts, though not all of the details that would be important to the development of plate tectonics, was recognizable. It was clear that earthquakes tend to occur in narrow belts that encircle the earth, that sometimes intersect, and that rarely end by tapering off to negligible activity. All seismologists, and many other earth scientists, were generally familiar with that global pattern. Strangely, so far as I know, and there is an important lesson here, none of us thought to ask why certain features of that particular pattern were as they were. For example, when two seismic belts intersected they never crossed. There were always three arms to the intersection, not four. As we shall see, the global pattern of the belts is consistent with the story of the plates. It might have led us closer to that story earlier had we emphasized study of the seismic pattern but somehow that point escaped us for sometime. We had all learned in high school science classes that we had to "ask the right questions," but in this case we ignored that advice.

As Gutenberg and Richter documented in their book, most earthquake foci are shallow, but in certain discrete and limited zones events as deep as 700 km occur. Deep earthquakes were known prior to publication of their text, mostly from studies in Japan, where Wadati had shown in a pioneering and initially controversial study that deep earthquakes do indeed occur, and that they occurred in a thin zone that dipped neatly beneath the Japanese island arc. It also became known early that nearly all deep earthquakes were associated with island arcs. But the global pattern of the deep earthquake zones, and the arcs, would not be explained until the 1960s.

Many attempts to understand the pattern of the arcs and the deep shocks were made prior to that time of course, but all had shortcomings. For example, Benioff associated the deep earthquake zone of Wadati with a major fault somewhat like that, but without the huge displacements, of modern tectonic theory. As a consequence of the early work, such zones are now termed Benioff , or Wadati-Benioff, zones. Although I have the highest regard for Benioff and his many contributions to earth science and am pleased to honor him, I prefer the name, Wadati-Benioff zone, because it recognizes Wadati's extraordinary efforts as well. Studies of deep shocks during that era were hampered, because, with the exception of the Japanese arc, deep earthquake zones were generally not well-instrumented with seismographs located in the area above the earthquake foci. Thus the depths of most shocks in those early days were determined from waves reflected from the surface to distant locations. Although this method is sound, and still used today, its application suffered then because of deficiencies in the quality of data and data interpretation at that time.

Studies of seismicity, particularly the work of Gutenberg and Richter, included much information on sizes (or magnitudes) of earthquakes, frequency of occurrence, aftershocks and foreshocks, etc., but this kind of information, though doubtlessly important otherwise, played only a minor role in the development of plate tectonics and then largely in a qualitative sense.

In addition to the studies cited above and based on body wave travel times, surface wave dispersion, and gross seismic wave amplitudes, during the pre-1960s innovative seismologists produced some less conventional studies based on other characteristics of the seismographic data. As a group, studies of this type were not important to the plate tectonics revolution. A few that were will be cited in later chapters as their relevance dictates.

To summarize this chapter briefly, it might be said that seismological studies of natural earthquakes during the first half of the 20th century made unprecedented sense of the information recorded by seismographs and established seismology as the principal source of information on the earth's deep interior. But it was obvious to seismologists that major problems and major opportunities for further advance remained. Let us now turn our attention from natural earthquakes to artificial sources of seismic waves in order to see what additional information they provided to aid and abet, and in some cases to hamper, the plate tectonics revolution.

3

Artificial Sources

Deep subterranean inspection
By the method of seismic reflection
Means knowing much clearer
Each lithological mirror,
A step to geologic perfection?

From the foregoing it should be more than clear that, though sometimes destructive and terrifying, natural earthquakes, through the seismic waves they generate, are an invaluable source of information on the earth's interior. And, conveniently for the scientists who study them, if not the public, natural earthquakes are abundant hence frequent, powerful, widely-distributed over many regions of the earth and through depths ranging to 700 km, and, as sources of seismic waves, inexpensive, even free. Thus the substantial early effort to study natural earthquakes that we have been discussing was mounted, and thus earthquakes became a critical source of information as the concept of plate tectonics was being developed.

There are some drawbacks to the use of earthquakes as sources of seismic waves for scientific purposes, however. Earthquakes cannot be predicted precisely, so special instrumentation cannot be laid out before the shock occurs. A fortuitous element is always present in study of natural earthquakes. Location and origin time are always determined from analysis of the seismic waves the quake generates and hence are never so well-known as they might be if direct geodetic and chronological measurements could be made. And, of course, earthquakes do not occur at many of the places where a seismologist might like to have the source of seismic waves for scientific purposes.

Early seismologists quickly recognized these shortcomings and also the advantages that might be gained from use of artificial sources that could be precisely located and timed, and monitored by arrays of special instruments that need only be deployed temporarily. As a consequence, and one way or another, artificial sources have been used in seismology for many decades. A representative example is the study of seismic waves generated by the detonation of a huge quantity of surplus military explosives at Helgoland following World War I. Though outdated now, at the time that study provided unprecedented information on the crust of Western Europe.

Artificial sources of seismic waves need not be large to be useful, however. Weight drops and hammer blows generate detectable seismic waves that penetrate the earth a few tens of feet and so provide information on the topmost layers of soil and bedrock. At the other extreme of scale, and as noted earlier, waves from large nuclear explosions can be detected after traveling through the very center of the earth. Studies of intermediate scale use as sources chemical explosions of various sizes, rapidly expanding air bubbles in water, and huge truck-mounted vibrators whose oscillatory signals can be made to simulate impulsive sources with appropriate computer processing. In this chapter, and elsewhere in later chapters, we shall discuss studies of seismic waves generated by various of these artificial sources, concentrating, of course, on those that had an obvious influence on the development of plate tectonics. The variety in such studies is so great that we must begin with some categorization.

Almost all studies that use seismic waves generated by artificial sources of small to moderate size fall into one of two classes, typically designated by the terms "reflection" or "refraction." In seismic reflection studies, a source at or near the surface generates compressional waves that are partially reflected at buried interfaces more or less below the source and returned to the surface where they are recorded by an array of seismic wave detectors deployed near the source location. The entire, and relatively compact, operation is moved along the surface and the source repeated frequently so that eventually a seismic profile is obtained. A seismic profile is analogous to the echogram, or water bottom profile, commonly obtained by the depth sounder on a boat or ship, likewise through the use of sonic sources repeated at adjoining surface locations.

In seismic refraction studies, the source is also near the surface but the sensors are deployed along a line extending to horizontal distances of, typically, about ten times the depth of the deepest interface being probed. The key information is obtained from waves that travel obliquely from the source to a near-horizontal interface below, are refracted so as to travel along the interface until again refracted up to surface detectors. For this method to work, the wave velocity must, in general, increase with depth, which it normally does in the earth.

The information obtained by these two methods is complementary. Refraction tends to provide better information on rock velocity, and reflection better resolution of rock structure, although these generalizations are oversimplifications and do not always hold. With this basic background, however, we can discuss some more specific applications and summarize the results.

The most common application of the refraction technique is in study of the thickness and velocity structure of the continental crust. Initially such investigations used near-earthquakes as the source, and with some success. Such work led to the discovery by A. Mohorovicic of the crust-mantle boundary that bears his name, or an abbreviation of it. But then, for the reasons noted above, artificial sources, such as quarry blasts that were set off for another purpose but that could be timed and located precisely, gradually replaced earthquake sources. Later, chemical explosives of the order of a ton or more of TNT were fired in specially drilled holes, or in bodies of water, with the primary purpose of generating seismic waves; these procedures are still used.

Seismic refraction studies of the crust have been carried out in many countries, including the USA and most notably the Soviet Union where a high level of activity prevailed for decades. The particular result of all this work that bore most heavily on the development of plate tectonics was the clear demonstration of the existence of the Moho, or crust-mantle boundary, at a depth of about 40 km most everywhere beneath the continents. The depth varies somewhat from place to place but is always of that order. Thus when comparable studies were also made of the ocean basins there was evidence to demonstrate the contrast in thickness and hence the clear distinction between continental and oceanic crust, a point of considerable importance in the global tectonic story.

As a result of the success of refraction work on land, the earliest attempts at seismic studies of the sea floor were based on the refraction method. After some preliminaries in which explosive sources were detonated and instruments were operated on the ocean floor, a simpler two-ship technique was devised. One ship fired explosive charges at shallow depths; the other listened with hydrophones (pressure sensitive devices) also at shallow depths. Successive shots were fired as the ships moved apart. Waves refracted through the oceanic sediments, the oceanic crust and the underlying mantle were recorded.

This work was pioneered on a small scale by Maurice Ewing beginning in the 1930s, but only began to flourish in the post WW II years, still under the leadership of Ewing who was by then building the Lamont Geological Observatory, now Lamont-Doherty Earth Observatory. Raitt at Scripps Institute of Oceanography, Hersey at Woods Hole Oceanographic Institution and Hill at Cambridge University also developed such programs during this era. The earliest efforts at obtaining a representative sample of the seismic properties of deep ocean basin crust were plagued by problems such as a) working too close to shore, b) using explosive charges that were too small and c) failure to observe truly reversed profiles. Eventually, and in spite of the high cost of operating ships and the huge size of the ocean basins, enough data were collected so that the typical deep sea crustal column could be described (Officer et al, 1952) and some valid generalizations about the nature of the rocks of the deep sea floor everywhere could be made.

The principal, and surprising, result was that the oceanic crust is everywhere much thinner than the continental crust. It is only about 5 km thick in contrast to 40 km for the continental crust. Hence the Moho, or top of the mantle, is only at a depth of 10 km or so beneath the ocean surface. This observation meant that oceanic areas could not simply be places where part of an old continent had subsided, as many earth scientists, some perhaps influenced by Plato's story of the lost continent of Atlantis, thought at the time. The ocean crust was something different, and must have been formed in a different way, a key point later as the concept of sea-floor spreading, an early step in the development of plate tectonics, was proposed.

Seismic refraction work at sea provided yet another observation that was an important, if not critical, clue to the understanding of global tectonics. The oceanic crust, it turned out, is overlain by a layer of sediments that is, in general, remarkably thin, perhaps less than a kilometer or so in total thickness . Had the ocean basins existed as they now are for a large fraction of earth history, the sediments should have been much, much thicker, given the rates of sediment deposition that are known. Thus those who thought the ocean basins had existed as they are now throughout much of earth history, the followers of the so-called permanence of ocean basins hypothesis, faced what became a well-known enigma when the observations of sediment thickness clearly demonstrated how thin those sediments are. Earth scientists eventually were driven by the enigma to a new model for creation of ocean basins, namely sea-floor spreading. Sea floor spreading results in geologically youthful ocean basins, and hence thin sediments, and it was a major step toward plate tectonics. Still more information on the history of the ocean basins was available from seismic exploration of the sediments but, it would turn out, it was the reflection technique, not refraction, that would produce the bulk of it.

With the exception of sampling by drilling which at best can only be carried out on a limited scale, seismic reflection studies in general provide the best and most detailed information on the uppermost layers of the earth. By far the most extensive, and intensive, use of the seismic reflection technique has been, and continues to be, in the petroleum industry where it is the basis for a multi-billion dollar per year exploration effort. Initially, and shortly after WW I when it was recognized that seismic exploration might be rewarding in the search for hydrocarbons, the petroleum industry attempted to utilize the refraction method for its purposes, but the superior resolution and more convenient operation of the reflection technique quickly made it preferable. Thereafter, the reflection technique has been used almost to the exclusion of others in industrial work.

Until the 1950s, almost all of the effort was on dry land where it was, of course, applied to exploration of sedimentary basins, the habitat of most petroleum. Plate tectonics would eventually prove useful in the search for understanding of those basins, but the seismic observations of dry land basins that were available in quantity and quality by the 1960s were not a major factor during the early stages of the development of plate tectonics. Instead it was seismic reflection studies of sediments on the sea floor, including particularly the deep sea floor, that had an important impact.

Seismic reflection studies of the deep sea floor, it might be said, began in earnest about 1950. As a young graduate student of Ewing at that time, I was privileged to observe some early testing of a primitive version of the reflection technique in an area of the Atlantic near Bermuda. Small charges, perhaps 10 pounds of TNT, were fired just beneath the sea surface and recorded by shallow hydrophones suspended from the ship. What seemed to be reflections from within the sedimentary layer were observed in that early study, but they could not be correlated from the site of one shot to the next, because the shots in that preliminary attempt were too widely-spaced. Later Maurice Ewing, his brother John Ewing, and co-workers developed a method for detonating explosives thrown from the moving ship much more frequently, and they began to track reflecting horizons and get revealing seismic profiles of the deep sea sediments, often including reflections from what we now know to be the base of the sediments or the top of the oceanic crust. The entire sedimentary section was being probed.

This development was a bonanza for those studying marine geology of the deep sea. Other academic research institutions joined the effort. And, as interest in offshore petroleum flourished, the industry entered the field and developed the technique further and began to apply it to offshore basins, mostly in areas of shallow water. One major advance, adopted by both camps, was the replacement of explosives by compressed air "guns" as sources that were safe, reliable and highly repetitive. A new era began. Seismic exploration of the sea floor proliferated. With industry exploring the shallow water basins in great detail, and the research institutions probing the enormous deep sea floor and other shallow water areas wherever they could, a huge new supply of unprecedented and highly revealing information on the sea floor sediments accumulated rapidly. It was a time of historic advance in human capacity to observe an important part of the earth's interior.

Of course, and as is nearly always the case when a part of the earth is observed for the first time in a novel way, those who saw the new observations were surprised, fascinated, and challenged to interpret them so as to advance understanding of the earth. They sought knowledge of the history of the ocean basins and, of course, of tectonics of major as well as lesser scale. For one thing, the reflection data verified, though not without some agony on the part of the interpreters due to the inconsistent reflective character of the oceanic crust and sediments, the surprising thinness of oceanic sediments that had been revealed by refraction studies. Data from both kinds of studies also showed that, though thin, the sediments tended to increase in thickness with distance from mid-ocean ridges. It was another fact of considerable importance that provided incentive and support for the sea floor spreading hypothesis as it was being proposed. And the reflection data, when combined with information on the age of a particular reflector that could be sampled at outcrop by coring, permitted some quantitative measurements of rates of sediment accumulation.

On one point the reflection data unfortunately proved misleading, at least during the preliminary stages of the work; they gave false overall impressions to scientists studying them. The problem arose because both the technique and the base of knowledge were evolving. The technique of reflection profiling, as described earlier, developed gradually, becoming more sophisticated and more revealing with time. In the early stages the method worked well where the sediments were layered and flat-lying, but often it did not provide useful data where the sediments were deformed. Now there are many parts of the sea floor where the sediments, and especially the youngest ones, are indeed flat, that is, never deformed. Thus much of the early marine reflection data showed flat-lying sediments and little else, and certainly little evidence of deformation. Consequently, those who saw those data in broad perspective and entered the debate over permanence or lack of permanence of the ocean basins tended to favor permanence. Maurice Ewing, surely one of the greatest of earth scientists and one who, more than anyone I know, foresaw an upcoming revolution in geology, was caught in this trap. He initially objected to the spreading hypothesis partly because of lack of evidence on deformation of the sea floor and abundance of evidence apparently favoring non-deformation, and partly because of the appeal of models favoring permanence and the stature of the proposers of such models. Later as better evidence became available he recognized the difficulty, reconciled the disagreement, and adopted a more flexible position.

The difficulty is best illustrated by the seismic reflection data for the trenches where they were particularly tricky. The observations initially showed only the obvious flat-lying sediments in the trenches, suggesting that there had been little deformation there. Later, as the trenches and the process that forms them became better understood, it turned out that the flat-lying sediments are all very young and in the area of the trench not yet subject to deformation. Elsewhere in the trench area however, there is a great deal of severe compressional deformation of sediments, including young material, caught in the landward wall of the trench, the so-called accretionary wedge. That deformation could not be resolved by the technique in its early stages. Hence it initially appeared, ironically, that the trenches, now known to be located at the sites of principal deformation in plate tectonics, were stable. The seaward wall of the trench, meanwhile and on the other hand, revealed extensional features in the form of grabens. It would turn out eventually that the grabens were evidence in favor of the subduction process for formation of the trenches, but initially they were interpreted by some as indication of extensional origin for the entire trench feature. Subduction, which will be discussed in detail in later chapters, is a key element of the plate tectonics hypothesis, and eventually and as quality and resolution improved, all seismic reflection data in the trench areas were reconciled with the subduction model.

At present the seismic reflection technique continues as the predominant seismic tool for exploring the ocean floors and sedimentary basins at sea, and on land, and the results complement those obtained by sampling the sediments and crust under the Deep Sea Drilling Project. The seismic reflection technique is now also being used routinely to study the entire thickness of the continental crust and the uppermost mantle. In a later chapter I describe the initiation of the COCORP project which was designed specifically for this latter purpose. COCORP is modeled after, and is a consequence of, the successful application of seismic reflection profiling for exploration of various other parts of the earth during pre-plate tectonic days. COCORP uses explosive sources and also "Vibroseis," the truck-mounted vibrators referred to earlier. It is providing a new type of fundamental information on the deep crust of the continents and hence has become a continual source of new discovery.

One other artificial source of seismic waves, nuclear explosions, deserves further comment. Except in the Soviet Union where they have been used as sources of seismic waves in refraction studies of the crust and upper mantle, and in the U.S. where tests related to seismic detection of underground nuclear shots have been conducted within the context of nuclear test ban treaty negotiations, nuclear explosions have not been detonated specifically for the primary purpose of generating seismic waves. However, most nuclear explosions detonated for other purposes are detected by the standard seismograph network and hence provide seismic data, usually data with unique characteristics. One story about such data is worth telling, partly because of the scientific value of the study and partly for the wry humor.

When large nuclear explosions were being tested clandestinely during early phases of the cold war, precise locations and times of detonation, even the firing itself, were kept secret. Bullen, uninvolved officially with the testing, nevertheless collected seismographic data on the events because of his interest in checking the Jeffreys-Bullen travel time tables discussed earlier. He found that, using standard seismological methods, he could determine the location and the origin time of the clandestine explosions reasonably well from the seismic data alone. In fact, the times consistently turned out to be so near to the even minute that Bullen suggested publicly that the detonations were timed to fire then. This deduction, which was never verified publicly but which was almost certainly correct, was somewhat to the chagrin of the testers who obviously should have chosen a random and hence less obvious firing time if they wished to keep it secret. In any case, in the remainder of his study Bullen verified the J-B tables and he did so by using artificial sources of seismic waves in a novel and productive way.

As an aside, I note that nuclear explosions fired underground and in the atmosphere, the stratosphere, and the deep ocean all produced seismic waves of unusual character and hence of special interest to seismologists, but studies of them revealed nothing critical to the development of plate tectonics and hence they will not be discussed further here.

Next, with the preceding background information on seismology as a base, we turn to the stream of ideas and events that would provide the setting and the opportunity for seismology to contribute directly to the plate tectonics revolution.

4

Continental Drift and Sea Floor Spreading, The Forerunners of Plate Tectonics

The plates in dynamic mosaic
Through history both fresh and archaic
Like bold engineers
For some two billion years
Have kept earth from becoming prosaic.

Near the end of the 16th century, 1596 A.D. to be exact, Abraham Ortelius in Antwerp published the third edition of his Thesaurus Geographicus. For that edition he added a short passage to a section from earlier editions that discussed myths such as Plato's tale of Atlantis. That new passage contains a simple but truly great idea, the notion that the continents of North America, South America, Eurasia and Africa were once joined together and have since drifted apart, creating the modern Atlantic ocean in the process. The passage was apparently overlooked or disregarded by scientists, historians of science, and everyone else until it was pointed out in 1994 , nearly four hundred years later, in an article by James Romm (Romm, 1994). So far as we know at this writing, that publication by Ortelius is the first record of that profound idea in all of history. Even though others would have the same idea independently later, it seems quite reasonable to assume that Ortelius was indeed the very first to have it, because he was in an especially favorable position to be among the first to see the critical geographical information on which the idea is founded.

Abraham Ortelius was one of the leading cartographers of the 16th century, the age of great geographical discovery. As school children know, it was a time when, inspired by the voyages of Columbus and Vasco de Gama, by religious zeal, and by economic gain, adventuresome seafaring men sailed to remote and unknown corners of the earth. Somewhat like modern scientists probing the atom, or the universe, on the interior of the earth, those observers of previously unexplored geographical regions, like present-day scientists publishing in journals, brought their observations to a central collecting point for organization and distribution. There a cartographer collated them to produce a map. The role of the cartographer was, in a sense, analogous to that of the synthesizer or theoretician of modern science who assembles disparate observations, organizes them into a unified and self-consistent story, or theory, and publishes it.

Ortelius was just such a synthesizer. He was not so much a designer or drawer of beautiful maps or a deviser of map projections or styles, as he was an assembler of data of others into a consistent whole. He is best known for producing the first modern world atlas, Theatrum Orbis Terrarium, a very successful commercial and intellectual endeavor that was translated into several languages and effectively superceded the charts of Ptolemy which had prevailed as the standard world map for some thirteen or fourteen centuries! Ptolemy's maps, though a magnificent contribution to knowledge at the time they were drawn, nevertheless incorporated certain features based on myth or imagination for areas then lacking in observations, and hence were sometimes misleading and certainly in need of major revision by the time of Ortelius.

As an assembler of maps of localities and regions by various other cartographers into a pattern that was more or less consistent and realistic on a global scale, Ortelius was therefore in a position to be the first, or at least one of the very first, to see the initial, reasonably accurate, map of the earth's surface, or at least of the parts of the surface surrounding the Atlantic Ocean and critical to the great idea. Like many great scientific discoverers of the past and present, when he saw and comprehended the key data in appropriate perspective, the great idea came to him.

There is an important and encouraging lesson for discovery-minded young scientists here. Ortelius was clearly a wise and learned man, but there is no suggestion in any of his work that he merits the label of genius. His discovery was not a product of genius; it was a product of inspiration brought about by association with fresh new observations of an important object, in this case a large portion of the surface of the earth. Many, probably most, big discoveries of science are made in analogous fashion. A truly exceptional mind is not essential. Becoming associated early, or first, with important new observations is commonly the key to discovery. Those who seek to discover may enhance their chances for discovery by maneuvering into a position which provides such association.

There is a second lesson to be learned as well. Ortelius' magnificent idea, though published where it was available to all, apparently had no effect on much of anything, certainly not the course of science. The idea was either quickly forgotten, or unnoticed, or ignored. It lay hidden until Romm's paper was published hundreds of years later and well after the idea had been had independently by others and eventually exploited. The lesson? To have an important impact on science, an idea, no matter how good or how correct, must also be timely and must be communicated appropriately and forcefully, not merely buried on a library shelf somewhere. Even though scientists regularly strive, as they should, to be first with a great idea, it is nevertheless often the case that what comes to be widely-known as a great and original idea by someone was had earlier and in less timely fashion by others, and then overlooked. A great idea must be communicated to, and driven to the attention of, many others if it is to have an impact. This observation provides some justification, and might provoke some sympathy, for those scientists who at times might appear overzealous about making their own controversial ideas widely-known.

Ortelius may well have been the first to propose the concept of continental drift, but his achievement is otherwise not unique. Later, others, on seeing a reasonably accurate map of the Atlantic and the continents bordering it, conceived and recorded the same idea or related ideas (see Romm for further discussion). Still others surely had similar thoughts without recording them. None prevailed, however, or had any lasting effect until 1912 when Alfred Wegener, the great German meteorologist and astronomer, not only had and published the same idea once again, but also assembled a wide variety of data bearing on this spectacular and basic concept that would be called "continental drift." Wegener also, and wisely, took the time and trouble to communicate his results widely and enthusiastically. As a consequence of the combination of his idea and his efforts, he would eventually become known as the "father of continental drift," but not until after years of controversy.

Earlier, and certainly during the time of Ortelius, geological observations relevant to the concept of continental drift were so scarce that they lent neither support nor contradiction to the idea. But by Wegener's time, many of the geological observations that are now widely recognized as support for the concept of continental drift were known, and Wegener was able to make a case which included many valid and important points based on those observations. For an idea to become a part of science, it is of course essential that appropriate and sufficient observations for testing and lending support to the idea be available. Wegener's initiative was timely in this regard; Ortelius' premature.

Wegener took pains to make his case known through lectures and publications and his efforts did not fall upon deaf ears, but neither did they convince all who heard him. His ideas were accepted by some, few in number and definitely the minority, and rejected or relegated to a status of little importance by others, definitely the majority and including many prominent leaders of earth science. From the time of first publication in 1912 until the plate tectonics ferment of the 1960s, Wegener's concept of continental drift was a subject of controversy and debate, but during that interval, and particularly among North American earth scientists, it usually seemed to be losing stature because of certain flaws, and it had had little enough credence when first presented. Its basic simplicity and rationality, however, gave it staying power so that it was never lost. An anecdote based on my personal experience might illustrate the state of affairs at that time.

In 1947, when I was a graduate student in physics at Columbia University and in need of financial support, I took a job as research assistant with Maurice Ewing, a professor in Columbia's Department of Geology. Ewing was a geophysicist and the project I worked on concerned sound waves in air, so, strictly speaking, I had little contact with, and little need for any knowledge of, geology in that job. That was fortunate, indeed, for at that time I had never taken a course in geology and knew almost nothing of the subject. However, my colleagues in Ewing's group were mostly working on other projects that were of a geological nature, so I soon developed an interest in geology and a desire to learn something about it. As a first step I bought and read a book that I had chanced upon in the bookstore. It was written by George Gamow and entitled "The Biography of the Earth." Gamow, a physicist, tried in that book to tell the history of earth in a self-consistent manner and in a style that would appeal to nonscientists. I think it fair to say that it was his attempt at a plausible and interesting tale of how earth had evolved rather than a strict review of the state of earth science at the time.

Among other things, Gamow's book incorporated Wegener's story of the drifting continents and it left a neophyte like me with the impression that continental drift was an established part of earth science. Later, as I learned that my colleagues were working on projects designed to determine the answers to such questions as whether the ocean basins were young or old, or why trenches and mountain belts existed, I was secretly perplexed. It seemed that they were unaware that continental drift explained all these things. In fact, they never even mentioned the concept. However, as the greenest of new recruits to the subject, I was understandably, and I hoped tactfully, reluctant to draw this point to their attention. So I held my tongue and waited for my understanding of geology to grow. Still later, as I began to take formal courses in geology, I learned that the hypothesis of continental drift was indeed well known to geologists, but in those same courses I was also heavily indoctrinated in prevailing arguments against the concept. Thus as a consequence and at the time, I relegated continental drift and Gamow's book to positions of less prominence in my thinking. Like most North American earth scientists of that era, I joined the school of data collectors who felt that far better observations of earth were in order before anyone jumped onto the bandwagon of some particular hypothesis.

Collecting new observations turned out to be the right thing to do, of course, as it almost invariably does when controversy or uncertainty arises in science. At the time of Wegener's proposal of the concept of continental drift, and even more so in the decades that followed, there was considerable observational information available on the geology of the continents. But the geology of the sea floor, which covers more than two-thirds of the earth's surface, was very poorly known, almost unknown at that time. Consequently, because of scarcity of observations for almost all marine areas, some felt free to claim that the continents drifted through the sea floor; others felt equally free to claim that the sea floor was merely sunken continental crust and that it made no sense to claim that one piece of continental crust could drift through more of the same. Controversy there was, and it would be resolved by more and better data. Then World War II came along, stirred up the world and in the process indirectly stimulated efforts to observe the sea floor thoroughly and comprehensively. Eventually those efforts produced the new information on bottom topography, on sea floor structure and nature, and, especially, on magnetic anomalies over the oceans that would be critical to the development of the concept of sea floor spreading, a key step in the discovery of plate tectonics.

In the post WW II years as observation of the sea floor began to flourish, certain types of observations on land were important as well. The first sign that I sensed of revival and strengthening of the hypothesis of continental drift came in the 1950's as paleomagnetic studies of continental rocks by British (notably Irving, Runcorn and Creer) and various American paleomagneticists (among them Graham, Cox, Doell and Dalrymple) showed that directions of remnant magnetism varied with age of rocks at a single locality and also varied among rocks at multiple locations in just such a way as could be explained by drift of the continents within a magnetic field that held its position relative to the rotational pole. These early data were at the very least suggestive and they drew some renewed attention to Wegener's concept of continental drift, but as I saw it at the time, most scientists were skeptical. It was relatively easy for them to be that way because the observations were sketchy, sometimes challenged, and sometimes interpreted in other ways. Most scientists of the time were not sufficiently stimulated to reorient their work toward study of continental drift as a consequence of the new developments. Another personal anecdote may be illustrative of those times.

In 1954, just after completion of graduate study, I made a trip to South Africa for the purpose of installing a special type of seismograph. During that visit, a local, distinguished, and considerably older, professor of geology, Lester King, kindly invited me to his home for breakfast one day and then graciously took me on a tour of the local geology. On that day I was much impressed by King's knowledge of not only local geology but also of the geology of all of Africa and even South America. And I should have been impressed for I was to learn later that King enjoyed an international reputation in geological circles; senior professors at my home institution, Columbia University, knew him well.

Later, and on a day near the end of my visit to Africa, King took me to his research laboratory where he introduced me to his co-worker who, under his direction, was busy sliding plastic scale models of the continents around the surface of a globe and trying to fit them together in various ways. King was doing research on continental drift, a matter of some astonishment to me as I had already labeled him an astute and learned geologist and as I had been taught in the graduate school of a distinguished American university that continental drift was impossible. Of course, it was an error of judgment on my part that I now regret, and that I would apologize for if King were still alive, but at the time my indoctrination in anti-drift was so strong that my mistaken response was to forgive King tacitly for an idiosyncrasy rather than learn from him!

That same error would be repeated by almost every one of my colleagues at Columbia, for, on invitation, King visited us a few years later and carried on a formal public debate over continental drift with Walter Bucher, a senior distinguished professor at Columbia. King clearly won the debate, and in the process, although he had not convinced them, he stimulated members of the audience, which consisted mostly of students and faculty, to consider and to argue the matter of continental drift in the months that followed. However, I think it is fair to say that all, or almost all, of us eventually returned to our anti-drift leanings as those discussions waned in a half year or so. We would all change ,of course, a few years later as sea-floor spreading and plate tectonics arrived.

During the period of the debate and throughout most of the 1950s as best I can recall, when the subject of drift was raised or debated, earthquakes were not directly a part of the argument, pro or con. Seismic studies on land, and particularly at sea, based on artificial sources were growing in quantity and quality during this period, however, and their importance in the drift debate grew, but mostly, as indicated previously, the crude early seismic evidence was initially interpreted as support for the anti-drifters.

In the late 1950s, earthquakes became a part of the chain of events that would lead to the concept of sea floor spreading and hence ultimately to plate tectonics. At that time marine geologists Bruce Heezen and his co-worker, Marie Tharp, were engaged in a lengthy project to prepare a spectacular new physiographic map of the ocean floors (Heezen et al, 1959). It was probably the first such attempt anywhere to make such a map for the entire sea floor, and the project had only become feasible as the Lamont archive of ocean soundings grew. Furthermore, the physiographic map that showed bottom features only schematically and pictorially avoided the restriction through military classification of use of most sounding data for the deep sea that prevailed then. At that stage, the data were limited and spotty, of course, and often a certain degree of interpretation was required. Tharp, who did most of the actual plotting and drawing (and who hence, and perhaps like Ortelius, was often the first to see new portrayals of the collected observational data) noticed that segments of mid-ocean ridges commonly exhibited a central valley, or rift. Furthermore, she noted that the earthquakes that occurred beneath the sea floor often were located directly beneath such a rift. That, it would turn out, was a very astute and important observation. It became the basis for speculation by Ewing and Heezen and Tharp who promptly used the known location of seismic belts beneath the sea floor to postulate that the scattered segments of mid-ocean ridges observed by that time were actually part of a single, huge, globe-encircling ridge. It was almost twice the earth's circumference in length and it was soon likened, in overall appearance, to the stitches of a baseball. This result was an important step, for the existence of such a feature of global scale, tectonically active as implied by the seismic activity, meant that the process that was deforming the earth, or at least the sea floor, was also of global scale. Earth's tectonic features were not just a consequence of randomly located, local events; they were related through some mechanism that encompassed much of the earth.

The globe-encircling rift system postulated by Ewing, Heezen and Tharp received considerable attention from many earth scientists, among them Harry Hess, a Princeton professor with a penchant for off-beat and innovative hypotheses designed to explain geologic observations ignored or overlooked by others. Hess had a long-standing interest in marine geology. During WWII, he had surveyed parts of the Pacific and found and named guyots, the flat topped seamounts now known to be there in abundance. He had helped to measure gravity at sea in the Caribbean and he had proposed what became a very well-known, although now considered incorrect, hypothesis to explain the 5 km thick ocean crust that was being found by marine geophysicists almost everywhere beneath the deep sea. Hess proposed, plausibly but incorrectly, that it was a surficial layer of serpentinized peridotite derived from the peridotitic mantle below with the addition of water.

Then in the early 1960s, Hess (1960) hit the jackpot. He proposed what would become known as the sea floor spreading hypothesis, the idea that the sea floor was spreading apart at the ocean ridges, specifically at the rifts within those ridges, and that material was welling up from the mantle below to create new sea floor at the cracks that marked the rifts and hence the spreading centers. He proposed that the spreading continued until entire ocean basins were formed. This concept would become a major building block as the plate tectonics model was developed. Although, as is often the case when a great advance in science is made, others, particularly Arthur Holmes (1944), had had similar or related thoughts earlier, those ideas were mostly overlooked or treated as curiosities and speculation when they appeared. Hess' proposal was attractive and it caught on. It had substance, and it was timely because new observations of the sea floor that would support the idea were just appearing and because other scientists were independently beginning to think along related lines. Dietz (1961), for example, coined the term "sea-floor spreading" and published an influential and widely-read paper on the topic. Dietz based the paper partly on work by Drake et al (1958) which pointed out the parallel between sedimentary troughs of the modern continental margin and the paired geosynclines of the Appalachians.

The critical evidence in support of Hess' model came from the map patterns of magnetic anomalies at sea, and from measurements on layered volcanic and sedimentary rocks that revealed that the earth's magnetic field has reversed itself in the past and just when those reversals occurred. Vine and Matthews (1963), in a simple yet elegant hypothesis, used Hess' concept of spreading to show how these two kinds of magnetic information, the field reversals and the spatial anomaly patterns, could be related. The Vine-Matthews hypothesis became a great success and the sea-floor spreading hypothesis received a substantial boost. It began to get increasing attention from geoscientists, especially and primarily those working on geomagnetism. Initially, and except for the seismicity pattern that suggested global continuity for the mid-ocean ridge system, there was no direct contribution to the sea-floor spreading hypothesis from earthquake seismology, but that was soon to change as a consequence of a suggestion made by the prominent Canadian geophysicist, Tuzo Wilson.

Wilson was a big thinker who seemed always eager and compelled to seek grander meaning in the observations of the earth than did many of his fellow scientists. He had long been fascinated by problems of large scale tectonics and in the 1950s had proposed some provocative and well-known hypotheses relating to orogenic belts and island arcs, but those hypotheses were eventually rejected, at least in the form proposed at the time. As soon as it became known, Wilson took up the sea-floor spreading idea, focusing initially on one aspect of it in particular. He championed the concept of the transform fault, an explanation for the peculiar rectilinear offsets commonly found along the mid-ocean ridges. Wilson showed how such offsets and related observations might be explained as a consequence of spreading at the rifts. If the transform fault hypothesis was correct, the rectilinear pattern provided strong support for the sea floor spreading hypothesis. Then, in a paper on the subject (Wilson, 1965), he suggested a crucial seismological test of the transform fault hypothesis.

At that point in time, although earthquakes had been used to define the world-encircling mid-ocean rift system and other seismic data had provided relevant information on marine and continental geology, earthquake seismologists had not been much involved with, or concerned with, these events that would turn out to be forerunners of the plate tectonics revolution. In fact, most earth scientists in all specialties, except perhaps geomagnetism, were paying at best only casual attention to what was going on.

As an earthquake seismologist at Lamont Geological Observatory, where there were many colleagues in marine geoscience, I was aware of the paleomagnetic results at sea, the geomagnetic field reversals, Hess' proposal of sea-floor spreading, the Vine-Matthews hypothesis, and Wilson's ideas all shortly after they appeared, and I was casually interested, but like almost every other earth scientist I was not sufficiently moved or excited by the news so that I reoriented my daily efforts to move into this subject.

Wilson's perceptive suggestion for a seismological test of the transform fault hypotheses was made widely available to seismologists through publication, but it generally fell upon deaf ears and was passed over. However, one young Lamont seismologist, Lynn Sykes, recognized the opportunity and capitalized upon it. In so doing he not only provided critical support for the transform fault and sea floor spreading hypotheses, but he also stimulated his Lamont colleagues in seismology, including me.

Consequently, and also because of its facilities and archives, the Lamont program in earthquake seismology became a major factor as the sea floor spreading story evolved into the plate tectonics revolution. Three later chapters concern primarily papers by seismologists Lynn Sykes, Bryan Isacks and Jack Oliver and are a central part of this book. The studies were all carried out at what was then the Lamont Geological Observatory, now the Lamont-Doherty Earth Observatory, of Columbia University. As the environment at Lamont, and particularly interaction with other programs there, especially that in geomagnetism, was critical to the nature and the success of those seismological studies, the next chapter gives a brief description and history of that institution, selectively emphasizing those aspects of that history and that setting that are especially relevant to our story.

5

The Origin and Early Days of the Lamont Geological Observatory and Its Program in Earthquake Seismology

Doc Ewing, the head of LGO
When asked by a visiting CEO
Where he stored his ships
Between annual trips
Said "I keep them working at sea-oh!"

The seeds of Lamont were sown near the end of World War II when Maurice Ewing joined the faculty of the Department of Geology at Columbia University. The Department faculty had properly recognized the growing importance of geophysics in earth science then, and chose Ewing to provide strength and leadership in that subject. That decision turned out to be an exceptionally wise one, but at the time probably no one-faculty member, administrator, or even Ewing himself-foresaw the magnitude and scope of what it would lead to. When Ewing retired from Columbia about a quarter of a century later, he left behind a large and prestigious institution with an international reputation and some 400 employees, almost half scientists or students of science, facilities that included deep sea research vessels and laboratories outfitted to study a wide variety of topics in earth science, and scientific projects operating at locations scattered throughout the world.

Ewing was an extraordinary man of exceptional vision and bold and daring dreams. He was a superb scientist and a hard-working tireless leader who led by "perspiration as well as inspiration." He was a force to be reckoned with in earth science, and he was widely respected, and sometimes envied, as such. When he came to Columbia, at about the age of 40, he had already recognized, correctly, that the sea floor was the great frontier of earth science of that era. Aided by increases in both the stature and the funding of science following WW II, Ewing began to build, at the main campus of Columbia on Morningside Heights in New York City, a research organization with lofty goals, a distinctive style, a spirit of innovation, and a level of inner confidence born of sound strategic thinking and continual success.

A critical part of Ewing's philosophy was the recognition of the great importance of new observations of the unknown. He saw fresh observations as the key to discovery. He sought to observe the earth everywhere and in every way that he could, and he gave special attention to the relatively unknown sea floor. If he took a ship to sea for the principal purpose of doing seismic work, the ship was also outfitted with other kinds of equipment as well, and the scientific staff was called upon to core and dredge and photograph the ocean bottom, sample and measure parameters of the water column, take biological samples, record the geomagnetic field, and measure precise water depths everywhere along the ship's track. To him, time spent at sea not making all possible kinds of observations was time and money wasted.

Although nominally a marine scientist, Ewing never felt confined to study only the sea. When he found that he could apply some of his earlier experience with acoustic wave propagation in ice and in shallow water to make earthquake observations provide information on the oceanic and continental crusts, he began a major program to operate seismographs on land, and later on the sea floor, in order to acquire appropriate data. He was also the first, or among the first, to advocate the landing of a seismograph on the moon.

Ewing's inspirational style and leadership, his lofty goals, his adventuresome, challenging, and scientifically fruitful activities, and his ability to tap funding sources to support them, soon brought young scientists and students of science to his fold. Most of those who joined him were fortunate indeed, for they were trained and inspired to careers that would make them leaders of science in their own right. Among that early group for various periods of time, and in more or less the order in which they joined, were Joe Worzel, Frank Press, Nelson Steenland, Ivan Tolstoy, Milton Dobrin, Paul Wuenschel, Gordon Hamilton, Dick Edwards, Sam Katz, Bruce Heezen, Jack Oliver, Chuck Drake, Bill Donn, John Ewing, Jack Heacock, Bernie Luskin and Chuck Officer.

During the late 1940s, Ewing's fledgling group at Columbia quickly grew to strain the facilities and quarters assigned to him on the main Columbia campus. Room for expansion and new kinds of equipment was needed, and the seismographs required a site less noisy than the Manhattan location. Columbia recognized the value and the potential of the Ewing-led activity, and the competition for his services from other universities, and met his need by providing the use of a 100 acre estate, the former home of Thomas Lamont, some fifteen miles up the Hudson river from the main campus. In 1949, Ewing's group, which then numbered about a dozen graduate students and several technicians, moved from the Morningside Heights campus in New York City to the village of Palisades in Rockland County, and the Lamont Geological Observatory was formed with Ewing as Director.

Also moving to and joining the Observatory at about that time was a small group in geochemistry led by Professor Lawrence Kulp. It too would grow to produce important earth science and many distinguished earth scientists, and it became an important component of early, and modern, Lamont but its activities and its achievements are beyond the scope of this book on seismology.

Ewing encouraged, and was supportive of, expansion of Lamont activities into almost any area of earth science, so long as the effort promised to be of high quality. Programs blossomed at Lamont in areas such as micropaleontology and oceanography as well as various branches of marine geology, geophysics and geochemistry. Ewing's emphasis on observation resulted in large and unusual archives of such diverse and information-laden things as ocean sediment cores, seismic and acoustic soundings, magnetograms and bottom photographs. Seismograms from the Palisades seismograph station were archived, along with data from outlying stations and data marked for discard by non-Lamont stations and donated to Lamont instead. A complete collection of the WWSSN microfilmed seismic data referred to earlier was obtained. This huge data collection would turn out to be a critical resource that gave Lamont scientists an advantageous position when the sea floor spreading hypothesis arose and as the plate tectonics concept was being developed.

The foregoing emphasizes Ewing's insatiable drive to collect new kinds of observations about little known parts of the earth, but that emphasis should not be interpreted to mean that he or his organization failed to stress the analysis and the interpretation of those data. Ewing, more than any other scientist I ever encountered, foresaw that somehow a great revolution in earth science was on the horizon, and he strove mightily to cause that revolution. It is sadly ironic that, for the reasons cited earlier, he was not the one to have the key ideas that led to sea floor spreading and then plate tectonics, and that when those ideas did appear the early and incomplete observations of the time happened to suggest that those ideas were wrong.

Nevertheless, the contributions to earth science by Ewing were monumental and widely recognized as such by other scientists. In his career, he made major advances in, and progressed through, a series of topics ranging from global acoustic wave transmission in the deep sea sound channel, to turbidity currents, to fundamental differences between the continental and oceanic crusts, to the globe-encircling mid-ocean ridge, to earthquake surface waves in continents, the ocean basins and the earth's mantle, and to the effect of atmospheric disturbances on water waves, to name just some of his contributions. He was truly a giant of his time.

Ewing's early opposition to sea floor spreading and then nascent plate tectonics has somehow led some to infer that he tried to suppress support for these concepts by controlling the work or the publications of scientists at Lamont during the period when those ideas were being tested and developed. Of course, I cannot claim to have monitored all of Ewing's actions, but I would like to go on record here as noting that I find these statements or inferences surprising and contrary to what I observed. During that critical period, I was a senior staff member at Lamont as well as a faculty colleague of Ewing. As head of the earthquake seismology program and one of the founding group of Lamont, I was clearly a part of the inner circle of leaders. Furthermore, as later chapters of this book document, my work, and that of my colleagues and students, was certainly some of the most pro-plate tectonics science to come from Lamont. At no time, however, did that work or the corresponding publications encounter any interference, or barriers, or even negative comment from the Director of the Lamont Geological Observatory. Ewing was a strong and sometimes partisan leader who did not hesitate to demonstrate his opinions and the depth of his conviction, but he was too much the solid scientist driven by the basic truth of observation in science to oppose those truths once attention was drawn to them and the case made.

In fact, during my 24 years of association with Ewing, first as a student and then as a colleague under his direction, I often had occasion to consult with him and to seek his approval. Almost invariably his reaction to a suggestion or a proposal was positive. Nearly always he not only approved but he also encouraged an expansion of what I had proposed. I can recall only a single time in those years when I encountered opposition from him and that was when I clearly overstepped my authority and tried mistakenly to spend some money that had been especially granted to him for another purpose! And even then he did not get nearly so upset as I would have had I been in his position. I have often thought that his encouragement and his habit of a positive-but-do-more response to an underling's initiative was a key to Ewing's success as a leader and administrator.

