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Earth's Shifting Crust

A Key to Some Basic Problems of Earth Science

by CHARLES H. HAPGOOD

with the collaboration of JAMES H. CAMPBELL

Foreword by

ALBERT EINSTEIN

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to FRED, WILLIE, PRU, *and* MARY G.

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"We know that there is no absolute knowledge, that there are only theories; but we forget this. The better educated we are, the harder we believe in axioms. I asked Einstein in Berlin once how he, a trained, drilled, teaching scientist of the worst sort, a mathematician, physicist, astronomer, had been able to make his discoveries. 'How did you ever do it,' I exclaimed, and he, understanding and smiling, gave the answer:

" 'By challenging an axiom!' "

Lincoln Steffens, *Autobiography* (p. 816)

FOREWORD *by Albert Einstein*

I frequently receive communications from people who wish to consult me concerning their unpublished ideas. It goes without saying that these ideas are very seldom possessed of scientific validity. The very first communication, however, that I received from Mr. Hapgood electrified me. His idea is original, of great simplicity, and—if it continues to prove itself—of great importance to everything that is related to the history of the earth's surface.

A great many empirical data indicate that at each point on the earth's surface that has been carefully studied, many climatic changes have taken place, apparently quite suddenly. This, according to Hapgood, is explicable if the virtually rigid outer crust of the earth undergoes, from time to time, extensive displacement over the viscous, plastic, possibly fluid inner layers. Such displacements may take place as the consequence of comparatively slight forces exerted on the crust, derived from the earth's momentum of rotation, which in turn will tend to alter the axis of rotation of the earth's crust.

In a polar region there is continual deposition of ice, which is not symmetrically distributed about the pole. The earth's rotation acts on these unsymmetrically deposited masses, and produces centrifugal momentum that is transmitted to the rigid crust of the earth. The constantly increasing centrifugal momentum produced in this way will, when it has reached a certain point, produce a movement of the earth's crust over the rest of the earth's body, and this will displace the polar regions toward the equator.

Without a doubt the earth's crust is strong enough not to give way proportionately as the ice is deposited. The only doubtful assumption is that the earth's crust can be moved easily enough over the inner layers.

The author has not confined himself to a simple presenta-

tion of this idea. He has also set forth, cautiously and comprehensively, the extraordinarily rich material that supports his displacement theory. I think that this rather astonishing, even fascinating, idea deserves the serious attention of anyone who concerns himself with the theory of the earth's development.

To close with an observation that has occurred to me while writing these lines: If the earth's crust is really so easily displaced over its substratum as this theory requires, then the rigid masses near the earth's surface must be distributed in such a way that they give rise to no other considerable centrifugal momentum, which would tend to displace the crust by centrifugal effect. I think that this deduction might be capable of verification, at least approximately. This centrifugal momentum should in any case be smaller than that produced by the masses of deposited ice.

AUTHOR'S NOTE: *To the Layman and the Specialist*

This book is addressed primarily to the layman. It is intended to be read by everyone interested in the earth and in the history and future of life on the earth.

I believe that the most important problems of science—that is, the most fundamental principles of scientific thought and method—may be understood by everyone. I think it is the obligation of scientists to make these essentials clear, for only in this way can science arouse public interest. It is unquestionable that without a public interest science cannot flourish.

The impression that serious scientific problems are far beyond the understanding of the average man constitutes a serious obstacle to the growth of this public interest. A caste system of specialists has been created, and it has produced a sort of intellectual defeatism, so that the layman tends to think that the conclusions he can reach with his own faculties are invalid, no matter how carefully he examines the evidence. This is an error that inhibits the spread of scientific knowledge and tends to discourage the recruitment of scientific workers, for every scientist has been an amateur to start with.

In addressing this book to the general public, I hope not only to promote a wider discussion of the basic problems of the earth; I hope also that from such increased interest will come more recruits for the study of the earth. This book is addressed also to the youth of high school and college age, who, in my opinion, are perfectly capable of reaching sound conclusions on the evidence set forth in it. From many of them, in the course of teaching, I have already received not only an enthusiastic response, but active and practical help.

But this book is necessarily addressed also to the specialists in the various fields with which it deals. These include geology, geophysics, paleontology, and climatology. It is precisely

here that an attitude discovered among the specialists raises a serious problem. There is a natural inclination among them to consider, each one, the evidence falling within his own field of competence, and that evidence alone. Necessarily, if the arguments affecting one field alone are considered, and all the rest are put aside, the weight of probability for the theory is very greatly reduced, and it becomes easy to conclude that, while interesting, it need not be taken very seriously. From this it is but a step to the conclusion that the theory had better be proved first in one of the other fields: it will then be soon enough to invest the necessarily considerable amount of time, effort, and expense in a restudy of the basic data affected by the new theory in the specialist's own field.

So it becomes a question of a scientific passing of the buck: the paleontologist tends to look to the geologist, the geologist to the geophysicist, and the geophysicist to the geologist, for the proof of the theory.

But in the nature of the case, this is a problem for all the sciences of the earth together. Here the specialists must become general readers, and the general reader must take on the responsibility of the scientist. By this I mean that the reader must examine the facts presented here for himself, and draw his own conclusions without looking to any authority except that of his own reason. If the reader will do that—and I now include the specialists—I have no fear of the consequences. Either he will accept the theory presented in this book or he will be inspired to look for a better one.

ACKNOWLEDGMENTS

When it comes time to write an acknowledgment of the assistance received from others in the preparation of a book, this job is sometimes accomplished in a perfunctory way; it is a job to be got over with but, at the same time, turned to advantage. I do not think that this is fair to the essentially social nature of science. The implication is usually obvious that the book is, in fact, the work of one or two perspiring and inspired persons, who, by themselves alone, have persevered against odds to complete an imperishable product. This distorts the process by which scientific and, indeed, all original work is done. Scientific research is essentially and profoundly social. Discoveries are not the product of single great minds illuminating the darkness where ordinary people dwell; rather, the eminent individuals of science have had many predecessors; they themselves have been merely the final organizers of materials prepared by others. The raw materials, the component elements that have made these great achievements possible, have been contributed by hundreds or thousands of people. Every step in the making of this book has been the result of contact with other minds. The work done by hundreds of writers over a number of centuries has been exploited, and the contributions of contemporary writers have been carefully examined. The product represents, I should like to think, a synthesis of thought; at the same time I hope its original elements will prove valid additions to the common stock of knowledge in the field.

Credit for the initiation of the research that led to this book belongs, in the first instance, to students in my classes at Springfield College, in Springfield, Massachusetts. A question asked me by Henry Warrington, a freshman, in 1949, stimulated me to challenge the accepted view that the earth's surface has always been subject only to very gradual change,

and that the poles have always been situated precisely where they are today. As the inquiry grew, many students made valuable contributions to it, in research papers. Among these I may name, in addition to Warrington, William Lammers, Frank Kenison, Robert van Camp, Walter Dobrolet, and William Archer.

Our inquiry first took organized form as an investigation of the ideas of Hugh Auchincloss Brown, and I am deeply indebted to him for his original sensational suggestion that icecaps may have frequently capsized the earth, for many suggestions for research that proved to be productive, for his generosity in sharing all his research data with us, and for his patience in answering innumerable letters.

In this early stage of our inquiry, when I was in every sense an amateur in many fields into which the inquiry led me, I received invaluable assistance from many specialists. These included several members of the faculty of Springfield College, especially Professor Errol Buker, without whose kindly sympathy our inquiry would have been choked in its infancy. Assistance with many serious problems was received from Dr. Harlow Shapley, of the Harvard Observatory, Dr. Dirk Brouwer, of the Yale Observatory, Dr. G. M. Clemence, of the Naval Observatory, and a number of distinguished specialists of the United States Coast and Geodetic Survey.

Our inquiry, in its third year, was involved in a difficulty that appeared to be insuperable, and from this dilemma it was rescued by an inspired suggestion made by my old friend James Hunter Campbell, who thereafter became my constant associate in the research project, and my collaborator. I must give credit to him for having taken hold of a project that was still an amateur inquiry, and transformed it into a solid scientific project.

When Mr. Campbell had developed his ideas far enough to assure us that the idea we had in mind was essentially sound, it became feasible to submit the results of our joint efforts to Albert Einstein, and we found him, from then on, a most sympathetic and helpful friend. Throughout an extended

correspondence, and in personal conference, his observations either corroborated our findings or pointed out problems that we should attempt to solve. With regard to our inquiry, Einstein made an exception to his usual policy, which was to give his reactions to new ideas submitted to him, but not to offer his suggestions for their further development. In our case, with an uncanny sense, he put his finger directly upon problems that were, or were to be, most baffling to us. We had the feeling that he deeply understood what we were trying to do, and desired to help us. Our association with him represented an experience of the spirit as well as of the mind.

In the later stages of our inquiry, many distinguished specialists and friends helped us with particular problems. Helpful suggestions have been contributed by Professor Frank C. Hibben, of the University of New Mexico, Professor Bridgman, of Harvard, Dr. John M. Frankland, of the Bureau of Standards, the late Dr. George Sarton, Professors Walter Bucher and Marshall Kay of Columbia University, Dr. John Scott, Mrs. Mary G. Grand, Mr. Walter Breen, Mr. Stanley Rowe, Dr. Leo Roberts, Mr. Ralph Barton Perry, Jr., Mrs. Mary Heaton Vorse, Mr. Heaton Vorse, Mr. Chauncey Hackett, Mrs. Helen Bishop, and Mrs. A. Hyatt Verrill. To Dr. Harold Anthony, of the American Museum of Natural History, our debt is enormous. It was he who afforded Mr. Campbell and me our first opportunity of discussing our theories with a group of specialists in the earth sciences, when he invited us to talk to the Discussion Group of the Museum. In addition, Dr. Anthony has helpfully criticized parts of the manuscript, and has helped me to get criticism from other experts. Captain Charles Mayo, of Provincetown, Massachusetts, in many long discussions over the years, has contributed innumerable valuable suggestions.

One far-sighted scientist without whose generous help this book in its present form would have been impossible is Dr. David B. Ericson, of the Lamont Geological Observatory. He has contributed many vitally important bibliographical suggestions, has corrected numerous technical errors, and has

provided needed moral support. I am equally indebted to Professor Barry Commoner, of Washington University, who not only read the manuscript to suggest improvements of content and style, but also helped me in the preparation of special articles for publication in the technical journals. Mr. Norman A. Jacobs, editor of the *Yale Scientific Magazine,* published the first of these articles.

During the last year I have received enormous assistance from Mr. Ivan T. Sanderson, who, as a biologist, has read the manuscript with a critical eye for misuse of technical vocabulary and for weaknesses in presentation. I have received invaluable help from Professor J. C. Brice, of Washington University, who has criticized the whole manuscript from a geological standpoint. I am deeply indebted to my aunt, Mrs. Norman Hapgood, for the first complete translation from the Russian of the report of the Imperial Academy of Sciences on the stomach contents of the Beresovka Mammoth, to Mrs. Ilse Politzer for the translation from the German of Einstein's letter of May 3, 1953, and to Mrs. Maely Dufty for assistance with the translation of his Foreword into English. To many personal friends, in addition to those mentioned, I owe thanks for encouragement and for suggestions that often turned out to have major importance. I am indebted to John Langley Howard for his assistance with the illustration of this book, to Mr. Coburn Gilman, my editor, for his innumerable constructive suggestions and his understanding spirit, to Mr. Stanley Abrons for his painstaking work in preparing the Glossary, and to Mr. Walter Breen for preparing the Index.

In the final typing of the manuscript Miss Eileen Sullivan has had to encounter and survive difficulties and frustrations that only she and I can have an idea of. I am very gateful for her help.

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Charles H. Hapgood

Keene Teachers College,
October, 1957.

INTRODUCTION: *A New Theory*

1. Some Unsolved Problems

A few years ago a great scientist, Daly of Harvard, remarked that geologists seem to know less about the earth than they thought they knew when he was a young man (100). This was an extraordinary statement, considering the very detailed studies that have been carried out in innumerable geological fields during his lifetime. Thousands of scientists, in all the countries of the earth, have studied the stratified rocks and the records of life contained in them; they have studied the structures of mountains and reconstructed their histories; they have studied the dynamic forces at work in the earth, and have extended our insights to an understanding of the features of the ocean bottoms and the deeper structures within the earth's crust.

Yet, despite this vast expansion of our detailed knowledge, many of the essential facts of the earth's development have escaped us. The late Hans Cloos, in his *Conversation with the Earth,* said, ". . . we know only the unimportant things and the details. Of the great slow strides of the earth's gigantic history we comprehend hardly anything at all" (85:84).

To begin with, the origin of the earth is itself still a matter of dispute. Until about thirty years ago it was a generally accepted theory that it originally condensed out of a hot gas, and that it has been cooling and contracting ever since. This was the "nebular theory." In recent decades difficulties have piled up in connection with this assumption, and at the present time an entirely opposite view is held by many geophysi-

NOTE: Figures referring to specific sources listed in the Bibliography (p. 396) are inserted in parentheses throughout the text. The first number indicates the correspondingly numbered work in the Bibliography, and the number following a colon indicates the page.

cists. The new idea is that the earth may have started as a small, cold planetesimal. It may have grown simply by attracting to itself many smaller particles, such as meteorites and meteoritic dust. It may have grown hot as a result of the internal pressures caused by its increasing mass, and because of the effects of the radioactivity of many bits of the matter it picked up on its endless journey through interstellar space.

Even a cursory glance at the current literature on this subject reveals the formidable character of the challenge it presents to the old theory and indeed to the whole structure of geological theory based upon it. Dr. Harold C. Urey reaches the conclusion, from impressive evidence, that the earth must have been formed at temperatures below the melting points of silicate rocks (437:112). He quotes the opinion of Bowen that the earth was formed as a solid (438:110). Gutenberg refers to the work of several geophysicists who have advanced similar views (194:191–92). Olivier argued, in 1924, that meteoric phenomena can be understood only in terms of a growing earth. He remarked, "The planetesimal hypothesis is the one to which we are logically led when we attempt to explain meteoric phenomena" (337:272). Coleman pointed to evidence that some of the ice ages in remote geological periods seem to have been colder than those of the more recent past (87:102). Slichter, summarizing the results of a conference of chemists, geologists, and geophysicists devoted to this subject, said,

> . . . In accordance with recent theories, the earth probably has grown by the accretion of relatively cool materials which were not molten at the outset. The chemists strongly favored the cool type of origin. . . . Our conceptions of the development of the primitive earth are, to say the least, obscure. It is even uncertain whether the earth today is cooling or heating at depth, but the odds seem to favor the hypothesis of a heating earth (395:511–12).

Inasmuch as it seems evident that neither view of the origin of the earth has been established, the layman is forced to conclude that the problem of the origin of the earth is unsolved.

More than twenty-five years ago, the geologist William H. Hobbs pointed out the consequences of the breakdown of the nebular theory, so far as geology was concerned:

> Far more than is generally supposed, the recent abandonment of the nebular hypothesis to account for the origin of the universe, must carry with it a rewriting of our science. This is particularly true of geology, for all that concerns seismology, volcanology, and the whole subject of the growth of continents and mountains (215:vii–viii).

But not only has there been no rewriting: actually, the very abandonment of the nebular hypothesis has not yet penetrated to the consciousness of the public. It is even true that many geologists, when they are addressing their remarks to the general public, write as if the cooling of the earth from an original molten state had never been questioned.

Within the frame of reference of this uncertainty regarding the earth's beginning, most geologists today unhesitatingly confess that we do not understand the origin of continents, ocean basins, mountain chains, or the causes of volcanic action. We have never solved the mystery of ice ages in the tropics, nor the equally strange mystery of the growth of corals and warm-climate flora in the polar zones. There is a dispute as to whether the present climatic zones have existed continuously from the earth's beginning. If so, we cannot account at all for the greater part of the fossils of plants and animals of the past that did not live within the limits of the present zones. If the zones have not continuously existed, no one has been able to show what factor can have operated to even out temperatures from pole to pole. When we turn to the theory of evolution, we find that the unsolved problems of origin, development, and extinction of species are many and basic. Everybody agrees that evolution has occurred, but nobody pretends to know how it happened. Our ideas of the tempo at which geological change has occurred in the past have been challenged in the most dramatic fashion by new evidence produced by techniques of dating based on radioactive isotopes. These new techniques have served to underline and emphasize the bankruptcy of the present theory of

the earth. They have, indeed, created many more problems than they have solved.

It became obvious to me, as I reviewed these problems, and went back over the controversies that had marked their consideration, that a sort of common denominator was present. I examined the original sources, and here I noticed that in the controversies that have raged among geologists over these separate questions in the last seventy-five years, somebody usually tried to explain the particular problem in terms of changes in the position of the poles. This, I found, was the common denominator. The authors of such theories, unfortunately, were never able to prove their assumptions. The opponents of the notion of polar change always managed to point out fallacies that seemed decisive. At the same time, no one was able to reconcile all the evidence in the different fields with the idea that the poles have always been situated where they are now on the earth's surface.

The theory here presented would solve these problems by supposing changes in the positions of the poles. Campbell has suggested that the changes have occurred *not by reason of changes in the position of the earth's axis, but simply through a sliding of its crust.* There is nothing new about this idea. It has been brought forward repeatedly over the last seventy-five years, and is advocated today by a number of scientists. This book brings together, I hope in comprehensible form, the evidence from many fields that argues for such shifts, evidence in many cases accumulated by others. In addition, it contains a new element. Campbell's concept of the mechanism by which movements of the earth's crust are accounted for is completely new, although elements of it have been contributed by others.

2. *Crust Displacement as a Solution*

To understand what is involved in the idea of a movement, or displacement, of the entire crust of the earth, certain facts

about the earth must be understood. The crust is very thin. Estimates of its thickness range from a minimum of about twenty to a maximum of about forty miles. The crust is made of comparatively rigid, crystalline rock, but it is fractured in many places, and does not have great strength. Immediately under the crust is a layer that is thought to be extremely weak, because it is, presumably, too hot to crystallize. Moreover, it is thought that pressure at that depth renders the rock extremely plastic, so that it will yield easily to pressures. The rock at that depth is supposed to have high viscosity; that is, it is fluid but very stiff, as tar may be. It is known that a viscous material will yield easily to a comparatively slight pressure exerted over a long period of time, even though it may act as a solid when subjected to a sudden pressure, such as an earthquake wave. If a gentle push is exerted horizontally on the earth's crust, to shove it in a given direction, and if the push is maintained steadily for a long time, it is highly probable that the crust will be displaced over this plastic and viscous lower layer. The crust, in this case, will move as a single unit, the whole crust at the same time. This idea has nothing whatever to do with the much discussed theory of drifting continents, according to which the continents drifted separately, in different directions. The objections to the drifting continent theory will be discussed later.

Let us visualize briefly the consequences of a displacement of the whole crustal shell of the earth. First, there will be the changes in latitude. Places on the earth's surface will change their distances from the equator. Some will be shifted nearer the equator, and others farther away. Points on opposite sides of the earth will move in opposite directions. For example, if New York should be moved 2,000 miles south, the Indian Ocean, diametrically opposite, would have to be shifted 2,000 miles north. All points on the earth's surface will not move an equal distance, however. To visualize this, the reader need only take a globe, mounted on its stand, and set it in rotation. He will see that while a point on its equator is moving fast, the points nearest the poles are moving slowly. In a given

time, a point near the equator moves much farther than one near a pole. So, in a displacement of the crust, there is a meridian around the earth that represents the direction of the movement, and points on this circle will be moved farthest. Two points 90 degrees away from this line will represent the "pivot points" of the movement. All other points will be displaced proportionally to their distances from this meridian. Naturally, climatic changes will be more or less proportionate to changes in latitude, and, because areas on opposite sides of the globe will be moving in opposite directions, some areas will be getting colder while others get hotter; some will be undergoing radical changes of climate, some mild changes of climate, and some no changes at all.

Along with the climatic changes, there will be many other consequences of a displacement of the crust. Because of the slight flattening of the earth, there will be stretching and compressional effects to crack and fold the crust, possibly contributing to the formation of mountain ranges. There will be changes in sea level, and many other consequences. In this book the potential consequences will be discussed in detail, and evidence presented to show that such displacements have frequently occurred in the earth's history, and that they provide an acceptable solution to the problems I have mentioned above.[1]

3. *A Possible Cause of Crust Displacement*

Some years ago Mr. Hugh Auchincloss Brown, an engineer, developed a theory that great polar icecaps might shift the poles by capsizing or careening the earth as a whole. He had a simple idea, suggested by his engineering experience. This was the concept of the centrifugal effect that may arise from

[1] To follow the argument presented in this book, the reader will find it helpful to use a globe. A small one will do. A globe is better than flat maps for the purpose of following the many simultaneous changes involved in a displacement of the crust.

the rotation of a body, if the body is not perfectly centered on its axis of rotation. Everyone has seen examples of the operation of centrifugal force. The principle can be demonstrated by the ordinary washing machine. I once put a heavy rug, all rolled up into a compact ball, into a washing machine, and of course when the machine was set in motion all the weight remained on one side of the axle. The rotation produced a very powerful sidewise heave. The centrifugal effect was sufficient to rip the bolts up out of what had been a fine antique floor. Engineers know that the slightest inaccuracy in the centering of a rapidly rotating mass, such as a flywheel, can result in shattering the rotating body.

Brown pointed out that a polar icecap is an enormous body placed on the earth's surface, and not perfectly centered on the axis of rotation. It must therefore create centrifugal effects, tending to unbalance the earth. He called attention to certain facts about Antarctica. Antarctica is a large continent, about twice the size of the United States. It is almost entirely covered by ice, and the ice is enormously thick. Antarctica contains many great mountain chains, some of them comparable to the Alps or the Rocky Mountains, but the ice is so thick that it reaches the tops of most of them, and sweeps over them. The ice sheet is thought to average a mile in thickness, and it may be twice as thick in places. It may contain as much as 6,000,000 cubic miles of ice. Much of this ice is an extra weight on the earth's crust because it has accumulated so fast that there has been insufficient time for the earth's crust to sink and adapt to it. As we shall see, Brown's surmise that the Antarctic icecap has developed rapidly, and is growing even now (rather than retreating), is well supported by much recent evidence.

With respect to the eccentricity of this mass, Brown pointed out that the earth is known to wobble slightly on its axis. The wobble amounts to about fifty feet, and the earth completes one wobble in about fourteen months. This means that the whole planet, including the icecap, is always off center by about that amount. Brown thought that this slight

eccentricity would, because of the enormous mass of the icecap, produce a great centrifugal effect tending to unbalance the globe. He made some mathematical calculations to show the possible magnitude of the effect. He suggested that, at some point, the icecap would grow so large that the centrifugal effect would suffice to shatter the crust in the earth's equatorial bulge, and permit the earth to wobble farther off center. Then the increasing radius of eccentricity would cause an increase of the centrifugal effect by arithmetical progression, until the earth capsized. He likened the earth's equatorial bulge—its slightly greater diameter through the equator—to a flywheel, which would be shattered by the centrifugal effect of the icecap.

When I first began to study Brown's ideas, I examined his two basic assumptions with some care. The first was the assumption of the centrifugal effect of bodies rotating off center, and that was sound enough. The second was the assumption that the equatorial bulge acted as a stabilizing flywheel to keep the earth steady on its axis. The investigation of this assumption involved long research. I finally found unequivocal support for Brown's contention in the works of James Clerk Maxwell and obtained further confirmation of it in correspondence with Dr. Harlow Shapley, of the Harvard Observatory, Dr. Dirk Brouwer, of the Yale Observatory, and Dr. Harold Jeffreys, of Cambridge University, England.

I now sought to find, if I could, the ratio of the unstabilizing centrifugal effect of the icecap to the stabilizing effect of the bulge. It was clear that the force of the icecap would either have to overcome the total stabilizing centrifugal effect of the bulge, or it would have to shatter the crust, so that the earth could start to rotate farther off center, thereby initiating a chain reaction of increasing centrifugal effects.

The first task was to estimate the centrifugal effect of the icecap. Here I thought that Brown had committed an oversight, to the disadvantage of his own theory. He considered the eccentricity of the icecap to be due to the earth's fifty-foot

wobble. I saw, on looking at the map, what seemed to me a much greater eccentricity. It was obvious that the South Pole was not at all in the center of the continent. This being so, then the icecap, which covers virtually all the continent, could not be centered at the pole. It seemed to me that the

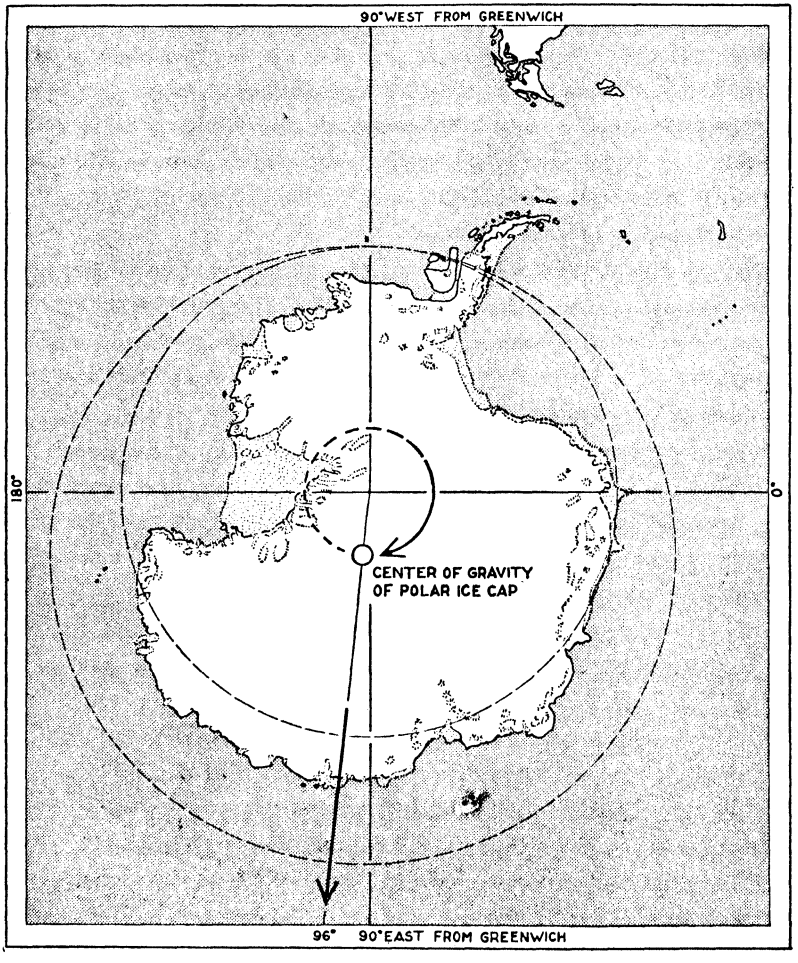

Fig. I. The Centrifugal Effect of the Antarctic Icecap

To visualize the centrifugal effect that may be caused by the Antarctic icecap, the reader should imagine the map of Antarctica actually rotating. The continent of Antarctica makes one complete rotation every

first step must be to locate the geographical center of mass of the Antarctic icecap, and then to apply the standard formula used in mechanics to determine the centrifugal effect. I asked my friend, Errol Buker, of the Springfield College faculty, to locate the geographical center. He and later Mr. Campbell each separately solved the problem, and obtained closely similar results. It appeared that the center was between 300 and 345 miles from the pole, allowing a margin of error for the uncertainties involved in the present state of Antarctic exploration. This, of course, involved a centrifugal effect thousands of times greater than that which could be derived from Brown's assumptions. On this basis Buker calculated the centrifugal effect, and the calculation was later revised by Campbell (Chapter XI). The calculation applied to the present Antarctic icecap only. The ice around the North Pole could be disregarded because, except for the Greenland cap, it is merely a thin shell of floating ice. The presence of the Arctic Ocean prevents any thick accumulation of ice.

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twenty-four hours with the rotation of the earth, and this is what causes the centrifugal effect.

The point at the intersection of the two meridians is the South Pole. This is one end of the axis on which the earth rotates. The small circle drawn about this point is shown passing through an off-center point about five degrees (or 345 miles) from the pole. This point is, so far as we can now estimate, the geographical center of mass of the icecap, which does not coincide with the South Pole because of the asymmetric shape of the continent.

The two larger circles, one drawn about the pole as a center and one drawn about the icecap's eccentrically located center of mass, are a mechanical convention used by engineers to illustrate the centrifugal effects of off-center rotation. If the map is visualized as rotating, the inner circle drawn about the pole represents the earth in stable rotation, while the outer circle, drawn about the center of the icecap, is undergoing violent eccentric gyration. The eccentricity results in an outward centrifugal "throw" in the direction of the meridian of 96° E. Long. The two arrows show how the force of the earth's rotation is transformed into a centrifugal effect at right angles to the earth's axis, an effect proportional to the weight of the ice and the distance of its center of mass from the axis.

The second problem was to measure the stabilizing centrifugal effect of the bulge. Since there was no record of any work having been done previously on this problem, it was necessary to work it all out from fundamentals. It involved difficult physical and mathematical problems. Here I was extremely fortunate in obtaining the generous co-operation of several of the distinguished specialists of the United States Coast and Geodetic Survey. They gave me a calculus with which Mrs. Whittaker Deininger, of the Smith College faculty, obtained a quantity for the stabilizing effect of the bulge.

Now we had two quantities that could be compared with each other: the centrifugal effect of the icecap, tending to upset the earth, and the stabilizing effect of the bulge. Unfortunately for the theory as it then stood, it appeared that the stabilizing effect of the bulge was greater than the eccentric effect of the icecap by several thousand times.

There is no question that this result, had it come earlier, would have brought the investigation to an end. But my geological research had been proceeding actively for more than two years and had produced such impressive evidence that I felt much opposed to the complete abandonment of the project. I discussed the difficulty that had arisen with my friend Campbell. It was indeed fortunate that I did so, for the solution came from him when he suggested that if the icecap did not have sufficient force to careen the whole planet, it might have sufficient force to displace the earth's crust over the underlying layers. As a sequel to this conversation, Mr. Campbell continued to work, for a number of years, on the implications of his suggestion. The details of his mechanism to account for crust displacement are presented in Chapter XI.

The hypothesis that has emerged as the result of this combination of elements is distinguished by its economy of assumptions. It appealed to Albert Einstein because of its simplicity. It appeared to him that it might be possible, on the basis of the simple common denominator of this theory

of displacement, to solve the many complex and interrelated problems of the earth that have so long resisted solution.

The simplicity of the idea may raise the suspicion that it can hardly be so very new. How can anything so extremely simple as the application of the formula for calculating centrifugal effects, a formula which appears in every high-school textbook of physics, to a polar icecap, have been completely overlooked? This thought occurred to me, but I found to my surprise that, despite the simplicity of the idea, it was one that had never been investigated. When I first discussed it with Professor Bridgman, at Harvard, he had the impression that it was a good idea; he called it a real problem, but he said he could not believe that it had never been considered by science. He suggested that I take it up with Professor Daly. I did so, and Professor Daly agreed that it was a real problem, but assured me that it had never, to his knowledge, been investigated. And so it turned out. I have looked pretty far through the technical literature and have found no studies covering it. Dr. George Sarton, the historian of science, confirmed this finding when he wrote me that "the combination of ideas is so new that the history of science has nothing to contribute to its understanding" (p. 391).

This book has been written with three objectives in mind. I have sought, in the first place, to establish beyond a reasonable doubt that numerous displacements of the earth's crust have occurred. I think that this idea may now be accepted without too much difficulty, especially in view of much recent work in the field of terrestrial magnetism. Secondly, I have tried to describe a mechanism to account for displacements (this is essentially the work of Mr. Campbell) and to present evidence showing that this mechanism alone can account for the facts. My third purpose has been to show that the hypothesis of crust displacement provides an acceptable solution of many of the problems of the earth.

It is quite natural that at first numerous objections should be raised to this theory. In our correspondence with specialists the principal issues that have come up to raise doubts

include the following: whether we have properly estimated the magnitude of the centrifugal effect; whether there is any layer below the crust weak enough to permit crust displacement; whether the Antarctic icecap is really growing, as the theory requires, or is in retreat; whether the centrifugal effect we postulate would not in practice merely cause the icecap to flow off from the Antarctic continent into the sea, rather than transmit its push to the crust; whether the thrust of the icecap, if it was transmitted to the crust, would be transmitted to the crust as a whole, as the theory requires, or would be absorbed in local readjustments of the crust; whether, if both the poles happened to fall in water areas, icecaps would not cease to develop, and thus the whole process of crust displacement be brought to an end; why, if crust displacements have been frequent in geological history, there are not evidences of more icecaps in the geological record; why, with that assumption, we find some rock formations that appear to have been undisturbed since the earliest times. All these objections, and many more, are fully, and I hope fairly, discussed in the following chapters. Therefore, if the reader finds himself asking questions that do not appear to be answered, I hope he will have patience. He may find that they are answered in later parts of the book.

TABLE I

The Geological Periods
(*After Krumbein and Sloss, 258:15*)

ERA	PERIOD	EPOCH
Cenozoic	Quaternary	Present
		Pleistocene
	Tertiary	Pliocene
		Miocene
		Oligocene
		Eocene
		(Paleocene)
Mesozoic	Cretaceous	
	Jurassic	
	(Liassic)	
	Triassic	
	(Thaetic)	
Paleozoic	Permian	
	Pennsylvanian	
	(Carboniferous)	
	Mississippian	
	Devonian	
	Silurian	
	Ordovician	
	Cambrian	
Pre-Cambrian	Keweenawan	
	Huronian	
	Tamiskamian	
	Kewatinian	

I : PAST THEORIES OF POLAR SHIFT

1. Older Theories

Of all the questions that have been debated in the sciences of the earth, perhaps the most fundamental and the most involved is that of the stability of the poles. This question has bedevilled science for about a hundred years. Despite every effort to establish the view that the poles have shifted during the history of the earth, or to prove that they have not, the controversy is just as lively today as ever. In fact, discussion of the issue has become much more active during the last decade. The new evidence bearing on this question, as we shall see, now strongly favors the idea of polar shift.

When the term "polar shift" is used, it may have several meanings. It may mean a change of the position of the earth's axis, with reference to the stars. Everyone has seen pictures of the solar system, with the earth, planets, and sun shown in relationship to one another. The earth is always shown slightly tipped. Its axis does not run straight up and down at right angles to the plane of the sun's equator, but slants at an angle.

Now, there is no doubt but that any change in the position of this axis would be very important to us. It might mean, for example, that the South Pole would point directly at the sun. We would then have one hot pole and one cold pole. The hot pole would never have any night, and the cold pole would never have any day. The occurrence of this kind of polar shift has seldom been supposed, for the reason that no force capable of shifting the axis has ever been imagined, other than, possibly, a major interplanetary collision.

A second cause of the shifting of the poles with reference to points on the earth's surface would be a change in the position of the whole planet on its axis, without change of the

position of the axis. The axis would point in the same direction—toward the same stars—but by a careening motion of the planet other points would be brought to the poles. Not the axis, but the whole planet, would have moved or swivelled around. This is the sort of change proposed by Brown.

As I have already mentioned, the principal obstacle to a shift of the earth on its axis lies in the existence of the earth's equatorial bulge, which acts like the stabilizing rim of a gyroscope. The early writers on this question, such as Maxwell (296) and George H. Darwin (105), all recognized that a shifting of the planet on its axis to any great extent would require a force sufficient to overcome the stabilizing effect of the bulge. But they were unable to see what could give rise to such a force, and dismissed the idea of a shift of the planet on its axis as utterly impossible and, in fact, not worth discussing.

This, however, left the evidence unaccounted for, and such evidence, from many sources, continued to accumulate. Fortified by their very strong conviction that a shift of the planet on its axis was impossible, astronomers and geologists insisted that all this evidence, such as fossil corals from the Arctic Ocean, coal beds and fossil water lilies from Spitzbergen, and many other evidences of warm climates in the vicinity of both the poles, simply must be interpreted in accordance with the assumption that the poles had never changed their positions on the face of the earth. This placed quite a strain upon generations of geologists, but their imaginations were usually equal to the task. They were fertile in inventing theories to account for warm climates in the polar zones at the required times, but these theories were never based on substantial evidence. Moreover, they never explained more than a small number of the facts, while essentially they conflicted with common sense. We shall have occasion to return to them again in later chapters, where the statements I have just made will be fully documented.

The discontent of the biologists and paleontologists, who were constantly finding fossil fauna and flora in the wrong

places, finally boiled over, and resulted in a number of new theories for polar change. New proposals were frequently advanced in the 1880's and 1890's and later, but they were met by the unyielding resistance of the highest authorities, basing themselves on the positions taken by the persons already mentioned. Moreover, it was easy to show defects and contradictions in these various theories, and to discredit them, one after another. All the assaults were successfully beaten back, except one.

2. *The Wegener Theory*

The exception proved to be the theory of Alfred von Wegener. The latter was a good scientist, though not a geologist. He was unwilling to be satisfied with theories that would account for only a few of the facts. He had a passion for broad, inclusive principles supported by tangible evidence. He found quantities of evidence that could not, in his opinion, be reconciled with the present positions of the poles. Inasmuch as the doctrine of polar permanence (and it was a doctrine any challenge to which evoked remarkable fury from recognized authorities) forbade any thought that the poles themselves had moved, or that the earth had shifted on its axis, Wegener suggested that the continents had moved. This would have precisely the same effect, for it would mean that, at different times, different areas would be found at the poles. And this was, in effect, a third way to account for shiftings of the geographical locations of the poles.

Wegener imagined that the continents, formed of light granitic and sedimentary rocks, had once composed a single land mass, but had been split and set in motion, drifting over a plastic substratum of the continents and oceans. He thought of this sublayer as really plastic and viscous, rather than rigid and strong. From a vast amount of fossil evidence of the plant and animal life of the past, he imagined that he could reconstruct the actual paths of the continents over

long periods of time. He proposed to explain the ice ages by this theory; he suggested that during the last ice age in the Northern Hemisphere, Europe and America had lain close together near the pole but that, since then, they had drifted apart.

Wegener's theory had great appeal. This was not because all of the evidence supported it, nor because its mechanics were very plausible, but because it was the only theory that, at the time, could make sense of the evidence of the fossil flora and fauna.

There were a number of weaknesses in the structure of this theory. One of these was that the evidence from different areas, for the same geological period, would not produce agreement as to where the poles were situated at a given time. Chaney, for example, wrote, "It is amusing to note . . . that in taking care of their Tertiary forests, certain Europeans have condemned ours to freezing. . . ." (72:484).

Wegener recognized the seriousness of this difficulty:

> Although the grounds for the shifting of the poles (in certain periods of the earth's history) are so compelling, nevertheless it cannot be denied that all previous attempts to fix the positions of the poles continuously throughout the whole geological succession have always led to self-contradiction, and indeed to contradiction of so grotesque a kind that it is not to be wondered at that the suspicion arises that the assumption of the shifting of the poles is built on a fallacy (450:94–95).

This difficulty, basic as it was, was by no means the worst. By various methods the knowledge of the structure of the earth's crust was extended, and it was finally found that the rock under the oceans, which Wegener had thought to be plastic enough for the continents to drift over it, is in fact very rigid. This means that the continents cannot drift without displacing a layer of rigid rock under the oceans, a layer thought to be at least twenty miles thick and comparatively strong. It is therefore impossible for the continents to drift. Dr. Harold Jeffreys, the noted geophysicist, basing his opinion on the evidence for a rigid and comparatively strong

ocean floor, said, ". . . There is therefore not the slightest reason to believe that bodily displacements of continents through the lithosphere are possible" (238:304; 239:346). The lithosphere, of course, is the crust. The geophysicist F. A. Vening Meinesz, according to Umbgrove, conclusively proved the considerable strength of the crust under the Pacific (430:70).

One of the arguments most frequently heard in favor of the Wegener theory is based on the apparent correspondence in shape between certain continents. It would seem, for example, that South America might be fitted together with Africa, and so on. It is claimed that this is evidence that the two were once parts of one land mass, which must have broken in two. It is even claimed that rock formations on opposite sides of the Atlantic match. However, some years ago, K. E. Caster and J. C. Mendes, two geologists who desired to prove this theory, spent a vast amount of time in South America, and travelled about 25,000 miles carrying on field investigations in order to compare in detail the rock formations of South America with those of Africa. Their conclusion was that the rock formations did not prove the theory. Neither, however, did the evidence they had found disprove it. They added, "Only time and more facts can settle the issue" (69:1173). Professor Walter Bucher, former President of the Geological Society of America, also answered this particular point. He published a map showing the United States as it would look if flooded up to 1,000 feet above the present sea level. The map shows that the eastern and western sides of the resulting inland sea correspond (57:459). Thus, if the sea were there now, it would look as if the two parts of North America had drifted apart. An alternative explanation of such parallel or corresponding features will be suggested in a later chapter.

Another objection to the Wegener theory is that it assumes that the sea bottoms are smooth plains. This assumption is necessary for the theory, for otherwise the continents could not drift over the ocean basins. As the result of the oceano-

graphic work of recent years, it has been discovered, in contradiction to this, that there are mountain ranges on the bottoms of all the oceans, and that some of these ranges are comparable in size to the greatest mountain ranges on land. Furthermore, several hundred volcanic mountains have been discovered spread singly over the ocean floors, many of them apparently of great age.

The Wegener theory involved the corollary that, as the continents had drifted very slowly across the smooth ocean floors, these floors had accumulated sediment to great thicknesses. It was thought that this sediment should provide an unbroken record for the whole period of geological time since the formation of the oceans. The greatest surprise of recent oceanographic exploration, however, has been the discovery that this supposed layer of sediment is nonexistent. The layer of sediment on the ocean bottom is uneven, in some places only a few feet or a few inches thick, and is rarely of great thickness. The matter of submarine sediments will be discussed more fully in later chapters.

Another startling contradiction to the Wegener theory is presented by recent data that have drastically changed our former ideas regarding the date of the last ice age in North America. We have learned, through the new technique of radiocarbon dating, that this ice age ended only 10,000 years ago. In Wegener's time it was considered by geologists that the ice age came to an end at least 30,000 years ago. Since Wegener supposed that Europe and North America had been situated close together and not far from the pole during the ice age, the new data have the effect of requiring an incredible rate of continental drift. Three thousand miles of drift in 10,000 years would amount to about 1,500 feet a year. Furthermore, movement at something like this rate must still be going on, for the momentum of a continent in motion would be tremendous. And what would be the consequence of a continuing movement at this rate? It would mean that oceanic charts would have to be revised every few years, and that shipping companies would have frequently to augment

their fares, because of the ever-increasing distance between America and Europe.

To cap the case, Gutenberg has shown that the various forces that Wegener depended upon to move the continents are either nonexistent or insufficient (194:209), while another geophysicist, Lambert, has stated that they amount to only one millionth of what would be required (64:162).

It is interesting to note that despite the quite overwhelming character of these objections, attempts are still made to rehabilitate or rescue the Wegener theory. Daly attempted, some years ago, to find a better source of energy for moving the continents (98); Hansen cleverly suggested that the centrifugal effects of icecaps might have moved the continents (199). A contemporary Soviet plant geographer, while recognizing the objections, nevertheless remarked of the Wegener theory that "it, nevertheless, constitutes the only plausible working hypothesis upon which the historical plant geographer may base his conclusions" (463). As recently as 1950 the British Association for the Advancement of Science divided about equally, by vote, for and against the Wegener theory (351).

This continuing interest in a theory that contains so many and such serious difficulties is eloquent confirmation of the insistent pressure of the evidence in favor of polar shifts. It seems clear that the only reason for the continuing reluctance to accept polar shifts is the absence of an acceptable mechanism to account for them. The Wegener theory, despite its appeal, was never generally accepted by scientists, who have remained, as a body, until very recently, opposed to any suggestion of polar shifts.

We must briefly consider the results of this *impasse*. The failure, over a long period of time, of successive proposals to account for polar change made it impossible for scientists to accept the field evidence, and to evaluate it on its merits. With no acceptable theory to account for changes in the positions of the poles, it was natural that such changes should be looked upon as impossible. With each successive failure

of a proposed theory, the reigning doctrine of the fixity of the poles was reinforced. As time passed this doctrine became deeply ingrained, so that all one needed to do to be labelled a crank was to suggest the possibility of polar changes.

There have been two principal consequences of this enthronement of doctrine. In the first place, the evidence amassed by those who had been led to attack it was quietly put aside. A part of the evidence was ingeniously explained away; most of it was simply ignored. The volumes containing it slept on the back shelves, or even in the storage rooms, of the libraries, gathering dust. For several years now I have been busy taking out and dusting off these old books, dragging the skeletons from the closets, and finding much authentic and incontrovertible evidence that changes of the geographical locations of the poles have occurred at comparatively short intervals during at least the greater part of the history of the earth.

The other consequence of the reigning dogma was the invention of theories to explain those facts that did not fit and could not be ignored. One such theory, already alluded to, was that climates were once virtually uniform from pole to pole; that there were mild, moist conditions enabling water lilies and magnolias to bloom in the long night under the Pole Star. No way of accounting for this was ever supported by a halfway reasonable display of evidence. Nevertheless, such was the magic of the dogma of the fixity of the poles that it was accepted, and is still accepted, by a considerable section of the scientific world. The sum total of the contradictions in this theory, and in the various theories advanced to explain ice ages, mountain formation, the history of continents and ocean basins, or evolutionary theory, will appear, as we proceed, to be essentially the result of the *impasse* between the evidence and the doctrine of the fixity of the poles. The necessity of reconciling the constantly accumulating facts in a number of fields with a basic error has produced a multiplicity of theories which are, in fact, a veritable cloud castle of conjectures, without substance.

3. New Proposals of Polar Shift

Since truth cannot be suppressed forever, it was inevitable that accumulating facts should eventually bring the polar issue again into the foreground. Gutenberg suggested that while continents cannot drift, perhaps they can creep (194: 211). The British astronomer Gold postulated that the earth's wobble on its axis could cause a plastic readjustment of its mantle sufficient to move the poles 90 degrees in a million years (176). The French geographer Jacques Blanchard suggested the possibility of extensive polar changes due to more pronounced wobbling of the earth in the past (38). Ting Ying H. Ma, of Formosa, raised the idea of a combination of continental drift with displacement of the outer shells of the earth (285–290). Bain thought of displacements of the crust to account for facts of ancient plant geography and fossil soils and suggested a mechanism to try to account for them (18). Pauly (342) revived the suggestion made by Eddington (124) that the earth's crust may have been displaced by the effects of tidal friction. Kelly and Dachille, in a provocative work on collision geology entitled *Target Earth,* offered the hypothesis of displacements of the earth's crust as the result of collisions with planetoids (248).

The most important recent contribution to the controversy has certainly been the evidence produced by geophysicists investigating terrestrial magnetism. This new evidence is so impressive that it has brought about a reversal of opinion in high geological quarters on the question of the permanence of the poles. One of the leading specialists in this field, Dr. J. W. Graham, has recently remarked:

> . . . Within the past couple of years there have appeared a number of serious papers dealing with the subject of polar wanderings by which is meant a shift of the geographic features of the earth's surface with respect to the axis of spin. Classical geophysical treatments of the type pioneered by Sir George H. Darwin early in this century have been re-examined in the light of our more recent knowledge of

the earth and its properties, and the conclusion is reached that, whereas polar wandering was formerly considered impossible, it now seems to some, at least, inevitable. These re-examinations were inspired by deductions based on the rock magnetism studies of the past few years (428:86).

In 1954 the results of one of these studies were made public by the British scientists Clegg, Almond, and Stubbs. They found impressive evidence of changed directions of the earth's magnetic field in past periods and concluded:

Finally, it seems therefore that the most likely explanation of the observed horizontal direction of magnetization of the sediments studied is that the whole land mass which now constitutes England has rotated clockwise through 34° relative to the earth's geographical axis. . . .

If such a rotation of England occurred, it could have been a local movement of a part only of the earth's crust, or alternately, the earth's mantle could have moved as a rigid whole relative to the geographical poles. The first hypothesis would consider the rotation either as a purely local movement or as part of a drift of large continental land masses. The second would adduce pole wandering as the operative mechanism. . . . (81:596).

Many speculations regarding polar changes are being put forward at the present time without suggesting any mechanism. Thus, in recent months Soviet scientists writing for the newspaper *Red Star* had the North Pole situated at 55° N. Lat. 60,000,000 years ago, and in the Pacific to the southwest of Southern California 300,000,000 years ago, while in this country Munk and Revelle suggested that the South Pole was once over Africa (315).

Needless to say, none of these concepts has been brought forward without evidence. The evidence is converging from many directions, with an effect of the confluence of many rivers into one mighty torrent. The summary of the evidence is the business of the following chapters.

II : THE ICE AGES

The evidence for displacements of the earth's crust is, as I have said, scattered over many parts of the earth, and comes from several fields of science. No other field, however, furnishes so dramatic a confirmation of it as glacial geology. Much new evidence has recently become available to supplement the older data relating to ice ages.

1. The Failure of the Older Theories

A little more than a hundred years ago people were astonished at the suggestion that great ice sheets, as much as a mile thick, had once lain over the temperate lands of North America and Europe. Many ridiculed the idea, as happens with new ideas in every age, and sought to discredit the evidence produced in favor of it. Eventually the facts were established regarding an ice age in Europe and in North America. People later accepted the idea of not one but a series of ice ages. As time went on evidences were found of ice ages on all the continents, even in the tropics. It was found that ice sheets had once covered vast areas of tropical India and equatorial Africa.

From the beginning, geologists devoted much attention to the possible cause of such great changes in the climate. One theory after another was proposed, but, as the information available gradually increased, each theory in turn was found to be in conflict with the facts, and as a consequence had to be discarded. In 1929, Coleman, one of the leading authorities on the ice ages, wrote:

> Scores of methods of accounting for ice ages have been proposed, and probably no other geological problem has been so seriously discussed, not only by glaciologists, but by meteorologists and biologists;

yet no theory is generally accepted. The opinions of those who have written on the subject are hopelessly in contradiction with one another, and good authorities are arrayed on opposite sides. . . . (87:246).

Recent writers, such as Daly (98:257), Umbgrove (429:285), and Gutenberg (194:205), agree that the situation described by Coleman is essentially unchanged. In January, 1953, Professor J. K. Charlesworth, of Queen's University, Belfast, expressed the opinion that

> The cause of all these changes, one of the greatest riddles in geological history, remains unsolved; despite the endeavors of generations of astronomers, biologists, geologists, meteorologists and physicists, it still eludes us (75:3).

A volume on climatic change, edited by Dr. Harlow Shapley (375), while introducing minor refinements in various theories, in no way modifies the general effect, which is that down to the present time the theorizing about the causes of ice ages has led nowhere.

2. *The Misplaced Icecaps*

One problem that writers on the ice ages have attempted to solve, sometimes in rather fantastic ways, but without success, is that of the wrong location of the great icecaps of the past. These icecaps have refused to have anything to do with the polar areas of the present day, except in a quite incidental fashion.

Originally it was thought that in glacial periods the icecaps would fan out from the poles, but then it appeared that none of them did so, except the ones that have existed in Antarctica. Coleman drew attention to the essential facts, as follows:

> In early times it was supposed that during the glacial period a vast ice cap radiated from the North Pole, extending varying distances southward over seas and continents. It was presently found, however, that some northern countries were never covered by ice, and that in

reality there were several more or less distinct ice sheets starting from local centers, and expanding in all directions, north as well as east and west and south. It was found, too, that these ice sheets were distributed in what seemed a capricious manner. Siberia, now including some of the coldest parts of the world, was not covered, and the same was true of most of Alaska, and the Yukon Territory in Canada; while northern Europe, with its relatively mild climate, was buried under ice as far south as London and Berlin; and most of Canada and the United States were covered, the ice reaching as far south as Cincinnati in the Mississippi Valley (87:7–9).

With regard to an earlier age (the Permo-Carboniferous), Coleman emphasized that the locations of the icecaps were even further out of line:

Unless the continents have shifted their positions since that time, the Permo-Carboniferous glaciation occurred chiefly in what is now the southern temperate zone, and did not reach the arctic regions at all (87:90).

He is much upset by the fact that this ice age apparently did not affect Europe:

Unless European geologists have overlooked evidence of glaciation at the end of the Carboniferous or at the beginning of the Permian, the continent escaped the worst of the glaciation that had such overwhelming effects on other parts of the world. A reason for this exemption is not easily found (87:96).

One of the most extraordinary cases is that of the great ice sheet that covered most of India in this period. Geologists are able to tell from a careful study of the glacial evidences in what direction an ice sheet moved, and in this case the ice sheet moved northward from an ice center in southern India for a distance of 1,100 miles. Coleman comments on this as follows:

Now, an ice sheet on level ground, as it seems to have been in India, must necessarily extend in all directions, since it is not the slope of the surface it rests on that sets it in motion, but the thickness of the ice towards the central parts. . . .

The Indian ice sheet should push southward as well as northward. Did it really push as far to the south of Lat. 17° as to the north? It

extended 1100 miles to the Salt Range in the north. If it extended the same distance to the south it would reach the equator (87:110–11).

The great South African geologist A. L. du Toit pointed out that the icecaps of all geological periods in the Southern Hemisphere were eccentric as regards the South Pole, just as the Pleistocene icecaps were eccentric with regard to the North Pole (87:262). Isn't it extraordinary that the Antarctic icecap, which we can actually see because it now exists, is the only one of all these icecaps that is found in the polar zone, where it ought to be?

Dr. George W. Bain, a contemporary writer to whom I shall refer again, has pointed out a very interesting feature of the great icecap that existed in the Permo-Carboniferous Period right in the center of tropical Africa—in the Congo. He has observed that the icecap, apparently, was asymmetric in shape: it spread from its center of origin much farther in one direction than in another (18:46). This is reconcilable with our theory, which depends upon the asymmetry of icecaps. It seems that this African ice sheet reached the present equator.

Coleman, who did a great deal of field work in Africa and India, studying the evidences of the ice ages there, writes interestingly of his experiences in finding the signs of intense cold in areas where he had to toil in the blazing heat of the tropical sun:

> On a hot evening in early winter two and a half degrees within the torrid zone amid tropical surroundings it was very hard to imagine the region as covered for thousands of years with thousands of feet of ice. The contrast of the present with the past was astounding, and it was easy to see why some of the early geologists fought so long against the idea of glaciation in India at the end of the Carboniferous (87:108).

> Some hours of scrambling and hammering under the intense African sun, in lat. 27° 5′, without a drop of water, while collecting striated stones and a slab of polished floor of slate, provided a most impressive contrast between the present and the past, for though August 27th is still early Spring, the heat is fully equal to that of a

> sunny August day in North America. The dry, wilting glare and perspiration made the thought of an ice sheet thousands of feet thick at that very spot most incredible, but most alluring (87:124).

When these facts were established, geologists sought to explain them by assuming that, at periods when these areas were glaciated, they were elevated much higher above sea level than they are now. Theoretically, even an area near the equator, if elevated several miles above sea level, would be cold enough for an ice sheet. What made the theory plausible was the well-known fact that the elevations of all the lands of the globe have changed repeatedly and drastically during the course of geological history. Unfortunately for those who tried to explain the misplaced icecaps in this way, however, Coleman showed that they reached sea level, within the tropics, on three continents: Asia, Africa, and Australia (87:129, 134, 140, 168, 183). At the same time, W. J. Humphreys, in his examination of the meteorological factors of glaciation, made the point that high elevation means less moisture in the air, as well as lowered temperature, and is therefore unfavorable for the accumulation of great icecaps (232:612–13).

3. World-wide Phases of Cold Weather

A widely accepted assumption with which contemporary geologists approach the question of ice ages is that the latter occurred as the result of a lowering of the average temperature of the whole surface of the earth at the same time. This assumption has forced them to look for a cause of glacial periods only in such possible factors as could operate to cool the whole surface of the earth at once. It has also compelled them to maintain the view that glacial periods have always been simultaneous in the Northern and Southern Hemispheres.

It is remarkable that this assumption has been maintained over a long period of time despite the fact that it is in sharp

conflict with basic principles of physics in the field of meteorology. The basic conflict was brought to the attention of science at least seventy years ago; it has never been resolved. It consists essentially of the fact that glacial periods were periods of heavier rainfall in areas outside the regions of the ice sheets, so that this, together with the deep accumulations of ice in the great ice sheets, must have involved a higher average rate of precipitation during ice ages. There is a great deal of geological evidence in support of this. Only recently, for example, Davies has discussed the so-called "pluvial" periods in Africa, and has correlated them with the Pleistocene glacial periods (107).

Now, meteorologists point out that if precipitation is to be increased, there has to be a greater supply of moisture in the air. The only possible way of increasing the amount of moisture in the air is to raise the temperature of the air. It would seem, therefore, that to get an ice age one would have to raise, rather than lower, the average temperature. This essential fact of physics was pointed out as long ago as 1892 by Sir Robert Ball, who quoted an earlier remark by Tyndall:

> . . . Professor Tyndall has remarked that the heat that would be required to evaporate enough water to form a glacier would be sufficient to fuse and transform into glowing molten liquid a stream of cast iron five times as heavy as the glacier itself (20:108).

William Lee Stokes has again called attention to this unsolved problem in his recent article entitled "Another Look at the Ice Age":

> Lowering temperatures and increased precipitation are considered to have existed side by side on a world-wide scale and over a long period in apparent defiance of sound climatological theory. Among the many quotations that could be cited reflecting the need for a more comprehensive explanation of this difficulty the following seems typical.
>
> "In the Arequipa region [of Peru], as in many others in both hemispheres where Pleistocene conditions have been studied, this period appears to have been characterized by increased precipitation as well

as lowered temperatures. If, however, precipitation was then greater over certain areas of the earth's surface than it is at present, a corollary seems to be implied that over other large areas evaporation was greater than normal to supply increased precipitation, and hence in these latter areas the climate was warmer than normal. This seems at first to be an astonishing conclusion. . . . We might propose the hypothesis that climatic conditions were far from steady in any one area, but were subject to large shifts, and that intervals of ameliorated conditions in some regions coincided with increased severity in others. The Pleistocene, then, may have been a period of sharper contrasts of climate and of shifting climates rather than a period of greater cold" (405:815–16).

From a number of points of view, the foregoing passage is extremely remarkable. Stokes recognizes the fact that the basic assumption of contemporary geologists regarding the glacial periods is in conflict with the laws of physics. Then, in the passage he quotes, he draws attention to the implications, which seem to point directly to crust displacement, for in what other way can we explain how one part of the earth's surface was colder and another, at the same time, warmer than at present?

One of the arguments that is advanced in support of the assumption of world-wide periods of colder weather (which remains the generally accepted assumption of glaciologists) has its basis in geological evidence purporting to prove that ice ages occurred simultaneously in both hemispheres. A decade ago, however, Kroeber pointed to the essential weakness of this geological evidence, when he showed the difficulty of correlating stratified deposits of different areas with each other:

. . . There is plenty of geologic evidence, in many parts of the earth, of changes of climates, especially between wet and dry areas; and some of these happened in the Pleistocene. But the correlation of such changes as they occurred in widely separated regions, and especially as between permanently ice-free and glaciated areas, is an intricate, tricky, and highly technical matter, on which the anthropological student must take the word of geologists and climatologists, and these are by no means in agreement. They may be reasonably sure of one series of climatic successions in one region, and of another in a second

or third region; but there may be little direct evidence on the correspondence of the several series of regional stages, the identification of which then remains speculative (257:650).

At the time that Kroeber remarked on the difficulty of correlating climatic changes in different parts of the world, we were not yet in possession of the data recently provided by the new techniques of radiocarbon and ionium dating, which will be discussed below. The effect of the new data has been to shorten very greatly our estimate of the duration of the last North American ice age. This estimate has been reduced, in the last few years, from about 150,000 years to about 25,000 years, or by five sixths. Now, if we adopt the view that ancient glaciations, of which we know little, may reasonably be considered to have been the results of the same causes that brought about the North American ice age, then we must grant that they, too, were of short duration. But if this is true, how is it possible to establish the fact that they were contemporary in the two hemispheres? A geological period has a duration of millions of years. An ice age in Europe and one in Australia might both be, for example, of Eocene age, but the Eocene Epoch is estimated to have lasted about 15,000,000 years. We can discriminate roughly between strata dating from the early, middle, or late Eocene, but we have no way of pinpointing the date of any event in the Eocene. Even with the new techniques of radiodating now being applied to the older rocks, it is possible to determine dates only to within a margin of error of about a million years. How, then, is it possible to determine that an ice sheet in one hemisphere was really contemporary with an ice sheet or an ice age in the other?

The attempt to maintain the assumption of the simultaneousness of glaciations for the older geological periods is manifestly absurd. I shall show in what follows that it is equally absurd for the recent geological time. It is my impression that the material evidence for the assumption was never impressive, and that the assumption was never derived empirically from the evidence but was borrowed a priori from

the parent assumption, that is, the assumption of the lowering of global temperatures during ice ages, which assumption is, as already pointed out, in conflict with the laws of physics.

If it is true that the fundamental assumption underlying most of the theories produced to explain ice ages is in error, we should expect that these theories, despite their many differences, would have a common quality of futility, and so it turns out. It is interesting to list the kinds of hypothetical causes that have been suggested to explain ice ages on the assumption of a world-wide lowering of temperature. They are as follows:

a. Variations in the quantity of particle emission and of the radiant heat given off by the sun.
b. Interception of part of the sun's radiation by clouds of interstellar gas or dust.
c. Variations in the heat of space; that is, the temperature of particles floating in space which, entering the earth's atmosphere, might affect its temperature.
d. Variations in the quantities of dust particles in the atmosphere, from volcanic eruptions or other causes, or variations in proportion of carbon dioxide in the atmosphere.

The objections to these suggestions are all very cogent. So far as the variation of the sun's radiation is concerned, it is known that it varies slightly over short periods, but there is no evidence that it has ever varied enough, or for a long enough time, to cause an ice age. Evidence for the second and third suggestions is entirely lacking. The fourth suggestion is deprived of value because, on the one hand, no causes can be suggested for long-term changes in the number of eruptions or in the atmospheric proportion of carbon dioxide, and, on the other, there is insufficient evidence to show that the changes ever occurred.

I should make one reservation with regard to the fourth suggestion. There is one event that would provide an adequate cause for an increase in the atmosphere of both vol-

canic dust and carbon dioxide, and that is a displacement of the crust. The extremely far-reaching consequences of a displacement of the crust with respect to atmospheric conditions, and the importance of the atmospheric effects of a displacement for other questions, will be discussed in Chapters VII and VIII.

The theories listed above were attacked by Coleman, who complained that they were entirely intangible and unprovable. He said:

> Such vague and accidental causes for climatic change should be appealed to only as a last resort unless positive proof some time becomes available showing that an event of the kind actually took place (87:282).

Another group of theories attempts to explain ice ages as the results of changes in the positions of the earth and the sun. These are of two kinds: changes in the distance between the earth and the sun at particular times because of changes in the shape of the earth's orbit, and changes in the angle of inclination of the earth's axis, which occur regularly as the result of precession. The argument that precession was the cause of ice ages was advanced by Drayson in the last century (117). The argument based on these astronomical changes has been brought up to date in the recent work of Brouwer and Van Woerkom (375:147–58) and Emiliani (132). It now seems that these astronomical changes may produce cyclical changes in the distribution of the sun's heat, and perhaps in the amount of the sun's heat retained by the earth, but it is agreed, by Emiliani and others, that by itself the insolation curve or net temperature difference would not be sufficient to cause an ice age without the operation of other factors, and so Emiliani suggests that perhaps changes in elevation coinciding with the cool phases of the insolation curve may have caused the Pleistocene ice ages. One weakness of this suggestion is, of course, the necessity to suppose two independent causes for ice ages.

There is another objection to be advanced against all

theories supposing a general fall of world temperatures during the ice ages. We have seen that ice ages existed in the tropics and that great icecaps covered vast areas on and near the equator. This happened not once, but several times. The question is, if the temperature of the whole earth fell enough to permit ice sheets a mile thick to develop on the equator, just where did the fauna and flora go for refuge? How did they survive? How did the reef corals, which require a minimum sea-water temperature of 68° F. throughout the year, manage to survive? We know that the reef corals, for example, existed long before the period of the tropical ice sheets. Furthermore, we know that the great forests of the Carboniferous Period, which gave us most of our coal, lived both earlier than and contemporarily with the glaciations of Africa and India, though in different places. Obviously, this would have been impossible if the temperature of the whole earth had been simultaneously reduced, for the equatorial zone itself would have been uninhabitable while all other areas were still colder. It is small wonder that W. B. Wright insisted, over a quarter of a century ago, that the Permo-Carboniferous ice sheets in Africa and India were proof of a shift of the poles (461).

4. The New Evidence of Radiocarbon Dating

The problem of the causes of ice ages has been still further complicated by a recent revolution in our methods of dating geological events. In the course of the last ten years all of our ideas regarding the dating of the recent ice ages, their durations, and the speed of growth and disappearance of the great ice sheets have been transformed. This is altogether the most important new development in the sciences of the earth. The repercussions in many directions are most remarkable.

In order to get an idea of the extent of the change, let us see what the situation was only ten years ago. As everybody is aware, geologists are used to thinking in terms of millions of

years. To a geologist a period of 1,000,000 years has come to mean almost nothing at all. He is actually used to thinking that events that took place somewhere within the same 20,000,000-year period were roughly contemporaneous. As to the ice ages, the older ones were simply thrown into one of these long geological periods, but there was no way to determine their durations (except very roughly), their speeds of development, or precisely when they happened. It was convenient to assume that they had endured for hundreds of thousands or for millions of years, though no real evidence of this existed. A good instance is that of the Antarctic ice-cap, of which we shall hear more below.

So far as the most recent division of geologic time, the Pleistocene, was concerned, geologists, with much more evidence to work from, saw that there had been at least four ice ages in a period of about 1,000,000 years. They consequently proposed the idea that the Pleistocene was not at all like previous periods. It was exceptional, because it had so many ice ages. They may have been misled by failure to take sufficient account of the fact that glacial evidence is very easily destroyed, and that, as we go further back into geological history, the mathematical chances of finding evidences of glaciation, never very good, decrease by geometrical progression.

Down to ten years ago—and, indeed, until 1951—it was the considered judgment of geologists that the last ice age in North America, which they refer to as the Wisconsin glaciation, began about 150,000 years ago, and ended about 30,000 years ago.

This opinion appeared to be based upon strong evidence. The estimates of the date of the end of the ice age were supported by the careful counting of clay varves (6) and by numerous seemingly reliable estimates of the age of Niagara Falls. As a consequence, experts were contemptuous of all those who, for one reason or another, attempted to argue that the ice age was more recent. One of these was Drayson, whose theory called for a very recent ice age. His followers produced

much evidence, but it was ignored. When the Swedish scientist Gerard de Geer established by clay varve counting that the ice sheet was withdrawing from Sweden as recently as 13,000 years ago, the implications were not really accepted, nor were his results popularly known. Books continued to appear, even thirty years afterwards, with the original estimates of the age of the icecap.

Then, following World War II, nuclear physics made possible the development of new techniques for dating geological events. One of these was radiocarbon dating.

The method of radiocarbon dating was developed by Willard F. Libby, nuclear physicist of the University of Chicago, now a member of the United States Atomic Energy Commission. It uses an isotope of carbon (Carbon 14) which has a "half-life" of about 5,568 years (115:75). A "half-life" is the period during which a radioactive substance loses half its mass by radiation. Among the very numerous artificial radioactive elements created in nuclear explosions some have half-lives of millionths of seconds; others, occurring in nature, have half-lives of millions of years. For geological dating it is necessary to have radioactive elements that diminish significantly during the periods that have to be studied, and that occur in nature.

Since radiocarbon exists in nature, and has a relatively short half-life, the quantity of it in any substance containing organic carbon will decline perceptibly in periods of a few centuries. By finding out how much carbon was contained originally in the specimen and then measuring what still remains, the date can be found to within a small margin of error.

When this method was first developed by Libby, it could date anything containing carbon of organic origin back to about 20,000 years ago. Since then the method has been improved, through the efforts of many scientists, and its range has been nearly doubled.

The first major result of the radiocarbon method was the

revelation that the last North American ice sheet had indeed disappeared at a very recent date. Tests made in 1951 showed that it was still advancing in Wisconsin as recently as 11,000 years ago (272:105); later tests indicated that the maximum of the ice advance may have been a thousand years later than that. When these dates are compared with other dates showing the establishment of a climate like the present one in North America, it seems that most of the retreat and disappearance of the great continental icecap (with its 4,000,000 square miles of ice) can have taken little more than two or three thousand years.

What is the significance of this new discovery, besides showing how wrong the geologists had been before? The fact is that so sudden a disappearance of a continental icecap raises fundamental questions. It endangers some basic assumptions of geological science. What has become of those gradually acting forces that were supposed to govern glaciation as well as all other geological processes? What factor can account for this astonishing rate of change? It seems self-evident that no astronomical change and no subcrustal change deep in the earth can occur at that rate.

When this discovery was made, I expected that the next revelation must be to the effect that the Wisconsin ice sheet had had its origin at a much more recent time than was suspected, and that the whole length of the glacial period was but a fraction of the former estimates. I had a while to wait, because radiocarbon dating in 1951 was not able to answer the question. By 1954, however, the technique had been improved so that it could determine dates as far back as 30,000 years ago. Many datings of the earlier phases of the Wisconsin glaciation were made, and Horberg, who assembled them, reached the conclusion that the icecap, instead of being 150,000 years old, had appeared in Ohio only 25,000 years ago (222:278–86). This conclusion has been so great a shock to contemporary geology that some writers have sought to evade the clear implications, by questioning the radiocarbon meth-

od. Horberg betrays evidence of the intensity of the shock to accepted beliefs when he says that the results of the evidence are so appalling from the standpoint of accepted theory that it may be necessary either to abandon the concept of gradual change in geology or to question the radiocarbon method.

In this book I am not going to question the general reliability of the radiocarbon method. I intend merely to question the theories with which the new evidence conflicts. Dr. Horberg says that the necessity to compress all the known stages of the Wisconsin glaciation into the incredibly short period of barely 15,000 or 20,000 years involves an acceleration of geological processes—snowfall, rainfall, erosion, sedimentation, and melting—that seems to challenge the principle laid down by the founder of modern geology, Sir Charles Lyell, over a century ago. Lyell's principle, called "uniformitarianism," was that geological processes have always gone on about as they are going on now.

The Wisconsin icecap went through a number of oscillations, warm periods of ice recession alternating with cold periods of ice readvance. Horberg is at a loss to see what could cause them to occur at the velocity required by the radiocarbon dates. Allowing for extra time for ice growth before the evidence of massive glaciation in Ohio 25,000 years ago, Horberg manages to expand this 15,000 years to 25,000 for the duration of the glacier, but this does not solve his problem. Even so, the radiocarbon dates seem to require an annual movement of the ice front of 2,005 feet, "two to nine times greater than the rate indicated by varves and annual moraines" (222:283).

The fact that these new facts call into question some basic ideas in geology is recognized by Horberg:

> Probably only time and the progress of future studies can tell whether we cling too tenaciously to the uniformitarian principle in our unwillingness to accept fully the rapid glacier fluctuations evidenced by radiocarbon dating (222:285).

Recent geological literature shows that a rather desperate effort is being made to blur the significance of the new data. We will return to this question later. Here I would like to suggest some far-reaching implications of these facts. We have seen an ice sheet appear and disappear in—geologically speaking—a twinkling of an eye. There are three deductions to be made:

a. Any theory of ice ages must give a cause that can operate so fast.
b. If the last icecap in North America appeared and disappeared in 25,000 years, we cannot assume that the ancient icecaps lasted for longer periods.
c. If other geological processes are correlated with ice ages, then their tempo must also have been faster than we have supposed, and a cause must be found for their accelerated tempo.

In later chapters we shall see that a displacement of the crust must accelerate these geological processes.

5. *The New Evidence from Antarctica*

Another kind of radioelement dating has provided us with new data as revolutionary in their implications as the data produced by the radiocarbon method. This is referred to as the radioelement inequilibrium method or (for short) the ionium method of dating. It was developed by Dr. W. D. Urry and Dr. C. S. Piggott, of the Carnegie Institution of Washington, before World War II (439, 440). In recent years it has been widely applied in oceanographic research by both American and foreign scientists.

The ionium method is used with sea sediments. It is based upon three radioactive elements, uranium, ionium, and radium, which are found in sea water and in sea sediments, and that decay at different rates. As the result of the different

rates of decay, the proportions of the three elements in a sample of sediment change with time, and thus it is possible, by measuring the remnant quantities of the three elements, to date the samples.The samples are obtained by taking long cores from the bottom of the sea. A core is obtained by lowering a coring tube from a ship. It pierces the bottom sediments and obtains a cross section of them. The ionium method permits dating back as far as about 300,000 years.

Among the materials first dated by Urry's method were some long cores that had been taken from the bottom of the Ross Sea in Antarctica by Dr. Jack Hough during the Byrd expedition of 1947–48. These cores showed alternations in types of sediment. There was coarse glacial sediment, as was expected, and finer sediment of semiglacial type, but there were also layers of fine sediment typical of temperate climates. It was the sort of sediment that is carried down by rivers from ice-free continents. Here was a first surprise, then. Temperate conditions had evidently prevailed in Antarctica in the not distant past. The sediment indicated that not less than four times during the Pleistocene Epoch, or during the last million years, had Antarctica enjoyed temperate climates. (See Figure XI, p. 306.)

Then, when this material was dated by Dr. Urry, it became plain that the numerous climatic changes had occurred at very short intervals. Moreover, it appeared that the last ice age in Antarctica started only a few thousand years ago. Hough wrote:

> The log of core N–5 shows glacial marine sediment from the present to 6,000 years ago. From 6,000 to 15,000 years ago the sediment is fine-grained with the exception of one granule at about 12,000 years ago. This suggests an absence of ice from the area during that period, except perhaps for a stray iceberg 12,000 years ago. Glacial marine sediment occurs from 15,000 to 29,500 years ago; then there is a zone of fine-grained sediment from 30,000 to 40,000 years ago, again suggesting an absence of ice from the sea. From 40,000 to 133,500 years ago there is glacial marine material, divided into two zones of coarse- and two zones of medium-grained texture.

The period 133,000–173,000 years ago is represented by fine-grained sediment, approximately half of which is finely laminated. Isolated pebbles occur at 140,000, 147,000 and 156,000 years. This zone is interpreted as recording a time during which the sea at this station was ice free, except for a few stray bergs, when the three pebbles were deposited. The laminated sediment may represent seasonal outwash from glacial ice on the Antarctic continent.

Glacial marine sediment is present from 173,000 to 350,000 years ago, with some variation in the texture. Laminated fine-grained sediment from 350,000 to 420,000 years ago may again represent rhythmic deposition of outwash from Antarctica in an ice free sea. The bottom part of the core contains glacial marine sediment dated from 420,000 to 460,000 years by extra-polation of the time scale from the younger part of the core (225:257–59).

It should be realized that the Ross Sea, from which these cores were taken, is a great triangular wedge driven right into the heart of the continent of Antarctica, to within about eight hundred miles of the pole. It follows that, when the shores of the Ross Sea were free of ice, and if free-flowing rivers were bringing down sediment from the interior, Antarctica must have been very largely an ice-free continent. Some of the fine sediment, it is true, does not indicate ice-free conditions for the whole continent, but only for the sea itself. The laminated sediment, consisting of distinct thin layers each representing the deposition of one year, suggests the results of a summer melting of an ice sheet not far from the sea, with swollen streams of melt water carrying the sediment to the sea. Such conditions do not suggest any wide deglaciation of Antarctica, though they do suggest conditions very different from those prevailing now. On the other hand, unlaminated deposits of fine sediment are consistent with a general and even with a total deglaciation of Antarctica. So far as we can see, such sediment can only have been brought down to the sea by rivers flowing from the interior of the continent. The very existence of such unfrozen rivers requires the deglaciation of a part of the continent. One is free to assume that the deglaciation applied to only a small area,

but there is really no good reason to adopt this assumption unless a cause can be shown for a local deglaciation.[1]

The importance of all this evidence is obvious when we realize that, as late as 1950, there appeared to be no question but that the icecap in Antarctica was millions of years old. According to Brooks, the geologists Wright and Priestly had presented conclusive evidence of the beginning of the present icecap as far back as the beginning of the Tertiary Period (52:239), some 60 or 80 million years ago. Now we have evidence of several periods of semiglacial or nonglacial conditions in Antarctica in the Pleistocene Epoch alone. This is sufficient to show us how little reliance can be placed upon the estimated durations of hundreds of thousands or millions of years for the glacial periods of the remote past.

We must realize, however, that the date found by Urry for the beginning of the deposition of glacial sediment on the

1 An astonishing bit of evidence suggesting the idea of a temperate age in Antarctica has been produced by Arlington H. Mallery, cartographer and archaeologist. Mr. Mallery solved the projection of an ancient map compiled in the sixteenth century by the Turkish geographer Piri Reis, from maps one of which is said to have been in the possession of Columbus, and the others of which were said by Piri Reis to have been preserved in the East since the time of Alexander the Great (who may have discovered them in Egypt). The projection had long baffled scientists, including the explorer-scientist Nordenskjöld, who had spent seventeen years trying to solve it. When Mr. Mallery solved the projection he found that the map showed all the coasts of South America, a great part of the coast of Antarctica, including Queen Maud Land and the Palmer Peninsula, and Greenland and Alaska. It appeared that the mapping of Antarctica had actually been done when the land was ice-free—before the icecap appeared. There was no indication as to what ancient people could have made the map, but Mr. Mallery concluded that the information on the map was at least five thousand years old, and perhaps much older. The topographic features on the map of Queen Maud Land corresponded remarkably to the features deduced from seismic profiles made during one of the recent Antarctic expeditions of the Navy. Mr. Mallery's statements were confirmed and supported by Mr. M. I. Walters, who, as a member of the Hydrographic Office, had checked the map in detail, and by Father Daniel Linehan, Director of the Weston Observatory of Boston College, who checked the seismic profile made by the Navy against the data on the map. Mr. Mallery's findings were presented in a radio broadcast by the Georgetown University Forum, Washington, D.C., in August, 1956. A verbatim report of the broadcast, with reproductions of the map, may be obtained from the Forum.

bottom of the Ross Sea 6,000 years ago is not the date of the beginning of the last change of climate in Antarctica. A considerable time must have elapsed between the fall of temperature on the continent and the beginning of the deposition of the glacial sediment. An icecap must grow to a considerable thickness before it can start to move by gravity, and can start to throw off icebergs at the coast. Moreover, we must suppose that the change of climate must have been gradual at first. It seems reasonable, therefore, to allow a period of the order of about 10,000 years from the time the climate started to change to the time when the glacial sediment began to be deposited on the sea bottom. This is all the more likely since most of the coasts of Antarctica seem to be bounded by mountain chains, which the icecap would have to cross.

Where do these considerations lead us? They lead us to the conclusion that the melting of the great icecap in North America about 10,000 years ago and the beginning of the massive advance of the Antarctic icecap may have been roughly contemporary; that as one icecap melted, the other one grew.

Let us pause to consider the implications of this astonishing conclusion. For one thing, it is clear that no such change as the growth or removal of even a considerable part of a continental icecap covering 6,000,000 square miles of the earth's surface can result from purely local causes. At present the Antarctic icecap profoundly affects the climate of the whole world. The great anticyclonic winds, blowing outward from the continent in all directions, influence the directions of ocean currents, and the climates of all the lands in the Southern Hemisphere. The North American icecap was equally a factor in world climate. If one icecap appeared when the other disappeared, then both of these great contemporary changes of climate must be supposed to have resulted from some cause operating on the globe as a whole. But what kind of cause could glaciate one continent and deglaciate the other? It seems quite clear that only a shift of the crust of the earth such as would have moved America

away from one polar zone and Antarctica toward the other one can adequately account for the facts. Moreover, it is true, as can be seen from any globe, that a movement of the crust sufficient to bring Hudson Bay down to its present latitude from the pole would at the same time account for the glaciation of all that half of Antarctica facing the Ross Sea.

There are a number of important conclusions to be drawn from these Antarctic data, in addition to the remarkable confirmation of a displacement of the crust. One is that the rapid rate of change indicated for the Wisconsin icecap may be typical for the whole Pleistocene, and therefore, very likely, typical of older periods in the earth's history. Hardly less important than this is the implication to be drawn from the apparent fact that the ice ages, in these two instances, were not contemporaneous. It follows, of course, that if the ice ages we know most about were not contemporary in the two hemispheres, there is no justification for assuming that those we know little or nothing about were contemporary. Clearly, with glacial periods so short, and the tempo of change so rapid, there is no justification for claiming glaciation in the two hemispheres hundreds of millions of years ago to have been simultaneous. This theory, which constitutes a harmful dogma of contemporary science, should now be abandoned, and with it must go most of the current speculations about the causes of ice ages.

I cannot conclude this chapter without warning the reader that, however clear these facts may be, there are still some who will insist that the Hough-Urry cores show that the recent glaciations were simultaneous in the two hemispheres. This argument is based on the strange period of high temperature that followed the ice age in North America, but was world-wide in its effects. This warm period has been well established, but its cause has been unknown. The essential facts are given by Professor Flint:

> . . . the evidence of the fossil plants, and, in addition, several entirely independent lines of evidence, establish beyond doubt that the climate (with some fluctuation) reached a maximum of warmth be-

tween 6,000 and 4,000 years ago; since then (again with minor fluctuations) it has become cooler and more moist down to the present time. Apparently as recently as 500 B.C. the climate was still slightly warmer than it is today. The warm, relatively dry interval of 2000 years' duration has been called the *Climatic Optimum*. It is the outstanding fact of the so-called post-glacial climatic history (156:487).

Dr. Hough attempts to identify the last warm period in Antarctica with this Climatic Optimum, but we have seen that, according to his own core, the last temperate period in Antarctica apparently began 15,000 years ago, and ended 6,000 years ago, thus enduring for some 9,000 years, while the Climatic Optimum began 6,000 years ago, when the temperate age was drawing to a close in Antarctica, and endured for only 2,000 years. It seems to me, therefore, that there is no good reason to identify the two.

For some time attempts were made to attack the reliability of the ionium method. I raised the question of these attacks in a conversation with Einstein and received assurance that in his opinion the method was reliable. Ericson and Wollin have recently shown that the results obtained by the ionium method agree very well with results achieved by other reliable means (Chapter IX). We shall have occasion to cite the work of Soviet scientists who appear to have used the ionium method successfully in the Arctic Ocean, and to have obtained results in good agreement with other methods of dating. We shall see that they have found a warm period for the Arctic to correspond with the warm period indicated for Antarctica, and at about the same time, just as we should expect if both areas lay outside of the polar circles when the last North American icecap was centered at Hudson Bay.

Volchok and Kulp have recently published the results of a study by which some sources of errors in the use of the ionium method for dating have been identified. Their results suggest that in some cases errors in dating may result from the use of cores that have not been continuously deposited, or that have been disturbed since their original deposition. However, in the cases of the Ross Sea cores cited above we

have three cores taken some distance apart that seem, nevertheless, to agree pretty well with each other, even though there are evidences of some disturbance, and their evidence is strengthened by their agreement with the Arctic cores to be discussed later on.

From our point of view, the precise accuracy of the dating is less important than the evidence that, in very recent time, there was deposition of temperate type sediment in the Ross Sea off the Antarctic coast.

6. Conclusion

It is clear that none of the great glaciations of the past can be explained by the theories hitherto advanced. The only ice age that is adequately explained is the present ice age in Antarctica. This is excellently explained. It exists, quite obviously, because Antarctica is at the pole, and for no other reason. No variation of the sun's heat, no galactic dust, no volcanism, no subcrustal currents, and no arrangements of land elevations or sea currents account for the fact. We may conclude that the best theory to account for an ice age is that the area concerned was at a pole. We thus account for the Indian and African ice sheets, though the areas once occupied by them are now in the tropics. We account for all ice sheets of continental size in the same way.

Stokes has provided an excellent list of specifications for a satisfactory ice age theory, every one of which is met by the assumption of crust displacements as the fundamental cause (405:815–16):

a. An initiating event or condition.
b. A mechanism for cyclic repetitions or oscillations within the general period of glaciation.
c. A terminating condition or event.
d. It should not rely upon unprovable, unobservable, or unpredictable conditions, when well-known or more simple ones will suffice.

e. It must solve the problem of increased precipitation with colder climate.
f. The facts call for a mechanism that either increases the precipitation or lowers the temperature very gradually over a period of thousands of years.

It is evident that a displacement of the crust could initiate an ice age by moving a certain region into a polar zone, while a later displacement could end the ice age by moving the same area away from the polar zone. The increased precipitation and the oscillations of the borders of the ice sheets can be explained by the atmospheric effects that would result from volcanism associated with the movement of the crust. These effects will be discussed in later chapters.

III : ANCIENT CLIMATES

In the last chapter it was argued that the ice ages can be explained only by the assumption of frequent displacements of the earth's crust. The ice ages, however, represent only one side of the problem. If they are instances of extremely cold climates distributed in an unexplained manner on the earth's surface, there were also warm climates whose distribution is equally unexplained.

In connection with these warm climates in the present polar regions, there arises a contradiction of an especially glaring character. On the one hand there is evidence that the distribution of plants and animals in the past did not, as a rule, follow the present arrangements of the climatic zones. On the other hand, the trend of the new evidence is to show that climatic zones have always been about as clearly distinguished by temperature differences as they are today. This is in flat contradiction to the assumption, still widely held, that the earth, during most of geological history, did not possess clearly demarcated climatic zones. We are forced to conclude that, since many ancient plants and animals were not distributed according to the present climatic zones, the zones themselves have changed position on the earth's surface. This requires, as we have seen, that the surface shall have changed position relative to the axis of rotation. We shall now examine the evidence that supports this conclusion.

1. *Ages of Bloom in Antarctica*

I have suggested that in very recent time, no more than 10,000 years ago, a large part of Antarctica may have been ice-free. If this interpretation of the marine cores from the Ross Sea is questioned by the conservative-minded, there

can, however, be no dispute whatever about the more distant past climates of Antarctica. Those who may be inclined to disbelieve that Antarctica could have possessed a temperate climate 10,000 years ago must be reminded of the evidence that Antarctica has many times possessed such a climate.

So far as we know at present, the very first evidence of an ice age in Antarctica comes from the Eocene Epoch (52:244). This was barely 60,000,000 years ago. Before that, for some billion and a half years, there is no suggestion of polar conditions, though very many earlier ice ages existed in other parts of the earth. Henry, in *The White Continent,* cites evidence of the passing of long temperate ages in Antarctica. He describes the Edsel Ford Mountains, discovered by Admiral Byrd in 1929. These mountains are of nonvolcanic, folded sedimentary rocks, the layers adding up to 15,000 feet in thickness. Henry suggests that they indicate long periods of temperate climate in Antarctica:

> The greater part of the erosion probably took place when Antarctica was essentially free of ice, since the structure of the rocks indicates strongly that the original sediment from which they were formed was carried by water. Such an accumulation calls for an immensely long period of tepid peace in the life of the rampaging planet (206:113).

Most sedimentary rocks are laid down in the sea, formed of sediment brought down by rivers from near-by lands. The lands from which the Antarctic sediments were brought seem to have disappeared without a trace, but of the sea that once existed where there is now land we have plenty of evidence. Brooks remarks:

> . . . In the Cambrian we have evidence of a moderately warm sea stretching nearly or right across Antarctica, in the form of thick limestones very rich in reef-building Archaeocyathidae (52:245).

Millions of years later, when these marine formations had appeared above the sea, warm climates brought forth a luxuriant vegetation in Antarctica. Thus, Sir Ernest Shackleton is said to have found coal beds within 200 miles of the South Pole (71:80), and later, during the Byrd expedition of 1935,

geologists made a rich discovery of fossils on the sides of lofty Mount Weaver, in Latitude 86° 58′ S., about the same distance from the pole, and two miles above sea level. These included leaf and stem impressions, and fossilized wood. In 1952 Dr. Lyman H. Dougherty, of the Carnegie Institution of Washington, completing a study of these fossils, identified two species of a tree fern called Glossopteris, once common to the other southern continents (Africa, South America, Australia), and a giant tree fern of another species. In addition, he identified a fossil footprint as that of a mammallike reptile. Henry suggests that this may mean that Antarctica, during its period of intensive vegetation, was one of the most advanced lands of the world as to its life forms (207).

Soviet scientists have reported finding evidences of a tropical flora in Graham Land, another part of Antarctica, dating from the early Tertiary Period (perhaps from the Paleocene or Eocene) (364:13).

It is, then, little wonder that Priestly, in his account of his expedition to Antarctica, should have concluded:

> . . . There can be no doubt from what this expedition and other expeditions have found that several times at least during past ages the Antarctic has possessed a climate much more genial than that of England at the present day . . . (349b:210).

Further evidence is provided by the discovery by British geologists of great fossil forests in Antarctica, of the same type that grew on the Pacific coast of the United States 20,000,000 years ago (206:9). This, of course, shows that after the earliest known Antarctic glaciation in the Eocene, the continent did not remain glacial, but had later episodes of warm climate.

Dr. Umbgrove adds the observation that in the Jurassic Period the flora of Antarctica, England, North America, and *India* had many plants in common (430:263).

There is one group of theories to which we cannot appeal because of their inherent and obvious weaknesses. These are the theories that try to explain warm and cold periods in Antarctica by changes in land elevations, changes in the

directions of ocean currents, changes in the intensity of solar radiation, and the like. It is obvious, for instance, that no hypothetical warm currents could make possible the existence of warm climates in the center of the great Antarctic continent if that continent were at the pole, and if by some miracle Antarctica did become warm, how possibly could forests have flourished there deprived of sunlight for half the year?

2. *Warm Ages in the North*

The Arctic regions have been more accessible, and consequently they have been more thoroughly explored, than the Antarctic. It was from them that the first evidence came pointing unmistakably to shifts in the geographical positions of the poles. Most of the theories developed by those defending the dogma of the permanence of the poles were specially designed to explain these facts, or rather, as it now seems, to explain them away.

One method of explaining away the evidence was to suggest that the plants and animals of past geological eras, even though they belonged to similar genera or families as living plants, and closely resembled them in structure, may have been adapted to very different climates. This argument often had effect, for no one could exclude the possibility that, in a long geological period, species might make successful adjustments to different climatic conditions. Where single plants were involved such a possibility could not be dismissed. Where, however, whole groups of species, whole floras and faunas, were involved, there was increased improbability that they could all have been adjusted at any one time to a radically different environment from that in which their descendants live today. For this reason, and because the structure of plants has a definite relationship to conditions of sunlight, heat, and moisture, biologists have abandoned this method of explaining the facts. Dr. Barghoorn, for ex-

ample, says that fossil plants are reliable indicators of past climate (375:237–38).

It may be worth while to review, very briefly, some high points of the climatic history of the Arctic and sub-Arctic regions, beginning with one of the oldest periods, the Devonian, and coming down by degrees to periods nearer our own. (During this discussion the reader may find it helpful to refer to the table of geological periods, page 23.)

The Devonian evidence is particularly rich, and includes both fauna and flora. Dr. Colbert, of the American Museum of Natural History, has pointed out that the first known amphibians have been found in this period in eastern Greenland, near the Arctic Circle, though they must have required a warm climate (375:256). Many species of reef corals, which at present require an all-year sea-water temperature of not less than 68° F. (102:108), have been found in Ellesmere Island, far to the north of the Arctic Circle (399:2). Devonian tree ferns have been found from southern Russia to Bear Island, in the Arctic Ocean (177:360). According to Barghoorn, assemblages of Devonian plants have been found in the Falkland Islands, where a cold climate now prevails, in Spitzbergen, and in Ellesmere Island, as well as in Asia and America (375:240). In view of this, he remarks:

> The known distribution of Devonian plants, especially their diversification in high latitudes, suggests that glacial conditions did not exist at the poles (375:240).

In the following period, the Carboniferous, we have evidence summed up by Alfred Russel Wallace, co-author, with Darwin, of the theory of evolution:

> In the Carboniferous formation we again meet with plant remains and beds of true coal in the Arctic regions. Lepidodendrons and calamites, together with large spreading ferns, are found at Spitzbergen, and at Bear Island in the extreme north of Eastern Siberia; while marine deposits of the same age contain an abundance of large stony corals (446:202).

In the Permian, following the Carboniferous, Colbert reports a find of fossil reptiles in what is now a bitterly cold

region: "Large Permian reptiles . . . are found along the Dvina River of Russia, just below the Arctic Circle, at a North Latitude of 65° " (375:259). Dr. Colbert explains that these reptiles must have required a warm climate. In summing up the problem of plant life for the many long ages of the Paleozoic Era, from the Devonian through the Permian, Barghoorn says that it is "one of the great enigmas" of science (375:243).

Coming now to the Mesozoic Era (comprising the Triassic, Jurassic, and Cretaceous Periods), Colbert reports that in the Triassic some amphibians (the Labyrinthodonts) ranged all the way from 40° S. Lat. to 80° N. Lat. About this time the warm-water *Ichthyosaurus* lived at Spitzbergen (375:262–64). For the Jurassic, Wallace reports:

> In the Jurassic Period, for example, we have proofs of a mild arctic climate, in the abundant plant remains of East Siberia and Amurland. . . . But even more remarkable are the marine remains found in many places in high northern latitudes, among which we may especially mention the numerous ammonites and the vertebrae of huge reptiles of the genera Ichthyosaurus and Teleosaurus found in Jurassic deposits of the Parry Islands in 77° N. Lat. (446:202).

For the Cretaceous Period, A. C. Seward reported in 1932 that "the commonest Cretaceous ferns [of Greenland] are closely allied to species . . . in the southern tropics" (373: 363–71). Gutenberg remarks: "Thus, certain regions, such as Iceland or Antarctica, which are very cold now, for the late Paleozoic or the Mesozoic era show clear indications of what we would call subtropical climate today, but no trace of glaciation; at the same time other regions were at least temporarily glaciated" (194:195). This evidence, linked in this way with the problem of the ice ages we have already discussed, reveals the existence of a single problem. Ice ages in low latitudes, and warm ages near the poles, are, so to speak, the sides of a single coin. A successful theory must explain both of them.

Following the Cretaceous, the Tertiary Period shows the same failure of the fauna and flora to observe our present

climatic zones. Scott, for example, says: "The very rich floras from the Green River shales, from the Wilcox of the Gulf Coast and from the Eocene of Greenland, show that the climate was warmer than in the Paleocene, and much warmer than today" (372:103).