Furthermore, for those who would picture Ewing as long a staunch opponent of plate tectonics, I draw attention here to a rarely cited paper, published in 1967 by John and Maurice Ewing, which suggested a slight modification of the sea floor spreading hypothesis shortly after it appeared. The modification had to do with a possible change in the rate of plate motion about ten million years ago and clearly implied an openness and willingness to accept and test the hypothesis at that very early date. I thus find myself in complete opposition to, and often somewhat astonished over, some statements that have been made and sometimes published about Ewing's supposed negative reaction and obstruction to certain developments in the plate tectonics revolution. Of course, it might be that Ewing treated some of his scientists in one way, others in another. However, I, and some others, think it is more likely that the conflicting views of Ewing are a consequence of different interpretations of his motives as he called upon Lamont scientists to justify thoroughly and in the best scientific fashion what they proposed to publish. Some saw this practice as unjustified and prejudicial opposition to a pet theory, others saw it as merely tough, hard, but nevertheless reasonable and good, science.

As in any organization, communication among the various groups at Lamont was, of course, for purely practical reasons, never ideal or complete, and it became less efficient as the Observatory grew. In the earliest years, the numbers of scientists and students were small, and regardless of interest or discipline, almost all worked in one building, the former mansion of the estate now known as Lamont Hall. However by the early 1960s, as the ferment over sea floor spreading and plate tectonics began, numbers had grown and additional buildings had become available, so the organization had subdivided into various groups, mostly along disciplinary lines, and they were spatially separated.

Thus the groups in geomagnetism, paleomagnetism, marine seismology, and earthquake seismology were no longer quartered in the same building, and communication among the groups, though fostered on a personal level by friendships, openness and common interest, was inhibited by the less than ideal spatial proximity. Often it took some time for news and excitement in one group to spread to others. Thus when the fervor over continental drift and sea floor spreading hit Lamont, it began with those working in some aspect of magnetism, and then spread to seismology and elsewhere.

More precisely, it was the high level of enthusiasm for those concepts that spread in that manner. The ideas, and the principles of the concepts were known about earlier by many at Lamont. We had all learned formally about Wegener's theory of continental drift, and some had even heard Hess propose the concept of sea floor spreading when he first presented it. However, we had somehow, I think, mentally assigned those concepts to a category that we might have labeled "interesting but supporting data less than compelling." Also in that category in our minds were other large scale tectonic hypotheses we had learned about from text books like Umbgrove's "Pulse of The Earth." Lake's idea, for example, of shear planes deep in the interior and intersecting the surface at the great arcs was one example.

Vening Meinsz's concept of plastic flow in the mantle to produce the trenches and their associated gravity anomalies was another. Generally we found such speculative hypotheses to be provocative and interesting, but they lay fallow somehow for they did not seem immediately relevant to our data and hence our work. The luke-warm nature of our interest in global tectonic hypotheses was soon to change.

Probably the first pro-drift scientist at Lamont was Neil Opdyke who worked in paleomagnetism. Unlike many of us who had done our graduate work at Columbia and then moved onto the scientific staff, Opdyke had arrived at Lamont by a circuitous route. Ted Irving, a distinguished paleomagneticist and an early advocate of drift, and I became acquainted on a bus during a geological field trip in California. On learning from me that Lamont was seeking a bright young paleomagneticist, Irving immediately recommended Opdyke, and I passed that information to Ewing who promptly hired him. At first on learning of him, I didn't know whether Opdyke was from Africa where he was working, or from Australia where Irving was working, or from England where Opdyke had received his Ph.D. under Keith Runcorn, another leading paleomagneticist and drifter, or somewhere else. Consequently, I had some concern over how Opdyke would fit into the Columbia University-New York City area environment. That concern vanished when it turned out to my surprise that Opdyke had attended Columbia as an undergraduate and, in fact, was captain of the football team there! The latter point was particularly to my chagrin as I had played on that same football team just a few years before Opdyke had done so. At Lamont, in addition to his solid observational studies on paleomagnetic stratigraphy, Opdyke was an early and influential voice in favor of drift, not a popular view at the time.

In addition to the group in paleomagnetism, Lamont had others working in geomagnetism. They observed the earth's field at sea along ship's tracks, and analyzed those data. Jim Heirtzler was the leader of the group that also included Walter Pitman, Xavier LePichon, Ellen Herron, Maurice Davidson and John Foster. Initially, and for years, the data on magnetic anomalies at sea were confounding, but a great moment of enlightenment arrived when it was discovered that one track showed that the magnetic anomalies were almost perfectly symmetric about a ridge. It was strong evidence in support of Hess' and Vine and Matthews' ideas of symmetric spreading at a ridge, and the Lamont geomagnetic group caught fire and hurried to interpret all of its unique set of marine magnetic data in that light and to advance the subject of marine tectonics in the process. They were spectacularly successful, as has been documented elsewhere (Glen, 1982).

Word of the remarkable developments in geomagnetism soon spread to the earthquake seismology group. John Foster, then a graduate student, visited me in my office one day to encourage attention from seismologists, and Heirtzler and his cohorts gave a private presentation of their results to several seismologists, including me, on another occasion. As we shall see, that presentation was a key factor as Lynn Sykes made the decision to pursue Tuzo Wilson's suggestion, or perhaps it was Wilson's challenge, on the testing of the transform fault hypothesis based on the data of earthquake seismology.

With that brief and cursory introduction to what was going on elsewhere at Lamont as background, we turn our attention now to a more detailed discussion of the earthquake seismology program there, where the studies that are a principal focus of this book were to be carried out.

One of the out-buildings of the Lamont estate when it was given to Columbia University was a spacious root-cellar, well buried beneath the soil for temperature stability. Shortly after the Lamont Observatory was initiated, the floor of the root-cellar was excavated to bedrock and a large concrete pier poured in contact with, and on top of, the Palisades diabase sill, a thick igneous rock formation familiar to all inhabitants of the New York city area because it forms the spectacular cliffs along the west shore of the lower Hudson River. The root-cellar thus was converted into the Lamont seismograph vault, and in a few years it became the site of one of the finest seismograph stations in the world. The Palisades station, as it was formally designated, had a variety of instruments that collectively measured three components of ground motion in several frequency ranges, and it was especially noted for the Press-Ewing instruments operating in the low frequency, or long period, segment of the seismic spectrum.

In those early days, Ewing was active in, among other things, research on earthquakes and, especially, earthquake-generated seismic surface waves. He provided inspiration, guidance, insight and, as usual, contagious enthusiasm. His prime partner in this endeavor was Frank Press who functioned as head of the Lamont earthquake seismology program, first while a graduate student and then as a young professor. Ewing, the intuitive veteran scientist with a capacity and a compulsion to wring the most from a given set of observational data, and Press, trained initially in mathematical physics but rapidly learning geophysics and geology, formed a powerful team that quickly made its mark in the world of seismology. Their specialty, which was built initially on Ewing's experience with dispersive elastic waves in ice and in shallow water, and on Press' facility with wave guide theory, was the dispersion of seismic surface waves. Such waves are guided by the earth's surface and the layers of generally low velocity associated with it, such as the oceanic water layer and the continental crust. Whereas others studying the surface wave "coda" had focused on the arrival time of the coda's beginning, Press and Ewing sought to understand the entire oscillatory train as a consequence of dispersion, the dependence of phase and group velocity upon period or wavelength. They were remarkably successful and soon became world leaders in this subject. They set the tone of Lamont research in earthquake seismology for much of the next decade or so, i.e. well into the 1950s.

As the graduate student of earthquake seismology next in line following Press, for a Ph.D. thesis, finished in 1953 and published in 1955 (Oliver et al, 1955), I followed the Ewing-Press lead and compared seismic surface waves crossing various parts of the Pacific with the Ewing-Press theoretical models of dispersion there. That study was solid enough, showing that large segments of former continental crust were not lying beneath the deep sea, as some who had proposed continental subsidence to account for the ocean basins would have had it, but by then the primary results were not particularly surprising or unexpected, so the paper had limited impact, and justly so. It might be instructive to note, however, that one secondary piece of information that was published but that I, and everyone else, promptly ignored, could have been a clue to the spatial variation in age of seafloor across the Pacific, and hence a stimulus for the idea of sea floor spreading well before Hess' suggestion of that concept. However, the evidence was either too subtle, or we too naive, to interpret it properly. The data showed that the bedrock surface was deeper in the western Pacific than in the eastern Pacific, a configuration now attributed to the greater age, and hence cooler temperatures and greater densities, of rocks more distant from the spreading center of the eastern Pacific. In other words, if we had been sufficiently insightful, we could have looked at those earthquake seismograms and found a clue that, with sufficient imagination, might have led us to generation of the great idea of sea floor spreading, a key component of plate tectonics. I make this point at some length to draw to the attention of young scientists that reliable observations, no matter how subtle, may carry a message of unanticipated importance and should not be dismissed or ignored without thorough consideration.

During the mid and late 1950s, the Lamont earthquake program that was founded on the Ewing-Press era of earlier years grew in size and scope. In 1955, Press left to take a position at Cal Tech where he became Director of the Seismological Laboratory. It was an early step in what became an outstanding career. He eventually moved from there to MIT and then to Washington where he was Science Advisor to President Carter and then President of the National Academy of Sciences. When Press departed, I was named to his vacated faculty position at Columbia and also became head of the earthquake program at Lamont, both positions that I held through mid-1971, i.e. through the time of the plate tectonics revolution.

The late 1950s and early 1960s were a time of great change in the nature of the earthquake program at Lamont. An important factor in that change was a new building that was constructed on the Lamont grounds for the earthquake seismology program in about 1960. In addition to more and better offices and laboratories, it provided spacious storage facilities for archiving seismograms. The new building was somewhat distant from Lamont Hall, the former home of the earthquake program and also the site of the director's office. Consequently, although it was certainly neither designed nor planned for by anyone, a net effect of the move of the earthquake group, plus concurrent increases in other activities of the Director, was that Ewing relinquished the day-to-day association with seismology that he had had when the quarters were adjoining, and new direction for the program began to originate almost entirely from other members of the earthquake group.

Though still rooted to a degree in surface wave studies, the program diversified markedly in the 50s and 60s as it moved into subjects such as model seismology, microseisms, nuclear test detection, microearthquakes, instrumentation development, and deployment of seismographs in such unusual configurations and locations as a network of 10 stations spanning the globe, a deep mine in New Jersey, the deep sea floor of the eastern Pacific, and the moon. The diversification was greatly aided, of course, by the new government funding that became available to seismologists through the space program, NSF expansion, and the need for basic research related to nuclear test detection. During that interval of expansion, the character of the earthquake seismology program evolved from a small, narrowly oriented operation that focused on a very limited number of topics to a much larger one covering many topics of great variety. Scientists, students and technicians in greater numbers and with greater diversity of talents and interests joined the program.

Lamont's reputation for innovative research in seismology attracted numerous graduate students of high quality. They came to learn, and they brought with them the vitality, energy and enthusiasm that fueled the program further. Each stimulated and influenced the others in numerous positive yet often intangible ways. Collectively they contributed to the setting that would make the Lamont contributions to plate tectonics possible. Many of those who carried out and completed their graduate work at Lamont then joined the staff as research scientists in newly added projects. One often hears it stated dogmatically that students who complete their undergraduate or graduate work in science at one institution should go elsewhere for graduate study or for postdoctoral research. That principle was violated regularly at Lamont, particularly in the case of graduate students, with little sign of negative effects and abundant indications of positive ones. In fact, the extraordinary espirit de corps of Lamont was at least partly a consequence of that practice.

Students who began their graduate study in one aspect of earth science often shifted emphasis to another aspect, or redirected their efforts through a spectrum of interests during their time at Lamont as student and then research scientist. George Sutton, for example, did seismic work at sea based on explosive sources, studied earthquake focal mechanisms and earthquake wave propagation, studied local earthquakes in equatorial Africa, and designed and participated in installation of unconventional seismographs at Palisades, on the moon, and on the floor of the deep sea of the eastern Pacific Ocean. Paul Pomeroy installed seismographs around the world, built an unusually successful instrument for detecting waves of long period, and studied waves generated by nuclear explosions in various environments. Jim Dorman, trained as a conventional geologist, learned geophysics, investigated aspects of seismic wave propagation and, when digital computers first appeared, became Lamont's expert in computer systems and applied that new capability to various aspects of seismology. Jim Brune studied wave propagation, microseisms, focal mechanisms, and microearthquakes, and developed an ingenious method for visualizing and analyzing the dispersion phenomenon. These former students, and many others, eventually left Lamont to take faculty positions at other universities but they left behind a legacy based on their accomplishments and spirit.

Also contributing heavily to the seismological program of that era were Jack Nafe, a Columbia-trained nuclear physicist who joined the geology faculty to pursue his interests in geophysics and who brought elegance, techniques, rigor, and depth of understanding of modern physics to the program, and a number of visiting scientists including Hans Berckhemer, Stephan Mueller, Inge Lehmann, Yasuo Sato, Bruce Bolt, George Thompson and others. John Kuo, initially a short-term visitor to Lamont, stayed to build an outstanding career at Columbia. Orson Anderson, a solid state physicist, used ultrasonic waves to investigate in an innovative fashion fundamental properties of certain materials of the earth. George Hade left a job in a local auto repair shop to become a superlative seismological engineer-technician who designed, tested and then installed seismographs at remote locations throughout the world, in the process handling difficult technical and diplomatic problems with equal aplomb and boosting the morale of those he encountered everywhere along the way.

Although the earthquake seismology program was administratively distinct, and physically separated from the quarters of, the marine seismology program, there was nevertheless ample interaction and communication between the two groups. The marine group carried out seismic reflection and refraction studies of the sea floor. It was led by John Ewing, Bill Ludwig, and Robert Houtz during the 1960s, but other Lamont scientists, some of whom held major administrative posts such as Joe Worzel, Lamont's associate director, and Chuck Drake and Jack Nafe, each for a time chairman of Columbia's geology department, found time to participate in the seismic work at sea.

The individuals named, plus many unnamed others, contributed not only expertise and energy directly or indirectly to the earthquake seismology program, but also they all added intangibly to the spirit and camaraderie of the Lamont group. There was sharing of interest, enthusiasm and vitality. Openness and congeniality about science, and almost everything else, was pervasive. There was some of the intra personal rivalry that characterizes science and on which science thrives, of course. However, any friction within the organization that resulted was typically resolved on the basis of what was best for the science. Few chose to miss even one of the Lamont parties which were numerous, open to all and always warm, cordial, and joyful. Lamont personnel worked hard and played hard. They took pride in doing the dirty and difficult kinds of jobs that involved ships and waves and trucks and sleds and dirt and ice and travel and midnight oil because the work was done for the higher purpose that was science. It was in this happily conducive but nevertheless demanding kind of setting that the Lamont seismological contributions to the plate tectonics revolution arose.

To organize those contributions to the plate tectonics revolution the following pages are divided into three chapters, each chapter focusing on a certain scientific paper but also reporting in some detail related papers and relevant other events of the time. The next chapter is centered on Sykes' seismological test (Sykes, 1967) based on Wilson's challenge in his paper on transform faulting. Using data on seismicity and on earthquake focal mechanisms, Sykes provided strong observational support for Wilson's model. The paper is a clear example of science by hypothesis testing and the chapter bears a corresponding title. The following chapter is centered on a paper by Oliver and Isacks (1967) which reports the discovery of the phenomenon of the down-going lithospheric slab in a convergent arc structure, a phenomenon that is the essence of the subduction process. This case is a clear example of discovery by serendipity and the chapter is so entitled. The next chapter to follow is essentially the climax of the book. It concerns the paper by Isacks, Oliver and Sykes (1968) entitled "Seismology and the New Global Tectonics." This paper is a clear example of science by synthesis and the title of the chapter reflects that view. It is a point worthy of some note that these three widely different approaches to science all produced results that are mutually consistent and were important, even critical, to the development of the plate tectonics revolution.

The message here for young scientists is, of course, that no one style of doing science is obviously superior or should be exclusive, and furthermore that science would be less effective if forced into any one such mode. I hope this point is made sufficiently clearly so that it will be noted by all peer reviewers! I shudder to think of how backward science might be if all research of the past had been confined, as some peer reviewers have erroneously recommended, to only projects for which the hypothesis is "clearly and explicitly" stated or the problem "sharply defined!"

6

Science by Hypothesis Testing

If it offsets a ridge, it's a transform
A geodynamical kind of a dance form
Twixt two plates that are mobile
In a pattern that's global
It can't be a tectonic chance form.

Most of the students who began graduate study in seismology at Lamont during the 1950s and early 1960s held bachelors' degrees in either physics or geology, or perhaps a branch of engineering. Few universities at that time gave degrees in geophysics or otherwise attempted to blend geology and physics at the undergraduate level. Thus most new Lamont students of seismology spent a part of their time remedying academic deficiencies in one or the other of these topics. In addition, most new students were inexperienced in research and had yet to make the transition from the learning mode of the classroom to the apprenticeship style of graduate study that called for full-fledged participation in all aspects of scientific research.

When Lynn Sykes arrived at Lamont as a new graduate student in seismology in the fall of 1959 he had no such deficiencies. He had B.S. and M.S. degrees from MIT with emphasis on geophysics, so he was already indoctrinated into the worlds of both geology and physics, and he also had supplementary training in mathematics and electrical engineering. Furthermore he had already spent a summer of research at Woods Hole Oceanographic Institution. He arrived, in every respect, ready to go into meaningful research. In classes, he quickly demonstrated the strength of his training and his outstanding ability and competence. He also demonstrated strong personal attributes as well. He had those intangible qualities that quickly made him outstanding as a researcher. In particular, he had a decided knack for finding and exploiting opportunities in science overlooked by others. Such a knack, cultivated or inherited, is a critical asset of outstanding scientists.

Lynn had chosen Lamont for graduate study after careful consideration of all of his opportunities. As an applicant with fine credentials, he was accepted for admission by graduate schools nationwide. He read journals and attended scientific meetings and found that papers by Lamont scientists were prominent and often predominant in the field of his interest. Then he visited Lamont and, I learned much later, was favorably impressed partly because I spent two hours of a beautiful Saturday in the spring talking with him about science and graduate study at Lamont. I tell this story not for self-adulation but to make the point by example and in the context of this book that success in science by an organization may depend as much on matters such as attention to recruiting as on more obvious factors.

Lynn began his research at Lamont, competently but rather straightforwardly, by following the style of seismology set earlier by Ewing and Press, as he carried out some very solid studies on seismic surface waves propagating over oceanic paths. During the course of this work, he noted, to no one's great surprise, that the locations of the earthquakes in the South Pacific that he was using as sources of surface waves were often greatly in error. Then, in trying to overcome this obstacle to better surface wave studies, he found that those location errors could be reduced by as much as an order of magnitude by using the reliable new data that had become available from the WWSSN and other sources in conjunction with newly developed computational techniques for hypocentral location. In so doing, he not only resolved a problem with the study that he was making, but he also recognized that he had found a technique that he could profitably extend to other areas.

To take advantage of the new opportunity, he began to relocate hypocenters in various parts of the world, particularly along the segments of the mid-ocean ridge system and then later along island arcs. The relocations employed a new computer program developed by Bruce Bolt who had foreseen the importance of newly developed digital computers in that aspect of seismology. It was yet another example of an important advance in technology having a near immediate impact on a branch of science.

Sykes soon found that, with the increased precision, he could find in various parts of the world new examples of the peculiar rectilinear patterns of ridges and fracture zones that were already known in selected locations elsewhere (but that had not yet been associated with the phenomenon of transform faulting). He wrote a series of papers that helped define such features in various remote regions of the earth ranging from the South Pacific to the Arctic, and in so doing added to knowledge of the overall configuration of the world rift system. Lynn (Sykes, 1966) also made a study of seismicity associated with the Tonga-Kermadec arc, and, among other things, showed that the deep shocks there are confined to a very thin zone, less than a few tens of kilometers thick. This observation was important for the Tonga-Fiji study described in the next chapter and will be referred to there. In the process of such studies, of course, Lynn developed a familiarity with the data sources and data archives, and the computing and processing procedures, that would serve him in good stead later as the plate tectonics revolution began.

As Sykes produced these early studies of ridges, and ridge offsets or fracture zones, he not only discovered and delineated those features but he also found something very peculiar, and, it would turn out, very significant, about the spatial pattern of seismic activity and its association with different parts of a fracture zone. To understand this important point, visualize an element of an idealized rift-fracture zone system (NGT, Fig. 4). Think of a slightly distorted cross, or mathematical plus sign, in which the lower half of the vertical line of the plus sign remains vertical but is offset somewhat, at the horizontal line, from the upper half. Now think of the two vertical segments as designating a north-south trending ridge that is offset at the unbroken horizontal line, which in turn corresponds to an east-west fracture zone. The east-west fracture zone is much longer than the offset between the two ridge segments and it continues to the west and to the east beyond the points of intersection with the segments of the north-south trending ridge.

Sykes found that the seismic activity associated with such a feature occurred along both of the north-south ridge segments, but only along the intermediate section of the fracture zone that lay between the ridge intersections. The extensions of the fracture zone beyond the ridge intersections (to the east and west in the example,) were almost completely inactive. That would turn out to be a key observation. The entire lengths of the huge fracture zones of the sea floor were, in other words, not seismically active; only the segments of the fracture zone between the ridge intersections were. The full significance of that observation was soon to be revealed.

In 1965, as a result of his interest in the seismicity of the area and his Lamont connection with the Tonja-Fiji deep earthquake program (discussed much more fully in the next chapter), Sykes was working in Fiji for a few months. While Lynn was there, Jim Dorman at Lamont read a paper written by Tuzo Wilson (Wilson, 1965) and published in a recent issue of Nature. The paper concerned the new class of faults that Tuzo was championing; they were called transform faults. In essence Wilson's hypothesis predicted that, for the fracture zone configuration just described, seismicity should follow just the ridge-intermediate segment-ridge pattern that Sykes had already found. There should be little or no activity along the extremities of the fracture zone. On the other hand, if the more conventional, transcurrent faulting, hypothesis was used to explain the offset, seismic activity should have occurred along the entire length of the fracture zone. That point was important and interesting in itself but there was an additional very important point.

The hypothesis predicted the focal mechanisms of shocks at each location along the ridge-fracture zone feature and a pattern for them different from that to be expected if the fracture zones were simply transcurrent faults rather than transform faults. Thus, if the ridges had been offset by transcurrent faulting along the fracture zone, the earthquake focal mechanisms should have the same sense of movement as the offset ridges suggested. On the other hand, if the offset ridge segments had never been together and were the loci of sea floor spreading, then the earthquake focal mechanisms should have the opposite sense of motion. This opportunity to use focal mechanisms in a critical test of an important new tectonic hypothesis came along just as the technique for determining focal mechanisms was evolving to the point where it could be very useful. Stauder at St. Louis had advanced the technique notably using WWSSN data and begun to produce reliable and informative focal mechanisms, although his results were not initially cast in the plate tectonics context. Brune at Lamont had used the technique in yet another context, and Isacks at Lamont, as described later, was focusing attention on focal mechanisms of, particularly, deep thrust earthquakes in Tonga-Fiji. Sykes' attention was directed primarily toward the mid-ocean shocks that turned out to be extensional or transcurrent in nature.

Dorman wrote a letter to Sykes calling Wilson's paper to his attention and Lynn quickly recognized the opportunity. He saw how to test Wilson's transform fault model rigorously. If, with the new WWSSN data, he could improve the reliability of the focal mechanisms of earthquakes as he had improved the locations of hypocenters, he could test Wilson's hypothesis not only with the seismicity pattern but also with the focal mechanisms and their spatial patterns. The idea was not only good; it was timely. At the time, as was the case for the sea floor spreading concept in general, Wilson's hypothesis of transform faulting was, I think it is fair to say, being taken somewhat lightly by the bulk of the scientific community. Some cynical scientists even expressed disdain for anyone who treated it seriously enough to work hard at testing it.

Lynn must have had some misgivings and an interval of indecision over whether he should launch a full scale effort to test Wilson's model exhaustively in the face of some guaranteed criticism. Furthermore, he was already engaged then in a study of seismicity and deep structure of island arcs (Sykes, 1966). At that critical point however, and as noted earlier, Lynn and I and one or two other seismologists were invited to the quarters of the geomagnetism group at Lamont by its leader, Jim Heirtzler. Jim and his cohorts gave us a very enthusiastic and exciting briefing on the work his group was doing on the spectacular symmetric magnetic anomaly data along the ocean ridges. The case for spreading as postulated by Hess, and associated magnetic anomaly formation as postulated by Vine and Matthews, was being supported sensationally by the geomagneticians. That meeting convinced Lynn that sea floor spreading had to be taken seriously and that the seismological test of Wilson's model was an important thing to do and worthy of an all-out effort.

To make a short story of a long and challenging task, Lynn made that effort and Wilson's transform fault model was supported in timely fashion. Sykes' data-laden paper was just what Wilson's idea needed at that time when it was being dismissed or downplayed by some as arm waving speculation. The paper was presented orally by Lynn at the NASA Goddard Symposium in New York City in 1966 and published in JGR (Sykes, 1967). Sykes' paper, and figures from it, became widely known. It was publicized heavily as support for his ideas by Wilson among others of course, and the sea floor spreading hypothesis of Hess received a much needed boost.

At the NASA-Goddard meeting, Sykes met Bullard, Vine, and Mckenzie for the first time, and the Lamont magnetics group and Mark Langseth of Lamont gave talks bearing on sea floor spreading. Lynn wisely published a version of his talk in JGR (Sykes, 1967), in addition to another version in the symposium volume which was much delayed by late submission of other papers. Had he not done so, his work might well have been preceded in print by the work of other seismologists who were beginning to recognize the opportunity for them in the early stages of plate tectonics.

In correspondence with me, Lynn has noted meetings or associations of some sort with Griggs, Urey and King from outside Lamont and Ludwig, Opdyke, and the magnetics and seismology groups within Lamont that aided or encouraged him in his plate tectonics-related studies.

To summarize then, Sykes' seismological contributions at this early stage of development of the global tectonic story in essence included the following:

a) recognition with others that precision of location of hypocenters and of focal mechanisms could be much improved through use of modern WWSSN data and newly developed computing procedures.

b) demonstration that the spatial distribution of seismicity supported Wilson's transform fault model and not the more conventional transcurrent fault model

c) demonstration that the pattern of focal mechanisms and individual focal mechanisms supported Wilson's transform fault model as well
d) establishment of seismological methods as a valid and important means for testing and contributing to the development of new tectonic models being proposed at the time.

In demonstrating the much improved reliability of focal mechanisms and their utility in the search for improved understanding of tectonics when WWSSN data were used, Sykes validated Hodgson's concept of studying globally distributed focal mechanisms as a way to investigate tectonics on a large scale, and made clear that great care had to be taken to insure that the raw data were reliable. Other seismologists, particularly Stauder and Bollinger at St. Louis University, had by this time been working on a similar tack and were also obtaining reliable focal mechanisms. However, they were not so much in the early mainstream of communication on sea floor spreading and global tectonics as were the Lamont people and so their recognition of the significance of their results in this context lagged somewhat, a situation also experienced elsewhere by others. There were significant delays from place to place in the transmittal throughout the world of science of the exciting new information that would eventually grow together to become plate tectonics, and any particular individual's contribution depended to a degree on just when new information happened to reach the individual. Being in the right place and being there at the right time were indeed important. This point will be stressed further in a later chapter.

One other point needs emphasis here. In retrospect, and to a newcomer to the science, what Sykes accomplished seems rather straightforward and the obvious thing to have done. Wilson proposed a hypothesis; Sykes tested the hypothesis. What happened seems just the next logical step in the progress of the science at that time. And in fact it was. But the next logical step of the science is not nearly so easy to see when the actual events are taking place as it is when those events are viewed in retrospect. Wilson published his idea in Nature, a journal widely distributed internationally and seen routinely by a large fraction of the scientific community. Yet no other seismologist recognized and pursued the opportunity as Sykes did. Lynn had the good fortune to (a) be working with the data and techniques that were necessary to solve the problem and (b) to be located where up-to-date communication on new developments in the science was maintained. Young scientists seeking to make important discoveries of their own might note these factors as they design their own careers. But they might also note that a certain degree of vision and daring was required of Sykes as he broke with convention to make a noteworthy contribution.

By the same token, we must note that not all important advances in the science are made in like manner. We now turn from our example of science by hypothesis testing to an example of science by serendipity. As we shall see once again in the next chapter, sound strategic and tactical thinking are important but so too are fate and good fortune.

7

Science by Serendipity

The reasoning style was induction
The data, a deep quake production
With waves much less stronga
At Fiji than Tonga
Eureka! A thing called subduction!

Aware of the exciting advances in the sea floor spreading story being made by Lamont geomagneticists and others outside Lamont, and with an insider's view of Lynn Sykes' positive seismological test of Wilson's transform fault hypothesis as that test developed, other Lamont seismologists began to pay increasing attention to the evolving tectonic story. But, I think it is fair to say that, although it is clear in retrospect that it was a time of special opportunity, beyond some extensions of Lynn's early work no one at first saw quite how to devise and embark on a major and radically different new seismological project designed specifically to take advantage of those exciting new developments and opportunities in large scale tectonics. We managed to do just that, but it was partly because fate and fortune intervened and gave us a helping hand.

For it would turn out that the project that would produce an important new advance in the evolving tectonic story had already been initiated for another, and what seemed at the time a separate, purpose. It was an effort that I shall refer to here as the Tonga-Fiji deep earthquake project. The Tonga-Fiji project was begun in late 1964 strictly for the purpose of observing and understanding deep earthquakes. It was conceived on the basis of sound reasoning about the state of knowledge of the earth , and on a strategy rooted in observation of the unknown, but it was not begun with the specific intent of testing, or collecting information relevant to, the new ideas of ocean floor dynamics. Happily however, and in timely fashion, the Tonga-Fiji project produced striking and unanticipated new evidence on the structure and dynamics of island arcs and hence on the process that would come to be recognized and known as "subduction." The Tonga-Fiji project also brought the concept of mobile lithospheric plates into the story of earth dynamics. These new ideas on what happened at the great island arcs and other major arcuate features, the sites of most deep earthquakes, would then be blended with the concepts of sea floor spreading, transform faults, the dynamic mosaic of lithospheric plates, and related ideas to become the concept of plate tectonics.

This chapter hence focuses on the Tonga-Fiji deep earthquake project, on the events and people that gave birth to the project and subsequent studies based on it, on the strategy and reasoning that led to the project, and on the serendipitous consequences of the observational program that made the project a part of the plate tectonics story. In another book (The Incomplete Guide to the Art of Discovery) written for another purpose, I chose to use the Tonga-Fiji project as the prime example of a case history of discovery. Readers with a special interest in how discoveries occur, or in another view of the Tonga-Fiji project, may wish to refer to that book (Oliver, 1993) as well as the following. Here I retell the story with a somewhat different perspective, trying particularly to set it appropriately within the flow of seismological contributions to the plate tectonics revolution.

As noted earlier, during the 1950s and 1960s the Lamont earthquake seismology program that was rooted in the early Ewing and Press studies of low frequency surface waves expanded aggressively into new subject areas. Seismic waves of ultra low frequency, and ultra high frequency were studied. Large earthquakes, small earthquakes, weather disturbances, and sometimes artificial sources generated the waves. New kinds of instruments were developed and deployed at various locations on earth and eventually on the moon.

Young seismology students entered the field, learned how the science operated, and then caught the spirit of seeking opportunities in seismology that had previously been hidden, or ignored, or overlooked. To those of us who were caught up in the bustle and swirl of activity, it was a marvelous time, and it was made so because we somehow had acquired an inner confidence that we were working in a fertile subject in which dedication and hard work would reveal important and hitherto unknown information about the earth, the domicile of humans. We had all been infected by the bug of earth science, probably by Ewing, and we reveled in the disease it gave us.

That period of the 50s and 60s was also a particularly good time for innovative science in general. Science was full of vitality, funds for support of imaginative and often risky projects were available, and the bureaucracy of funding sources was neither entrenched, nor stereotyped, nor self-protective. A sound and imaginative project that explored new scientific horizons had a good chance of being funded and, once funded, such projects were carried out with a level of dedication and pride and unselfish drive and persistence that stood out.

Some projects that were organized under, and nominally a part of, Lamont's earthquake program spilled out into topics beyond the strict realm of earthquake seismology. During the International Geophysical Year (1957-58) which brought into existence the Lamont worldwide network of Press-Ewing seismographs referred to earlier, Lamont also supplied some personnel for the remote scientific field station on T-3, the ice island floating in the Arctic Ocean. As I had had some previous experience in Arctic science, I was given the task of finding and hiring appropriate scientific personnel for that station. The work and the living conditions at that isolated post were certainly not routine. There was the difficult environment, all snow and ice. There was limited companionship and danger of cabin fever. And the runway for planes that delivered personnel and supplies was built on snow that made it unsuitable for use during the summer. Personnel were flown to the station during late spring and had to remain until early fall. Obviously not everyone would or could work effectively under such conditions. Individuals with some unusual personal traits as well as appropriate skills had to be sought.

For the summer of 1957, I had hired a scientist for that field position well in advance of the departure date, but as that date approached he reneged, and I had to find a last minute replacement. With no other applicants for the Arctic job, I chose the best, as yet unhired, candidate for summer work elsewhere at Lamont. He was a junior in Columbia College, majoring in geology and physics. He had spent three summers doing seismic reflection work in the steamy swamps of the Gulf Coast of Louisiana, but had no previous Arctic experience and so was marginally qualified on that score. Nevertheless, a professor who had had him in class spoke highly of him, so I called him at home in Louisiana where he had gone following the end of spring semester. It was a Thursday afternoon. In a few words, I described the project and the job, the trying conditions and the remote location, and the enforced isolation, and then asked if he wanted the job. There was a pause of a very few seconds while he pondered this situation that was completely new to him, and then, decisively he responded "Yes!" "All right," I said, "Be here on Monday morning, ready to go." He arrived on schedule, adjusted quickly to his new lot, departed shortly thereafter for T3, and, fortunately for both of us, spent a successful and rewarding field season on the ice island.

I tell this story at some length because that individual was Bryan Isacks who, after completing that summer in the Arctic and his senior year at Columbia, became a graduate student and then a research scientist at

Lamont and eventually played a key role in the Tonga-Fiji project and most of the other Lamont seismological contributions to plate tectonics. His name will appear frequently in the following and, in fact, that phone call began a personal association between us that has prevailed to this day and through employment at two institutions, Columbia, and Cornell where Bryan is currently Chairman of the Department of Geological Sciences and I am a Professor Emeritus. When I see him now, I often think of how different the lives of both of us and many others would have been if he had said "no" instead of "yes" to my outlandish last minute offer of summer confinement and adventure on the frozen Arctic Ocean. Fate, once again, was kind.

Both Bryan and I followed somewhat tortuous paths from that first phone call to eventual collaboration on the Tonga-Fiji project. Let us now turn our attention to how that project began.

During the mid 1950s, my personal research efforts were partly devoted to study of surface waves in the continents, particularly the propagation of higher modes of guided waves. The results were, I think, some solid and lasting contributions to the science of seismology, but as such they got the attention of seismologists and few others, certainly not the general public or the inhabitants of the political world. Unexpectedly that degree of isolation of seismologists from other elements of society was soon to change.

In the late 1950s, while studying seismograms from the Palisades station, I found an indication of some surface waves that had been generated by an underground nuclear explosion in Nevada. It was the first time such waves had been seen at such a large distance from a rather small, buried, nuclear source. I soon found myself catapulted into the political world where interest in a nuclear test ban treaty had been aroused and intensified. As a consequence of that interest, those few seismologists with experience related to monitoring clandestine nuclear explosions were thrust into positions as technical experts at the treaty negotiations. Geneva, the meeting place for international diplomacy, became the new center of activity for a small number of seismologists from countries of the East and the West.

It was heady business for awhile as one sometimes got the impression that the future of the world depended on whether seismologists could distinguish an earthquake from an explosion, but after a few years of experience with intense political bickering and little to show for it, I yearned to return to spending a larger part of my time on fundamental scientific research. Therefore I began to cast about in search of a new activity to which I could devote a part of my time. Fortunately, the situation was such that there was no need for haste in making a decision about just when to begin or just what aspect of science to pursue. There was, in other words, an unusually good opportunity for developing a project based on a well thought out, sound strategy. I gave the matter very careful and prolonged thought and eventually hit upon the subject of deep earthquakes as one that looked promising. There were two reasons for this decision, one strategic, one practical.

Strategically, deep earthquakes seemed a prime and timely target. That deep shocks occurred and that they occurred in zones dipping beneath arcs to depths of about 700 km was known, mostly from work by Japanese seismologists led by Wadati. Except for some speculative hypotheses however, almost nothing was known about the nature of deep shocks, or why deep shocks occurred where they did. Furthermore, deep shocks were so prominent and so frequent that they had to be important components of the seismological and tectonic stories of earth dynamics. In addition, past observations of deep shocks were relatively sparse and spotty so that it seemed the subject was ripe for an observational program. In typical Lamont style, we could set out to observe them thoroughly, even though there was no particular hypothesis to be tested.

On the more pragmatic side, there was good reason to think that a sound project to study deep earthquakes could be funded. Solid earth scientists had realized the advantages of international cooperation during the IGY (1957-58) and they sought to maintain and extend those advantages through a sequel to IGY called the Upper Mantle Program. It was designed to stimulate earth scientists to focus their attention on that poorly known part of the earth, the upper 1000 km or so. The existence of the internationally fostered project caused the National Science Foundation to look favorably on proposals for studies of phenomena of the upper mantle, and, of course, that was just what and where the deep earthquakes were. In fact, they were most of the modern action in that part of the earth and hence made an appealing target.

At Lamont, we bandied the idea of a project on deep earthquakes about and then held a graduate seminar on the subject to explore it further. The style of the seminar was intensive; each student was required to read all of the assigned papers and come to class prepared to lead the discussion and critique of them. They (and the professor!) thus developed their ability to make a hard and tough evaluation of the literature and to formulate and select ideas for future research. I cannot recall the names of all of the handful of attendees, but both Lynn Sykes and Bryan Isacks were among them. Lynn was stimulated by what he heard in the seminar to extend some of his studies of hypocentral locations to include more deep events. He was also an enthusiastic supporter of an observational effort designed to observe deep shocks more thoroughly, but at that point he was not in a position to commit a large portion of his time to a field project.

Bryan Isacks was also enthusiastic about the project and, indeed, could devote himself to it and, it would turn out, did. After completing the Arctic field work and then finishing college, Bryan had entered graduate school and begun work in seismology. Among other things he participated, together with George Hade, Lamont's all-purpose seismological engineer, in the installation and operation of instruments at a Lamont station in a deep mine of the New Jersey Zinc Company in Ogdensburg, New Jersey. The mine observatory was designed initially to accommodate strain meters and seismographs for the purpose of studying earth tides and seismic waves of ultra long periods. Bryan's efforts there took another tack as he observed and studied waves at the opposite, high frequency, or short period, end of the spectrum. As a result, his Ph.D. thesis focused on small, local earthquakes and on the peculiar, high-frequency, compressional and shear waves traveling through the earth's upper mantle between Caribbean earthquakes and the mine in New Jersey. Such waves, which had been discovered and studied by Ewing and others a few years earlier, would turn out to be of special significance during analysis of the Tonga-Fiji data. Fate, it seemed, or at least coincidence, was at work once again.

As the plans for it crystallized, the Tonga-Fiji project of course called for tactical decisions as well as strategic ones. A site had to be chosen for a detailed observational program. After considerable deliberation, we picked the Tonga-Fiji area, principally because of the high level of deep earthquake activity there, but also because the political and logistical situation was favorable, and because there had been little if any previous seismological observational effort there. It seemed a place where a large amount of data could be collected in a reasonable time. We decided to run a small network of seismograph stations in that area, with each station recording three components of ground motion in a high frequency range that was selected because the shocks would all be relatively close to, say within 1000 km of, the recording stations. The project was funded by the National Science Foundation as part of the US Upper Mantle Program. The task of installation of the network on the islands of Tonga and Fiji followed.

Installing unusual and unfamiliar scientific instruments at remote locations in foreign countries where such activities are non-routine is no simple matter of course, and various practical and logistical problems arose. They were eventually resolved, principally by Bryan Isacks who, with the same verve and determination that had earlier taken him to the Arctic on the spur of the moment, moved his wife and three small children to Fiji for more than a year. From that base, and with intermittent help from George Hade, Lynn Sykes and others, he arranged for and installed a reliable seismograph network that spanned the countries of Tonga and Fiji.

Two stations, principally because of their locations, would turn out to be critical. One was located on an island of the outer arc of Tonga, not far from the deep Tonga trench. The principal seismic activity of Tonga, like that of most island arcs, occurs within a zone that outcrops near the inner wall of the trench and extends to depth while dipping at a fairly steep angle (about 45°) beneath the island arc and in a direction normal to the trench i.e. to the west in Tonga. Earthquakes occur throughout the zone which is thin, perhaps only a few tens of kilometers thick as shown by Lynn Sykes, (Sykes, 1966) and which extends from the surface to depths of about 700 km. Seismic waves from earthquakes in that zone, including the very deepest shocks, therefore traveled obliquely up to the Tonga station, traversing, or passing near to, the seismic zone for almost the entire path length.

The other critical station location was in Fiji where the distance from the hypocenter of a very deep earthquake was about the same as it was from that same hypocenter to the station in Tonga. The Fiji station, however, recorded waves that traveled obliquely through an aseismic portion of the earth's mantle. The geometry and lengths of the paths to the two stations, in other words, were nearly identical. The difference was that one path traversed through or near the seismic zone; the other path traversed an aseismic and hence more normal part of the mantle.

As the project began and before the data began to accumulate, the distinction between the two kinds of paths based on presence or absence of seismic activity did not get much attention. Seismologists of that era were accustomed to thinking of the earth as spherically symmetrical so that variations of velocity and other parameters might occur with a change in depth, but not with a change in lateral position. It was the time of what some have referred to as the "layered onion" structure of the earth, with each layer of the onion different from the layer above or below it but lacking in lateral differences within the layer. There was good reason to hold this view, at least as a first approximation, because, as noted earlier, seismic wave travel times are almost entirely a function of distance and they are almost independent of location. Hence the spherically symmetric view of earth prevailed, even though it was perfectly obvious that there was great lateral variation in one particular deep earth phenomenon, earthquake activity. Most parts of the earth had no seismic activity at depth, a few places had continually recurring shocks through a range of depths.

To reconcile such an obvious observation with the spherically symmetric model, the differences in seismic activity were, often implicitly, attributed to variations in dynamic response to stress, and not to variations in composition or temperature or elastic properties or some other factor. At some point during the evolution of the Tonga-Fiji project, I had the hunch that the deep earthquakes were occurring where they did because the material of that part of the earth was somehow anomalous and unlike the mantle at comparable depths elsewhere, but the project was not begun with such speculation as the basis. Furthermore, in keeping with the style of seismology that prevailed at the time and in which seismologists focused attention on spatial variations of velocity and little else within the earth, when we considered the possibility of an anomaly, we guessed, or tacitly assumed, that any seismic anomaly that manifested itself in the Tonga-Fiji region would be an anomaly in velocity.

That guess was off the mark. A velocity anomaly was indeed eventually discovered, but the revelation about earth structure that followed was based not on subtle spatial differences in velocity but rather on blatantly obvious spatial variations of another seismological parameter, attenuation. Attenuation of seismic waves in the earth is typically difficult to measure precisely, so seismologists of that era had often bypassed the subject or given it little attention at best. In Tonga-Fiji, gross spatial differences in attenuation were huge and robust and unexpectedly easy to measure; those differences in attenuation became the critical clue that led to the understanding of a major component of the plate tectonics story.

Bryan Isacks was the first to recognize the unusual nature of the new observations. Like Abraham Ortelius and Marie Tharp, Bryan, living and working at the central collecting point for data from the entire Tonga-Fiji network, was the first to see the new data. Seismograms of the frequent deep shocks in Tonga-Fiji quickly revealed to him the startling, unanticipated, and critically important piece of information. High frequency shear waves from deep shocks traveling up the seismic zone to Tonga were often as much as three orders of magnitude larger in amplitude than the same waves traveling up the path to Fiji that was comparable in distance but aseismic! High frequency shear waves were being attenuated severely along aseismic paths but propagated efficiently through, or near to, the seismic zone. It was a striking contrast in observation for a science in which a difference of only a few percent in some parameter was often sufficient to merit intense study. We were delighted with that new piece of what had to be important information on the earth, but it was many months before we came to recognize the full meaning of it, and just how important it was.