In this Eocene Epoch we find evidence of warm climate in the north that is truly overwhelming. Captain Nares, one of the earlier explorers of the Arctic, described a twenty-five-foot seam of coal that he thought was comparable in quality to the best Welsh coal, containing fossils similar to the Miocene fossils of Spitzbergen. He saw it near Watercourse Bay, in northern Greenland (319:II, 141–42). Closer examination revealed that it was, in reality, lignite. Nevertheless, the contained fossils clearly indicated a climate completely different from the present climate of northern Greenland:

> The Grinnell Land lignite indicates a thick peat moss, with probably a small lake, with water lilies on the surface of the water, and reeds on the edges, and birches and poplars, and taxodias, on the banks, with pines, firs, spruce, elms and hazel bushes on the neighboring hills . . . (319:II, 335).

Brooks thinks that the formation of peat bogs requires a rainfall of at least forty inches a year, and a mean temperature above 32° F. (52:173). This suggests a very sharp contrast with present Arctic conditions in Grinnell Land.

DeRance and Feilden, who did the paleontological work for Captain Nares, also mention a Miocene tree, the swamp cypress, that flourished from Central Italy to 82° N. Lat., that is, to within five hundred miles of the pole (319:II, 335). They show that the Miocene floras of Grinnell Land, Greenland, and Spitzbergen all required temperate climatic conditions, with plentiful moisture. They mention especially the water lilies of Spitzbergen, which would have required flowing water for the greater part of the year (319:II, 336).

In connection with the flora of Spitzbergen, and the fauna mentioned earlier, it should be realized that the island is in polar darkness for half the year. It lies on the Arctic Circle, as far north of Labrador as Labrador is north of Bermuda.

Wallace describes the flora of the Miocene. He points out that in Asia and in North America this flora was composed of species that apparently required a climate similar to that of our southern states, yet it is also found in Greenland at 70° N. Lat., where it contained many of the same trees that were then growing in Europe. He adds:

> But even farther North, in Spitzbergen, 78° and 79° N. Lat. and one of the most barren and inhospitable regions on the globe, an almost equally rich fossil flora has been discovered, including several of the Greenland species, and others peculiar, but mostly of the same genera. There seem to be no evergreens here except coniferae, one of which is identical with the swamp-cypress (*Taxodium distichum*) now found living in the Southern United States. There are also eleven pines, two *Libocedrus,* two *Sequoias,* with oaks, poplars, birches, planes, limes, a hazel, an ash, and a walnut; also water lilies, pond weeds, and an Iris—altogether about a hundred species of flowering plants. Even in Grinnell Land, within 8¼ degrees of the pole, a similar flora existed . . . (446:182–84).

It has been necessary to dwell at length on the evidence of the warm polar climates, because this is important for the discussion that follows. Too often, in theoretical discussions, the specific nature of the evidence tends to be lost sight of.

3. Universal Temperate Climates—A Fallacy

The evidence I have presented above (and a great deal more, omitted for reasons of space) has long created a dilemma for geology. Only two practical solutions have offered themselves. One is to shift the crust, and the other is to suggest that climatic zones like the present have not always existed. It is often suggested that the climates have been very mild virtually from pole to pole, at certain times. The extent to which this theory is still supported is eloquent evidence of the power of the "dogma" of the permanence of the poles. When one inquires as to the evidence for the existence of such warm, moist climates, a peculiar situation is revealed. There is no evidence except the fossil evidence that the theory is

supposed to explain. Could there be a better example of reasoning in a circle? Colbert cites evidence that the Devonian animals were spread all over the world, and then remarks that therefore ". . . it is reasonable to assume . . . that the Devonian Period was a time of widely spread equable climates, a period of uniformity over much of the earth's surface" (375:255). According to him, the same situation held true through the Paleozoic and Mesozoic, and even much later periods (375:268). Other paleontologists reasoned in the same way. Goldring, for example, remarked: "The Carboniferous plants had a world-wide distribution, suggesting rather uniform climatic conditions" (177:362). She drew the same conclusions from the world-wide distribution of Jurassic flora (177:363). But it is clear that when a theory has been concocted to explain a given set of facts, those facts themselves cannot be adduced as proof of the theory. This is circular reasoning. A theory must, first, be shown to be inherently reasonable, and then it must be supported by independent facts.

Is such a theory inherently reasonable? The answer is that it is not. It involves, in the first place, ignoring the astronomical relations of the earth. The theory requires us to assume the existence of some factor powerful enough to counteract the variation of the sun's heat with latitude. As Professor Bain, of Amherst, has pointed out, in an article to be discussed further below,

> . . . The thermal energy arriving at the earth's surface per day per square centimeter averages 430 gram calories at the equator but declines to 292 gram calories at the 40th parallel and to 87 gram calories at the 80th parallel . . . (18:16).

What force sufficiently powerful to counteract that fact of astronomy can be suggested, and, more important, supported by convincing evidence?

It was thought at first that universal temperate climates might be accounted for by the theory of the cooling of the earth. Those who proposed this theory (253, 292) argued that

in earlier ages the earth was hotter, the ocean water evaporated much more rapidly, and it formed thick clouds that reflected the sun's radiant energy back into space. The cloud blanket shut out the sun's radiation but kept in the heat that radiated from the earth itself, and this acted to distribute the heat evenly over the globe. The cloud blanket must have been thick enough to make the earth a dark, dank, and dismal place. Since, as Dr. Colbert shows, fossils are found outside the present zones appropriate to them even in recent geological periods, such conditions must have obtained during about 90 per cent of the earth's whole history, and most of the evolution of living forms must have taken place in them.

For a number of reasons, including the difficulty of explaining how plants can have evolved without sunlight, this theory has been abandoned. We have also seen that the idea that the earth was even hotter than now has recently been undermined. This has destroyed the solidity of the theory's basic assumption.

The fact that the theory never was reasonable is shown from Coleman's arguments against it, advanced more than a quarter of a century ago. He pointed out that not only are ice ages known from the earliest periods (from the Pre-Cambrian) but there is evidence that some of these very ancient ice ages were even more intensely cold than the recent ice age that came to an end 10,000 years ago (87:78). No less than six ice ages are known from the Pre-Cambrian (430:260). The evidence of one of these Pre-Cambrian or Lower Cambrian ice ages is interestingly described by Brewster:

> In China, in the latitude of northern Florida, there is a hundred and seventy feet of obvious glacial till, scratched boulders and all, and over it lie sea-floor muds containing lower Cambrian trilobites, the whole now altered to hard rock (45:204).

It is obvious that such ice ages (and evidences of more of them are frequently coming to light) are in conflict with the theory of universal equable climates. Some of them are found

right in the midst of periods thought to have been especially warm, such as the Carboniferous.

Coleman presents other geological evidence against the theory. The fact that most of the fossils found are those of warm-climate creatures is, he thinks, misleading. Plants and animals are more easily fossilized in warm, moist climates than they are in cold, arid ones. Fossilization, even under the most favorable conditions, is a rare accident. The fauna and flora of the temperate and arctic zones of the past were seldom preserved (87:252). Thus, while the finding of fossils of warm-climate organisms all over the earth is an argument against the permanence of the present arrangement of the climatic zones, it is not an argument for universal mild climates.

Another argument against such climates may be based upon the evidences of desert conditions in all geological periods. These imply world-wide variations in climate and humidity. Both Brooks (52:24–25, 172) and Umbgrove (430: 265) stress the importance of this evidence. One of the most famous formations of Britain—the Old Red Sandstone—is, apparently, nothing but a fossil desert. Coleman points to innumerable varved deposits in many geological periods as evidence of seasonal changes (87:253), which, of course, imply the existence of climatic zones.

Ample evidence of the existence of strongly demarcated climatic zones through the earth's whole history (at least since the beginning of the deposition of the sedimentary rocks) comes from other sources. Barghoorn cites the evidence of fragments of fossil woods from late Paleozoic deposits in the Southern Hemisphere that show pronounced ring growth, indicating seasons; he also points out that in the Permo-Carboniferous Period floras existed that were adapted to very cold climate (375:242). Colbert himself reports good evidence of seasons in the Cretaceous Period, in the form of fossils of deciduous trees (375:265).

Umbgrove cites the geologist Berry, who states that the fossilized woods from six geological periods, from the Devo-

nian to the Eocene, show well-marked annual rings, indicating seasons like those of the present time. Furthermore, Berry goes on to say:

> Detailed comparisons of these Arctic floras with contemporary floras from lower latitudes . . . show unmistakable evidence for the existence of climatic zones . . . (430:266).

Brooks concludes, on the basis of Berry's evidence, that climatic zones existed in the Eocene (52:24). Ralph W. Chaney, after a study of the fossil floras of the Tertiary Period (from the Eocene to the Pliocene), concluded that climatic zones existed (72:475) during that whole period. The distinguished meteorologist W. J. Humphreys, whose fundamental work, *The Physics of the Air,* remains a classic, remarked in 1920 that there was no good evidence of the absence of climatic zones from the beginning of the geological record. Finally, Dr. C. C. Nikiforoff, an expert on soils (both contemporary and fossil soils), has stated that "In all geological times there were cold and warm, humid and dry climates, and their extremes presumably did not change much throughout geological history" (375:191). We will return, below, to the significance of fossil soils, and present other evidence showing persistence of sharply demarcated climatic zones during the earth's history. But where, at this point, does the evidence leave us?

On the one hand, the evidence shows that the plants and animals of the past were distributed without regard to the present direction of the climatic zones. I have been unable to do more than suggest the immensity of the body of evidence supporting this conclusion. On the other hand, the attempt to deny the existence, in the past, of sharply demarcated climatic zones like those of the present has failed. It may even be said to have failed sensationally. There is no scrap of evidence for it, except the evidence it is supposed to explain, while, on the other hand, it is in contradiction both with the fundamentals of astronomy and the preponderance of geological evidence.

So we are left with a clear-cut conclusion: Climatic zones have always existed as they exist today, but they have followed different paths on the face of the earth. If changes in the position of the axis of rotation of the earth, and of the earth upon its axis, are equally impossible, and if the drift of continents individually is rendered extremely improbable for numerous weighty reasons, then we are forced to the conclusion that the surface of the earth must often have been shifted over the underlying layers.

4. The Eddington-Pauly Suggestion

Another suggestion for displacements of the earth's crust, to which I have briefly referred, should now be further discussed. Its author, Karl A. Pauly, has contributed new lines of evidence in support of such shifts. He has based his displacement theory on Eddington's suggestion that the earth's crust may have been displaced steadily through time by the effects of tidal friction. Eddington's idea has serious weaknesses, but the evidence for displacements presented by Pauly is most impressive.

Pauly suggests that a study of the elevations above sea level of the terminal moraines of mountain glaciers in all latitudes can establish a correlation of elevation with latitude. It is true that many factors influence the distance a mountain glacier may extend downward toward sea level, but latitude is one of them, and by using a sufficient number of cases it is possible to average out the other factors, and arrive at the average elevation of mountain glacier moraines above sea level for each few degrees of latitude from the equator toward the poles. This gives us a curve that makes it possible to compare the elevations of the terminal moraines of mountain glaciers that existed during the Pleistocene Period. Pauly finds that these moraines do not agree with the curve, indicating unmistakably a displacement of the earth's crust (342: 89).

Pauly cites another impressive line of evidence in support of his conclusions. He has compared the locations of coal deposits of several geological periods (many of which are now in polar regions) with the locations of icecaps for the same periods. He lists 34 coal deposits regarded as of Jurassic-Liassic age and 17 of Triassic-Thaetic age, and finds that, if it is assumed that the centers of the icecaps of that time were located at the poles, then these coal deposits would have been located within or just outside the tropics, as would be correct. He says:

> The very definite location of these coal deposits within the Tria-Jura tropical and subtropical zones cannot be mere coincidence. The distribution indicates the lithosphere has shifted (342:96).

Of the Permo-Carboniferous coal deposits, which, he points out, are very widely distributed over the earth, he says that "95 out of 105 listed in *The Coal Resources of the World* lie within or just outside of the tropics as determined by the assumption that the North or South Pole lay under the center of one of the Permo-Carboniferous ice sheets" (342: 97).

5. *The Contribution of George W. Bain*

Not long ago Professor George W. Bain, of Amherst, in an article in the *Yale Scientific Magazine* (18), went considerably beyond the categories of evidence that we have so far considered. He discussed the specific chemical processes controlled by sunlight and varying according to latitude, and the remanent chemicals typical of soils developed in the different climatic zones. He extended this sort of analysis also to marine sediments.

Bain's approach to the problem of evidence of climatic change has many advantages. It avoids, for one thing, the objection that has been raised against some of the plant evidence: that plants of the past may have been adjusted to

climates different from those in which their modern descendants live. I believe his method establishes beyond question the existence of climatic zones all through the geologic past.

Dr. Bain begins with a precise definition of each climatic zone in terms of the quantities of the sun's heat reaching the earth's surface. He points out that, as is known, the seasonal variation of this heat increases with distance from the equator (18:16). He then describes the global wind pattern resulting from this distribution of the sun's energy, defining clearly the conditions of the horse latitudes, in which most of the earth's deserts are found, and the meteorology of the polar fronts. He shows that there are distinct and different complete chemical cycles in each of these areas, and corresponding cycles in the sea. Many of the chemical compounds produced in each of these areas are included, naturally, in the rocks formed from the sediments, and they remain as permanent climatic records.

It is impossible, because of limitations of space, to do justice to Dr. Bain's comprehensive approach to this question. There appears to be no room for doubt, however, that great differences exist between the mineral components of the different climatic zones, as determined by the amount of the sun's radiant heat. With regard to the polar soils, in addition, it is noteworthy that they are developed in circles on the earth's surface, rather than in bands. Temperate and tropical soils are, of course, found in bands, since the zones are bands that encircle the earth.

It will be clear to the reader that Dr. Bain has established a sound method for the study of the climates of the past. He has applied his method to the study of the climates of two periods, the Jurassic-Cretaceous and the Carboniferous-Permian, with very significant results. He has concluded, first, that climatic zones, representing the different distributions of solar heat, existed in those periods just as at present. This is proved by the specific remanent chemicals included in these rocks, which differ exactly as do the sediments of the different

zones at the present time. This is, of course, fatal for the theory of universal equable climates.

His second conclusion, of even greater importance, is that the directions of the climatic zones have changed enormously in the course of time. He finds the equator running through the New Siberian Islands (in the Arctic Ocean) in the Permo-Carboniferous Period, and North and South America lying tandem along it (18:17). The evidence he uses seems to establish his essential point (and ours) that the climatic zones themselves have shifted their positions on the face of the earth.

Dr. Bain has drawn some interesting further conclusions. He states that the earth's crust must have been displaced over the interior layers, and that "fixity of the axis of the earth relative to the elastic outer shell just is not valid. . . ." (18: 46). He points to the fossil evidence of the cold zones (distributed in circular areas) and says, ". . . The recurrent change in position of these rings through geologic time can be accounted for now only on the basis of change in the position of the elastic shell of the earth relatively to its axis of rotation" (18:46).

6. *The Contribution of T. Y. H. Ma*

Dr. Bain pointed out, in the paper above mentioned, that among other indications of latitude, sea crustaceans and corals may indicate latitude either by the presence or absence of evidence of seasonal variations in growth. It happens that corals have been very thoroughly investigated from precisely this point of view.

By a remarkable parallelism of development, another theory of displacement of the earth's crust took shape on the opposite side of the earth at about the same time that Mr. Campbell and I started on our project. Professor Ting Ying H. Ma, an oceanographer, then at the University of Fukien, China, came to the conclusion, after many years of study of

fossil corals, that many total displacements of the earth's whole outer mantle must have taken place. I did not become aware of Professor Ma's work until I was introduced to it by Dr. David Ericson, of the Lamont Geological Observatory, in 1954. Dr. Ericson has, in fact, taken a leading role in introducing Professor Ma's work to American scientists.

For about twenty years previous to the time I mention, Professor Ma had intensively pursued the study of living and fossil reef corals. He very early noticed that characteristic of reef corals referred to by Dr. Bain, but hitherto ignored by writers on corals. He saw that, at distances from the equator, there were seasonal differences in the rates of coral growth, and that the evidences of these were preserved in the coral skeleton. Specifically, he observed that in winter the coral cells are smaller and denser; in summer they are larger and more porous. Together, these two rings make up the growth for one year.

Studying living coral reefs in various parts of the Pacific, comparing, measuring, and tabulating coral specimens of innumerable species, making photographic studies of the coral skeletons, Professor Ma established that the rates of total annual coral growth for identical or similar species within the range of the coralline seas increased with proximity to the equator, and that seasonal variation in growth rates increased with distance from the equator.

Other writers on corals have pointed out that there are numerous individual exceptions and irregularities in coral growth rates, deriving from the fact that the coral polyps feed upon floating food, which may vary in quantity from place to place, from day to day, and even from hour to hour (125: 20–21; 298:52–53). Professor Ma, however, has guarded himself against error by a quantitative and statistical approach. In several published volumes of coral studies (285–290) he has compiled tables running into hundreds of pages, and his studies have involved thousands of measurements.

When this indefatigable oceanographer had worked out these relations of growth with latitude, he possessed an effec-

tive tool with which to investigate the climates of the past. It was possible now to arrive at a very good idea of the conditions under which fossil corals grew. Professor Ma studied hundreds of specimens of fossil corals from many of the geological periods. He devoted entire separate volumes to each of the Ordovician, Silurian, Devonian, Cretaceous, and Tertiary Periods (285–289).

As Ma assembled the coral data for past periods, it became plain to him that the total width of the coralline seas had never varied noticeably from the beginning of the geological record. Not only was the existence of seasons, of climatic zones, in the oldest geological periods clearly indicated; it was also indicated that the average temperatures of the respective zones were about the same as at present.

The second result of Ma's studies was to establish that the positions of the ancient coralline seas and, therefore, of the ancient equators were not the same as at present. They changed from one geological period to another. Ma first came to the conclusion that this could be explained only by the theory of drifting continents. Down to about 1949 he sought to fit the evidence into that theory. By 1949, however, the continuing accumulation of the evidence led him to adopt the theory of total displacements of all the outer shells of the earth over the liquid core. By some instinct of conservatism, however, he did not abandon the theory of floating continents, but combined it with the new theory.

Ma's coralline seas ran in all directions; one of his equators actually bisected the Arctic Ocean. But he had great difficulty in matching up his equators on different continents. If, for example, he traced an equator across North America, he could not match it with an equator for the same period on the other side of the earth, to make a complete circle of the earth. He therefore supposed that the continents themselves had been shifting independently, and this had had the effect of throwing the ancient equators out of line. He therefore allowed, for each period, enough continental drift to bring the equators into line, and it seemed, when he did

this, that in successive geological periods he did have increasing distances between the continents, as if the drift had been continuous.

Subsequently, Professor Ma developed his theory into a complete system, which is most interesting, and yet to which, I think, serious objections may be raised.

Corals are, according to Ma, excellent indicators of the climate for the time in which they grew, but, by the nature of the case, since corals grow only in shallow water, and grow upwards only as far as the surface, the period of time represented by a single fossil coral reef is of the order of a few thousand years only, as compared with the millions of years embraced by a geological period.

How short the continuous growth of a coral reef may be is indicated by numerous studies of the coral reefs of the Pacific. A. G. Mayor, for example, says:

> . . . The modern reefs now constituting the atolls and barriers of the Pacific could readily have grown upward to sea-level from the floors of submerged platforms since the close of the last glacial epoch (298:52).

At Pago Pago Harbor borings were made down to the basalt underlying the reef, and after estimates of the growth rate were arrived at, the age of the reef (Utelei) was estimated at 5,000 years. When these spans are compared with those of entire geological periods of the order of 20,000,000 or 30,000,000 years, it is clear how fragile must be any conclusions based on the assumption that a given coral reef in Europe was contemporary with another one in North America. It is quite impossible in the present state of our knowledge to decide that they were in fact contemporary.

This means that Ma's corals for a period like the Devonian may be indications of different equators that existed at different times during that period of 40,000,000 years. Therefore it is obvious that thousands of coral specimens would be required to give any certainty as to the actual climatic history of an entire geological period.

Very possibly Ma could have avoided combining the two different theories—the slipping of the shell of the earth and the drifting of continents—if he had supposed a sufficiently frequent slipping of the crust. The frequency of the displacements suggested by the theory presented in this book, which would involve many different equators in a single geological period, would remove his difficulties. As it is, he has to face all the geophysical and geological objections to the drifting continent theory, as well as difficulties with his displacement theory.

7. *On the Rate of Climatic Change*

Studies appear from time to time in which attempts are made to trace climatic changes in specified areas over periods of millions of years. In one of these, for example (72), the conclusion is reached that there was a gradual cooling of the climate during a great many million years of the Tertiary Period. It is true that no cause of such a progressive cooling can be pointed to; neither is there any explanation as to why the climatic change had to be so gradual. It is simply assumed that the climatic change had to be gradual, and that the cause of the change had to be such as to explain imperceptible climatic changes over millions of years of time.

It is important to define as clearly as possible the nature of the evidence on which these conclusions are based. In the example I am considering, the following facts have decisive importance:

a. The period of time involved in an alleged cooling of the climate is of the order of 30,000,000 years.
b. Wherever reference is made to the specific strata of rock selected for analysis of the climatic evidence (consisting of included fossils), it is clear that the time required for the deposition of a particular layer was of the order of 10,000 years.

c. It follows that during 30,000,000 years it would be possible to have about 3,000 different layers of sedimentary rock.
d. A vast majority of these layers cannot be sampled, either because they no longer exist, or because they do not contain fossils, or simply because of the amount of work involved.
e. As a result, only the most unsatisfactory kind of spot checking is possible. Perhaps a dozen strata out of 3,000 may be studied, and from these it must be obvious that no dependable climatic record can be established.
f. Even with the unsatisfactory spot checking so far attempted, reversals of climatic trends have been observed (72).
g. Climatic conditions indicated by a layer of sediments deposited during a brief period of time in one location cannot be assumed to indicate the direction of climatic change over a great region, or over the whole earth. It seems quite as reasonable to suppose that climatic change in other regions at the same time was in a different direction. Furthermore, it cannot be assumed that two sedimentary deposits in different areas are of the same age because they both indicate climatic change in the same direction.

It must be concluded that all claims for gradual climatic changes in the same direction over long periods of time and over great areas are unsupported by convincing evidence. The existence of such long-term trends can be supported by no reasonable hypothesis. We are left with the conclusion that climatic change has probably taken place within relatively short periods of time, and possibly in opposite directions for different areas at the same time, as, indeed, would be a natural consequence of displacements of the earth's crust.

IV : THE MOUNTAINS

PART I. The Folding and Fracturing of the Crust

By far the most magnificent features of the earth's crust are the lofty mountain ranges that are found on all the continents, exciting the wonder of man, and those other, equally tremendous mountain ranges that lie drowned in the silent depths of the sea. These mountain ranges carry in their intricate formations much of the history of the earth's crust. If we could know the forces that produced them, we could grasp the basic dynamic principles of the earth's development. Unfortunately, though the mountains have long been the subject of intensive scientific investigation, they have preserved their secrets well. The most important of these secrets is the secret of their birth. What forces within the earth were responsible for their formation? As of now, we do not know.

Nothing could better betray the extent of our ignorance of the dynamic processes that have shaped the face of the earth than this confession of ignorance. Yet, it is agreed by geologists that no theory has so far satisfactorily explained mountain building. Daly, for example, has referred to the process of the folding of the rock strata, a phase of mountain building, as "an utterly mysterious process" (70d:41). Gutenberg has concluded that none of the present theories will do. He remarks that "all the forces discussed so far seem to be insufficient to produce the formation of mountains" (194: 171), and this includes, of course, the long-exploded (but still widely current) theory that ascribes mountains to the cooling and shrinking of the earth. As to this, Gutenberg remarks, ". . . other scientists have pointed out that the cooling of the earth is not sufficient to produce the major part of the crumpling, especially since investigations of the radioactive heat which is produced inside the earth have indicated that

the cooling of the earth is less than it had been originally believed. . . ." (194:192). Bullard, reviewing the third edition of Harold Jeffreys's basic work, *The Earth,* notes the absence of progress toward solving the problem of mountain building, since the second edition twenty-five years ago (59). Pirsson and Schuchert, the authors of a general text on geology, conclude a section on the cause of mountain building with the statement: "It must be admitted, therefore, that the cause of compressive deformation in the earth's crust is one of the great mysteries of science, and can be discussed only in a speculative way" (345:404).

What is the nature of this problem that has so far baffled science?

1. The Problem of Crustal Folding

It is important to take into account the fact that there are several different kinds of mountains, and that their origins may be ascribed to somewhat different circumstances, even though (as we shall see) they may be related to one underlying cause. Some mountains are caused by volcanic eruptions. These consist of piles of volcanic matter. Some of the greatest mountains on the earth's surface are volcanic mountains. Many of them are found on ocean bottoms, and when they rise to the surface they form the island chains (such as the Hawaiians) that are especially numerous in the Pacific. Sometimes volcanic islands or mountains can be formed quickly, as was the case recently in Mexico, where a large mountain, Paricutín, was developed in a few years to a height of several thousand feet from a lava flow that started in a cornfield on the level ground. Some mountains result from a vast flow of molten rock that gathers under the crust at one spot and domes it up. The causes of these events are unknown.

Many mountains, and even whole ranges of mountains, are brought into existence in part by the cracking of the earth's crust, accompanied by the tilting of the separated blocks. The

Sierra Nevada Mountains of California appear to have been formed in this way. According to Daly, they represent the tilting of a single block of the earth's crust some 600 miles long (98:90). Some folding of the crust, however, had previously taken place. Many great chasms, extended cliff formations, and rift valleys appear to have been formed by the cracking and drawing apart of the crust, and by the elevation or subsidence of the different sides. The great African Rift Valley is perhaps the best-known example of this sort of formation; the rift of which it is a part, as we shall see below, has recently been connected with a world-wide system of great submarine rifts. The cause of all this cracking and tilting is still one of the mysteries of science.

The greatest mountain systems on the earth's surface have been formed as the result of the lateral compression and folding of the crust. Since folding is the cause of most mountain building it must hold our particular attention. As already suggested, science is particularly at a loss to explain the folding. A number of suggestions have been advanced, but they are all deficient for various reasons.

A part of the public is under the impression that mountains have been formed by the action of running water, wearing away the stone, eroding the tablelands, and depositing layers of sediment in the valleys and in the sea. Although it cannot be denied that erosion has been a powerful factor in shaping many mountains, and may have been the main factor in shaping some of them (for example, Mt. Monadnock, in New Hampshire, which I can see from my window as I write these words), it cannot have been the principal cause of the formation of our great folded mountain ranges.

Geologists who have argued in favor of this theory have pointed out that the deposition of sediment in narrow crustal depressions may have been a cause of the folding of the crust. The folding could have resulted in part from the sinking of the valley bottoms under the weight of the sediments. The process will be found described in detail in almost any textbook of geology. There are serious objections to it, and no geol-

ogist today considers it a satisfactory explanation. One objection is that this process of folding is essentially local. It cannot explain the greatest mountain systems, some of which virtually span the globe. It cannot explain, for example, the almost continuous line of mountain ranges that includes the Rockies, the Andes, and the Antarctic Mountains, and which extends for a total distance of almost half the circumference of the earth. Neither can this theory explain the numerous submarine mountain ranges that have, in recent years, been discovered on the bottoms of the Atlantic, Pacific, and Arctic Oceans. Moreover, it has been pointed out that in many cases folding of the crust has taken place without any deposition of sediment, and therefore must have been due to other causes. The geologist Henry Fielding Reid remarked:

> . . . There are many deeps in the ocean, such as the Virgin Islands Deep, the Tonga Deep, and others, which appear to have sunk without any material deposit of sediments. . . . (354).

For these various reasons, then, geologists have come to the conclusion that erosion is only a secondary cause of mountain building (345:382–84). We shall consider this again.

Another common impression, as already mentioned, is that mountain formation has been due to the cooling and shrinking of the earth. It was reasonable, perhaps, as long as the theory of the cooling of the earth was unquestioned, to try to explain the origin of folded mountains in this way, for, of course, if the earth shrank in size, even only slightly, as a result of cooling, some wrinkling of the crust must be the result. The fact that the pattern of wrinkles that would be produced in this way (and which could be deduced fairly clearly) bore no resemblance whatever to the patterns of the existing mountain ranges, did not greatly diminish the currency of this theory, though it did bring about a devastating attack upon it by one competent geologist whose views we shall discuss below.

We have seen that there is now an impressive body of evidence and opinion against the theory of a molten origin for

the earth. The doubts that have gathered about this assumption are sufficiently serious to prevent us from basing any theory of mountain building upon it (for no theory can have greater probability than its own basic assumptions). But even if this were not the case, even if the molten origin of the earth were a demonstrated fact, still, it was pointed out twenty-five years ago, by Clarence Dutton, that the shrinking of the globe would not explain the folded mountains. Dutton had two objections. First, he said that the calculated amount of the shrinkage that could have occurred since the crust was formed, by the reduction of temperatures, would not account for the volume of the mountains known to have existed during geological history. Secondly, he pointed out that the kinds of pressures that would exist in the crust as a result of the shrinking of the earth could not produce mountain ranges of the existing patterns. On this point, he said:

> . . . As regards the second objection, which, if possible, is more cogent still, it may be remarked that the most striking features in the facts to be explained are the long narrow tracts occupied by the belts of plicated strata, and the approximate parallelism of their folds. These call for the action of some great horizontal force thrusting in one direction. Take, for example, the Appalachian system, stretching from Maine to Georgia. Here is a great belt of parallel synclinals and anticlinals with a persistent trend, and no rational inquirer can doubt that they have been puckered up by some vast force acting horizontally in a northwest and southeast direction. Doubtless it is the most wonderful example of systematic plication in the world. But there are many others that indicate the operation of the same forces with the same broad characteristics. The particular characteristic with which we are concerned is that in each of these folded belts the horizontal force has acted wholly or almost wholly in one direction. But the forces that would arise from a collapsing crust would act in every direction equally. There would be no determinate direction. In short, the process would not form long narrow belts of parallel folds. As I have not time to discuss the hypothesis further, I dismiss it with the remark that it is quantitatively insufficient and qualitatively inapplicable. It is an explanation that explains nothing that we want to explain. . . . (122:201–02).

It is indeed astonishing to note that though a quarter of a century has passed since this statement was made, and though

leading geophysicists today sustain Dutton's views (194:192), the impression is still widespread, and not merely among laymen, that mountains are, more or less, understandable as the consequence of the cooling of the earth. The cause of this inertia is, very likely, the absence of any alternative, acceptable theory of mountain building.

In recent years many geologists have agreed with Dutton that the mountains were folded by some immense force operating horizontally on the earth's crust. Furthermore, they have come to recognize that the force or forces involved in mountain folding acted on the earth's crust as a whole and at the same time. Thus, one of our leading geophysicists, Dr. Walter Bucher, of Columbia, remarked:

> Taken in their entirety, the orogenic [mountainous] belts are the result of world-wide stresses that have acted on the crust as a whole. Certainly the pattern of these belts is not what one would expect from wholly independent, purely local changes in the crust (58:144).

The same thing was pointed out by Dr. Umbgrove:

> . . . But the growing amount of stratigraphic studies make it increasingly evident that the terrestrial crust was subjected to a periodically alternating increase and decrease of compression. . . . I feel there is overwhelming evidence that the movements are the expression of a common, world-wide, active, and deep-seated cause. . . . (430:31).

Dr. Umbgrove was impressed by another characteristic of this world-wide force. It did not act continuously. It was not always acting to expand or squeeze sectors of the crust to fold them into mountains. It acted only at certain times, and then, for other periods, it was inactive. There was a sort of periodicity to its operation. This periodicity extended also to other aspects of the earth's geological history:

> The geologist comes across periodicity in many of the pages which he is arduously deciphering—in the sequence of the strata, for instance, and their contents of former organisms. . . . He observes it elsewhere, in the deep-seated forces that bring subsidence first in one area and then in another . . . in the intrusion of liquid melts or "magma" rising from some deeper part of the earth's interior; in the

ing. Joly attempted to prove that the accumulation of radioactive heat in the earth resulted in mountain building at intervals of 30,000,000 years (244; 235:153). Gutenberg, however, says that details of Joly's theory have been disproved (194:158) and, moreover, that the theory includes no mechanism to account for the 30,000,000-year intervals (194:188). It is impossible to see that the resulting upheaval of the surface could produce mountain ranges of the patterns that exist. Joly's theory does postulate a growing earth, but whether the crust bursts occasionally or is continually collapsing because of shrinking, it all amounts to the same thing: neither theory meets the requirements. Attempts have also been made to explain periodicity as the result of long-range astronomical cycles, but they have been unsuccessful (430:281–82). It is obviously difficult to explain mountain building by astronomical cycles.

For some years, geologists have been looking for a mountain-folding force below the earth's crust. They have been investigating the possibility of the existence of currents in the semiliquid layers under the crust, and speculating on the possible effects of such currents, if they exist, on the crust itself. It has been suggested that such currents, rising under the crust, or sinking, might fold the crust. A sinking current, for example, would have the effect of drawing the crust together over it, and pulling it down, forming wrinkles, in long narrow patterns, like the mountain ranges. Calculations have been made of the forces that could be brought to bear upon the crust in this way. Vening Meinesz prefers this way of accounting for mountain building:

> If we examine the pattern of great geosynclines over the earth's surface, we cannot doubt that their cause must have a world-wide character. The geology in these belts points to horizontal compression in the crust, at least during the later stages of their development. The two main hypotheses suggested to explain these great phenomena are (1) the thermal-contraction hypothesis, and (2) the hypothesis of subcrustal current systems of such large horizontal dimensions that, vertically, they must involve at least a great part of the thickness of the mantle and probably the whole mantle (349:319).

Vening Meinesz summarizes the arguments against the thermal-contraction hypothesis (the cooling of the earth), and argues for the second theory. It is interesting, in passing, to note that one of his arguments against the contraction theory is that "In large parts of the earth's surface . . . tension seems to exist in the crust at the same time that folding takes place elsewhere, and this fact is difficult to reconcile with thermal contraction (giving compression) throughout the crust. . . ." (349:320). He is here saying that the earth's crust was being stretched in some places and compressed in others, at the same time, which is inconsistent with the cooling and contracting theory. It is, however, quite consistent with the crust displacement hypothesis.

Now, as to the subcrustal current hypothesis, we may note that Meinesz is assuming currents travelling for great distances horizontally, and moving in great depths of hundreds of miles below the crust. Naturally, the movement of such masses of rock could potentially create pressures to stagger the imagination. Gutenberg discusses the work of many men who are studying subcrustal currents (194:186, 191). The chief weakness of the theory is the absence of any real evidence for the existence of such currents. It is suggested, for example, that thermal convection might account for them, or chemical changes of state in depth might account for them, or mechanical factors might be at work, but, meanwhile, there is no real evidence that such currents really exist. Some geologists have claimed to have found evidence of cyclonelike patterns in rock structures (194:188), but these appear to have been of small magnitude; they therefore may have been formed in small pockets of molten rock. They do not provide reliable evidence for the existence of gigantic crust-warping currents, such as would be required for mountain building.

The problem that we are involved with here is that of the origin of the geosyncline. Geologists refer to a downward fold in the crust of major proportions as a geosyncline. An upward fold (or arch) is a geoanticline. They are sometimes

ments and possibly through a number of successive displacements of the crust, to the formation of folded mountain ranges.

The systematic presentation of this theory requires us to consider the two different phases of displacement—equatorward and poleward—separately, for they have very different results. We will begin with the consideration of the effects of a displacement of a crustal sector toward the equator.

In a shift in that direction, a crustal sector is submitted to tension (or stretching), and this tension is relieved by the fracturing that takes place when the bursting stress exerted on the crust has come to exceed the strength of the crust. (For Mr. Campbell's calculations of the quantity of the bursting stress, as compared with estimates of crustal strength, see Chapter XI.) Until fractures appear and multiply, the crust cannot move over the bulge. After the fracturing permits the movement to begin, the crustal blocks tend to draw slightly apart. The spaces between them are immediately filled by molten material from below.

Let us form a clear picture of this crustal stretching, from the quantitative standpoint. It is important to estimate the stretch per mile, if we are to visualize the results. Taking the globe as a whole, the difference between the polar and equatorial diameters is about 26 miles. The circumferences, therefore, differ by about 78 miles. If the crust were displaced so far that a point at a pole was displaced to the equator, the polar circumference would have to stretch 78 miles to fit over the equator. This would amount to about 17 feet in the mile. Since the magnitude of displacements, however (according to evidence to be presented later), seems to have been of the order of no more than about 30 degrees, or one third of the distance from pole to equator, the average stretch per mile may have amounted to five or six feet, or one foot in a thousand.

It would be a mistake to visualize this stretching of the crust in the equatorward-moving areas as evenly distributed around the whole circumference of the globe. Obviously, the

real events would not correspond to this. The crust would be under bursting stress, and this would be relieved spasmodically, during the movement of the crust, by fractures at the weakest points. A fracture through the crust at one point would relieve the stress for perhaps hundreds of miles. Since the elasticity of the crust is slight, the stretching or extension of the crust would consist of the drawing apart, to varying distances, of the fractured blocks. Generally speaking, the fewer the fractures, the farther their sides would draw apart. It would be possible that the total amount of the stretching of the earth's circumference would be concentrated in relatively few critical areas.

It must also be kept in mind that some parts of this area being displaced toward the equator will be displaced farther than others. The greatest displacement will occur along the line, or meridian, drawn from the pole through the center of mass of the icecap and so around the earth; or, if any unexpected factor should deflect the direction of the movement, the greatest displacement will occur along whatever meridian represents the direction of the movement. As I have pointed out, at two pivot points on the equator 90 degrees away from this meridian there will be little or no movement, and the points in between will move proportionately to their distances from the meridian. The tension, or stretching, will be proportional to the amount of displacement. It therefore will be greatest along the central meridian of movement, and it is here that Mr. Campbell expects the first major fractures of the crust to develop.

It is important to visualize the nature of the crust on which this tension is exerted. The crust is comparatively rigid, having little elasticity, but it is not strong. It varies in thickness and strength from place to place. As we shall see, it is even now penetrated by great systems of deep fractures of unexplained origin. These inequalities of strength will be very important in determining the reactions of the crust from place to place to the tension exerted upon it; they will de-

termine the precise locations, and to some extent the patterns, of the fractures that will result.

Without attempting to anticipate a more detailed discussion, to be introduced later, of the forces involved in this fracturing of the crust, I would like to remark that the forces required for the fracturing are by no means so great as might be at first supposed. It is a question of relatively slight forces exerted over considerable periods of time.

If we disregard the factors that may locally influence the locations and sizes of fractures, a general pattern may be indicated to which they will tend to conform. Mr. Campbell has worked out this pattern schematically, and has indicated it in Figures II, III, and IV. The reader will note that the fractures take two directions. There are the north-south, or meridional, fractures, which Mr. Campbell refers to as the major fractures, and then there are minor fractures at right angles to them.

Mr. Campbell anticipates that numerous major fractures will occur parallel to each other as the crust moves. The formation of very numerous minor faults at right angles to the major faults will form a gridiron pattern of fractures. Mr. Campbell has suggested a method for visualizing the process. If the reader will cup his hands and place them together, with fingertips touching and the fingers of each hand close together (as if they lay on the surface of a sphere), and then imagine the sphere growing, and causing the fingertips of both hands to spread apart, and at the same time the fingers of each hand to spread apart, he may visualize the process. The gap between his hands will now represent a major fracture, and the gaps between the fingers of each hand will represent the minor fractures at right angles to it. The reader will see, a little later on, how closely this projection of fracture effects corresponds to the real phenomena in the earth's crust.

Another important aspect of these fractures is shown in Figure III. Mr. Campbell has indicated that, owing to the changing arc of the surface as the crustal sector moves

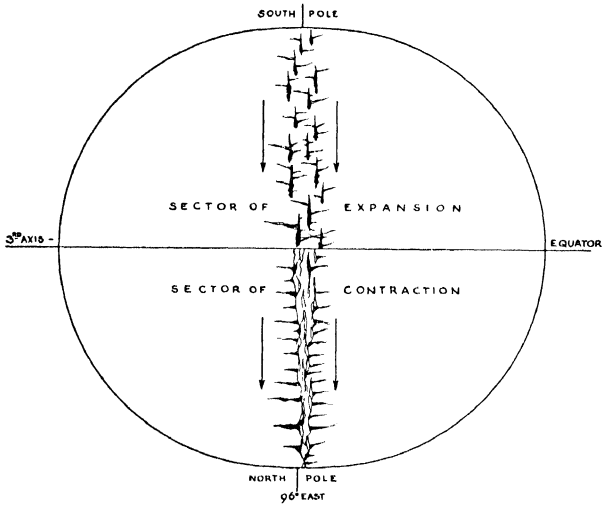

Fig. II. Mountain Building: Patterns of Fracture and Folding
The lithosphere, or crust, is represented in a future movement resulting from the effect of the present icecap in Antarctica. Since the latter's center of mass is on (or near) the meridian of 96° E. Long., the crust is represented as moving in that direction from the pole. The sector of expansion *is moving equatorward and therefore being extended. The* sector of contraction *is moving toward the North Pole from the equator and therefore being compressed.*

In the sector of expansion, parallel major faults can be observed, with minor faults at right angles. The wavy lines suggest the effects of local differences in crustal strength. The pattern of the fractures is indicated, but not their number; a very large number of meridional fractures might be formed, while the minor fractures would be even more numerous.

In the sector of contraction, crustal folding is shown only schematically. It is represented as if all the folding is taking place along one meridian, although in reality there would probably be many parallel zones of mountain folding at considerable distances from each other. Campbell indicates that this movement will be accompanied by fracturing of the crust, with faults running at right angles to the main axes of the folds. The third axis, *which runs through the equator, is considered to be the axis on which the crust turns. The points directly at the two ends of this axis do not move.*

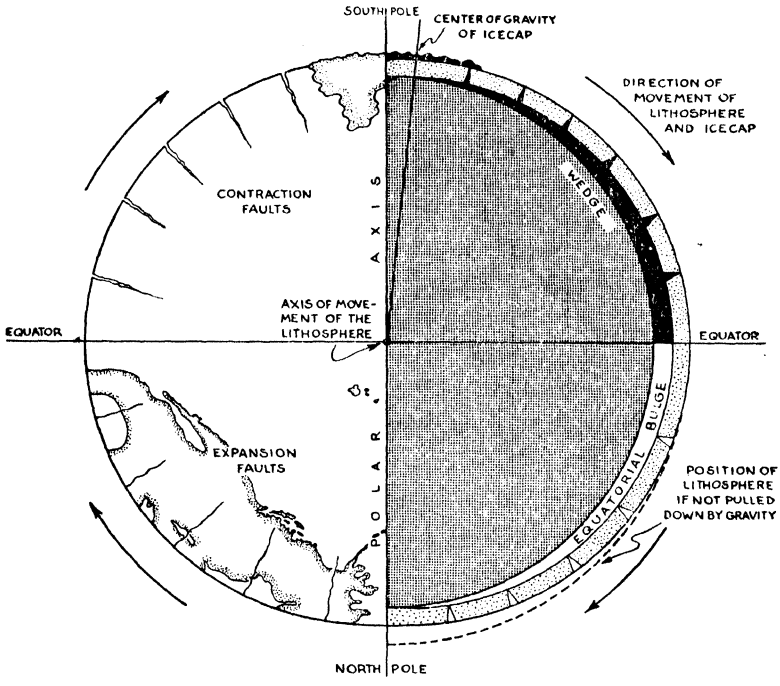

Fig. III. Vertical View of the Earth with Cross Section at 96° E. Long. *This figure illustrates a number of simultaneous effects of displacement. The* upper right-hand quadrant *shows a sector of the crust displaced toward the equator. Here the lessening arc of the surface will cause faults to open from the bottom. The* lower right-hand quadrant *shows a sector of the crust displaced toward a pole. Here the increasing arc of the surface results in faults opening from the top. The* lower left-hand quadrant, *which is a vertical view of a sector moving equatorward, shows major meridional faults, which have opened from the bottom. The* upper left-hand quadrant, *which is a vertical view of a sector displaced poleward, shows meridional faults opening from the top.*

The reader should visualize the left-hand quadrants as if looking straight down on the earth at the point where the central meridian of displacement (96° E. Long. in this case) crosses the equator.

NOTE: *In this and other drawings the South Pole has been shown at the top, reversing the usual position. This has been done for reasons of convenience and because our theory has been developed with the Antarctic icecap as the center of attention. In actuality, there is no such thing as "up" or "down" in space. The North Pole is usually shown at the top, but this is merely a convention of cartographers.*

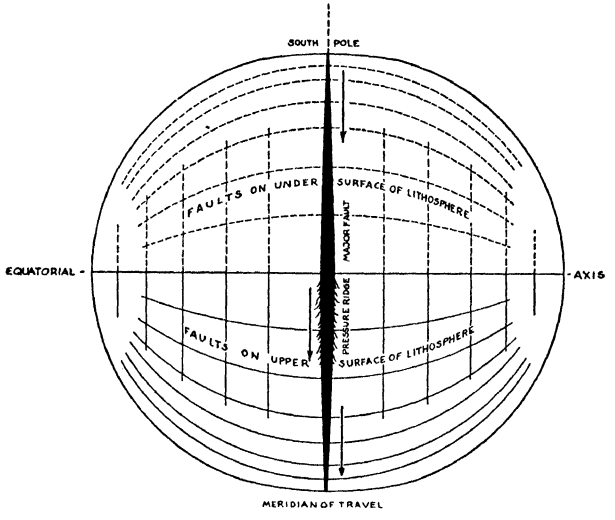

Fig. IV. Patterns of Fracture

This figure indicates schematically the mechanics of faulting and folding in a displacement of the crust. It is suggested, for purposes of illustration only, that all effects are concentrated on the meridian of maximum crust displacement. Therefore, only one major meridional fault is shown in the upper hemisphere, which is moving toward the equator. Dotted lines indicate other faults opening from the bottom of the lithosphere, or crust, as the arc of the surface diminishes.

Across the equator, where the surface is moving toward the pole, and compression is resulting, the continuation of the major expansion fault is shown as a pressure ridge, which may later become the main axis of a mountain range. Again, for purposes of illustration only, it is assumed that all folding will take place along the meridian of maximum displacement. If the major fault is filled with molten magma, and the magma solidifies, then this intruded matter, which has expanded the crust, must add to the folding in the lower hemisphere, which is moving toward a pole.

In the lower hemisphere the unbroken lines indicate the fractures opening from the top, as the arc of the surface increases.

equatorward, the fractures will tend to open from the bottom. This would, of course, favor the intrusion into them of magma from below, and, accordingly, Mr. Campbell shows them filled up (in black). At the same time, as the reader may

see, fractures in areas moving poleward would tend to open from the top. These might be less likely to reach sources of molten rock; accordingly, they are not shown filled up. Whether these fractures would or would not fill up (and perhaps the probabilities are that they would), the configuration of the resulting solidified veins in the rocks would be very different from that in fractures that had opened from the bottom. Campbell has suggested that this way of explaining existing fracture patterns in the crust could be an aid in prospecting for ores, most of which occur in such veins. It would be a question of ascertaining, for the general region, whether the veins being investigated were part of either a poleward type or equatorward type of pattern, and from this it might be possible to deduce whether the vein was to peter out or not. Campbell believes that the hypothesis provides numerous possibilities for the exploration of the crust, some of which may prove eventually to be of commercial value.

The time element is essential to visualizing the general process of a displacement. Some concept of the probable speed of the displacement is required. A basis for such an estimate is provided by evidence that will be fully considered later, but I may here anticipate by saying that displacements may have required periods of from 10,000 to 20,000 years. This means that this amount of time would be available for the creation of the system of fractures we are considering. It means, for example, that a single major fracture, which might involve, let us say, the pulling apart of the crust to a distance of several miles and the filling up of the crack with molten material from below, might be formed over a period of several thousand years, during which time there might be spasmodically renewed earthquake fracturing and volcanic effects, interrupted by periods of quiet. It is obvious that the amount of time available for the work of extension and fracturing of the crust is sufficient to permit the process to complete itself without undue or incredible violence.

We must now consider a question that relates to mountain building, and at the same time involves another of the major

unsolved problems of geology. It is connected with our phase of equatorward crust displacement. It has to do with the filling of the fractures by molten magma from below. Campbell considers that this filling of the fractures is the first step in mountain building, or at least in the formation of a geosyncline. Obviously, it is possible to start the process at other points; this is therefore only a matter of convenience, and for the purpose of drawing a clear picture of the process for the reader. But Mr. Campbell points out that the process of the filling of the cracks, and the later solidification of the intruded material, adds extension to the crust; there is now more surface. When, in future shifts of the crust, this area passes over the equator toward a pole, or moves poleward from where it is, the extended surface has to yield to the resulting compression by folding more than it would have had to do had there been no molten intrusions in the first place. It is, therefore, reasonable to call this the first step in mountain building, although there is as yet no folding, and no uplift of the rock strata.

But this question of molten intrusions into the crust raises another sore point. It has been, until now, a very difficult thing to explain the rise of molten matter into the crust. Geologists have speculated as to what force could have shot up the molten matter that formed the innumerable "dikes" and "sills," as the resulting veins are called. They have not been able to agree upon the question. No reasonable explanation of these millions of magmatic invasions of the crust has been found.

Of course, it is realized that the crust of the earth is, in a sense, a floating crust. The materials of which it is composed are lighter, it is assumed, than the materials below, and are solid, as compared with the plastic or viscous state of the underlying layers. The crust can be thought of as floating in hydrostatic balance in the semiliquid lower layer. This is generally understood among geologists. It follows logically from this that, if two or more blocks of the crust got separated with cracks between them, the "molten" material

would rise in the crack, and the blocks would sink, until the cracks were filled up far enough to establish hydrostatic balance. But this did not solve the problem; it did not help because nobody could imagine what could produce the necessary pulling apart of the blocks.

For those who like to see complicated problems made simple, Mr. Campbell's presentation of this matter is worth considering. He suggests that the concept of a great sector of the crust being stretched, and thereby fractured in innumerable places at one time, permits a comparison to be made with an ice sheet, which is floating on water, and which undergoes fracturing. Just as the individual pieces of the ice floe sink, until they have displaced their weight in water, and the water rises in the cracks between the pieces, so he visualizes the behavior of the crust during its displacement equatorward. He sees this as the explanation of the fact that although the crust is shot through with igneous invasions of all sorts, these are hardly ever known to reach the surface of the earth. He compares the behavior of the crust during displacement with the behavior of ice as follows:

> . . . As a matter of fact the lithosphere (or crust) can be likened to ice floating on water, a solid and lighter form of a substance floating in a liquid and heavier form of a similar substance. The solid and lighter substance sinks in the heavier and liquid substance until it displaces its own weight in the heavier and liquid substance and then floats with its surplus bulk above the surface of the heavier liquid, which in the case of ice would be one tenth of its bulk. To put it another way, if you were out on a lake where the ice was ten inches thick, and you were to bore a hole through the ice to the water, the water would rise in the hole to within one inch of the surface of the ice and remain there. Now, that is exactly what happens to the lithosphere. It sinks into the asthenosphere (or subcrustal layer) until it displaces its own weight of the substance of the asthenosphere and a state of equilibrium is reached. That will bring the substance of the asthenosphere far up into the lithosphere, wherever it finds an opening or a fault that reaches all the way to the bottom of the lithosphere (66).

Purely for purposes of illustration, and not as an accurate

picture of the facts, Mr. Campbell has made a very rough calculation, as follows:

> Assuming that the lithosphere is composed of granite that has a weight of 166 pounds to the cubic foot, and the asthenosphere consists of soapstone with a weight of 169 pounds per cubic foot, the lithosphere being three pounds lighter per cubic foot than the asthenosphere, it would float in the heavier asthenosphere leaving 1.775% of its volume above the surface of the asthenosphere, and as the lithosphere is assumed to be forty miles deep in this case, then 1.755% of forty miles would be .71% of a mile above the top of the asthenosphere. That is, the soapstone molten asthenosphere would rise up into the fault to within three quarters of a mile of the surface of the earth. . . . (66).

Summarizing his general thoughts regarding the effects of an equatorward displacement of a crustal sector, and the hydrostatic balance of the crust itself, Mr. Campbell has remarked:

> I think you should stress this point, for, while the geophysicists have seen faults in the earth's crust, and have seen many of these faults that they knew had been filled up from below, they didn't have any logical solution of what caused the faults, nor did they connect the faults with the formation of our mountains (66).

3. The Effects of Poleward Displacement

In the poleward displacement of sectors of the crust, compression, instead of extension, would be the rule. The magnitudes and the distribution of forces, and the time element, would, of course, be the same. Otherwise, the effects would be very different.

We should have, in the first place, some folding of the rock strata. As with the fractures, the precise locations of the rock foldings, their number, and their magnitudes would be controlled by the amount of the displacement locally, the local variations of crustal strength (which would be less where geosynclines already existed), and the distances of the areas concerned from the central meridian of movement.

The amount of the folding would be increased as the result of any previous process of extension of that area of the crust in any previous displacement.

The elastic properties of the crust would probably be of much greater importance in this compressive phase than in the extensive phase of a displacement. This is because compression could lead to flexing or bending of the crust, to a slight degree, without a permanent change of shape. It might be possible to bend or flex the crust slightly, and hold it so for thousands of years, without fracture or folding of the rock strata, or even without much plastic flow of the materials. This would mean no permanent change in the conformation of the surface. A compressive tension might be exerted for thousands of years, causing a flexing, and then be relaxed, permitting the crust to return to its original shape. It may be supposed that in areas sufficiently removed from the meridian of maximum displacement, the compressive tensions on the crust might be contained by its tensile strength, and the crust might yield elastically, without deformation. If this occurred, however, the total amount of the compression for the whole circumference of the globe would probably be concentrated at comparatively few points, where the compressive stresses happened to be in excess of the strength of the crust; here there would be a considerable amount of folding of the rock strata. It is obvious, also, that these points would tend to coincide with existing geosynclines, which would naturally represent comparatively weak zones, where the crust would be less able to withstand the horizontal stress.

Mr. Campbell suggests that in an area displaced poleward, no fewer than four pressures will be operating simultaneously on the crust. There will be, in the first place, two pressures developing from opposite directions toward the meridian of displacement. These will arise because of the diminishing circumference. Two other pressures will simultaneously develop at right angles to these, as the result of the reduced radius. Since the radius is only one sixth of the cir-

cumference, the forces will be in proportion; the folds due to the first compression will tend to be six times as long (and accentuated) as those due to the second compression. The former may ultimately correspond with the long axes of the mountain ranges, and the latter to their radial axes. The long, narrow, folded tracts referred to by Dutton are thus explained.

In Figure II Mr. Campbell has suggested an idealized representation of the formation of a mountain chain by a displacement of the crust. The reader will note the long major axis, and the shorter radial axes. That this is a fairly close approximation to the patterns of existing mountain ranges is obvious; however, a number of modifying factors must be recognized. In the first place, we do not contemplate that a mountain range can be completed in the course of one movement of the crust. It is quite obvious, from the quantitative considerations already mentioned, that a single displacement could cause comparatively little folding, even if, as the result of elastic yielding, most of the folding was concentrated in a few areas. It is certain that many displacements would be required to make a large mountain range, and since successive displacements will not necessarily occur in the same directions on the earth's surface, the resulting patterns might rarely conform to the idealized pattern. And yet, if most of the folding in one displacement happened to be concentrated in one area, and if one or more successive displacements happened to concentrate folding in the same area, a mountain range might come into existence in a comparatively short period of two or three hundred thousand years. We will return to this chronological aspect again.

It should not be thought that Mr. Campbell is in disagreement with Dutton's statement, quoted above, that the compressive mountain-folding forces have acted in one direction only on the earth's surface. This would be a misunderstanding of the case. The laws of physics require the operation of equal and opposite forces for the production of effects. A compression is the result of two equal and opposite pressures.

There is still one definite direction, such as northeast-southwest, in which the compression operates on the crust.

4. The Mountain-Building Force

An apparently formidable objection that has been raised to this theory of mountain formation is that the force provided by the icecaps cannot be sufficient. When you look at the towering summits of the Sierras from your speeding plane, and your eye takes in the numberless peaks fading away into the far horizon, you are impressed by the thought of the enormous force that must have been required to raise this great chain of mountains. How could all this have been the result of a gentle pressure applied to the crust of the earth by distant icecaps?

Campbell has shown that the apparent discrepancy between the cause and the effect, here, is the result of a misapprehension as to the identity of the actual force responsible for the mountain folding. His calculations (given and discussed in Chapter XII) show that the thrust transmitted to the lithosphere by the icecap is of the right order of magnitude to bring about the fracturing of the crust. The icecap may therefore be responsible for the movement of the crust; yet it is not the force directly responsible for the folding of the mountains. The latter is a much greater force. Mr. Campbell shows that the mountain-folding force is none other than the force of gravity itself. He suggests that the icecap performs the function merely of sliding the crust horizontally to a place where the force of gravity can act upon it. When an area is moved toward a pole, where the radii of the earth are shorter, circumference is shorter, and surface required is less, there is a surplus of surface, and this, being pulled down by gravity, must fold. From this point of view, it appears that the mountains *are not pushed up at all,* and therefore, no lifting force is required; instead, it is the surface of the earth *that is pulled down, by gravity,* nearer to the earth's center,

as a sector of the crust approaches a pole. Where this happens, the surplus surface must fold. Thus it is the force of gravity, over a large area, that folds the crust in a small area.

It may help the reader to grasp this idea if he will visualize a flat area on the equator which, in process of being displaced with the crust as far as a pole, has been folded enough to produce mountains six miles high. Now, actually, the peaks of those mountains are no farther from the center of the earth than the flat area was at the equator. Their altitude, with reference to the earth's center, is unchanged. What has changed altitude, however, is the rest of the surface, outside the mountain chain. That has been pulled down six miles. What pulled it down, obviously, was the force of gravity, and the reason it was pulled down was that it was first shifted horizontally to a place where gravity could act upon it.

Here, then, is the answer to the long-standing enigma of the source of the energy for mountain folding. The mountains are not lifted up at all; the surface is pulled down, the force of gravity does the pulling, and folding results where there happens to be excess of surface.

5. *Existing Fracture Systems as Evidence for the Theory*

It is ordinarily considered a strong argument in favor of a hypothesis if it enables one to anticipate the discovery of phenomena. Campbell has shown that the theory of displacements of the earth's crust calls for the existence of great systems of parallel fractures, intersected by other fractures at right angles to them. It was some time after Mr. Campbell began to consider this matter, and quite independently of him, that I became aware of the fact that such fracture patterns do, in fact, extend over the whole face of the globe, and that geologists are in agreement that their origin is unexplained. Many years ago Hobbs pointed out that they must have been the result of the operation of some world-wide force:

> The recognition within the fracture complex of the earth's outer shell of a unique and relatively simple pattern, common to at least a large portion of the surface, obscured though it may be in local districts through the superimposition of more or less disorderly fracture complexes, must be regarded as of the most fundamental importance. It points inevitably to the conclusion that more or less uniform conditions of stress and strain have been common to probably the earth's entire outer shell (217:163).

As I have pointed out, Mr. Campbell's projected pattern of fractures is a sort of gridiron, with major fractures paralleling the meridians, and minor fractures at right angles to them. In the actual earth's surface, however, there are two such patterns. One of them consists of north-south fractures paralleling the meridians, intersected by east-west fractures paralleling the equator. The second gridiron is diagonal to the first; the lines run northeast-southwest, and northwest-southeast. As to why there should be two such distinct fracture complexes in the crust, I shall have more to say later on. Hobbs insists that the existence of these world-wide patterns points to a cause acting globally; they could not have been the result of local causes; the force causing the fracturing must have acted simultaneously, so to speak, over a great part of the whole surface of the earth:

> . . . The results of this correlation possess considerable significance inasmuch as it is clear that over quite an appreciable fraction of the earth's surface, the main lines of fracture betray evidence of common origin. . . . (218:15).

The fracturing of the crust under the operation of some global force has been accompanied by much tilting and relative movement of blocks of considerable size, resulting in the alteration of topographic features. One of the earlier geologists, Lapworth, remarked with considerable truth, though also with some exaggeration, that

> On the surface of the globe this double set of longitudinal and transverse waves is everywhere apparent. They account for the detailed disposition of our lands, and our waters, for our present coastal forms, for the direction, length and disposition of our mountain ranges and plains and lakes (430:296).

It is clear, I think, from what has already been said, that Lapworth was in error in ascribing the folded mountains to the effects of fracturing alone. However, it may well be that formation of block mountains, such as the Sierra Nevadas, can be accounted for in this way. Innumerable other features of the crust have been formed or obviously much affected by the fracture patterns. Hobbs, for example, has maps of river systems in Connecticut and Ontario, showing how closely the rivers and their tributaries follow the lines of the fracture systems (216:226). Many river beds, many submarine canyons, were never created by subaerial erosion; they were, instead, the results of deep fractures in the crust, later occupied by rivers or by the sea. On the land erosion no doubt often, if not usually, completed the shaping of the valleys, while turbidity currents may have created or deepened canyons in the unconsolidated materials of the ocean bottom (137, 139, 140).

A succession of theories to account for the world-wide or "planetary" fracture patterns has been developed and rejected. As soon as it became clear that these patterns could not be explained as the result of local forces, the problem was recognized as very formidable. Sonder, a Swiss geologist, attempted to explain them as the result of a difference in the compressibility (or elasticity) of the rocks of the continents as compared with those under the oceans. But Umbgrove pointed out that this would call for independent fracture systems for each of the continents, whereas existing fracture patterns extend to several continents (430:300–01).

The Dutch geologist Vening Meinesz suggested that the fracture patterns could be explained mathematically by a displacement of the crust of the earth. He postulated one displacement about 300,000,000 years ago, through about 70° of latitude (194:204ff). Umbgrove rejected this theory because he saw that there were many features of the earth's surface that could not be explained by the particular displacement suggested by Vening Meinesz. This is not at all remarkable, since it is quite impossible to see how any one

displacement, and particularly one 300,000,000 years ago, can be made to explain most of the earth's present topographic features. Umbgrove was justified in rejecting the Vening Meinesz theory, but he admitted that this left him with no explanation at all. ". . . On the other hand, it means that the origin of both lineament systems remains an unsolved problem" (430:307).

Some writers have suggested that the two fracture systems originated at different times, and this is a very important point. Umbgrove says:

> It is a rather widespread belief that the origin of faults with a certain strike dates from a special period, whereas faults with a markedly different strike would date from another well-defined period. In certain areas this conviction is founded upon sound arguments. . . . (430:298).

He continues:

> Some authors, however, have doubtless overrated the relation between the direction and the time of origin of a fault system. As a typical example, I may mention Philipp, who once advanced the opinion that the direction of the principal fault lines of northwestern Europe changed from W. and W.N.W. in the Upper Jurassic toward N. or N.N.E. in the Oligocene, and thence E.N.E. in the upper Tertiary and Pleistocene. He added the hypothesis that their rotation could have been caused by a large displacement of the poles. In the meantime it has been shown that some faults with a meridional strike date from much older periods. Moreover, large and well-defined faults with a N.N.W. direction dating at least from the Upper Paleozoic appear to have been of paramount influence in the structural history of the Netherlands. Therefore Philipp's hypothesis has to be abandoned because it is inconsistent with well-established facts (430: 298).

Abandoned much too soon! The reader can easily see that the objections Umbgrove raises to Philipp's theory are removed by the present theory of crust displacements. With this assumption, it would be inevitable that, in the long history of the globe, the poles would often be found in about the same situations. If the strikes of the fault systems are related to the positions of the poles, those of later periods

would often coincide approximately with those of earlier periods.

We find in this very fact the answer to another of the mysteries of geology, the so-called "rejuvenation" of similar features in the same geographical situations at various times. The term "rejuvenation" is a commonplace of geological literature, and is especially emphasized by Umbgrove. He is puzzled by the fact that old geological features have repeatedly been called back to life. It seems that this renewal of old topographies may be explained by the accidental return of the poles to approximately the same places.

There is nothing remarkable about the fact that only two world-wide fracture systems can now be recognized in the crust. If each successive displacement produced a new grid-iron pattern of fractures and resulting surface features, it must, in addition, have disrupted the evidence of previous patterns. In a long series of displacements, the older fracture patterns must soon be reduced to an indistinguishable jumble. It is probable that the two systems now recognizable date only from the last two displacements of the crust (to be discussed later), even though many of the fractures and individual topographic features now coinciding with these systems may date from remote periods.

It is not true, of course, that in one displacement of the crust all fractures all over the earth will form a single rectilinear pattern. This can be made clear from an example. Let us suppose (as we shall in Chapter VII) that North America was moved directly southward at the end of the last ice age. Campbell has suggested that major fractures would run north and south (meridionally) and minor fractures east and west, and this would be true of the whole Western Hemisphere, which was, presumably, moved southward, and of the opposite side of the earth, which was equally displaced northward. But what about Europe? If, before the last displacement, the pole was situated in or near Hudson Bay, it seems that the last displacement must have created diagonal and not meridional fractures in Europe, for the reason that

Europe was nowhere near the meridian of displacement. Thus, in one given displacement, a meridional fracture pattern will be created near the meridian of displacement, a diagonal fracture pattern in very large areas approximately 45 degrees from this meridian, and, of course, no fracture pattern in the "pivot" areas, 90 degrees from the meridian of displacement, where no displacement will occur.

Lately, the oceanographic research work under the direction of Ewing has resulted in tracing a globe-encircling crack in the bottoms of the Atlantic, Indian, and Pacific Oceans, and has connected it with the Great Rift Valley in Africa. The pattern that has been traced out is about 40,000 miles long; it is reported that there is seismic activity at present along the whole length of the crack, suggesting recent disturbance of the area and a still-continuing process. The crack appears to average two miles in depth and twenty miles in width. The fact that it is connected with the Rift Valley in Africa, that it bisects Iceland, and apparently invades Siberia, indicates that it is not a phenomenon of ocean basins only. It is, on the contrary, clearly a global phenomenon. *The Columbia Research News,* published by Columbia University, in its issue of March, 1957, described the discovery thus:

> In January, Columbia University geologists announced the discovery of a world-wide rift believed to have been caused by the pulling apart of the earth's crust. The big rift traverses the floors of all the oceans and comes briefly to shore on three continents in a system of apparently continuous lines . . . 45,000 miles long.
>
> Throughout its vast length the world-wide rift seems to be remarkably uniform in shape, consisting of a central valley or trench averaging 20 to 25 miles in width and flanked on either side by 75 mile-wide belts of jagged mountains rising a mile or two above the valley. The peaks of the highest mountains in the system are from 3,600 to 7,200 feet below the ocean's surface while the long undersea stretches of the rift valley itself lie from two to four miles down. In addition to being marked by its topography, the globe-circling formation is the source of shallow earthquakes that are still going on along its entire length—an indication that, if the rift is due to a pulling apart of the earth's crust, the geological feature is a young and growing one.

What could be the cause of such a pulling apart of the crust? Surely not a shrinking and cooling of the earth. It is also very unlikely, it will be admitted, that the earth could be growing fast enough to produce this split and the accompanying geological instability. On the other hand, a displacement of the crust, or rather a series of them, may explain the facts. It even appears that here, in this system of profound cracks in the crust, we have evidence of the existence of zones of crustal weakness along which, perhaps time and again, the splits have occurred that have permitted the displacement of the crust, and at the same time have relieved some of the resulting tension and thereby limited the tectonic consequences of the displacements so far as other areas of the earth's surface are concerned.

To return briefly to the question of block mountains, Mr. Campbell has a further suggestion as to the way in which compression in a poleward displacement, and subsequent fracturing, may combine to cause them. One of the problems that awaits solution in geology is the cause of the widespread doming and basining of the crust that occurs from place to place. The domes are sometimes of considerable extent. Examples of basins include the Gulf of Mexico and the Caspian and Black Seas. Campbell points out that if an area is displaced poleward, and is thereby subjected to four compressions, as already mentioned, limited areas will be entrapped by these compressions, and doming must result; conversely, in areas moved equatorward the reverse must occur, and larger or smaller basins will tend to be produced.

A block mountain might tend to be produced, Mr. Campbell thinks, if a major fault should bisect a domed-up area. This would create the possibility that the abutting rock sections of one half of the dome might give way, allowing half the dome to collapse, and pushing subcrustal viscous or plastic rock under the other half of the dome, thus rendering the latter permanent. This effect, however, would depend upon many local circumstances.

PART II. Volcanism and Other Questions

In the preceding part of this chapter we have sketched the principal problems that are basic to the formation of the folded mountains and block mountains, and have examined the planetary fracture systems in the light of Campbell's mechanism for crust displacement. There are, however, a number of other aspects of this general problem that must now engage our attention. We must consider, in turn, the remarkable phenomena of volcanism, in their relationship to crust displacement. In connection with the creation of volcanic mountains we must consider briefly the question of the origin of the heat of the earth, an unsolved problem of great interest. We must then examine the relationship of crust displacement and mountain building to the question of changes in the sea level. Finally, we must consider the problem of the chronology of mountain building.

6. *Volcanism*

We have seen that one kind of mountain is the volcanic mountain. Volcanic phenomena cover a wide range; all of these must be considered in order to see how far they can be related to a general cause. The phenomena that need explaining include volcanic eruptions, the creation (sometimes rapid) of volcanic mountains on land or in the sea, the genesis of volcanic island arcs, and last but not least the vast lava flows or lava floods that have at times in the past inundated great areas of the earth's surface.

Since volcanoes occur frequently, and are the most dramatic manifestations of volcanism, they have been thoroughly studied, and a whole literature has been devoted to them. It is astonishing, therefore, that neither the causes of volcanoes nor the present distribution of volcanic zones on

the earth's surface has as yet received an acceptable explanation. As in the case of other unsolved problems, the absence of certainty has led to a multiplicity of theories. Jaggar, one of the best field observers of volcanoes, refers to the two leading theories thus:

> It would be hard to imagine any more completely different explanations for the same phenomenon than is R. A. Daly's doctrine of the causes of volcanic action, as compared with the crystallization theory of A. L. Day (235:150).

Dr. A. L. Day was formerly director of the Geophysical Laboratory in Washington; his theory is based upon geophysical experiments conducted in the laboratory. He observed that the crystallization of rock from the molten state resulted in some increase in volume. He assumed that the whole crust was once molten, and that as it cooled it continued to contain, here and there, comparatively small pockets of molten rock. When such pockets of molten rock finally were cooled to the crystallization point, then expansion would occur, and great pressures would be set up, which might lead to eruption at the surface. This theory is based upon the assumption of the molten origin of the earth, and carries with it the corollary that volcanic eruptions are essentially local phenomena. Dr. Day insisted that volcanoes were not connected with a molten layer under the crust, and were not related to events occurring over large areas.

Professor R. A. Daly based his opposed theory on his observations of the field evidence of geology. He insisted that only the assumption of a molten layer under the crust could account for the countless facts of igneous geology. His theory is reconcilable either with the assumption of the molten origin of the globe or with the theory of a growing and heating earth.

Jaggar objects to Day's view that volcanoes are purely local. He says:

> There is some reason to think that a very long crack in the bottom of the Pacific Ocean, with interruptions by very deep water, extends

> all the way from New Zealand to Hawaii, because there are striking sympathies of eruptive data between the volcanoes of New Zealand, Tonga, Samoa and Hawaii (235:23).

He lists a number of eruptions with their dates to show their intimate connection. In particular he mentions the eruption of August 31, 1886, on the island of Niuafoo, Polynesia:

> . . . Only two months before, Tarawere Volcano was erupted disastrously in New Zealand, indicating volcanic sympathy between two craters hundreds of miles apart on the same general rift in the earth's crust (235:95).

These observations imply that a connection may exist, at least in some cases, between volcanoes at great distances from each other, because of their being located along the same crack in the earth's crust. This implies a connection between the deep fracturing of the earth's crust and volcanism. We have seen that Columbia scientists have just discovered a vast connected system of rift valleys, or cracks in the crust, extending over the surface of the whole planet, and associated at the present time with constant seismic disturbances. Jaggar makes it clear that volcanic eruptions, as well as earthquakes, may be associated with such rifts. Since the crust is relatively thin, it is reasonable to suppose that the molten rock erupting in volcanoes at great distances from each other must come from below the crust, and that it is not created by any processes occurring within the crust itself. All this is confirmation of Daly's position.

Another theory of volcanic action that should be mentioned briefly is that associated with the name of W. H. Hobbs. It was his view that volcanic action could result from horizontal pressure arching up a sector of the crust. This is based on the fact that if a rock that is too hot to crystallize at normal pressures is subjected to great pressure, it may take the solid state. Subsequently, the release of the pressure is all that is required to restore the rock to its liquid condition. In the earth's crust considerable amounts of rock may be held in the solid state by the pressure of overlying strata. Then, if horizontal pressure arches the crust, the

pressure on the rock below will be relieved, and the rock will resume a liquid state. If the arching results in cracking at the surface, or in sufficient lateral squeezing of the liquid pockets, eruption may take place. This effect may account for the vast masses of igneous rock that are found associated with the folded mountain ranges (215:58), but it is necessary to ask the question, What causes the arching of the crust? Obviously, volcanism, according to this theory, must be traced to the cause of the arching. Hobbs's theory is not very satisfactory because he cannot explain the arching.

It is clear that volcanism might occur as the result either of the process imagined by Day or of that imagined by Hobbs, for several different causes might produce liquid pockets in the crust. But it is equally clear that neither they nor Daly has advanced a theory to account for volcanoes, volcanic zones, plateau basalts, and volcanic mountains. Einstein, when he first received some material outlining the theory proposed in this book, wrote me that it was the only theory he had ever seen that could explain the volcanic zones (128). These, of course, can be explained as zones of fracture (such as the rift valleys just mentioned) resulting from crust displacements.

7. *The Volcanic Island Arcs*

Campbell has suggested an explanation for the formation of the volcanic island arcs, so many of which are found in the Pacific, and which consist of chains of volcanic mountains in the sea. He shows not only how our theory of crust displacement may account for the formation of these volcanic mountains but also how it may account for their occurrence in graceful curves:

> As a sector of the lithosphere, or crust, moves toward the equator, the motion is fastest and the tension is greatest on the meridian of movement, and great north-south faults will open up, beginning there and spreading east and west. At the same time transverse faults

of the earth. Smart, for example, is of the opinion that radioactivity cannot produce heat in the earth as fast as it can be radiated through the crust into outer space (396:62).

Some of the essential facts in any consideration of the question of the origin of the earth's heat may be summarized as follows. First, we know nothing of the temperature of the earth's interior. We can only guess at it. We have made deductions concerning it from the heat gradient observed in the world's deepest mines. As we descend to a depth of about four miles, the heat steadily increases (194:139). We have assumed that the increase of heat continues at the same rate farther down, perhaps all the way to the earth's center, but there are a number of facts that throw doubt on this assumption. For one thing, Daly thought he saw evidence that the heat gradient differs in America and in Europe, being somewhat steeper in North America (194:139). This would imply that there is more heat in the earth's crust in North America than there is in Europe. Benfield produced much more evidence of variations in heat from place to place (28) which are difficult to reconcile with a uniform heat gradient in the earth.

Geophysicists have now concluded that the earth's heat originates in the crust itself, and does not come from the deep interior (194:157). The considerations on which this conclusion is based are too technical for discussion here, but there seems to be no reason to doubt their validity. This conclusion is, of course, irreconcilable with any assumption that the earth's heat is simply the remnant of far higher temperatures prevailing in a molten stage.

A matter of great importance for the general problem is the rate at which heat migrates through the crust, and is dissipated into outer space. Geophysicists have determined that the rate of heat migration through the crust is extremely slow. Jeffreys calculated that it would take 130,000,000 years to cool a column of sedimentary rock 7 miles below the earth's surface by 250° C. (241:136). As a result of this, the climate of the earth's surface is determined entirely by the

radiant heat of the sun, and is uninfluenced by heat from within the earth. We shall have to consider the bearing of this on another well-known fact, which is that earthquakes, and other movements within the crust, are known to produce heat as a consequence of friction between the moving crustal blocks (194:158). Then, earthquakes are most frequent in areas where there are distortions of the gravitational balance of the crust, while heat gradients are steeper in such areas (194:141). This indicates that any factor causing such distortions may be a factor in the production of the earth's heat.

Considering these facts, what are the implications, so far as the earth's heat is concerned, of a displacement of the earth's crust? Can there be any doubt that a crust moving slowly over a period of a good many thousand years must generate an immense quantity of heat within itself? There can be no doubt of this. The widespread fracturing, the friction between crustal blocks, resulting from the increased number of earthquakes, could have no other result. Moreover, Frankland has pointed out that friction between the crust and the layer over which it moves must produce heat, which may itself facilitate the displacement (168).

The heat thus produced would migrate both inwards into the body of the earth and outwards into space. But, since the rate of dissipation of this heat is so extremely slow, it follows that displacements at relatively short intervals might produce heat more rapidly than it could be dissipated. Over hundreds of millions of years slight increments of heat from this source may have accumulated to produce the earth's present temperature. The assumption of frequent crust displacement thus suggests a third possible source of the earth's heat, in addition to those mentioned by Gutenberg.

If it is true, as Daly thought, that the heat gradient is steeper in North America than in Europe, this fact serves as additional confirmation of a displacement of the earth's crust at the end of the Pleistocene. Later I shall present evidence to suggest that the crust moved at that time in such a direction as to bring North America down from the pole

to its present latitude. If this occurred, it meant a displacement of about 2,000 miles for eastern North America, but of only about 500 miles for western Europe. Quite obviously, crust adjustments and resulting friction must be proportional to the amount of the displacement, and therefore friction and resulting heat could be expected to be somewhat greater in America.

To return, now, to our plateau basalts, we may observe that, in a situation where the crust of the earth was continuously in motion over an extended period, a build-up of heat in the crust might cause considerable melting in its lower parts where the temperature was already very close to the melting points of the rocks. This increase of heat would link itself quite naturally, therefore, to an increase in the number and intensity of volcanic eruptions, and to lava flows of all kinds. By means of these eruptions and flows some of the heat would be dissipated into the air; much of it, however, imprisoned in the lower part of the crust, would simply increase the volume of the molten magmas.

While the increase of heat in the crust would naturally favor larger lava flows, another factor would create the possibility of massive flows, or lava floods. A massive displacement of the crust, because of the oblateness of the earth, must produce temporary distortions of its shape, and of the gravitational balance of the crust. The force of gravity subsequently must gradually force the crust to resume its normal position. This, of course, involves great pressure upon the crust, and upon the molten or semimolten liquid material under or within the crust. Pressures of this kind might occasionally lead to the eruption of plateau basalts. The probable magnitude of the distortions of the crust resulting from displacement will be considered in detail later on. It must not be supposed, however, that every displacement of the crust must inevitably produce lava floods. The latter would perhaps be the result of an unusual combination of pressures and fractures. The same combination of forces which might, in one situation, produce volcanic mountains and island arcs

might, under other circumstances, produce a doming up of the crust in a local area or a lava flood.

9. *Changing Sea Levels*

An important problem closely related to that of mountain building is that of the cause of very numerous, and in some cases radical, changes in the elevations of land areas relatively to the sea level. Umbgrove finds that mountain folding has been related, in geological time, with uplift of land areas, or with withdrawal or regression of the sea (430:93). However, it is clear that the uplifts were not confined merely to the folded areas, that is, to the mountains themselves, but affected large regions. Such uplifts, where whole sections of the earth's crust were elevated without being folded, are referred to as epeirogenic uplifts, to distinguish them from the uplifts of the folded mountain belts which may have resulted from the folding itself, and which are referred to as orogenic uplifts. As to the extent of the resulting changes in sea level, Umbgrove says:

> . . . The most important question concerns the depth to which the sea-level was depressed in distinct periods of intense regression, in other words, the extent of the change to which the distance between the surface of the continents and the ocean floors was subjected during the pulsating rhythm of subcrustal processes. Joly was the only one who approached this question from the geophysical side, and he arrived at an order of 1000 meters. . . . (430:95).

It becomes necessary, therefore, to find a connection between the cause of the folding of the crust and the cause of general, or epeirogenic, changes of elevation of continents and sea floors. Fortunately, this problem is not really so difficult as it may seem at first glance. That it can be solved in terms of the assumption of displacements of the earth's crust is, I think, clear from the following considerations.

Gutenberg has pointed out that if a sector of the crust, in gravitational equilibrium at the equator, is displaced pole-

ward by a shift of the whole crust, it will be moved to a latitude where gravity is greater, because gravity increases slightly toward the poles. Its weight will be thereby increased, and to remain in gravitational equilibrium it must seek a lower level: it must subside. The water level in the higher latitude adjusts easily, of course. Gutenberg points out, however, that if the movement of the crust occurs at a rate greater than the rate at which the sector may sink, by displacing viscous material from below itself, the result will be that the sector will stand (for a time) higher relatively to sea level than it did before. I give Gutenberg's own words:

> Movements of the earth's crust relative to its axis must be accompanied by vertical displacements. A block with a thickness of 50 kilometers in equilibrium near the equator should have a thickness of 49.8 near the poles to be bounded by the same equipotential surfaces there. If it moves toward a pole, it must sink deeper to keep in equilibrium. If the process is too fast for maintenance of isostatic equilibrium, positive gravity anomalies and regressions are to be expected. Thus regression may be an indication that an area was moving toward a pole, and transgressions that it was moving toward the equator (194:204–05).

According to Gutenberg, an area moved about 6,000 miles from the equator to a pole would stand about 1,200 or 1,400 feet higher above sea level, if the speed of the displacement was too rapid for maintenance of gravitational equilibrium. The speed of displacement that is suggested by the evidence to be presented later is such as to eliminate entirely the possibility that the crustal sector could sink and remain in gravitational equilibrium. Consequently, by our theory, a poleward movement of any sector of the crust will result in uplift, and in regression of the sea. In addition, it appears to me that since any sector displaced poleward would also be compressed laterally, this would offer another obstacle to its subsidence. It would have to overcome the lateral pressures, as well as displace underlying material.

The amount of the uplift of an area displaced poleward would depend, of course, on the amount of the displacement. As will be made clear later, much geological evidence ap-

pears to suggest that displacements may have amounted, on the average, to no more than a third of the distance from a pole to the equator. If this is true, then the resulting uplift to be expected should be of the order of about one third of the uplift he suggested, or from 400 to 500 feet. We shall see, later, how well this agrees with the evidence.

There is another factor that would operate in the same direction as the effect mentioned by Gutenberg, to alter the elevation of land areas and sea bottoms. Unlike the gravitational effect, however, this second factor would tend to a permanent change in sea levels, and might therefore, cumulatively, result in important changes in the distribution of land and sea. It is a question of the permanent consequences of the stretching or compression of the crust. As we have seen, an area displaced poleward must undergo compression because of the shortened radius and circumference of the earth in the higher latitudes. This compression must result in the folding of rock strata, which will be likely to occur mainly in areas where the crust has already been weakened by the formation of geosynclines. The effect of the folding will be to pile up the sedimentary rocks that have been formed from sediments deposited in the geosynclines, causing them to form thicker layers. These thicker layers of lighter rock will tend, even after gravitational adjustments have taken place, to stand higher above sea level. The effect of one displacement in this respect would be slight, but the accumulation of the effects of many displacements through millions of years could lead to extremely important changes in the distribution of land and sea areas. Numerous displacements of the earth's crust could, in fact, constitute an essential, and perhaps even the basic, mechanism for the growth of continents.

Equally important for the general question of sea levels are the effects to be expected from a displacement of a sector of the crust toward the equator. Here the crust will be subjected to tension, or stretching. We have already noted that in this process innumerable fractures will be created in the

crust, and these will tend to be filled up with magma from below. Since this magma, invading the crust, will average higher specific density than the rocks of the crust, it may increase the general weight of the crust, and thus depress it, causing a deepening of the sea. This would not occur if the separated blocks simply sank in the underlying magma until they displaced their own weight, in the manner suggested by Campbell. In that case, the crust would weigh no more than before. It seems, however, that volcanic activity is accomplished by very complex chemical processes, and by the absorption of vast quantities of lighter rock and its transformation chemically into heavier rocks, to the accompaniment of much throwing off of gases into the atmosphere. It is also true, as we have noted, that massive lava flows may occur on the sea bottoms (or even, perhaps, within the crust, at points below the sea bottoms) as a result of displacement of the crust. These could have the effect of weighting the crust. Moreover, an equatorward displacement of an area must result in a gravitational effect opposite to that of the poleward displacement mentioned by Gutenberg. In this case, the crust must rise to achieve gravitational balance. In so doing it may have to draw into itself a considerable amount of the heavier rock underlying the crust. This obviously would tend to weight the crust.

The foregoing factors, added together, may account for the observed deepening of the oceans, and the increase of their total surface area, from the poles to the equator. A careful survey indicates that this deepening is on the order of one kilometer or, perhaps, 4,000 feet (233).

There is still another factor that may affect sea levels, but in an unpredictable way. It seems clear, for several reasons, that a displacement of the earth's whole crust must result in considerable readjustments and redistribution of materials of different densities on the underside of the crust. While these can hardly be predicted, they must affect the elevation of points at the earth's surface.

Geologists believe that the underside of the crust has un-

evennesses, corresponding to those at the surface, and that the crust varies considerably in thickness from place to place. They think, for example, that the crust is thicker under the continental surfaces, and thinner under the oceans, and that it is thickest of all under mountain ranges and high plateaus. Continents and mountain ranges not only stick up higher but they also stick down deeper. That is because they are composed, as an average, of lighter rock. The analogy is to an iceberg. An iceberg floats with one tenth of its mass above sea level, and nine tenths of it submerged. It is lighter than water per unit volume, and floats in the water displacing its own weight, and leaving its own excess volume above the surface. Continents and mountain chains, composed on the average of lighter rock, stand in the same sort of hydrostatic, gravitational balance, and their downward projections are thought to be much greater than their upward, visible projections. The downward projections of mountain chains are called "mountain roots."

The underside of the crust, then, has a sort of negative geography. The features of the upper surface are repeated in reverse on the undersurface, although, naturally, the details are missing. The effects are rather smoothed out. We should expect that the Rocky Mountains would make a sizable bump on the underside of the crust, but we couldn't expect to find any small, sharp bump just under Pikes Peak. The tensile strength of the crust, though limited, is sufficient to smooth out the minor features.

As we attempt to envisage the situation at the bottom of the crust, we must remember that the rocks are subjected to increasing pressure with depth, and probably to increasing heat, and as a consequence they must tend to lose their rigidity and strength. We don't just come suddenly to the boundary of the crust at a given depth. On the contrary, the crust just fades away. The rocks of the lowest part of the crust must be very weak indeed, so that a very slight lateral pressure may suffice to displace them.

It follows that when lateral pressures develop during a displacement of the crust, as the downward projections of continents and mountains are brought to bear against the upward extension of the viscous layer below the ocean basements, large blobs of this soft rock of lesser density will be detached from the undersides of the continents, or mountain ranges, and will get shifted to other places. If, as a result of this shifting around, the average densities of vertical columns extending from the bottom to the top of the crust get changed, then there will eventually be corresponding changes of elevation at the surface. Some areas might, as a result, tend to rise, and others to sink. This could account, naturally, for changes of sea level, and for many topographical features such as basins and plateaus.

To sum up the question of sea levels, it appears that the assumption of displacements of the crust (especially if they are considered to have been numerous) may help to explain them. It seems able to explain why glaciated areas (which we consider to have been areas displaced poleward) appear to have stood higher relatively to sea level, and why periods of warm climate in particular regions appear to have been associated with reduced elevation of the land, and transgressions of the sea. The theory seems to satisfy Umbgrove's conclusion that sea-level changes have resulted from some "world-embracing cause" (430:93). It accounts, too, for Bucher's suggestion that regressions of the sea have resulted from subcrustal expansion, and transgressions from subcrustal contraction, for this, obviously, is only another way of looking at a displacement of the crust (58:479). (If an area is displaced poleward, the effect of subcrustal contraction is created; if it is displaced equatorward, the effect of subcrustal expansion occurs.) At the same time it provides an explanation for the rhythmic changes of sea levels through geological history that so mystified Grabau:

> This rhythmic succession and essential simultaneousness of the transgressions as well as the regressions in all the continents, indicates

a periodic rise and fall of the sea-level, a slow pulsatory movement, due apparently to alternate swelling and contraction of the sea-bottom (183).

10. Some Light from Mars

Some very significant facts emerge from recent studies of other members of the solar system, especially from the work of Dr. Harold Urey, *The Planets: Their Origin and Development* (438). This is the work in which the theory of accretion of planets is developed, in contradiction to the older theory of the cooling globe. Dr. Urey also discusses the present state of knowledge regarding the structure of the moon and Mars.

It appears that there are mountains on the moon, but in Dr. Urey's opinion these have been created by collisions with minor celestial bodies. Where the colliding body hit the moon more or less head on, craters (the largest more than 100 miles across) were formed. Planetesimals that merely grazed the moon's surface left long ridges and valleys. Where the heat created by the impacts caused extensive rock melting, vast lava floods apparently took place, which cooled off, in tens or hundreds of thousands of years. The absence of air and water has resulted in an absence of erosion on the moon's surface, so that the features created by the collisions have not been obliterated except in cases where the lava flows have swamped them.

In the case of Mars, the story is different. Urey assumes that Mars was once like the moon, both in size and in surface features. The removal of these features, which no longer exist, he thinks must have been due to the work of atmosphere and water. He gives reasons for believing that Mars did have more water at one time, but that it escaped from the planet by a process that is also going on, more slowly, on earth. He states:

> . . . The surface appears to be smooth, a condition most easily explained as due to the action of water during its early history and no mountain building since then (438:65).

In another place he says:

> . . . Mars appears to have no high mountains, and it is difficult to understand this unless it had some initial water. (In order for it to remain without mountains no folded mountains must have been formed subsequently; but this is another subject.) The formation of Mars and its surface followed a course similar to that of the earth (438:118).

So we see that Mars and the earth appear to have followed similar courses of development. They are similar in chemical composition and in structure, and have similar atmospheres. There are, apparently, only two important points of difference, other than size. Mars, unlike the earth, has very little water, so that its polar icecaps are thought to be no deeper than hoarfrost, and disappear entirely in summer; and Mars, unlike the earth, has no folded mountains.

A thought-provoking fact: on Mars, no great icecaps—and no folded mountains, no volcanic phenomena, no fault mountains! Surely this is no coincidence. Surely, it is suggestive of the fact that these features on earth have been the consequence of displacements of the crust, and that these displacements have been owing to the agency of great polar icecaps. It might seem, at first glance, that the absence of folded mountains on Mars might be explained by the absence of deep accumulations of sediments produced by the weathering of rocks under the action of water, and the accumulation of these sediments in geosynclines with subsequent folding, but we have seen that geologists do not claim to explain the original creation of the geosynclines, nor to identify the source of the compressive stress that brings about mountain folding.

11. Undisturbed Sections of the Crust

It has been objected that over extensive areas there are rock formations that appear to have been little disturbed over very great periods of time. If the crust has been displaced

as often as is required by this theory, why would not the crust be universally folded to a far greater extent than it is?

I think this objection has been partly answered where I pointed out that in a single displacement of the crust the folding would be comparatively slight, and that it would be confined to a small part of the earth's entire surface. I have suggested that it would be greatest along the meridian of the crust's maximum displacement, but that at some point between this meridian and the two areas suffering no displacement, the compressions would tend to fall below the elastic limit of the crustal rocks, so that the crust would simply bend elastically, and then return to its original, apparently undisturbed position, in some subsequent movement. It may be added that most of the changes of elevation resulting from a displacement of the crust would tend to be epeirogenic—that is, they would be broad uplifts or subsidences of large regions resulting from the tilting of great segments of the crust, rather than merely local deformations of the rock structures.

Another point that may be urged in answer to this objection is that, apparently, over considerable periods the poles have tended to be situated again and again in approximately the same areas, possibly owing to the configuration of the continents. This would result in leaving some areas far removed for long periods from the meridian of maximum displacement of the crust.

12. *The Chronology of Mountain Building*

Another objection that may be raised to this theory of mountain building is that there are supposed to have been only a few great mountain-making epochs in the world's history of two or more billion years, and that these epochs have been separated by very long periods when mountains were eroded away, and no new ones made. I shall indicate two reasons for holding that this concept is an illusion.

The first reason is that the record of the rocks is incom-

plete. It has been estimated that if all the sedimentary beds of all geological periods were added together (that is, the entire amount of sediment that has been weathered out of the mountains and continents and accumulated to make sedimentary rocks since the beginning of geological time), the total thickness of sediment would be about eighty miles. At the present time, however, the average thickness of the sedimentary rocks of the upper part of the earth's crust is estimated to be no more than a mile and a half (33a). What has happened to all the missing sediment? The answer is that it has been used over again. At the present time, all over the earth, the forces of the weather and the sea are busy wearing away or grinding up rock, and most of the rock they are destroying is sedimentary rock. Thus more than 95 per cent of all the sedimentary rocks formed since the beginning of the planet has been destroyed. As a result of this, geologists have been forced to piece together this geological record from widely separated beds. They find a part of the Silurian sediment in the United States, and another part in Africa, and so on.

The enormous difficulty of piecing together the geological record from these discontinuous and scattered beds is rendered even greater by the fact that vast areas of what were once lands are now under the shallow epicontinental seas, and even under the deep sea (as we shall see in the next chapter). Let us remember, too, that even among the still-existing beds now to be found on the lands, only a tiny percentage are at or near the surface and thus available for study. And of these a large proportion are in such remote and geologically unexplored areas as Mexico, the Amazon, and Central Asia. And still, despite these enormous handicaps, new periods of mountain formation are constantly being discovered. Umbgrove remarks that a long list of them has been "gradually disclosed to us" (430:27). It seems to me that there is unjustifiable complacency in the assumption that the list of mountain-forming epochs is now complete.

How can we reach a reasonable guess as to the number that remain undiscovered?