Meanwhile, new observations and coordination of our data with other kinds of observations improved our knowledge of the phenomenon. The zone of low attenuation included the seismic zone and a hundred, or at least some tens of kilometers, beneath it. The zone not only contrasted strongly with its surroundings in terms of attenuation, but it was also of slightly higher velocity than its surroundings. Background information from stations throughout the world told us that high seismic attenuation of high frequency shear waves in the mantle was the case most everywhere and hence that it was the seismic zone of Tonga that was anomalous. It was the path from the deep shocks to Fiji that was typical.

We puzzled for months over the meaning of our new data until at last the moment of enlightenment arrived abruptly; the so-called "Eureka phenomenon" that has been reported by other scientists in the case of surprising discoveries, struck us. It happened as Bryan and I sat in my office at Lamont staring at a sketch on the blackboard that showed the seismic zone of anomalously low attenuation dipping beneath Tonga-Fiji. Why was it there? And what was it? Then, searching for a clue, I said "The attenuation properties of this zone are somewhat like those of the shallow mantle between the Caribbean and Palisades" (as Ewing and later Isacks had observed it). "Why don't we assume that the mantle beneath the Pacific east of Tonga is the same as it is in that part of the Atlantic. Then we could draw it like this." And I drew the now familiar figure showing a horizontal layer of oceanic crust and mantle that bends in the vicinity of the trench and then dips beneath the arc and follows the seismic zone to great depths. Almost before I completed the new sketch, Bryan said "Of course. It's underthrust!" We sensed immediately that we had the answer and that it was a big one. The rocks that underlay the normal deep sea floor to a depth of about 100 km or so were descending as a layer into the interior in the vicinity of the trench-island arc system. Furthermore, they could be traced to, and hence must have moved to, at least a depth of 700 km. It was underthrusting on a huge scale! Suddenly, as this model became clear, all sorts of things began to fall into place as we envisioned a new kind of dynamics for island arcs. We were filled with excitement and euphoria as we began to recognize that we had the key to a remarkable range of important problems of earth science.

For example, at that time a major enigma had arisen as a consequence of the growing success of the sea floor spreading concept. If, as the spreaders claimed, the sea floor was parting to make room for material from below and new surface area was being created at the ridges, how was the entire earth responding so as to accommodate the new surface area? Some claimed that the earth was expanding at just the proper rate to provide the needed increase in surface area. To account for the expansion, they challenged the constancy with time of the gravitational constant. Others claimed the earth was not expanding but that surface material was being destroyed elsewhere at just the rate that it was being created at the spreading centers.

But among this latter group there was disagreement over just where the surface material eventually descended into the interior. Some said it sank beneath the continents through some poorly understood mechanism and in a spatial pattern that produced the observed deformation of surface rocks there. Others, perceptively and more or less correctly, thought the deep sea trenches were the places where surface material was descending into the interior. Some, like Holmes, had earlier visualized great convection cells in the mantle that brought material up at the ridges and carried it down at the trenches. But none, so far as I know, at that time visualized the prominent role of the strong, near surface layer of lithosphere in the process, nor sensed clearly the full set of relations between that down-going lithosphere and other phenomena of island arcs-the trenches, volcanoes, earthquakes, etc. Our model, with its down-going underthrust slab of lithosphere associated with the arcs quickly began to serve as the basis for explanation of these phenomena. It subdued the earth expanders and the non-arc sinkers, and provided a specific mechanism and model for what was happening at the arcs that became the foundation for almost all subsequent studies of arcs and a critical element of global tectonics.

For us, it was a major conjectural step to go from evidence on the spatial variation of attenuation and elastic properties in the earth to a mechanical model with huge slabs of strong lithosphere moving about on a weak, viscous asthenosphere i.e. a model based largely on rheological properties. There was some intuition and speculation involved of course, but the reasoning always had a base in studies by others of other kinds of evidence, as well as its prime basis in our seismological observations. We made the key assumption, as we later learned Anderson had done earlier in another context, that efficient, high frequency, faster seismic wave propagation correlated with high strength, and attenuation and slightly lower velocities with low strength. Thus Gutenberg's "low velocity layer" described earlier became evidence for asthenosphere beneath lithosphere.

Studies of gravity and glacial rebound had produced rheological models with a strong layer over a weak one (Daly, 1940) that could now be reconciled with seismic observations. While in school studying earth science, it had always seemed strange to me that earth scientists used two distinctly different nomenclature systems to describe the upper part of the earth's interior, one the crust-mantle system, the other the lithosphere-asthenosphere system, yet little effort was made to reconcile the two. Thus I was relieved and pleased when we seemed to have found a way to do so.

With the assumption that efficient propagation indicated strength, the correlation between degree of attenuation and the spatial position of ray paths could be used to determine the thickness of the lithosphere. In our study, that figure turned out to be about 100 km, a number that is still considered a good first approximation for a layer that almost certainly varies considerably in thickness from place to place and for which the measurement of the depth to the lower boundary is tricky. Even the definition of that boundary is subjective. Other aspects of the model seemed to make good sense intuitively, although it was some time before quantitative studies were made, often by others.

For example, it seemed that because of the very low heat conductivity of the earth, the down-going slab had to be significantly colder than its surroundings. The earthquake activity seemed concentrated in or near down-going oceanic crust at the top of the 100 km thick slab and not distributed through the entire thickness of the lithospheric slab which, of course, included the entire oceanic crust and part of the upper mantle . And the deformed shape of the slab-flat beneath the ocean floor, curved abruptly near the trench, and then near planar again at depth-fit our intuitive sense of what should happen to it as a result of hydrodynamical effects, although only after we did some simple experiments to observe the effect, but on this particular point we never proceeded much beyond the intuitive stage.

As the story developed, and so long as we confined our attention to observations and physical models that fell within the realm of geophysics, we felt reasonably comfortable about the conclusions we were reaching. A model that calls for underthrusting of hundreds or thousands of kilometers and corresponding deformation on a large scale must have geological consequences that go well beyond that realm, however. As it seemed that the underthrusting phenomenon must also be related to, and was probably the cause of, volcanic activity of the arc, we had to determine whether the volcanological or petrological evidence supported or disproved the model.

We searched the literature but found no evidence that we could recognize as definitive on this point until we found an unusual paper by Coats (1962) in which he proposed underthrusting of the sea floor to depths of a hundred or so kilometers to account for petrological observations in the Aleutians. His results seemed fully compatible with our suggestion of underthrusting to still greater depths, and hence to reconcile Aleutian geology with our model. We were relieved and encouraged to find this supporting evidence, and Coats' paper, which had not received great attention initially, quickly and deservedly became well-known as an important and innovative contribution to earth science.

Needless to say, the success we were having with the Tonga-Fiji data and the down-going slab model made Bryan and me into enthusiastic adherents to what was a small but growing group of advocates of a new earth dynamics. Lynn Sykes was already on that tack and he was stimulated further by the Tonga-Fiji results. We all began to expand our thinking from the island arc scale to the global scale, and, although we did not work out the particular configuration of the plates and the geometrical scheme of their motions (that was being done by Jason Morgan at about the same time), we nevertheless had the general concept of a global set of moving elements of lithosphere in mind. In fact we described our ideas in print as the "mobile lithosphere" concept.

A paragraph of our paper, (Oliver and Isacks, 1967) which was submitted to JGR prior to the time of the famous AGU meeting of 1967 when the paper was presented orally, stated:

"Although the concept presented here of a lithosphere that is discontinuous, underthrust at island arcs, and spread apart or flexed in other areas is based partly on assumption and speculation, the seismic data supporting the concept are sufficient in substance and the implications to geotectonics are so broad that it merits serious consideration."

We were on the right track with that paragraph all right, but more tentative than we would have been just a few months later. In fact, in September of that year (1967) at the International Symposium on Continental Margins and Island Arcs held in Zurich, we gave a similar paper, later published in the Canadian Journal of Earth Sciences (Oliver and Isacks, 1968), but with considerably greater emphasis on the mobile lithosphere concept and its potential for resolving complex problems of tectonics and surface geology. As we analyzed the Tonga-Fiji data and began to think about the down-going slab of lithosphere and the moving plates on a more global scale, we were, of course, bringing in and integrating information from other sources such as geomagnetism and marine geophysics to fill out the story, but we felt that our independent recognition, based on seismic data, of the important role played by the lithosphere was original and unique. But, of course, as nearly always happens in science, that wasn't quite so.

A few weeks before the AGU meeting of 1967, Walter Elsasser, a well-known physics professor from Princeton and Maryland gave a talk at Lamont during which he presented his theoretical model of earth tectonics in which a rigid outer layer that he called the "tectosphere" played an important role. In some ways his tectosphere was clearly equivalent to the lithosphere as we used that term, and so we had a good discussion about our mutual interests following his talk and, I think, reinforced each other's views. Elsasser's ideas were a remarkable example of sound conjecture by a theoretician who had a solid sense of appreciation for the observations of earth science, but Elsasser published his ideas in a rather obscure place (Elsasser, 1967) and they have never received the attention and credit they merited.

Our paper was definitely not the only study of relevance to plate tectonics that was presented at that remarkable 1967 meeting of AGU. Several exciting papers on geomagnetism and sea floor spreading were given at that meeting and so was Jason Morgan's paper on the global configuration of the plates and the simple scheme for describing their motions. For the first time, I think, the idea that something of enormous importance was happening to earth science began to spread widely beyond the handful of early investigators and through the entire earth science community. The atmosphere at that meeting was electrifying. People were literally dashing through the halls from one session to another, stopping briefly to cry "Did you hear that one?!" when they encountered a friend. I have never attended another scientific meeting where the level of excitement was comparable to, or even approached, that of the 1967 AGU meeting.

Many came to discuss our paper with us after it was given, and we received enthusiastic support from people like Tuzo Wilson, who was trying at that time to integrate everything that was relevant into the tectonic story and so was delighted with our results, and Dave Griggs, who was pleased and enthused to find some hard evidence that indicated that his early ideas and his model experiments on mantle convection in the laboratory (Griggs, 1939) were on the right track and receiving some support from observations of the earth. And of course there were some skeptics, but their numbers decreased remarkably over the next year or so as the story was strengthened further.

Before our paper was published, we learned that Katsumata (1967), Utsu (1967), and even Honda (1956) earlier, had found in Japan, as we had for Tonga-Fiji, that the seismic zone there corresponded to a zone of low attenuation, so we referenced them as support for our observations by adding a note in proof to that effect. The Japanese studies reinforced our observations but the early word about sea floor spreading and related matters had not reached Japan and their results were not interpreted by them to indicate lithospheric underthrusting as we had done with the Tonga-Fiji data.

Our paper appeared in JGR in August of 1967, and it was widely cited by other authors and often referred to by speakers for awhile. Two figures from the paper have often been reproduced. One showed the cross-section through the down-going slab with zones of high and low attenuation labeled as such. The other, identical geometrically, showed the same zones labeled as lithosphere and asthenosphere. The second figure was commonly used for a while by others to illustrate the essence of the down-going slab model of arc dynamics. (See NGT, Fig. 1 for a three-dimensional version of this figure.)

The text of the paper suffered a bit because of a fluke in the editing process. At the time, JGR was in the process of abolishing footnotes, so when our manuscript arrived with an early footnote to a line in the introduction, the editor promptly moved it into the text, thereby disturbing what I, at least, saw as the smooth flow of the text of the introduction. In any case, the time of that paper is long past and it is now largely forgotten; it was certainly never so well known as the paper that is the focus of the next chapter. Nevertheless, for me the thrill of discovery was greater in the Tonga-Fiji study than in any scientific effort I have participated in before or since.

Those who have read this chapter carefully will recognize that serendipity was a major factor in the discovery that arose. There was some sound strategic and tactical thinking as the project was conceived and as it developed, but it was only because of a series of fortunate and fateful coincidences that the essence and the full scope of the discovery arose and evolved. Discovery-bound scientists can strive to position themselves favorably so that an important discovery is more likely to happen to them, but whether it does is still somewhat a matter of fortune, still a matter of being, through chance, in the right place and at the right time. We turn now to yet another study, this one of decidedly different style than either Lynn Sykes' paper of 1967 or that by Oliver and Isacks (1967), but one that also produced important and widely-known contributions to the development of the concept of plate tectonics, and that brought the interplay between seismology and plate tectonics to the attention of almost all earth scientists.

8

Science by Synthesis

First they called it Tectonics, New Global
To describe the lithosphere, mobile
Plate Tectonics it became
But what's in a name?
It's the science that's ignoble or noble.

With the sparkling 1967 meeting of AGU a stimulus as well as a highlight, interest and activity in research relating to the developing story of global dynamics accelerated in the period immediately thereafter. It was still too early to describe what was happening as a consequence of a "bandwagon effect," but individuals and small groups at scattered locations were sensing the winds of change and developing the fervor that would eventually create that unmistakable bandwagon.

The early success of Lynn Sykes' seismological paper on transform faults and the widespread attention given to the paper by Bryan Isacks and me on the down-going slab, plus the excitement generated by the 1967 AGU meeting and the news of assorted advances elsewhere, stimulated Lynn, Bryan and me to join together for intensive further action. We were already part of the small fraction of members of the earth science community, mostly geomagneticians and a few geotectonicists, who had sensed that big things were happening in earth science and that bigger things still were likely to follow. It was easy to see that seismology was almost certain to be a major contributor to those developments. We also recognized that we had been fortunate enough to get a temporary, and almost certainly short-lived, lead on other seismologists. Therefore we felt that, at Lamont with its archives of data, supporting facilities, and colleagues in related fields, we were in a strong and probably leading position to make a truly major advance in both global dynamics and seismology by relating those two subjects thoroughly and comprehensively. In fact, we convinced ourselves that we had a once-in-a-lifetime opportunity and we set out with great enthusiasm and urgency to make the most of it. I make those statements realizing, of course, that not all scientists may have seen our status and their status at that time in the light that we did, but I describe here our attitude and our perspective, as best I can recall it, as a frank report and evaluation of how some of us were thinking at that time of change and unrest in earth science.

In any event, Lynn, Bryan and I made an ambitious plan with the ultimate goal of writing a grand paper that would report a comprehensive study of every aspect of the field of earthquake seismology that bore on the developing story of global tectonics. We intended to test the new ideas on tectonics against all relevant seismological observations, to develop and enhance the story of global tectonics with new ideas derived from the seismological perspective, and to call attention to problems and opportunities in the field of seismology revealed through the perspective of the new tectonics.

To make a long story short, we did all of those things as best we could and we published a lengthy paper in JGR of September 1968 entitled "Seismology and the New Global Tectonics." Had we written it a few years later the title of that paper would have been "Seismology and Plate Tectonics." At that time, the term "plate tectonics" was not in use and so far as I know had not been devised. We used the term "new global tectonics" or sometimes, "mobile lithosphere concept" for the nascent subject that would eventually become commonly known as "plate tectonics." We referred to the entities now known as the plates as "blocks," or "thin blocks," or "tabular blocks," or "lithospheric blocks ."

There was considerable apprehension on our part as we published the paper because it was a rather abrupt and radical departure from convention and we feared that someone or something unforeseen might arise to prove that it was a major misstep. However, and fortunately, the paper was a major success. It was widely read and widely cited, partly I suppose because of the innovative content, partly because the comprehensive review of so much data gave it an air of authority, and partly because it was readable by non-specialists. And it was timely, bringing the news and excitement of the developing plate tectonics story to the large and varied international audience that JGR reaches, just as that audience was primed for that news. For the authors, I am happy to say, the paper turned out to be the satisfying and once-in-a-lifetime accomplishment that we had dreamed it might be.

In the following, I discuss the paper and the efforts that went into it, attempting not merely to describe the methods and results in the scientific fashion that the paper relies upon almost exclusively, but also to portray the nature of some of the activity, the spirit, and particularly the interactions with other scientists and their work. Those who want complete details of the science may, of course, consult the original paper as found in the appendix. From here on in this chapter, as in earlier chapters, that paper, "Seismology and the New Global Tectonics," will be referred as the "NGT paper."

As noted previously, the early work of the geomagneticists on the relation between magnetic anomalies and sea floor spreading was a strong stimulus for Lamont seismologists as, during the mid 1960s, we first moved into the stream of activity that would produce plate tectonics with the papers described in the two preceding chapters. By the spring of 1967 however, concepts and ideas arising from elsewhere were providing additional stimuli, particularly for phenomena of global scale. Elsasser's theoretical model of tectonics mentioned earlier became known to us. Then Jason Morgan of Princeton (Morgan, 1968) presented his geometrical model based on Euler's theorem that described the motion of the plates so simply and elegantly. In the process he made and reported the first representation of the configuration of the six major plates of the earth. Xavier LePichon, who had come from France to Lamont to study oceanography aand who was once an outspoken opponent of continental drift, correctly sensed the change that was in the air and, combining Morgan's model with information on spreading rates from the magnetic anomalies, calculated and plotted the relative rates and directions of motion at plate boundaries over much of the world. LePichon's maps and calculations (LePichon, 1968) became an important base for relating seismological parameters to plate motions in the NGT paper, and we were fortunate once again to have the author of a paper so relevant to our work as a colleague at Lamont. A sign of recognition by others of the importance of seismological evidence to the new tectonics arose as McKenzie and Parker (1967), referencing our paper on Tonga-Fiji, used the plate concept to explain focal mechanisms of earthquakes, volcanism and other features of the northern Pacific. Finding that others were on a similar tack was, of course, a spur to us as we worked on the NGT paper.

As we began work on the NGT paper, Lynn, Bryan and I recognized that a large intensive effort was ahead of us and that it would have to be done expeditiously if we were to stay at the forefront of seismological research on this topic. In organizing the work, we considered the possibility that one of us would take prime responsibility for the paper with the others contributing secondarily, but we rejected that plan because it would have put too much of the burden on one person. Instead we agreed that all three of us would work as hard as we could and with a sense of urgency until the paper was in final form. So as to foster this arrangement we agreed that, after the paper was completed, we would determine the order of authors by lot. We did so and the paper has a footnote on the first page indicating that authorship was ordered in that manner.

The footnote provoked some to suggest that we must have had a major dispute over authorship that caused us to turn to resolution in that way, but that suspicion was incorrect. Relations among Lynn, Bryan, and me were amicable at the time and have remained so ever since. Furthermore, our names all became fairly well-known as a result of that paper so order of authorship was not nearly so important as some might have imagined. One wag, I think it was Jim Gilluly, a prominent senior geologist, said he thought the paper must have been written by Charles Dickens because the names of the three authors were similar to those of characters in Dickens' novel "Oliver Twist!"

The organization of the NGT paper and of the studies on which it was based was the result of rather careful consideration. We chose to organize the efforts and the text according to the principal effects of the new global tectonics, and not according to the classical divisions of seismology. Thus major subdivisions of the paper have titles like "ridges," "island arcs" and "movements on a global scale," rather than "travel time curves" "focal mechanisms" or "seismicity," although these latter topics are discussed in minor subdivisions. This style of organization seemed to work out well, and given the success of plate tectonics as a unifying theme for all of earth sciences, it is surprising that it is not more common. All modern textbooks of beginning geology that I know, for example, organize the subject of solid earth science along classical lines and then include a chapter on plate tectonics somewhere later in the book. It seems that alternatively one might present plate tectonics at the start as the underlying scheme of how earth works, and then discuss various kinds of observations and evidence as they fit into, and are a consequence of, that plate tectonics model.

We hoped that the NGT paper would appeal to a large and diverse audience so we tried to limit jargon, to make the introduction and most other sections readily readable by non-seismologists, and to include some figures that were cartoon-like and designed to illustrate concepts rather than to present data. One figure, the block diagram (NGT Fig. 1) that illustrates the basic elements of what is now plate tectonics, became very popular and has been reproduced frequently and in a wide variety of places. It appears in many textbooks, and it is considered one of a set of what some (see Le Grand, 1986) have called the "icons" of plate tectonics. The "icons" in this sense are a few figures that, taken together, communicate graphically and almost at a glance the essence of plate tectonics to the uninitiated.

Years later, recognition in retrospect of the special and crucial importance of those icons in the spreading of the concept of plate tectonics both within and outside the scientific community has left me firmly convinced that scientists should make much greater effort to convey their primary scientific results in pictorial fashion, with simple figures to accompany conventional text, be it jargon-filled or jargon-free. Few readers of a scientific journal struggle through every word of text; most skim the abstract and leaf through the figures. The essence of the paper will reach a far larger audience if conveyed by the figures.

As, Lynn, Bryan and I were preparing the NGT paper, other seismologists at Lamont were catching the plate tectonics fever. Jim Dorman had been the first Lamont seismologist to capitalize on the advent of digital computing. It was the time of the IBM 650, a major technological marvel then but no match for a desktop of today. Nevertheless, that early computer opened many new horizons in geophysics. Having long been familiar with the relatively crude maps of seismicity found in Gutenberg and Richter's book, and then noticed how J.P. Rothé in France had improved the detail and information content of such maps by using more modern data, I suggested to Jim, and he agreed, that he might use the computer to go one step further and plot the new hypocentral data of the WWSSN era. Perhaps, we thought, a still more revealing set of maps would result. Jim and Muawia Barazangi took on that job, and with considerable effort produced a set of global maps of seismicity that also became icons of the plate tectonics story.

The maps showed far more epicenters and more precisely and accurately located epicenters than previous maps, and some maps showed epicenters for shocks in only certain ranges of hypocentral depths. All of the maps were published in a separate paper by Barazangi and Dorman (1969), and the one showing shocks at all depths was also reproduced in the NGT paper (NGT, Fig. 15). That map was a dramatic demonstration of how the seismic belts define the plate boundaries and in turn how most earthquakes occur at those boundaries. Everyone who saw it grasped that fundamental relationship at first glance and the paper became widely-known.

Barazangi at the time was a graduate student relatively new to Lamont, having arrived there from his native Syria by way of the University of Minnesota where he had obtained a masters degree in geophysics. He would follow his first and very notable paper in seismology with many other contributions on seismology and tectonics, some mentioned elsewhere in this book. When the seismicity maps were published, Jim Dorman was a former Lamont graduate student turned research scientist. He was broadly based in geology and geophysics. He not only foresaw the potential of digital computers in earth science but he was instrumental in bringing Lamont its first central computing system.

In a related effort at about the same time, Paul Pomeroy at Lamont was using the newly-found computing capability to make films that showed frame by frame the evolution of global seismicity with time. However, with rare exceptions, at that early stage the temporal patterns were so complex that we could not characterize the activity in a simple manner. That subject continues as a prime research topic today as improved methods of investigation are being developed. Pomeroy's work and expertise provided support for Barazangi and Dorman as they made their now-famous maps, however.

The widespread attention given the Barazangi-Dorman map stimulated others to try to make still more informative seismicity maps. Some did so by using new computer graphic techniques capable of producing figures with several colors. Somewhat ironically these maps, though well-done and full of information, never received the attention or the widespread distribution of the Barazangi-Dorman maps, simply because at that time it was so much easier to reproduce, publish and otherwise manipulate black and white figures than colored ones. Furthermore the black and white maps produced an immediate visual impact on the viewer because of the stark black-white contrast, whereas the more gently-toned colored maps required prolonged concentration and resolution by the viewer before a comparable impact was felt. I draw attention to these matters to emphasize again the special importance of designing figures for scientific communication that do not merely hold content but that also convey that content readily and at first glance.

The Barazangi-Dorman map of seismicity for the entire earth (with supplements for the polar regions that were missing from the mercator projection) was the basis for many deductions and conclusions of the NGT paper. The narrow globe-encircling seismic belts formed a pattern that enclosed certain large areas and so outlined the plates. Almost all seismicity occurred in those belts and at those plate boundaries, but a few shocks were enigmatically scattered within plates and through portions of the interiors of continents. The pattern had other distinctive characteristics. At intersections of seismic belts, three segments came together, not four or more. Belts, in other words and as noted earlier, did not cross one another. It was just the pattern to be expected for the boundaries of a mosaic of irregular plates. Zones of plate divergence, the spreading centers with their ridge-transform-fault configuration, displayed only moderate seismic activity and, as Sykes had found, that activity occurred at the ridges and at the segments of fracture zones between ridges, not along entire fracture zones. Earthquakes at the divergent margins were all of shallow focus, and seismic activity in general was moderate, not only in frequency of occurrence but also in size. No very large earthquakes occurred at the divergent margins.

Most of the earth's seismic activity, including the very largest earthquakes, and all of the shocks at depth including the very deepest shocks, occurred at the generally arcuate convergent margins. In sum, the global pattern of seismicity turned out to fit in astonishing detail the geometrical pattern of the plates. Furthermore, it turned out that, with few if any exceptions, earthquakes always occur within lithospheric plates or at points of interaction between two lithospheric plates. No earthquakes occur in the asthenosphere or elsewhere in the earth.

Thus global seismicity provided spectacular support for the plate tectonic theory; perhaps at the time it made the most convincing argument yet for those just being indoctrinated into the subject. The claims of circular reasoning based on the use of seismicity data to outline the plates and then in turn the use of the outline of the plates to explain the seismicity were dispelled as other kinds of evidence such as the patterns of trenches, ridges, arcs and volcanoes fell into place in the new scheme. Still anomalous and unexplained were the shallow seismicity scattered through parts of the continents and a few deep shocks, particularly beneath Spain, but those were minor components of the global pattern. It is hard to express here the joy and satisfaction felt by seismologists as, almost overnight, we went from knowing only that earthquakes occurred in a distinctive pattern to understanding just why it happened that way.

The successful merger of the plate tectonics model and the global seismicity patterns was on the one hand satisfying and on the other hand humbling. Our egos were alternately inflated and deflated as we found that details and features of the seismicity revealed important information on tectonics but at the same time called attention to the fact that some of those observations had been available to us earlier and ignored or overlooked. The three-armed nature of the intersection of seismic belts, for example, could have been pointed out much earlier based on crude maps of seismicity but, to the best of my knowledge, never was. Before the mid 1960s we seismologists could and should have been more observant and more inquisitive about many such matters than we were. We focused on topics that were in vogue then and ignored or failed to resolve some that would be crucial later on. I hope that calling attention to those embarassing shortcomings and oversights of that era will encourage modern scientists to step back occasionally and search for things of comparable importance that they might be overlooking today.

In addition to the global geographical patterns of earthquake foci and maximum earthquake magnitudes, another kind of global information became the basis for an important test of the plate tectonics theory in the NGT paper. It concerned the focal mechanisms of earthquakes and particularly the spatial pattern of the orientations of those focal mechanisms. The efforts of Honda, Byerly, Hodgson, and others were coming of age. Whereas the work of those pioneers had suffered because of unreliable data and primitive computing technology, by the time of the NGT paper Stauder, Sykes, Isacks, Brune and others had recognized the much improved quality of the new data from the WWSSN. Reliable mechanisms were being determined then by Stauder and colleagues at St. Louis University and by a few others. With new computing facilities seismologists began to produce reliable focal mechanisms for shocks throughout the world. Sykes had begun with examination of the ridge-transform fault areas, and he joined other Lamont seismologists already working on the zones of convergence, to include the entire world. Isacks, in particular, had been focusing on Tonga-Fiji earthquakes and, once the plate concept arose, came up with the notion of slip vectors as an indicator of relative plate motions. He demonstrated the agreement of slip vectors with convergence in Tonga, with the hinge fault at the northern end of the arc, and with the transform fault leading to Fiji, and presented the results at the 1967 IUGG meeting in Zurich. Although this work was carried out and presented orally before McKenzie and Parker's paper with a related theme appeared, the paper did not appear in print until 1969 (Isacks et al, 1969) partly as a result of delay in preparation and partly because a reviewer required six months to complete the review! That was a very long delay indeed during this period when the science was advancing so rapidly.

By the time of the NGT paper, more than eighty slip vectors based on focal mechanism studies for various regions of the world could be plotted on a global map and compared with the theoretical directions of slip calculated by LePichon for the six-plate model (NGT, Figs. 2&3). The overall fit of focal mechanism data to the LePichon model was remarkably good and, although there were some minor discrepancies that could be attributed to the contortions and complexities of the intraplate zone, the focal mechanism data provided yet another kind of strong support for the plate tectonic theory. The dynamics of the plates, in other words, seemed to be the principal cause of most of the modern world's earthquakes, and the type of motion at a particular place reflected the entire global dynamic story.

Although but one of the many revelation during the plate tectonics revolution, it was a pleasant surprise to recognize suddenly that there was a simple, readily understood, relation among major shocks throughout the world. A few years later I accompanied Clarence Allen of Cal Tech on a trip to the Philippines where a major earthquake had occured along the great Philippine Fault. After some difficult travel to a remote village near the epicenter, we found ample geological evidence for the fault motion and, sure enough, the fault, which had moved about three meters, had done so in a left-lateral sense, just as the global plate theory predicted! It was euphoric to realize and confirm that, with just a map, the plate model, and a few calculations made at an office desk, we could now predict accurately the sense of movement of the earth during a major earthquake at a remote site in another hemisphere.

The remarkable fit of worldwide seismic data to a very simple theory of global scale seemed awesome enough, but just as remarkably the moving plate model also seemed capable of producing explanations for a wide range of seismological and other phenomena at regional and local scales as well. For the convergent zones, or "sinks" as they were sometimes called in contrast to the "sources" of surface material at the ridges, the NGT paper reported a variety of seismological evidence that was relatable to the plate model. There was, of course, as described in the preceding chapter, the zone of high attenuation and high velocity associated with the deep seismic zone of island arcs as shown by Oliver and Isacks (1967) for Tonga-Fiji. There was evidence for a similar zone beneath Japan from Katsumata, Wadati and Utsu. And there were signs of a similar velocity anomaly in the Aleutians from studies of travel times by Cleary (1966), Herrin and Taggart (1966), and Carder et al (1967) of waves from Longshot, a nuclear explosion buried there. Finding velocity anomalies was easier when the source was artificial because the time and location of the origin of the seismic waves were precisely known.

Seismic data from smaller artificial sources provided other kinds of useful information, particularly in the vicinity of the trenches. Refraction studies, penetrating only to very shallow depths, showed that the top of the mantle was at greater depths beneath the trench than it was beneath the normal deep sea floor. Based on this information, and on the gravity data that revealed large negative anomalies associated with the trenches, some suggested the trenches were zones of extension. Others thought that the mantle was merely warped downward at the trenches. Some thought the crust was thinner or down-dropped and the mantle pulled apart there. However it turned out that the refraction data could be interpreted straightforwardly if the deeper position of the top of the mantle was explained as a consequence of the descent of the lithospheric slab, of which the mantle, of course, was the major part.

As noted earlier, the extensional model of trench formation received a temporary boost when it was shown by reflection studies that sediment-filled grabens oriented so as to indicate extension normal to the trench occur beneath the outer wall of some trenches. However it was quickly recognized that the bending of the lithosphere in the down-going slab model would also produce such extension, a kind of skin effect as the slab bent as it went through the zone of maximum curvature. It was also suggested in the NGT paper that the grabens, which of course were filled with marine sediments, might incorporate some of those sediments into the regime of the down-going slab and so carry them to depths of a hundred kilometers or so where, following Coats, they would be a significant factor in the generation and formation of volcanic rocks of the arc. The grabens thus provided support and not contradiction for the down-going slab of the plate tectonics model (NGT, Fig. 8).

Stauder (1968) made a beautiful study of focal mechanisms of shallow shocks in the Aleutian arc, demonstrating that the slip vectors of the large compressional earthquakes beneath the inner wall of the trench maintained an orientation consistent with the direction of regional plate motion and not the local orientation of the arc. On the other hand, the outer wall earthquakes associated with the zone of grabens were extensional in nature, with the axis of extension everywhere normal to the arc. These results were precisely what the plate model predicted, and so provided strong support for it.

Some attacked the model of the downgoing slab on the grounds that seismic reflection studies often showed undisturbed flat-lying sediments on the floors of the trenches and hence indicated that the trenches could not be the sites of great compressional deformation. As noted earlier, this objection was countered by pointing out that the flat-lying sediments were probably very young and that they were undisturbed because they were lying on the outer part of the trench floor that had not yet reached the deformation zone. Indeed, against the inner wall of the trench there was a large low density zone that could be attributed to the part of the once similar flat-lying trench floor sediments that had since been deformed and piled up against that wall as they were scraped from the down-going slab. Some of those sediments had been scraped from the slab while others, perhaps mostly those in grabens, were carried to depth with the slab.

Later, and after the NGT paper, as the resolution of the seismic reflection technique was improved and samples of sediments were collected by coring and drilling, it became clear that indeed all the evidence on both the flat-lying and the deformed sediments was consistent with the down-going slab model, or, as it is now known, the subduction model of trench dynamics. In fact, the global view in plate tectonics context showed that young active trenches were in general relatively free of large volumes of sediments, whereas older and less active trenches had more sediments. Of course this generalization was oversimplified because the nature of nearby sources of sediments was an important factor as well. In any case, the differences among trenches with regard to sediment volume and distribution that had once been an enigma to marine geologists seemed to be explainable under the new model of subduction that was based initally on seismic data. Other kinds of geologic data were beginning to fall into place as well and would continue to do so later and well after the time of the NGT paper.

One interesting point of the NGT paper concerned a possible relation between the plate model and the generation of tsunamis. Most major tsunamis have occurred in association with shallow earthquakes in convergent zones at locations where the plate model predicts thrust mechanisms. This association makes sense because a thrust earthquake produces the large vertical displacement of the sea floor that would generate a large tsunami, but a strike-slip earthquake does not. It was yet another case of the global theory explaining a local effect. Whether other phenomena, such as submarine landslides, slumps, turbidity currents, or earthquakes associated with normal faulting and which also produce a large component of vertical displacement of the surface, are factors in the generation of some tsunamis remains an open question however.

In the convergent zones, in addition to the low level of shallow seismic activity of extensional nature associated with the outer wall grabens, and the very high level of shallow thrusting activity associated with the inner wall, there was also some minor, also compressional, shallow activity, behind the arc to account for. Although this activity is more scattered and less well delineated than the activity associated with the grabens or the zone of thrusting at the inner wall, compression in the lithosphere behind the arc seemed reasonable enough and compatible with the model. Later, however, it would turn out that extension could also occur behind the arc at certain times and places but such extension also could be explained by the model. This phenomenon is discussed further in the next chapter.

And then, of course, there were the deep shocks that occurred in the convergent zones only. They were common through a wide range of depths in some arcs, but sparse and not so widely distributed in others. At the time the NGT paper was being prepared, Isacks and Sykes were in the process of making a special study of focal mechanisms of deep shocks, and prior to publication of that study (Isacks et al., 1969) they reported a summary of the results in the NGT paper. The results were somewhat unexpected but, as usual, seemed compatible with the down-going slab model. The deep earthquakes, it turned out, were not shearing motions corresponding to the movement of the slab through or past the asthenosphere as some had expected. Instead they corresponded to stresses acting within the lithospheric slab. Either the axis of maximum compressive stress or the axis of least compressive stress was oriented down the dip of the seismic zone and hence down the dip of the slab (NGT, Fig.11).

Furthermore, the spatial pattern of focal mechanisms, and consequently stress orientation, varied from arc to arc. In some arcs, where the seismic zone penetrated to great depths, i.e. seven hundred kilometers, down-dip compressive stresses seemed to dominate at all depths. For other arcs, intermediate depth shocks seemed to correspond to down-dip extensional stresses. Later, and after the time of the NGT paper, Isacks and Molnar (1969) would show that the focal mechanisms of deep and intermediate depth shocks varied within arcs and from arc to arc in such a manner as to support the idea that lithospheric slabs were sinking freely until the leading edge of the slab eventually sank to a depth where it encountered resistance to further sinking. Thus stresses within slabs differed depending on whether the particular slab was sinking freely or being impeded by resistance at depth. This subject would become more complex as it became clear that contortions of the slab and other factors affect the seismic activity as well, but no data that represent a serious challenge to the slab model have so far been reported.

The NGT paper presented a figure (NGT Fig. 14) to illustrate schematically four ideas about the state of a downgoing slab including (a) a sinking or underthrusting slab freely penetrating the asthenosphere, (b) a slab encountering resistance after it penetrated to some great depth in the asthenosphere (c) a slab being assimulated at depth into the asthenosphere and (d) a sinking slab broken by extension so that a detached piece preceded the main slab. This figure was intended to illustrate hypotheses that might provide the basis for further research. So far as I know, 25 years later all four are still viable hypotheses and all are being investigated currently along with some other variations conceived more recently.

For the mid-ocean ridges, the sources of new surface material, the NGT paper of course reviewed Sykes' by then well-known results described in a previous chapter and based on patterns of seismicity and focal mechanisms at the ridges and fracture zones. It also drew attention to the tendency for swarms of earthquakes, perhaps related to volcanic activity, to occur along the ridges. Modest attention was devoted to detailed study of certain regions such as the Gulf of Aden and the Gulf of California where the seismic data provided valuable information bearing on the tectonics and the genesis of those pull-apart features (NGT, Figs. 5 & 6).

Elsewhere in the paper there are numerous references to one or another kind of seismic evidence bearing on particular geologic features or regions such as the Alps, or the San Andreas Fault, or Southern Chile, or the Gulf of California. In a sense, these references to specific selected geological features of regional scale were but a harbinger of things to come, for in the light of the new plate tectonics model all features of the earth of such scale would soon come under re-examination by investigators scattered throughout the world. In another sense, the references were a sign, coming as they did from seismologists who had limited experience and expertise in geological studies of those regions, that a new framework for understanding and integrating geology on a regional as well as a global scale had suddenly become available. It would turn out that not only seismologists but almost every earth scientist would experience an expansion in breadth of interest and understanding well beyond that of the individual's particular specialty as a consequence of the power of unification of the plate tectonics concept.

Not all of the attention in the NGT paper was directed toward the influence of seismological evidence on problems of global dynamics or regional tectonics. The NGT paper also included a speculative section on possible effects of the new global tectonics on the field of seismology. Probably most important of all in this regard was the new and dramatically different perspective on earth dynamics that plate tectonics engendered. Also important was the early demonstration of how seismic activity related in surprising detail to the new plate model.

Another important change, both technically and psychologically, was the departure from the spherically-symmetric, layered earth model of deeper portions of earth. Spherical symmetry had never been claimed by anyone for the thin and heterogenous surficial crustal layer, of course. But the entire earth from directly beneath that crustal layer to the very center had previously been visualized from the classically layered-onion perspective of spherical symmetry. The new plate tectonics model extended the asymmetry of that near-surface crustal layer to the depths of the deepest earthquakes, almost seven hundred kilometers, and suggested the possibility of still deeper asymmetry as a consequence of slab penetration. And finally it was forecast that the new earth dynamics would integrate seismology much more closely with many of the disciplines of geology than had been the case.

Science has a tendency to fragment into specialties during intervals between times of major advance, the intervals of what Kuhn called problem solving, and then pull itself together when a new framework for integration, or what Kuhn called a new paradigm, is discovered. Plate tectonics was clearly a major new paradigm in that sense, and the whole of earth science became ripe for integration when it became viable. It is, of course, hard to measure quantitatively just how plate tectonics affected the overall activities of any one individual or any particular group of earth scientists, but there is an abundance of ancedotal evidence to support the claim that the thoughts and activities of nearly every earth scientist, seismologists of course included, were affected in some very substantive way by the coming of plate tectonics.

Finally, I would like to describe the circumstances and goings-on behind a few selected portions of the NGT paper in an effort to convey not so much the science of the moment but rather the magic and excitement that seems to pervade the air when scientists come across a new way to explain vast quantities of diverse observations, many of a kind that had received little or no prior attention or notice from those scientists, or perhaps any scientists.

For example, with the underthrusting of mobile lithosphere in arcs as a concept in hand, and with LePichon's model of relative plate motions as a spur, for the NGT paper we plotted down-dip lengths of deep earthquake zones against LePichon's theoretical convergence rates for various arcs. The resulting graph (NGT, Fig. 16) showed a clear linear correlation for the data of most arcs, with the slope corresponding to a time of about ten million years. That time could have been the duration of the most recent interval of seafloor spreading if spreading were intermittent. Intriguingly, the time was just that suggested by Ewing and Ewing in their study of the variation of thickness of sea floor sediments with distance from a spreading center. Or the interval could have been the time for assimilation of the slab into the mantle to the point where it could no longer sustain earthquakes.

The important point for this discussion, though, is that at that particular time it was not only possible but also incredibly easy to make a simple little plot by hand on a single sheet of graph paper and so discover something fundamental about how the entire earth had organized itself with regard to deep earthquake activity. Just a short time earlier no one would have dared to dream that such a simple scheme of organization prevailed for that phenomenon. Just a short time, perhaps a few years, later almost all of the simple relations like that had already been revealed. For those who would discover, being there at the right time was indeed important.

In a similar example, Bryan and I took my desktop globe, shaded the geographical locations of deep seismic activity, measured the shaded areas with a planimeter, corrected for dip, divided the total by the length of the world rift system and the ten million year time constant, and so got a tentative figure for the average annual rate of sea floor spreading along the entire rift system. The half-velocity was 1.3 cm/yr. What that number meant was subject to the assumptions we had made of course, but the very fact that we could casually pick up a nearby globe, do a few very unsophisticated things, and come out in an hour or two with an answer that seem reasonable and consistent with other data such as spreading rates based on magnetic anomalies, left us with the euphoric feeling that we really were on the right track to understanding the earth.

Searching for yet another quick and hitherto unanticipated confirmation of the plate model, I recruited a new Lamont graduate student, Peter

Molnar, to make a global study of P and especially S waves propagating through the uppermost mantle, the shallow part of the earth that is normally a part of the lithosphere. The study wasn't quite so quick and easy as the two just described, for some 1500 seismograms had to be examined, but Pete, who would become an outstanding earth scientist, found that, sure enough, where the model called for a single, unbroken plate, high frequency seismic waves propagated well (Molnar and Oliver, 1969). Where the model called for a plate boundary, high frequency waves were attenuated. The results were consistent with the plate model everywhere that a test could be made. The plate model had produced an explanation for why literally thousands of earthquake seismograms, filling the shelves of observatories around the world, for some paths had one appearance, for some different paths another. Testing the predictions of a theory is a common way to test the theory. Plate tectonics successfully passed this particular test, and many others, with little difficulty. In so doing, it invariably left those fortunate enough to be at the right place at the right time brimming with the joy of discovery.

From the foregoing, it should be clear that the NGT paper and related studies were transforming a part of earth science and making the subject of seismotectonics into a fertile field for further study. When Lynn, Bryan and I wrote the NGT paper we hoped, of course, that the paper would have such an effect and that it would become one of the basic papers of the science, but we had to weather a period of some anxiety while we awaited the response of the scientific community to the paper. Like most scientists, I have never been one to put much stock in omens or mystical signs. However, when, during this period, the Lamont publications office assigned to the NGT paper the contribution number 1234, I almost became a numerologist!

Now let us turn from the NGT paper and contemporary activities and events and summarize what happened during the next few years as others joined and contributed to what was becoming the plate tectonics bandwagon, and plate tectonics became a well-rounded, widely-known, comprehensive theory of earth dynamics.

9

Full Fledged Plate Tectonics

In the realm of the new plate tectonics
Seismology functions as phonics
While paleomagnetical data
And geological strata
Are simply two forms of mnemonics.

The national meeting of AGU in the spring of 1967 (there was only one national meeting each year at that time) probably best marks the moment when the news and the spirit of the upcoming earth science revolution began to reach a substantial fraction of the earth science community. Partly as a consequence of that meeting, the next few years, running through the remainder of the 1960s and the early 1970s, would see a spate of contributions from scientists in a variety of disciplines that had not previously been much heard from on the subject. The combined force of evidence from that diversity of sources in support of the new earth dynamics grew rapidly and soon became irresistible to all but the most die-hard skeptics. By the end of that period, the concept of plate tectonics was firmly established and known by that name to most earth scientists, and to much of the rest of the world as well. Scientists in other fields and interested laymen quickly learned about and were fascinated by the geological revolution.

The subject was popular because it was easy to grasp the principles and the simple beauty of plate tectonics, and because certain of the effects such as earthquakes, volcanoes and mountain-building were topics of long standing human concern and fascination. Intensive research stimulated by the new paradigm continued beyond the burst of innovative contributions of the late 60s and early 70s and continues today. But now plate tectonics reigns as the established and almost unchallenged paradigm of solid earth dynamics as it has for some 25 years. Of course, it is possible that plate tectonics will be superseded by a still better model of global dynamics at some time in the future, and of course improvements in the early model have been made in that quarter of a century, but in my judgment, beyond suggestions for additional modest improvement of the basic model, there is no sign on the horizon at present of a still better and basically different model.

As work related to plate tectonics spread through many disciplines, and publications on the topic proliferated, it became a near-impossibility for one individual to monitor or to summarize what took place in every specialty, and I make no attempt to do so thoroughly here. However, to give the flavor of what was happening and to set a background for some comments about the developments in seismology that followed the early seismological contributions discussed in preceding chapters, I try here to summarize some advances in other disciplines and to cite a few contributions that might be thought of as milestones. I do this with some trepidation, knowing that this brief summary will surely be incomplete and that some friends and colleagues will be rightfully disappointed because I have failed to include their favorite contributions. To them, I apologize. The uncomfortable nature of this situation for the author is perhaps one good argument for the writing of such histories by historians laboring only well after the participants have disappeared from the scene. Those historians might not get it completely right either, but objections will not come from eyewitnesses.

Let us begin the summaries by focusing on syntheses of global scale. In some specialties, the opportunity for global synthesis was unparalleled. The geomagneticians were able to survey and then correlate and date magnetic anomalies over large fractions of the oceanic areas of the earth and, as the history of reversals of the magnetic field was extended back in time and refined, to determine the age of the oceanic crust almost everywhere beneath the deep sea. The task required some modest effort of course, but it was incredibly simple compared to the prodigious effort that innumerable geologists had contributed collectively over more than a century as they mapped the ages of the continental rocks that were less than half the area of the ocean basins.

The oceanic crust was all young, of course, less than about 200 million years old, and the maps of oceanic crust showed how the ages of the rocks of the sea floor increased with distance from the diverging plate boundaries and how it appeared that crust was disappearing at the convergent plate boundaries. The geomagneticians were challenged for a time by skeptics who questioned the correlations of the anomalies from one ship's track to an adjoining but distant neighbor, but, as the data gaps were filled in, they, and almost all remaining skeptics of the plate tectonics model itself, had to succumb eventually and join what by then had become the almost irresistable plate tectonics bandwagon.

When drilling of the deep sea floor commenced under the JOIDES program, some of the very earliest results from drilling showed that the age of the sea floor, as determined from paleontological dating of the sediments immediately overlying the crustal rocks, was just that predicted by the magnetic anomaly-based studies. Once drilling data were obtained at a number of places and for a range of different ages, and shown to be consistent with the magnetic analyses, those combined data became perhaps the strongest confirmatory evidence yet for the validity of the plate tectonics model, or at least the sea-floor spreading component of it. They convinced all but the most recalicitrant skeptics.

For me, having once watched fellow graduate students in the early 1950s labor endlessly and fruitlessly over the early and perplexing scanty information on what seemed almost randomly scattered anomalies of the magnetic field at sea, it was astonishing that those magnetic data had suddenly fallen into place so beautifully and had revealed the ages of the sea floor everywhere. But it was also encouraging because that revelation suggested that other confounding puzzles of earth science might ultimately be resolved in similar fashion and with similar enlightening consequences at some time in the future.

Also on a global scale, and once the basic pattern of the plates and their movement was postulated, the spatial patterns of a variety of other phenomena began to make sense. The configuration of earthquake belts, the spatial distribution of shallow and deep shocks and of the maximum size of shocks in a particular area, as noted earlier, were just a start.

Physiographic features fell into place. The oceanic ridges and fracture zones were characteristic of the diverging plate margins at sea, and the rift valleys characteristic of diverging plate boundaries on land. Island arcs, or just arcs, and deep sea trenches occurred at the converging margins. The global distribution of volcanoes made sense, those with explosive acidic volcanism were located at the convergent margins where exotic surface material was carried down into the mantle, those producing more basic rocks were at the divergent margins. That left certain volcanic island and sea mount chains unexplained temporarily, but Wilson, among others, demonstrated the regular progression of ages in such chains and attributed it to passage of a lithospheric plate over a "hotspot" in the asthenosphere, each island being formed through volcanism just as it was above the hotspot.

What caused a "hotspot" was a mystery for awhile, but Morgan accounted for the hotspot phenomenon as a consequence of plumes of mantle material rising from deep in the earth to a region near the surface beneath each hotspot. Other geophysicists built upon this idea and tried to use hotspots as reference points to determine motion of the plates relative to an axis fixed in the interior and not simply relative to another, and also moving, plate. The detailed nature of plumes is still controversial but the concept seems a reasonable base for a plausible model.

Some scientists showed how various kinds of geophysical observations that were not a part of the early story fit the model and provided new information on earth dynamics. Early measurements of heat flow through the sea floor pioneered by Bullard at Cambridge and Revelle and Maxwell at Scrpps were shown to be more or less compatible with sea floor spreading, although some doubt was cast later on the interpretation of some of these observations because of suspected convective circulation of fluids within the sediments and crustal rocks. Sclater and Francheteau (1970) and others showed how oceanic rocks, formed hot at the spreading centers, cooled and contracted as they moved away from the center. The earth maintained isostatic equilibrium so, as the rocks cooled, the average depth of the ocean increased in a regular and predictable way. The measurements fit the theory and this study was widely recognized as one kind of confirmation of the plate tectonics model. Also, to my chagrin, the calculations explained the obscure results I had obtained on depths of the oceanic crust from the surface wave studies in my thesis described earlier, and then blissfully ignored. Fortunately, no one noticed.

In the 1920s, an ingenious Dutch geophysicist named Vening Meinesz began to develop what would become a reliable means for measuring gravity at sea to a precision of a few parts per million. It was based on swinging pendulums mounted in submarines that submerged to depth during observation so that accelerations due to water waves were avoided. After World War II, observations of gravity proliferated throughout the oceans as opportunity for scientific observations on submarines expanded. Lamont, led by Worzel, Talwani, Drake and others operated such a program. The most striking result of observation of gravity at sea was the revealing of large negative gravity anomalies associated with the deep sea trenches of island arcs. They indicated a deficiency of mass larger than that missing from the trench. Vening Meinesz (1952) tried to account for the deficiency as a consequence of a symmetric down-buckling of low-density plastic mantle beneath the trench due to lateral compression normal to the arc. Others claimed that the deficiency was due not to compression but to the major extension that also produced the topographic trench. Still others felt the negative anomaly could be explained by a large accumulation of low density sediments beneath part of the trench. The latter explanation was more or less correct, but was only a part of the story that eventually would be revealed.

When, as described earlier, it was proposed on other grounds that a trench is a prominent feature of an arc as a consequence of a down-going, and presumably high density, slab of lithosphere, that new model seemed incompatible not only with the tectonic models that the interpreters of gravity data alone had proposed but also with just the raw gravity data alone. How, it was asked, could adding a large quantity of high density material to a region in the form of a deep slab produce a negative rather than a positive anomaly? Eventually the matter was resolved when it was recognized that the narrow negative anomaly was superimposed on a much broader and hence less apparent positive anomaly. The positive anomaly spread over almost the entire region of the arc and was attributable to the high density, and mostly deep, slab. The narrower and more prominent negative anomaly near the trench was interpreted as a consequence of the shallow accretionary wedge of sediments scraped from the top of the slab and plastered against the inner wall of the trench as the slab was subducted and descended. So, after some early and misplaced concern, the great gravity anomalies associated with the deep sea trenches were fully reconciled with the down-going slab model, and gravity joined the list of disciplines providing support for the plate tectonics concept.

Lurking behind the scenes as the plate tectonics model evolved was the suspicion that the whole thing wouldn't work simply because the rheology of earth materials just wouldn't permit the earth to behave in the matter suggested. That view had a long standing base of support because Harold Jeffreys, the extraordinary British geophysicist with a series of major scientific achievements to his credit, had opposed Wegener's theory of continental drift on those grounds, and he stood firm on his arguments and opposed plate tectonics on similar grounds. When Holmes in the 1930s, and others later, had proposed the idea of huge convection cells in the mantle, they had done so in very sketchy fashion and with no firm quantitative basis.

In 1969, Don Turcotte, an aerospace engineer from Cornell turned geophysicist while on sabbatical leave at Oxford, and Ron Oxburgh, a geologist there, published a quantitative study that showed that convection in the mantle as postulated for the plate tectonics model was possible and likely. That study (Turcotte and Oxburgh, 1968) seemed to satisfy many of the skeptics and set the stage for additional, more detailed, studies by them and others. Just how flow in the interior takes place to accommodate or to drive the movement of the plates remains a somewhat controversial subject at this writing however, but that some appropriate kind of movement can occur seems no longer in doubt as a result of such studies and of the complementary observations based on glacial rebound and other phenomena.

The earliest stages of the plate tectonics revolution were based primarily on Euler's simple theorem of spherical geometry, on evidence from two disciplines of geophysics, geo/paleomagnetism and seismology, and on evidence from marine geology. Evidence from more conventional land-based geology, except as general background information, was not a key part of the early action. Of course, that had to change if the new theory of earth dynamics was to be truly global, because land-based geology constitutes a major and very significant fraction of the observational evidence of earth science. A global theory had to be compatible with that important evidence. And change it did, at first largely through the efforts of John Dewey and Jack Bird who, in a remarkable synthesis entitled "Mountain Belts and The New Global Tectonics" published in JGR of May 1970, showed how, and what kinds of, geologic evidence could be used to demonstrate how mountain belts around the world were products of plate evolution, revealing in the process new information about the histories of mountain belts and the processes that formed them. The Dewey and Bird paper was widely read and very influential. Skeptics who had scoffed at nascent plate tectonics because it was "all geophysics with no geology" were forced to modify their positions or confront a rising tide of pro-plate tectonics geologists.

In spite of their use of the term "new global tectonics" in the title, which was apparently designed to parallel "Seismology and the New Global Tectonics," Dewey and Bird often used the equivalent term "plate tectonics" in the text. So far as I know, that was one of the first papers to use that now widely-accepted term. Just how and where the term "plate tectonics" originated seems not widely-known. I have even been erroneously credited with originating it myself. It is my impression that the term was not invented by someone well-known for early work on the subject. It seems to have been used sporadically at first until eventually, and rather suddenly, it became the accepted name for the subject. In any case "plate" is probably a better term than "thin lithospheric block," or even "tabular block" or some other variant, so "plate tectonics" it now is, everywhere from a modern newspaper article on a destructive earthquake to a text on the history or philosophy of science to a highly technical article in the best of scientific journals.

Other geologists pushed the subject further and accounted for once enigmatic features of continental and oceanic geology under the new paradigm. The term "subduction" was born and understanding of the processes associated with a down-going lithospheric slab refined. Ophiolites were identified as once parts of the sea floor brought to the near-surface of continents as a result of convergent plate margin processes. Petrologists and geochemists entered the scene as a paper by Robert Kay and others (1970) showed how the rocks of the oceanic crust were formed through partial melting of the mantle. A long controversy began over how water and sediments subducted by the down-going slab of lithosphere in island arcs influenced the composition and generation of magmas and lavas, but the basic elements of the subduction model have withstood challenge.

Island arcs got other kinds of attention as well. Karig (1970), in a study of ocean floor relief and sediment distribution for the region behind the Tonga-Kermadec trench, showed that the evolution of an arc, a convergent and hence one might think mostly a compressional feature, could include extension in parts of the system, resulting in an inter-arc basin behind the main volcanic arc. What at first seemed a contradiction to the large scale view of arcs as convergent features was reconciled with that view by appealing to secondary patterns in the flow of the asthenosphere as it accomodated the penetrating lithosphere and also moved magma to volcanoes at the surface. That secondary flow acted on the surface rocks so as to produce local extension behind the main arc.

Minear, Toksoz and others made quantitative numerical studies of the thermal history of the down-going slab of lithosphere and its surroundings and showed, as earlier intuitive suggestions and crude calculations had suggested, that for a reasonable convergence rate at least part of the slab remained cooler than adjacent asthenosphere to depths of nearly 700 km. Whether the slab lost its identity completely at that depth would become a matter of continuing controversy but the concept of the slab as a body that maintains lower temperature for a long period seems firmly established.

The enormous job of trying to recreate, with plate tectonics as the basic paradigm, the geological histories of rocks and rock masses on all scales including that of the continents began. For the recent past, and going back to the splitting of the supercontinent Pangaea about two hundred million years ago, the task was not overly difficult, for abundant magnetic data on the age of the ocean floor, the geometric configurations of shorelines, and the fit of rock formations across areas once intact but now broken apart and separated by spreading oceans could be used. In fact, many early conclusions about jigsaw-like fits of continents by Wegener and other early drifters were shown to be compatible with the new ideas of plate dynamics. One particularly important and revealing advance came with the recognition that large segments of the continents were a consequence of the accretion at various times of many smaller terranes that had been transported separately across long distances and then plastered against a continental mass.

For, pre-Pangaean time, data are more sparse. No old ocean floor remains except perhaps beneath a few inland seas such as the Caspian or where it has been thrust on to continental crust; it has long since been subducted. Geologic data on older rocks since reworked and partially obliterated by more recent events must be pieced together to provide a segment of history that is plausible and consistent with other parts of global geological history for all time. This subject, of course, is still being developed and has practical as well as purely scientific importance. The reconstructions that result are often useful, for example, in exploration for minerals and hydrocarbons. On the less technical side, the geologic history of earth in toto prevails as solid earth science's most important contribution to the intellectual sphere as humans strive to understand humans and the meaning of human existence on earth and in the universe.

With that very brief and sketchy account of the accomplishments and goals of earth scientists as the effects of the new paradigm were felt throughout the science as background, let us turn now to some developments in seismology that came about because of the flourishing of the concept of plate tectonics. Bear in mind that, in the 1970s, the scope and diversity of the activities in plate tectonics-related seismology, and the numbers of scientists active in the subject, were already much greater than they had been in the mid and late-1960s. Plate tectonics was getting increasing attention, not only within earth science circles, but throughout much of the rest of the scientific community and parts of the non-scientific community as well. It had become newsworthy and scientists who were already involved were called upon to spread the news.

Scientists who publish papers with overlapping content in different scientific journals are often subjected to criticism and discouraged from doing so. However, as a result of the NGT paper in JGR, Lynn, Bryan and I were invited and encouraged by editors and others to write updated versions of that paper for the journals of other scientific societies and various other less technical publications. Invitations to lecture on the subject were frequent and they came from a wide variety of organizations. Never known for the charismatic presentation of an outstanding popularizer of science, nevertheless I spoke on invitation to audiences at colleges, universities, local chapter meetings of professional societies, international scientific meetings, committee meetings, large corporations, museums, Rotary clubs, and an off-beat quasi-religious institution. I once gave consecutive lectures to an audience of Boy Scouts and then to a group of scientists at the National Academy of Sciences and used the same set of slides! The text was worded differently in each of the above cases but the basic content of all the lectures was the same-the essentials of plate tectonics and the relation of that new concept to seismology.

For those readers who are not familiar with custom in scientific circles and who may have the mistaken impression that such lecturing is a financial bonanza for the lecturer, I hasten to point out that, with the exception of a couple of talks at industrial laboratories, those science lectures were provided as a public service by my university and resulted in no fees whatsoever for the lecturer other than travel expenses!

The press of speaking engagements and writing activities that followed the NGT paper, and the press of additional administrative duties as I became chairman of Columbia's geology department forced me to curtail my research somewhat during the late 1960s. Meanwhile the NGT paper opened new opportunities for the authors and, in 1971, I left Columbia and Lamont to become chairman of the geology department at Cornell where there was a rare opportunity to rebuild a department of a major research university around a new focus. Bryan Isacks, Muawia Barazangi and George Hade also moved from Columbia to Cornell, and shortly thereafter Jack Bird, Dan Karig and Bob Kay, all known for their early contributions to plate tectonics, joined the Cornell department. Don Turcotte, already at Cornell, transferred from aerospace engineering to geology and the department took on a forward-looking, plate tectonics flavor that made it stand out from departments elsewhere that were not in a position to undertake such major rebuilding. The reoriented department attracted many fine graduate students and developed a variety of important research projects, some of which are discussed in the following. At Cornell during the 1970s, with Sidney Kaufman and Larry Brown, I initiated the COCORP project which is based on the kind of strategy that was successful in bringing about plate tectonics. COCORP is discussed in more detail in the next chapter.

Lynn Sykes moved into the faculty position that I vacated at Columbia and, working with various students and post docs, produced a series of fine papers on seismotectonics. Some followed the theme of some of his previous work but were applied to parts of the world previously unstudied in this manner, and some broke new ground. Among other things, Lynn became interested in earthquake prediction, and his background in plate tectonics helped in the development of the seismic gap theory of earthquake prediction. The seismic gap theory is based on the idea that, as lithospheric plates converge at, say, an island arc, the intermittent motion along segments of the arc as represented by individual shallow thrusting earthquakes must eventually even out along the entire arc. Thus that segment of the arc that has gone longest without having a major shock is the part of the arc most likely to have the next one. This concept has enjoyed some success, and may be the most reliable method for prediction at the present time, but neither it nor any other method yet has the capability of providing in advance the precise time, location, and size of an impending earthquake. That goal seismologists, and the inhabitants of earthquake-prone areas, continue to seek.

After the NGT paper, Lynn turned a part of his attention to the seismological aspects of the comprehensive nuclear test ban treaty, still being actively pursued in international diplomatic circles, and became one of the more prominent seismologists in that activity in which plate tectonics plays a modest role, though not a decisive one.

Bryan Isacks, at the time of the NGT paper already more heavily into the study of arcs and their deep earthquake zones than most anyone else, continued to probe that subject in the following years. Working with Sykes, Molnar, Barazangi and others, and as mentioned earlier, he showed how the spatial patterns of focal mechanisms varied from arc to arc depending upon the maximum depth and extent of the deep earthquake zone. The observations indicated that the slabs were sinking, as a consequence of their high density, until they eventually encountered resistance at depth. Contortions in the deep earthquake zones apparently indicated deformation of the slab at depth, perhaps as a consequence of asthenospheric flow or resistance to settling.

The Tonga-Fiji project moved to Cornell with Bryan and me. Eventually, under Bryan's leadership, it first shifted west of Fiji to the New Hebrides arc where strain, tilt and leveling measurements as well as seismic observations were made, and then to the Andes of South America, another convergent plate structure but in this case one involving the margin of a continent. All of the studies under this project provided information of higher resolution on the configuration and dynamics of down-going slabs in these arcs, but revealed no evidence incompatible with the basic subduction model of plate tectonics. Isacks' work in the Andes resulted in much improved understanding of the formation of the central Andean Plateau and the Bolivian orocline. Recently, the project has evolved so as to reveal the role of long term erosion of topography on the tectonics of mountain belts. Surprisingly, it is turning out that the prolonged effects on surface relief that result from non-uniform spatial patterns of precipitation and erosion in mountainous regions can, when integrated over millenia, be a factor in geodynamics. Climate, in other words, may influence the way in which the tectonic plates move because masses at the surface are slowly redistributed through erosion.

Seismological research of the 1970s related to plate tectonics took place against a background of activity much different from that of the mid 1960s. Seismologists, and earth scientists of all kinds, almost everywhere were entering the competition to make new contributions to the understanding of tectonics on a local, regional, and global scale. Studies and information proliferated and indoctrination into the subject was no longer a simple matter of learning a few concepts, a little marine geology, and the latest news from geomagnetism. Innovative ideas about the consequences of plate tectonics and tests of them were springing up in most of the specialties of earth science. Doing something new and informative became easier in one sense as topics not yet probed under the plate tectonics paradigm caught attention, but quickly became harder in another sense as the most obvious opportunities were seized upon by a crowd of investigators. And there was no longer the smell of an undiscovered paradigm in the air; rather the 1970s marked the early phase of problem-solving under a new paradigm.

Seismologists, including especially those bypassed or left out of the excitement of the 1960s, were aggressive about developing new topics for study, or new angles on old topics. The island arcs and deep earthquake zones continued to receive seismological attention. For example, it was proposed that the down-going slab had achieved its shape as a consequence of successive shearing of portions of the slab as it descended. That concept, however, turned out to be inconsistent with the data on focal mechanisms. It was proposed that prevailing currents of global scale in the asthenosphere resulted in consistently greater dips in slabs dipping east than in those dipping west, the evidence on slab dips coming, of course, from the configuration of the deep seismic zones. That matter is not yet fully resolved and the pattern of asthenospheric flow continues to be a controversial topic.

It was suggested that anomalies in seismic wave propagation could be detected at depths greater than that of the deepest earthquakes, indicating that the slab, though aseismic in its lower reaches, had nevertheless penetrated to those greater depths. Or perhaps, it was suggested, the leading portion of the slab had descended to a level of resistance, or buoyancy, and then drifted horizontally away from the deep earthquake zone. It was also suggested that detached, aseismic portions of the descending slabs sink through the entire mantle to its base at the core-mantle boundary. The core-mantle boundary is also thought by some to be the source of the plumes that rise to the surface creating hotspots and other effects. Such speculations, though often the crux of unresolved controversy, have stimulated seismologists to gather evidence on the existence and configuration of heterogeneities in the mantle.

Currently tomographic techniques are used to seek mantle heterogeneities, and comprehensive studies of the interior have been made using statistical techniques and huge quantities of data. They seem to reveal zones of higher than normal and lower than normal velocities and, based on their spatial distribution, the anomalies have been associated with phenomena of the interior related to plate dynamics. However, the marginal signal to noise ratios of the data and various other factors have left some lingering doubts about the reliability of the results.

Substantial seismological effort has gone into determination of the thickness of the lithosphere, a parameter that probably varies considerably with location. Some have found the lithospheric thickness to be much greater than average, perhaps several hundreds of km thick, beneath the continents. Others dispute this claim and find a thinner lithosphere there. Such controversies are often clouded by ambiguity in definition of the term lithosphere and by disagreement over just what certain observations tell us about the lithosphere. At one time or another the lithosphere might be defined as the layer of strength, the layer of efficient propagation of high frequency P and S waves, the layer of high velocity overlying the mantle low velocity layer, or the layer above a particular reflecting horizon, all this in the face of the distinct possibility that the boundary is a gradational rather than a sharp and distinct one.

The case is further complicated by evidence that continental earthquakes mostly occur within only the upper 20 km or so of the crust, i.e. only the uppermost part of what we typically designate as the lithosphere. Presumably that is the brittle portion and it overlies a more ductile lower crust and mantle. The nature and configuration of the lithosphere thus remain controversial topics and ones being actively investigated. There is no serious objection at the present time, however, to the general concept of large thin plates of lithosphere capable of transmitting stresses over long distances. Recent maps of stresses in the crust support this statement by revealing patterns of stress that are consistent over continent-scale dimensions.

Another phase of earthquake seismology that has developed since the 1960s is the determination of anisotropy of rocks at various depths in the earth. Such studies are based particularly on observations of splitting of the shear wave. Some assumptions are involved, but it may be that the anisotropy measured is a consequence of flow that aligns certain crystals. If so, there is potentially a way to measure flow, present or past, in the asthenosphere and perhaps resolve some major questions of earth dynamics.

Almost all of the post 1960s efforts and advances noted in the last few paragraphs became feasible on a reasonable scale only because of the advent of seismograph networks producing data that can be readily manipulated in digital form. Digital recording and related developments in computing techniques have brought the field of seismology the potential to do things once thought beyond reach or once unimagined. So far, however, although the capacity to manipulate and analyze data has been much enhancd by the digital revolution, the new techniques have not yet produced a conceptual revolution in the science. The change has so far been more in style and thoroughness than content. The conceptual revolution may not be far off, however.

Finally, seismologists using earthquakes as sources of seismic waves have not been alone in seismological efforts to test and build upon the early version of plate tectonics. Artificial sources have also been much used, particularly by marine geophysicists and industrial geophysicists, to produce spectacular seismic reflection profiles that reveal information particularly about sedimentary basins on land and at sea. Of all geophysical techniques, it is the reflection technique that produces by far the highest resolution and most detailed information on the interior. In general, the new information on the sedimentary layers tends to fit well and complement the new tectonics. For example, the structure of the accretionary wedges of sediments at the inner walls of trenches has been resolved in striking detail and seems to fit well the plate tectonics story of sediments scraped there from the down-going slab. The major decollement beneath the wedge, and separating it from the slab, has been traced by seismic reflection profiling to considerable depths, and it appears just where, and as, the model predicted. The sedimentary basins that are the habitat of petroleum have become better understood based on the relation between the new seismic data and the plate tectonics model, and the basins have been organized into a classification scheme based on the model. Most of the seismic reflection effort is directed toward the search for hydrocarbons, but the search for certain minerals is now beginning to rely more on seismic reflection profiling and on interpretations based on plate tectonics.

So, in contrast to the days of turmoil and doubt of the 1960s, plate tectonics with its components of sea floor spreading, subduction, and transform faulting has come of age as the prime paradigm of earth dynamics. Neither the challenges that have arisen to date, nor the alternatives that have been suggested, have gained momentum, or even much attention, suggesting that it will be some time, if ever, before a fresh new paradigm replaces plate tectonics. Refinements arise, of course, and will continue to do so, but the basic concept has not been shaken.

In the following chapter, I make some comments on the strategies and philosophies that led to plate tectonics and also attempt some comments on fruitful activities for the future, based on first-hand experience with the plate tectonics revolution.

10

Strategy, Philosophy, and Things Like That

What we need is a logarithmic form
To rate paradigm shifts 'gainst a norm
We could just declare then,
Plate tectonics was a "10"
And avert philosophical storm.

As the 1960s began, the earth, as humans perceived it, was a round ball drifting through space. It had a thick, inert, rocky mantle that encased a central, molten, metallic core, and a complex yet static pattern of land and sea covered the surface. That image of a relatively placid earth was about to change dramatically. By the end of the 1960s, the molten core remained in that image but the once-inert mantle had become dynamic, still solid but nevertheless deforming readily and carrying heat convectively from the hot core to the cool surface. There a remarkably thin, strong, brittle, cold, outer layer, loosely-speaking a frozen shell, enclosed the earth's interior. And the shell was neither static nor intact; it was broken into a few large plates that moved about in conjunction with circulation in the mantle below. In so doing the plate motion caused the disturbances that shaped and modified that thin outer shell that is so important to us because it is the habitat of the human race.

For earth scientists that transition from a near-static image of earth to a much more dynamic one corresponded to an abrupt paradigm shift, a fundamental change in the basic framework of the science, an historic and almost certainly a once-in-a-lifetime event. As such it not only expanded knowledge of earth and opened fertile new areas for research but it also gave scientists of that era a unique opportunity to observe in person and at close quarters just how a science operates before, during, and after a major upheaval.

In this chapter therefore, I focus not so much on the basic science of plate tectonics but rather on certain operational aspects of science as they bore on the revolution. I do so in the hope that provocation of some contemplation of such matters may be helpful to those scientists attempting to foment yet another major advance in the science. Specifically, in the following I discuss styles of science, choice of frontiers, peer review, communication of science, the role of scientific societies, and some selected opportunities and new directions for the field of seismology and earth science in general.

It is somewhat presumptuous, of course, for any individual to attempt a comprehensive analysis of phenomenona that each of us saw from a different perspective and in a different light. No scientist knows precisely what went on in the heads of fellow scientists as the revolution developed, or even remembers precisely what went on in his or her own head. I like to think of the participants in the plate tectonics revolution as analogous to droplets of water in a braided stream. Each droplet entered the stream at a different time and place, and each interacted from time to time with other droplets as various rivulets formed and intertwined. All were driven in the same general direction and toward the same goal, yet each followed a different course and underwent different influences. Eventually all ended up neatly channeled within a single broad valley as part of a more smoothly and more slowly flowing unidirectional river. The motion of the constituent droplets is less chaotic now, but none, of course, knows what stream gradients or waterfalls lie ahead. To attempt to describe the nature of that stream based on the history of one droplet is, of course, a risky business, but at least the attempt may provoke others to contemplate what happened to the entire stream based on their own experiences with a part of it.

Styles of Science

In previous chapters I have drawn attention to some styles of science that were important to the revolution, particularly to the seismological component of it. They include science by hypothesis testing or what some might call the hypothetico-deductive method, science by serendipity or the inductive method, and science by synthesis, in a sense broad application of the first two. All of these styles, and variations and combinations of them, played a role in the plate tectonics revolution. Science thrives on diversity. It would be a serious mistake for science as a whole to curtail the diversity of approaches in science and so divert attention away from any one of these styles. However, in seeking to foment another revolution that advances the science in a major way, as scientists are wont to do, we might first ask just what it was that triggered the plate tectonics revolution and so sent earth science off in a new direction. Perhaps the sequence of events that led to success then might serve as a guide to direct or prioritize activities in the future.

In my opinion, and I think many others would agree, it was the effort to obtain diverse and comprehensive geophysical observations of the deep sea floor during the interval immediately following World War ll that triggered the revolution. In this effort, the inductive style, i.e. science by serendipity, prevailed. It was primarily the call of exploration of the unknown and not the desire to test a hypothesis that drove the scientific effort. Of course, there were some hypotheses about the nature of the deep sea basins at the time. Wegener's hypothesis of drift was one, the idea that the Pacific was a scar left by the moon's departure another. But the post WW II exploration of the oceans was not specifically designed to test those or other hypotheses. Rather the scientific goal was probing of the great unknown earth science frontier of that era, the ocean basins, just to see and then to understand what was there. It was fresh new observations of those great marine basins that initiated the string of events of the 1960s that would lead to plate tectonics, and there can be little doubt about it.

To make that conclusion, and so to focus on a broadly-based observational program, is by no means to minimize the importance of the ingenious and inspired ideas of clever individuals that moved the revolution along or to deny those individuals respect and credit for their contributions. Nor is it to denigrate the significance of the wealth of geological information collected earlier on dry land that became the basis for part of what followed. Nor is it to make light of early, pre-WW ll, progress in any branch of science that played a role in the coming of plate tectonics. As I have tried to show in the case of seismology, the early exploratory work in a field is a critical and necessary step. Nevertheless, it seems clear that, during that post-WW II period, the deep sea floor stood out as the prime frontier of earth science, and observation of it, just to see what was there, was the thing to do. Those who led the effort to explore the ocean basins in this manner led the science to major advance. I have already cited Lamont in this regard and in some detail because of its special role in earthquake seismology and because of my familiarity with that organization, but I hasten to add here that other organizations were similarly inspired and dedicated, notably among them Scripps Institute of Oceanography, Woods Hole Oceanographic Institution and Cambridge University. And, of course, it was interest in understanding the oceans for the military purposes of the Cold War that in part stimulated the funding for marine research during that era, but I refer here to the motives for the basic scientific program.

That prime lesson from the plate tectonics revolution seems clear enough; it is neither a new one in science nor a surprising one. The success of basic exploration of the ocean basins in instigating the scientific upheaval is by no means unprecedented. In science, exploration of a new frontier almost always produces surprising discovery. To hasten the pace to the next revolution, we need only identify the next major frontier and then observe it thoroughly and in every significant way possible.

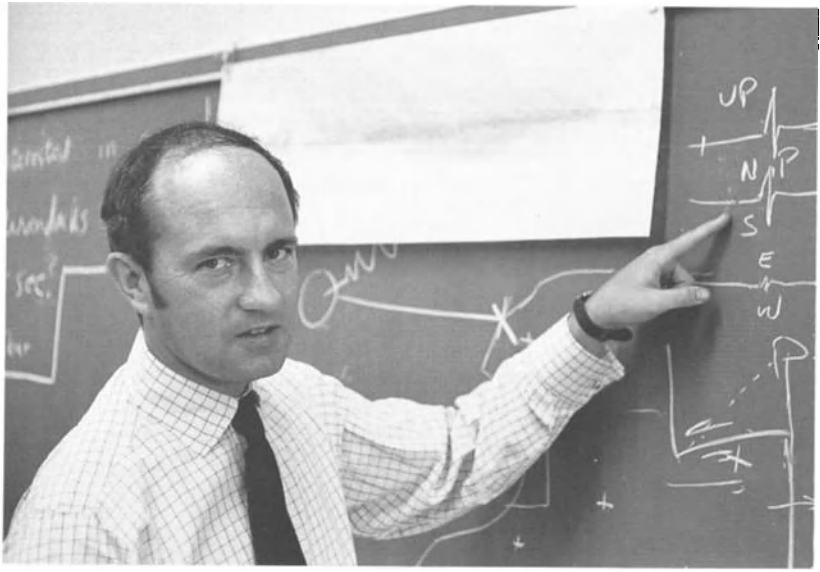
Of course, to identify that frontier is no simple task; it is one that calls for insight, vision, and good-fortune. It is a continuing challenge to the leaders of a branch of science to seek out promising major frontiers and to make certain that no possibility is bypassed or overlooked. And, of course, it is not only new discovery that science seeks but also continual testing, reevaluation, and upgrading of all scientific theory. A little later, I shall draw attention to what I believe is the, or at least a, major frontier of modern earth science, the modern counterpart of the deep ocean basins of the post-W.W.II years. It is the entire buried continental crust. For the present however, let us continue the discussion of styles of science, and what to do about, or with, them.

Science in the inductive style of exploration of the unknown not only triggered the early stages of the plate tectonics revolution, it also provided the observational basis for much of what followed. It was the observational data base that stimulated the serendipitous discovery that brought scientists to the key concepts of the revolution. It is difficult to see how the revolution could ever have come about if observation had been conducted solely to test specific hypotheses. Nevertheless, science by hypothesis testing with its emphasis on deduction also played an important role in certain aspects of the revolution. Wegener, Hess, Vine and Matthews, Wilson, Morgan, and others envisioned the "way it might be" and so stimulated testing by observation that moved the science ahead. It is difficult to see, however, how those ideas would have caught on or how the revolution would have blossomed if those hypotheses had been proposed in the absence of a substantial quantity of relevant observation.

To demonstrate this point, one need only note how Wegener's ideas sputtered for decades until the post WW II observations of the deep sea basins were made, and the misconceptions of scientists who had obstructed the idea of continental drift could be overruled. Or note that Ortelius made an important and related hypothesis centuries earlier in the complete absence of observation of both the sea floor and the geology of the land surface and that it was immediately consigned to obscurity. From these examples alone, it seems clear that the way to begin an episode of major discovery in science, at least an observational science like earth science, is through exploration of a new and important frontier. Once the comprehensive base of fresh new observations is available, a kind of interchange between the inductive and deductive modes can be very beneficial, but it seems most conducive to major discovery to begin in the inductive style.

Jack Oliver at press conference, 1960s

Maurice Ewing and Jack Oliver with seismograph recorders at Lamont Hall, about 1960



Lynn Sykes, 1968

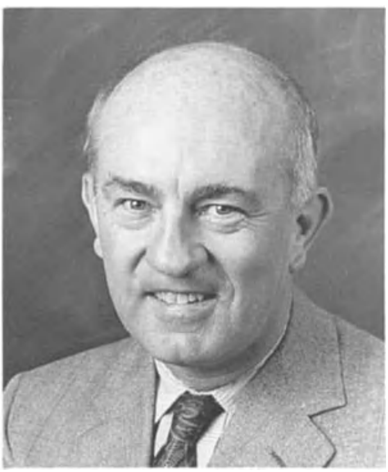
AFOSR Advisory Committee, early 1960s—left to right: William Stauder, S.J., St. Louis University; Thomas O'Donnell, Gulf Research; James T. Wilson, University of Michigan; Hugo Benioff, California Technical University; Sidney Kaufman, Shell Development (later at Cornell University); Jack Oliver, Columbia University (later at Cornell University)

Installation of seismograph station in Fiji, 1965

Installation of Lamont seismograph station at Niumate in Tonga, 1964

George Hade at seismograph station in deep mine,
Ogdensburg, New Jersey, about 1960

George Hade and Merrill Connor, seismograph station in deep mine, Ogdensburg, New Jersey



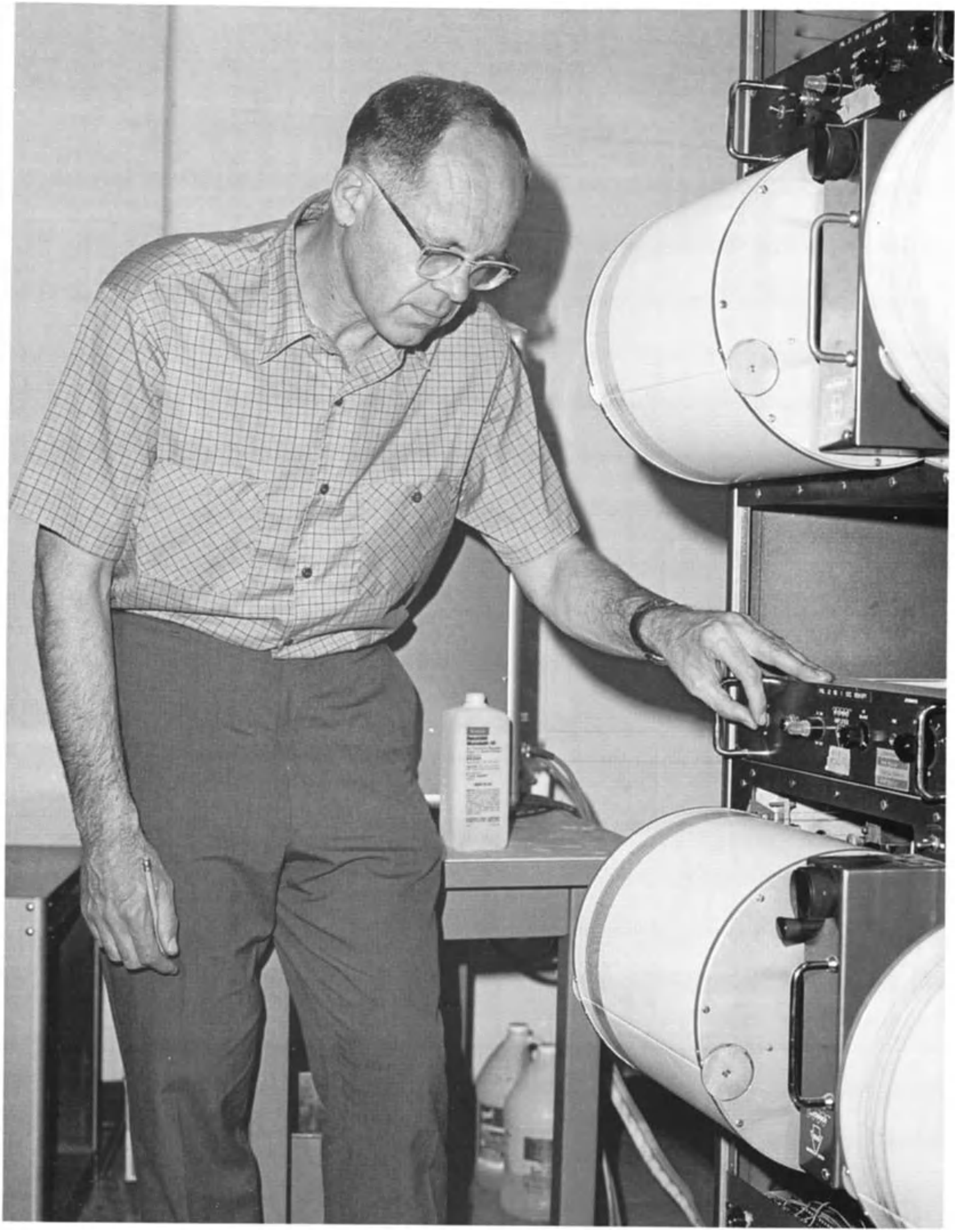
Top: Bryan Isacks, 1990s;

Left: Lynn Sykes, 1992;

Bottom: Visit to Cornell and COCORP project by Director of NSF; left to right: James Krumhansl, Richard Atkinson, NSF, and Jack Oliver

Jack Oliver's retirement party, 1993—left to right:Jack Oliver, Frank Press, and Sid Kaufman

Oliver retirement party, 1993. Top row, left to right: Jim Dorman, Walter Mitronovas, Bob Metsger, Jack Oliver, Mark Langseth, Frank Press, Jim Brune, Bryan Isacks; Bottom row, left to right: Klaus Jacob, George Hade, George Sutton, Chuck Drake, Dick Edwards, John Kuo, Judy Healey



Jack Oliver at seismograph recorder, late 1960s

I make this point at some length in order to stress the value and the primary importance of the inductive style as well as the need to maintain diversity of styles of science. In modern science, there are certain factors related to organization, justification of effort, and funding that tend to force the science toward the deductive hypothesis-testing style to the exclusion of others. More generally, it seems that, during the periods of problem solving as opposed to periods of paradigm shift, hypothesis testing tends to predominate. Hypothesis-testing is usually a somewhat more tidy kind of operation than is observation of a frontier, and hence is more easy to organize and administer. It is also somewhat easier to justify to non-scientists because the goals are clear-cut and because a particular project usually has a relatively short and finite life. Inductive, serendipity-based, science is less easy to categorize and delimit and may seem less appealing to non-scientists. Consequently, hypothesis testing is often, too often, heavily favored by the political world and the bureaucracy, by review committees, by peer reviewers and the like.

The historical record of discovery described here for the case of the plate tectonics revolution is a firm basis for challenge of that strong favoritism for science by hypothesis testing over science by serendipity, i.e., for the deductive style over the inductive style.. A science is most likely to be productive of major discovery if it is not controlled and fettered and directed by hypothesis test after hypothesis test. Sole reliance on hypothesis testing limits a science to progress at a rate dependent upon the imagination of the participating scientists. Science need not be limited in that manner. Inductive science favoring serendipity offers better prospects for the startling advances that can far surpass the rate at which human minds can envision new discoveries.

Science by synthesis usually involves a combination of the inductive and deductive styles as applied to many and varied observations, hypotheses, and theories. Science by synthesis was very important in the plate tectonics revolution. In addition to the seismological studies cited earlier, examples include the global map of marine physiography prepared by Heezen and Tharp, the global map of ages of the sea floor based on magnetic anomalies by Heirtzler and others, and the interrelating by many geologists of a wide variety of geological phenomena with the plate tectonic model as the basis. These few spectacular examples from earth science are climactic ones and attention-getters, but perhaps they will highlight the opportunity and the need for synthesis of almost all active scientific subjects on a continuing basis.

Later, while discussing publication, I shall try to make a strong case favoring more funding for synthesis and for preparation and publication of more books and articles that review subjects thoroughly and in depth as one way to overcome some current problems of communication of modern science. But let us turn now to the topic of the major frontier of earth science at present. This topic is a part of this book because it is important and because the history of plate tectonics provides a basis for deliberation and decision on this crucial matter.

Frontiers of Earth Science

It is my firm conviction, based on personal experience with study of the earth, that the buried continental crust and adjoining uppermost mantle constitute the major frontier of earth science at present. The position of this part of the earth on the list of targets for human exploration of earth seems analogous now to that of the ocean basins just after WW II. Both the ocean basins and the continental crust must be important to the understanding of the earth if for no other reason than that they are huge. Furthermore, except for the surface where the geology is everywhere known on at least a reconnaissance basis from geological observation, and the sedimentary basins where petroleum exploration has focused much attention and effort on subsurface exploration, and a few other locations where drilling or other special circumstances have produced detailed information, the continental crust has been explored only spottily and then only with techniques of low or modest resolution. Like the floor of the deep oceans after WW II, or like the continent of North America during arrival of the early settlers, the continental crust at this point in history awaits exploration in detailed comprehensive fashion.

There are, of course, numerous other important frontiers of modern earth science. For seismology, for example, the core-mantle boundary is clearly one, the inner core perhaps another. I focus on the continental crust here however, partly because it is my personal favorite and because, like the ocean basins, it has the potential for providing scientists with huge quantities of diverse and revealing data.

Of course, I do not mean to imply that every frontier of modern earth science is necessarily a spatially-defined entity. For example, the existence and the nature of microbial life at depth in the earth is clearly a frontier topic of modern biology and geology. The role of fluids traveling through and interacting with permeable rocks of the crust is another frontier topic. Nor do I imply that the potential for further important discovery concerning the floors of the ocean basins is exhausted. Students of earth science of this era are blessed with the opportunity to explore many challenging topics, far too many to enumerate or discuss in detail here.

As exploration of the buried continental crust progresses, by analogy with the history of exploration of other earth frontiers we can anticipate surprise and revelation and eventual satisfaction as we develop a much improved understanding of that part of the earth and those parts that interact with it. In all probability, our current concepts of what is there and what has happened there will undergo major revision. Perhaps another paradigm is in store. A major opportunity for earth science can be exploited by forming a program for comprehensive exploration of the continental crust. And the inductive style, science by serendipity, should be stressed. The program will flourish if, like its counterpart directed toward the oceans after WW II, a variety of kinds of observational methods are employed in an effort to survey the territory comprehensively in both a spatial and a multi-disciplinary sense. The critical ideas will, as they always have, spring up from someone, somehow, somewhere, once those observations become available and known. The program will sputter fitfully if the style of science is hypothesis testing alone. Generation of hypotheses, and hypothesis testing, is appropriate of course, but if only that style is employed, advances will be more or less limited by the imagination of the scientists involved. Those minds will have better and more creative ideas if stimulated by new sets of comprehensive and diverse observations.

On the more practical and humanistic side, the continental crust deserves to be explored thoroughly because it is the part of the universe that humans live on and derive their livelihood from. The upper portions of the crust are accessible now and provide resources that support human life; still deeper portions will become accessible as technology improves with time. We need to know what resources are there and how they can be used effectively to support and benefit the human population. For the foreseeable future, and then some, most humans are not going to live on, or travel to, another planet. They are destined to remain on Spaceship Earth. They need and deserve to know fully the resources that are available to them on this planet.