The second reason for holding that the idea of rare mountain-building periods is quite illusory is perhaps even more persuasive. It seems that a remarkable error has vitiated the interpretation of the evidence regarding these alleged periods. The error has been exposed by the development of nuclear methods of dating recent geological events, already referred to, and to be discussed more fully later. These have revealed an unexpectedly rapid rate of geological change. The error, I think, consists in interpreting the geological evidence on the assumption that conditions as revealed in a particular deposit in one area necessarily determine world-wide conditions. Thus, evidence of an ice age in a particular deposit in one place has been interpreted as meaning a period of lowered temperature for the whole world at that time. In the same way, mountain-building revolutions were assumed to affect all parts of the world at once. The idea that mountain building might go on on one continent while another went scot-free was not entertained.

The contemporaneousness of these events in different parts of the world rested, as we shall see, upon a very vague idea of geological time. The techniques for dating the older geological formations never did, and do not now, allow reliable conclusions regarding the contemporaneousness of mountain building on different continents any more than they permit such conclusions regarding climatic changes. Margins of error amounting to millions of years must always be allowed. Triassic folding in India need not be contemporary with Triassic folding in North America, because the Triassic Period is estimated to have lasted about 35,000,000 years! Calculations of the rates at which the weather wears away mountains have shown that mountain ranges may be worn away in much less time than that.

Thus, we cannot place reliance on the accepted notions of the occurrence of mountain-building revolutions in time and space, but must hold that the process was, in all probability,

much more continuous than has been supposed, but confined to smaller parts of the earth's surface at any one time. Further support for this view is provided by the geologist Stokes, who remarks, in connection with the history of the Rocky Mountains:

> Although the Rocky Mountain or Laramide Revolution is popularly supposed to have occurred at the transition from the Cretaceous to the Tertiary, it has become increasingly evident that mountain building was continuous from place to place from the late Jurassic or early Cretaceous and that deformation continued through the early Tertiary and Quaternary (405:819).

In other words, mountain building went on continuously in North America from the Jurassic Period, about 100,000,000 years ago, into the Pleistocene Epoch, which is considered to have come to an end 10,000 years ago! This is excellent evidence in support of the conclusion that, in all probability, none of the alleged mountain-building revolutions occurred in widely separated periods, with long, quiet periods in between.

Krumbein and Sloss point out that this view is, in fact, becoming widely accepted by geologists. They remark that "Gilluly . . . recently examined the evidence for and against periodic diastrophic disturbances, and he showed that such disturbances are much more nearly continuous through time than is generally supposed," and they conclude:

> Added complexity arises as additional stratigraphic studies afford data which imply that tectonic activity is continuous through time. The classical concept that a geological period represents a long interval of quiescence closed by diastrophic disturbances is not fully supported by these newer data (258:343).

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V : CONTINENTS AND OCEAN BASINS

1. The Central Problem

None of the mysteries of the earth is more baffling than the question of the origin and history of the continents and ocean basins. One of the most useful applications of the theory of crust displacements will be its application here.

As with the problems already considered, there are many theories that are in violent conflict. The conflict is broad and deep, and since it involves two or three branches of science, which have adopted antagonistic points of view, it may even be called a civil war in science. It is fought over one issue: whether the present continents and deep ocean basins have been permanent features of the earth's crust since the formation of the planet, or whether they have not.

It would take too long to review the history of this war, for it extends far back into the nineteenth century. Instead, it will suffice to outline the principal positions adopted by the antagonists. Before we do this, however, it is essential to emphasize that most contemporary geologists, knowing the mystery surrounding these principal features of the earth's surface, have refrained from making very positive statements. Professor Daly, for example, after admitting that the formation of continents could not be accounted for under the theory of the solidification of the crust from an originally molten state, remarked that

> We are now face to face with a principal mystery of nature. Actually, the earth's substance *is* differentiated into the form of continent overlooking deep ocean basin. That obvious, infinitely important fact, dry land on a continental scale, has to find its place in any theory of the earth. The problem is as difficult as it is fundamental. . . . (98:305).

Daly goes on to say that all he is able to offer on the subject is a guess, but that any reasonable guess is better than simply

avoiding the issue, which, he observes, is the course too often taken.

Daly's own guess will not do for us, because it is based both on the theory of an originally molten globe and on the theory of drifting continents. He suggests that when the earth was entirely molten, the lighter rock, which now forms the granitic foundations of the continents, was floating on top of the heavier rock, of basaltic composition, and crystallized first, making a thin layer over the planet's whole surface. Then, for some reason (not entirely clear) all this lighter rock slid toward one hemisphere and piled up, making a supercontinent. Later this supercontinent broke up and drifted apart, as suggested by Wegener.

Jeffreys also refers to the difference between the average chemical composition of the continents and that of the ocean floors. There is a difference in the densities of the two kinds of rock, and this is the reason why the continents stand high and the ocean beds are low. But what brought about this difference of composition is itself unexplained. It is, says Jeffreys,

> . . . closely connected with the great problem of the origin of the division of the earth's surface into continents and ocean basins, which has not yet received any convincing explanation (239:159).

Professor Umbgrove also has admitted that the field is wide open to any reasonable speculation. He feels that he is confined to mere guessing, but justifies what he writes thus:

> . . . But why should we not enter [this field] if everyone who wants to join us in our geopoetic expedition into the unknown realm of the earth's early infancy is warned at the beginning that probably not a single step can be placed on solid ground? (430:241).

In view of this state of affairs, I shall not apologize if, at times, in the course of this and the following chapter, I shall seem to the reader to be venturing beyond the point where speculation can be immediately checked by the facts. To a certain extent, my suggestions will be simply logical deduc-

tions from the general theory of crust displacements, and may be incapable, at least at this stage, of direct proof.

2. *The Views of the Geophysicists*

In this civil war between the sciences, the first group I shall call upon to present their side of the case are the geophysicists. Now the geophysicists, by and large, have very definite views about the continents, even though they cannot explain their origin. Their consensus is that the continents have been permanent features of the earth's crust from the "beginning," and this involves an equal permanence for the ocean basins. Changes of sea level there have been: so much cannot be denied; but according to the geophysicists these can have been only relatively important. At times the continental shelves (the narrow strips along the coasts where the water is only a few hundred feet deep) have been laid bare, and at other times the oceans have invaded the low parts of the continents, but such changes (while unexplained) have been slight; they have not affected the main masses of the continents. The continents, then, are original features dating, in their present positions, from the unknown beginnings of the planet.

Of course, geophysicists would never make such broad statements as these, unless they had what seemed to them sufficient evidence. Their argument is easily stated. They point to the differences in composition. The continental rock is less dense, on the average, than the rock under the oceans. The force of gravitation brings all sectors of the earth's crust into rough balance, and this means that the lighter parts will stick up higher, like pieces of wood or ice floating on water. The continental sectors of the crust are considered to be both lighter and thicker than the oceanic sectors. The greater thickness makes up for the less density, so that things balance off.

This principle of the gravitational balance of the crust is

referred to as the principle of "isostasy." We shall see, later, that there are some serious difficulties with the general theory of isostasy, as a consequence of which it cannot be regarded as definitely established. Still, on the whole, there is much to be said for it. And so the geophysicists ask how can anything alter the major concentrations of lighter or heavier rock, which, according to the theory of isostasy, must determine the locations of continents and ocean basins? A continent could not be destroyed without getting rid of a large amount of lighter granitic and sedimentary rock, and a new continent could not be raised up without producing a vast amount of new rock of that kind. Since these things are impossible, changing continents around is impossible, and the less said about it the better.

3. *The Views of the Biologists*

While the geophysicists were developing these ideas, based upon laboratory experiments and principles of physics, the biologists and paleontologists were busily engaged with an entirely different question, which led them to diametrically opposite conclusions. They were classifying and comparing plants and animals from all parts of the world, those living today and those that lived in ages past. They were soon confronted by the fact that in many cases the same species of plants and animals could be found on lands separated by whole oceans. How was this to be explained? It could not be maintained that all these forms of life—snails, grasshoppers, ferns, fresh-water fish, and elephants—had all built rafts, like *Kon-tiki,* in which to cross the Pacific. Nor was it possible to explain the distribution of all sorts of species by means of ocean currents, migratory birds, or winds. Behring Strait would not do either, because the plants and animals of the warm climates could hardly be tempted to chance the rigors of the Arctic merely to reach America or to escape from it.

As we have seen, when the paleontologists studied the life of the remote past they found the same thing. The distributions of the fossil plants and animals did not seem to pay any attention to the present shapes or positions of the continents. Therefore, not knowing what the geophysicists were up to—or not caring—the biologists and paleontologists decided between themselves that they were in need of some new continents, or rather, in need of some old, now nonexistent continents, or at least a large number of former land connections across the present oceans. And so they went right ahead, and invented them.

Wegener, of course, managed to explain a mass of evidence by moving the continents (450:73–89). Since the refutation of his theory, the same evidence needs a new explanation. Dodson gave the evidence for a North Pacific land bridge (not Behring Strait) based on the distributions of 156 genera of plants (115:373). Gregory also produced evidence for a Pacific land bridge (191). DeRance and Feilden presented the evidence for a land connection between North America and Europe in the early Carboniferous (319:II, 331–32). Coleman, basing himself no doubt on paleontological evidence, remarked that "India has many times been connected with Africa" (87:262).

Dodson reconstructed the history of the Isthmus of Panama, as indicated by the distributions of fossil plants and animals. According to him, North and South America were connected in the Cretaceous and in the early Paleocene, but later in the Paleocene, Panama was completely submerged. During the following Eocene and Oligocene there were islands but no continuous land in the area. The islands were completely submerged late in the Oligocene. Land connection between the continents was re-established in the Pliocene, that is, very lately (115:375–76). It must be emphasized that the explanation for all these land changes is missing. There is no basis in the geological evidence for the assumption that the isthmus was never more than just barely submerged. There is no reason to exclude the possibility that it was rather

deeply submerged at times. Later I shall show that the problem cannot be solved by any theory that the melting of ice-caps in "interglacial periods" periodically raised the water level.

One writer, who is considered a very special authority on the climates of the past, Dr. C. E. P. Brooks, gave a list of continents that must have existed about 300,000,000 years ago, if the distribution of plants and animals at that time is to be explained. He even names them (52:247–51):

a. Nearctis, a "primitive North American continent."
b. North Atlantis, including Greenland and western Europe.
c. Angaraland, occupying part of the present Siberia.
d. Gondwanaland, a huge continent extending from South America to India via South Africa.

He states, further, that the evidence shows that Nearctis and North Atlantis were connected by a land bridge at about Lat. 50° N., and that the first three continents were separated from Gondwanaland by a great ocean, the Tethys Sea, which extended from New Guinea to Central America. Another authority, Beno Gutenberg, writes:

> . . . Nearly all specialists on such problems conclude that during certain pre-Tertiary periods land connections existed across sections of the present Atlantic and Indian Oceans. . . . During certain geological periods land life was able to roam from land to land; on the other hand, such former connections of continental areas prevented sea life from moving from one part of the Atlantic to another (194: 208–09).

Gutenberg thus bears witness to the fact that the sea life as well as the land life of the past supports the idea of important changes in the positions of land masses.

We can see that the suggestions advanced are of two kinds: sunken continents, and changing land bridges between continents. Land bridges, of course, are more easily explained than sunken continents. However, we shall see that, for a number of reasons, they will not suffice of themselves. There

is evidence that points insistently to the former existence of whole continents in what are now oceanic areas. Only recently, for example, Dr. Albert C. Smith, of the Smithsonian Institution, concluded, from a massive study of the plants of the islands of the Southwestern Pacific, that the islands must be merely the remnants of an ancient Melanesian continent that broke up about 10,000,000 or 20,000,000 years ago (397). We shall see that there is plenty of evidence, besides the evidence of fossils, to support his conclusion.

From what has been said about mountain formation, I think it is clear that the matter of the appearance and disappearance of land bridges is accounted for at the same time that the mountain ranges are accounted for. A displacement of the crust will lead to the uplift of long, narrow, folded tracts on the sea bottom as well as on land. One of these, coming into existence on the bottom of a shallow sea between two major land masses, could connect such land masses and constitute a land bridge. Its subsidence in a later movement of the crust could separate the land masses, and the subsidence could be partial, leaving islands, or total.

It is interesting to see that Umbgrove found himself compelled, because of a mass of geological evidence, to support a theory of rapidly appearing and disappearing land bridges, which was advanced by Willis and Nolke, though he admitted that "Their origin and submersion will probably remain a mystery for some time to come. . . ." (430:238).

Land bridges have been very convenient for many scientists seeking to avoid the horrid alternative of former continents. According to the picture drawn by some writers, these bridges were long snakelike arms, wriggling out this way and that, which just happened to make the right connections between the right continents at the right times for the convenience of the right plants and animals. Often when the threat of a former continent loomed so imminently that its avoidance seemed hopeless, a land bridge would save the day. Most paleontologists were satisfied with land bridges, and did not insist on sunken continents, but some could not help

feeling that there was something artificial about the idea they therefore continued to speculate about former conti nents.

Should you ask, How did all this activity on the part o the biologists and paleontologists strike the geophysicists I can answer that it did not strike them at all. This can b explained partly by the fact that geophysicists generally d not read books on paleontology, and vice versa. So far as th geophysicists were concerned, the speculations of the paleon tologists could be discounted as the insubstantial imagining of persons unacquainted with geophysics. To study the bio logical literature allegedly supporting these speculations was of course, not the function of geophysicists. It was outsid their field, and, moreover, beyond their competence. Th connection between these sciences was a distant one. The re lations between them were cool, to say the least.

4. Geologists Allied with Biologists

It would have been a sad thing for the biologists and paleon tologists had they not been able to find allies in the sever struggle in which they were engaged (without, for the mos part, being aware of it). But find allies they did. For it soor developed that the geologists would not be content with th limitations on continental change imposed by the geophysi cists. They could not be satisfied with land bridges. Fron purely geological studies of the stratified rocks of many land throughout the world came quantities of evidence insistentl suggesting that the continents and ocean basins could no have had the permanence demanded by existing concepts ir geophysics.

To begin with, there is an extraordinary contradiction ir the very fact that, while continents are supposed to have beer permanent, nearly all the sedimentary beds that compos them were laid down under the sea. There is no denying thi

fact. According to Schuchert, North America has been submerged no less than seventeen times (369a:601). According to Humphreys, the sea has covered as much as 4,000,000 square miles of North America at one time (231:613). Termier argued that the sedimentary beds composing the mountain ranges extending eastward from the Alps to Central Asia, which were laid down under the sea, would have required that the ancient Tethys Sea, in which they were laid down, should have been about 6,000 kilometers (or perhaps 4,000 miles) across (419:221–22).

Geophysicists tend to argue that such seas, which clearly did exist, were merely shallow affairs, invasions of the continents by the ocean owing to some unknown cause. The positive evidence for this, based on the apparent absence from the sedimentary rocks of sediments formed in the very deep sea, has a fallacy in it, as will be made plain later. The positive evidence against the assumption that all these seas were shallow seas is, on the other hand, enormously strong. Umbgrove, for example, remarks:

> . . . Not only have parts of the continents foundered below sea-level since pre-Cambrian times but they have even done so until quite recently, and their subsidence occasionally attained great depths! The present continents are but fragments of one-time larger blocks. . . . (430:30).

A particularly important example of such foundering seems to have occurred in the North Atlantic, off the northeastern coast of the United States. It has been found that the sediments that compose the northeastern states were derived in ages past from a land mass to the eastward in the present North Atlantic. This could have been Brooks's continent of North Atlantis.

Some geologists, cowed by the geophysicists, have attempted to argue that these sediments might have been derived from a land mass situated on the present continental shelf, but the argument fails from every point of view. Brewster, for example, comments:

> It must have been a large continent, for the sand and gravel and mud which the rivers washed out to sea and the waves ground up on the shore have built up most of half a dozen big states, while in some places the deposits are a mile thick (45:134–35).

Umbgrove says that while it is impossible to estimate the size of the land mass (called "Appalachia" by the geologists), it was clearly large, to judge from the fact that it has been possible to trace out in the sedimentary beds of the Appalachian Mountains the outline of an enormous delta formed by a giant river flowing out of the land mass to the east (430:35–38).

Now the continental shelf of North America ends abruptly a very short distance from the coast. It is an extremely narrow strip between the coast and the so-called "continental slope," where the rock formations dip down suddenly and steeply into the deep sea. Its average width is only 42 miles, and its maximum width does not exceed 100 miles (46). If the sediments had been derived from a land mass on this continental shelf, this very narrow land mass would have had to carry huge and repeatedly uplifted mountain ranges. Furthermore, since drainage would naturally have carried sediments down both slopes of these mountain ranges, a large proportion of the material would have been carried eastward and deposited in what is now the deep ocean; but there is no evidence of this.

The suggestion that the enormous volume of sediments forming the northeastern states of the United States came from the continental shelf must be considered improbable. On the other hand, it is plain that the former continent in the North Atlantic could not have been eroded away by rivers any farther down than approximately sea level. Erosion did not dispose of the continent, nor create the deep-sea basin. After erosion had finished its work, the continent itself sank to a great depth. Umbgrove has cited recent oceanographic research by Professor Ewing of Columbia, showing that this ancient land mass of Appalachia now lies subsided about two miles below the continental shelf (430:35–38).

This extraordinary case is by no means unique, for Umbgrove has pointed out that the sediments composing much of Spitzbergen and Scotland come from the ocean west of them, while those composing the west coast of Africa come from a former land mass in the present South Atlantic. Most interesting of all, he indicates that the deepest of the world's deep-sea troughs (east of the Philippines), about seven miles deep, gives evidence that it was once part of a very large continent (430:38).

The evidence produced by Coleman, showing that a continental ice sheet once invaded Africa from the sea, and that the Indian ice sheet must have extended on land far to the south of the present tip of India, is to the same effect. It serves to answer conclusively the argument about continental shelves. You can put just so much on a shelf.

According to Umbgrove, there is ample evidence of repeated upward and downward oscillations of the floor of the entire Pacific (430:236). In a kind of rhythm, the great ocean has become alternately shallower and deeper. In the absence of any explanation of this phenomenon, Umbgrove becomes geopoetic. There seems to him to be something almost mystical in this slow pulsation of the living planet. He finds that the unexplained upward and downward movements are not limited to sea areas:

> . . . It should be noted that blocks that were first submerged, then elevated, and then once more submerged and elevated, are also met with on the continents. The sub-Oceanic features and the similar continental characteristics cannot be explained at present, for our knowledge of pre-Cambrian history and terrestrial dynamics is not yet extensive enough. . . . (430:241).

Comparatively radical vertical changes in the positions of land masses are evidenced by a considerable number of ancient beaches (some of them, however, not very old) which are now found at great elevations above sea level, and sometimes far inland from the present coasts. Thus the geologist P. Negris claimed to have found evidences of beaches on three mountains of Greece: Mt. Hymettus, Mt. Parnassus,

and Mt. Geraneia, at, respectively, 1,400 feet, 1,500 feet, and 1,700 feet above sea level. He found a beach on Mt. Delos at 500 feet (324a:616–17). William H. Hobbs cited a particularly interesting case of a beach of recent date now 1,500 feet above sea level, in California:

> Upon the coast of Southern California may be found all the features of wave-cut shores now in perfect preservation, and in some cases as much as fifteen hundred feet above the level of the sea. These features are monuments to the grandest of earthquake disturbances which in recent times have visited the region (216:249).

It would be possible to multiply endlessly the evidence of the raised beaches, which are found in every part of the world. Many of them may imply changes in the elevations of the sea bottoms, such as are suggested by Umbgrove.

One of the most remarkable features of the earth's surface is the Great Rift Valley of Africa. The late Dr. Hans Cloos pointed out that the high escarpment along one side of this valley was once, quite evidently, the very edge of the African continent: not just the beginning of the continental shelf, but the very edge of the continental mass. In some vast movement that side of the continent was tremendously uplifted, and the sea bottom was uplifted with it as much as a mile, and became dry land. This is so interesting a matter, and of such special importance for our theory, that I quote Dr. Cloos at length:

> There are two rims to the African continent. Twice the fundamental problem arises: why do the continents of the earth end so abruptly and plunge so steeply into the deep sea? . . . Even more astounding, what is the meaning of the high, raised and thickened mountain margins that most continents have? (85:68).
>
> . . . The short cross-section through the long Lebombo Chain looks unpretentious, but it illuminates events far from this remote plot of the earth. For here the old margin of the continent is exposed. Not so long ago, during the Cretaceous Period, the sea extended to here from the east. The flatland between the Lebombo hills and the present coast is uplifted sea-bottom. . . . What we see are the flanks of a downward bend of High Africa toward the Indian Ocean. . . .

> But we see much more: the sedimentary strata are followed by volcanic rocks to the east of the hills. Some parallel the strata like flows or sheets, poured over them and tilted with them. Others break across the sandstone layers and rise steeply from below. This means that as the continent's rim was bent downward at the Lebombo hills, the crust burst, and cracks opened through which hot melt shot upward and boiled over.
>
> So the eastern margin of Africa at the turn of the Paleozoic Period was a giant hinge on which the crust bent down, to be covered by the ocean. What we see here is merely a cross-section . . . one can go further north or south, and even to the other side of the continent and discover that great stretches of this unique land have suffered the same fate. The oceans sank adjacent to the continents, and the continent rose out of the ocean (85:73–74).

Cloos makes it clear that in one geological period the continent was bent down so that a part of it became sea bottom (not merely continental shelf) and that at a later period it was uplifted some 6,000 feet, the sea bottom became land, and the continental margin was shoved far to the east. When we contemplate gigantic movements of this sort, it seems reasonable to take the geophysical objections to changes in the positions of the continents with a grain of salt. If a large part of a continent can be shown not to have been permanent, it is unnecessary to assume the permanence of any of it. On the other hand, such changes need to be explained, and they need to be reconciled with basic principles of physics. The fact that theories of continent formation and history hitherto proposed have failed to solve the problem reduced a recent writer on lost continents to the following confession of ignorance:

> Since somebody can bring good, solid objections on one ground or another against all these hypotheses, however, we had better agree that nobody knows why continents or parts of continents sink, and let it go at that. No doubt a sound explanation, perhaps combining features of the older theories, will be forthcoming some day (64:161).

It may be useful to consider, in juxtaposition, the African Rift with the question of the North Atlantic land mass already discussed. In a sense, the two are complementary. In

one case a continent apparently subsided; in the other it first subsided and then was raised up. Quite obviously the movements in both directions must have been related to one fundamental dynamic process. The physical geology of the Rift, which can be directly examined, shows that the indirect evidence of the sedimentary rocks of the northeastern states of the United States, and of Scotland and Spitzbergen (and the paleontological evidence), must be taken seriously. The evidence in favor of an important land mass in the present North Atlantic cannot be dismissed.

5. *The Evidence of Oceanography*

A great deal of new evidence bearing on the question of the permanence of continents and ocean basins has resulted from the oceanographic research of recent years. There has been a revolution in our ideas of the nature of the ocean floor. Formerly, it was thought that the ocean floors were continuous, flat marine plains, buried thousands of feet deep under an accumulation of sediments extending back in unbroken series to the earliest geological periods. After all, the theory of permanence of the ocean basins required this concept. It has been found, however, that, on the contrary, there are hills, valleys, mountain systems, and canyons on the sea bottom very much like those on land. There is no continuous thick layer of sediments. The features of the ocean bottom appear to resemble, in singular fashion, those of the land. One of our leading geologists, Richard Foster Flint, has written:

> Sound-wave surveying . . . has revolutionized our picture of the ocean floor. Instead of the plainslike surfaces that were once believed to be nearly universal, broad areas of the floor are now known to have an intricacy of detail that rivals that of complex land surfaces. In some places the detail seems to have resulted from local warping and faulting of the crust and submarine volcanic activity, but in others it consists of valley systems somewhat like those that diversify the land.

Geologists are not agreed as to whether these features are valley systems cut by streams and later submerged or depressions excavated by currents beneath the sea (155).

Now this problem must be examined as a whole. Certain facts are now obvious and can be plainly stated.

In the first place, if a continent can be lifted up a mile, and the sea floor exposed, as in the case of Africa, surely it can also be let down. Thus there is no reason for anyone to lose his temper at the idea that some of these drowned surfaces were once above sea level. On the other hand, it is not necessary to claim that they were in all instances eroded above sea level. We saw, in the last chapter, that the basic processes of mountain formation, folding, faulting, and volcanism, are of a kind that can take place just as well below as above sea level. The only factor in mountain formation that is mainly operative on the land is erosion, and that is, as we have seen, a secondary factor.

Secondly, one of the most impressive arguments in favor of the permanence of the ocean basins is that almost all the sedimentary rocks that compose the continents appear to be made of sediments that were laid down in comparatively shallow water, on or near the continental shelves. We have already seen, however, that parts of continents (at least) have been submerged to great depths, and that parts of the deep-sea bottom have been uplifted to form land. Why, then, have rocks composed of typical deep-sea sediments not been found?

A number of factors may account for this. The primary factor may be the rate of sedimentation. In the deep sea this is extraordinarily slow—as low as one inch in 2,500 years. Near the coasts it can be hundreds of times more rapid.

The theory presented in this book provides a mechanism that would tend to operate against the consolidation of this deep-sea sediment into rock. Frequent displacements of the crust of the earth would naturally be accompanied by increased turbulence on the ocean bottom, by which sediments would be dispersed and mixed with other sediments. There has been in recent years a great extension of our knowledge

regarding the operation of turbidity currents (137, 139, 141) caused by the slumping of sediments from the continental slopes, and by other forces. It seems that such currents, even now, are powerful enough to bring about considerable rearrangement of the unconsolidated sediments of the ocean bottom. A displacement of the crust would greatly magnify their force, for it would cause extensive changes in the directions of major ocean currents, changes in sea and land levels, extensive volcanism in the sea as well as on land, and an increased number and a greater intensity of earthquakes, which would occasion extensive slumpings of sediments along the continental slopes. If we consider that one such displacement would, in all probability, keep the turbulence at a high point for several thousand years, we can conclude that the resulting dispersal of deep-sea sediments would probably be on a considerable scale.

Finally, since we cannot suppose that any area would be uplifted rapidly from the deep sea to the surface (that is, all the way in the course of a single displacement), it follows that in most cases deep-sea sediments would be raised into shallow water, where they would be exposed for a long time in an unconsolidated state to the erosive action of the much more rapid currents near the surface before they would be likely to be raised above sea level. A very small proportion of deep-sea sediment would then be mixed by the currents with a large proportion of sediment typical of shallow seas, and would, in most cases, entirely disappear. These factors together dispose of this argument for the permanence of ocean basins.

Another interesting line of evidence with respect to this problem is provided by the recent discovery on the bottoms of the oceans, already mentioned, of several hundred mountains of varying heights, which have been given the name of "sea mounts." These have the common characteristic of being flat-topped. Apparently, their tops were made flat by the action of the sea at the time they were at the sea level. Now

the flat tops are submerged anywhere from a few hundred feet to three miles below sea level.

When these sea mounts were first discovered, they were explained in accordance with the theory of the permanence of ocean basins (210). It was proposed that as the sediments gathered in enormous thickness on the ocean floor through hundreds of millions of years, the floor actually gave way, and sank, taking the sea mounts down below sea level. This theory was undermined, of course, by the discovery that no such thick layer of sediments exists on the ocean floors, but that, on the contrary, the layer of sediments is in some places extremely thin, or even virtually nonexistent.

Another line of evidence helps to dispose completely of this explanation of sea mounts. Foraminifera are minute protozoa that live in the sea. Their species vary with differences in the depth and temperature of the water in which they live, and those of past geological periods, found in fossil state, differ from living species. Studies of fossilized foraminifera from the tops of the sea mounts have revealed that they are much younger than the sea mounts have been assumed to be (197). Comparatively recent species have been gathered from the tops of sea mounts subsided to great depths. Unless turbidity currents could suffice to carry such deposits long distances across the ocean floor and then upwards to the tops of the sea mounts, another cause for the subsidence of the sea mounts must be found. When we remember that Umbgrove referred to frequent upward and downward oscillations of the floor of the Pacific, resulting from an unknown cause, we can see that the idea of a gradual and continuous subsidence of the ocean floor under the weight of accumulating sediments is a singularly weak one, for even if the supposed layer of sediments existed, the theory still unaccountably ignores the recurring uplifts of the sea bottom.

The foregoing considerations reveal the essential weakness of the conclusion that the seas that periodically invaded the continents were always shallow seas—"epicontinental" seas, flooding the permanent continents. First there was land;

then, no doubt, shallow seas; after that, in some cases, very deep sea, then again shallow sea, and finally again land, all in the same place. But the interludes of deep sea may have been, in many cases, very short, and the sedimentation resulting may have never been consolidated. Thus the deep sea could come and go, and nobody the wiser. New evidence bearing on this problem is now available as the result of recent Soviet oceanographic work in the Arctic. Soviet scientists have found evidence that the Arctic Ocean itself has existed only since the comparatively recent Mesozoic Era (364:18). We shall return to this evidence.

It seems reasonable to conclude that at least some of the problems presented by the continents and ocean basins are soluble in terms of the principles described in this and previous chapters. Land links may be explained as the consequences of mountain formation on the sea bottom; temporary and limited uplift or subsidence of large areas may result directly from their poleward or equatorward displacement. The major changes, however—the enormous elevations and subsidences, the destruction and creation of continents—require us to examine the deepest possible consequences and implications of crust displacement. We must now undertake this deeper examination. This requires us to take another glance at the nature and structure of the crust of the earth, to its full depth, as far as our present geophysical knowledge permits.

6. *The Deeper Structure of the Earth's Crust*

It has, until lately, been the impression that the earth's crust, considered as a crystalline layer between 20 and 40 miles thick, was itself composed of various layers, with rocks of increasing density at increasing depths. This would have been a natural development with a cooling earth, for it might be supposed that the lighter materials in a liquid would tend to float on the heavier ones, and would solidify in the order

of their density, with the lightest on top. Daly, in 1940, proposed the following layering of the crust, at least under the continents:

a. The sedimentary rocks, at the surface.
b. Below these a layer of basement rocks of granitic type, ending about 15 kilometers, or 9 miles, down.
c. A third layer of rock, of somewhat greater weight, about 25 kilometers, or 15 miles, thick.
d. A fourth layer, about 10 kilometers, or 6 miles, thick, of still heavier rock.

Having proposed these layers, however, he added that "the exact depths of the discontinuities are not easily demonstrated" and "it seems clear that each of the breaks varies in its depth below the rocky surface" (97:17). In another place he gave evidence of light matter at the very bottom of the crust (97:223). These layers, then, according to Daly, are very peculiar. There is nothing regular about them. On the one hand, he gives rough estimates of their thicknesses. On the other hand, he indicates that these estimates are of little value. They are, in fact, mere rough averages; they indicate a trend toward increasing density with depth, together with enormous confusion in the distribution of materials.

In view of the extreme uncertainties of Daly's view of the structure of the earth's crust, it can hardly come as a complete surprise that the most recent geophysical investigation of the structure of the crust by the method of sound-wave surveying has failed to show any distinct layering of the crust. The geophysicists Tatel and Tuve have reported that the results of the most recent studies, using the most recent techniques, indicate that rocks of greater or less density are intermixed in utter confusion, that the essential structure of the crust is really that of a rubble, on a large scale (416: 107).

The main argument of the geophysicists who speak in favor of the permanence of the continents, and, consequently, of the ocean basins, is based on the observed difference in

composition of continents and of the crust under the oceans, a difference that has been verified for the uppermost few miles of the continental and oceanic sectors of the crust, but not for the greater depths.

Now, if everything below, say, a depth of ten miles were layered everywhere at equal depths with rock of equal densities, no quarrel whatever could be had with the geophysicists who argue for the permanence of continents and ocean basins. For in that case, only a massive change in the distribution of the superficial layers of light and heavy rock could change the distribution of continents and ocean basins. Granitic or sedimentary rocks at the surface would have to be destroyed or created in enormous quantity to destroy or create a continent.

It is entirely otherwise with the structure as suggested by Daly and as revealed in the recent geophysical surveys. To understand this it is necessary only to visualize that the relative elevation of the surface at any point is determined by the *average* density of the entire column of matter between the surface at that point and the bottom of the crust, where, presumably, the inequalities at different points are pretty well averaged out.

If the crust is not definitely layered, if, as both Daly and recent geophysicists agree, there are radical variations in the structure, then the vital changes may occur at any depth, deep down as well as at the surface. There is reason to believe that massive changes may occur more easily deep down than at the surface. Thus, the crust might be weighted in its lower parts by an intrusion of a great mass of molten rock of high density from below, a very likely result of a displacement. In either case, whether the addition of the heavy matter occurred near the surface or far below it, the result would equally have been a depression of the surface, with a consequent encroachment of the sea. Obviously, a repetition of such movements could subside a continent to a great depth, without altering the composition of the superficial formations.

On the other hand, a shifting of the masses of lighter rock, which might have formed the downward projections of continents and mountain chains, as the result of a displacement, could lighten certain sectors of the crust and result in their uplift.

That rocks of light weight are to be found at the very bottom of the crust (and even in the downward projections under continents and mountain chains that extend to greater depth) might, as a matter of fact, have been deduced from Daly's observation that the process of mountain folding has involved the whole depth of the crust, and not just its surface layers (97:399). His suggestion is that since the folding of the crust to form mountain ranges involves its horizontal shortening, the horizontal shearing movement has to take place at the level where displacement will be easiest, which will be at the bottom of the crust where the rock has minimum, or zero, strength. For it is plain that at a level at which the rock possessed any considerable tensile strength, the shearing of one layer over another horizontally would be practically out of the question.

It would seem, from these considerations, that the commonly used terms "sial" and "sima" to differentiate lighter from heavier material in the crust, especially when they are presumed to indicate different layers, have very little meaning. They amount to trends merely, and take no account of the detailed distributions of the materials of different density either vertically or horizontally. It would be wrong, therefore, to assume that just because we find a layer of basalt on the floor of the Atlantic (as has been recently reported) this layer necessarily extends to the bottom of the crust, and is not underlain, at greater depth, by sedimentary and granitic rocks of less density. It is even true that the layer of basalt may have been extruded during the subsidence of the sea bottom in the last movement of the crust, and have been, in itself, one cause of the subsidence.

Jaggar, for one, considered that it was far more reasonable to account for subsidence or elevation at the surface by

changes of weighting deep in the crust than by erosion and sedimentation at the surface. He remarked:

> . . . It would seem possible that intrusive and extrusive processes may lighten or weight the crust much more profoundly than the movement of sediments (235:153).

How, precisely, would these processes be apt to be set in motion by a displacement of the crust? We saw that a displacement would cause a general fracturing of the crust, the creation of a new world-wide fracture pattern. In areas moved toward the equator, the extension of the surface area would involve some pulling apart of the crust, the separation of the fragments, their subsidence into the semiliquid melt below, and the rise of the magma into the fractures, with, at some points no doubt, massive eruptions on the surface. Differential movements of blocks of the crust would occur as each sought its gravitational equilibrium, some rising and others subsiding. In areas moved poleward compression would be the rule, with some folding of the crust, with block faulting accompanied by tilting of larger or smaller blocks. In these areas the strata of lighter rock would grow thicker, from being folded upon themselves; in equatorward-moving areas, on the other hand, they would tend to grow thinner, because much of the lighter rock might be engulfed in the rising heavy magma.

However, major changes in the situation of continents require additional processes, even though changes such as those suggested above might, if they accumulated over long periods of time, produce very important results. The massive changes, capable of subsiding or elevating continents, might be of two different sorts, though both of them must, in the nature of things, remain speculative for the present. One of these would be the massive intrusion of immense quantities of heavy magma into the crust (resulting finally in plateau basalts). Such an effect could be produced by subcrustal currents set in motion by a displacement, and would have the effect of causing a major subsidence at the surface. The other cause of massive change in the average density of a given col-

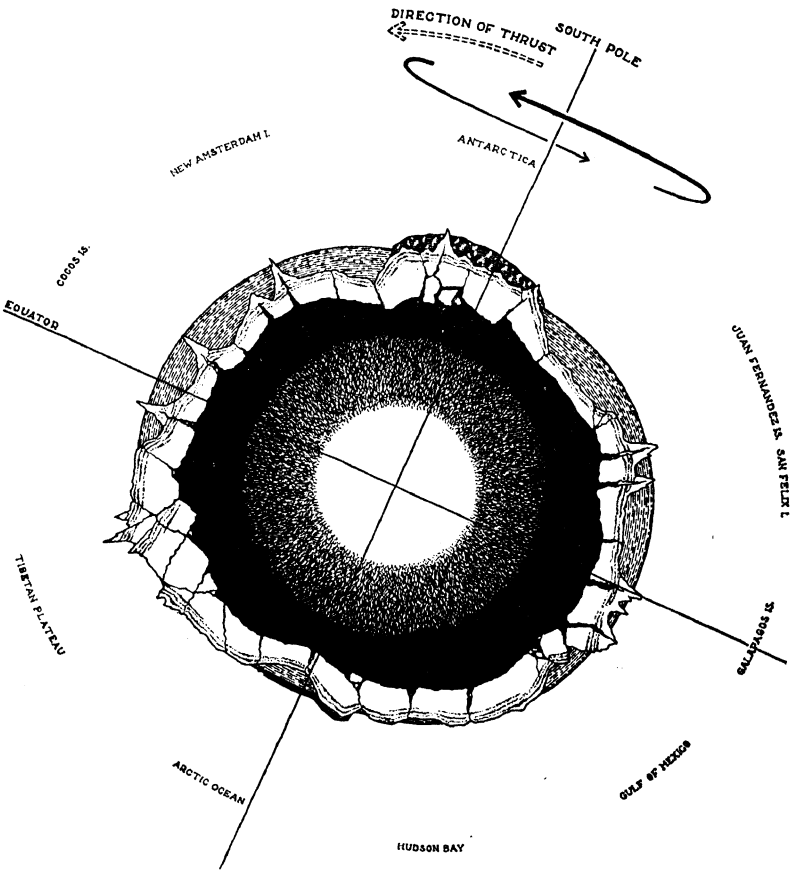

Fig. V. Consequences of Displacement: Cross Section of Earth at 96° East of Greenwich Showing Centrifugal Effect of Icecap

In Figs. V and VI the artist imaginatively suggests some consequences of a displacement of the crust. On this page we see a cross section of the earth before the movement, with the eccentric icecap above the equilibrium surface of the earth. On page 155 we see a cross section of the earth after a displacement through about 30 degrees of latitude. The drawings are not to scale. It has been necessary, for purposes of illustration, to exaggerate the oblateness of the earth, the depths of the oceans, and the thickness of the crust several hundred times. The drawings illustrate: (1) The subsidence of the crust in areas displaced toward the equator, relatively to sea level, and its uplift in areas moved poleward. (2) The

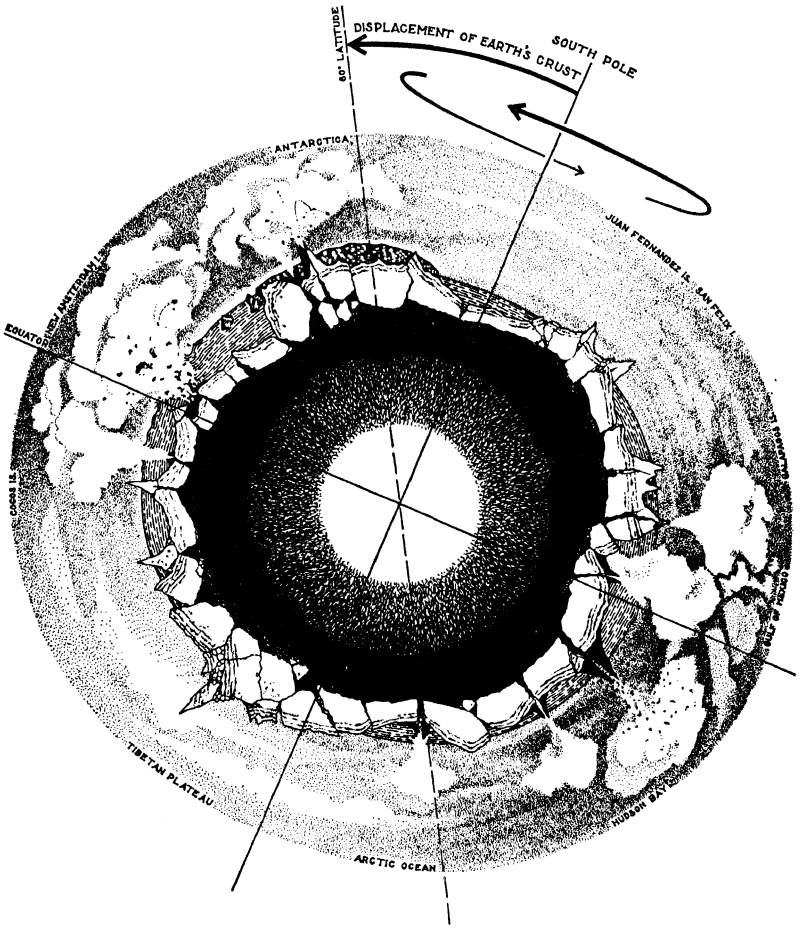

Fig. VI. Consequences of Displacement: Cross Section of Earth at 96° East of Greenwich Showing Hypothetical Effect of Shift of Crust

fracturing and tilting of crustal blocks, suggested schematically. It should be understood that in reality the blocks would be comparatively small and the fractures would be numbered in millions. (3) The displacements of parts of the downward projections of the crust under continents and mountain chains, which would have an effect on the permanent elevation of the surface relatively to sea level. (4) The volcanism attending the movement, with the projection into the atmosphere of volcanic dust capable of causing meteorological effects.

umn of the crust (that is, a section extending from top to bottom of the crust) would be a shifting of light matter from one point to another under the bottom of the crust. This matter was discussed in the last chapter. While in that chapter relatively minor effects were considered, it is true, nevertheless, that the shifting of light matter from one point under the crust to another could take place on a very large scale.

What might be the upshot of all these changes during a displacement of the crust? The result might well be that while the distribution of light and heavy matter near the surface would be unchanged, its distribution (average density) between the surface and the bottom of the crust would be materially changed. This is undoubtedly the direction in which we must look for a solution of the problem of continents and ocean basins, as a means of reconciling geophysical ideas with the evidence of biology and geology.

I cannot close without reference to a singular confirmation of the line of reasoning adopted in this chapter, which I find in Umbgrove's discussion of the work of the geologist Barrell, with whom he disagreed.

Umbgrove is considering the question of the submergence of continents. It is clear from his discussion that Barrell's conception of the process requires a theory of crust displacement. Umbgrove states the problem thus: If we are to suppose the submergence of continents, we must either suppose a change in the amount of the ocean water, which, if it increased, could flood a continent (or several at once), or, if the water remained about the same in quantity, the submergence of one continent must be balanced by the elevation of another. He presents the findings of specialists to show that the quantity of water on the earth's surface has remained about the same, from the earliest times, and adds:

> Should one, nevertheless, cling to the theory of submerged continents, the only alternative would be to assume that while vast blocks were being submerged in one area, parts of the ocean floor of almost identical size were being elevated in others. . . .

> It is not quite clear, however, why such opposed movements should have occurred in areas of almost equal extent. Nor is it clear why these movements should have occurred in such a way that the sea-level remained comparatively stable. . . . (430:235–36).

But a displacement of the crust—or several displacements—would fulfill all these requirements. In a displacement two quarters of the surface, opposite each other, must move toward the poles, while the other two quarters must move toward the equator. Whatever forces tend to produce uplift in the poleward-moving areas will be balanced by equal forces producing subsidence in the quarters moving equatorward. And the sea level would be stable, except for very minor fluctuations.

Barrell himself suggests that subsidence of continental areas would be aided by liquid intrusions, "the weight of magmas of high specific gravity rising widely and in enormous volume from a deep core of greater density into these portions of an originally lighter crust. . . ." (430:235–36).

Barrell's suggestion points to the chief weakness of the geophysical argument in favor of the permanence of the continents. As I have already pointed out, geophysicists seem, too often, to take as the frame of reference only the outermost ten miles or so of the crust. Theoretically they base calculations on the full depth of the crust, but practically this assumption is cancelled out by the assumption that the crust is arranged in layers of equal density, so that significant changes of density in depth are excluded. But if the real possibilities of changes of average density in the full depth of the crust are taken into account, the difficulties in the face of the subsidence and elevation of continents vanish.

VI : THE SHAPE OF THE EARTH

In the two preceding chapters we have considered the evidence for displacements of the earth's crust provided by mountain chains, earth fractures, volcanic zones, continents, and ocean basins. We have had occasion to refer, a number of times, to the effects of the force of gravity, and of the earth's rotation, on the earth's crust. The understanding of the theory of crust displacement presented in this book depends entirely on a correct understanding of these two forces, and of the operation of what we call the principle of isostasy. The ideas are not difficult, provided certain essentials are kept in mind.

1. Isostasy and the Icecap

The globe we live on is shaped primarily by the force of gravitation. The weight of the materials of which it is composed is greater than their strength, and they have therefore been bent, broken, or forced to flow by this force until the globe has become a sphere. It is not, however, a perfect sphere. The earth deviates from being a perfect sphere because it is rotating rapidly, and the rotation produces a centrifugal effect that tends to throw the materials outward at right angles to the earth's axis, and against the force of gravity. This slightly modifies the earth's shape, producing a bulge around the equator and a flattening at the poles. The earth modifies its shape until the two forces are in balance, and the resulting oblate sphere is called the "geoid."

Of course, there is always a considerable amount of shifting of materials going on on the face of the earth, rivers bringing down sediment to the sea, icecaps growing and melting, and these changes are constantly upsetting the bal-

ance and requiring readjustments. The crust of the earth, being very thin as compared with the whole diameter of the globe, is correspondingly weak, and usually gives way if much material is accumulated on it at any one spot. The layer under the crust is considered to be quite weak, and it permits the crust to give way by flowing out from under. This process is called "isostatic adjustment." Funk and Wagnalls define isostasy as follows:

> Theoretical condition of equilibrium which the earth's surface tends to assume under the action of terrestrial gravitation as affected by the transference of material from regions of denudation to those of deposition, and by difference of density of various portions of the earth's mass near the surface.

In the last chapter we considered at length the effects of the differences of density of rocks under oceans and continents, and how heavy rocks tend to reach equilibrium at lower elevation, and lighter rocks tend to be found in continents and mountain chains.

According to the principle of isostasy, it has been widely assumed that an icecap such as the one in Antarctica should be in good isostatic equilibrium. Of course, if this were true, the centrifugal effect of the icecap would be balanced at every point by the force of gravity, and there would be no effect left over to tend to shove the crust over the plastic layer below. There might be a slight effect resulting from the elevation of the continent and the icecap above the mean earth surface, but it would be too small to consider. Thus, our theory depends upon our ability to show that isostasy has not operated effectively in Antarctica. The question becomes one of estimating the difference between the rate of growth of the ice sheet, the rate of yield of the crust, and the rate of flow of the plastic layer under the crust. The calculation must also take account of the amount of load that may be borne by the crust, indefinitely, out of isostatic adjustment, because of its degree of tensile strength. Campbell's calculations of the centrifugal effect of the icecap, and of the resulting bursting stress on the crust (Chapter XI),

are based on the assumption that the whole mass of the icecap is uncompensated. They are only approximations, and must be modified as soon as more information as to the degree of the isostatic adjustment of the icecap is available.