There are other bases for justification of intense exploration of the continental crust in the near future. Exploration of the ocean basins after WW II was facilitated by the availability of new instruments and new techniques, developed in part during the war for wartime purposes, that could be adapted to observation of the ocean floor. Seismic and acoustic devices, magnetometers, and navigation tools are some examples. Currently, and analogously, there are geophysical techniques and deep drilling methods available and waiting to be applied to study of the buried continental basement. We need only to mount the effort to apply them in widespread and effective fashion.

The wide variety of geophysical techniques that is available for exploration of the deep continental crust includes the seismic reflection and refraction, earthquake seismology, gravimetric, electrical, electromagnetic, magnetic, thermal, and satellite positioning techniques. There is also a variety of geochemical and geological techniques. By way of illustration, I discuss here the seismic reflection and deep drilling techniques.

Based on the strategical reasoning of the preceding paragraphs, at Cornell in the early 1970s Sid Kaufman, a retired industry geophysicist with an eye to innovation in earth science and the know-how to make it happen, and I began a project now known as COCORP (Consortium for Continental Reflection Profiling). We were soon joined by Larry Brown as a graduate student soon to become a new professor at Cornell. The essence of the COCORP project is the application of the powerful seismic reflection technique of the petroleum industry to exploration of the entire continental crust. COCORP is devoted to exploration to depths greater than those normally explored in the search for oil, and to exploration, wherever they are found, of the crystalline rocks of the basement that are not normally of interest to modern industry.

COCORP has surveyed a number of deep seismic reflection profiles in the US, observing in the process only a small fraction of what eventually can and must be observed, and has demonstrated beyond all reasonable doubt the value and efficacy of application of the seismic reflection technique to study of the deep basement (Brown et al, 1986). There are clearly many large, eerie, geological features, normally hidden from view but revealed by the seismic technique, within the deep crust and sometimes the upper mantle. In some areas such as the Appalachians, COCORP data have been critically important to our present level of understanding of major tectonic features. From the COCORP experience and that of related programs that have sprung up in the US and abroad, there can be no doubt that society will eventually demand, and profit from, the knowledge of the subsurface geologic structure of the continents that the seismic reflection technique, drilling, and other means of exploring the crust can provide. How and when that knowledge is obtained is no longer a technical matter; it is a political and financial one.

Drilling is, of course, an obvious and proven technique for exploring, and for recovering resources from, the earth's interior. It is estimated that some two million holes have been drilled in the United States, most relatively shallow but many over 5 kilometers in depth. To date, most deep holes have been drilled in the sedimentary basins, the habitat of petroleum. The record depth so far, some 12 km or about a third of the thickness of the continental crust, was drilled in a scientific experiment in the Soviet Union. It is not unreasonable to hope that depths as great as 20 km or more may eventually be penetrated and sampled.

Widespread exploration of the continents by deep drilling seems a certainty for the future, partly for purely scientific purposes and partly, perhaps largely, because of possible practical benefits. Surely it is in the interest of humans to know what exists and is accessible just a few kilometers beneath their feet. Drilling should be done hand in hand with seismic reflection exploration, using both conventional 2D and modern, high resolution, 3D methods. Other kinds of geophysical exploration based on the seismic, electrical, electromagnetic, gravity, heat flow, etc. methods, all should be part of a coordinated program for comprehensive exploration of the continents. The stimulus and the role for such a program can be found in the history of exploration of the sea floor in the 50s and 60s that led to plate tectonics.

Perceptive readers may claim a possible inconsistency between the proposed future exploration of the continental basement under an "organized" program, and the actual post WW II exploration of the ocean basins used here as a guide or model. For the most part, the exploration of the ocean basins was not organized above the level of the individual research laboratory. That is, no one, so far as I know, tried to make certain that Scripps, Lamont, Woods Hole and the others operated under a coordinated central plan for exploring all the ocean basins. Instead the initiative and the decisions on scope of research and research targets were left to the leaders of those institutions. In fact, this approach worked well in this case because the research laboratories were sufficiently large and diverse so that they could readily handle the task. At present, there are no parallel organizations on which to base a comprehensive exploration program for the continents. Almost certainly the best way to explore the continents would be to form not one but several such organizations. A structure with such research units taking the lead would almost certainly prove preferable to operation under a single central organization within the Beltway. With the multi-organizational research lab structure, the apparent inconsistency referred to at the beginning of this paragraph would vanish; history would have again provided an appropriate precedent.

Peer Review

One major difficulty with operation under a single, central organization is that, with only a small number of administrators and advisors in sole control of a large program, innovation and flexibility are commonly stifled because of absence of competition from outside the organization. Judgments based on peer review, rather than on the individual initiatives and insights of the top scientists, become the controlling factor. That is not to imply that peer review is all bad. To some extent control by peer review is good because, in democratic fashion, it rules out initiatives that are clearly sub-par. Unfortunately however, peer review often, and also in democratic fashion, rules against the exceptionally good initiatives as well. That is because the truly outstanding discoveries in science come as a surprise to even the very best of scientists including, of course, the peer reviewers. Thus prior to learning of the discovery, those scientists acting as peer reviewers might well reject the proposed effort that eventually would have led to the discovery.

This phenomenon can be neatly documented. It was demonstrated clearly and spontaneously at the American Geophysical Union spring meeting of 1992 held in Montreal. A special session was held at that meeting in honor of what was called, somewhat arbitrarily, the 25th anniversary of the coming of plate tectonics. At that session eight scientists, all key participants in the revolution during the peak of the upheaval a quarter of a century earlier, gave presentations. All were given a minimum of guidance from others on what to say. The eight talks, in other words, were developed independently with virtually no coordination among the speakers.

If my memory is correct, and I think it is because I was so strongly impressed by the phenomenon at the time, five of those eight speakers spontaneously drew attention to some aspect of the revolution that had been unjustly delayed or otherwise impeded by peer review. Ideas that would turn out later to be key to the revolution were rejected in proposal form or in papers submitted for publication. I have never seen a more dramatic and clear-cut demonstration that peer review does not work well, or at least as effectively as it might, when a truly major advance in science is being made. Remember that those complaints were made by prominent scientists 25 years after the incidents, long after those scientists might have been suspected of harboring sour grapes, or of having an ax to grind, or of having a ruffled ego. I do not mean to imply that peer review should be abandoned for it is not clear that a better method for judgment of basic science can be devised, but we must keep our eyes open and be aware that peer review may impede progress at some of the most critical times in science, specifically those times of major discovery and paradigm shift.

A somewhat related matter is the unfortunate tendency of bureaucratically controlled funding agencies to break programs into small pieces, each piece run by a single manager and each piece militantly protected by its constituency.

As one means to overcome the deficiencies of the peer review system, I suggest that some sort of publication be designed to encourage the printing of radical ideas and hypotheses that might not make it into the regular scientific journals because of rejection based on peer review. A non-reviewed, free-wheeling journal would, of course, contain many hypotheses doomed to failure, and the journal would draw some derogatory comments, but, if done circumspectly, it would draw many readers, stimulate the imagination, and provide a means for airing a radical hypothesis that might catch on and reorganize some branch of science. Perhaps such a "journal" will evolve as the new electronic forms of communication continue to develop.

Communication of Science

From the history of plate tectonics in the earlier pages, as well as from histories of other branches of science, it seems clear that, in order for an individual to make an impact in the world of science, just having a good idea is not sufficient. It is also necessary that the idea be communicated appropriately to the milieu where the idea can catch on. As Menard (1986) has pointed out, ideas are plentiful in science. Most, including some very good ones, have little impact on the mainstream of science at least partly because they are not communicated properly. Appropriate communication of a good idea, in other words, is as important as generation of the idea. That point seems obvious, yet it is often overlooked. Wegener, for example, made his ideas widely-known through oral presentation and publication. Holmes' ideas are readily available in his outstanding text (Holmes, 1944), but his original paper on the subject that would become plate tectonics was published in Transactions of the Geological Society of Glascow and hence overlooked by Americans and others whose libraries did not include that journal and who would become leaders in the establishing of plate tectonics.

Good communication is also vitally important in other ways in science. The history of plate tectonics demonstrates very clearly that the important contributions of many individuals were made at least partly because those individuals happened to be in the right place and at the right time, and not necessarily because those particular individuals were exceptionally gifted. Being at the right place at the right time often meant that the individual was situated so as to receive and assemble the key information, which was often split into multiple components and which revealed the opportunity for discovery before others were so informed. Good communication not only within a discipline but also, and especially, across discipline boundaries was important both early on and as the revolution progressed and broadened its scope. Good papers written with a minimum of jargon and those with the special figures that become "icons" became important, as noted earlier, partly because they communicated information readilly across discipline boundaries. It is easy to make the case that good communication is not only desirable in science, it is essential. It is deserving of special attention and effort on the part of the scientist to assure that he or she is favorably situated within the communication network, and that his or her papers of importance and broad interest are written for an audience of non-specialists.

Sometime ago, but after the transformation of earth science during the 1960s and early 1970s, someone attempted to list the dates when various earth scientists had "converted" from the "fixist" camp to the "plate tectonics" camp. At first, some thought that such a chronology might provide a measure of the scientific ability, or the expertise, or just the "savvy," of the individuals involved. But it is far more likely that the ordering of that list was much more dependent on the times when the significant and convincing information happened to reach each individual. Nearly every earth scientist recognized the importance of the new concept once the news and the essence of the concept were conveyed to that individual. The history of the plate tectonics revolution makes the point emphatically that communication is of the essence in science and so implies that maintaining and enhancing the means of active and efficient communication in science deserve continuing and appropriate attention.

At present, communication in science appears to be in the early stages of a period of major transition. On the one hand, there is the well-established, conventional form of recording and communicating science through the scientific paper in the scientific journal. This style was set in the nineteenth century and has served science well, but with time it has developed some notable deficiencies as a consequence of growth and increasing specialization in science, proliferation of jargon, and a style of presentation of information that is not optimized with regard to communication and retrieval of essential information.

Even if nothing else were to happen to affect the realm of scientific communication, the established system of publication in scientific journals of papers, all written in stereotyped style, would need reevaluation and modernization. Modern scientific papers are often not being read by the intended audience and hence are not communicating information well, if at all. Too many are written only for the small circle of specialists in that field who know the jargon or, who through peer review, influence the activity and funding in that specialty. Certainly all the information of modern science is not being communicated, or stored, optimally. Often things already reported in the literature are "discovered" anew by someone unable or unwilling to keep up with the record of past discovery. If this trend continues and the problems continue to grow, we may someday reach an equilibrium state in which supposedly new scientific discoveries occur at, or even less than, the rate at which the same scientific discoveries already made earlier are forgotten!

But something else is happening in the realm of scientific communication, and in all other forms of communication. It is the electronic revolution, a truly awesome phenomenon of our times. We have at present seen only the beginning of what promises to be a complete upheaval in the ways in which society stores and communicates information and in which individuals learn from that information. From the beginnings we can already sense the awesome potential of this revolution and some of the problems that will be encountered. I do not pretend to clairvoyance here and make no specific predictions on how it will all turn out but one point seems clear. The electronics revolution, coupled with growing deficiency in the present system, is going to force a major change in the way scientists store and communicate the information of science. It is already beginning to do so. Furthermore, the effects will be profound and go well beyond the simple processes of storage and retrieval of information.

To illustrate this point with just one example, consider the matter of thorough observation of all of the earth's surface geology. This huge task has occupied innumerable geologists for decades, even centuries. They have walked over almost every part of the dry land surface of the earth, eventually to produce comprehensive information in the form of maps, at least on a reconnaissance basis, of all of that surface. Now, because of the coming of the electronic age, that task will almost certainly have to be redone-completely redone! The reason it will be redone is that the raw information so obtained has been collated and then stored, but in most cases only stored selectively in interpreted form, primarily geologic maps. Going from the raw observations to geologic maps was necessary to, among other things, organize and reduce the amount of information stored and transmitted.

In the new electronic age, however, it is possible to store huge quantities of data. It may eventually be possible to store, and retrieve on command, any or all of the raw observational data of geology, not just the interpreted and condensed forms of those data. From the raw data bank, computers will quickly produce, for example, maps emphasizing those features of particular interest at the moment, not merely those selected by some early map maker. Surely such flexibility would be a boon to the science. The catch is that those raw observations on the earth's geology have not been stored systematically, nor are they in computer compatible form. Eventually we shall want all of those data in that computer compatible form and so probably we shall have to re-observe most of the world's surface geology, this time recording those observations in a new fashion in order to get it that way. The task is not trivial, of course, but neither is it insurmountable, and modern technology will make it easier than it once was.

At present we face the danger that progress in earth science is being skewed away from the optimum course because only a fraction of the observational data of our science are in computer compatible form.

Computers are making possible the application of marvelous new techniques for handling, processing, and analyzing data, but those techniques can only be applied to computer compatible data. However, our science is about organization of observations, all observations, of earth. We eventually must take steps to insure that all important raw observations of earth, including particularly those made in the field by geologists, are computer compatible, so that the direction of the science is not restricted or restrained by incompatibility of some of our most important data with new electronic devices.

The Role of Scientific Societies

Let us now turn to the role of scientific societies in the plate tectonics revolution. Because of the broad scope of the subject, almost all earth science societies were affected in some manner by the revolution, some during its early formative stages, others later. There can be little doubt that, of all scientific societies, the American Geophysical Union had the biggest impact on the growth of the plate tectonics revolution, and vice versa. The extraordinary AGU meeting of 1967 has already been cited several times in this regard in earlier chapters. Furthermore, a disproportionate number of key publications on plate tectonics and its forerunners appeared in AGU's Journal of Geophysical Research. Scientists of the discovery era clearly thought of AGU as the place where the action in the subject was to be found. Why was this so? Was it just chance and good fortune that AGU was focused in just the right subject areas so as to capture the central position as those subjects were integrated under the new concept of plate tectonics? Or were the policies of AGU different from those of other societies in a way that fostered the revolution?

I think the answer to both of those questions is yes. Clearly AGU was in the right place at the right time to capture the plate tectonics action, including particularly the forerunners. For example, geophysical observations of the deep sea floor were a key piece of early information, and AGU, by welcoming presentations of this then rather unusual kind of information on the earth at its meetings and in its publications, established itself as a, if not the, place to report such observations. Furthermore, through wise planning, and probably some good fortune, the AGU organization included sections in most of the specialties that would be important to the revolution. And certainly not all of those specialties were popular at the time they were formed. I can recall, for example, meetings during the 1950s when attendance at the sessions of the Tectonophysics section often numbered half a dozen at most. Later, as plate tectonics blossomed, multiple sessions of that particular section packed the meeting rooms.

Certain aspects and policies of AGU set it apart from other earth science societies, and leaders of the AGU during that early era deserve credit for their foresight. For one thing, AGU set out to be interdisciplinary, merging geology and physics and chemistry. AGU covered a broad range of specialties and hence the audiences it generated were more attractive to a speaker with a topic of broad significance than was a more specialized society. Such societies, confined to one narrow subdiscipline, attract almost exclusively speakers who feel their talk has appeal only for those specialists. In the age of modern science when the biggest discoveries are commonly cross-or inter-disciplinary, it seems obvious that the society with the broader purview is more likely to get the most important papers, unless of course, the society is so broad that only a small fraction of its members might be interested in the paper even though it might span several specialties. Thus the Seismological Society of America, the Mineralogical Society of America and the Paleontological Society were too narrowly oriented to capture the main action of the plate tectonics revolution, the American Association for the Advancement of Science was too broadly oriented, and the American Geophysical Union was just about right.

Furthermore, as best I can recall, AGU had a relatively liberal policy on the subject matter of papers, whether printed or presented orally. Submitted papers of some merit that did not clearly fit within the boundaries of a particular AGU section were nevertheless somehow shoehorned into the meeting or the journal. Some papers that presented ill-conceived hypotheses or that were based on questionable reasoning slipped in the back door under this policy, and generated some scorn and negative comments in the halls of the meeting place after the presentation of course, but in the long run the society was not harmed by this policy and eventually the liberal policy brought the society benefit and the science both stimulation and advance.

As noted earlier, and in my opinion, scientific circles should provide more rather than fewer opportunities for presentation of the off-beat, long shot, hypothesis. AGU's success in capturing the excitement of the plate tectonics revolution, and the failure of other societies to do so when they might have, demonstrates this point.

Furthermore, although I cannot cite specific experiences from the plate tectonics revolution to support it, it is also my opinion that most modern scientific societies, though focused appropriately on hard science, should include sections on the history and philosophy of their science as well. If appropriate activities developed, hard scientists might learn strategies and tactics from history and the analysis of it, and historians and philosophers would have closer interaction with active scientists and hence firmer grounding in the science than they might otherwise experience.

Opportunities in Seismology

And then there is the question of what ideas this particular history of seismology's role in the plate tectonics revolution might provoke and so improve the science of seismology and related disciplines in the future. This topic is a difficult one because in most ways the science of seismology is currently in a healthy state, certainly far healthier than it was in the early days of the subject as discussed in previous chapters. Since the middle of this century, there has been a marked increase in the numbers of seismologists, and in the numbers of exceptionally talented and well-trained seismologists. Observational and analytical facilities have grown remarkably. Furthermore, modern seismologists have been aggressive and adept at developing new subject areas, new approaches, and new techniques.

To cite just a few of those developments, I draw attention to current or recent work on topics such as paleoseismicity; focal mechanisms; digital data handling and processing; tomogaphy; prompt high speed transmission of information; earthquake-related geodetic studies; slow earthquakes; spatial and temporal patterns of seismicity; and the exploration of the crust and upper mantle using explosive and vibratory sources. Against this array of advances and signs of vitality, it is difficult to earmark fertile new areas for research in the future, but a few suggestions follow nevertheless.

First, and this will come as no surprise to seismologists for the point has often been made before, there is a strong need to remedy the dearth of observations for the areas of the ocean basins by operating numerous observing stations on or below the sea floor. Some two-thirds of the earth's surface area, except for a few island stations and an occasional research instrument on the ocean bottom, is free of seismic stations. The resulting and enormous gap in information grows in importance as more and more we seek to resolve departures from the spherically-symmetric model of earth. At some time in the future we must observe the seismic motion of the entire earth's surface in a comprehensive manner; the oceanic areas are clearly the sites of the greatest deficiency in coverage.

Interaction between seismology and geology on a regular but selective basis commonly takes place now and with some striking examples of success. However there is potential for considerable increase in productive interaction in the future. The case of exploration of the continental crust provides a good example of that potential. Traditionally, for the uppermost crust, particularly the basement, the observational base is primarily geological in nature and the data are very numerous and extremely complex. As a consequence, a style of reasoning and of analysis of such data that might be termed the "geological style" prevails. For the lower portion of the crust, the data base is primarily geophysical and the data, in contrast to the case for the upper crust, are sparse indeed. Consequently, a "geophysical style" of reasoning involving numbers and models with geometrically simple components, is the norm.

However, as more detailed, more complex, and more plentiful information on the deep crust is acquired, as it surely will be through application of the 2D and 3D seismic reflection techniques, a situation more like the case for the upper crust will develop. The "geological style" of reasoning will supersede the "geophysical style" and will be applied at increasingly greater depths, eventually to include all of the crust and uppermost mantle, and perhaps eventually all of the mantle. Seismologists can foresee this development and move aggressively into this interdisciplinary area by appropriately adjusting the tenor of the scientific meetings of their societies and the scope of their specialized journals. Geologists can likewise move further into the subject by extending the purview of their organizations. Continual adjustment and some overlapping of the scope and nature of the societies will insure that this important interdisciplinary subject does not fall into a crack between organizations, or force the formation of a new and overly specialized one.

History shows that seismologists have made remarkable progress in their subject through emphasis of an analytical style of study of seismological data, following in general the reductionist mode pioneered in physics. In this mode, data on the very complex motions of the earth following an earthquake are organized and generalized by recognizing and defining characteristic phases and wave trains that are found most everywhere, regardless of position on the earth. Such information is, as noted earlier, the basis for much of what we know of the earth's interior.

However, there remains a storehouse of information recorded on multitudinous seismograms from innumerable seismograph stations that cannot be so readily generalized because it is location specific. Such information has occasionally been studied in special cases but much of it is ignored, at least partly because to conventionally trained seismologists it seems too complex and too diverse to be handled neatly. Seismologists might take a tip here from geologists who commonly face large quantities of complex, location specific, data. They have developed various ways of handling and describing such information-geologic maps in all their variety provide an example. Seismologists might develop similar techniques. Nearly every station seismologist eventually recognizes information peculiar to that station but, at present, nowhere are those observations catalogued and portrayed so that they can be assimilated by any seismologist anywhere. There seems a huge opportunity here as emphasis on the earth's subtle 3D characteristics grows. The study by Molnar and Oliver (1969), cited earlier as support for the plate tectonics theory, is one very crude example of the use of this kind of information in this fashion.

The lack of summaries or syntheses in book or map form of the kind of seismological information just referred to draws attention by a particular example to a general and widespread shortcoming in science. It was mentioned briefly in an earlier chapter. Modern science has produced a huge, virtually inestimable, amount of knowledge of the natural world. Most of that information is written in the form of articles in scientific journals and stored on the shelves of libraries. But most of those journal articles are narrowly oriented. They concern primarily and often exclusively some fragment of information that the author hopes might someday bear on much broader subjects. However, comprehensive syntheses of such information on particular subjects are limited. There is, in other words, and in my opinion, a crying need for more books or summaries or reviews that thoroughly and authoritatively synthesize knowledge on particular topics in a manner that is more compact than the numerous and diverse journal articles on which such a book can be based yet is far more detailed than, say, a section of a general textbook.

Such books exist for some selected topics, of course, but not for many and not nearly in sufficient quantity to make communication of information in science as efficient as it might be. The problem arises because the kind of book I refer to will not have a sufficiently large audience to be profitable commercially and hence is not encouraged by private publishers. Funding agencies for science, though generous in their support for journal articles, are typically reluctant to fund syntheses because the work is not "new" research. We need to encourage efforts by individuals who will review journal articles in various subject areas and draw the essence of knowledge of those subjects into a form of review, a book or perhaps now a video, for those who need the information but have not the time for a thorough literature review of their own. If we do not soon mount an effort to organize scientific knowledge, one that goes well beyond that of review articles in journals and the all too occasional review volume, as noted earlier science may someday degenerate to the point where most "new" discoveries are all rediscoveries of phenomena once known, but then hidden and forgotten!

Seismographs operating as they do for 24 hours per day, day after day, at numerous locations record a huge amount of data on not only earthquakes but also on the background noise of the earth, a phenomenon called "microseisms." Although microseisms have been studied from time to time and in some detail, this subject is not exhausted. Furthermore it calls for knowledge of meteorology and oceanography as well as seismology, because winds generate the ocean waves that in turn generate the microseisms. It calls for knowledge of geology because microseisms are mostly surface waves of such short wavelengths that they are highly sensitive to spatial changes in geology. We can conclude that the subject is not exhausted because microseismic activity is not understood sufficiently well to enable prediction of seismic background noise at any particular time and place. This subject calls for additional attention and for fresh ideas on how to study, analyze, and organize the background noise of the earth.

During the electronics revolution, and particularly since the early phases of the digital era, advances in means for handling and processing seismic data have been truly astounding, and seismology would be far more primitive if it had failed to capitalize on those newly available capabilities. I am a strong supporter of these developments and do not intend the following point to detract in any way from the importance of the new digital tools of seismology.

However, when viewing the science from the historical perspective, it is evident that the excitement that pervades the subject of seismology is partly a consequence of the surprise that is inherent in the earth's seismological activity. There is something awesome and fascinating about learning that the earth has just produced another major earthquake, no matter how many such events one has observed in the past. There is something stimulating about working in a subject in which each day might bring yet another unexpected and information-laden event. Thus there is the danger that something intangible is lost when seismic motion of the earth is recorded solely in non-visible electronic form such as tapes or discs. The excitement and the vitality of old-time seismology depended partly on the old pen and ink recorders that gave even the casual viewer a sense of earth as a normally quiescent body whose peace was occasionally interrupted by a huge catastrophic event. Such an event brought pain and suffering to some but wonder and excitement and intrigue to others as questions about the location and size of the event were answered before their eyes as wave forms slowly accumulated on the recording in full view of spectators.

I urge seismologists to maintain, in addition to modern means for electronic data storage, visibly recording devices (and not just a video with the last few minutes of data on the screen) so that generations of future seismologists, and others, scientist and non-scientist alike, can experience that thrill and excitement and stimulation. Let the earth, in its unpredictable yet characteristic style, tell them that it is dynamic, that it has ruptured once again, and that seismology and plate tectonics, working together in the manner we learned to understand during the 1960s are at it once again.

In the ideal world as I see it from my biased position as a seismologist, every institution of higher learning and every high school giving instruction in earth science would have such a seismograph, visibly recording and prominently displayed, so that all students can sense the adventure of monitoring the unpredictable fits and starts of our planet.

And then, last but surely not least, there is the continuing opportunity to relate new kinds of observations to earthquake occurrence, as technology develops to make those new observations possible. The prime current example of such opportunity concerns refinements in global positions based on satellite networks which seem now to be capable of providing long term strain and strain rate information in seismically active areas. Surely the understanding of earthquakes, and plates, will be enhanced by this development.

11

Epilogue

In a world beset with defiances
It behooves us to build more alliances
So why not immerse
Technical jargon in verse
And integrate art with the sciences?

Finally, I would like to share some misgivings about authoring and publishing a book like this one. There is a danger, I sense, that some young scientists will read histories like this that portray cases of dramatic success in science and feel depressed and unfortunate because they missed it. They may feel that as a result of an accident of birth that brought them along a little too late, they were not around for the fun, the excitement, the satisfaction, and the accolades of the plate tectonics revolution.

For those who feel that way I have advice in the form of a single contraction: "Don't." No one working in earth science today should feel that they have missed out on opportunity for discovery for there surely will be more big and exciting discoveries in earth science in the future. That is clearly evident, and a virtual certainty, because much of the earth is unobserved by the many and various means that are available to us. There are many important observations of earth remaining to be made. Fresh new observations of an important part of earth nearly always produce surprises and discoveries. Thus if young scientists today get out and make those observations in the style of earth explorers of the past, or in a still better style, they will surely share in future revelation about earth.

To strengthen this point further, I note that when I was a student, well before the plate tectonics revolution, my fellow students and I often experienced a sense of discouragement when we read a paper that described some earlier earth scientist's triumph. We had come along too late and we had missed the days of golden opportunity, we thought. I distinctly recall feeling that way when I read Gilbert's paper (Gilbert, 1928) on the geology of the "Basin and Range Province." Perhaps if I had come along a generation or two earlier, I could have done something like that, I thought. "Was that the last great opportunity?" We asked ourselves that question over and over again. Well, of course it wasn't and my fellow students and I were all dead wrong and pitifully naive to imagine that all the big discoveries had already been made. Plate tectonics was just ahead, and had we been smart enough and optimistic enough, perhaps we might have sensed that upcoming revolution, as a few of our leaders did. In no case, however, should we have lost hope.

Based on that experience with those early misperceptions, and the many indications of promise of future great discovery about the earth cited elsewhere in this book, I foresee a period of exciting discovery in earth science for at least a century or more. If modern young scientists set visionary goals and pursue them with skill, determination and perseverance, the exciting story of the discovery of plate tectonics will surely fade into the background as they report their own astonishing discoveries about earth. For the older generation the era of discovery of plate tectonics was a joy to experience and it remains a joy to remember, but for the younger generation it should be but a stimulus and a stepping stone. All of us - old scientists, young scientists, and the public at large - will be better off if it turns out that way.

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INDEX

T

APPENDIX

"Seismology and the New Global Tectonics"

by

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Seismology and the New Global Tectonics[1]

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A comprehensive study of the observations of seismology provides widely based strong support for the new global tectonics which is founded on the hyptheses of continental drift, sea-floor spreading, transform faults, and underthrusting of the lithosphere at island arcs. Although further developments will be required to explain certain part of the seismological data, at present within the entire field of seismology there appear to be no serious obstacles to the new tectonics. Seismic phenomena are generally explained as the result interactions and other processes at or near the edges of a few large mobile plates of lithosphere that spread apart at the ocean ridges where new surficial materials arise, slide past one another along the large strike-slip faults, and converge at the island arcs and arc-like structures where surficial materials descend. Study of world seismicity shows that most earthquakes are confined to narrow continuous belts that bound large stable areas. In the zones of divergence and strike-slip motion, the activity is moderate and shallow and consistent with the transform fault hypothesis; in the zones of convergence, activity is normally at shallow depths and includes intermediate and deep shocks that grossly define the present configuration of the down-going slabs of lithosphere. Seismic data on focal mechanisms give the relative direction of motion of adjoining plates of lithosphere throughout the active belts. The focal mechanisms of about a hundred widely distributed shocks give relative motions that agree remarkably well with Le Pichon's simplified model in which relative motions of six large, rigid blocks of lithosphere covering the entire earth were determined from magnetic and topographic data associated with the zones of divergence. In the zones of convergence the seismic data provide the only geophysical information on such movements.

Two principal types of mechanisms are found for shallow earthquakes in island arcs: The extremely active zone of seismicity under the inner margin of the ocean trench is characterized by a predominance of thrust faulting, which is interpreted as the relative motion of two converging plates of lithosphere; a less active zone in the trench and on the outer wall of the trench is characterized by normal faulting and is thought to be a surficial manifestation of the abrupt bending of the down-going slab of lithosphere. Graben-like structures along the outer walls of trenches may provide a mechanism for including and transporting sediments to depth in quantities that may be very significant petrologically. Large volumes of sediments beneath the inner slopes of many trenches may correspond, at least in part, to sediments scraped from the crust and deformed in the thrusting.

Simple underthrusting typical of the main zone of shallow earthquakes in island arcs does not, in general, persist at great depth. The most striking regularity in the mechanisms of intermediate and deep earthquakes in several arcs is the tendency of the compressional axis to parallel the local dip of the seismic zone. These events appear to reflect stresses in the relatively strong slab of down-going lithosphere, whereas shearing deformations parallel to the motion of the slab are presumably accommodated by flow or creep in the adjoining ductile parts of the mantle. Several different methods yield average rates of underthrusting as high as 5 to 15 cm/yr for some of the more active arcs. These rates suggest that temperatures low enough to permit dehydration of hydrous minerals and hence shear fracture may persist even to depths of 700 km. The thickness of the seismic zone in a part of the Tonga arc where very precise hypocentral locations are available is less than about 20 km for a wide range of depths. Lateral variations in thickness of the lithosphere seem to occur, and in some areas the lithosphere may not include a significant thickness of the uppermost mantle.

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The lengths of the deep seismic zones appear to be a measure of the amount of under thrusting during about the last 10 m.y. Hence, these lengths constitute another 'yardstick' for investigations of global tectonics. The presence of volcanism, the generation of many tsunamis (seismic sea waves), and the frequency of occurrence of large earthquakes also seem to be related to underthrusting or rates of underthrusting in island arcs. Many island arcs exhibit a secondary maximum in activity which varies considerably in depth among the various arcs. These depths appear, however, to correlate with the rate of underthrusting, and the deep maxima appear to be located near the leading (bottom) part of the down-going slab. In some cases the down-going plates appear to be contorted, possibly because they are encountering a more resistant layer in the mantle. The interaction of plates of lithosphere appears to be more complex when all the plates involved are continents or pieces of continents than when at least one plate is an oceanic plate. The new global tectonics suggests new approaches to a variety of topics in seismology including earthquake prediction, the detection and accurate location of seismic events, and the general problem of earth structure.

Introduction

This paper relates observations from the field of seismology and allied disciplines to what is here termed the 'new global tectonics.' This term is used to refer in a general way to current concepts of large-scale tectonic movements and processes within the earth, concepts that are based on the hypotheses of continental drift [*Wegener*, 1966], sea-floor spreading [*Hess*, 1962; *Dietz*, 1961], and transform faults [*Wilson*, 1965*a*] and that include various refinements and developments of these ideas. A comprehensive view of the relationship between seismology and the new global tectonics is attempted, but there is emphasis on data from earthquake seismology, as opposed to explosion seismology, and on a particular version of the sea-floor spreading hypothesis in which a mobile, near-surface layer of strength, the lithosphere, plays a key role. Two basic questions are considered. First, do the observations of seismology support the new global tectonics in some form? To summarize briefly, they do, in general, give remarkable support to the new tectonics. Second, what new approaches to the problems of seismology are suggested by the new global tectonics? There are many; at the very least the new global tectonics is a highly stimulating influence on the field of seismology; very likely the effect will be one of revolutionary proportions.

The mobile lithosphere concept is based partly on an earlier study [*Oliver and Isacks*, 1967], but, as presented here, it incorporates ideas from *Elsasser* [1967], who independently developed a model with many similar features based on entirely different considerations, and ideas from *Morgan* [1968] and *Le Pichon* [1968], who pursued this concept further by investigating the relative motion in plan of large blocks of lithosphere.

Figure 1 is a block diagram illustrating some of the principal points of the mobile lithosphere hypothesis. In a relatively undisturbed section, three flat-lying layers are distinguished: (1) the *lithosphere*, which generally includes the crust and uppermost mantle, has significant strength, and is of the order of 100 km in thickness; (2) the *asthenosphere*, which is a layer of effectively no strength on the appropriate time scale and which extends from the base of the lithosphere to a depth of several hundred kilometers; and (3) the *mesosphere*, which may have strength and which makes up the lower remaining portion of the mantle and is relatively passive, perhaps inert, at present, in tectonic processes. (Elsasser refers to the lithosphere as the tectosphere and defines some other terms somewhat differently, but the terminology of *Daly* [1940] is retained here. The term 'strength,' which has many definitions and connotations, is used here, following Daly, in a general sense to denote enduring resistance to a shearing stress with a limiting value.) The boundaries between the layers may be gradational within the earth. The asthenosphere corresponds more or less to the low-velocity layer of seismology; it strongly attenuates seismic waves, particularly high-frequency shear waves. The lithosphere and the mesosphere have relatively high seismic velocities and propagate seismic waves without great attenuation.

At the principal zones of tectonic activity within the earth (the ocean ridges, the island arc or island-arc-like structures, and the major

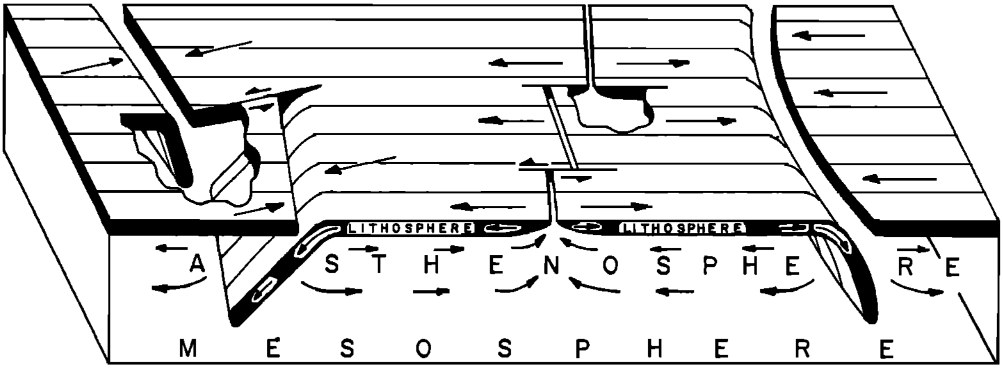


Fig. 1. Block diagram illustrating schematically the configurations and roles of the lithosphere, asthenosphere, and mesosphere in a version of the new global tectonics in which the lithosphere, a layer of strength, plays a key role. Arrows on lithosphere indicate relative movements of adjoining blocks. Arrows in asthenosphere represent possible compensating flow in response to downward movement of segments of lithosphere. One arc-to-arc transform fault appears at left between oppositely facing zones of convergence (island arcs), two ridge-to-ridge transform faults along ocean ridge at center, simple arc structure at right.

strike-slip faults) the lithosphere is discontinuous; elsewhere it is continuous. Thus, the lithosphere is composed of relatively thin blocks, some of enormous size, which in the first approximation may be considered infinitely rigid laterally. The major tectonic features are the result of relative movement and interaction of these blocks, which spread apart at the rifts, slide past one another at large strike-slip faults, and are underthrust at island arcs and similar structures. *Morgan* [1968] and *Le Pichon* [1968] have demonstrated in a general way and with remarkable success that such movement is self-consistent on a worldwide scale and that the movements agree with the pattern of sea-floor spreading rates determined from magnetic anomalies at sea and with the orientation of oceanic fracture zones. *McKenzie and Parker* [1967] used the mobile lithosphere concept to explain focal mechanisms of earthquakes, volcanism, and other tectonic features in the northern Pacific.

Figure 1 also demonstrates these concepts in block diagram form. Near the center of the figure the lithosphere has been pulled apart, leaving a pattern of ocean ridges and transform faults on the surface and a thin lithosphere thickening toward the flanks beneath the ridge as the new surface material cools and gains strength. To the right of the diagram the lithosphere has been thrust, or has settled, beneath an island arc or a continental margin that is currently active. At an inactive margin the lithosphere would be unbroken or healed. The left side of the diagram shows two island-arc structures, back to back, with the lithosphere plunging in a different direction in each case and with a transform fault between the structures. Whereas the real earth must be more complicated, particularly at this back-to-back structure, this figure represents, in a general way, a part of the Pacific basin including the New Hebrides, Fiji, Tonga, the East Pacific rise, and western South America.

The counterflow corresponding to movement of the lithosphere into the deeper mantle takes place in the asthenosphere, as indicated schematically by the appropriate arrows in the figure. To what extent, if any, there is flow of the adjoining upper part of the asthenosphere in the same direction as the overlying lithosphere is an important but open question, partially dependent on the definition of the boundary. A key point of this model is that the pattern of flow in the asthenosphere may largely be controlled by the configurations and motions of the surface plates of lithosphere and not by a geometrical fit of convection cells of simple shape into an idealized model of the earth. It is tempting to think that the basic driving mechanisms for this process is gravitational instability resulting from surface cooling and hence a relatively high density of near-surface mantle materials. Thus, convective circulation in the upper mantle might occur as thin blocks of lithosphere of large horizontal dimensions slide laterally over large distances as they descend; a compensating return flow takes place in the asthenosphere. The process in the real earth must be more complex than this simple model, however. The reader is referred to *Elsasser* [1967] for a discussion of many points relating to this problem.

Alternatively, the surface configuration might be taken as the complicated response of the strong lithosphere to relatively simple convection patterns within the asthenosphere. Thus, the basic question whether the lithosphere or the asthenosphere may be thought of as the active element, with the other being passive, is not yet resolved. Probably, however, there has been a progressive thinning of the convective zone with time, deeper parts of the mantle having also been involved during early geologic time.

Figure 2, adapted from *Le Pichon* [1968] with additions, shows the plan of blocks of lithosphere as chosen by Le Pichon for the spherical earth and indicates how their movements are being accommodated on a worldwide scale. The remarkably detailed fit between this scheme, based on a very small number of rigid blocks of lithosphere (six) and the data of a number of fields, is very impressive. The number and configuration of the blocks of lithosphere is surely larger than six at present and almost certainly the pattern has changed within geologic time, but the present pattern must, in general, be representative of at least the Quaternary and late Tertiary. The duration of the current episode of sea-floor spreading is not known. Some evidence suggests that it began in the Mesozoic and has continued rather steadily to the present. Other evidence [*Ewing and Ewing*, 1967] indicates that the most recent episode of spreading began about 10 m.y. ago. This suggestion is considered here because it opens new possibilities for explaining certain seismological observations, particularly the configuration of the deep earthquake zones. Other explanations for such evidence are also considered, however.

With this one very simple version of the new global tectonics as background it is possible to begin considering the data, but in this process it soon becomes evident that much more detailed information on the earth is available and that the hypothesis and the earth model can be developed much further. These developments are presented later in the text as the relevant data are discussed.

This paragraph gives a brief review of some of the developments leading to the new global tectonics. A number of contributions vital to the development of the current position on this topic are cited, but the review is not intended to be comprehensive. The literature bearing on this topic is voluminous, is widespread in space and time, and differs in degree of relevance, so that a thorough documentation of its development is a job for a historian, not a scientist. The hypothesis of continental drift had a substantial impact on the field of geology when it was proposed in 1929 by *Wegener* [1966], but until recently it had not received general acceptance, largely because no satisfactory mechanism had been proposed to explain the movement, without substantial change of form, of the continents through the oceanic crust and upper mantle. When many new data became available, particularly in the fields of marine geology and geophysics, *Hess* [1962] and *Dietz* [1961] proposed that the sea floor was spreading apart at the ocean ridges so that new 'crust' was being generated there while older 'crust' was disappearing into the mantle at the sites of the ocean trenches. The driving mechanism for this spreading was thought to be convection within the mantle. The remarkable success with which the hypothesis of sea-floor spreading accommodated such diverse geologic observations as the linear magnetic anomalies of the ocean [*Vine and Matthews*, 1963; *Pitman and Heirtzler*, 1966], the topography of the ocean floor [*Menard*, 1965], the distribution and configuration of continental margins and various other land patterns [*Wilson*, 1965*a*; *Bullard*, 1964; *Bullard et al.*, 1965], and certain aspects of deep-sea sediments [*Ewing and Ewing*, 1967] raised this hypothesis to a level of great importance and still greater promise. The contributions of seismology to this development have been substantial, not only in the form of general information on earth structure but also in the form of certain studies that bear especially on this

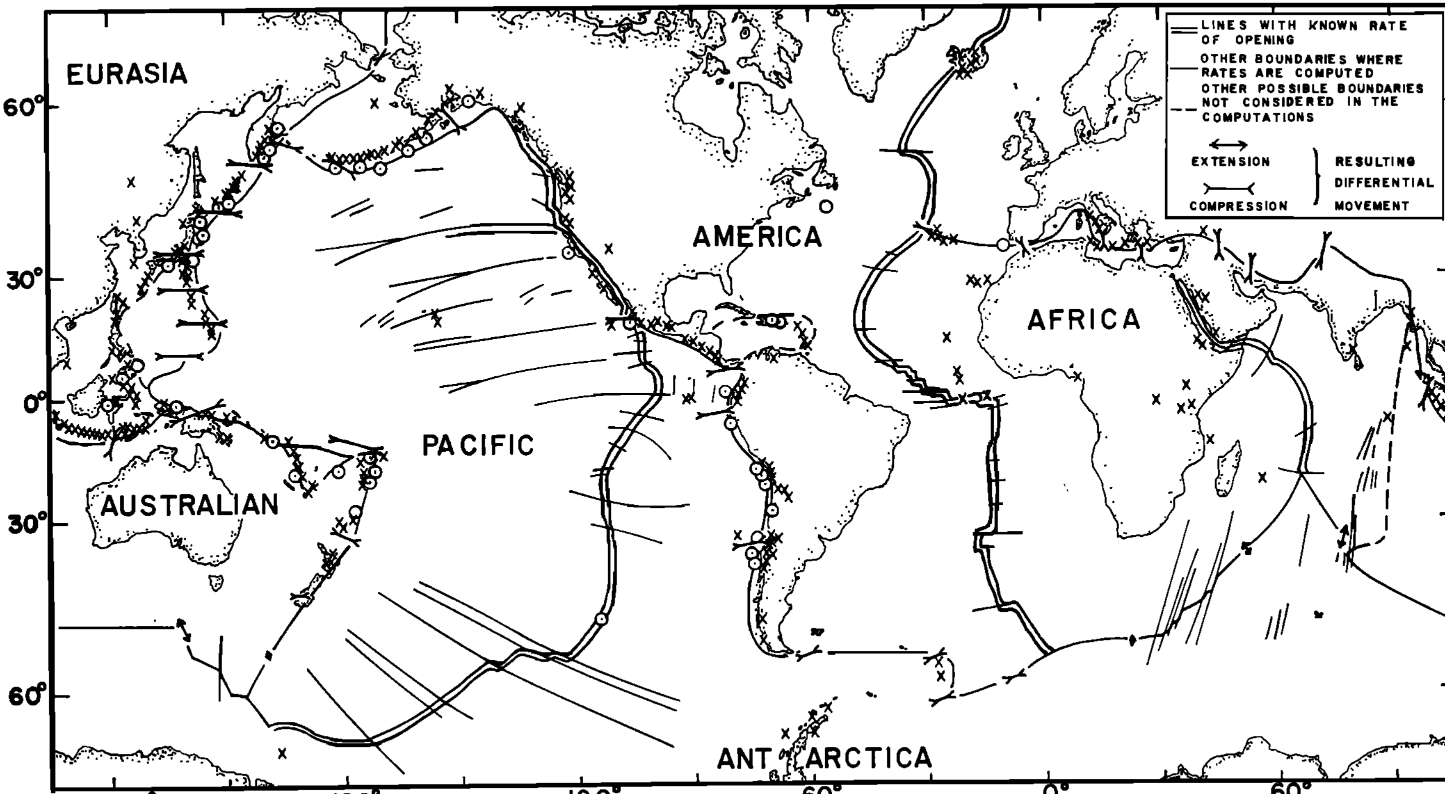


Fig. 2. Computed rates of compression and extension along boundaries of six lithospheric blocks [after *Le Pichon*, 1968]. Computed movements were derived from rates of spreading determined from magnetic data and from orientations of fracture zones along features indicated by double lines. The extensional and compressional symbols in the legend represent rates of 10 cm/yr; other similar symbols are scaled proportionally. Symbols appearing as diamonds represent small computed rates of extension for which the arrowheads coalesced. Historically active volcanoes [*Gutenberg and Richter*, 1954] are denoted by crosses. Open circles represent earthquakes that generated tsunamis (seismic sea waves) detected at distances of 1000 km or more from the source.

hypothesis. Two specific examples are *Sykes*'s [1967] evidence on seismicity patterns and focal mechanisms to support the transform fault hypothesis of *Wilson* [1965*a*] and *Oliver and Isacks*'s [1967] discovery of anomalous zones that appear to correspond to underthrust lithosphere in the mantle beneath island arcs.