On the other hand, it does not necessarily follow that the gravity measurements to be taken in Antarctica during the coming year will be able to provide a definite answer to the question. It is obvious that the theory of isostasy is at present under considerable attack, and that differences of opinion exist as to the validity of the various methods of interpreting the findings about the gravitational balance of the crust. Daly himself remarked that probably none of the present methods for determining the degree of isostatic adjustment of a crustal sector was capable of getting very close to the truth. A far more serious attack on the theory was made some time ago by Hubbert and Melton. They pointed out that all methods of reducing gravity data to measure isostatic compensation depend upon assumptions regarding the distribution of materials in the earth, which must be regarded as essentially unknown (226:688). They conclude:

> The fields providing data on the subject of isostasy are geodesy [measurement of the shape of the earth], seismology [study of patterns of earthquake waves], and geology. The data of the first, which until recently have provided the main support of isostatic theory, have been shown by Hopfner to be invalid. The data of the second have only an indirect bearing on the question. The data of the last are more often than not contrary to isostatic expectations. Hence, the theory of isostasy must for the present be regarded as resting upon a none too secure foundation, and it is hardly trustworthy for use as a major premise in present discussions of earth problems (226:695).

In the following pages I shall proceed on the assumption that the isostatic principle is generally correct, even though present methods of measurement are unsatisfactory. I shall assume that the materials of the earth's surface are under constant gravitational pressure to conform to the ellipsoidal shape of the earth, and that this applies to the icecap, and will bring about a tendency of the crust under the icecap

to yield and sink, displacing soft rock from below as the ice-cap grows in weight. At the same time I should like to present a number of considerations indicating that there is, at the present time, a massive departure from isostatic equilibrium in Antarctica.

One of the most astonishing things revealed by the new techniques of radioelement dating is, as we have seen, the rapid rate of the growth both of the present ice sheet in Antarctica and of the last great icecap in North America. Even before the new knowledge was available, however, various authorities had agreed that there must be a considerable lag between the growth of an ice sheet and the adjustment of the crust to it by subcrustal flow of the plastic rock out from under the glaciated tract. The geologist Wilhelm Ramsay, referring to this lag, said:

> . . . As the icecaps grew bigger and thicker, they loaded more and more the areas occupied by them. The crust of the earth gave way, and began to sink, but not in the proportion that the loads increased (352:246).

Ramsay pointed out that the "rebound" of the crust following the removal of the ice still continues thousands of years after the end of the ice age, showing how slowly the crust adjusts. Daly also referred to this lag:

> Owing to the stiffness of the earth's materials and the sluggishness of their response to deforming forces, the basin [around the Baltic] persisted long after the ice melted away. The lower parts of Scandinavia and Finland were kept submerged under the sea. . . . (97:190).

According to Daly, the present isostatic distortion in the center of the area once occupied by the great Scandinavian ice sheet is the equivalent of "a plate of granite with a thickness of 160 to 270 meters of rock" (97:386–87). This amounts to about three times as much ice. Thus it would amount to an ice sheet between 1,500 and 2,500 feet thick, and this in spite of the fact that isostatic adjustment has theoretically been proceeding in that area for perhaps 15,000

years. If we assume that the lag was the same during the growth of the icecap, we can see that there may have been an enormous excess of matter on the crust when the Scandinavian icecap reached its apogee.

In another way, Daly shows how slowly the compensating flow that permits adjustment takes place. He refers to the fact that erosion from a continent or large island appears to create negative anomalies (deficiencies of mass) on the land and positive anomalies in the sea (97:297–98). This is so because the crust does not adjust as rapidly as erosion takes place. Yet we can easily see that the accumulation of ice is faster than the weathering of rock and the deposition of sediments in the sea.

The geophysicist Beno Gutenberg has made a number of statements that strongly support the same conclusion. In discussing the zone within the earth where adjustment is carried on, he says:

> . . . It is inferred that this depth represents (in geologically stable regions) the critical level below which strain is nearly reduced to zero by subcrustal flow in periods of, say, 100,000 years (194:316).

The period of 100,000 years indicated by Gutenberg appears to be a multiple of the time required for the growth of a vast continental icecap. In another place, he refers directly to the lag between the growth or retreat of an ice sheet and the adjustment of the crust. After listing other ways in which the isostatic balance of the crust may be disturbed, he adds:

> A probable instance . . . is the lag in complete compensation of the load provided by the Pleistocene ice sheets. This is shown by the recoil of the tracts unloaded by the melting of the ice (194:319).

He remarks, further:

> The processes by which isostasy is maintained must be extremely slow, and consequently, this equilibrium is liable to disturbance by geological events (194:318).

Gutenberg and Richter, in their volume on *The Seismicity of the Earth,* express the known facts about adjust-

ment within the rigid crust itself. They remark: ". . . it requires several thousand years for the strains due to the removal of the ice load to be reduced to half" (195:101). This and the preceding statements may serve to establish the reasonable presumption that any icecap, at the height of its rapid accumulation, must be largely uncompensated.

But even if, during the rapid growth of the present Antarctic icecap, a degree of isostatic adjustment has taken place, this does not end the matter for us. We must ask, Where does this plastic rock go? Presumably it flows out, under the crust below the ocean bed, beyond the fringes of the ice-covered continent, at a depth of twenty to forty miles below the surface of the earth. It may raise the crust somewhat for a distance beyond the edges of the ice sheet. Dr. Harold Jeffreys gave it as his opinion that, if a gravity survey of Antarctica should be undertaken, we might expect to find marked positive anomalies around the coasts (241). But, since in general the plastic rock under the crust is not flowing into any area of deficient mass, the flow, upraising the coasts or the sea bottoms off the coasts, must still constitute excess matter, and must exert a centrifugal effect about equal to the former effect of the ice that has now been brought into equilibrium.

Let us consider this point a little further. We have noted, above, that according to the definition of isostasy, the process is a response to the transfer of material from one region to another: sediment is removed from an area of "denudation" and transferred to an area of "deposition." The area of deposition, then, sinks under the load, and a flow of rock under the crust moves back to the area of denudation, compensating for the material that has been removed, and restoring the equilibrium. But where can the plastic rock displaced by the icecap flow? No adjacent area has been lightened by the removal of sediment; the plastic rock must, then, wherever it flows, create a distortion, a surplus of matter, which will have a centrifugal effect.

From the standpoint of the theory presented in this book,

it makes no difference whether the excess mass that creates the centrifugal effect is constituted of ice or whether it is in part constituted of rock. It may be concluded, therefore, that the isostatic process itself is ineffective in counteracting the centrifugal effects of icecaps.

We may conclude, then, first that the rate at which isostasy may work is too slow to keep up with the deposition of the ice; and second, that in so far as it does work it will not eliminate the centrifugal effect of the uncompensated mass, but will merely substitute rock for ice.

2. *The Antarctic Icecap Is Growing*

The theory presented in this book requires, of course, that a polar icecap should grow, and continue to grow, though not perhaps steadily, until it is big enough to move the crust. In this connection it is important to examine certain current ideas about the Antarctic icecap.

It is widely believed that the Antarctic icecap, like some ice fields in the Northern Hemisphere, is in recession, and was once greater than it is now. This is a mistaken impression, the error of which can be easily shown, even without any more data from Antarctica. I will venture to remark, indeed, that any further data from that continent are likely to show that the icecap there is in a phase of rapid expansion.

I will begin with the northern ice fields, about which more is known. There is no doubt that most of them are melting, but the exceptions are very significant. These include the Greenland icecap, which appears to be holding its own (after having considerably expanded since the Viking settlements there during the Middle Ages), and the Baffinland icecap, which is growing. The mountain glaciers of the American northwest have, apparently, started a readvance (329). The most important fact regarding those northern ice fields that have been in retreat is that their retreat began only about the year 1850, and that, before that, they were expand-

ing generally for about 300 years. Between 1550 and 1850 the old Viking settlements in Greenland were overwhelmed by the advancing ice. From this it follows that the present trend in the northern ice fields is not a long-term trend, but only an oscillation. Therefore, it must not be deduced that we are just getting out of a glacial period, and that all ice fields are retreating and will continue to retreat.

Recent weather research has made it plain that alternating climatic phases of slightly colder or warmer temperatures have been the rule for some thousands of years. Such phases would have no connection with any movement of the earth's crust, but are probably related to varying atmospheric factors, such as slight variations in the amount of volcanic dust in the air, or short-term sunspot cycles. It is known that the warmest phase of the climate, since the ice age, occurred between 6,000 and 4,000 years ago. This did have a connection with the most recent displacement of the crust, as I will show later on. This warm period is in itself sufficient evidence that there has been no steady warming of the climate since the icecaps melted.

It is even true that a slight warming of the climate, such as appears to have occurred since about 1850, may increase precipitation of snow on the Antarctic continent. This suggestion has been advanced by a number of meteorologists. It is based on the following reasoning: The Antarctic average temperature is much colder than that of the Arctic. A slight warming of world temperatures will increase the amount of the humidity in the atmosphere. In northern regions, the increased humidity in the world's atmosphere may result in increased rainfall, while in Antarctica, because of the lower temperature, the increased precipitation may mean increased snowfall.

Up to the present time, only one impressive body of evidence has been produced to prove that the Antarctic icecap has receded from a former greater extent. This evidence consists of numerous indications of ice action on the barren sides of Antarctic mountains as much as 1,000 feet above the

present levels of the ice. As matters stood before the development of nuclear methods of precision dating, this evidence appeared conclusive. It certainly argued strongly that the icecap was greater in the past.

We are now able to take a different view of the matter. Now we know that this icecap is largely of very recent growth. We know, too, that before a preceding temperate age in Antarctica there was another icecap, and that there were several icecaps during the comparatively short period of the Pleistocene alone. We can assume, moreover, that the ice centers of the different ice sheets were in different places, so that the distribution of the ice—its thickness in different places—would be different from now, even if the total amount of ice were about the same. (See Figure VII.)

If the question is asked, Cannot we tell from the appearance of the striations, and other evidences of ice action on the Antarctic mountainsides, how long ago they were made?, the answer is No. The processes of weathering and erosion in Antarctica are slow. The striations, even if they were very old, could look as if they were made yesterday. Henry, in the *White Continent,* discussing the visit of Rear Admiral Cruzen in 1947 to a camp at Cape Evans that had been abandoned by Scott more than thirty-five years before, throws some light on this peculiarity of Antarctica:

> . . . From the camp's appearance, the occupant might have left only within the past few days. Boards and rafters of the cabin looked as if they had just come from the saw mill; there was no rot on the timbers; not a speck of dust on the nailheads. A hitching rope used for Manchurian ponies looked new and proved as strong as ever when it was used to hitch the helicopter. Biscuits and canned meats still were edible, though they seemed to have lost a bit of their flavor. A sledge dog, which apparently had frozen to death while standing up, still stood there looking as if it were alive. A London magazine, published in Scott's day and exposed to the elements since his departure, might have been printed that morning (206:44).

The same factors—intense cold, with absence of the destructive process of alternate melting and refreezing, and absence

of water action, and the absence of minute organisms—would preserve indefinitely the freshness of the glacial evidences. If there are any who still hesitate to accept the evidence of several successive ice ages in Antarctica during the Pleistocene, let them remember that we recognize four in North America and in Europe during that period, and that even here it is not always easy to assign glacial evidences to the correct glaciation.

Once it is fully recognized that the geological evidence of Antarctic ice recession must be reinterpreted, we are in a better position to evaluate the large mass of evidence now at hand regarding the present accumulation of snow in Antarctica. Geologists have hesitated, because of the earlier evidence, to interpret the data at all. Among the important items of information are the following: Henry mentions evidence of an accumulation of 18 feet of snow in seven years on the Antarctic barrier ice (206:75). The party that visited Antarctica on the icebreaker *Atka* in 1954 found evidence of the accumulation of 60 feet of snow at one spot since 1928:

> One of the *Atka*'s three Bell helicopters took off with Commander Glen Jacobsen, the ship's captain, as observer. It landed at the 1928 camp and found that one of the three towers was completely buried in snow although it had originally stood more than 70 feet high. The two others barely showed above the drifts (411).

In 1934, when Byrd made his second trip to Antarctica, he found that the Ross Shelf ice had encroached 12 miles on the sea since Scott charted it in 1911 and that there was much more ice in the Bay of Whales than when Amundsen visited it in 1911–13. Recently, Bernhard Kalb wrote in the New York *Times:*

> . . . Little America II—the 1933–35 base—had been built directly on top of snow covered Little America I—the 1928–30 base. But the last twenty years of snowfall had obliterated that base, too. The only reminders that there is a sort of Antarctic Troy entombed in the Ross Sea shelf—a spectacular table of glacier-fed ice floating in the sea, three times the size of New York State—were two steel radio towers and the tops of half a dozen wooden antenna poles. The towers,

dating from 1929, had been seventy feet high; now less than ten feet of them could be seen (246:59–60).

The best evidence for the rate of snow accumulation in Antarctica, however, comes from some scientific measurements taken in connection with "Operation Highjump" by the United States Navy, in the years 1947–48. The reports of this thorough study were included in the Army Observer's Report of the expedition, and were made available to me through the kindness of Admiral Byrd. Observations were made at a number of points; the snowfall was found to have averaged nine inches per year since the previous Byrd expedition (12).

Of course, snow does not accumulate equally at all points in Antarctica. In many exposed places it may not accumulate at all, but may be blown away by the wind. In the interior it is reasonable to suppose it may accumulate at a slower rate than on the coasts. Since measurements taken in a few areas only may be seriously misleading, we should briefly review the general factors controlling the snowfall and snow accumulation.

First, let us cite the testimony of a scientist-explorer, Nordenskjöld, who was the first to take scientific measurements of the snowfall in Antarctica. He also took temperature records during many months. Of these he said:

> They also prove that there is a tremendous difference between an arctic and an antarctic summer climate, and that our summer was colder than winters in southern Sweden. But the temperature alone does not give a true idea of the conditions in South Polar regions, and the following example will serve to illustrate some other points of view. I had arranged a row of bamboo rods on the glacier, in order to measure the changes in the height of the ice caused by thawings and snowfalls. During the winter this height was found to be constant, and not the slightest part of the snow which then fell remained on the glacier. But during the summer, on the other hand, the height of the snow covering increased by 25 centimeters (9.75 inches) and this amount still remained when we left these tracts one year later.
>
> Thus the reader must imagine a climate where winter is as severe

as winter in western Siberia, and so stormy that every particle of snow blows away; where the summer, even in the low latitudes where we were, is as cold as near the North Pole, and is, moreover, such that snowdrifts and glaciers increase during the warmest season of the year (335:253–54).

Nordenskjöld's camp, where he took these observations, was, apparently, about 64° S. Lat., or more than a thousand miles from the pole.

From this account it is evident that while a little melting may occur from time to time in Antarctica (and some has been recently reported), such melting must be entirely inconsequential. As to the quantity of precipitation over the whole continent, the following considerations seem important. First, the low temperature means a comparative lack of humidity in the air; precipitation could not equal that in temperate or tropical regions, for cold air will not hold as much moisture as warm air. Second, wind pattern and topography are both important factors. The wind pattern is as follows: There are winds blowing toward the pole at high elevations; these have, of course, crossed oceans on their way to the pole, and have picked up moisture. As they approach the pole they are compressed and chilled, and they contract and lose moisture in the form of snow. The air, now having greater density, sinks to the ice surface and moves outward in anticyclonic pattern. Winds blow outward in all directions from the pole, bearing with them great quantities of snow. Much of the snow is borne out to sea, but much is deposited in every nook and cranny, in all declivities, and, from the beginning of the growth of the icecap, the fringing coastal mountain chains have aided in the storage of snow. They do not prevent the high-altitude winds, bearing their moisture, from entering the continent, but they do, naturally, interfere to some extent with the outward-flowing, low-altitude winds, forcing them to deposit snow.

Now, these conditions have naturally been the same since the beginning of the growth of this continental icecap, and they must have prevailed with the previous icecaps. But we

see that the icecap has accumulated nonetheless, and that it is now accumulating. Thus we can safely conclude that these anticyclonic winds have not prevented, and can never prevent, the continuing growth of the ice sheet.

One other factor may limit the accumulation of ice. Icebergs break off from the icecap every year in great numbers, and it has even been suggested that they may amount to roughly the entire annual deposition of snow upon the continent. How far this is from representing the true state of affairs can be determined from the following considerations.

In the first place, the icebergs form, it is generally agreed, because the ice sheet is flowing slowly outward from the pole in all directions, by the effect of gravity. The ice has accumulated in the central area of the continent to a great but as yet unknown depth, and from this central area the surface of the ice sheet slopes gently downward toward the coasts. It is recognized that what sets the ice sheet in motion is not the slant of the land it lies on (it would move even if the land were all flat) but the angle of the slope of its own surface: the gradient (87:46). This being the case, two factors govern the speed of movement: the more gradual the gradient, the slower the ice flows, while the colder the ice, the greater its viscosity and its resistance to movement. Now, in Antarctica it has been observed that the gradient is only one third of the gradient in Greenland, where, despite a more rapid movement of the ice, the glacier still maintains itself approximately in a static condition. The Antarctic, in addition, is much colder, and therefore its ice is more viscous, more rigid, more resistant to motion. What the temperatures deep in the icecap are is at present unknown, but they are probably considerably lower than those in the northern glaciers. Coleman notes how much slower is the movement of ice in Antarctica than it is in Greenland (87:44). (See n. 1, p. 192.)

Brown has correctly pointed out that a large production of icebergs, such as we note in Antarctica, is a sign of an expanding glacier, while a dwindling supply of icebergs is evidence of an icecap in decline (54). Einstein was of the opinion that

the flow-off of icebergs could not even be an important factor in reducing the rate of the annual increment of ice on the glacier (128). The general problem of the Antarctic icecap may be summarized thus:

First, we know that there is never any considerable melting of snow in Antarctica. We have radioelement evidence of an enormous expansion of the icecap there in recent millennia. We also have evidence that it is now accumulating. We can add that studies carried out by Captain Charles W. Thomas, of the United States Coast Guard, of the radiolaria (minute organisms) contained in samples of bottom sediments from the Antarctic have recently convinced him that "during the last 5,000 years the waters surrounding this continent (Antarctica) have been getting colder" (411) just as would have to be expected with a growing icecap.

Secondly, if Antarctica has always been at the South Pole, what conceivable factor could have operated to prevent the formation of an icecap there until the comparatively recent Eocene Period, only about 60,000,000 years ago? Once an icecap had formed, what other factor could have interrupted the glaciation, so as to bring about the growth of luxuriant forests there in later periods? Since it has been shown that climatic zones like the present have clearly existed during the whole of geological history (Chapter III), would we not be justified in expecting the icecap to have accumulated continuously in Antarctica at least since the first known pre-Cambrian ice age, about two billion years ago?

Thirdly, Campbell has made the significant observation that the Antarctic icecap never melts, yet we know that ice sheets elsewhere on the globe have melted again and again. It has proved impossible to account for the ice sheets that once existed and melted away in areas now near the equator. Campbell points out that even if the rate of snowfall in Antarctica is low compared with the precipitation in warmer climates, yet the icecap has, in the oceans, an unlimited supply. It follows that, if the present icecap is not large enough

to start a movement of the crust, it will simply continue to grow, drawing upon the endless resources of the oceans until it is big enough.

3. A Suggestion from Einstein

From the foregoing, it is clear that there is a basis for the presumption that the Antarctic icecap is largely an uncompensated mass (an extra weight on the surface of the earth), that it has grown continuously since the disappearance of nonglacial conditions in the Ross Sea area only a few thousand years ago, and that it is growing now. Einstein recognized, in the Foreword to this book, the centrifugal momentum that such an uncompensated mass, situated eccentrically to the pole, would create when acted upon by the earth's rotation, and he saw that the centrifugal momentum would be transmitted to the crust. But he also raised, in the last paragraph of the Foreword, another interesting question. If an icecap can have such an effect, so can any other uncompensated mass. It is necessary to investigate any existing distortions within the crust itself and to learn whether it may contain uncompensated masses of a magnitude comparable to the Antarctic icecap, and thus capable of causing comparable centrifugal effects. For the inference is obvious: if such masses are in existence, but have not moved the crust, it follows that the crust may be anchored too solidly to be moved by the centrifugal effect of icecaps.

One of the troubles with the theory of isostasy is that the failures of the crust to adapt to gravitational balance have been found to be more numerous and more serious than expected. Daly lists and discusses a large number of them. It appears, for example, that the whole chain of the Hawaiian Islands, with their undersea connecting masses of heavy basalt, are uncompensated (97:303). These islands rise from the deep floor of the Pacific, and their peaks tower two and a half miles above sea level. Their gigantic weight rests upon

the crust, and under the weight the crust has bent down slightly, but it has not given way. This is the more remarkable since the islands appear to be several million years old. It indicates that at this point the earth's crust is strong enough to bear a very considerable weight without yielding. The Great Rift Valley of Africa, which we have already discussed, is uncompensated, despite its great age (97:221). There are also enormous anomalies in the East Indies. According to Umbgrove, Vening Meinesz found that the negative anomalies (that is, the deficiency of matter) in the great ocean deeps in that area and the positive anomalies on each side caused a total gravity deviation of 400 milligals. One milligal, according to Daly, would amount to about 10 meters of granite (97:394), so that the total deflection of the crust from gravitational balance here would amount to 4,000 meters of granite, or, roughly, three miles of granite, which, in turn, would be the equivalent of an ice sheet about nine miles thick. And the crust has borne this enormous strain, apparently, for some millions of years. According to Daly, the Nero Deep, near the island of Guam, has deviations from gravitational balance of the same magnitude (97:291). Among uncompensated features on the lands are the Harz Mountains, in Germany (97:349), and the Himalayas, which stand about 864 feet higher than they should (97:235). A particularly interesting case is that of the island of Cyprus, of considerable size, which stands about one kilometer, or 3,000 feet, higher than it should, and yet shows no signs of subsiding. Daly says:

> From Mace's table of anomalies and from his map, it appears that we have here a sector of the earth, measuring more than 225 kilometers in length and 100 kilometers in width, and bearing an uncompensated load equal to one kilometer of granite, spread evenly over the sector. . . . (97:212–13).

These facts would appear to argue a very considerable strength of the crust to resist the pressure toward establishment of gravitational, or isostatic, balance. However, in all the cases so far mentioned it is true that the deviations have

occurred in comparatively narrow areas. The Hawaiian Islands, for example, represent a long, narrow segment of the crust. Obviously the crust can support loads with small span more easily than loads with a very great span. These deviations, therefore, may not tell us much about the gravitational status of the Antarctic icecap, which, of course, has an enormous span, since it covers a whole continent. Since they are insignificant quantitatively as compared with the possible effect of the continental icecap of Antarctica, they will not, of themselves, answer Einstein's question.

Of more importance are isostatic anomalies of broad span, and these are, surprisingly, quite plentiful. Daly mentions one along the Pacific coast. This is a negative anomaly—a deficiency of mass. Daly explains that according to one formula (the "International Formula"), it covers an area 2,100 miles long, and 360 to 660 miles wide; according to another formula (the "Heiskanen"), it is reduced to one half both in intensity and in extent (97:371). Taking the lesser estimate, the deficiency of mass over this large area still amounts to the equivalent of a continuous ice sheet 1,000 to 1,200 feet thick. So it appears that over this large span the crust can bear that amount of negative weight (that is, of pressure from within the earth) without giving way, at least for a short period of time. In other parts of the United States there are positive anomalies of the same magnitude, and these obtain over large areas.

A far more extraordinary case is an enormous area of negative mass that covers part of India and most of the adjacent Arabian Sea. The width of the negative area in India is 780 miles. Daly, after noting the challenge presented by this fact to the whole theory of isostasy, goes on to say:

> The situation becomes even more thought provoking when we remember that Vening Meinesz found negative Hayford anomalies all across the Arabian Sea, 2500 kilometers in width. Apparently, therefore, negative anomalies here dominate over a total area much greater than, for example, the huge glaciated tract of Fenno-Scandia [Finland and Scandinavia]. And yet there is no evidence that the

lithosphere under India and the Arabian Sea is being upwarped. The fact that Fenno-Scandia, though less (negatively) loaded than the Arabian Sea-India region, is being upwarped, as if by isostatic adjustment, emphasizes the need to examine the Asiatic field with particular care. . . . (97:365).

Let us remember that a negative load means simply pressure from within the earth outwards, and positive load pressure from the surface inward. In principle, they are the same in so far as their evidence for the strength of the crust goes. It seems that here the crust is quite able to bear a large load over a great span without yielding. Daly points out that many parts of India are distorted on the positive side; there is an excess of matter over considerable areas, and he remarks:

. . . India, among all the extensive regions with relatively close networks of plumb-bob and gravity stations, is being regarded by some high authorities as departing so far from isostasy that one should no longer recognize a principle of isostasy at all. . . . (97:224–25).

A particularly important aspect of these great deviations from gravitational balance of the crust in India is that they are not local distortions, not the result of local surface features such as hills and valleys. These surface features may well once, and quite recently, have been in good isostatic balance. The distortion lies deeper:

. . . In India practically all the gravity anomalies seem to have no apparent relation to local conditions. Only one explanation seems possible—that is, that they are due to a very deep seated gentle undulation of the lower crustal layers underlying all the superficial rocks; it is evidently a very uniform, broad sweeping feature at a great depth, and must be uncompensated, since if it were compensated it would cause no anomaly at the surface (97:241–42).

Forced to find some way of explaining how the crust could bear such loads (positive and negative) in India and still yield easily to isostatic adjustment in other areas, Daly suggests that the strength of the crust in India might be explained by a recent lateral compression of the whole peninsula, which,

he says, is evidenced by the folding there of the young sedimentary rocks (97:391–92).

Daly does not suggest a possible cause for this lateral compression of the whole peninsula; such a compression, part of the process of mountain building, he has already characterized as "utterly mysterious." But it must be clear that it is precisely the type of distortion that might be expected to result from a displacement of the earth's crust. Such a movement could very well account both for the depression of lower India and for the uncompensated elevation of the Himalayas. It can be said, moreover, that no displacement of the crust could possibly take place without creating, at some points on the earth, precisely such deep-lying gentle undulations of the crust.

But still another point may be urged in support of this solution of the problem. We shall see, later on, that the last movement of the crust appears to have been approximately along the 90th meridian, with North America moving southward from the pole. This movement would have subjected India to maximum displacement and to maximum compression. In this last movement India would have been moved across the equator and northward toward the pole, to its present latitude.

Daly's suggestion that compression may increase the tensile strength of the crust opens up most interesting possibilities. We may find here, in connection with the theory of crust displacement, a solution to very puzzling problems of isostatic theory. The crust of the earth shows enormous differences from place to place in its degree of isostatic adjustment and in its sensitivity to the addition or removal of loads. Applying Daly's suggestion, we may infer that the differences may owe their origin to recent displacements of the crust. Areas recently moved poleward, having undergone compression and still retaining compression, would, according to Daly's suggestion, have greater strength to sustain the distortions; areas recently displaced equatorward, having undergone extension, or stretching, would have less strength to resist

gravitational adjustment, and, moreover, the widespread fracturing accompanying the movement would facilitate adjustment.

This suggestion of Daly's also has great significance for the understanding of the absence of much volcanism in the polar regions. It has been observed that these regions are relatively quiet, with respect to volcanoes. There is only one volcano in the whole continent of Antarctica, so far as we know. What can be the reason for this? It may be thought that this may result from the polar cold, but this cannot be true. The influence of surface temperatures penetrates only a short distance into the crust; volcanoes originate from greater depths. The solution may be found in the fact that, according to our theory, both the present polar areas are areas that were moved poleward in the last movement of the crust, and were therefore compressed. Consequently, the crust in those areas was less fractured and now has greater strength to prevent volcanic action. This increased strength may also have the effect of adding to the ability of the crust to sustain the increasing weight of the icecap, without giving way, thus tending to add to the uncompensated proportion of the icecap. In addition, Antarctica may well show isostatic distortions of the crust itself, equivalent to the positive anomalies in India.

The importance of finding a reasonable solution for the profound contradictions in the theory of isostasy has been emphasized by several recent writers. Professor Bain, of Amherst, writes:

> Isostatic adjustment exists only in imagination. I present the existence of peneplains in witness thereof. Establishment of the Rocky Mountain peneplain or the Old Flat Top Peneplain of the western states requires erosion of at least 10,000 feet of the rock over the main arch of the Front Range. The rivers wore the land down slowly to grade equilibrium without observable rise of the unloaded region or subsidence of the loaded region throwing all gravity out of equilibrium. Then in the brief interval of a small part of a geological epoch the land surface rose to re-establish near gravity equilibrium. . . . (19).

Now, as I understand Professor Bain's statement, his point is that in numerous instances erosion has worn away mountain ranges, leaving flat plains (peneplains), and in the instance he cites it seems that during the prolonged period when the erosion was taking place (erosion that resulted in removal of no less than 10,000 feet of rock from one area, and the deposition of the resulting sediments in another), the crust did not respond by rising in the first area and sinking in the second. Gravitational balance was thus sadly set askew, and remained so for a long time. Then, relatively suddenly, equilibrium was re-established. How do we explain this?

I think it is necessary to take into consideration the fact that just as compression will be at a maximum along the meridian of displacement of the crust in the poleward direction, extension or stretching will likewise be at a maximum along the same meridian in the equatorward direction. But, in both cases, areas removed from this meridian will be displaced proportionately less, and large areas will undergo very little or no displacement, and consequently very little or no compression or extension. Since, as we saw in the last chapter, successive movements of the crust may oscillate along meridians placed close together, it follows that, for long periods, compression may be sustained in particular areas and isostatic adjustment impeded in those areas. Eventually, a movement of the crust in a different direction will permit the delayed adjustment to take place.

In this way, too, we may explain the data upon which Dr. Jeffreys based his conclusion that isostasy is an exceptional condition of the earth's surface, which is re-established only at long intervals. The theory presented in this book offers a solution for the cause of the geological revolutions which, he supposed, shattered the crust at long intervals, bringing about the formation of mountains, and permitting the re-establishment of crustal balance.

With regard to the vast negative or positive distortions of isostasy, the displacement theory has a solution to offer. Let us suppose a movement of the crust causing widespread slight

distortion of the earth from its equilibrium shape, distortion such as now prevails across parts of India and all of the Arabian Sea. It is essential to realize that the long persistence of such anomalies, and the apparent lack of any tendency to adjustment, may have no relationship to the strength of the crust. It may be due, quite simply, to the fact that the matter in the sublayer (the asthenosphere) is too viscous to flow rapidly, and that when it has to flow such great distances, and in such great volume as would be required to compensate the sweeping undulations of the geoid caused by a movement of the crust, great periods of time are required, periods so long that our instruments have not been able to detect the progress of isostatic adjustment.

The advantage of the theory of crust displacements is that it can reconcile the data supporting the conviction of geologists that the crust must be too weak to support major loads out of adjustment over great spans of territory, with the observed fact that in some cases it appears to do so. Furthermore, we may, with this theory, grant the crust enough strength under certain conditions (of compression) to support heavy loads of narrow span, such as the Hawaiian Islands, and still understand its extreme weakness in areas of extension, where it appears to adjust easily to rather minor loads.

Einstein, in the Foreword, referred to the possible centrifugal effects of these distortions within the crust. The following principles apply:

a. A positive load on the crust, like the icecap, will exert a centrifugal effect equatorward; correspondingly, the effects of negative loads must be poleward.
b. The effects of positive loads on one side of the equator will be opposed to the effects of positive loads on the other side of the equator; equal positive loads in equal longitudes and latitudes will cancel each other across the equator, and the same is true of negative loads.

c. Despite the fact that such loads may cancel each other wholly or in part, in so far as the transmission of a net centrifugal momentum to the crust in any given direction is concerned, nevertheless their opposition will involve the creation of persisting stresses in the crust, and these may be a cause of seismic activity.

d. Crustal distortions, unlike icecaps, are comparatively permanent features; many may persist through one or more displacements; their effect will change quantitatively according to their changes of latitude and longitude.

e. At the termination of each crustal movement, the distortions of the rock structures of the crust should be approximately balanced across the equator. In a period of several thousand years following such a movement, however, the process of isostatic adjustment, proceeding faster in some areas than in others, may disturb this balance and predispose the crust to a new displacement.

f. We may conclude, in answer to the question raised in the Foreword, that while some of these distortions are massive, they tend to be balanced across the equator, and that the principal disturbing factor, from the quantitative standpoint, must in all probability be the rapidly growing continental icecap.

4. The Triaxial Shape of the Earth

We cannot leave the subject of the gravitational adjustment of the earth's surface without mentioning the greatest distortion of all, the triaxial deformation of the earth. It is all the more important to consider this question since here we shall see, at one and the same time, a solution for one of the greatest of geological conundrums, and one of the most powerful arguments in support of the theory of displacements of the earth's crust.

Not long ago, scientists became aware of the fact that there

is a deviation in the shape of the earth from the idealized form of a flattened, or oblate, spheroid. The increasingly accurate measurements of geodesy have shown that the earth has bumps and irregular lumps in various places, which seem to correspond to a third axis running through the earth. As a result of this, scientists now consider that the true shape of the earth is that of a "triaxial ellipsoid."

An axis, of course, is not a material thing. It is only a line that somebody imagines running through a sphere to give a dimension to that sphere in that direction. Three axes of the earth mean one through the poles, on which the earth rotates (the axis of rotation); one through the equator, called the equatorial axis, twenty-six miles longer than the polar axis; and now a third axis, roughly through the equator, at an angle to the other equatorial axis.

The result of having two axes of different lengths running through the equator is, of course, that the equator itself is a little flattened; it is oval, rather than truly circular. The flattening is very slight. According to Daly, one axis through the equator is 2,300 feet longer than the other (97:32); Jeffreys, according to Daly, prefers half that figure. Daly finds that the longer diameter through the equator (the major axis) runs from the Atlantic Ocean, at 25° W. Long., to the Pacific, at 155° E. Long., and the shorter diameter (or minor axis) runs from the western United States, at 115° W. Long., to the Indian Ocean, at 65° E. Long. (97:32). Just as the actual amount of the flattening of the equator is uncertain, so are the precise situations of the major and minor equatorial axes. More recently, determinations by the United States Coast Geodetic Survey have suggested a slightly different position for one of these axes. Moreover, the third axis apparently does not run precisely through the equator. The result is that the earth's shape is distorted by protuberances of various sizes and shapes. If we take Jeffreys's estimate of their magnitude, we see that they amount to the equivalent of about 2,000 feet of rock, or over a mile of ice, and of

course the anomalies have enormous spans, on the order of thousands of miles.

Despite their vastly greater magnitude, these triaxial protuberances have one thing in common with those in India. Just as Daly observed that the Indian anomalies must result from sweeping undulations of the geoid at some depth in the crust, underlying all the surface features, so do the triaxial protuberances indicate distortion in depth rather than at the surface. In India the surface features would be in fairly good isostatic adjustment if the deep-seated undulations were disregarded, while the geodesist Heiskanen, according to Daly, found that if he disregarded the triaxial protuberances —if he regarded the triaxial ellipsoid as the natural shape of the earth—all his anomalies were reduced to one half, both in extent and in intensity (97:368).

It does not seem reasonable simply to disregard distortions of the shape of the earth of this magnitude, unless we have an explanation of them that is convincing. Daly provided an explanation, but for a number of reasons it seems to me unsatisfactory.

It was plain to him that the strength of the crust could not possibly support such enormous distortions over such spans. Therefore he made one or two alternative suggestions, advancing them as possibilities only. He suggested, first, that assuming an original molten condition of the earth, it is possible that the material in the liquid melt was not of uniform density on opposite sides of the earth, and that therefore when the mesosphere (the inner solid shell underlying the asthenosphere) solidified, it was heavier on one side than on the other—that is, lopsided—and the resulting unevenness of gravity at the surface influenced the equilibrium, that is, the elevation from place to place, of the surface layers. This is an ingenious suggestion, but it requires the assumption of the cooling of the earth, which is itself doubtful. Thus this particular explanation rests upon speculation, and upon speculation that is not well supported.

The same is true of Professor Daly's second suggestion. He

supposes that the lopsidedness of the internal shell may have resulted from the separation of the moon from the earth, at which time the bed of the Pacific may have been created. The arguments that once supported this theory of the origin of the moon have, in recent years, been gradually whittled away, until little remains of them. This, then, is also a hazardous speculation.

Professor Daly's fertile mind has produced a third suggestion. He feels that perhaps the triaxiality may have resulted from the effects of continental drift, which he felt himself compelled to support because there was no other way to explain the innumerable facts of paleontology and geology, many of which have been already cited in this book. We have seen, however, that continental drift will not do.

It seems that all the arguments that Professor Daly uses to support his suggestion that the triaxial protuberances are not supported by the crust, but from below the crust, fail to stand examination. They are supported by no convincing mass of evidence. There is obviously a sort of desperate urgency about them. A strong need impels him to hoist them up. The nature of this need is perfectly clear.

The need is to save the theory of isostasy. It is to smooth the path in front of a theory that has a lot of useful applications and has a great deal to be said for it. The theory is threatened by the unexplained anomalies referred to above; it is still more threatened by these massive distortions of the shape of the planet, the triaxial protuberances. They are wholly and absolutely irreconcilable with the known principles of physics, as opposed to speculations. Either the shape of the earth is established by the balance of the force of gravity and the centrifugal effect of the rotation, or it is not. The geoid, so established, is distorted, and it proves impossible to explain the distortion either by the resistance of the crust to the aforementioned forces or by the (undemonstrated) lopsidedness of the internal shell.

But displacements of the earth's crust may explain the matter, and in the simplest possible fashion.

We have seen that areas displaced poleward in a movement of the crust will be elevated relatively to sea level. Two areas will be displaced poleward at the same time, one to each pole, and both will be elevated somewhat with reference to the equilibrium surface. The distance through the earth between these points will be increased slightly. At the same time, two other areas will be displaced equatorward. They will subside, and the diameter through the earth between them will be shortened to some extent. These areas will be centered on the meridian of the movement of the crust. At 90 degrees' remove on each side from this meridian, there will be no movement; here are the so-called "pivot areas" that do not change their latitude. They will therefore not change their elevation: a diameter through the earth between them will be unchanged.

As the consequence of this, we see that in one direction the diameter of the earth through the equator is shortened; in the other direction through the equator it is not. The result must inevitably be the ellipticity, or ovalarity, of the equator.

The consequences of the displacement do not end here. As we have stated from time to time, much complicated folding and faulting of the crust, much shifting of matter below the crust, would be inevitable or likely, and these would have effects at the surface, including basining and doming. Hence, some of the protuberances now being discovered may have nothing to do with the triaxial distortions, and may simply be confused with them.

Now, it is clear that this explanation of the triaxiality requires neither complicated and hazardous speculations about the earth's interior nor an incredible strength in the earth's crust. The protuberances remain because the matter below the crust is too highly viscous to flow the great distances and in the great volume that would be required to re-establish the normal shape of the earth: that is, it is too viscous to have been able to do so in the very short period that has elapsed since the last movement of the crust. But no doubt the read-

justment is proceeding slowly; no doubt the triaxial bumps are now the reduced remnants of those that existed at the end of the last movement of the crust.

If all anomalies in the crust cause centrifugal effects, then these vast triaxial protuberances must do so. These must, as I have pointed out, be balanced across the equator, or have been so at the termination of the last movement. Since then, isostatic adjustment has probably been proceeding, and therefore the balance of forces established when the crust stopped moving may now no longer exist. The instability of the crust may have been thereby increased, and the effects of its instability may supplement the increasing thrust of the growing, eccentric icecap. This suggests that the balance of the crust in its present position may be only a "trigger balance."

5. *The State of Matter Below the Crust*

The theory presented in this book depends upon the relationship between three factors: the quantity of the momentum transmitted to the crust by the icecap, the tensile strength of the crust, and the degree of weakness prevailing in the subcrustal layer, or asthenosphere. In Einstein's opinion, the existence of sufficient weakness in the asthenosphere to permit the displacement of the crust was the *only* doubtful assumption of the theory. However, we find that one and the same assumption is required for this theory, and for the whole theory of isostasy. It is my opinion that the theories stand or fall together.

In conceiving of the asthenosphere, we should not imagine a layer of soft rock distinguished from the crust and from the inner shells of the earth by sharp lines of demarcation. Instead, one grades off insensibly into another, and in all probability inequalities in thickness exist from place to place.

It is the general opinion of geophysicists that at a certain depth in the crust increasing heat and pressure bring about a

diminution of the tensile strength and rigidity of the rock. The decline of strength continues to the bottom of the crust. Dr. Jeffreys remarks: ". . . At some depth . . . it begins to decrease and may be a tenth of that of surface rock at a depth of 30 miles. . . ." (238:202).

At the bottom of the crust, perhaps about 36 miles below the surface of the earth, an important change of state apparently takes place. The heat reaches the melting point of the rocks, and the rocks can no longer crystallize. Since the strength of rocks depends mostly upon a structure of strong, interlocking crystals, the change of state implies a disappearance of strength. For this reason Professor Daly considers the asthenosphere to be "essentially liquid," ". . . For it is hardly to be doubted that a rock layer, too hot to crystallize, has only a minute strength, or no strength whatever" (97:399–400).

Jeffreys is in agreement that the melting point of rock should be reached about 36 miles down (238:140), judging from the heat gradient.

If it were only a question of the crystalline or noncrystalline structure of the rock, we would readily conclude that the asthenosphere could offer no serious resistance to the displacement of the crust. The crust would be truly (to use Einstein's term) a "floating crust."

But we must also take into account another quality of matter, which is viscosity. Materials possess varying degrees of viscosity. Viscosity can make a liquid act like a solid. If, for example, a high diver hits the surface of the water at a bad angle he may kill himself, because the water, though liquid, requires time to flow, and if the impact is too sudden there is no flow and the liquid acts as a solid. Tar is an example of a more viscous substance. Taffy candy is highly viscous, and can be cut with scissors, and yet, given a certain amount of time, it will flow like a liquid. The effect of pressure on different substances is to increase their viscosity, to make them stiffer. They will then resist sudden shocks better, but will flow like liquids if subjected to steady pressure for a con-

siderable period of time. The layer immediately under the earth's crust is subjected to high pressure, and therefore, although it is liquid, it may be stiff. The stiffness can be expected to be least immediately under the crust, and to increase with increasing depth.

However, it cannot be assumed that the viscosity immediately under the crust is very great. Bridgman has pointed out that it depends to a great extent on the particular chemical composition of the rocks, which necessarily is uncertain (50). Moreover, though pressure increases viscosity, heat diminishes it, again differently for different chemical substances. It is difficult to estimate the net effect of the operation of these opposite influences at any point under the crust.

Daly presents evidence that the viscosity of the asthenosphere must be very low. He cites, first, evidence from the edges of the area recently occupied by the Scandinavian icecap. According to isostatic principles, viscous rock must have flowed out from under the section of the crust loaded by the icecap. If the rock was very stiff, Daly argues, it would not have flowed very far, but would have upheaved the crust around the fringes of the ice sheet. Evidences of such upheaval should be observable, but there are none. He thinks that there should have been an upheaval of the Lithuanian plain, but none occurred. Hence his conclusion is that the asthenosphere must have low viscosity: ". . . And there is no apparent necessity for excluding the possibility of effectively zero strength" (97:389).

The meaning of this geological evidence appears to be that the asthenosphere must be a true liquid in terms of pressure applied over periods of the length required for the growth of the Scandinavian icecap, which would be the same length of time that we suppose would be involved in a displacement of the crust.

As a second line of evidence for a weak sublayer, Daly points out (as already mentioned) that in mountain making the crust is folded to its full depth, that horizontal sliding has to occur to permit this folding, and that horizontal sliding

would be impossible if the asthenosphere had any considerable strength. In his opinion mountain formation requires the existence of a zone of easy shear.

Thirdly, Daly urges the importance of the general body of evidence of igneous geology:

> . . . The existence of a liquid or approximately liquid asthenosphere is strongly suggested by the countless facts of igneous geology. The hypothesis that the lithosphere is crystalline, a few scores of kilometers in maximum thickness, and everywhere underlain by a hot vitreous substratum provides what appears to be the best working theory of the chemical nature of magmas and their modes of eruption. . . . In general, no petro-genetic theory that does not recognize a specific world-encircling asthenosphere of this kind has been found to explain so many facts of the field. . . . (97:399–400).

Among specific evidences that point, in Daly's opinion, to a really liquid asthenosphere are the plateau basalts, which, as we have seen, were formed by immense floods of liquid magma that engulfed hundreds of thousands of square miles of the surface at one time.

Jeffreys opposes Daly's view of the asthenosphere, and argues for continuation of considerable strength to a depth of several hundred miles. Daly notes his argument, and answers it.

Jeffreys's argument is based on the fact that an analysis of earthquake waves shows that some earthquakes originate at depths up to 420 miles below the surface. Presumably, they can originate only as the result of fracture in a solid substance, which necessarily would have some strength. Daly remarks:

> For example, Jeffreys deduces, from the reality of deep-focus shocks, a strength of about 1000 kilograms per square centimeter for the material reaching down to the 700-kilometer level at least. This is about the strength of good granite in the testing machine.
>
> To reconcile that conclusion with the demonstrated degree of isostatic equilibrium, Jeffreys suggests that isostasy is a highly exceptional condition of the earth. He assumes the condition to have been established during major orogenic disturbances, and preserved for only a relatively short time after each paroxysm of mountain-making (97:400–01).

It is not easy to reconcile Jeffreys's views here with those his I have quoted above regarding the decline of strength rock with increasing pressure in the crust. It is evident at if Jeffreys is right, the theory of isostasy is reduced to shambles. But Daly presents a counterargument to show at he is not right. He suggests that at great depths the vis-sity would be so high that a comparatively sudden ac-mulation of strain from some cause could fracture the rock if it were a solid. Laboratory experiments conducted by ridgman have shown that solids subjected to pressures so eat as to make them flow behaved in very peculiar fashion. hey would flow, but the flow would, at times, be interrupted / fracture and slip. Daly also quotes Gutenberg to the effect at ". . . Whereas at normal depths the accumulation of rain is made possible by the strength of the rocks, at the eater depths the high coefficient of viscosity is sufficient, id no conclusion as to strength can be drawn" (97:403). hus the counterargument is a strong one. All that is neces-ry, in fact, both for the theory of isostasy and for the theory displacements of the crust is a thin layer of extreme weak-ess at the top of the asthenosphere, where the viscosity ould be much less than at the greater depths.

According to Daly, Jeffreys adopted his view because he uld see no reason for the development of sudden stresses the greater depths. Daly argues that this cannot settle the atter; perhaps we shall eventually discover a sufficient use. Daly also says, when speaking of the earth's triaxiality, at it means "stress at depths of thousands of kilometers" 7:404). Putting two and two together, I would suggest that e deep-focus earthquakes result from the triaxiality, and at the stresses develop suddenly enough to exceed the lim-s of the viscosity at that depth and cause fracture, because e triaxial bulges are not permanent features of the planet, ut recent deformations, which must set up strains at con-derable depths.