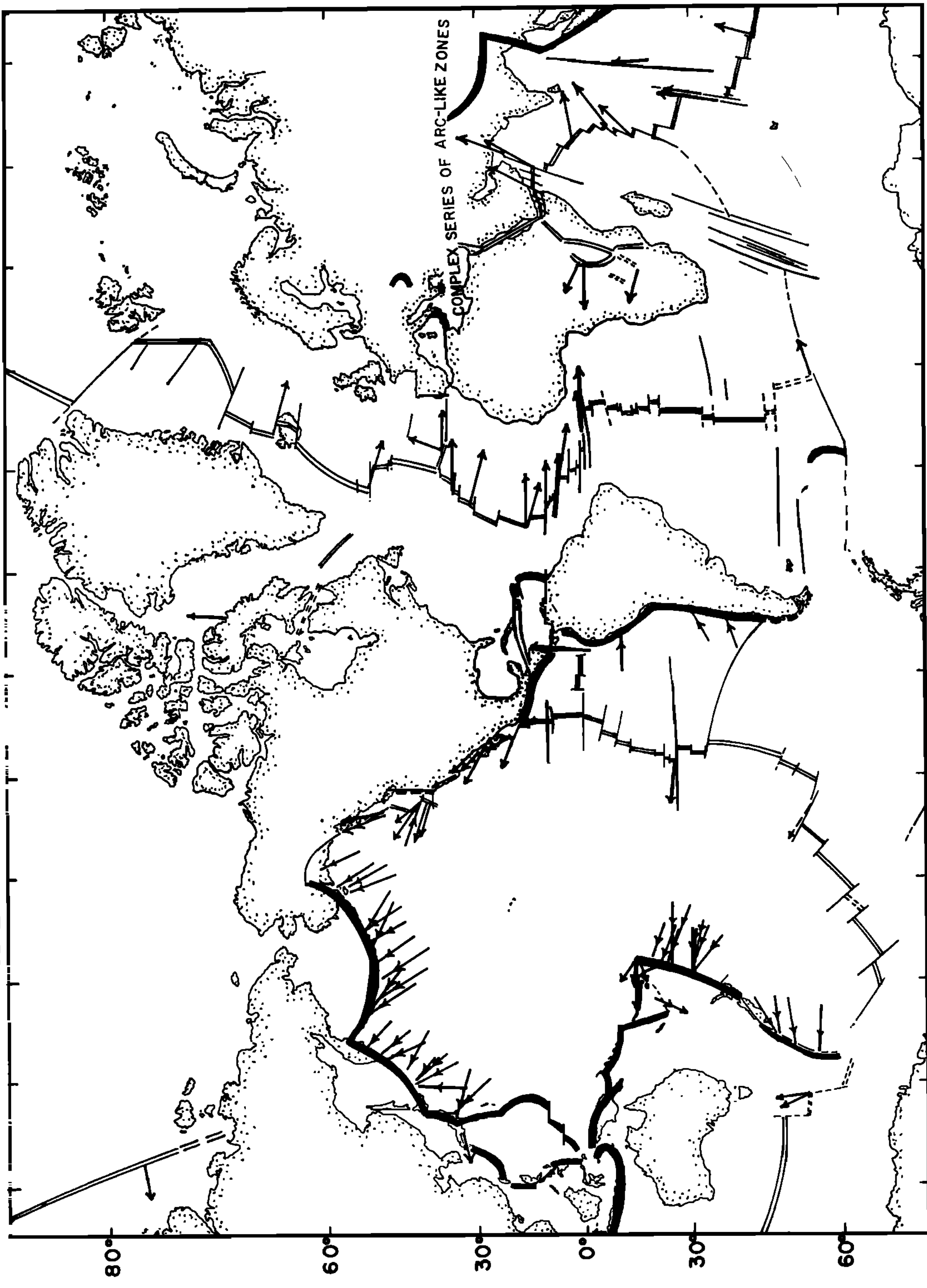
There are many important seismological facts that are so apparent that they are commonly accepted without much concern as to their origin; they fall into place remarkably well under the new global tectonics. For example, the general pattern of seismicity, which consists of a number of continuous narrow active belts dividing the earth's surface into a number of stable blocks, is in accord with this concept. In part this agreement is by design, for the blocks were chosen to some extent on this basis, but data from other fields were used as well. That the end result is internally consistent is significant. Zones predicted by the theory to be tensional, such as ocean rifts, are sites of only shallow earthquakes (the thin shallow lithosphere is being pulled apart; earthquakes cannot occur in the asthenosphere), and the general level of seismic activity and the size of the largest earthquakes are lower there than in the more active compressional features. In the compressional features (the arcs) large, deep earthquakes occur and activity is high as the lithosphere plunges into the deeper mantle eventually to be absorbed. Deep earthquakes can occur only where former crustal and uppermost mantle materials are now found in the mantle. Where one block of the lithosphere is moving past another along the surface at the zones of large strike-slip faulting, seismic activity is shallow, but occasional rather large shallow earthquakes are observed. Some zones combine thrusting and strike-slip motion. The general pattern of earthquake focal mechanisms is in remarkable agreement with the pattern predicted by the movements of the lithosphere determined in other ways and provides much additional information on this process. The depth of the deepest earthquakes (about 700 km) has been reasonably well known, but unexplained, for many years. The mobile lithosphere hypothesis offers, at this writing, several possible alternatives to explain this observation. Many similar points are raised in the remainder of this paper. Other hypotheses on global tectonics, for example, the expanding earth and the contracting earth hypotheses, have been far less satisfactory in explaining seismological phenomena.

Certainly the most important factor is that the new global tectonics seem capable of drawing together the observations of seismology and observations of a host of other fields, such as geomagnetism, marine geology, geochemistry, gravity, and various branches of land geology, under a single unifying concept. Such a step is of utmost importance to the earth sciences and will surely mark the beginning of a new era.

In the remainder of this paper, the relationship between the new global tectonics and the field of seismology is discussed for a variety of topics ranging from seismicity to tsunamis, from earth structure to earthquake prediction. In each case what the authors judge to be representative, reliable evidence from the field of seismology is presented. This judgment is based on the quality of raw data and their analysis, not on the relation of the results to the new global tectonics. Reasonable speculation is presented where it seems proper. The organization of the paper is based not on the classical divisions of seismology but on the principal effects predicted by the new global tectonics and relevant to seismology. As a result of the remarkable capacity of the new global tectonics for unification, an obvious division of material among the sections was not completely achieved, however.

The first two sections present seismological evidence that the worldwide rift system and island arcs are the sources and sinks, respectively, for surficial material. The third section on compatibility of movements on a worldwide scale is closely related to the first two sections.

Fig. 3. (*Opposite*) Summary map of slip vectors derived from earthquake mechanism studies. Arrows indicate horizontal component of direction of relative motion of block on which arrow is drawn to adjoining block. Crests of world rift system are denoted by double lines; island arcs, and arc-like features, by bold single lines; major transform faults, by thin single lines. Both slip vectors are shown for an earthquakes near the western end of the Azores-Gibraltar ridge since a rational choice between the two could not be made. Compare with directions computed by Le Pichon (Figure 2).


COMPLEX SERIES OF ARC-LIKE ZONES
90°
30°
30°
90°
150°
150°
80°
60°
30°
0°
30°
60°

Evidence from seismology on the structure of the mantle in terms of a lithosphere, an asthenosphere, and a mesosphere is so voluminous and well known that the section on this topic, the fourth section, presents primarily additional evidence of particular relevance to the new global tectonics. The fifth section, on the impact of the new global tectonics on seismology, is less documented by data than the previous sections partly because the impact of the new global tectonics is quite recent. The natural lag in pursuing this aspect is such that there has been to date relatively little emphasis in this particular field. This section is, then, somewhat speculative and, hopefully, provocative.

Few scientific papers are completely objective and impartial; this one is not. It clearly favors the new global tectonics with a strong preference for the mobile lithosphere version of this subject. In the final section, however, we report an earnest effort to uncover reliable information from the field of seismology that might provide a case against the new global tectonics. There appears to be no such evidence. This does not mean, however, that many of the data could not be explained equally well by other hypotheses (although probably not so well by any other single hypothesis) or that further development or modification of the new global tectonics will not be required to explain some of the observations of seismology. It merely means that, at present, in the field of seismology, there cannot readily be found a major obstacle to the new global tectonics.

Mid-Ocean Ridges—the Sources

Displacements along fracture zones. Recent studies of earthquakes have revealed several important facts about the nature of displacements on the ocean floor [*Sykes*, 1967, 1968]. The recognition of the worldwide extent of the mid-ocean ridge system (Figures 2 and 3) [*Ewing and Heezen*, 1956] led to a great interest in the significance of this major feature to global tectonics. Although the ridge system appears to be a continuous feature on a large scale, the crest of the ridge is actually discontinuous in a number of places (Figures 1, 2, and 3). These discontinuities correlate with the intersections of the ridge and the major fracture zones—long linear zones of rough topography that resemble major fault zones on the continents. The apparent displacements along these fracture zones have been explained in at least three different ways, including simple offset of the ridge by strike-slip faulting [*Vacquier*, 1962], in situ development of the ridge crests at separate locations accompanied by normal faulting along fracture zones [*Talwani et al.*, 1965*b*], and transform faulting [*Wilson*, 1965*a*].

Transform faults. Although the concept of simple offset tacitly assumes the conservation of surface area, the growth or the destruction of surface area is basic to the definition of the transform fault. In this hypothesis the active portion (BC in Figure 4) of a strike-slip fault along which large horizontal displacement has occurred ends abruptly at the crest of a growing ocean ridge. The horizontal displacement along the fault is transformed (or absorbed) by sea-floor growth on the ridge; the growing ridge is, in turn, terminated by the fault. Two separate segments of ridge crest can be joined (Figure 4) by a strike-slip fault of this type; these faults are called transform faults of the ridge-ridge type.

Wilson [1965*a*] recognized that the sense of

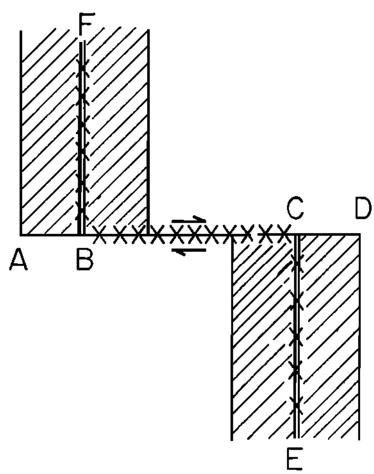


Fig. 4. An idealized model of sea-floor spreading and transform faulting of the ridge-ridge type. Hatching indicates new surface area created during a given period of sea-floor spreading along the active ridge crests BF and CE. Present seismicity (indicated by crosses) is confined to ridge crests and to segment BC of the fracture zone AD Arrows denote sense of shear motion along active segment BC.

shear displacement along transform faults of the ridge-ridge type would be exactly opposite that required for a simple offset of the two segments of ridge crest. He also pointed out that seismic activity along transform faults should be confined to the region between the two ridge crests (segment BC in Figure 4). If the crestal zones are being displaced by simple offset, however, seismic activity should be present along the entire length of the fracture zone.

Earthquake mechanisms. The first motions of seismic waves from earthquakes offer a means for ascertaining the sense and type of displacements on fracture zones. First-motion studies are often called 'fault-plane solutions' or 'focal-mechanism solutions.' Although many earth scientists have been disappointed by the large uncertainties involved in many first-motion studies, investigations of focal mechanisms were vastly upgraded by the installation of the World-Wide Standardized Seismograph Network [*Murphy*, 1966]. Reliable calibration, availability of data, high sensitivity, use of seismographs of both long and short periods, and greater geographical coverage are some of the more important characteristics of this network, which commenced operation in 1962. Various studies using data from these stations have confirmed that a double couple (or a shear dislocation) is an appropriate model for the radiation field of earthquakes [*Stauder*, 1967; *Isacks and Sykes*, 1968]. Hence, the first motions observed at seismograph stations around the world may be used to determine the orientation and the sense of the shear motion at the sources of earthquakes in various tectonic regions. Additional background information on earthquake mechanisms will be introduced in later sections as further clarification is required.

Mechanisms along world rift system. *Sykes* [1967] examined the focal mechanisms of seventeen earthquakes along various parts of the world rift system. In his study all the earthquakes located on fracture zones were characterized by a predominance of strike-slip motion. In each case the shear motion was in the correct sense for transform faulting (Figure 4), but it was consistently opposite in sense to that expected for simple offset. This is an instance in the earth sciences in which a yes-or-no answer could be supplied by data analysis. The sense of motion (left lateral) along one of the major fracture zones of the East Pacific rise (a branch of the mid-ocean ridge system) is illustrated in Figure 1.

Sykes also showed that earthquakes located on the ridge crests (segments BF and CE in Figure 4) but not located on fracture zones are characterized by a predominance of normal faulting. Normal faulting on ocean ridges had long been suspected because of the existence of a rift valley near the crest of large portions of the ridge system [*Ewing and Heezen*, 1956]. More than fifty mechanism solutions (Figure 3) have now been obtained for the world rift system [*Sykes*, 1968; *Tobin and Sykes*, 1968; *Banghar and Sykes*, 1968]; they continue to confirm the pattern of transform faulting and normal faulting described by Sykes. Nearly the same tectonic phenomenon is observed for each of the major oceans.

Seismicity. The distribution of earthquakes is another key piece of seismic evidence for the hypothesis of transform faulting. Nearly all the earthquakes on the mid-ocean ridges are confined either to the ridge crests or to the parts of fracture zones that lie between ridge crests [*Sykes*, 1967]. Seismic activity along a fracture zone ends abruptly (Figure 4) when the fracture zone encounters a ridge; only a few earthquakes have been detected from the outer parts (segments AB and CD) of most fracture zones. If the transform fault theory is correct, the areas of sea floor that are now bounded by the outer inactive parts of fracture zones were once located between two ridge crests; these blocks of sea floor moved beyond either crest as spreading progressed. Thus, the age of deformation becomes older as the distance from an active crest increases.

Earthquake swarms. The occurrence of earthquake swarms along the world rift system suggests that the crestal zone probably is characterized by submarine volcanic eruptions [*Sykes et al.*, 1968]. Earthquake swarms are a distinctive sequence of shocks highly grouped in space and time with no one outstanding principal event. Although these sequences sometimes occur in nonvolcanic regions, most of the world's earthquake swarms are concentrated in areas of present volcanism or geologically recent volcanism [*Richter*, 1958; *Minikami*, 1960]. Large swarms often occur before volcanic eruptions or accompanying them; smaller swarms may be indicative of magmatic activity that failed to reach the surface as an eruption.

From the seismograph records at Palisades, New York, *Sykes et al.* [1968] recognized more than twenty swarms of earthquakes occurring during the past 10 years. These swarms commonly lasted a few hours or a few days. Although many of the larger earthquakes along the world rift system occur on fracture zones and are characterized by strike-slip faulting, nearly all the swarms are restricted to the ridge crests (segments BF and CE in Figure 4) and seem to be characterized by normal faulting. Swarms are commonly (but not always) associated with volcanic eruptions on islands or on or near the crest of the world rift system.

From a simulation of magnetic anomalies *Matthews and Bath* [1967] *and Vine and Morgan* [1967] estimate that most of the new surface material along the world rift system is injected within a few kilometers of the axis of the ridge. In Iceland, where the rift may be seen and studied in detail, postglacial volcanism is confined largely to the median rift that crosses the island [*Bodvarsson and Walker*, 1964]. The rift apparently marks the landward continuation of the crest of the mid-Atlantic ridge (Figures 2 and 3).

The lack of weathering in rock samples, the young ages measured by radioactive and paleontologic dating of rocks and core materials, and the general absence of sediment as revealed by bottom photographs and by reflection profiling all attest to the youthful character of the crestal zones of the mid-ocean ridge [*Ewing et al.*, 1964; *Burckle et al.*, 1967; *van Andel and Bowin*, 1968; *Dymond and Deffeyes*, 1968]. Thus, the occurrence of earthquake swarms is compatible with the hypothesis that new surface materials are being emplaced magmatically near the axes of the ocean ridges. The large earthquake swarms (and perhaps some of the smaller swarms) may be indicative of eruptions or magmatic processes in progress near the ridge crests. Nonetheless, more work is needed to ascertain if a causal relationship exists between the two phenomena.

Synthesis of data for ridges. Seismological evidence of various types seems to provide a definitive argument for the hypotheses of transform faulting and sea-floor spreading on the mid-ocean ridge system. These data are in excellent agreement with evidence of spreading from magnetic anomalies, ages of rocks, and the distribution of sediments [*Vine*, 1966; *Heirtzler et al.*, 1968; *Wilson*, 1963; *Burckle et al.*, 1967]. The world rift system must be recognized as one of the major tectonic features of the world. It is characterized nearly everywhere by extensional tectonics, sea-floor growth at its crest, and transform faulting on its fracture zones.

The focal depths and the maximum magnitudes of earthquakes, the narrowness of seismic zones, and the propagation of S_n waves along ocean ridges and transform faults will be described in the sections on worldwide compatability of movements and on additional evidence for the existence of the lithosphere.

Implications for continental drift. The similarity of the earthquake mechanisms along nearly the entire length of the ridge system suggests that transform faulting and spreading have been occurring in these regions for extended, but as yet unspecified, periods of time. The distribution of magnetic anomalies, paleomagnetic investigations, and the shapes of continental blocks that supposedly were split apart by spreading furnish a more complete history of the processes of sea-floor spreading and transform faulting. A question of particular interest is: Have the various segments of ridge grown in place; i.e., has the en echelon pattern of ridges and fracture zones prevailed throughout an episode of sea-floor spreading?

Both the Gulf of Aden and the Gulf of California are thought to have opened by continental drift during the last 25 m.y. [*Hamilton*, 1961; *Laughton*, 1966]. If drift occurred in these areas, the displacements are at most a few hundred kilometers. If continental drift can be confirmed for these features, inferences about drift on an ocean-wide scale are placed on a much firmer basis.

Figure 5 shows the distribution of structural features, earthquake epicenters, and earthquake mechanisms for the Gulf of Aden [*Sykes*, 1968]. Nearly all the epicenters are confined either to northeast-striking fracture zones or to the ridge that extends from a branch of the mid-ocean ridge (the Carlsberg ridge) near 9°N, 57°E to the western part of the Gulf of Aden near 12°N, 43°E. This ridge coincides with the rough central zone in Figure 5. As in other parts of the world rift system the earthquakes occurring on

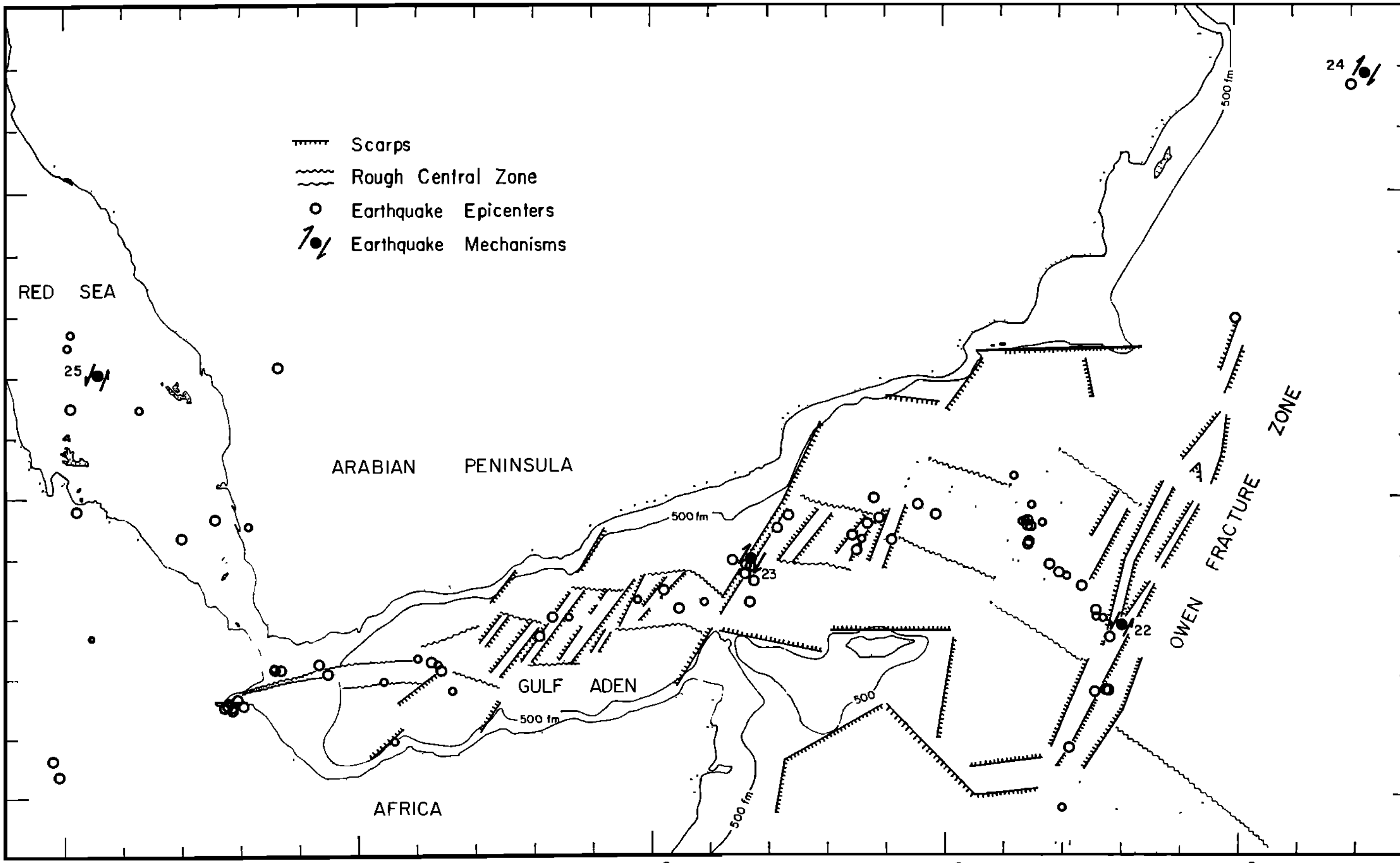


Fig. 5. Structural features of the Gulf of Aden [after *Sykes*, 1968]. Relocated epicenters of earthquakes for the period 1955 to 1966. Scarps and rough central zone after *Laughton* [1966]. Seismicity and focal mechanisms support hypothesis of spreading by ocean ridge-transform fault mechanism.

fracture zones are mostly restricted to the regions between ridge crests. Mechanism solutions for events 22 and 23 (numbers after *Sykes* [1968]) indicate transform faulting of the ridge-ridge type.

If the opening of the Gulf of Aden was accomplished through a simple process of sea-floor spreading and transform faulting, the fracture zones should join points in Arabia and in Africa that were together before the drifting commenced. Also, the fracture zones should not continue into the two continental plates. *Laughton* [1966] has shown, in fact, that these faults do not continue inland. In addition, his pre-Miocene reconstruction, in which the two sides of the Gulf of Aden are moved together parallel to the fracture zones, juxtaposes a large number of older structural features on the two sides of the gulf. The en echelon arrangement of segments of ridge is also mirrored in the stepped shape of the continental margins of Arabia and Africa. Hence, the present en echelon pattern seems to have prevailed since the initial breakup of these two blocks about 5 to 25 m.y. ago.

A similar pattern of en echelon ridges is present in the Gulf of California (Figure 6). Earthquake mechanisms from this region are indicative of a series of northwesterly striking transform faults with right-lateral displacement [*Sykes*, 1968]. These transform faults, which are arranged en echelon to the San Andreas fault, connect individual segments of growing ridges in the Gulf of California. Hence, sea-floor spreading and transform faulting also were responsible for the displacement of Baja California relative to the mainland of Mexico. If these two blocks are reconstructed by horizontal displacements parallel to the northwesterly striking fracture zones, the peninsula of Baja California is placed in the indentation or 'nitch' of the mainland of Mexico near 21°N, 106°W. Thus, the two pieces appear to fit together in this reconstruction. *Wilson* [1965*a*] has pointed out that the stepped shape of the fracture-zone-ridge pattern in the equatorial Atlantic is mirrored in the stepped shape of the coastlines and the continental margins of Africa and Brazil.

Island Arcs—The Sinks

Almost anyone who glances casually at a map of the world is intrigued by the organized patterns of the island arcs. The close association of the major ocean deeps with these arcs is obvious and suggests exceptional subsidence in these zones, but other facts are equally striking. Nearly all the world's earthquakes in the deep and intermediate range, most of the world's shallow earthquakes, and the largest departures from isostatic equilibrium are associated with island arcs or arc-like structures, as shown by *Gutenberg and Richter* [1954]. Volcanoes, sea-level changes, folding, faulting, and other forms of geologic evidence also demonstrate the high level of tectonic activity of these features. A concept of global tectonics in which the arcs do not play an important role is unthinkable. If crustal material is to descend into the mantle, the island arcs are suspect as sites of the sinks.

The asymmetrical structure of the arcs and the associated pattern of earthquake occurrence in the mantle led many investigators (e.g., *Vening Meinesz* [1954], *Benioff* [1954], *Hess* [1962], *Dietz* [1961]) to postulate that the structures are the result of compressive stresses normal to the arc and are the sites of vertical movements in various convective schemes. Although such ideas were supported by the investigations of focal mechanisms of earthquakes made by *Honda et al.* [1956] and by the gravity studies of *Vening Meinsz* [1930] and *Hess* [1938], later analyses by *Hodgson* [1957] for focal mechanisms and by *Talwani et al.* [1959] and *Worzel* [1965] for gravity led to different conclusions. This section reviews the data and shows that there is strong support for the compressive nature of island arcs and for their role as sites where surface material moves downward into the mantle. In particular, a variety of evidence supports the model of the arc shown in Figure 1. In this model the leading edge of the lithosphere underthrusts the arc and moves downward into the mantle as a coherent body. The proposed predominance of strike-slip faulting in island arcs [*Hodgson*, 1957] is not in agreement with this model but appears, in view of recent and vastly improved seismic data, to be based on unreliable determinations of focal mechanisms [*Hodgson and Stevens*, 1964]. The extensional features of structures based on gravity and seismic data appear to be surficial and can be reconciled with, and in fact are predicted by, the new hypothesis.

High-Q and high-velocity zones in the mantle

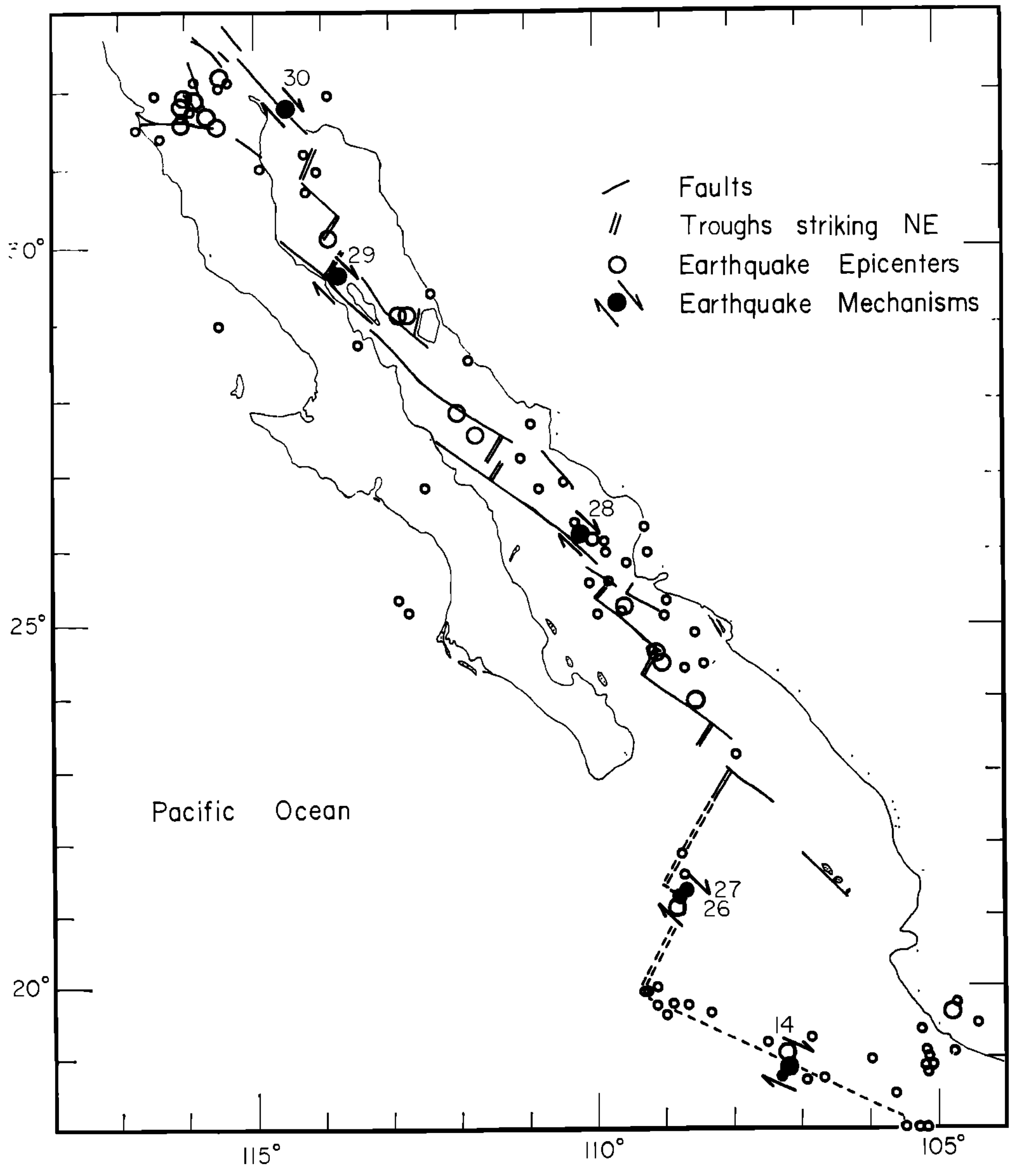


Fig. 6. Structural features of the Gulf of California [after *Sykes*, 1968]. Relocated epicenters of earthquakes for the period 1954 to 1962. Seismicity and focal mechanisms support the hypothesis of spreading by ocean-ridge–transform-fault mechanism.

beneath island arcs. The gross structure of an idealized island arc as shown in Figure 1 is based on the results of *Oliver and Isacks* [1967]. Their study was primarily concerned with the Fiji-Tonga area. Comparison of seismic waves generated by deep earthquakes in the seismic zone and propagated along two different kinds of paths, one along the seismic zone and one through an aseismic part of the mantle, demonstrated the existence of an anomalous zone in the upper mantle. The anomalous zone was estimated to be about 100 km thick and to be bounded on the upper surface by the seismic zone. Thus, the zone dips beneath the Tonga arc at about 45° and extends to depths of almost 700 km. The zone is anomalous in that attenuation of seismic waves is low and seismic velocities are high relative to those of the mantle at comparable depths elsewhere. Recent studies of the Japanese arc [*Wadati et al.*, 1967; *Utsu*, 1967] have confirmed the existence of such a structure for that region. Similar zones appear to be associated with other island arcs [*Oliver and Isacks*, 1967; *Cleary*, 1967; *Molnar and Oliver*, 1968].

The presence of a high-velocity slab beneath an island arc introduces a significant azimuthal variation in the travel times of seismic waves. Such variations with respect to source anomalies are shown by *Herrin and Taggart* [1966], *Sykes* [1966], *Cleary* [1967] and are indicated by the data of *Carder et al.* [1967] all for the case of the Longshot nuclear explosion. With respect to station anomalies such variations are shown by *Oliver and Isacks* [1967], *Utsu* [1967], *Cleary and Hales* [1966], and *Herrin* [1966] from data from earthquakes. These effects must therefore be taken into account as sources of systematic errors in the locations of earthquakes and the construction of travel-time curves. The large anomaly of Q associated with the slab must play a very important role in the Q structure of the mantle, especially for studies based on body waves from deep earthquakes. Studies in which this effect is ignored [e.g., *Teng*, 1968] must be reassessed on this basis.

Oliver and Isacks associated the anomalous zone with the layer of low attenuation near the surface to the east of Tonga. In their interpretation of the data, they correlated low attenuation with strength to arrive at the structure of Figure 1 in which the lithosphere, a layer of strength, descends into the mantle. This configuration suggests the mobility of the lithosphere implied in Figure 1 and described in the introduction. Based on current estimates of lithosphere velocities and other parameters, the down-going slab would be much cooler than its surroundings for a long time interval. Although there is little evidence supporting a direct relation between low attenuation and strength, an indirect relation based on the dependence of each parameter on temperature is reasonable. This point is discussed further in another section.

Bending of lithosphere beneath an island arc. The evidence supporting the model in which the lithosphere plunges beneath the island arc is varied. To explore this point further, consider first the configuration of the upper part of the lithosphere in the vicinity of an island arc (Figure 7*a*). Seismic refraction studies of a number of island arcs have been made. Invariably they show the surface of the mantle, which is shallow beneath the deep ocean, deepening beneath the trench, as suggested by Figure 7*a*. Although some authors suggest that the mantle merely deepens slightly beneath the islands of the arc and shoals again behind the arc, evidence for such a structure is incomplete. Mantle velocities beneath the islands, where determined, are low, and there is no case for which the data could not be interpreted as suggested in Figure 7*a* (see, e.g., *Badgley* [1965] and *Officer et al.* [1959]). In fact, the difficulty experienced in documenting the model in which the mantle is merely warped beneath the islands is evidence against this model. The main crustal layer as determined from seismic refraction studies seems to parallel the surface of the dipping mantle beneath the seaward slope of the trench. In some interpretations the crustal layer thins beneath the trench; in others it thickens or remains constant. Perhaps these are real variations from trench to trench, but the data are not always definitive.

Thinning of the crust has been interpreted by *Worzel* [1965] and others as an indication of extension, and there is considerable evidence in the structure of the sediments on the seaward slopes of many trenches supporting the hypothesis of extension (see, e.g., *Ludwig et al.* [1966]). Figure 8, one of Ludwig's sections across the Japan trench, demonstrates this point dramatically. Several graben-like structures are seen on the seaward slope of the trench. Although such evidence for extension has been cited as an argument against sea-floor spreading and convection on the basis that down-going currents at the sites of the ocean deeps would cause compression normal to the arcs, the argument loses its force when the role of the lithosphere is recognized. All the evidence for extension relates only to the sediments and crust, i.e.,

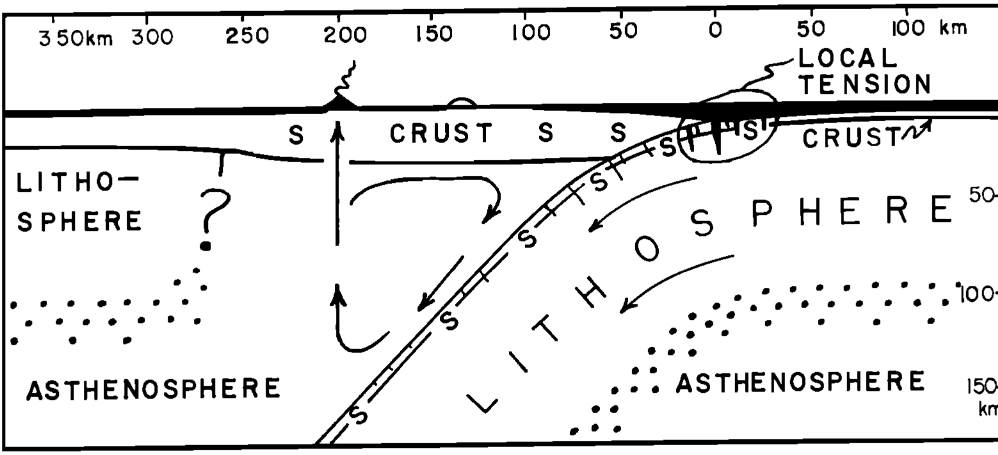


Fig. 7a.

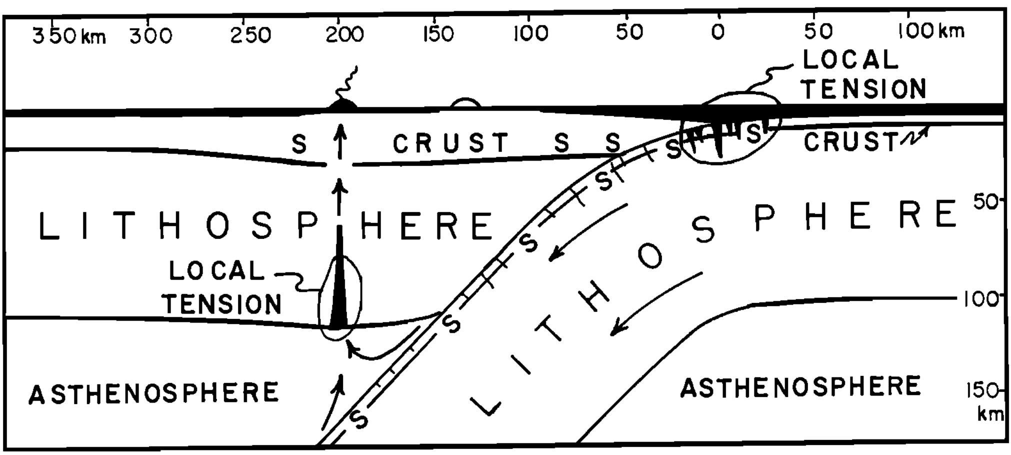


Fig. 7b.

Figure 7 shows vertical sections through an island arc indicating hypothetical structures and other features. Both sections show down-going slab of lithosphere, seismic zone near surface of slab and in adjacent crust, tensional features beneath ocean deep where slab bends abruptly and surface is free. (In both sections, *S* indicates seismic activity.) (*a*) A gap in mantle portion of lithosphere beneath island arc and circulation in mantle associated with crustal material of the slab and with adjoining mantle [*Holmes*, 1965]. (*b*) The overriding lithosphere in contact with the down-going slab and bent upward as a result of overthrusting. The relation of the bending to the volcanoes follows *Gunn* [1947]. No vertical exaggeration.

the upper few kilometers of the lithosphere. For the models pictured in Figure 7 in which a thick strong layer bends sharply as it passes beneath the trench, extensional stresses are predicted near the surface on the convex side of the bend even though the principal stress deeper in the lithosphere may be compressional. Earthquake activity beneath the seaward slope of the trench is, in general, infrequent and apparently of shallow depth. The focal mechanisms that have been determined for such shocks indeed indicate extension as predicted, i.e. normal to the trench, the axis of bending (*Stauder* [1968] and T. Fitch and P. Davis, personal communi-

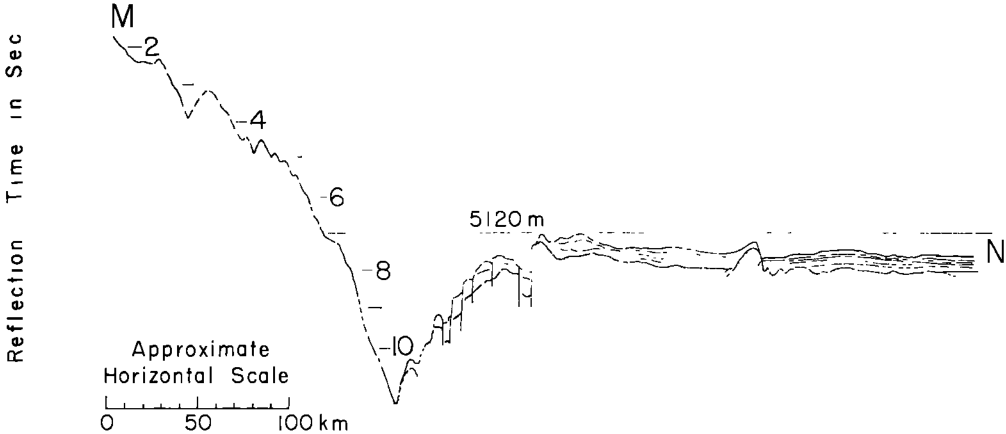


Fig. 8. Seismic reflection profile across the Japan trench extending easterly along 35°N from point M near Japan to point N [after *Ludwig et al.*, 1966]. Vertical scale represents two-way reflection time in seconds (i.e., 1 sec = 1 km of penetration for a velocity of 2 km/sec). Note block faulting along seaward slope of trench demonstrating extension in crust and inclusion of sediments in basement rocks. Also note shoaling of oceanic basement on approaching trench as suggested by work of *Gunn* [1937]. Vertical exaggeration ~25 : 1.

cations). Stauder demonstrates this point very well in a paper on focal mechanisms of shocks of the Aleutian arc.

The extensional features also suggest a mechanism for including and transporting some sediments within the down-going rock layers. As implied by Figure 7*a*, sediments in the graben-like features may be carried down to some depth in quantities that may be very significant petrologically, as suggested by *Coats* [1962]. Probably not all the sediments carried into the trench by motion of the sea floor or by normal processes of sedimentation are absorbed in the mantle, however. There are large volumes of low-density material beneath the inner slope on the island side of most trenches [*Talwani and Hayes*, 1967] that may correspond to sediment scraped from the crust and deformed in the thrusting. Unfortunately, the structure of these low-density bodies is not well explored, and, in fact, the very difficulty of exploring them may be an indication of their contorted nature, which results from great deformation.

The above arguments apply to trenches that are relatively free of flat-lying sediments, such as the Japan or Tonga trenches. The occurrence of substantial quantities of flat-lying undeformed sediments in some other trenches has been cited as evidence against underthrusting in island arcs [*Scholl et al.*, 1968]. Accumulation of underformed sediments depends on the ratio of rate of sediment accumulation to rate and continuity of underthrusting, and such data must be evaluated for each area with these factors in mind. The South Chile trench, for example, has a large sedimentation rate but no associated deep earthquakes, suggesting little or no recent thrusting. The results of Scholl et al. must be considered in this light. The Hikurangi trench (east of northern New Zealand), another example of a partially filled trench, is also thought to be in a zone of low convergence rate (see *Le Pichon* [1968] and Figure 2).

Underthrusting beneath island arcs. The shallow earthquakes mentioned above that indicate extension normal to the arc occur relatively infrequently and appear always to be located beneath or seaward of the trench axis. The earthquakes that account for most of the seismic activity at shallow depths in island arcs are located beneath the landward slope of the trench and form a slab-like zone that dips beneath the island arc [*Fedotov et al.*, 1963, 1964; *Sykes*, 1966; *Hamilton*, 1968; *Mitronovas et al.*, 1968]. This point is illustrated in Figure 9, which shows a vertical section through the Tonga arc. Note that, for a wide range of depths, foci are confined to a zone 20 km or less in thickness.