Archibald Geikie describes experiments done a long time

ago, which are still suggestive of the probable behavior of material in the asthenosphere:

> The ingenious experiments of M. Tresca on the flow of solids have thrown considerable light on the internal deformations of rock-masses. He has proved that, even at ordinary atmospheric temperatures, solid resisting bodies like lead, cast iron, and ice may be so compressed as to undergo an internal motion of their parts, closely analogous to that of fluids. Thus a solid jet of lead has been produced, by placing a piece of the metal between the jaws of a powerful compressing machine. Iron, in like manner, has been forced to flow in the solid state into cavities and take their shape. On cutting sections of the metal so compressed, their particles of crystals are found to have arranged themselves in lines of flow which follow the contours of the space into which they have been squeezed. . . . (170:316).

It seems altogether unlikely that material under the pressures prevailing at the bottom of the crust, and at very high temperatures, could resist the shearing movement of the crust over it. Let us remember that in a displacement of the crust very little material, comparatively speaking, would actually have to flow. The crust would simply start to slip over the asthenosphere. The action would be one of gliding, the most economical form of motion, though, as already explained, the downward protuberances of continents and mountain chains might act to slow down the movement.

To conclude, then, the asthenosphere could have considerable viscosity, even at the top, and yet offer no definite obstacle to the displacement of the crust. Once the crust started to move, the braking influence of the viscosity would steadily decline, for with the increasing distance of the center of mass of the icecap from the axis, the centrifugal effect would be multiplied (Chapter XII). No friction between the two layers could suffice to absorb this ever increasing thrust. It would have to continue until the motive force was removed by the melting of the icecap in the warmer latitudes. And, in the meantime, as Frankland has suggested, the friction would have been productive of heat that might have further facilitated the movement.

I have not referred to another important aspect of the behavior of materials under pressure, one that may have its importance. If the viscosity of matter increases with pressure, so does plasticity. Solids become more and more plastic under pressure. At the bottom of the crust matter is under enormous pressure. That means that very little force may be required to overcome its rigidity. It also means that when a critical point is reached, the material may deform suddenly. It does not give way at a speed proportional to the pressure applied, as in the case of a viscous liquid, but at a speed that has no relationship to the applied force. Bridgman has pointed out that the exact degree of plasticity at the bottom of the crust, like that of the viscosity, cannot be determined because of our ignorance of the chemical composition of the materials.

This question of plastic deformation raises an interesting possibility. What if, at a certain point in the displacement, viscous deformation gives way to plastic deformation? Suppose the movement starts slowly as a gliding over a viscous surface. Suppose it gains enough speed so that, at the interface between the crust and the underlying plastic layer, a plastic type of yielding occurs. The interactions here would be complex, but the possibility looms that at times the displacement of the crust could take place with considerable speed.

I have mentioned that the gliding motion of the crust might be impeded at times by the downward projections of its undersurface. However, if there is one situation in which plastic deformation might be considered probable, it is one in which these downward projections—for example, the underbody of a continent—were displaced against upward projections of the asthenosphere under the ocean basins. Here considerable pressures would arise, and various circumstances might concentrate these pressures in narrow regions and intensify them; the pressures might thus reach the critical point of plastic deformation, and result in the abrupt shearing off of larger or smaller segments of the downward pro-

jections of the crust—the roots of continents and mountain ranges. This would result in comparatively sudden changes at the surface, at least in areas being moved equatorward and thus stretched and weakened. It would also facilitate the process of planing off and shaping the continental sides as suggested in the last chapter.[1]

[1] Newspaper reports of observations now being made in Antarctica, as part of the scientific program of the present International Geophysical Year, indicate a depth for the ice sheet at the South Pole of about 8,000 feet. This suggests that Campbell and I may have considerably underestimated the total mass and weight of the icecap. At the same time, the reports suggest very low prevailing temperatures, and therefore high rigidity, for the ice, at some depth below the surface. (See p. 170.)

VII : NORTH AMERICA AT THE POLE

In the preceding chapters much evidence has been presented to support the contention that the earth's crust has often been displaced. Perhaps the reader will feel that the general evidence is sufficient. It remains, nonetheless, to show beyond a reasonable doubt that such a movement actually did occur in one specific instance. I have already suggested that the last movement may have been the immediate cause of the end of the last ice age in North America and in Europe. In this chapter I will review the evidence for this. At the same time, I will try to show why the circumstances of the displacement themselves indicate and, in fact, require the further conclusion that the icecap in North America must itself have been the agent of the displacement.

1. The Polar Icecap

Several independent lines of evidence, each individually extremely impressive, unite to suggest that the Hudson Bay region lay at the North Pole during the so-called Wisconsin glaciation.

The first line of evidence is based on the shape, and on the peculiar geographical position, of the last North American icecap. Kelly and Dachille point out that the area occupied by the ice was similar both in shape and in size to the present Arctic Circle (248:39). Many geologists have remarked on the unnatural location of the icecap. It occupied the northeastern rather than the northern half of the continent. Some of the northern islands in the Arctic Ocean, and northern Greenland, were left unglaciated (87:28, note). Alaska and the Yukon had mountain glaciers but no continuous ice sheet. Then, the ice is known to have been thicker

and to have extended farther south on the low central plains of the Mississippi Valley than it did on the high mountain areas in the same latitudes farther west. But according to accepted ideas about glaciation, if the ice age was the result of a general lowering of world temperatures, the ice should have formed first in the mountain areas, and it should have extended farther south on them than in the low plains. There has been no explanation of this, which may have been one of the problems that led Daly to remark that "The Pleistocene history of North America holds ten major mysteries for every one that has already been solved" (93:111).

The assumption that the Hudson Bay region then lay at the pole would make the facts easy to explain, for in this case the western highlands would lie to the south of the plains region, and one would therefore expect thicker ice on the plains lying nearer the pole. Absence of continuous glaciation in Alaska and in the Arctic islands would be easily explained. Furthermore, the fact that the European ice sheet was thinner than the North American and did not extend so far south would be understandable. The relationship between the North American and contemporary European glaciations will be discussed further below.

A second line of evidence for the position of North America at the pole consists of the new data regarding recent climatic change in Antarctica, already discussed (Chapter II). A movement of the crust that would move North America southward about 2,000 miles would also necessarily move Antarctica that much nearer the South Pole (see globe). Therefore, a displacement of the crust accounts both for the deglaciation of North America and for the expansion of the icecap in Antarctica, and it accounts for the two events being simultaneous. No other hypothesis so far suggested can account for climatic revolutions in opposite directions on the two continents. No assumption of ice ages resulting from a simultaneous world-wide reduction of temperature will fit the facts. In a personal interview, I once asked Einstein if

he could see any logical alternative to crust displacement as the explanation of these facts. He replied that he was persuaded of the soundness of the method of crust radioelement dating developed by Professor Urry, and that he saw no other reasonable explanation of the evidence (see page 364). A third line of evidence is that presented by Dr. Pauly, already discussed (Chapter III).

A fourth line of argument is developed by Lawrence Dillon, who shows, first, that the essential condition governing the growth of ice sheets seems to be not the average year-around temperature, nor the amount of annual precipitation, but the mean summer temperature (114:167). He points out that no ice sheets form at the present time in areas with mean summer temperatures of 45° F. or higher, and suggests that they probably didn't in the past. He cites, as a good illustration of this principle, the northeastern section of Siberia, which is unglaciated despite the fact that it is the "cold pole" of the world, and although it has a higher annual precipitation than Greenland or Antarctica. But the summer temperature is high, and this he thinks is the controlling factor.

Dillon points out, next, that the existence of the Wisconsin glacier would have demanded a decrease of 25° C. in average summer temperatures as they exist now (114:167). But he notes that according to Antevs the average temperature decrease in late Pleistocene time along the 105th meridian in southern Colorado and northern New Mexico as compared with the present was only 10° F., while (according to Meyer) the average temperatures during the glacial period in the equatorial Andes were only 5° or 6° F. lower than at present (114).

Thus Dillon shows that there was no uniform decrease of summer temperatures during the glacial period. No world-wide factor, such as variations in solar radiation, reduced the temperature. The range of summer temperatures would be understandable if the ice sheet were a polar icecap, however, and the range appears to require that assumption.

> . . . On the other hand, the only apparent alternate hypothesis—that of a uniform depression of the mean temperature of say 10° F. would suggest a July mean of 60° F. for the ice sheet's lower boundary, which is similar to that of present-day England, or northern Germany, or the State of Maine, but with somewhat colder winters. Since no glaciers or permanent snow fields are known to exist today under such mild climates, it seems scarcely likely that they could have done so in former times (114:168).

Dillon does not explicitly suggest a movement of the crust, but he leaves no alternative.

A fifth line of argument may be based on some evidence used by Wegener to support his theory of drifting continents. He quoted the glaciologist Penck as saying that the Pleistocene snowline lay about 1,500 to 1,800 feet lower in Tasmania than in New Zealand, and added, "This is very difficult to understand because of the present nearly equal latitudes of the two localities" (450:111). Wegener, of course, explained the matter by his theory of continental drift. If, however, his theory is rejected, crust displacement may provide a solution, for if the Hudson Bay region was then located at the North Pole, as we suppose, Tasmania would have been a good many degrees nearer the South Pole than New Zealand, as a glance at the globe will make plain. Another bit of evidence that fits in here is the apparent retreat of glaciers in South Australia about 10,000 years ago (16).

A sixth line of argument may be based on the evidence for world-wide volcanism at the end of the Wisconsin glaciation. Extensive volcanic activity is an inevitable corollary of a general movement of the earth's crust. I shall present the argument that the volcanism incident to the movement accounts for the numerous oscillations of the Wisconsin ice sheet, and for the following "Climatic Optimum."

A seventh line of evidence is provided by the mass of data relating to changes in sea level at the end of the ice age. I shall attempt to show that these changes cannot be explained by the melting of the northern icecaps, though they may be explained by a displacement of the crust, on the basis of principles already discussed.

An eighth line of evidence is presented by the story of the extinctions of many kinds of animals at the end of the ice age, and this is so important that it will require a chapter by itself (Chapter VIII). Much additional evidence based on marine and land sediments will also be presented later (Chapter IX).

2. *The Displacement Caused by the Ice Sheet*

It may be argued that convincing evidence of a displacement of the crust by no means requires the further conclusion that the movement at the end of the ice age was the result of the centrifugal effects of the North American icecap. A dozen other possibilities may be thought to exist. Several of them may be worth considering. Why, then, must we jump to the conclusion that the event was related causally to the icecap?

There have been several suggestions to account for shifts of the crust by other agencies than icecaps. What is the common element of these suggestions? It can easily be pointed out. All of them involve long periods of time. Gold's suggestion involves periods of the order of a million years. Bain's involves periods of a great many million years between movements. Ma's involves long periods between movements, terminated by cataclysms. Eddington's type of displacement, if it could be made to work at all, would necessarily be very slow. Besides their common inability to explain the velocity of events revealed by the new methods of radioelement dating, the suggestions are unsatisfactory also because they are vague as regards the mechanism of displacement. They can be grounded neither upon detailed observations nor upon mathematical calculations. The mechanism developed by Campbell, on the other hand, is quite definite and precise (although it, too, necessarily must involve assumptions). Of special importance is the fact that Campbell's mechanism is capable of being checked against geological observations in some detail.

To begin with, it is clear that a massive centrifugal effect must have been created by the Wisconsin ice sheet, if the considerations presented in Chapter VI are sound. Radioelement dating has shown that the ice sheet developed in a very short time. A high degree of isostatic compensation of the icecap is therefore unlikely, even if isostatic compensation could really eliminate the effect.

It is significant that the Wisconsin ice sheet was asymmetrical in its distribution about the center from which it spread. If we assume that the ice center from which the icecap radiated coincided at that time with the pole, then this asymmetrical distribution must have resulted in a centrifugal effect. Furthermore, it appears that the great bulk of the ice lay to the south of the ice center, and so therefore the direction of the resulting centrifugal thrust would have been southward, and the result would have been to shift the Hudson Bay region due south from the pole toward its present latitude. This is indeed in remarkable agreement with the theory. The facts are reported by W. F. Tanner, writing in *Science,* under the title "The North-South Asymmetry of the Pleistocene Ice Sheet" (414).

There are some comparisons between this North American icecap and the present icecap in Antarctica that are worth making. The North American icecap is thought to have covered about 4,000,000 square miles, as compared with the nearly 6,000,000 square miles of the present Antarctic cap. It may be asked, Why should the smaller North American icecap have started a slide of the earth's crust, when this larger one in Antarctica has not? The answer to this appears to lie in the different degrees of eccentricity, or asymmetry, of the two icecaps. In Antarctica, the pole is fairly near the center of the continent, so that the real asymmetry of the icecap is not at first glance apparent. In North America the presumed pole in Hudson Bay or perhaps in western Quebec was on the eastern side of the continent, quite near the sea. In this situation, the icecap was more eccentric. Its center of gravity was in all probability much farther from the pole

than is the case in Antarctica, and the centrifugal effect accordingly would have been much greater in proportion to the quantity of ice.

If the pole was situated in the Hudson Bay region, the closeness of the sea would have been a factor aiding the rapid growth of the icecap, and giving less time for possible isostatic adjustment.

An additional observation worth making, perhaps, is that if this vast ice sheet had developed so rapidly at the present latitude of Hudson Bay, the centrifugal effect would have been colossal. If the centrifugal effect of the Antarctic icecap, with its center of gravity 345 miles from the pole, is sufficient to produce a bursting stress almost equal to the estimated tensile strengths of the crust, the smaller North American icecap, with its center of gravity about 2,500 miles from the pole, would have produced a bursting stress many times greater than the crustal strength. Why this must be so, the reader may see by referring to Figure XII (p. 343). On this plate, the second parallelogram represents the centrifugal effect of the present Antarctic icecap on the assumption that the icecap could be displaced, without melting, as far as the 45th parallel of latitude. It is evident that at the 45th parallel the centrifugal effect would be approximately six times greater than the effect produced by the icecap with its center of mass where it is now, about 345 miles from the pole. The bursting stress would be increased in proportion, being always 500 times the centrifugal effect (Chapter XI). It seems unreasonable to suppose that at the end of the ice age the crust could have withstood a stress six times greater than our present estimate of its strength.

The reader is free to conclude from the foregoing, either that the North American icecap must have been a polar icecap (because it could never have developed to its full size at the present low latitude of the glaciated region without moving the crust) or that the movement of the crust from any such agency is impossible. But, as we have seen (Chapter VI),

he would have trouble in finding a reasonable basis for the latter conclusion.

We can therefore conclude that, on the whole, the North American icecap is a good candidate for the position of prime mover in the last displacement of the crust. The argument will be strengthened when we consider, below, its detailed history, and the implications of the extraordinary tempo of its development and of its subsequent decay. The essential argument in favor of icecaps is the time factor, for the rate of their accumulation and of their melting is obviously many times faster than that of any other process creating unbalance in the distribution of materials on the earth's surface.

Horberg has recently collected and studied, as already mentioned, the radiocarbon dates bearing on the history of the Wisconsin ice sheet. According to him, the following is its short and violent history (222:281):

a. The first known advance of the icecap—its first appearance in Ohio—is dated at merely 25,100 years ago. This is called the "Farmdale Advance." It was formerly thought to have occurred as much as 100,000 or even 150,000 years ago. This date, then, cuts the time for the later history of the ice sheet by about three quarters. Six different radiocarbon dates, all of the Farmdale Advance, show that the expansion continued until at least 22,900 years ago, or for about 3,000 years. Then there was an unexplained interval of warm climate (which I will explain later on), called the "Farmdale-Iowan Interstadial." This warm period lasted about 1,500 years, during which the ice withdrew a certain distance.

b. Following the recession, a new advance of the icecap occurred. This is referred to as the "Iowan Advance." It began about 21,400 years ago, lasted about 700 years, and was interrupted by a new recession about 20,700 years ago.

c. This second recession, after less than a thousand years, was succeeded by an extremely massive advance during the period from 19,980 to 18,050 years ago. These dates must not be taken as absolutely exact; there is always a small mar-

gin of error. This new expansion, called the "Tazewell Advance," apparently carried the Wisconsin icecap to its maximum extension and greatest volume.

d. The Tazewell Advance was interrupted by a prolonged period of warmth and recession called the "Brady Interval" or "Brady Interstadial." This lasted between three and four thousand years. It began before 16,720 years ago, and ended sometime after 14,042 years ago. The ice retreated a long way.

e. A fourth advance of the ice sheet beginning about 13,600 years ago, and continuing to about 12,120 years ago (called the "Cary Advance"), was followed by the "Two Creeks Interstadial," an interval of warmth and recession, about 11,404 years ago.

f. A fifth advance of the ice, referred to as the "Mankato Advance," appears to have taken place between 10,856 and 8,200 years ago. The high point of this advance is called the "Mankato Maximum." Another writer, Emiliani, finds that a sixth expansion of the ice sheet, the "Cochrane Advance," took place less than 7,000 years ago (132).

g. There was a sudden, virtually complete disappearance of the ice sheet (which had, however, according to Flint, been getting thinner ever since the Tazewell Advance) (375:177). It disappeared in an extraordinarily short period, as shown by a postglacial date from Cochrane, Ontario, 6,380 years ago.

h. The significance of the postglacial date from Ontario (close to the center of the former ice sheet) is increased when we compare it with the date of the postglacial Climatic Optimum, which Flint finds to have occurred between 6,000 and 4,000 years ago. The climate during the Optimum, according to Brooks (52:296–97), averaged about 5 degrees warmer than at present. There is a very difficult problem here of accounting for the velocity of these events. It is obvious that the cold glacial climate of North America must have warmed up to something like the present prevailing temperatures before it could warm up still further to the point reached in the Optimum. But if so, could the whole warming process

have taken place in 380 years? It seems probable that the Cochrane Advance was local and minor. The Mankato Maximum, however, was not. The entire transformation of the climate must then have taken place in about 2,000 years. In this short interval a continental icecap disappeared. It had been growing thinner for a long time, to be sure—for about 10,000 years, since the Tazewell Advance—but its final dissolution was sudden. Compared with the usual geological time concepts, even the period of 10,000 years for the decline of the ice sheet from the end of the Tazewell Advance is incredibly rapid. Horberg, as I have mentioned, has pointed out that if the radiocarbon method is valid, the rate at which the ice must have advanced and retreated indicates that geological processes (especially meteorological processes like rainfall) must have been greatly accelerated during the ice age. Now it is easy to show that these processes inevitably would have been accelerated by a movement of the crust; we shall return to consider this matter in detail below.

As a matter of fact, it is not necessary to depend wholly upon radiocarbon dating to establish the extraordinary velocity of the geological events of the ice age. Professor Urry's method of radioelement dating, used to date the cores obtained by Hough from Antarctica and elsewhere, shows precisely the same thing: the datings obtained by this method indicate several rapid glaciations and deglaciations of Antarctica, and correlated world-wide changes of climate. We will consider these again in Chapter IX.

A third line of evidence tending to the same effect is presented by Emiliani, who has applied a technique of determining ancient temperatures of sea water that was developed by Harold C. Urey. Urey's method is based on an isotope of oxygen. Emiliani has noted many important temperature changes in a comparatively short period during the latter part of the Pleistocene; he has reached the conclusion that the four known Pleistocene glaciations all occurred in the last 300,000 years. He agrees essentially with Horberg as to the date of the beginning of the Wisconsin glaciation (132).

Assuming the radiocarbon dates to be correct, then, we find that at the end of the Tazewell Advance there was a recession, and that despite the readvances the ice gradually thinned until the ice sheet disappeared. This can be accounted for by the assumption that the crust was in motion, and that it continued to move slowly during all or most of the 10,000 years during which the icecap was in intermittent decline. As I have already pointed out, there is no other reasonable explanation for the disappearance of the ice sheet. But the assumption is strengthened by a most remarkable fact. It would have to be considered probable, as following naturally from the theory, that as the crust moved there would be a period, possibly prolonged, when the melting on the equatorward side of the icecap would be balanced and even more than balanced by further build-up of the icecap on the poleward side. Thus, as the Wisconsin icecap moved southward, build-up of the ice would continue on its northern side. The result would be that the ice center, the center of maximum thickness, from which the ice sheet would move out by gravity in all directions, would be displaced to the north. And this is exactly what happened. Coleman writes:

> Two important facts have been established by Low, who worked over the central parts of the Labrador sheet: first, that the center of the glaciation shifted its position, at one time being in Lat. 51 or 52, later in Lat. 54, and finally in Lat. 55 or 56. Instead of beginning in the north and growing southward it reversed this direction; second, that the central area shows few signs of glaciation, so that the pre-glacial debris due to ages of weathering are almost undisturbed. A broad circle around it is scoured clean to the solid rock. . . . (87:117).

There really could be no more eloquent confirmation of the southward displacement of the earth's crust. We see verification here of one of the important mechanisms of displacement as suggested by Campbell, which is that the continuing build-up of the ice sheet on its poleward side as it moves away from the pole will be a factor in prolonging the movement. This may, indeed, result in the prolongation of the

movement until the arrival of an oceanic area (in this case, the Arctic Ocean) at the pole. As to the second fact that, according to Coleman, was established by Low, I shall suggest an explanation later (Chapter VIII).

3. *The Cause of the Oscillations of the Ice Sheet and the Cause of the Climatic Optimum*

But what about the alternating phases of retreat and readvance of the ice sheet? The retreats can be explained, of course, by the assumption that the icecap was moving slowly into lower latitudes with the displacement of the crust. But how are the readvances to be explained? Up to the present there has been no explanation for these.

I have already suggested that a corollary of any crust displacement is an increase of volcanic activity. There have been times in the past, however, when the quantity of volcanic action has been extraordinary (231:629). As an example of this, there appears to be evidence that in a small area of only 300 square miles in Scandinavia during Tertiary times there may have been as many as 70 active volcanoes at about the same time. Bergquist, who cites the evidence, remarks, "Volcanic activity on this scale, erupting through about 70 channels, and concentrated in a relatively short period, must have been very impressive" (31:194).

It seems likely that a phase of intense volcanism would be favored where a sector of the crust was moving toward the equator, and undergoing stretching (or "extension") together with widespread fracturing, for we must remember that this pulling apart of the crust would permit the release of many pre-existent strains, the upward or downward adjustment of blocks that had been held out of isostatic adjustment for a longer or shorter time. There would certainly be a general rise of igneous matter into millions of new fractures, and occasionally this could result in overflows at the surface, including, as has already been pointed out, vast lava floods.

A special phase of the volcanism must now attract our attention. Most volcanoes produce dust, sometimes in vast quantities (87:271). This dust is rapidly distributed through the atmosphere. The effects of volcanic dust on the climate have been the subject of intensive studies (231, 375). We must stop for a moment to summarize the essential results of these studies.

The fundamental work on the relationship of volcanic dust to climate is *The Physics of the Air,* by Humphreys, which has been cited in earlier chapters. Humphreys shows that volcanic dust can have a remarkable effect in lowering temperature. He points out that the effect of the particles depends upon whether they happen to be more efficient in intercepting the sun's light and reflecting it back into space than they are in preventing the radiation of the earth's heat into outer space. What is important is the size and shape of the dust particles as compared with the wave lengths of the radiation. Particles of a given length will have great reflecting and scattering power on sunlight, and none on the radiation of heat from the earth (which, of course, is not in the form of light). Humphreys concludes that it is necessary to determine the approximate average size of the individual grains of floating volcanic dust, as well as the wave lengths of the radiation involved. He accomplishes this satisfactorily. After mathematical treatment of the various factors he concludes: ". . . the shell of volcanic dust, the particles all being of the size given, is some thirty fold more effective in shutting out solar radiation than it is in keeping terrestrial radiation in. . . ." (231:580). He also points out:

> . . . The total quantity of dust sufficient . . . to cut down the intensity of solar radiation by 20% . . . is astonishingly small—only 174th part of a cubic kilometer, or the 727th part of a cubic mile. . . . (231:583).

This, of course, means that the sun's radiation is reduced to this extent over the whole surface of the earth. It requires only a few days for volcanic dust projected into the

upper atmosphere to be distributed around the world. Apparently, the amount of dust produced by the eruption of Mt. Katmai in Alaska in 1912 was sufficient slightly to lower the temperature of the whole earth's surface for a period of two or three years (87:270; 231:569). For long-range effects a continuous series of explosions would be necessary, because volcanic dust settles out of the atmosphere in periods of the order of three years. Humphreys presents a great deal of evidence correlating variations in average annual global temperatures through the nineteenth century, with specific volcanic eruptions. He establishes the fact that the eruptions certainly had an important influence.

If this is true of our times, what should we expect to result from the activation of very great numbers of volcanoes during a displacement of the crust? Not only would the temperature fall, and perhaps very drastically, but continuing volcanic outbursts would keep it low. At the same time, the alternation of periods of massive outbursts with periods of quiet would produce violent variations of the climate, between extremes of cold and warmth.

Here we have our explanation of the five or six major readvances of the Wisconsin ice sheet (there were, apparently, many more minor ones). In all probability, they resulted from the long continuation of massive outbursts of volcanism. The readvances of the ice are explained by volcanism, and the volcanism is explained by the displacement of the crust.

It is not necessary, however, for us merely to assume without evidence that there must have been unusual volcanic activity at the end of the ice age. On the contrary, there is a rather remarkable amount of evidence of excessive volcanism during the decline of the Wisconsin icecap. It comes from many parts of the earth. For North America it is particularly rich. From radiocarbon dating we have learned that during the last part of the ice age there were active volcanoes in our northwestern states. One of the greatest eruptions was that of Mt. Newberry in southern Oregon less than 9,000 years

ago (242:23). Other late glacial or early postglacial volcanic activity in Oregon was reported by Hansen (199). Farther south the story is the same:

> In Arizona, New Mexico and southern California there are very fresh looking volcanic formations. The lava flow in the valley of the San Jose River in New Mexico is so fresh that it lends support to Indian traditions of a "river of fire" in this locality (235:113).

Volcanic disturbances in South America about 9,000 years ago have been dated by radiocarbon (242:45). Huntington reported "lava flows of the glacial period interstratified with piedmont gravel" in Central Asia (232:168). Ebba Hult de Geer quoted Franz Firbaz as follows: "The volcanic eruptions that produced the Laacher marine volcanic ash are about 11,000 years old, or a little older. . . ." (108:515). Hibben suggested that the extinctions of animals in Alaska at the end of the ice age may have been due to terrific volcanic eruptions there, of which the evidence is plentiful (218). We will return to his account later.

Volcanic dust is not the only important product of volcanic eruptions. They also produce vast quantities of carbon dioxide gas. Tazieff, for example, estimated that in one eruption he observed in Africa, along with about seventy-eight million tons of lava, the volcano emitted twenty billion cubic yards of gas (416:217), not all of which, of course, was carbon dioxide.

The carbon dioxide emitted by volcanoes has an important effect on global temperature, but one quite different from the effect of the volcanic dust. Being a translucent gas, it does not interfere with the entrance of sunlight, of radiant heat, into the atmosphere. But it is opaque to the radiation of the earth's heat into outer space. A small quantity of the gas will act effectively to prevent loss of heat from the earth's surface. A considerable increase in this small percentage will tend to raise the average temperatures of the earth's surface.

Carbon dioxide differs from volcanic dust also in the fact that because it is a gas it will not settle out of the atmosphere.

It will remain until, in the course of time, it is absorbed by the vegetation, or by chemical processes in the rock surfaces exposed to the weather. Therefore, as compared with volcanic dust, carbon dioxide is a long-range factor, and its effect is opposite to that of the dust.

In any displacement of the crust it follows that massive outbursts of volcanism must have added to the supply of carbon dioxide in the air. Its proportion in the atmosphere must have finally been raised far above normal. In consequence, it is likely that whenever volcanic activity declined sufficiently to permit a warming of the climate, the high proportion of carbon dioxide in the air may have acted to intensify the upward swing of the temperature. This would have increased the violence of the oscillations of the climate, and would have accelerated many geological processes.

Evidence that the proportion of carbon dioxide in the air was, in fact, higher toward the end of the ice age than it is now is provided by recent studies of gases contained in icebergs. Scholander and Kanwisher, writing in *Science,* reported that air frozen into these bergs, presumably dating from the ice age, showed lower oxygen content than air has at the present time, and theorized:

> Possibly this ice was formed as far back as Pleistocene time, when cold climates may have curbed the photosynthetic activity of green plants over large parts of the earth, resulting in a slight lowering of the oxygen content of the air (368:104–05).

The weight of a great deal of evidence presented in this book is opposed to this particular speculation; we must suppose, on the contrary, that the earth's surface as a whole was then no colder than it is now, and that just as many plants were absorbing carbon dioxide and releasing oxygen into the air then as now. But the same fact—the lower proportion of oxygen—may perhaps be explained by supposing a higher proportion of carbon dioxide, especially if we assume a massive increase in the proportion of that gas in the air.

Another consideration that greatly strengthens this line of

thinking about the carbon dioxide is that the assumption of a cumulative increase in the proportion of the gas in the air, during the movement of the crust and the waning of the ice sheet, helps to explain not only the extraordinarily rapid final melting of the ice but also the succeeding Climatic Optimum.

The Climatic Optimum is the most important climatic episode since the end of the ice age; the fact of the occurrence is well attested, but it is unexplained. Scientists have been aware that this 2,000-year warming of the climate could have resulted from an increase in the carbon dioxide content of the air, but this has not been helpful, since hitherto no way has been found by which to account for an increase of the required magnitude. No other possible cause of the warm phase (such as an increase in the quantity of the sun's radiant heat) has been supported by tangible evidence. It seems that the assumption of a displacement of the crust furnishes the first possibility of a solution.

To return, for a moment, to the question of the several readvances of the ice, it may be asked, Why did the volcanism occur in massive outbursts separated by quieter periods? Why was it not continuous through the whole movement of the crust? Campbell has suggested an answer. It is quite possible that the fracturing of the crust, necessary to permit the displacement, was itself spasmodic. We may assume that when the mounting bursting stress brought to bear on the crust by the growth of the icecap finally reached the critical point (that is, the limit of the crust's strength), considerable fracturing occurred in a rather short time, accompanied by massive volcanism. The crust would now start to move, and it would continue to move easily to the distance permitted by the extent of the fractures so far created. The movement might then come to a halt, and the accompanying volcanism would tend then to subside. Meanwhile, on the poleward side of the icecap the ice still would be building up, and the bursting stress resulting from it would again be on the increase. New fracturing would eventually occur, with

a new outburst of volcanism, and the movement would be renewed. In the intervals between phases of intense volcanism the dust would settle out of the air, the carbon dioxide would take effect, and the climate would rapidly grow warmer. This warm "interstadial," however, would affect different sides of the icecap differently; it could cause important recessions of the ice sheet on the equatorward side, and at the same time, because of the accompanying rise in humidity, it could augment the snowfall on the poleward side.

The crust would continue to move, even though with each recurring warm period the ice sheet grew thinner, because the icecap, at each successive stage, would have been moved farther from the axis of rotation, so that the effects of the diminishing quantity of the ice would be effectively counterbalanced by the multiplication of the centrifugal effect per unit volume of the remaining ice (see Chapter XI).

Finally a time would come when the rising temperatures of the lower latitudes and the accumulation of carbon dioxide would so far exceed the refrigerating effects of volcanic outbursts that the latter would become impotent to maintain the icecap. With the reduction of the icecap below a certain point, the crust would cease to move, volcanic disturbance would decline, the air would be cleared of dust, and within a short time the accumulated carbon dioxide would usher in the warm phase of the Optimum. The story ends with the absorption of the carbon dioxide by the vegetation, the reduction of its percentage to normal, and the establishment of a climate like the present.

4. The Glaciation of Europe

Further confirmation of the position of North America at the pole during the Wisconsin glaciation comes from Europe. Radiocarbon dating has revealed some very interesting

facts about the relationship of the American and Scandinavian glaciers.

It has been shown, by Flint (375:175) and others, that there is a correspondence in the timing of the phases of advance and retreat of the ice on both sides of the Atlantic. This is exactly what we should expect, considering that the causes of the oscillations, the volcanic dust and the carbon dioxide, were world-wide in their effects.

But despite this synchronism, there is also an important difference between the two glaciations: it is clear that the icecap in Europe underwent proportionately greater diminution with each phase of the recession after the Tazewell Maximum. The assumption that the glaciations on both sides of the Atlantic were in all respects precisely contemporary, that they advanced and retreated equally at equal times, has now produced contradictions of a very glaring character. It has placed two specialists, Ernst Antevs and Ebba Hult de Geer, at odds with each other.

The basis of the contradiction is as follows: The late husband of Ebba Hult de Geer, Gerard de Geer, was the author of the so-called "Swedish Time Scale." This is a method of geological dating based on countings and comparisons of annually deposited layers of clay (varves) in lakes. De Geer first developed the method more than a generation ago. In a number of instances datings established by it have been well confirmed by the more recent radiocarbon method.

De Geer found that by about 13,000 years ago the Scandinavian icecap had retired from Germany and England, and that the ice front lay across Sweden. It is obvious that its withdrawal from Germany and England must have started thousands of years earlier. By 13,000 years ago a large percentage of the whole European icecap was gone. In America, however, the reduction of the Wisconsin icecap had proceeded nowhere near so far. Twice again, after this, the American icecap expanded in the Cary and Mankato Advances. The great icecap, though thinner, still occupied most of its original area.

Now, what Dr. Antevs says is that the radiocarbon dates from America don't make sense, and the radiocarbon method must be wrong (9:516). He attacks the method because the rates of withdrawal of the ice which it suggests are to him fantastic. He complains particularly about the disproportion in the indicated speeds of withdrawal in America and Europe. The radiocarbon method has indicated that the Mankato Maximum occurred between 11,000 and 10,000 years ago. This stage has been related chronologically to the so-called Salpausselka Stage in Europe. Antevs considers this totally unreasonable, because

> This correlation equates a point at less than one-quarter of the American ice-sheet radius with one at the half way mark in Europe. Clearly this lop-sided matching cannot be right.

He says, further:

> Other implications are equally unreasonable. . . . The Canadian ice sheet would still have touched Lake Superior when the Scandinavian ice sheet had entirely disappeared. . . .

Antevs makes the conflict more explicit, as follows:

> The North American ice sheet would still have extended to the middle of the Great Lakes when the Scandinavian ice sheet had entirely disappeared, for the latter had melted from the Angermanalven basis by 8550 B.P. [Before the Present], and from Lapland by 7800 B.P., and what is more, the ice would still have lingered in these lakes while the distinctly warmer Altithermal [Climatic Optimum] came and went. The ice retreat would have been exceedingly slow during the Altithermal but extraordinarily rapid during the last 3500 years, which have been only moderately warm (9:519).

Antevs is therefore driven into a blank rejection of radiocarbon dating. He insists, in contradiction to all such datings, that the icecap must have left the Canadian Mattawa Valley about 13,700 years ago, and that the Mankato Maximum (which he refers to as the Valders Maximum) must have occurred about 19,000 years ago (9:520). In the discussion in the pages of the *Journal of Geology,* between him and Mrs. de Geer, Mrs. de Geer insists on the high reliability of the

radiocarbon method and on the general agreement of the radiocarbon dates with the dates found by the Swedish Time Scale. With regard to the date of the Mankato Advance challenged by Antevs, she says:

> The whole method of C_{14} determinations, however, is taken up by America's most clever research men and practiced very critically —most of all the special test at Two Creeks. As they were startled by the low figures of years obtained, they repeated the investigation several times. As the same value always recurred, such critical persons might well have examined eventual deficiencies of the material before publishing a result regarded generally as unbelievable. Since such a procedure was not found necessary, the test is probably reliable, although many others may be doubtful (108:514).

However, Antevs succeeds in making it plain that some of the late Dr. de Geer's dates, as found by the method of counting clay varves, are inconsistent with radiocarbon dates.

Now here is a shocking conflict between experts, each with years of experience in the field and direct access to all the relevant data. How can it be resolved? It seems very likely that the evidence stressed by both is largely, though not entirely, sound. Yet the difference between them is a major difference.

This contradiction may be resolved by the simple assumption that North America lay at the pole during the Wisconsin period. By this assumption, Europe would have been a long way south of the Hudson Bay region. As I pointed out earlier, the thinner European ice, and the fact that it did not reach so far south as ice did in America, can be accounted for in this way. The more rapid retreat of the European glacier is entirely understandable on the assumption that it occupied a lower latitude. The simultaneous phases of retreat and advance, then, and the faster general retreat of the European glacier are both understandable.

This problem of the relationship of the American and European glaciations raises another question. Why was it that, with a pole in Hudson Bay or western Quebec, Great Britain and Scandinavia were glaciated at all, since Scandi-

navia at least must then have lain somewhat farther from the pole than it does now? Furthermore, why did Alaska then have many great mountain glaciers, but no continuous ice sheet? The latter problem is intensified by the consideration that the particular movement of the crust that we are supposing here, while it lowered the latitude of eastern North America a great deal, must have slightly raised the latitude of Alaska, especially that of northern Alaska. The reader can make these relationships clear to himself by referring now to a globe. We are assuming the displacement to have occurred along the 90th meridian.

The explanation of the glaciation of northwestern Europe is, I think, as follows. First, the heaviest glaciation of Europe is not contemporary with the Wisconsin ice sheet, but was the consequence of an earlier polar position, which will be discussed further on (Chapter IX). Secondly, the comparatively thin European ice sheet of Wisconsin time (which in Britain consisted really only of discontinuous mountain glaciers) was made possible by a very special combination of meteorological conditions. In North America a vast icecap extended eastward from its center near Hudson Bay. Much of the continental shelf in this whole area was then above sea level, as, indeed, it should have been to agree with our general theory, and this was covered by ice. Then the anticyclonic winds, blowing outward in all directions from the icecap, had only to cross the narrow North Atlantic, raising moisture from the sea and depositing it upon Scandinavia and Britain.

At first glance it might seem that a pole in Hudson Bay would have involved a heavy glaciation of Greenland, but there are reasons to suppose that it might, on the contrary, involve a deglaciation. Depending on the precise location of the pole, parts of Greenland would have lain farther from it than they do from the present North Pole. Of greater importance, however, is the fact that the Arctic Ocean would have been a temperate, and even, on the Siberian side, a warm temperate, sea. It would be likely, in these circumstances,

that a warm current like the Gulf Stream would have been flowing at that time out of the Arctic and down the coast of Greenland. Such a warm current might easily have deglaciated the island (or rather, islands). It might, however, have been deflected from Scandinavia and the British Isles by land masses in the North Atlantic, to be discussed later on.

If this deglaciation, indeed, reflects what really happened in Greenland, then there must have been a warm interval in Europe between the period of massive glaciation, to be discussed later, and the much less severe glaciation of late Wisconsin time. The present glaciation of Greenland would have been the consequence of the passage of the pole from the Hudson Bay region to its present location, with the refrigeration of the Arctic Ocean. The final warming of the climate both in Europe and in America would have been the consequence of the disappearance of the North American icecap, and of the pattern of anticyclonic winds which it had created.

So far as the glaciation of Alaska is concerned, again, the climate there was colder than it is now because of the vast refrigerating effect of the icecap that covered 4,000,000 square miles of the continent. Just as, at present, the Antarctic icecap makes the South Polar region colder than the Arctic (because it is a perfect reflector of the sun's radiant energy back into space), so then the great Wisconsin icecap meant that the prevailing temperatures at the center of the ice sheet (presumably the pole) were much lower than the temperatures prevailing now at the present North Pole, where no great icecap exists. But, although the intensely cold anticyclonic winds blowing off the Wisconsin icecap made Alaska colder than it is now, and thereby produced larger glaciers than exist at present, still these winds blew only over continuous land, and not over the sea, and so they could not pick up the moisture required to produce a continuous ice sheet. This explains why Alaska warmed up at the end of the North American ice age, even though it actually may have moved closer to the pole.

Another interesting argument is used by Antevs to buttress his position against radiocarbon dating. It is based on the evidence of crustal warping at the end of the ice age. Radiocarbon dating would, he says, require a fantastic rate of crustal warping, considered impossible by geophysicists. He says:

> My dating of the Cochrane stage at 11300–10150 B.P. is directly supported by the fact that long ages were required for the crustal rise which has occurred in the region since its release from the ice. At the south end of James Bay the rise of land relative to sea-level amounts to 600–700 feet. . . . The upward movement of some 650 feet equals the rise of the Scandinavian center of uplift during the last 8,200 years. Since the rates of modern uplifts are similar, or one meter a century in the Scandinavian center, and probably 70–80 centimeters (2.3–2.6 feet) a century at James Bay, the past rates may also have been similar. Since, furthermore, the uplifts in the two regions may have been essentially equal in general, the regression of the shore line in the James Bay region by some 650 vertical feet must have taken several thousand years, perhaps 8,000–10,000 years. . . . (108:520).

However, a displacement of the crust, with America moved farther than Europe, would solve this problem. Processes of adjustment of the crust would have a velocity proportional to the amount of the displacement of the particular area. By assumption, North America was displaced more than 2,000 miles to the south, but the southward displacement of the glaciated area in Europe amounted to only about 500 miles. The assumption of crust displacement offers the first possibility of reconciling the observed rates of crustal warping in America with geophysical principles.

5. *Changes in Sea Level at the End of the Ice Age*

There was a remarkable number of changes in the elevation of lands, and their interconnections, at the end of the ice age. The idea that they can all be explained either by a general rise of sea level due to the melting of ice or by the isostatic

rebound of the areas after the ice left is, however, fallacious. Let us consider, first, the question as to how far the melting of ice, raising the sea level, can solve the problem.

The new radioelement data from Antarctica, as we have seen, strongly suggest that the huge total quantity of ice supposed to have existed during the ice age is an illusion. It now appears that while the glaciers were at their maximum in North America a large part of Antarctica was ice-free. This is the only reasonable interpretation of the Antarctic data. It is therefore doubtful that the amount of ice then was very different in amount from that existing now. We have noted that for about 10,000 years the Wisconsin ice sheet was growing thinner, until its final disappearance. If this was the result of the southward movement of the icecap—if North America was then moving southward—Antarctica must, at the same time, have been moving into the Antarctic Circle. Therefore, as the ice sheet gradually thinned in North America, as it withdrew in Europe, the Antarctic icecap must have been in process of expansion. The water released by the melting in North America may have been mostly locked up again in the gathering Antarctic snows.

It follows from this that the process, during a period of perhaps 10,000 years, was simply one of transfer of ice masses from the Northern Hemisphere to Antarctica. It is difficult to say whether the tempos of melting in North America and of accumulation in Antarctica were always closely in line, or which may have been faster. No doubt alternations took place. In consequence, there may have been minor fluctuations of sea level, without a major universal rise.

Yet such a rise of the sea level in some parts of the world did take place. It has already been pointed out that such changes must accompany a displacement of the crust. We have merely to decide which method of accounting for the facts is most reasonable.

If the rise of the sea was due to melting ice, it should, admittedly, have been quantitatively proportional to the quantity of the ice that is assumed to have melted. It should, of

course, have been the same in all parts of the world (allowing some differences, perhaps, for vertical movements of the land locally). It should have been universal—that is, the evidences should be observable everywhere, on all the continents. There is, however, strong evidence in conflict with each of these propositions.

The maximum rise in sea level that can be ascribed to the melting of the Pleistocene ice sheets (assuming that the Antarctic icecap existed contemporaneously with them) is about 300 feet. This is a liberal estimate. Yet, in a recent study, Fisk and McFarlan show that the sea level during the Wisconsin glaciation (on American coasts) was 450 feet below the present level (153:294–96). Moreover, according to them, this is a minimum estimate, and the probabilities favor a greater lowering of the sea level in the late Pleistocene. Still more interesting, they give a chart showing that the lowest sea level was earlier than 28,000 years ago, or considerably before the maximum of the Wisconsin ice sheet. This date was established by radiocarbon (153:281). It can only mean that the low sea level must be attributed to a cause other than the withdrawal of water from the ocean to form that ice sheet.

Furthermore, Fisk and McFarlan show that the sea was rising 20,000 years ago, before the completion of the massive Tazewell Advance that carried the Wisconsin icecap to its maximum size (153:298). Surely, if the sea level were controlled by the glaciers, it should have been falling. Finally, Fisk and McFarlan show that the sea level had risen to within 100 feet of its present level by 10,000 years ago. Yet we know that by that time the Wisconsin glaciation was a mere shadow of its former self, while the Scandinavian had virtually ceased to exist. Is it likely that the remnants of these ice sheets could later have raised the ocean level 100 feet? The question is rendered even more doubtful by a news item that comes to me while I write these lines. It is a dispatch to the New York *Times* by John Hillaby, dated from Sheffield, England, September 2, 1956, giving an account of

the meeting of the British Association for the Advancement of Science. Hillaby describes a paper by Professor Harold Godwin of Cambridge University in which the professor gives the results of extensive research into the question of the date of the separation of England from the Continent. The date has been found to be 5,000 B.C., or 7,000 years ago. The report shows that the research work was very thorough. Now, obviously, by 7,000 years ago the Scandinavian icecap was long since gone, and the North American ice sheet was reduced to a few Canadian remnants. Yet only now did the North Sea bottom sink, and the English Channel become flooded by the sea. There is evidently something wrong here. There is a suggestion here that the floodings were produced by readjustments of the crust, and not by glacial melt water.

There is evidence that the sea rose (or the land subsided) farther on the western than on the eastern side of the Atlantic. This, of course, suggests that the development was not related to an increase of melt water. A good deal of this evidence was presented years ago by J. Howard Wilson, in his interesting *Glacial History of Nantucket and Cape Cod* (454). Wilson argued that eastern North America must have stood from 1,000 to 2,500 feet above its present level during the ice age. If we take the lesser estimate and compare it with the findings of Fisk and McFarlan (which they give as minima only) we can see that they are in pretty good agreement. Coleman was in agreement with Wilson, but based his opinion on the evidence of submarine canyons, which, as I have already mentioned, may have been created by fracturing of the crust rather than by subaerial erosion and subsequent subsidence. Wright and Shaler, however, presented evidence for a 2,000-foot higher elevation of Florida during the ice age (460), an elevation that would mean a very different distribution of land in the Caribbean during the period.

In two different ways this evidence agrees with our displacement theory. First, as the direction of the movement of the North Atlantic region would hypothetically have been equatorward, some subsidence of the ocean basin was log-

ically to be expected. Then, as the western side of the Atlantic was closer to the meridian of maximum displacement, it would have been displaced through more degrees of latitude, and in consequence there should have been greater subsidence on the American side of the ocean. Our theory implies that the Hudson Bay region was moved southward about 2,000 miles, while at the same time the southward movement of France amounted to no more than five hundred. The ratio of these distances is about four to one, and this is very close to the estimated subsidence on the western side of the Atlantic, of about 1,000 feet, as compared to that on the eastern side, of less than 300.

From the other side of the globe comes equally impressive evidence. Wallace argued for a subsidence of at least 600 feet of the coastlines of Southeast Asia and Indonesia at the end of the Pleistocene. These areas lie close to the same meridian of maximum displacement, the 90th meridian, which runs through Labrador, and accordingly they should have been displaced the same distance as eastern North America, and the resulting subsidence should have been of the same order. The Philippines are thought to have become separated from Asia only some 10,000 years ago; the separation of New Guinea from Australia and of Java from Sumatra may have been even more recent. Again, the subsidence may have considerably exceeded 600 feet, which Wallace gives as a minimum (444:24–25). Needless to say, a rise of the sea level of this extent cannot be explained as the result of melting of glaciers.