In the focal mechanisms of the shallow

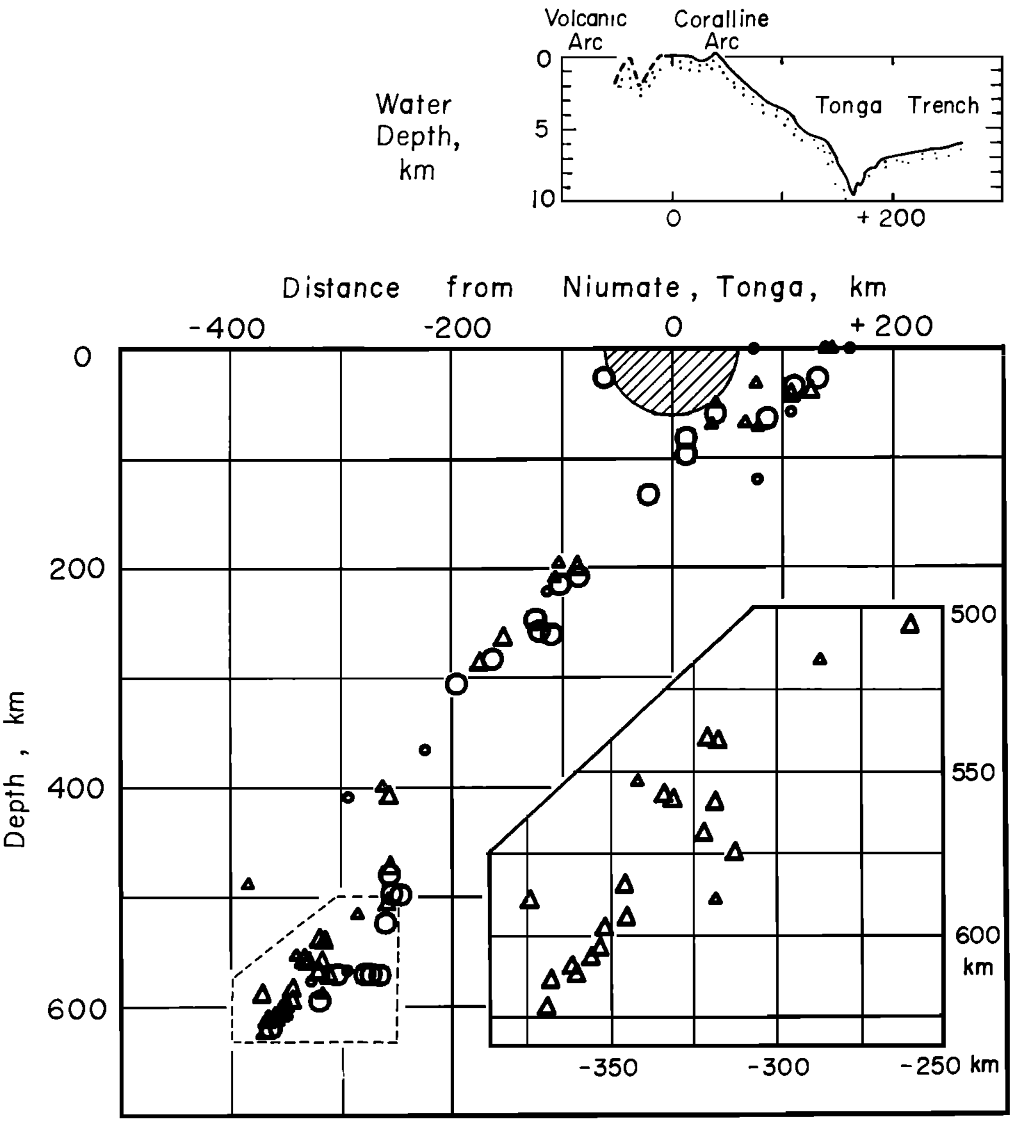


Fig. 9. Vertical section oriented perpendicular to the Tonga arc. Circles represent earthquakes projected from within 0 to 150 km north of the section; triangles correspond to events projected from within 0 to 150 km south of the section. All shocks occurred during 1965 while the Lamont network of stations in Tonga and Fiji was in operation. Locations are based on data from these stations and from more distant stations. No microearthquakes from a sample of 750 events originated from within the hatched region near the station at Niumate, Tonga (i.e., for *S-P* times less than 6.5 sec). A vertical exaggeration of about 13 : 1 was used for the insert showing the topography [after *Raitt et al.*, 1955]; the horizontal and vertical scales are equal in the cross section depicting earthquake locations. Lower insert shows enlargement of southern half of section for depths between 500 and 625 km. Note small thickness (less than ~20 km) of seismic zone for wide range of depths.

shocks along the slab-like zone of the Tonga-Kermadec arc, *Isacks and Sykes* [1968] find consistent evidence for underthrusting of the seaward block beneath the landward block. Abundant evidence for a similar process for various island arcs of the North Pacific is found by *Stauder* [1962, 1968], *Udias and Stauder* [1964], *Stauder and Bollinger* [1964, 1966*a*, *b*], *Aki* [1966], and *Ichikawa* [1966]. Critical evaluation of focal mechanism data by *Adams* [1963], *Hodgson and Stevens* [1964], *Stauder* [1964], and *Ritsema* [1964] shows that the generalization that strike-slip faulting is predominant in island arcs is based on unreliable data and possible systematic errors in the analyses. The recent data, greatly improved in quality and quantity, indicate that, in fact, dip-slip mechanisms are predominant in island arcs. The thrust fault mechanisms characteristic of shallow earthquakes in island arcs thus appear to reflect directly the relative movements of the converging plates of lithosphere and the downward motion of the oceanic plate. The compatibility of these motions as determined by focal mechanism data with the worldwide pattern of plate movements is discussed later and is shown to be excellent.

Considerable evidence for underthrusting in the main shallow seismic zone exists in other kinds of observations. Geodetic and geologic studies of the Alaskan earthquake of 1964 [*Parkin*, 1966; *Plafker*, 1965] strongly support the concept of underthrusting. Geologic evidence also indicates the repeated occurrence of such thrusting in this arc during recent time [*Plafker and Rubin*, 1967]. Data from other arcs on crustal movements are voluminous and have not all been examined in light of the hypotheses of the new global tectonics. In fact, in many arcs the principal zone of underthrusting would outcrop beneath the sea and important data would be largely obscured. One important point can be made. It is well known [*Richter*, 1958] that vertical movements in island arcs are of primary importance. This contrasts with the predominantly horizontal movement in such zones as California, where strike-slip faulting predominates.

Other shallow activity in island arcs. In some island arcs there is appreciable shallow seismic activity landward of the principal seismic zone. This activity, which is distinct from that of the deep seismic zone below it, appears to be confined mainly to the crust and to be secondary to the activity along the main seismic zone. The Niigata earthquake of June 16, 1964, appears to be located in such a secondary zone of the North Honshu arc. The mechanism of this earthquake [*Hirasawa*, 1965] indicates that the axis of maximum compressive stress is more nearly horizontal than vertical and trends perpendicular to the strike of the North Honshu arc. It is interesting that this stress is also perpendicular to the trend of Neogene folding in North Honshu [*Matsuda et al.*, 1967]. These results might indicate some compressive deformation of the overriding plates in the models of Figure 7.

Deep earthquakes: the down-going slab. The shallow seismic zone indicated by the major seismic activity is continuous with the deep zone, which normally dips beneath the island arc at about 45°. The thickness of the seismic zone is not well known in most cases, but it appears to be less than about 100 km and some evidence suggests that it may, at least in some areas, be less than 20 km. Figure 9 illustrates this point for a section through the Tonga arc. Although the surface approximating the distribution of hypocenters may be described roughly as above, it is clear that significant variations from this simple picture exist and are important. For example, the over-all dips may vary from at least 30° to 70°, and locally the variation may be greater, as suggested by the data in Figure 9. For the Tonga-Kermadec arc, the number of deep events is large and the zone can be defined in some detail [*Sykes*, 1966]. Sykes was able to show, as a result of a marked curvature of the northern part of the Tonga arc, a clear correlation between the configuration of the deep seismic zone and surface features of the arc, thereby demonstrating the intimate relationship between the deep and the shallow processes.

For most arcs, however, the number of deep events, particularly since the World-Wide Standardized Seismograph Network has been in operation, is relatively small; therefore, the deep zones cannot be defined as precisely as one might desire. Nevertheless, sufficient information is available on the pattern of seismic activity so that the concept of the mobile lithosphere can be tested in general, and it must be assumed that subsequent detailed studies of other island arcs may reveal contortions in the seismic zone comparable with the contortions already found in Tonga-Fiji.

Focal mechanisms. The simple underthrusting typical of the shallow earthquakes of the principal zones does not, in general, persist at great depths. For shocks deeper than about 100 km the orientation of the focal mechanisms varies considerably but exhibits certain clear-cut regularities. To understand these regularities, it is important to recall what is determined in a focal mechanism solution. The double-couple solution, which appears to be the best representation of most earthquakes, comprises two orthogonal nodal planes, either of which may be taken as the slip plane of the equivalent shear dislocation. Bisecting these nodal planes are the axis of compression, P, in the quadrants of dilatational first motions and the axis of tension, T, in the quadrants of compressional first motions. The axis formed by the intersection of the nodal planes is the null, or B, axis parallel to which no relative motion takes place. If one nodal plane is chosen as the slip plane, the pole of the other nodal plane is the direction of relative motion of the *slip vector*. It is important to realize that the primary information given by a double-couple solution is the orientation of the two possible slip planes and slip vectors. The interpretation of the double-couple mechanisms in terms of stress in the source region requires an assumption about the failure process. The P, T, and B axes correspond to the maximum, minimum, and intermediate axes of compressive stress in the medium *only* if the shear dislocation is assumed to form parallel to a plane of maximum shear stress in the medium, i.e., a plane that is parallel to the axis of intermediate stress and that forms a 45° angle to the axes of maximum and minimum stress.

Patterns of focal mechanisms for deep earthquakes. The most striking regularity in the orientation of the double-couple focal mechanisms of deep and intermediate earthquakes is the tendency of the P axes to parallel the local dip of the seismic zone. Figure 10 illustrates this point for the three zones (Tonga, Izu-Bonin, and North Honshu) for which reliable data are most numerous. This figure also shows that, although the orientation of the axes of tension and the null axes tend to be less stable than the compressional axes, these axes are not randomly oriented. The axis of tension tends to be perpendicular to the seismic zone; the null axis, parallel to the strike of the zone. These generalizations are shown schematically in Figure 11. The slip planes and slip directions are thus systematically *nonparallel* to the seismic zones; the orientations are therefore difficult to reconcile with a simple shearing parallel to the seismic zone as suggested by the common concept of the zone as a large thrust fault. *Sugimura and Uyeda* [1967] sought to reconcile the observations with that concept by postulating a reorientation of crystalline slip planes perpendicular to the axis of maximum compressive stress, such that in the case of horizontal compression the slip planes would tend to be vertical.

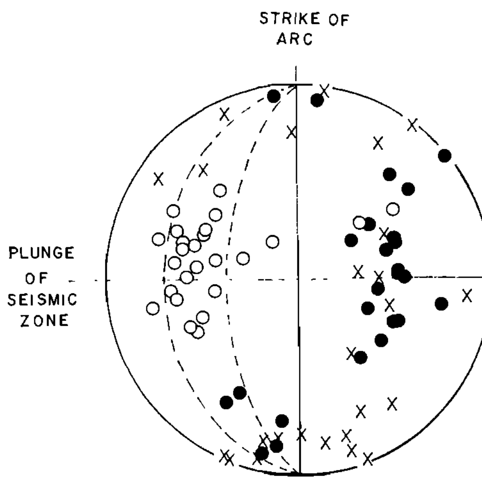


Fig. 10. Orientations of the axes of stress as given by the double-couple focal mechanism solutions of deep and intermediate earthquakes in the Tonga arc, the Izu-Bonin arc, and the North Honshu arc. Open circles are axes of compression, P; solid circles are axes of tension, T; and crosses are null axes, B, all plotted on the lower hemisphere of an equal-area projection. The data, selected from available literature as the most reliable solutions, are taken from *Isacks and Sykes* [1968], *Honda et al.* [1956], *Ritsema* [1965], and *Hirasawa* [1966]. The data for each of the three arcs are plotted relative to the strike of the arc (Tonga arc, N 20°E; Izu-Bonin, N 15°W; North Honshu arc, N 20°E). The dips of the zones vary between about 30° and 60°, as indicated by the dashed lines in the figure. Note the tendency of the P axes to parallel the dip of the seismic zone and the weaker tendency for the T axes to be perpendicular to the zone.

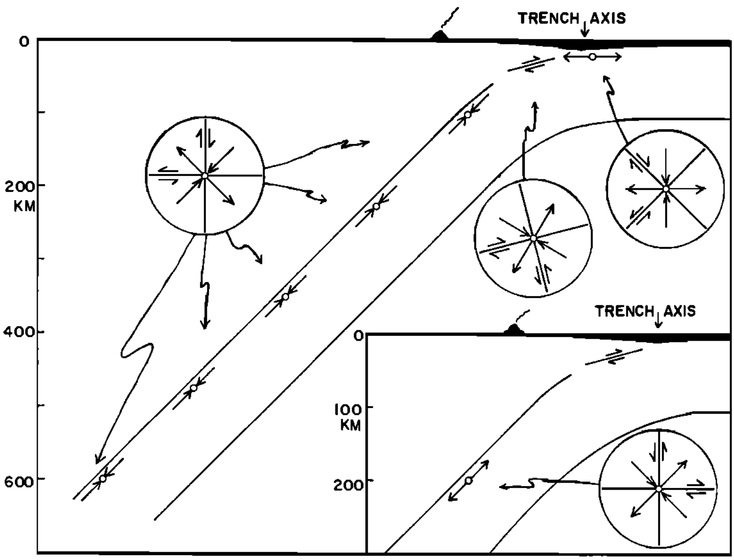


Fig. 11. Vertical sections perpendicular to the strike of an island arc showing schematically typical orientations of double-couple focal mechanisms. The horizontal scale is the same as the vertical scale. The axis of compression is represented by a converging pair of arrows; the axis of tension is represented by a diverging pair; the null axis is perpendicular to the section. In the circular blowups, the sense of motion is shown for both of the two possible slip planes. The features shown in the main part of the figure are based on results from the Tonga arc and the arcs of the North Pacific. The insert shows the orientation of a focal mechanism that could indicate extension instead of compression parallel to the dip of the zone.

Alternatively, *Isacks and Sykes* [1968] show that, if it is assumed that the slip planes form at angles with respect to the axis of maximum compressive stress that are not significantly different from 45°, then a very simple interpretation can be made on the basis of the model of Figure 1. In this interpretation the axis of maximum compressive stress is parallel to the dip of the seismic zone (i.e., parallel to the presumed motion of the slab in the mantle), and the axis of least compressive stress is perpendicular to the zone or parallel to the thin dimension of the slab.

The tendency for the compressive axes to be more stable than the other two axes can be interpreted to indicate that the difference between the intermediate and the least principal stresses is less than the difference between the greatest and the intermediate principal stresses. In general, the stress state may be quite variable owing to contortions of the slab, as suggested by Figure 9. Possibly large variability in the orientations of the deep mechanisms would, therefore, be expected, especially near parts of the zone with complex structure.

The important feature of the interpretation presented here is that the deep earthquake mechanisms reflect stresses in the relatively strong slab of lithosphere and do not directly accommodate the shearing motions parallel to the motion of the slab as is implied by the simple fault-zone model. The shearing deformations parallel to the motion of the slab are presumably accommodated by flow or creep in the adjoining ductile parts of the mantle.

Do the stresses in the slab vary with depth? In particular, the axis of least compressive stress, the T axis, may be parallel to the dip of the zone if the material at greater depths were sinking and *pulling* shallower parts of the slab [*Elsasser*, 1967]. In the Tonga, Aleutian, and Japanese arcs, the focal mechanisms indicate that the slab is under compression parallel to its dip at all depths greater than about 75 to 100 km. In these arcs, therefore, any extension in the slab must be shallower than 75 to 100 km.

Very limited evidence from the Kermadec [*Isacks and Sykes*, 1968], New Zealand (North Island) [*Adams*, 1963], South American (A. R. Ritsema, personal communication), and Sunda (T. Fitch, personal communication) arcs suggests, however, that mechanisms indicating extension of the slab, as shown in the insert of Figure 11, may exist at intermediate depths in some arcs. Further work is required to distinguish such mechanisms from the underthrusting type of mechanism characteristic of earthquakes at shallow depths or from complex mechanisms related to changes in structure or contortions of the slab.

Process of deep earthquakes. The idea that deep earthquakes occur in downgoing slabs of lithosphere has important implications for the problem of identifying the physical process responsible for sudden shear failure in the environment of the upper mantle. That deep earthquakes are essentially sudden shearing movements and not explosive or implosive changes in volume is now extensively documented (see *Isacks and Sykes* [1968] for references). Anomalous temperatures and composition might be expected to be associated with the down-going slab, either or both of which may account for the existence of earthquakes at great depths.

Several investigators [*Raleigh and Paterson*, 1965; *Raleigh*, 1967] concluded that dehydration of hydrous minerals can release enough water to permit shear fracture at temperatures between about 300° and 1000°C. Although *Griggs* [1966] and *Griggs and Baker* [1968], assuming normal thermal gradients, suggested that these reactions would not take place for depths greater than about 100 km, rates of underthrusting as high as 5 to 15 cm/yr suggest that temperatures low enough to permit these reactions to occur may exist even to depths of 700 km. Certainly a re-evaluation of these processes is in order.

The lowest temperatures, the largest temperature gradients, and the largest compositional anomalies would probably be most marked near the upper part of the slab, i.e. the part corresponding to the crust and uppermost mantle in the surficial lithosphere. Thus, the seismic activity associated with these anomalies might be expected to concentrate near the upper part of the slab, as is suggested in Figures 7*a* and 11 and supported by the data shown in Figure 9. Although catastrophic phase changes may be ruled out as direct sources of seismic waves on the basis of the radiation pattern of the waves, the possibility remains that the stresses responsible or partly responsible for shear failure may result from somewhat slower phase changes.

Seismic activity versus depth. Frequencies of earthquakes versus depth for several island arcs are shown in Figure 12. There are two main results emerging from these analyses. (1) In all island arcs studied the activity decreases in the upper 100 to 200 km approximately exponentially as a function of depth with a decay constant of about 100 km [*Sykes*, 1966]. (2) At greater depths the seismic activity in many (but not all) island arcs increases relative to the exponential decay extrapolated from shallower depths, and the seismic activity shows a fairly well-defined maximum in some depth range in the upper mantle. The variation of seismic activity with depth is thus grossly correlated with the variation of seismic focal mechanisms with depth and supports the generalization that deep earthquake mechanisms have a different relationship to the zone than shallow mechanisms do. In this correlation the earthquakes that define the shallow exponential decay in seismic activity are characterized by the underthrusting type mechanisms, whereas the deeper earthquakes appear to be related to the stresses in the down-going slab.

There is an approximate correlation of the decrease in seismic activity versus depth with a similar general decrease in seismic velocities, Q, and viscosity in the upper 150 km. These effects may be related to a decrease in the difference between the temperature and local melting temperature. Thus, the decrease in activity with depth may correspond to an increase in the ratio of the amount of deformation by ductile flow to that by sudden shear failure. An implication of this interpretation is that the exponential decay constant of 100 km may roughly indicate the thickness of the overthrust plate of lithosphere. This interpretation is illustrated in Figure 7*b*, in which the overriding plate of lithosphere is in 'contact' with the down-going plate along the seismic zone. As shown in a later section, the assumption that the depth distribution of shallow earthquakes

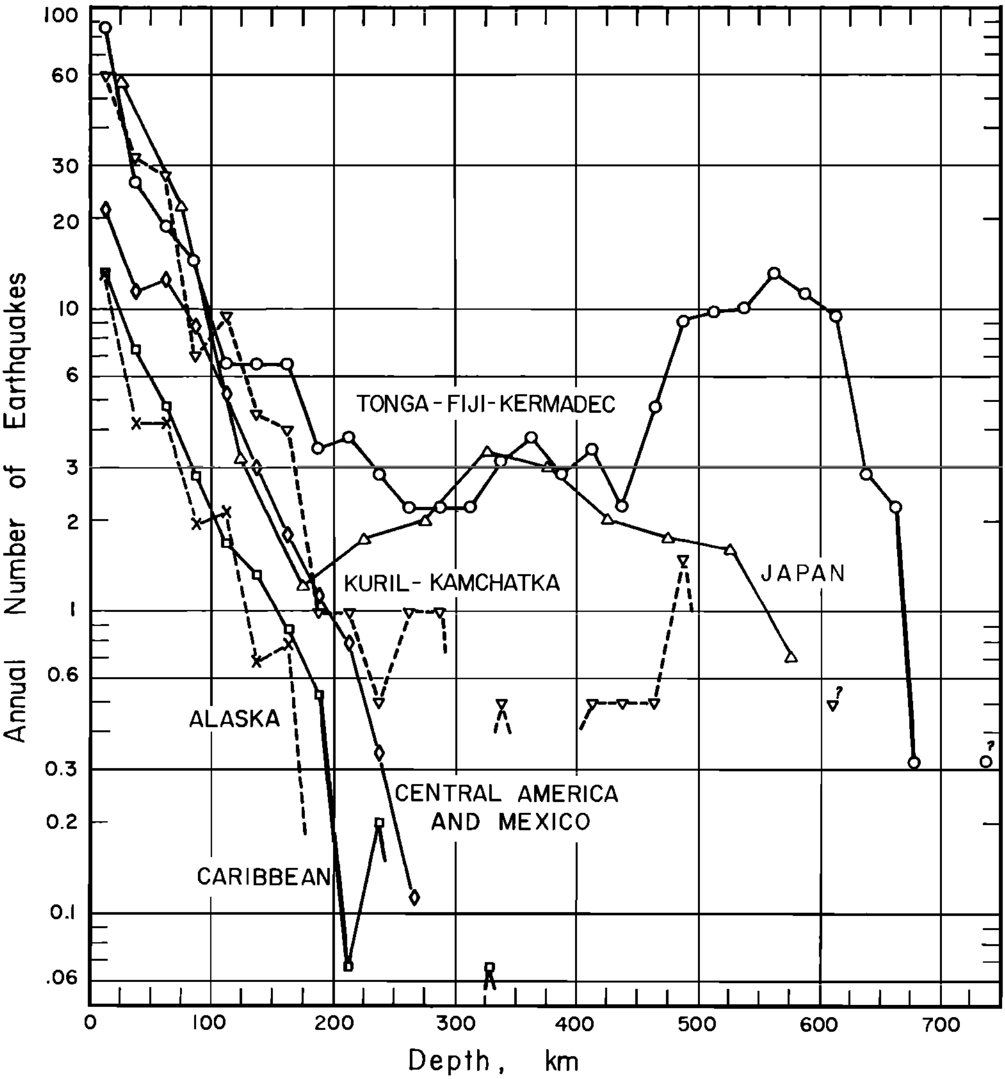


Fig. 12. Number of earthquakes per 25-km depth intervals as function of depth for several island arcs. Except for Japan, data are from *Sykes* [1966]. Data for Japan expressed as percentage of events per 50-km depth intervals [*Katsumata,* 1967]. Since the various curves were not normalized for the sample lengths and for the lower limit of detectability in each area, only the relative shapes and not the absolute levels of the various curves should be compared with one another. The number of earthquakes per unit depth within the upper 200 km of all these island arcs is approximately proportional to exp $(-Z/100)$, where Z is the depth in kilometers. Peaks in activity below 200 km appear to fluctuate both in amplitude and in depth among the various arcs.

of the mid-ocean rift system yields a measure of the thickness of the lithosphere is not an unreasonable one.

Several lines of evidence do not, however, support the existence of thick lithosphere directly beneath and behind the arc as shown in Figure 7*b*. *Oliver and Isacks* [1967] and *Molnar and Oliver* [1968] show that high-frequency S_n does not propagate across the concave side of island arcs, which probably indicates that the uppermost mantle there has low Q values. This result is in agreement with the low P_n velocities generally found beneath islands of many arcs. Also, the active volcanism and high heat flow characetristic of the concave side of island arcs [*Uyeda and Horai*, 1964; *Sclater et al.*, 1968] suggest that the lithosphere may be thin there. These data are qualitatively fitted by the model shown in Figure 7*a*. One implication of this figure is that at least part of the shallow earthquake zone might not result from the contact between two pieces of lithosphere but might instead indicate an embrittled and weakened zone formed by the downward moving crustal materials [*Raleigh and Paterson*, 1965; *Griggs*, 1967]. In this case the exponential decay in activity might reflect changes in the properties of the earthquake zone as a function of depth. Thus, both models in Figure 7 must be retained for the present.

Although in some arcs such as the Aleutians or Middle America the exponential decay in activity appears to be the only feature present in curves of activity versus depth, most arcs exhibit a more or less well-defined maximum in activity in the mantle, as illustrated in Figure 12. The approximate ranges of depth of these maxima are shown in Figure 13 for several island arcs. The main point of this figure is to show that the depths of these maxima vary considerably among the various arcs and do not appear to be associated with any particular level of depth in the mantle, contrary to general opinion. As shown in Figure 13, the depths of the deep maxima are approximately correlated with the rates of convergence in the arcs as calculated by Le Pichon. As will be shown later (see Figure 16), the correlation is considerably better between the rate of convergence and the *length* of the zone measured along the dip of the zone. Thus, the simplest explanation, one direct consequence of the model of Figure 1, is that the deep maxima are near the leading parts of the down-going slabs.

Two features of the distributions shown in Figures 12 and 13 may be related to certain levels of depth in the mantle. Although the length of the seismic zone measured along the dip of the zone exceeds 1000 km for several cases, no earthquakes with depths greater than 720 km have ever been documented. The U. S. Coast and Geodetic Survey (USCGS) has located no earthquakes with a depth greater than 690 km during the period 1961–1967. These depths are near the region of the mantle in which gradients in the variation of seismic velocities may be high [*Johnson*, 1967]. *Anderson* [1967*a*] argues that this region corresponds to a phase change in the material. These depths may therefore be in some way related to the boundary of the mesosphere as shown in Figures 1 and 14 and as discussed in the next section. The second feature is the absence of maxima around 300 km. Thus, in a worldwide composite plot of activity versus depth, a minimum in activity near this depth generally appears.

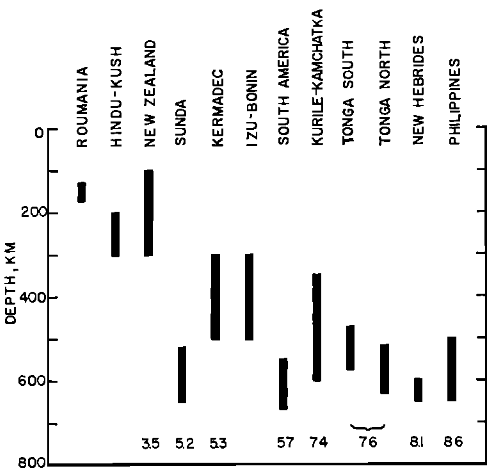


Fig. 13. Depth range of maxima in the seismic activity (numbers of earthquakes) as a function of depth in island arcs and arc-like structures for which data are sufficiently numerous. The data are from *Gutenberg and Richter* [1954], *Katsumata* [1967], *Sykes* [1966], and listings of earthquakes located by the USCGS in the preliminary determination of epicenters (PDE). The numbers at the bottom of the figure give the rate (in centimeters per year) of convergence for the arc as plotted in Figure 2. Note that the maxima occur over a wide range of depths and that the depths appear to correlate, in general, with the calculated slip rate.

Downward movement of lithosphere in the mantle: some hypotheses. Although the concept is so new that it is difficult to make definitive statements, a brief speculative discussion is in order to emphasize the importance of these results in global tectonics. Figure 14, four hypothetical and very schematic cross sec-

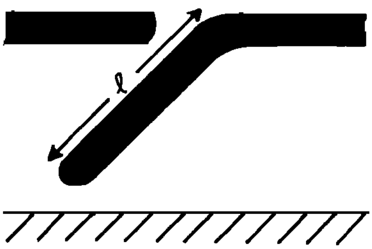


Fig. 14*a*. Length *l* is a measure of the amount of underthrusting during the most recent period of sea-floor spreading.

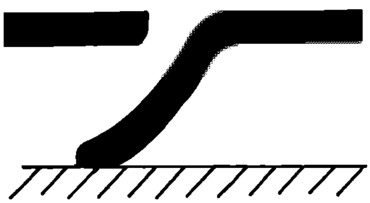


Fig. 14*b*. Lithosphere is deformed along its lower edge as it encounters a more resistant layer (the mesosphere).

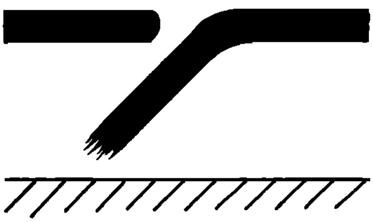


Fig. 14*c*. Length of seismic zone is the product of rate of underthrusting and time constant for assimilation of slab by upper mantle.

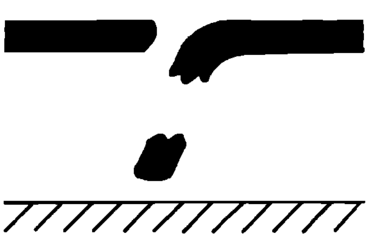


Fig. 14*d*. A piece (or pieces) of the lithosphere becomes detached either by gravitational sinking or by forces in the asthenosphere.

Figure 14 shows four possible configurations of an underthrust plate of lithosphere in island arcs. Solid areas indicates lithosphere; white area, asthenosphere; hatched area, mesosphere.

tions of an island arc, illustrates some points that should be considered.

Figure 14*a* shows a case in which the lithosphere has descended into the mantle beneath an island arc. In this model the lithosphere has not been appreciably modified with regard to its potential for earthquakes and the length of the submerged portion, *l*, and hence the depths of the deepest earthquakes are dependent on the rate of movement down dip and the duration of the current cycle of sea-floor spreading and underthrusting.

In Figure 14*b* the leading edge of the descending lithosphere has encountered significant resistance to further descent and has become distorted. In this situation, the depth of the deepest earthquakes in such a zone depends on the depth to the mesosphere. *Sykes'* [1966] analysis of the relocations of earthquakes in the Tonga arc (see also Figure 9) reveals the presence of contortions of the lower part of the seismic zone which might indicate a phenomenon similar to that pictured in Figure 14*b*. This model has the interesting consequence that a cycle of sea-floor spreading might be terminated or sharply modified by the bottoming of the lithosphere at certain points.

Figure 14*c* indicates schematically that the depths of the deepest earthquakes might depend on modification of the lithosphere by its environment. In this model the depth of the deepest shocks depends on the rate of descent and the rate of modifications or absorption of the lithosphere.

In Figure 14*d* the lower portion of the descending part of the lithosphere is not connected with the upper portion, possibly because it has pulled away as a result of a large density contrast between the sinking part of lithosphere and the surrounding mantle. Another possibility is that the lower piece represents a previous episode of movement, so that the break between the pieces then represents a period of quiescence in the surface movements. For example, the Spanish deep earthquake of 1954 [*Hodgson and Cock*, 1956] and the very deep earthquakes beneath the North Island of New Zealand [*Adams*, 1963] might indicate isolated pieces of lithosphere. A variant of Figure 14*d* is the case in which movements of the ductile material of the asthenosphere, movements that could be quite different from the movements of the surficial lithospheric plates, could deform the slabs and possibly break off pieces. For example, the marked contortions of the deep seismic zone of Tonga may be explained by such deformation. Thus, although the evidence is at present only suggestive, such evidence is important because of the implications of the hypotheses with regard to the dynamics of the system *within the asthenosphere*. Various combinations of the effects illustrated in Figure 14 may also be considered.

Lateral terminations of island arcs. The discussions above are based on, and apply largely to, the structure of an island arc taken in a vertical section normal to the strike of an arc. The three-dimensional configuration of the arc must also be considered. The plate model of tectonics provides, in a simple way, for the termination of an island arc by the abrupt or gradual transition to a transform fault, by a decrease in the rate of convergence to zero, or by some combination of these. In the first case the relative movement that is predominantly normal to the zone of deformation changes to relative movement that is predominantly parallel to the zone. In the second case the pole governing the relative motion between the plates may be located along the strike of the feature. *Isacks and Sykes* [1968] describe what may be a particularly simple case of the first possibility. The northern end of the Tonga arc appears to end in a transform fault that strikes approximately normal to the arc. In this case evidence is also found for a scissors type of faulting in which the downgoing Pacific plate tears away from the part of the plate remaining at the surface.

Summary of data on island arcs. The lithosphere model of an island arc thus gives a remarkably simple account of diverse and important observed features of island arcs. The existence and distribution of earthquakes in the mantle beneath island arcs, the anomalous transmission properties of deep seismic zones, and the correspondences in the variations of seismic activity and the orientations of the focal mechanisms as functions of depth are all in agreement with the concept of a cooler, relatively strong slab moving through a relatively ductile asthenosphere. The bend in the slab required by this movement provides a simple means of reconciling the conflicting evidence for extension and compressional features of island arcs. The results suggest that there are two basic types of focal mechanisms. The first type is apparently confined to shallow depths and directly accommodates, and therefore indicates the direction of, the movements between the plates of lithosphere. The second type indicates stress and deformation *within* a plate of lithosphere and includes, besides the deep and intermediate earthquake mechanisms, the normal-faulting mechanisms at shallow depths beneath the axis of the trench and, possibly, the shallow earthquake mechanisms located landward of the underthrust zone. The deep earthquake zones may provide the most direct source of information on the movement of material in the asthenosphere and on the basic question of the relative importance of the lithospheric and asthenospheric motions in driving the convective system. The global pattern of motions between the plates, derived in part from the shallow focal mechanism in island arcs, provides a severe test of the hypothesis of plate movements in general and provides in particular key evidence for the conclusion that island arcs are the major zones of convergence and downward movements of the lithospheric plates. This evidence, including observations of directions as well as rates of movement, is discussed in the next section.

Compatibility of Movements on a Worldwide Scale

In this section deformations along the world rift system and along island arcs and major mountain belts are examined for their internal consistency and for their global compatibility. The major finding is that these displacements can be approximated rather precisely by the interactions and the relative movements of large plates of lithosphere, much of the deformation being concentrated along the edges of the plates and relatively little deformation being within the individual plates themselves. It has long been recognized that recent deformations of the earth's surface are concentrated in narrow belts. These belts, which largely coincide with the major seismic zones of the world, include the world rift system, island arcs, and such island-arc-like features as active mountain belts and active continental margins. These major tectonic features do not end abruptly;

they appear to be linked together into a global tectonic scheme.

Continuity of seismic belts and distribution of seismic activity. Figure 15, a compilation of about 29,000 earthquake epicenters for the world as reported by the U. S. Coast and Geodetic Survey for the period 1961 to 1967 [*Barazangi and Dorman*, 1968], shows that most of the world's seismic activity is concentrated in rather narrow belts and that these belts may be regarded as continuous. Thus, if global tectonics can be modeled by the interaction of a few large plates of lithosphere, this model can account for most of the world's seismic activity as effects at or near the edges of the plates. Figure 15 also shows that the earthquakes occur much more frequently, in general, in the zones of convergence, the arcs and arc-like features, than in the zones of divergence, the ocean ridges. Along the ocean ridges, where the less complicated processes of tectonics are apparently occurring, the zones are narrow; on the continents, where the processes are apparently more complex, the zones are broad, and distinctive features are not easily resolved. Deep earthquake zones, indicated in Figure 15 only by the width of epicentral regions behind arcs, correspond to the zones of underthrusting. Thus, all the major features of the map of seismic epicenters are in general accord with the new global tectonics. No other hypothesis has ever begun to account so well for the distribution of seismic activity, which must rank as one of the primary observations of seismology. The details of the configuration of the seismic belts of Figure 15 are discussed further in other sections of this paper.

Slip vectors. Figure 3 illustrates the distribution of these major tectonic features and summarizes azimuths of motion as indicated by the slip vectors determined from various studies of the focal mechanisms of shallow-focus earthquakes. Deep and intermediate earthquakes as well as shallow earthquakes with normal faulting mechanisms near trenches were not represented in this figure, since these mechanisms are not thought to involve the relative displacements of two large blocks of lithosphere. Earthquake mechanisms were included in Figure 3 only when, by careful examination of the first-motion plots, we could verify that the slip vectors were reasonably well determined. These data were taken from *Stauder* [1962, 1968], *Stauder and Udias* [1963], *Stauder and Bollinger* [1964, 1966*a*, *b*], *Harding and Rinehart* [1966], *Ichikawa* [1966], *Sykes* [1967, 1968], *Banghar and Sykes* [1968], *Isacks and Sykes* [1968], and *Tobin and Sykes* [1968]. Although no attempt was made to ensure that the collection of mechanism solutions represented all the reliable previous work, nonetheless, the data are thought to be representative; no attempt was made to select the data by criteria other than their reliability. In some cases such as the aftershocks of the great Alaska earthquake of 1964 and the aftershocks of the large Rat Islands earthquake of 1965 [*Stauder and Bollinger*, 1966*b*; *Stauder*, 1968], only representative solutions were included, since the number of solutions for these regions was too large to depict clearly in Figure 3. Solutions cited as reliable by *Ritsema* [1964] as well as those of *Honda et al.* [1956], for example, were not used because they pertain to subcrustal shocks.

From each mechanism solution used one of two possible slip vectors was chosen as indicative of the relative motions of the two interacting blocks of lithosphere. For each slip vector the arrow depicts the relative motion of the block on which it is drawn with respect to the block on the other side of the tectonic feature. Since the double-couple model (or shear dislocation) appears to be an excellent approximation to the radiation field of earthquakes, it is not possible to choose from seismic data alone one of the two possible slip vectors as the actual motion vector (or alternatively to choose one of two possible nodal planes as the fault plane).

Nevertheless, the choice of one vector is not arbitrary but is justified either by the orientation of the vectors with respect to known tectonic features such as fracture zones or by the consistency of a set of vectors in a given region. For earthquakes located on such major transform faults as the oceanic fracture zones, the San Andreas fault, and the Queen Charlotte Islands fault (Figure 3), one of the slip vectors is very nearly parallel to the transform fault on which the earthquake was located. Observed surface breakage and geodetic measurements in some earthquakes, the alignment of epicenters along the strike of major transform faults, the linearity of fracture zones, and petrological evidence for intense shearing stresses in the

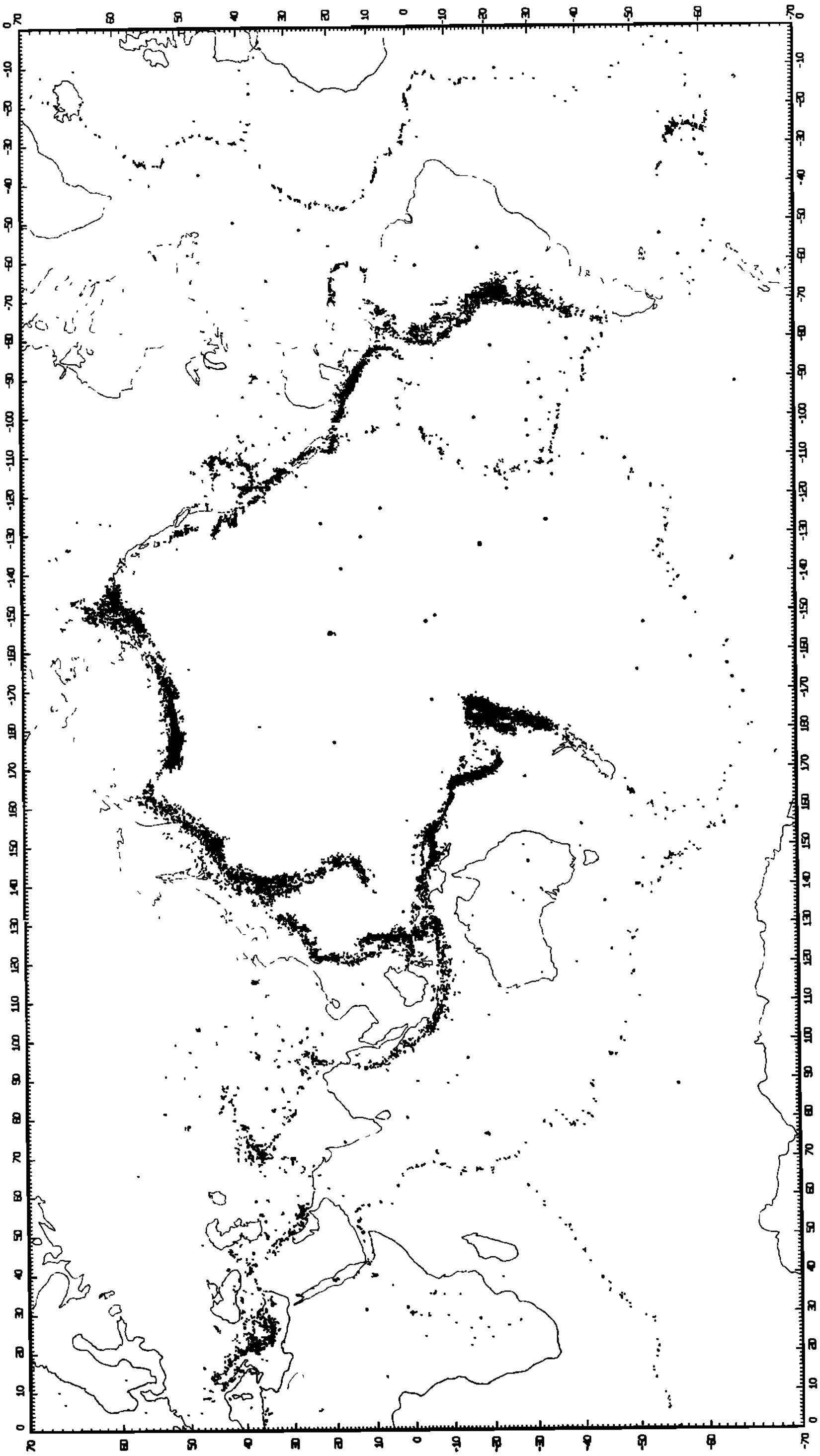


Fig. 15. Worldwide distribution of all earthquake epicenters for the period 1961 through 1967 as reported by U. S. Coast and Geodetic Survey [after *Barazangi and Dorman*, 1968]. Note continuous narrow major seismic belts that outline aseismic blocks; very narrow, sometimes steplike pattern of belts of only moderate activity along zones of spreading; broader very active belts along zones of convergence; diffuse pattern of moderate activity in certain continental zones.

vicinity of fracture zones constitute strong evidence for making a rational choice between the two possible slip vectors [*Sykes*, 1967]. The choice of the other possible slip vector (or nodal plane) for many of the oceanic fracture zones would indicate strike-slip motion nearly parallel to the ridge axis. On the contrary, earthquakes along the ridge crest but not on fracture zones do not contain a large strike-slip component but are characterized by a predominance of normal faulting.

Evidence for motions of lithospheric plates. One of the most obvious features in Figure 3 is that the slip vectors are consistent with the hypothesis that surface area is being created along the world rift system and is being destroyed in island arcs. Along the mid-Atlantic ridge, for example, slip vectors for more than ten events are nearly parallel to one another and are parallel to their neighboring fracture zones within the limits of uncertainty in either the mechanism solutions (about 20°) or the strikes of the fracture zones.

Morgan [1968] and *Le Pichon* [1968] showed that the distribution of fracture zones and the observed directions and rates of spreading on ocean ridges as determined from geomagnetic data could be explained by the relative motions of a few large plates of lithosphere. They determined the poles of rotation that describe the relative motion of adjacent plates on the globe. Our evidence from earthquake mechanisms and from the worldwide distribution of seismic activity is in remarkable agreement with their hypothesis. Although their data are mostly from ridges and transform faults, earthquake mechanisms give the relative motions along island arcs as well as along ridges and transform faults.

Le Pichon used data from ocean ridges to infer the direction of motion in island arcs. His predicted movements (Figure 2), which are based on the assumption of conservation of surface area and no deformation within the plates of lithosphere, compare very closely with mechanism solutions in a number of arcs. This agreement is a strong argument for the hypothesis that the amount of surface area that is destroyed in island arcs is approximately equal to the amount of new area that is created along the world rift system. Thus, although modest expansion or contraction of the earth is not ruled out in the new global tectonics, rapid expansion of the earth is not required to explain the large amounts of new materials added at the crests of the world rift system. This approximate equality of surface area is, however, probably maintained for periods longer than thousands to millions of years, but minor imbalances very likely could be maintained for shorter periods as strains within the plates of lithosphere. More exact knowledge of these imbalances could be of direct interest to the problem of earthquake prediction.

Figure 3 suggests that nearly all the east-west spreading along the East Pacific rise and the mid-Atlantic ridge is taken up either by the island arcs of the western Pacific or by the arc-like features bordering the west coasts of Central and South America. Much of the north-south spreading in the Indian Ocean is absorbed in the Alpide zone, which stretches from the Azores-Gibraltar ridge across the Mediterranean to southern Asia and then to Indonesia.

Relative motions in the southwest Pacific. Le Pichon's computed directions of motion in the Tonga and Kermadec arcs of the southwest Pacific agree very closely with mechanisms we obtained from a special study of that region [*Isacks and Sykes*, 1968]. Mechanisms south of New Zealand along the Macquarie ridge [*Sykes*, 1967; *Banghar and Sykes*, 1968] indicate a combination of thrust faulting and right-lateral strike-slip motion. These data suggest that the pole of rotation for these two large blocks is located about 10° farther south than estimated by *Le Pichon* [1968]. Although the Pacific plate is being *underthrust* in the Tonga and Kermadec arcs and in northern New Zealand, this plate is apparently being *overthrust* along the Macquarie ridge (see also *Summerhayes* [1967]). In this interpretation the Alpine fault is a right-lateral transform fault of the arc-arc type that connects two zones of thrusting with opposing dips. Computed slip vectors for this region also indicate a component of thrust faulting either along the Alpine fault itself or in other parts of the South Island of New Zealand. *Wellman*'s [1955] studies of Quaternary deformation, which indicate a thrusting component as well as a right-lateral strike-slip component of motion along the Alpine and associated faults, seem to be in general accord with this concept.

Likewise, the Philippine fault appears to connect a zone of underthrusting of the Pacific floor near the Philippine trench with a region of overthrusting west of the island of Luzon near the Manila trench. Also, the existence of a deep seismic zone in the New Hebrides arc that dips toward the Pacific rather than away from the Pacific as in the Tonga arc [*Sykes*, 1966] is understandable if the Pacific plate is being overthrust in the New Hebrides and underthrust in Tonga (Figure 1). The ends of these two arcs appear to be joined togther by one or more transform faults that pass close to Fiji, but additional complications appear to exist in this area.

North Pacific. The uniformity in the slip directions and the distribution of major faults along the margins of the North Pacific (with perhaps some systematic departure in the slip directions off the coast of Washington and Oregon) indicate that only two blocks are involved in the major tectonics [*Tobin and Sykes*, 1968; *Morgan*, 1968; *McKenzie and Parker*, 1967]. In this scheme, the San Andreas fault, the Queen Charlotte Islands fault, and a series of northwesterly striking faults in the Gulf of California are interpreted as major transform faults [*Wilson*, 1965*a*, *b*]. The observed rates of displacement along the San Andreas as determined geodetically [*Whitten*, 1955, 1956] are very similar to the rates determined from the seismicity by means of a dislocation model [*Brune*, 1968]. These rates are in close agreement with the rates inferred from magnetic anomalies for the region of growing ridges at the northwestern end of the San Andreas fault [*Vine and Wilson*, 1965; *Vine*, 1966]. Estimates of the total amount of offset along the San Andreas [*Hamilton and Meyers*, 1966] are comparable to the amount of offset needed to close the Gulf of California and to the width of the zones of northeasterly striking magnetic anomalies off the coast of Oregon and Washington [*Vine and Wilson*, 1965].

Thus, the present tectonics of much of the North Pacific can be related to the motion of the Pacific plate relative to North America and northeastern Asia. The slip vectors strike northwesterly along the west coast of the United States and Canada, represent nearly pure dip-slip motion in southern Alaska and the eastern Aleutians, and have an increasingly larger strike-slip component along the Aleutian arc as the longitude becomes more westerly. Displacements in the Kurile, Kamchatka, and Japanese arcs are nearly pure dip-slip and represent underthrusting of the Pacific plate beneath the arcs.

The system of great east–west fracture zones in the northeastern Pacific (Figure 2) apparently was formed more than 10 m.y. ago. Since that time the directions of spreading changed from east–west to their present northwest–southeast pattern [*Vine*, 1966]. The more westerly strike of the slip vectors along the Juan de Fuca and Gorda ridges is consistent with the hypothesis that the area to the east of these ridges represents a small separate plate [*Morgan*, 1968; *McKenzie and Parker*, 1967] that was underthrust beneath the coast of Washington and Oregon to form the volcanoes of the Cascade range. A few earthquakes that have been detected in western Oregon and Washington are apparently of a subcrustal origin [*Neumann*, 1959; *Tobin and Sykes*, 1968]. The tectonics of this block appears to be quite complex. Internal deformation within this block, as indicated by the presence of earthquakes [*Tobin and Sykes*, 1968], and the small number of subcrustal events previously mentioned suggest that the tectonic regime in this region is being readjusted.

Depths of earthquakes, volcanism, and their correlation with high rates of underthrusting. The depths of the deepest earthquakes, the presence of volcanism, and the occurrence of tsunamis (seismic sea waves) seem to be related closely to the present rates of underthrusting in island arcs. In the series of arcs that stretches from Tonga to the Macquarie ridge the depths of the deepest earthquakes generally decrease from about 690 km in the north to less than 100 km in the south [*Hamilton and Evison*, 1968]. The rates of underthrusting computed by Le Pichon also decrease from north to south. Volcanic activity, which is prominent north of the South Island of New Zealand, dies out when the deepest part of the seismic zone shoals to depths less than about 100 to 200 km. Similarly, the depth of deepest activity in the Aleutian arc and the number of volcanoes (Figure 2) appear to decrease from east to west as the rate of underthrusting decreases In this arc the rate of underthrusting decreases because the slip vectors become more nearly parallel to the arc. Volcanism and the depth of seismic activity increase spectacularly as the slip vectors change from a predominance of strike-slip motion in the western Aleutians to largely dip-slip motion in Kamchatka, the Kurile Islands, and Japan. Most of the world's active volcanoes are located either along the world rift system or in regions that contain intermediate-depth earthquakes. Hence, the latter volcanoes are presumably located in the regions where the lithosphere has been underthrust to depths of at least 100 km.

Tsunamis. Although some prominent seismologists (e.g., *Gutenberg* [1939]) have argued that some of the largest tsunamis (seismic sea waves) are generated by submarine slides, many other geophysicists maintained that sudden dip-slip motion along faults during large earthquakes is the principal generating mechanism for most of the world's more widespread tsunamis. L. Bailey (personal communication), following a lecture by Sykes, suggested that tsunami generation might correlate with areas of large dip-slip motion. This suggestion may have considerable merit.

Figure 2 shows the epicenters of earthquakes for which tsunamis were detected at distances of 1000 km or greater [*Heck*, 1947; *Gutenberg and Richter*, 1954; *U. S. Coast and Geodetic Survey*, 1935–1965]. The distance criterion was used to eliminate waves of more local origin, some of which actually may be related to seiches or to submarine slides. Although this compilation undoubtedly is not complete even for the present century, the conclusions drawn here should not be seriously affected by the choice of data.

Since most of the earthquakes generating tsunamis are located in regions that are characterized by a high rate of dip-slip motion (Figure 2), the two phenomena appear to be causally related in many cases. Although most of the world's largest earthquakes occur in these areas and are characterized by a large component of thrust faulting, large earthquakes along major strike-slip faults near the coasts of southeast Alaska and British Columbia have not generated sea waves that were observable at distances greater than about 100 km. An earthquake located off the coast of California in 1927, however, generated a wave detected in Hawaii [*Gutenberg and Richter,* 1954]. Other generating mechanisms, such as volcanic eruptions and submarine slumping, cannot be excluded as the causative agents for at least some tsunamis.

Gutenberg [1939] argued that the epicenters of several earthquakes generating tsunamis were located on land. Although the epicenter, which represents the point of initial rupture, may be located inland, the actual zone of rupture in great earthquakes is now known to extend for several hundred kilometers. In many of the great earthquakes associated with island arcs and with active continental margins at least a portion of the zone of rupture is located in water-covered areas. Hence, the location of the epicenter itself is not an argument for the absence of significant vertical displacements in nearby submarine areas. *Utsu* [1967] has shown that, as a result of the anomalous zones in the mantle beneath island arcs, locations of shallow shocks based on teleseismic data are usually landward of the actual location. Both improved locations based on a better knowledge of seismic wave travel times in anomalous regions such as island arcs and rapid determinations of focal mechanisms may be of substantial value in tsunami warning systems.

Length of seismic zones in island arcs. The systematic changes in the seismic zones in the Aleutians and in the southwest Pacific suggest that the lengths of zones of deep earthquakes might be a measure of the amount of underthrusting during the last several million years. Using the maps of deep and intermediate-depth earthquakes prepared by *Gutenberg and Richter* [1954], *Oliver and Isacks* [1968] estimated the area of these zones and divided the total area by the length of the world rift system (about two great circles) and by 10 m.y., which is the duration of the latest cycle of spreading based on data from ocean-floor sediments and from magnetics [*Ewing and Ewing,* 1967; *Vine,* 1966]. They obtained an average rate of spreading for the entire rift system of 1.3 cm/yr for the half-velocity. This value is reasonable for the average velocity of spreading along the world rift system.

The hypothesis that the lengths of deep seismic zones are a measure of the amount of underthrusting during the past 10 m.y. is examined in greater detail in Figure 16 and in Table 1. Figure 16 illustrates the lengths of the seismic zones in various arcs and the corresponding rates of underthrusting as calculated by *Le Pichon* [1968] from observed velocities of sea-floor spreading and the orientation of fracture zones along the world rift system. In nearly all cases the regions with the deepest earthquakes (and hence the longest seismic zones as measured along the zones and perpendicular to the arcs) correspond to the areas with the greatest rates of underthrusting; regions with only shallow- and intermediate-depth events are typified by lower rates of underthrusting. Since the calculated slip rates and some of the measured lengths may be uncertain by 20% or more, the correlation between the two variables is, in fact, surprisingly good.

Although six points fall well above a line of unit slope, which represents an age of 10 m.y., all but one of the lengths are within a factor of 2 of the lengths predicted from the hypothesis that these zones represent materials underthrust during the last 10 m.y. Three of the more discrepant points, which are denoted by crosses on the figure, represent a small number of deep earthquakes that are located in

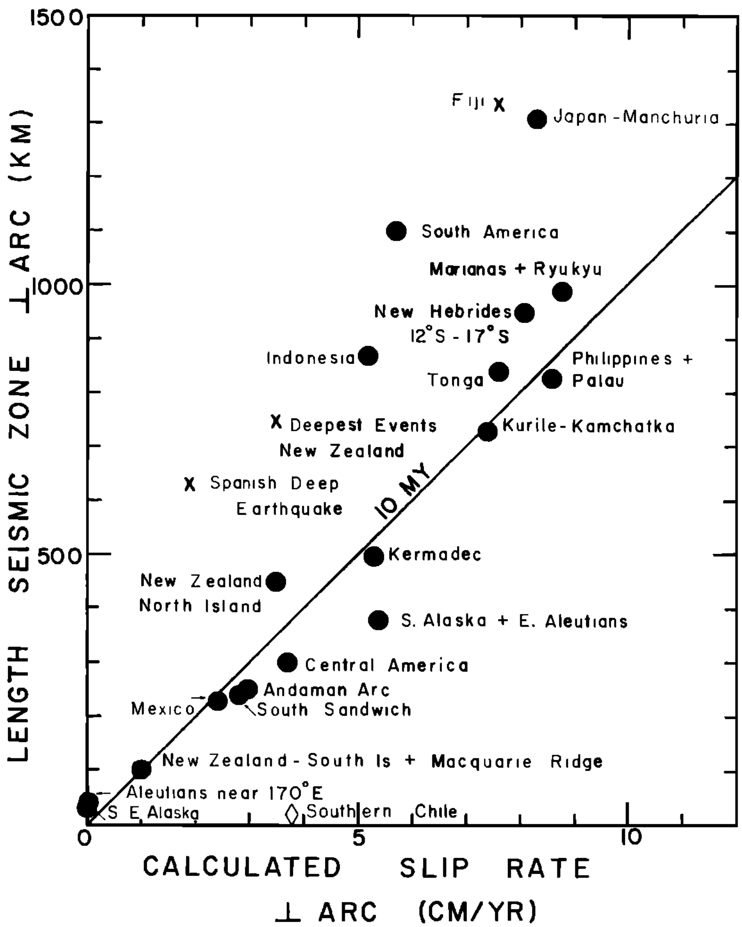


Fig. 16. Calculated rates of underthrusting [*Le Pichon*, 1968], and length of seismic zone for various island arcs and arc-like features (solid circles), for several unusual deep events (crosses), and for Southern Chile (diamond). The solid line indicates the theoretical locus of points for uniform spreading over a 10-m.y. interval.

TABLE 1. Comparison of Estimated Slip Rates for Three Island Arcs
Values are in centimeters per year.

Island Arc	Slip Rate from Shallow-Focus Seismicity, after *Brune* [1968]	Calculated Slip Rate, Data from Ocean Ridges, after *Le Pichon* [1968]	Slip Rate* Assuming Length Seismic Zone Measures Amount Underthrust during Last 10 m.y.
Tonga	5.2	7.6	8.4
Japan	15.7	8.8	16.3
Aleutians	3.8	6.1	4.0

* Corrected for azimuth of slip vector using data of *Le Pichon* [1968]. Rates given for the three methods are the magnitudes of the total slip vectors and not the magnitudes of the dip-slip components used in Figure 16.

unusual locations with respect to the more active, planar zones of deep earthquakes. They include the unusual deep Spanish earthquake of 1954, three deep earthquakes in New Zealand, and a few deep events under Fiji that appear to fall between the deep zones in the Tonga and New Hebrides arcs.

Other estimates of slip rates in island arcs. Slip rates for Tonga, Japan, and the Aleutians (Table 1) were calculated (1) from the dislocation theory using data on the past occurrence of earthquakes [*Brune*, 1968], (2) from the observed spreading velocities along ocean ridges assuming that all of the spreading is absorbed by underthrusting in island arcs [*Le Pichon*, 1968], and (3) from the lengths of the seismic zones assuming these lengths are a measure of the amount of underthrusting during the last 10 m.y. For each arc the three rates are within a factor of 2 of one another.

The 10-m.y. isochron. Two hypotheses may explain the correlation between the length of seismic zones and the computed rate of underthrusting (Figure 16). One theory, which was mentioned earlier, assumes that the present seismic zones in island arcs were created during a recent episode of spreading which began about 10 m.y. ago (Figure 14*a*). In the other hypothesis, 10 m.y. is regarded as the approximate time constant for assimilation of the lithosphere by the upper mantle (Figure 14*c*). Since the zone of anomalously high heat flow on ocean ridges appears to be confined to regions less than about 10 m.y. old [*McKenzie*, 1967; *Le Pichon and Langseth*, 1968], 10 m.y. is a reasonable time constant for the creation of the normal oceanic lithosphere. Hence, this value is not an unreasonable first approximation to the time constant for assimilation of the lithosphere in island arcs. It should be recognized, however, that the time constant for ocean ridges may be uncertain by more than a factor of 2 because of scatter in heat flow data. At present, it does not seem possible to ascertain which of the two alternative proposals (Figure 14*a* or 14*c*) governs the lengths of seismic zones in island arcs.

With the exception of the data for southern Chile (the diamond in Figure 16) there are no points lying significantly below the 10-m.y. isochron. A reduction in slope at the higher spreading rates might occur if the lithosphere were suddenly modified at a given depth through warming or a phase change that would prevent deeper earthquakes from occurring. These processes apparently have not led to significant decreases in the lengths of the seismic zones compared with those predicted by the 10-m.y. isochron in Figure 16.

Southern Chile. In southern Chile between 46° and 54°S, the absence of observable deep activity, the near-absence of shallow seismicity, and the presence of a sediment-filled trench [*Ewing*, 1963; *Hayes*, 1966] are in obvious conflict with the predicted length of the seismic zone. Le Pichon's omission of two very active features, the West Chile ridge and the Galapagos rift zone (Figure 15) in his analysis may, however, explain this discrepancy. *Wilson* [1965*a*] argued that since Antarctica is almost surrounded by spreading ridges, the coast of South America south of its juncture with the

West Chile ridge at 46°S should not be seismically active. An alternative explanation for the near-absence of shallow seismicity is that activity during the past 70 years of instrumental seismology is not representative of the long-term activity. This explanation is, however, difficult to apply to the hypothetical deep seismic zone that is predicted from Le Pichon's calculations. Hence, we feel that either Wilson's proposal or some other explanation is needed to account for the tectonics of southern Chile.

Departures from the 10-m.y. isochron. Several factors could explain the six points that fall well above the 10-m.y. isochron in Figure 16: (1) Some pieces of lithosphere became detached from the main dipping zones of deep activity by active processes, such as gravitational settling of slabs of lithosphere or convection currents in the asthenosphere (Figure 14*d*). (2) The initiation of underthrusting and spreading was not simultaneous in all regions. (3) Some or all anomalous deep events may be related to previous episodes of underthrusting. (4) The computed slip rates are not correct. (5) If the thermal time constant for assimilation of the lithosphere is generally about 10 m.y. (Figure 14*c*), the anomalous points may represent pieces of lithosphere with anomalously high time constants so that they are not completely assimilated.

Although some of the computed spreading rates may be in error (particularly the values for Indonesia and South America may be in error because the magnetic data in the Indian Ocean are poor and because the West Chile ridge and the Galapagos rift zone (Figure 14) were not included in Le Pichon's analysis), it is difficult to argue that these rates are greatly in error for each of the six anomalous points. The possible ramifications of the various alternative proposals should be exciting enough to encourage further study of these new approaches to the distribution of deep earthquakes.

Interaction of continental blocks of lithosphere. The Alpide belt, which comprises much of the Mediterranean region, the Middle East, and large parts of central and southern Asia, was not included in Figure 16 because it is very difficult to define the total amount of underthrusting. Seismic actvity in the Alpide zone, unlike that along either the ocean ridges or the typical island arcs, occupies a very broad region (Figure 15). Spreading in the Indian Ocean is apparently being absorbed in several subzones within the Alpide belt. This conclusion is supported by the widespread distribution of large shallow earthquakes and by the relatively small number of intermediate- and deep-focus earthquakes [*Gutenberg and Richter*, 1954].

Although, in principle, it seems reasonable to describe the tectonics of Eurasia by the interaction of blocks of lithosphere, it is not yet clear how successful this idea will be in practice because of the large number of blocks involved. The interaction of blocks of lithosphere appears to be much more complex when all the blocks are continents or pieces of continents than when at least one is an oceanic block. In addition to activity in the Alpide belt, earthquakes in East Africa, northern Siberia, and western North America (including Alaska) are more diffused in areal extent (Figure 15). In contrast, seismic zones associated with ocean ridges, island arcs, and many active continental margins appear to be extremely narrow and well defined. Several factors may explain these differences: (1) The lithosphere may be more heterogeneous in some or all continental areas and, hence, may break in a more complex fashion. (2) Old zones of weakness in continental areas may be reactivated. (3) Because of its relatively low density, it may not be possible to underthrust a block of *continental* lithosphere into the mantle to depths of several hundred kilometers. This third point is supported by the relatively large areas of older continental rocks.

As much of the sea floor appears to be relatively young, plates of oceanic lithosphere probably do not contain a large number of zones of weakness, whereas the continental plates, which are older, seem to contain many zones of weakness. Except for a few earthquakes along the extensions of fracture zones, the ocean basins are extremely quiet seismically. Continents, however, exhibit a low level of activity in many areas that do not appear to be undergoing strong deformation. This contrast in activity does not appear to be an artifact of the detection system but rather appears to be related to the different character of the oceanic and continental lithosphere.

One exception to this rule is the near-absence of observable activity in Antarctica. Unlike the other continents, Antarctica is almost surrounded by ocean ridges that are probably migrating outwardly [*Wilson*, 1965*a*]. Hence, the antarctic plate may not be subjected to stresses as large as the stresses occurring in other continents.

Summary of evidence for global movements. The concentration of seismic activity and the consistency of individual mechanism solutions for mid-ocean ridges, for island arcs, and for most of the world's active continental margins indicate that tectonic models involving a few plates are highly applicable for these areas. Since individual mechanism solutions in a given region are highly coherent, each solution may be regarded as an extremely pertinent datum. It is not necessary to resort to complex statistical methods for analyzing the relative motions of these large blocks. For these regions much of the scatter in many previous mechanism studies appears to be largely a result of poor seismic data and errors in analysis. Although the concept of interacting blocks of lithosphere may not be as easily applied to the complex interactions of continental blocks in such areas as the Alpide belt, a consistent (but complex) tectonic pattern may yet emerge when the pattern of seismicity is well defined and when a sufficiently large number of high-quality mechanism solutions is available. Thus, the new global tectonics among other things appears to explain most of the major seismic zones of the world, the distribution and configuration of the zones of intermediate and deep earthquakes, the focal mechanisms of earthquakes in many areas, and the distribution of a large number of the world's active volcanoes.

Additional Evidence for Existence of Lithosphere, Asthenosphere, and Mesosphere

Complex tectonic patterns near the earth's surface. A number of objections have been raised to models of sea-floor spreading that involve simple symmetrical convection cells extending from great depths to the surface of the earth. They include such statements as: Since the Atlantic and Indian oceans are both spreading, the tectonics of Africa should be of a compressional nature rather than of an extensional nature, as reported for the East African rift valleys; if spreading from the eastern flank of the East Pacific rise near Mexico is absorbed only a few hundred kilometers from the rise in the Middle America trench, it is difficult to imagine how materials on the western flank of the rise travel more than 10,000 km before they are absorbed in the trenches of the western Pacific. Although some of the fracture zones on ocean ridges may be as long as 1000 km, some of these zones are less than 50 to 100 km apart. It is almost impossible to believe that these long narrow strips reflect the shape of independent convection cells. These and several similar dilemmas may be resolved if a layer of greater strength (the lithosphere) overlies a region of much lesser strength (the asthenosphere).

The configuration of the asthenosphere and of the various pieces of lithosphere may in some ways be analogous to blocks of ice floating on water. Although the surface pattern of the ice may be very complex, the pattern of convection in the water below or in the air above may be very simple, or it may be complex and of a completely different character than that of the motions of the ice. This analogy may be relevant to the more complex nonsymmetrical tectonic pattern exhibited by the earth's surface. A lithospheric model readily accounts for the asymmetrical structure of island arcs and for the symmetrical configuration of ocean ridges. It also explains the rather smooth variations in rates of spreading in a given ocean.

In this model the ridges and island arcs are dynamic features, and hence they appear to move with respect to one another. Thus, a region of extensional tectonism may be located between two spreading ridges. What is demanded in this model, however, is that surface area be conserved on a worldwide basis. To what extent the motions of large plates of lithosphere are coupled to motions in the asthenosphere remains an unsolved problem.

Depths of earthquakes along the world rift system. The existence of a layer of strength near the earth's surface is compatible with the observation that all earthquakes on the ocean ridges are apparently of shallow focus (i.e., less than a few tens of kilometers deep). Using a dislocation model, *Brune* [1968] estimated that the zone of earthquake generation for some oceanic fracture zones appears to be less than 10 km in vertical extent.

The San Andreas fault of California appears to be a major transform fault connecting growing ocean ridges off the west coast of Oregon and Washington with spreading ridges in the Gulf of California (Figure 3). Observed seismic activity along the San Andreas system is confined to the upper 20 km of the earth, and much of this activity is confined to the upper 5 or 10 km [*Press and Brace*, 1966]. Most estimates of the depths of faulting in the great San Francisco earthquake of 1906, the Imperial Valley earthquake of 1940, and the Parkfield earthquake of 1966 are within the upper 10 to 20 km of the earth [*Kasahara*, 1957; *Byerly and DeNoyer*, 1958; *Chinnery*, 1961; *McEvilly et al.*, 1967; *Eaton*, 1967]. Since displacements of at least 100 km (and probably 300 to 500 km) probably have occurred along the San Andreas fault [*Hamilton and Meyers*, 1966], it is almost certain that deformation by creep without observable earthquake activity must be occurring at depths greater than 20 km. Thus, an upper layer of strength in which earthquakes occur associated with a zone of low strength below seems to be demanded for California and for other parts of the world rift system.

Variations in thickness of the lithosphere along the world rift system. Although earthquakes on the mid-Atlantic, mid-Indian, and Arctic ridges occur along the ridge crests themselves as well as along fracture zones, observable seismic activity along much of the East Pacific rise is concentrated almost exclusively along fracture zones [*Menard*, 1966; *Tobin and Sykes*, 1968]. With the exception of the Gorda ridge off northern California, which appears to be spreading relatively slowly, earthquakes are only rarely recorded from the crest of most of the East Pacific rise. Since evidence from magnetic anomalies indicates a relatively fast rate of spreading (greater than 3 cm/yr for the past 1 m.y.) along much of the East Pacific rise [*Heirtzler et al.*, 1968] and since earthquakes are present on the bounding transform faults, much of the crest of this rise appears to be spreading by ductile flow with little or no observable seismic activity.

Menard [1967] and *van Andel and Bowin* [1968] observed that ridges with spreading rates higher than about 3 cm/yr are typified by fairly smooth relief, a thin crest, and the absence of a median rift valley. Ridges with lower spreading rates are, however, characterized by high relief, a thick crust, and a well-developed median valley. *van Andel and Bowin* [1968] suggest that materials undergoing brittle fracture may be thin at sites of faster spreading because higher temperatures might occur at shallower depths. The crest of the East Pacific rise is characterized by heat flow anomalies that are broader and apparently of greater amplitude than the anomalies associated with ridge crests in the Atlantic and Indian oceans [*Le Pichon and Langseth*, 1968]. Thus, higher temperatures beneath the East Pacific rise may suppress the buildup of stresses large enough to generate observable seismic activity.

If the occurrence of earthquakes and the presence of a median rift valley correlate with high strength and their absences (in spite of large deformations) correlate with low strength, the lithosphere may be very thin or almost nonexistent near the crest of the East Pacific rise. The lithosphere must be present, however, within a few tens of kilometers or less of the crest to account for the much higher seismic activity along fracture zones. It is possible that seismic activity along the crest of the East Pacific rise may occur mainly as small earthquakes that either are not detected or are rarely detected by the present network of seismograph stations. A microearthquake study using either hydrophones or ocean-bottom seismographs could furnish important information about the mode of deformation at the crest of these submarine ridges.

High-frequency S_n waves crossing ocean ridges. *Molnar and Oliver* [1968] studied S_n propagation in the upper mantle for about fifteen hundred paths; they did not observe any high-frequency S_n waves for ocean paths that either originate at or cross an active ridge crest. Their observations also suggest that the uppermost mantle directly beneath ridge crests is not included in the lithosphere but that the uppermost mantle must be included in the lithosphere beyond about 200 km of the crest in order to explain the propagation of high-frequency S_n from several events on some of the larger fracture zones. The distance for any given ridge may depend on the spreading rate, but the data were inadequate to confirm this assumption. The thinning or near-absence of lithosphere in a zone near the ridge crest would be compatible with the magmatic emplacement of the lithosphere near the crests of ocean ridges and with a gradual thickening of this layer as it cools and moves away from the crest. This thinning may also explain the maintenance of near-isostatic equilibrium over ocean ridges [*Talwani et al.*, 1965*a*].

Thickness of the lithosphere from heat flow anomalies. If the spreading rates inferred from magnetic anomalies are accepted, the calculated heat flow anomaly over ridges for a model of a simple mantle convection current that extends to the surface is not compatible with the observed heat flow anomaly [*Langseth et al.*, 1966; *Bott*, 1967; *McKenzie*, 1967]. These observations are, however, compatible with the computed heat flow for a simple model of cooling lithospheric plate approximately 50 km thick [*McKenzie*, 1967]. In this model the heat flow anomaly results from the cooling of the lithosphere after it is emplaced magmatically near the axis of the ridge. Because of the scatter in the heat flow data and because of the simplifying assumptions used to obtain the estimate of 50 km, the value for the thickness either may be uncertain or may vary by a factor of 2 or more. The occurrence of earthquakes as deep as 60 km beneath Hawaii [*Eaton*, 1962] may also be used as an estimate of the thickness of the lithosphere. *Orowan* [1966] tried to fit the observed heat flow pattern for ridges with models in which the crust is stretched on the ridge flanks and the rate of spreading is a function of distance from the ridge. His model does not appear to be compatible either with the symmetry of magnetic anomalies close to the ridge or with the narrow width of the seismic zones on most ocean ridges.

Maximum sizes of earthquakes. The sizes of the largest earthquakes along ocean ridges and along island arcs may be related to the thickness of the lithosphere in each of these tectonic provinces. The world rift system (including California, southeast Alaska, and east Africa) accounts for less than 9% of the world's earthquakes and for less than 6% of the seismic energy in earthquakes; island arcs and similar arcuate structures contribute more than 90% of the world's energy for shallow earthquakes and nearly all the energy for deep earthquakes [*Gutenberg and Richter*, 1954]. The relatively small amount of seismic activity on the ridge system and its largely submarine environment probably explain why the significance and the worldwide nature of this tectonic feature were not clearly recognized until about 12 years ago.

Although *Richter* [1958] lists more than 175 very large earthquakes (magnitude M greater than or equal to 7.9) as originating in arcs and arc-like features, he reports only 5 events of this size for the world rift system. Whereas the largest event on the ridge system was of magnitude 8.4, the largest known earthquakes from island arcs were of magnitude 8.9. This difference in magnitude corresponds to about 8 times as much energy in the larger shocks. Most (and perhaps all) of the earthquakes on the ridge system with magnitudes greater than about 7 appear to have occurred along major transform faults.

Thus, the maximum magnitudes of earthquakes during the last 70 years for various tectonic features may be summarized as follows: island arcs, 8.9; major fracture zones, 8.4; ridge crests in the Atlantic, Indian, and Arctic oceans, about 7; most of the crest of the East Pacific rise (with the exception of the Gorda ridge), few (and possibly no) events larger than 5. The relative frequencies of very large earthquakes and possibly the upper limits to the sizes of earthquakes appear to be related to the area of contact between pieces of lithosphere that move with respect to one another. On the crest of the East Pacific rise the zone of contact between brittle materials is either absent or is confined to a thin surficial layer; at the crests of other ridges the lithosphere appears to be somewhat thicker. For fracture zones the thickness of the lithosphere may be as great as a few tens of kilometers; thus, the maximum magnitudes may be limited by the length of the fracture zone and by the thickness of the lithosphere. In island arcs the thickness of the lithosphere may be greater than that on the ridges because the temperatures beneath ridges are higher and perhaps because some material is added to the bottom of the lithosphere as it moves away from a ridge crest. Since the dip of seismic zones in island arcs usually is not vertical, the area of contact between plates of lithosphere may be increased by this factor alone. For example, the zone of slippage in the great Alaska earthquake of 1964 appears to dip northwesterly at a shallow angle (about 10°) and to extend for about 200 km perpendicular to the Aleutian island arc [*Plafker*, 1965; *Savage and Hastie*, 1966]. Since the dip is shallow, this zone does not extend more than about 40 km below the surface at its deepest point. Thus, it is not unreasonable that much greater amounts of elastic energy can be stored and released along island arcs than along the mid-ocean ridges.

Since single great earthquakes apparently have not involved a rupture that extended along the entire length of the larger island arcs [*Richter*, 1958], some other factor appears to limit the length of rupture and hence the maximum sizes of earthquakes. Tear faults (i.e., transform faults of the arc-arc type) within the upper thrust plate may divide island arcs into smaller subprovinces that are not completely coupled mechanically.

Some properties of the lithosphere and asthenosphere. Thus far we have defined the lithosphere as a layer of high strength and the asthenosphere as a layer of low strength. Since the rheological properties of the mantle are not at all well understood, we have purposely used the term strength in a general sense without being more specific about the actual mechanisms of deformation. Thus, it is not at all certain that various techniques for measuring the thickness and the presence of the lithosphere necessarily yield comparable values since these methods probably sample different physical parameters. It is encouraging, however, that estimates of the thickness of the lithosphere from analyses of heat flow anomalies, the absence of earthquakes (in spite of large deformation), the requirements of isostasy and the maintenance of mountains [*Daly*, 1940], the amplitudes and wavelengths of gravity anomalies [*McKenzie*, 1967], the propagation of high-frequency S_n waves, and a transition in the types of earthquake mechanisms in island arcs are about 100 km or less. It is not clear, however, to what extent the variations in these estimates represent real differences in thickness or merely differences in the relations between thickness and the various physical parameters measured.

The asthenosphere in the three-layered model shown in Figure 1 also roughly coincides with the low-velocity zone for S waves [*Gutenberg*, 1959; *Dorman et al.*, 1960; *Anderson*, 1966; *Ibrahim and Nuttli*, 1967], regions of either low velocity or nearly constant velocity for P waves [*Lehmann*, 1964*a*, *b*] and of low density or nearly constant density [*Pekeris*, 1966], a region of high attenuation for seismic waves, particularly S waves [*Anderson and Archambeau*, 1964; *Anderson*, 1967*b*; *Oliver and Isacks*, 1967], and a low-viscosity zone in the upper mantle [*McConnell*, 1965]. Although these observations yield very little information about the actual mechanisms of dissipation, they are consistent with the hypothesis that the asthenosphere is a region of low strength bounded below by the mesosphere, a region of greater strength. The simplest explanation for these phenomena is that the closest approach to melting (or partial melting) occurs in the asthenosphere. That relative displacements of the earth's surface may be modeled by a series of moving plates is a strong argument for a region of low strength (the asthenosphere) in the upper mantle. Nevertheless, the physical properties and configuration of the asthenosphere and the lithosphere may vary from place to place. In fact, variations of these types seem to be required to account for many of the complexities of the outer few hundred kilometers of the earth. The evidence for such lateral variations is now so strong as to demand reevaluation of studies of velocity and Q structure based on models that consist only of concentric spherical shells.

Conflicting Seismological Evidence and Some Problems

The new global tectonics, in one form or another, has been remarkably successful in explaining many gross features and observations of geology, but its development must be continued in an effort to establish a theory that is effective throughout the earth sciences at levels of increasingly greater detail. Many difficult problems remain. A quantitative understanding of the processes in the mantle that result in the observed surface features is urgently needed. The roles of the earth's initial heat, gravitational energy, radioactive heat, and phase changes must be understood. The mechanics of flow in the mantle is little known. The history of sea-floor spreading through geologic time and the relation between the vast quantities of geologic observations and the hypotheses must be worked out. A vital unanswered question is why island arcs and ocean ridges have their particular characteristic patterns. These grand questions are of concern to seismologists, not only because there is general interest in the hypotheses but also because the solutions may very well be dependent on evidence from seismology. In the absence of all but the most preliminary attempts to solve these problems, however, it is difficult to speculate on the direction and force of the evidence. At the present there appears to be no evidence from seismology that cannot eventually be reconciled with the new global tectonics in some form. Some traditional views that once would have appeared contradictory (such as, for example, the assignment of substantial rigidity to the entire mantle over a long time interval because of efficient propagation of shear waves with periods of about 10 sec) have long been discounted on the basis of information on glacial rebound, gravity, and, more recently, creep along long strike-slip faults, such as the San Andreas fault. That deep earthquakes generate shear waves and radiate waves in a pattern characteristic of a shear dislocation can hardly be taken as evidence for strength throughout the mantle in that depth range in light of a hypothesis that suggests that the mantle in the deep earthquake zones is much different from the mantle at comparable depths elsewhere.

The arc-like patterns of the active zone is but one of the problems of a subject that might be termed 'lithosphere mechanics.' Others are the shape of the deep zones, which sometimes appear to be near-planar after turning a rather sharp corner near the surface; the distribution of stress and strain in the lithosphere, not merely in active seismic areas but throughout the world; the interaction of lithosphere and asthenosphere; and flow in the asthenosphere.

Some specific evidence from seismology that does not readily fit into the new global tectonics at present does exist, however. For example, the locations of the Spanish deep earthquake and certain deep shocks in New Zealand and Fiji are not easily explained by current thinking, as has been pointed out above. The patterns of minor seismicity, including an occasional large earthquake in certain continental areas such as the St. Lawrence Valley and the Rocky Mountains and broad regional scattering of epicenters as in east Africa, are not simply explained as yet, nor is the almost complete lack of earthquakes in the oceanic lithosphere, which presumably can and does transmit stress over large distances. The occurrence of large active strike-slip faults that trend along the arc structure, such as the Alpine fault in New Zealand, the Philippine fault, the North Anatolian fault in Turkey, and the Atacama fault in Chile, ofters some difficulties. Tentative explanations have been proposed for the Alpine and Philippine faults; therefore, this matter may be resolved. Perhaps related to this problem is the occurrence in island arcs of occasional earthquakes with large strike-slip components along faults subnormal to the arc. The tectonics of the arc can hardly be as simple as that implied by Figure 1.

A general problem involving seismology concerns contrasts between continental and oceanic areas [*MacDonald*, 1964, 1966]. Some studies [e.g., *Brune and Dorman*, 1963; *Toksöz and Anderson*, 1966] show corresponding structural differences to depths of several hundred kilometers. Are such differences contrary to the new global tectonics? Might the observations be equally well satisfied by other models that are compatible with requirements of the new hypotheses? A serious difficulty may arise here if they cannot. Comparable and related problems arise in other disciplines. Observation suggests that heat flow per unit area is about the same under the oceans as under the continents. If it is assumed that the heat flux is largely due to radioactive decay and that radioactivity is heavily concentrated in certain continental rocks, lateral heterogeneity of the upper mantle is required and the mixing predicted by the new tectonics may be so great as to destroy such heterogeneity. Perhaps the amount of radioactivity in the earth has been highly overestimated, as has occasionally been suggested [*Verhoogen*, 1956], and much of the heat lost at the surface is transported from the deep interior by convection. More accurate determination of deep structure by seismological techniques is thus relevant to the problem of the amount of radioactivity in the earth and its distribution. Seismology is also vitally linked with heat flow in the island arcs, where low heat flow values appear to correlate with zones of descending lithosphere but anomalous high values appear over the deep seismic zones, and at the ocean ridges, where high heat flow may correspond with low strength and hence low seismic activity.

Other disciplines are also involved. The new structures based mainly on seismological information must be tested against gravity data. The record in the ocean sediments surely provides the most complete history of the ocean basins and hence must provide insight into seismological processes. Perhaps the crucial evidence on the validity of new global tectonics will come from cores of the entire sedimentary column of the ocean floor.

In petrology an interesting question concerns the origin of the belts of andesitic volcanoes. *Coats* [1962] proposed that ocean crust and sediments were thrust into the mantle under the Aleutian arc and subsequently erupted with mantle rock to account for the petrology of those islands. Can all the andesites of the active tectonic belts be explained in this manner? Are there any young (less than 10 m.y.) andesites of the same type that are *not* associated with active deep earthquake zones and arcs? Can this sort of information be used to identify ancient arcs?

The important problems arising from the new global tectonics are many; some are crucial. Evidence from seismology against the new global tectonics appears, however, to lack force.

Effects of New Global Tectonics on Seismology

That seismology is providing abundant and important information for testing the new global tectonics is demonstrated in other sections of this paper and elsewhere. To date, most of the seismological work related to this topic has been so directed. The countering impact of the new global tectonics on seismology must also be carefully and thoroughly considered for indications of new directions for, and new attitudes toward, seismological research. This section is largely speculative and some of the points may seem farfetched. If, however, a basic understanding of global tectonics is imminent, even the most imaginative and wisest forecast of its effect on seismology is likely to be too conservative. Nor is seismology alone in this regard, for all branches of geology and geophysics related to the earth's interior will be comparably affected. The assumption that a major advance in our understanding of global tectonics has been achieved is tacit in the following.

Seismicity is an important branch of seismology and one that will be strongly affected by the impact of the new global tectonics. Such basic questions as why earthquakes occur largely in narrow belts separated by large stable blocks, why these belts are continuous on a worldwide scale, why they branch, why certain details of their configuration are as they are, why intermediate and deep earthquakes occur in some areas and not others are in the process of being answered today. There is every indication that the relation between seismic activity and geology will soon be understood to a much greater extent than contemplated heretofore. Improved accuracy in hypocenter location resulting from a better knowledge of velocity structure will facilitate this development. It will also assist in solving seismology's chief problem of a political nature, distinguishing between earthquakes and underground nuclear explosions. The self-consistent worldwide pattern of focal mechanisms will also be valuable here. Perhaps location of new seismograph stations in the proper relation to high-Q zones within the earth will result in improved detection capability. Important questions still remain. For example, why have the earthquake belts and the associated tectonic belts assumed their present configuration? What can be learned about paleoseismicity and its relation to modern seismicity? How can the pattern of minor seismicity, including the occasional major earthquake outside the established seismic belts, be fit into the new global tectonics?

Closely related to the distribution of seismic events in time and space and to tectonics is the focal mechanism of the earthquake. Data of quality and quantity from modern observing stations can provide information for determination of focal mechanisms that severely test the new hypotheses and are crucial to their development. It appears that with only modest advances in technique properly documented earthquakes will provide reliable detailed information on tectonic activity. Focal mechanism, stress drop, slip, and orientation of principal stresses should be available from individual earthquakes, and singly or cumulatively these data will be integrated with the tectonic pattern.

Implicit in the new global tectonics is a new attitude toward mobility of the earth's strata. Measurements of fault creep and other forms of earth strain over recent short intervals of time are based on a variety of measuring techniques that include geodetic surveying, tide and strain gaging, and measurements of earthquake slip. Various methods of field geology give data extending over varying but greater intervals of time. When all these data are reduced to velocities that describe the motion of one point in the earth relative to another point located in an adjacent relatively underformed block, values are obtained that are in the same range as the velocity values associated with sea-floor spreading determined through analysis of geomagnetic data. This range is from about 1 to perhaps 10 cm/yr or more. These values are considerably higher than the values usually assumed in the past by most earth scientists considering deformation of this type; hence, new attitudes toward old problems must be anticipated. A prime example in seismology, cited above, is the association of the length along the dip of a deep seismic zone with the amount of relatively recent underthrusting in a region.

With the new global tectonics, interrelationships on a large, perhaps worldwide, scale can be predicted and perhaps observed. Thus, major seismic activity in one area could be related to that in an adjoining, or perhaps distant, area associated with the same lithospheric unit or units, for, although the propagation time for the effect may be long, stress may be transmitted over large distances through the lithosphere. Of special interest is the new insight into the subject of earthquake prediction, even prevention. Although an empirical method of prediction could by chance be found in the absence of an understanding of the process, an effective method is much more likely to be achieved if a basic understanding of the earthquake phenomenon is established. The new global tectonics offers great promise for such an achievement. It has already provided a theory that predicts over-all strain rates in tectonic areas throughout the world. It suggests a means for predicting the maximum size of an earthquake in a given region. It provides a framework in which to relate measurements of distortion, such as strains, tilts, and sea-level changes, with observations on the mechanism of the earthquake. It has established the continuity of active zones and has shown that apparently inactive segments of otherwise scismic belts must, indeed, be active, either by subsequent earthquakes or by creep. Refinements and further developments must be anticipated.

Seismology has long been the principal source of information on the structure of the earth's interior and is likely to continue in that role, with or without the new global tectonics. The new hypotheses will, however, certainly stimulate radically new approaches to the exploration of the earth's interior. A common and powerful technique of seismology involves the use of simplified earth models for prediction of certain observed effects. The new global tectonics calls for an entirely new kind of model. Layered models in which the shells are spherically symmetric are now outdated for many areas of the earth. Models based on the effect of spreading and growth of the lithosphere at the rifts and underthrusting at the island arcs must be tested against observation. The conventional division of the earth's surface into oceanic and continental areas, or oceanic, shield, and tectonic areas, requires a new look when the lithosphere, with its lateral variations, is involved. New models in which the age and the thickness of the lithosphere, as well as other properties, are taken into account are required.

In the new tectonics, information on properties and parameters such as attenuation, strength, creep, viscosity, and temperature is critically needed. Further efforts must be made to understand the relation between such properties and the seismic properties that are measured in a more straightforward manner. At the island arcs, where cold exotic materials are descending into the mantle, nature is performing the experiment of subjecting what are normally near-surface materials to the higher temperatures and pressures of the asthenosphere, an experiment in many respects much like the ones being performed in various laboratories. As our techniques for measuring the composition of the material and the dynamic and environmental parameters of this situation improve, this experiment should yield important information on many topics. Of great interest to seismologists is the long-standing problem of the mechanism of deep earthquakes. The radiation patterns of seismic waves from many events present a reasonably consistent pattern but one that cannot readily be explained in detail by existing hypotheses. The variation of seismic activity with depth is another source of information. Are the focal mechanisms of earthquakes at depth really as much like those in near-surface brittle materials as they seem? What is the role of water, of other interstitial fluid, of partial melting? Are phase changes in the exotic mantle materials important, perhaps not as sources of seismic waves but as concentrators of stress that leads ultimately to rupture? How are the seismic observations of focal mechanism, spatial and temporal distribution, energy, etc., related to the material of the mantle and what can we learn about that material? How are these data related to the general configuration of the seismic zone and of the geology of the island arc? These are some of the topics on which this experiment may provide information.

Surely, the most striking and perhaps the most significant effect of the new global tectonics on seismology will be an accentuated interplay between seismology and the many other disciplines of geology. The various disciplines which have tended to go their separate ways will find the attraction of the unifying concepts irresistible, and large numbers of refreshing and revealing interdisciplinary studies may be anticipated. For example, the geomorphology of an area of raised beaches takes on new light for those interested in paleoseismicity; the tectonic significance of a feature of the ocean floor is determined by its seismicity and by the mechanism of the earthquakes; the petrology of a volcano of an island arc is related in a meaningful way to the seismic activity below; the worldwide phenomena of seismology provide crucial evidence on the basic processes of the earth's interior that have shaped and are shaping the surficial features of interest to classical geology. Even if it is destined for discard at some time in the future, the new global tectonics is certain to have a healthy, stimulating, and unifying effect on all the earth sciences.

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