I have already mentioned the fact that some geophysicists seriously doubt that the rise of the land around the former glaciated tracts since the end of the ice age is due to isostatic rebound. It may be more correctly accounted for as a part of the aftermath of the last displacement of the crust. We have seen that polar areas are, according to the theory, areas recently moved poleward. Accordingly, they have undergone compression and uplift, the major part of the uplift being due to the lag in isostatic readjustment of the crust to the

variation of gravity with latitude (Chapter IV). An equatorward movement of such an area would cause extra subsidence —more than would occur with an area in isostatic equilibrium at the start of the movement. And subsequently, isostatic adjustment would re-elevate the area, but not to its original, excessive extent. This interpretation of the rebound of the glaciated tracts has the advantage that it can reconcile the facts there with the point of view expressed by Gilluly, and with the data from other parts of the world that so greatly puzzled Daly.

While there is no evidence that the sea level rose all over the world and to the same extent everywhere at the end of the ice age, there is a good deal of evidence that it has fallen somewhat since. One specialist in this field, Anderson, reported evidence of a fall of sea level amounting to between 100 and 140 feet, and extending over a vast area. He made a point of emphasizing that this could not be explained by the postglacial isostatic rebound of the formerly glaciated tracts of North America and Scandinavia. He is thoroughly puzzled by what seems to him an inexplicable fact:

> . . . what was the cause of a fall in sea-level at a time when it should have been rising owing to the melting of the ice? (4:493).

This fall of sea level is a matter of very great interest. I have already suggested that down to the disappearance of the glaciers in the Northern Hemisphere, the melt water from them may have pretty well balanced the growth of ice in Antarctica, so that there was no important change of sea level. With the disappearance of those northern ice sheets, however, the situation changed. There was now no longer a supply of melt water to balance the withdrawal of water to be locked up in the form of snow in Antarctica; consequently the sea level had to fall. Even the magnitude of the fall is in agreement, if we suppose that by about 10,000 years ago, when the northern icecaps dwindled away, the Antarctic icecap was half grown. For it is estimated that if the whole amount of ice now in Antarctica were suddenly melted, it

would suffice to raise the sea level between 200 and 300 feet. Half of it, therefore, would account for the amount of the fall in sea level noted by Anderson.

There is a widespread impression that the sea level is now rising all over the world, but this impression seems to be mistaken. It is natural, considering the widely publicized opinion that all present-day icecaps are in retreat, that people should rush to interpret a relative rise of the sea level at a few localities as indications of a general rise, caused by the assumed current melting of ice in both hemispheres. An examination of the data on which this claim is based shows, however, that the evidence is quite insufficient. I recently made an inquiry of the United States Coast and Geodetic Survey regarding this matter and received in reply a communication from Dr. H. E. Finnegan, Chief of the Division of Tides and Currents, in which he stated:

> . . . Long period tide records from control stations maintained by the Coast and Geodetic Survey show that there has been a relative rise of sea-level along each of the coasts of the United States. The rate of rise varies somewhat with the length of series and different regions. During the past 20 years the relative rise of sea-level along our East Coast has been at the rate of two hundredths of a foot per year. On our Pacific Coast the rate has been somewhat less.
>
> In Alaska, the tide records for Ketchikan show no definite change in sea-level. At certain places farther north, however, the records indicate a relative fall of sea-level. . . . (152).

This can, I think, be regarded as a summary of the facts presently known on this subject. It is plain that it does not add up to any universal rise of the sea level. Not only is no such rise indicated; exactly the opposite is implied by the facts. The facts show that different parts of the United States are subsiding at different rates, that Alaska is not subsiding at all, and that places farther north are actually rising. What reason is there to bring the sea into it? A "eustatic" change in sea level is not indicated by these facts, but differential movements of parts of the continent are. Moreover, the data come

from a very small part of the earth's surface. Equally careful measurements along all the coasts of all the continents would be necessary to establish the fact of a general rise in sea level. They could as easily establish that the sea level is falling.

Additional evidences of the fall of the sea level in post-glacial times are provided by Halle, for the Falkland Islands (196), by Pollock, for Hawaii (349a), and by Sayles, for Bermuda (366a). Umbgrove, basing his statement on quite other sources, concludes that "the sea-level has fallen over the whole world in comparatively recent times" (430:69).

A quite remarkable bit of evidence comes from Greenland. There a whale was recently discovered well preserved in the permafrost (the permanently frozen ground). It was dated by radiocarbon, and found to be 8,500 years old. It was found in beach deposits 43.6 feet above the present sea level. The highest beach in the area was 130 feet above the present sea level. It is hard to see how the elevation of this beach could be ascribed to isostatic rebound of the crust since the ice age, for there has been no lightening of the ice load on the crust in Greenland. How, then, is this frozen whale to be interpreted? I think we can accept it as fairly good evidence of a general fall of sea level resulting from the withdrawal of water from the oceans to feed the growing Antarctic icecap.

From the Philippines comes additional evidence that in those areas where the sea level rose at the end of the North American ice age, the rise was of a magnitude that cannot be explained on the theory of glacial melt water, but, on the contrary, requires the assumption that important changes took place in the crust itself. Warren D. Smith has written:

> It must be said that the geological history and structure of the Philippines, as studied in recent years by both Dr. Dickerson and myself, seem to indicate that the changes since the Pleistocene in the Philippines have been profound enough to have caused the disruption of land bridges and to have brought about the present isolation of its masses by flooding. . . . (399a:467).

We may note that Smith makes no reference to a rise of sea level because of the melting of glaciers. The subsidence

of the islands, and their separation from Asia, are attributed to deformation of the earth's crust itself. Moreover, it is unlikely that Smith had any conception of how recently these events occurred. He probably thought of the Pleistocene (and the ice age) as ending 20,000 or 30,000 years ago. Consequently, the structural changes in the crust that he discusses seem to have occurred at a rate which, like the unwarping of the crust in North America discussed by Antevs, is inconsistent with the speeds of geological change normally considered by geologists. There appears to be no rational explanation for such an acceleration of the tempo of geological change, except a displacement of the crust.

There is another important problem connected with the changes in sea level. It seems that many of them occurred in an abrupt fashion, so suddenly that the continuous cutting of the coastline by the sea was unable to keep up with the vertical movement of the land. Brooks refers to numerous strandlines at elevations of about 90, 126, and 180 feet above the present sea level, which may be traced over considerable areas (52:491). It seems reasonable that if the rise of the land in these localities (or the general fall of the sea) was gradual, the erosive action of the sea would have been able to keep extending the beach downward continuously. We would then have a continuous beach formation extending from 180 feet above the present sea level, down to the present sea margin. We have, on the contrary, a series of completely distinct elevated beaches. It would seem that the changes in elevation were comparatively rapid.

There is a possibility that this phenomenon is connected with the irregularities of the process of crust displacement referred to above. If the interstadials and the repeated readvances of the glaciers during the North American ice age resulted from the process I have described, the same process of storage and sudden release of stresses in the moving crust could easily account for abrupt changes in the elevation of sections of the crust. I am not suggesting that they occurred in periods of a few days or hours. The facts would be satisfied

by the assumption that they occurred in periods of the order of a few centuries. But it is clear that these beaches cannot be accounted for by a theory of postglacial upward adjustment, for there is no reason why this adjustment should have taken place in jumps. It would have been, by its nature, a gradual and even process.

6. *Darwin's Rising Beachline in South America*

A singularly impressive piece of evidence for a recent displacement of the crust may be found in the journal of Charles Darwin. Sir Archibald Geikie summarized Darwin's findings thus:

> On the west coast of South America, lines of raised terraces containing recent shells have been traced by Darwin as proofs of a great upheaval of that part of the globe in modern geological time. The terraces are not quite horizontal but rise to the south. On the frontier of Bolivia they occur from 60 to 80 feet above the existing sea-level, but nearer the higher mass of the Chilean Andes they are found at one thousand, and near Valparaiso at 1300 feet. That some of these ancient sea margins belong to the human period was shown by Mr. Darwin's discovery of shells with bones of birds, ears of maize, plaited reeds and cotton thread, in some of the terraces opposite Callao at a height of 85 feet. Raised beaches occur in New Zealand and indicate a greater change of level in the southern than in the northern end of the country. . . . (170:288).

If we attempt, by analyzing this evidence in accordance with the assumptions of the displacement theory, to reconstruct the course of events, we reach the following conclusions: Since the evidence of human occupation is found at an elevation of 85 feet, it seems reasonable to suppose that a fall of the sea level of that extent may have occurred within historical times. On the other hand, the continuously rising strandline down the coast to Valparaiso, continued in New Zealand, indicates a tilting of the earth's crust, involving South America and New Zealand, but not involving a general change in the sea level. The magnitude of the upheaval sug-

gests that it may have occurred earlier than the 85-foot general fall in sea level, and may have required much more time. The 85-foot fall in the general sea level we may explain as the result of the withdrawal of water to Antarctica. The uptilting of the continent may be seen as the result of its poleward displacement.

The effect postulated by Gutenberg, to account for uplift of areas displaced poleward, cannot account for the tilting, but another effect may. This is the increasing compression of the poleward-moving sector as the result of the progressive shortening of the radius and circumference of the earth in the higher latitudes. The compressions resulting from this have been discussed. They result inevitably from the increasing arc of the surface and the increasing convergence of the meridians.

VIII : THE GREAT EXTINCTIONS

When this theory was first presented to a group of scientists at the American Museum of Natural History, on January 27, 1955, Professor Walter H. Bucher, former President of the Geological Society of America, made an interesting observation. I had presented evidence to support the contention that North America had been displaced southward and Antarctica had been moved farther into the Antarctic Circle by the movement of the crust at the end of the ice age. Professor Bucher pointed out that, if this were so, there must have been an equal movement of the crust northward on the opposite side of the earth. He asked me whether there was evidence of this. I said I thought there was. I am presenting the evidence here.

1. The Extinction of the Mammoths

The closing millennia of the ice age saw an enormous mortality of animals in many parts of the world. Hibben estimated that as many as 40,000,000 animals died in North America alone (212:168). Many species of animals became extinct, including mammoths, mastodons, giant beaver, sabertooth cats, giant sloths, woolly rhinoceroses. Camels and horses apparently became extinct in North America then or shortly afterwards, although one authority believes a variety of Pleistocene horse has survived in Haiti (365). The paleontologist Scott is enormously puzzled both by the great climatic revolution and by its effects:

> The extraordinary and inexplicable climatic revolutions had a profound effect upon animal life, and occasioned or at least accompanied, the great extinctions, which, at the end of the Pleistocene,

decimated the mammals over three-fifths of the earth's land surface (372:75).

No one has been able to explain these widespread extinctions. I shall attempt to explain them as consequences of the last displacement of the crust, but, since the extinctions took place both in North America and in Asia—that is, both in the area presumably moved southward and in the area presumably moved northward, I shall concentrate first on Asia. There we shall find no difficulty in producing evidence to show that the climate of eastern Siberia grew colder as North America grew warmer, just as the theory requires.

Among all the animals that became extinct in Asia, the mammoth has been the most studied. This is because of its size; because of the great range of its distribution, all the way from the New Siberian Islands in the Arctic Ocean, across Siberia and Europe, to North America; because pictures of it drawn by primitive man have been found in the caves of southern France and Spain; but most of all, perhaps, because well-preserved bodies of mammoths have been found frozen in the mud of Siberia and Alaska. Ivory from these frozen remains has provided a supply for the ivory trade of China and Central Europe since ancient times.

A study of the reports on the frozen mammoths reveals some very remarkable facts. In the first place, they increase in numbers the farther north one goes, and are most numerous in the New Siberian Islands, which lie between the Arctic coast of Siberia and the pole. Secondly, they are accompanied by many other kinds of animals. Thirdly, although ivory is easily ruined by exposure to the weather, uncounted thousands of pairs of tusks have been preserved in perfect condition for the ivory trade. A fourth point is that the bodies of many mammoths and a few other animals have been preserved so perfectly (in the frozen ground) as to be edible today. Finally, astonishing as it may seem, it is not true that the mammoth was adapted to a very cold climate. I shall first take up this question of the mammoth's alleged adaptation to cold.

2. *The Mammoth's Adaptation to Cold*

It has long been taken for granted, without really careful consideration, that the mammoth was an Arctic animal. The opinion has been based on the mammoth's thick skin, on its hairy coat, and on the deposit of fat usually found under the skin. Yet it can be shown that none of these features mean any special adaptation to cold.

To begin with the skin and the hair, we have a clear presentation of the facts by the French zoologist and dermatologist H. Neuville. His report was published as long ago as 1919 (325). He performed a comparative microscopic study of sections of the skin of a mammoth and that of an Indian elephant, and showed that they were identical in thickness and in structure. They were not merely similar: they were exactly the same. Then, he showed that the lack of oil glands in the skin of the mammoth made the hair less resistant to cold and damp than the hair of the average mammal. In other words, the hair and fur showed a *negative* adaptation to cold. It turns out that the common, ordinary sheep is better adapted to Arctic conditions:

> We have . . . two animals very nearly related zoologically, the mammoth and the elephant, one of which lived in severe climates while the other is now confined to certain parts of the torrid zone. The mammoth, it is said, was protected from the cold by its fur and by the thickness of its dermis. But the dermis, as I have said, and as the illustrations prove, is identical in the two instances; it would therefore be hard to attribute a specially adaptive function to the skin of the mammoth. The fur, much more dense, it is true, on the mammoths than on any of the living elephants, nevertheless is present only in a very special condition which is fundamentally identical in all of these animals. Let us examine the consequences of this special condition, consisting, I may repeat, in the absence of cutaneous glands. The physiological function of these glands is very important. [Neuville's footnote here: It is merely necessary to mention that according to the opinion now accepted, that of Unna, the effect of the sebum is to lubricate the fur, thus protecting it against disintegration, and that of the sweat is to soak the epidermis

with an oily liquid, protecting it also against desiccation and disintegration . . . the absence of the glandular secretions puts the skin in a condition of less resistance well known in dermatology. It is superfluous to recall that the sebaceous impregnation gives the fur in general its isolating properties and imparts to each of its elements, the hairs, its impermeability, thanks to which they resist with a well-known strength all disintegrating agents, and notably those which are atmospheric. Everyone knows to what degree the presence of grease produced by the sebaceous glands renders wool resistant and isolating, and to what degree the total lack of this fatty matter lessens the value of woolen goods. . . .] (325:331–33).

Neuville, then, points out in the foregoing passage both that the mammoth lacks sebaceous glands and that the oil from these glands is an important factor in the protection of an animal against cold. It is probable, also, that protection from damp is more important than protection from low temperature. Oil in the hair must certainly impede the penetration of damp. The hair of the mammoth, deprived of oil, would seem to offer poor protection against the dampness of an Arctic blizzard. Sanderson has pointed out that thick fur by itself means nothing: a lot of animals of the equatorial jungles, such as tigers, have a thick fur (365). Fur by itself is not a feature of adaptation to cold, and fur without oil, as Neuville points out so lucidly, is, if anything, a feature of adaptation to warmth, not cold.

The question of the importance of oily secretions from the skin for the effectiveness of resistance of fur or hair to cold and damp is, however, highly involved. Very many inquiries directed to specialists in universities, medical schools, and research institutes over a period of more than five years failed to elicit sufficiently clear and definite answers until, finally, Dr. Thomas S. Argyris, Professor of Zoology at Brown University, referred me to the Headquarters Research and Development Command of the United States Army. This agency, in turn, very kindly referred me to the British Wool Industries Research Association. I addressed an inquiry to them, regarding the effects of natural oil secre-

tions from the skin on the preservation of wool. They replied in general confirmation of Neuville:

> . . . Those interested in wool assume that the function of the wool wax is to protect the wool fibres from the weather and to maintain the animal in a dry and warm condition. Arguments in this direction are of course mainly speculative. We do know, however, that shorn wool in its natural state can be stored and transported without entanglement (or felting) of the fibres, while scoured wool becomes entangled so that, during subsequent processing, fibre breakage at the card is significantly increased. It seems reasonable, therefore, to assume that the wool wax is responsible not only for conferring protection against the weather but also for the maintenance of the fleece in an orderly and hence more efficacious state (458).

It appears that there has been no scientific study of the precise points at issue here; no one has measured in any scientific way the quantitative effect of oily secretions in keeping heat in or moisture out. Despite this fact, however, we are at least justified, on the basis of the facts cited above, in rejecting the claims advanced for the hair of the mammoth as an adaptive feature to a very cold climate.

Neuville goes on to destroy one or two other arguments in favor of the mammoth's adaptation to cold:

> . . . It has been thought that the reduction of the ears, thick and very small relatively to those of the existing elephants, might be so understood in this sense; such large and thin ears as those of the elephants would probably be very sensitive to the action of cold. But it has also been suggested that the fattiness and peculiar form of the tail of the mammoth was an adaptive character of the same kind; however, it is to the fat rumped sheep, animals of the hot regions, whose range extends to the center of Africa, that we must go for an analogue to the last character.
>
> It is therefore, only thanks to entirely superficial comparisons which do not stand a somewhat detailed analysis, that it has been possible to regard the mammoth as adapted to the cold. On account of the peculiar character of the pelage the animal was, on the contrary, at a disadvantage in this respect (325:331–33).

There remains the question of the layer of fat, about three inches thick, which is found under the skin of the

mammoth. This fat is thought to have provided insulation against the bitter cold of the Siberian winter.

The best opinion of physiologists is opposed to the view that the storage of fat by animals is a measure of self-protection against cold. The consensus is, on the contrary, that large fat accumulation testifies chiefly to ample food supply, obtainable without much effort, as, indeed, is the case with human beings. Physiologists agree that resistance to cold is mainly a question of the metabolic rate, rather than of insulation by fat. Since the length of capillaries in a cubic inch of fat is less than the length of capillaries in a cubic inch of muscle, blood circulation would be better in a thin animal. We might ask the question, Which would be more likely to survive through a Siberian winter, a man burdened with fifty or a hundred pounds of surplus fat or a man of normal build who was all solid muscle, assuming that winter conditions would mean a hard struggle to obtain food? Dr. Charles P. Lyman, Professor of Zoology at Harvard, remarked, regarding this question of fat:

> It is true that many animals become obese before the winter sets in, but for the most part it seems likely that they become obese because they have an ample food supply in the fall, rather than that they are stimulated by cold to lay down a supply of fat. Cold will ordinarily increase the metabolic rate of any animal which means that it burns up more fuel in order to maintain its ordinary weight, to say nothing of adding weight in the form of fat. The amount of muscular activity in the daily life of either type of elephant is certainly just as important as the stimulus of cold as far as laying down a supply of fat is concerned (284).

This statement suggests that there is no basis for the assumption that the fat of the mammoths adapted them to an Arctic climate. On the other hand, it is quite true that the storage of fat in the fall may help animals to get through the winter when food is scarce. The winter does not, however, have to be an Arctic winter. A winter such as we have in temperate climates is quite cold enough to cut the available food supply for herbivorous animals. It seems that under

favorable circumstances even the African and Indian elephants accumulate quite a lot of fat. F. G. Benedict, in his comprehensive work on the physiology of the elephant, considers it a fatty animal (27).

The resemblances between the mammoth and the Indian elephant extend further than the identity of their skins in thickness and structure, and the fact that they were both fatty animals. Bell suggests that they were only two varieties of the same species:

> Falconer insists on the importance of the fact that throughout the whole geological history of each species of elephant there is a great persistence in the structure and mode of growth of each of the teeth, and that this is the best single character by which to distinguish the species from one another. He finds, after a critical examination of a great number of specimens, that in the mammoth each of the molars is subject to the same history and same variation as the corresponding molar in the living Indian elephant (25).

It is clear that the similarities in the life histories of each of the teeth of these two animals are more important than the differences in the shapes of the teeth, which were such as might easily occur in two varieties of the same species. It cannot be denied that two varieties of the same species may be adapted to different climates, but it must be conceded that the adaptation of two varieties of the same species, one to tropical jungles and the other to Arctic conditions, is against the probabilities.

3. *The Present Climate of Siberia*

The people who lay the greatest st[illegible], are a[illegible] a rudde[illegible] the adaptation of the mammoth to cold ignore the other animals that lived with the mammoths. Yet we know that along with the millions of mammoths, the northern Siberian plains also supported vast numbers of rhinoceroses, antelope, horses, bison, and other herbivorous creatures, while a variety of carnivores, including the sabertooth cat, preyed upon them. What good

does it do to argue that the mammoth was adapted to cold when it is impossible to use the argument in the case of several of the other animals?

Like the mammoths, these other animals ranged to the far north, to the extreme north of Siberia, to the shores of the Arctic Ocean, and yet farther north to the Lyakhov and New Siberian Islands, only a very short distance from the pole. It has been claimed that all the remains on the islands may have been washed there from the mouths of the Siberian rivers by spring floods; I shall consider this suggestion a little later.

So far as the present climate of Siberia itself is concerned, Nordenskjöld made the following observations of monthly averages of daily Centigrade temperatures during the year along the Lena River (334):

January	—48.9	July	15.4
February	—47.2	August	11.9
March	—33.9	September	2.3
April	—14	October	—13.9
May	— 0.14	November	—39.1
June	13.4	December	—45.1

The average for the whole year was —16.7. Since zero in the Centigrade scale is the freezing point of water, it will be seen that only one or two months in the year are reasonably free from frost. Even so, there must be frequent frosts in [illegible] notwithstanding occasional high midday temperatures. No doubt it was knowledge of these conditions that caused the great [illegible] of modern geology, Sir Charles Lyell, to remark that it [illegible] be impossible for herds of mammoths and [illegible] to subsist, throughout the year, even in the southern part of Siberia. . . .

If this is the case with Siberia, what are we to think when we contemplate the New Siberian Islands? There the remains of mammoths and other animals are most numerous of all. There Baron Toll found remains of a sabertooth cat, and a fruit tree that had been ninety feet high when it was

standing. The tree was very perfectly preserved in the permafrost, with its roots and seeds (113:151). Toll claimed that green leaves and ripe fruit still clung to its branches. Yet, at the present time, the only representative of tree vegetation on the islands is a willow that grows one inch high.

Now let us return to the question of whether all these remains were floated out to the islands on spring floods. Let us begin with a backward view at the history of these islands. Saks, Belov, and Lapina point to evidence that there were luxuriant forests growing on the New Siberian Islands in Miocene and perhaps Pliocene times (364). At the beginning of the Pleistocene the islands were connected with the mainland, and the mammoths ranged over them. In the opinion of these writers the vast numbers of mammoth remains on Great Lyakhov Island indicate that they took refuge on the island when the land was sinking (364:4, note). There is no evidence that they were washed across the intervening sea.

The improbabilities in this suggestion of transportation of these hundreds of thousands of animal bodies across the entire width of the Nordenskjöld Sea, for a distance of more than 200 miles from the mouth of the Lena River, are simply out of all reason. Let us see exactly what is involved.

First, we should have to explain why the hundreds of thousands of animals fell into the river. To be sure, they did not fall in all at once; nevertheless, they must have had the habit of falling into the river in very large numbers, because only one body in a very great many could possibly float across 200 miles of ocean. Of those that floated at all only a few would be likely to float in precisely the correct direction to reach the islands. Islands, even large ones, are amazingly easy to miss even in a boat equipped with a rudder and charts. The Lena River has three mouths, one of which points in a direction away from the islands. The two other mouths face the islands across these 200 miles of ocean. Occasionally, a piece of driftwood might float across the intervening sea. Occasionally, perhaps, an animal—if for some reason it did not happen to sink, if it were not eaten by fishes—might be

washed up on the shore of one of the islands. It seems probable that only an incredibly powerful current could transport the body of a mammoth across 200 miles of ocean.

But let us suppose that somehow the animals are transported across the ocean. What then? The greatest of the New Siberian Islands is about 150 miles long and about half as wide. Not one single account of the explorations on these islands has mentioned that the animal remains are found only along the beaches. They are obviously found also in the interior. Are we to suppose that the floods of the Lena River were so immense that they could inundate the New Siberian Islands, 200 miles at sea? It is safe to say that all the rivers of Europe and Asia put together, at full flood, would fail to raise the ocean level 200 miles off the coast by more than a few inches at most.

But, again, let us suppose that the remains were merely washed to the present coasts, and not into the interior. How then were they preserved? How were hundreds of thousands of mammoths placed above high-water mark? Storms, no doubt, but whatever storms can wash up, other storms can wash away. No accumulation of anything occurs along the coasts because of storms. All that storms can do is to destroy; they can grind up and destroy anything. And they would have ground up and destroyed all the bodies, including, of course, the 90-foot fruit tree with its branches, roots, seeds, green leaves, and ripe fruit.

I think it is plain that the only reason suggestions of this kind are advanced is that there is need to support some theory that has been developed to explain some other part of the evidence, some local problem. Moreover, there is need, always need, to discredit the evidence that argues for drastic climatic changes.

Naturally, the knowledge that the Arctic islands, though they are now in polar darkness much of the year, were in very recent geologic times able to grow the flourishing forests of a temperate climate, eliminates any need to insist that they were always as cold as they are today. Thus,

it is not a question at all of whether the climate grew colder, but merely a question of when the change occurred. I have already discussed the evidence showing that it occurred (for the last time, anyway) when North America moved southward from the pole.

Campbell has contributed a suggestion with regard to the alleged floating of hundreds of thousands of bodies across the Nordenskjöld Sea. He notes that bodies ordinarily float because of gas produced by decomposition. Decomposition is at a minimum in very cold water, and therefore bodies ordinarily do not float in very cold water. As an example of this he points to a peculiarity of Lake Superior. The waters of this lake are very cold. This may be because they are supplied, as some people think, by underground springs from the Rocky Mountains, far away. And there is an old saying in the lake region that "Lake Superior never gives up its dead." But the Arctic Ocean is as cold as the springs fed by the glaciers of the Rockies. The water of the Lena would not be warm even in midsummer, but during the spring floods—when the Lena would be swollen with the melt water of the winter snows—the water at such times would be frigid, and the bodies of animals drowned in it would not decompose, nor would they float. They would tend to sink, instead, into the nearest hole, and never come to the surface.

4. A Sudden Change of Climate?

We may reasonably conclude that the climate of Siberia changed at the end of the Pleistocene, and that it grew colder. Our problem is to discover what process of change was involved. On the one hand, our theory of displacement of the crust involves a considerable period of time, and a gradual movement; on the other hand, the discovery of complete bodies of mammoths and other animals in Siberia, so well preserved in the frozen ground as to be in some cases still edible, seems to argue a cataclysmic change.

To those who, in the past, have argued for a very sudden catastrophe, the specialists in the field have offered opposing theories to explain the preservation of the bodies. One of these was that as the mammoths walked over the frozen ground, over the snow fields, they may have fallen into pits or crevasses and been swallowed up and permanently frozen. Or, again, they might either have broken through river ice and been drowned, or they might have got bogged while feeding along the banks.

There is no doubt that a certain number of animals could have been put into the frozen ground in just the manner suggested above. That this is the explanation for the preservation of the mammoths' bodies generally, however, is unlikely for a number of reasons.

It is not generally realized, in the first place, that it is not merely a matter of the accidental preservation of eighty-odd mammoths and half a dozen rhinoceroses that have been found in the permafrost. These few could perhaps be accounted for by individual accidents, provided, of course, that we agreed that the animals concerned were Arctic animals. The sudden freezing and consequent preservation of the flesh of these animals might be thus explained. But there is another factor of great importance, which has been consistently neglected. It has been overlooked that meat is not the only thing that has to be frozen quickly in order to be preserved. The same is true of ivory. Ivory, it appears, spoils very quickly when it dries out.

Tens of thousands of skeletons and individual bones of many kinds of animals have been discovered in the permafrost. Among them have been found the enormous numbers of mammoths' tusks already mentioned. To be of any use for carving, tusks must come either from freshly killed animals or have been frozen very quickly after the deaths of the animals, and kept frozen. Ivory experts testify that if tusks are exposed to the weather they dry out, lose their animal matter, and become useless for carving (280:361–66).

According to Lydekker, about 20,000 pairs of tusks, in

perfect condition, were exported for the ivory trade in the few decades preceding 1899, yet even now there is no end in sight. According to Digby, about a quarter of all the mammoth tusks found in Siberia are in good enough condition for ivory turning (113:177). This means that hundreds of thousands of individuals, not merely eighty or so, must have been frozen immediately after death, and remained frozen. Obviously, it is unreasonable to attempt to account for these hundreds of thousands of individuals by the assumption of such rare individual accidents as have been suggested above. Some powerful general force was certainly at work. Lydekker gives many hints of the nature of this force in the following passage:

> . . . In many instances, as is well known, entire carcasses of the mammoth have been found thus buried, with the hair, skin and flesh as fresh as in frozen New Zealand sheep in the hold of a steamer. And sleigh dogs, as well as Yakuts themselves, have often made a hearty meal on mammoth flesh thousands of years old. In instances like these it is evident that mammoths must have been buried and frozen almost immediately after death; but as the majority of the tusks appear to be met with in an isolated condition, often heaped one atop another, it would seem that the carcasses were often broken up by being carried down the rivers before their final entombment. Even then, however, the burial, or at least the freezing, must have taken place comparatively quickly as exposure in their ordinary condition would speedily deteriorate the quality of the ivory (280:363).

He continues:

> How the mammoths were enabled to exist in a region where their remains became so speedily frozen, and how such vast quantities of them became accumulated at certain spots, are questions that do not at present seem capable of being satisfactorily answered; and their discussion would accordingly be useless. . . . (280:363).

Lydekker was not alone in feeling the futility of considering these mysterious facts. For many years, in this field as in others, there has been a tendency to put away questions that could not be answered. However, we shall return to his statement. I shall try to show later on how all the details of the phenomena he describes can be made understandable. For

the moment, I would like to point out simply that some sort of abrupt climatic change is required. This conclusion is reinforced by the results of recent research in the frozen foods industry. This has produced evidence that throws additional doubt on the theory of the preservation of the bodies of mammoths by individual accidents. It seems that the preservation of meat by freezing requires some rather special conditions. Herbert Harris, in an article on Birdseye in *Science Digest,* writes:

> What Birdseye had proved was that the faster a food can be frozen at "deep" temperatures of around minus 40 degrees Fahrenheit, the less chance there is of forming the large ice crystals that tear down cellular walls and tissues leaving gaps through which escape the natural juices, nutriment and flavor (202:3).

Harris quotes one of Birdseye's engineers as saying:

> . . . take poultry giblets; they can last eight months at 10 below zero, but "turn" in four weeks above it. Or lobster. It lasts 24 months at 10 below but less than twenty days at anything above. . . . (202:5).

In the light of these statements the description of the frozen mammoth flesh given by F. F. Herz is very illuminating. Quoted by Bassett Digby in his book on the mammoth, Herz said that "the flesh is fibrous and marbled with fat." It "looks as fresh as well frozen beef." And this remark is made of flesh known to have been frozen for thousands of years! Some people have reported that they have been made ill by eating this preserved meat, but occasionally, at least, it is really perfectly edible. Thus Mr. Joseph Barnes, former correspondent of the New York *Herald Tribune,* remarked on the delicious flavor of some mammoth meat served to him at a dinner at the Academy of Sciences in Moscow in the 1930's (24).

What Birdseye proved was that meat to remain in edible condition must be kept very cold—not merely frozen, but at a temperature far below the freezing point. What the edible mammoth steaks proved was that meat had been so kept in at least a few cases for perhaps 10,000 to 15,000 years in the

Siberian tundra. It is reasonable to suppose that the same cause that was responsible for the preservation of the meat also preserved the ivory; and therefore that tens or hundreds of thousands of animals were killed in the same way.

How can such low temperatures for the original freeze be reconciled with the idea of individual accidents unless at least the animals died in the middle of the winter? It is quite certain that such temperatures could never have prevailed at the surface or in mudholes during "spring freshets." Ripe seeds and buttercups, found in the stomach of one of the mammoths, to be discussed later, showed that his death took place in the middle of the summer. It is obvious that during the summer the temperature at the top of the permafrost zone was and is 32° F. or 0° Centigrade, neither more nor less, since by definition that is where melting begins. And from that point down there would be only a relatively gradual fall in the prevailing temperature of the permafrost.

Even if mammoths died in the winter, it is difficult to see how very many of them could have become well enough buried to have escaped the warming effects of the thaws of thousands of springs and summers, which would have rotted both the meat and the ivory, unless there was a change of climate.

The theory that mammoths may have been preserved by falls into pits or into rivers encounters further objections. Tolmachev, the Russian authority, pointed out that the remains are often found at high points—on the highest points of the tundra (422:51). He notes that the bodies are found in frozen ground, and not in ice, and that they must have been buried in mud before freezing. This presents a serious problem because, as he says,

> . . . As a matter of fact, the swamps and bogs of a moderate climate with their treacherous pits, in northern Siberia, owing to the permanently frozen ground, could exist only in quite exceptional conditions (422:57).

Howorth remarked on this same problem:

> While it is on the one hand clear that the ground in which the bodies are found has been hard frozen since the carcasses were entombed, it is no less inevitable that when these same carcasses were originally entombed, the ground must have been soft and unfrozen. You cannot thrust flesh into hard frozen earth without destroying it (225a:313).

Since Tolmachev can think of no other solution to this problem, he finds himself forced to conclude that the mammoths got trapped in mud when feeding on river terraces. We have seen that this conflicts seriously with the conditions of temperature required for the preservation of the meat, whether they were feeding on the terraces during the summer, when, presumably, the fresh grass supply would be available there, or whether they were shoving aside the heavy snowdrifts during the winter to attempt to get at the dead grass below. For in either case they would fall into unfrozen water, the temperature of which could not be lower than 32° Fahrenheit. Furthermore, if this is the way it happened, why are the animals often found on the highest point of the tundra?

Thus we see that the further we get into this question the thornier it becomes. We shall have, for one thing, to face the problem of the apparently sudden original freeze. How sudden, indeed, must it have been? How can we account for it on the assumption of a comparatively slow displacement of the earth's crust? So far as the first question is concerned, recent research has contributed interesting new data.

Research on the mechanics of the freezing process and its effects on animal tissues has been carried forward considerably since the experiments conducted by Birdseye's engineers. In a recent article in *Science,* Meryman summarizes the recent findings. These are based on extremely thorough laboratory research, and they modify, to some extent, the Birdseye findings.

Meryman shows that initial freezing at deep temperatures is not required for the preservation of meat. On the contrary, such sudden deep freeze may destroy the cells. He remarks,

"Lovelock considers —5° C. as the lowest temperature to which mammalian cells may be slowly frozen and still survive." Furthermore, the tissues survive gradual freezing very well:

> In most, if not all, soft tissue cells there is no gross membrane rupture by slow freezing. Even though it is frozen for long periods of time, upon thawing the water is reimbibed by the cells, and their immediate histological appearance is often indistinguishable from the normal.

It appears that what damages the cells is dehydration, caused by the withdrawal of water from them to be incorporated in the ice. This process goes on after the initial freezing:

> . . . The principal cause of injury from slow freezing is not the physical presence of extracellular ice crystals, but the denaturation incurred by the dehydration resulting from the incorporation of all free water into ice (304:518–19).

There are only two known ways, according to Meryman, to prevent this damage. First, ". . . the temperature may be reduced immediately after freezing to very low, stabilizing temperatures." The other way is artificial; it consists of using glycerine to bind water in the liquid state, preventing freezing.

Meryman shows that once the temperature has fallen to a very low point, it must remain at that point if the frozen product is to escape serious damage. The reason for this is that except at these low temperatures, a recrystallization process may take place in ice, in which numerous small crystals are combined into large ones. The growth of the large crystals may disrupt cells and membranes. He remarks:

> At very low temperatures, recrystallization is relatively slow, and equilibrium is approached while the crystals are quite small. At temperatures near the melting point, recrystallization is rapid, and the crystals may grow to nearly visible size in less than an hour (304: 518).

I am reminded, in writing these lines, of my experience in truck gardening. In trying to reduce damage from frost, I

often resorted to a method that was effective but mysterious, for I could not understand why it worked. I learned that if the vegetables got frosted—even heavily frosted—they would not be seriously damaged if I could manage to get out before sunrise and thoroughly hose them off, washing away the frost. If, however, the sun should rise before I was finished, the unwashed vegetables would be damaged. It would seem, according to the explanation given by Meryman, that the frost damage was the result of recrystallization of the ice that had formed within the vegetable fibers. Small crystals, growing into large ones in the hour or so before the sun was up far enough to melt them, evidently caused the damage.

It follows, from this analysis of the mechanics of freezing, that the preservation of mammoth meat for thousands of years may be accounted for by normal initial freezing, followed by a sharp fall in temperature. Whenever the meat was preserved in an edible condition the deep freeze must have been uninterrupted; there must have been no thaws sufficient to bring the temperature near the freezing point.

Let us now take a closer look at one of these preserved mammoths, and see what it may have to tell us.

5. *The Beresovka Mammoth*

Perhaps the most famous individual mammoth found preserved in the permafrost was the so-called Beresovka mammoth. This mammoth was discovered sticking out of the ground not far from the bank of the Beresovka River in Siberia about 1901. Word of it reached the capital, St. Petersburg. It so happened that, a long time before, word of another mammoth had come to the ears of Tsar Peter the Great. With his strong interest in natural science, the Tsar had issued a ukase ordering that whenever thereafter another mammoth was discovered, an expedition should be sent out by his Imperial Academy of Sciences to study it.

In accordance with this standing order, a group of dis-

tinguished academicians entrained at St. Petersburg and proceeded to the remote district of Siberia where the creature had been reported. When they arrived they found that the wolves had chewed off such parts of the mammoth as projected aboveground, but most of the carcass was still intact. They erected a structure over the body, and built fires so as to thaw the ground and permit the removal of the remains. This process was hardly agreeable, since, the moment the meat began to thaw, the stench became terrific. However, several academicians remarked that after a little exposure to the stench, they became used to it. They ended by hardly noticing it.

Eventually the body of the entire mammoth was removed from the ground. The academicians, meantime, made careful observations of its original position. They saw evidence that, in their opinion, the mammoth had been mired in the mud. It looked as if its last struggles had been to get out of the mud, and as if it had frozen to death in a half-standing position. Strangely enough, the animal's penis was fully erect. Two major bones, a leg bone and the pelvic bone, had been broken as if by a fall. There was still some food on the animal's tongue, and between his teeth, indicating an abrupt interruption of his last meal. The preliminary conclusion suggested by these facts was that the animal met his death by falling into the river. A little later on we shall re-examine this conclusion.

Very special interest attached to the analysis of the contents of this animal's stomach. These consisted of about fifty pounds of material, largely undigested and remarkably well preserved. While the foregoing data were obtained from a translation of parts of the report of the academicians, published by the Smithsonian Institution, the section dealing with the stomach contents was specially translated for this work by my aunt, Mrs. Norman Hapgood. Since there are many interesting points essential to an understanding of the question, which can be noted only by a reading of the report itself, and which do not figure in the published accounts, I

reproduce the stomach analysis by V. N. Sukachev, with omission of technical botanical terms where possible, and with omission of bibliographical references to Russian, German, and Latin sources, and some shortening of the comment (410).

We can definitely establish the following types of plants in the food in the stomach and among the teeth of the Beresovka mammoth [Latin names are those of the Russian text]:

a. *Alopecurus alpinus sin.* The remains of this grass are numerous in the contents of the stomach. A significant portion of it consists of stems, with occasional remnants of leaves, usually mixed in with other vegetable remains. . . . All these remains are so little destroyed that one is able to establish with exactitude to what species they belong. . . .

Measurements of the individual parts of these plants, when compared with the varieties of the existing species, showed that the variety contained in the food was more closely related to that now found in the forest regions to the south of the tundra than to the varieties now found in the tundra. Nevertheless, this is an Arctic variety and is widely spread over the Arctic regions, in North America and Eurasia. However, in the forested regions it runs far to the south.

b. *Beckmannia eruciformis (L.) Host.* The florets of this plant are numerous in the contents of the stomach and usually are excellently preserved. [The detailed description of the remains (with precise measurements in millimeters) shows the species to be the same as that of the present day, although a little smaller, which may be the result of compaction in the stomach. At the present time the species is widely prevalent in Siberia and in the Arctic generally. It grows in flooded meadows or marshes. It is also found in North America, the south of Europe, and a major part of European Russia (although it has not been reported from northern Russia), almost all of Siberia, Japan, North China, and Mongolia.]

c. *Agropyrum cristatum (L.) Bess.* Remains of this plant are very numerous in the contents of the stomach. [They are so well preserved that there is no doubt as to the exact species. The individual specimens are slightly smaller than those of the typical more southern variety growing today, but this could be the result of some reduction of size because of pressure in the stomach, which is noted in other cases.]

The finding of these plants is of very great interest. Not only are they scarcely known anywhere in the Arctic regions, they are even, so far as I have been able to discover, very rare also in the Yakutsk dis-

trict. . . . Generally speaking the *Agropyrum cristatum L. Bess* is a plant of the plains (steppes) and is widespread in the plains of Dauria. . . . The general range of this plant includes southern Europe (in European Russia it is adapted to the plains belt), southern Siberia, Turkestan, Djungaria, Tian-Shan, and Mongolia.

Nevertheless, the variety found in the stomach differs slightly from both the European and Oriental-Siberian varieties found today.

d. *Hordeum violaceum Boiss. et Huet.* [After a detailed anatomical description of the remains of this plant in the stomach contents, the writer continues.] Our specimens are in no particular different from the specimens of this species from the Yakutsk, Irkutsk, and Transbaikal districts. [The plant is, apparently, no longer found along the Lena River, except south of its junction with the Aldan River. It is found in dry, grassy areas. It is not found in the Arctic regions.] Its northernmost point is apparently Turochansk. . . . Generally speaking, in Siberia this plant is a meadow plant and is also found in moister places in the plains.

e. *Agrostis sp.* . . . it does not appear possible to identify the species positively. [Apparently, no plant precisely similar is known at the present day. Thus it may represent an extinct form.]

f. *Gramina gen. et sp.* A grass, but preservation is not good enough to allow any more precise identification.

g. *Carex lagopina Wahlenb.* The remains of this sedge are numerous in the contents of the stomach. [The specimens exactly resemble varieties growing today. The measurements show no reduction in size. Its range extends to the shores of the Arctic Ocean. It is found in mountainous regions, including the Carpathians, Alps, and Pyrenees. It is also found in the peat bogs of western Prussia, in Siberia as far south as Transbaikalia and Kamchatka, in eastern India, North America, and the southern island of New Zealand.]

h. [Omitted—apparently a numbering error in the text.]

i. *Ranunculus acris L.* [The specimens in the stomach did not permit identification of the precise variety of this buttercup, though pods equally large are occasionally found.] The general range of this plant is very great. It includes all Europe and Siberia, it stretches to the extreme north, spreads to China, Japan, Mongolia, and North America. However, over this area this species very much deteriorates into many varieties which are considered by some to be independent species. [This plant grows in rather dry places. It is not at present found growing together with the *Beckmannia Eruciformis,* although it is found with it in the stomach.]

j. *Oxytropis sordida (Willd) Trantv.* In the contents of the stomach were found several fragments of these beans. . . . In the frag-

ments taken from the teeth there were found eight whole bean pods in a very good state of preservation; they even in places retained five beans. . . . [The plant is now found in Arctic and sub-Arctic regions, but also in the northern forests. It grows in rather dry places.]

In addition to the nine species mentioned above, and described in the report, with numerous measurements, the author reports that two kinds of mosses were identified in the stomach contents by Professor Broterus, of Finland. There were five sprigs of *Hypnum fluitans (Dill.) L.* and one sprig of *Aulacomnium turgidum (Wahlenb.) Schwaegr.* The first is common in Siberia north of the 61st parallel of latitude and to the marshlands of northern Europe. Both of them "belong to species widely distributed over both the wooded and the tundra regions."

The report states, further, that another scientist, F. F. Herz, brought back several fragments of woody substances and bark from beneath the mammoth, and of the species of vegetation among which it was lying. Very surprisingly, these were found to differ in a marked degree from the contents of the stomach. A larch (*Larix sp.*) was finally identified, but the genus only, not the species.

Another tree identified in a general way was *Betula Alba L.s.I.,* but the exact species could not be determined. The same was true of a third tree, *Alnus sp.* "All three of these kinds grow at present in the Kolyma River basin, and along the Beresovka, as they are widespread in general from the northern limits of the wooded belt to the southern plains."

The general conclusions reached in the report are as follows.

a. The remains of plants in the mammoth's mouth, among its teeth, were the same as the stomach contents, and represented food the mammoth had not yet swallowed when it was killed.

b. The food consisted preponderantly of grasses and sedge. "No remains at all of conifers were found." Therefore, "one may conclude that the Beresovka mammoth did not, as was previously thought, feed mainly on coniferous vegetation but

mainly on meadow grasses." Evidently he wandered into low, moist places, and also into higher, drier places such as are now found in the same region.

c. "The finding of the wood remains under the mammoth, and even the cliff itself where the mammoth was lying, suggest that he was not feeding in the place where he died. The majority of the vegetation in his food did not grow along cliffs or in conjunction with species of trees."

d. The discovery of the ripe fruits of sedges, grasses, and other plants suggests that "the mammoth died during the second half of July or the beginning of August."

The report concludes that while the contents of the stomach do not prove that the climate was warmer in the days of the mammoth than it is today, neither do they exclude the possibility that it was warmer. However, the climate was, in any case, not much warmer. The evidence provides no clew to the cause of the extinction of the mammoths.

6. *The Interpretation of the Report*

On the assumption that we are dealing with a displacement of the earth's crust beginning about 18,000 years ago and ending about 8,000 years ago, possibly punctuated by pauses and renewals of movement, and by massive outbursts of volcanism that accounted for the repeated readvances of the ice in North America, and by warm phases between these readvances, when the temperatures may have been warmed by the increasing percentage of carbon dioxide in the air, we may attempt to reconstruct the progression of events in Siberia. This may furnish a basis for the interpretation of the report, and of the other facts about the mammoths cited above.

To begin with, if North America was moving gradually southward during this period (along the 90th meridian, as we assume), then East Asia was moving northward along the continuation of the same meridian, and it moved at the same

rate, to the same distance, with the same pauses, if any. The climate would be growing gradually colder, but with interruptions, for the colder and warmer phases caused in North America by the volcanic dust and carbon dioxide (produced by volcanic eruptions) would be universal; they would affect the whole earth's surface in the same direction at the same time. In Siberia, warm phases would check the deterioration of the climate temporarily, while in North America the cold phases would act to check its improvement. The total change of climate in Siberia during this whole period would be very great. Siberia would, at the beginning of the movement, have been enjoying a warm temperate climate, warmer than that of New York at the present time.

During the whole period, changes would gradually be taking place in the flora in eastern Siberia. Plants adapted to wide ranges of climate, capable of surviving in the increasing cold, would continue to grow in Siberia. It is interesting to note in the foregoing report of the contents of the mammoth's stomach that every single plant or tree associated with the time of the mammoth's death has a range extending considerably to the south of that latitude today. Plants unable to survive in the increasing cold would retreat toward the south, as two or three of the plants found in the stomach evidently did. Arctic species and varieties would tend to invade the region as the climate grew colder. The contents of the mammoth's stomach would simply represent the mixture of plants growing in Siberia during the particular part of the period in which he lived. We learn from Runcorn that Soviet scientists have dated a mammoth from the Taimir Peninsula, considerably to the westward in Siberia, by radiocarbon, and have found it to be about 12,300 years old (361). This means that mammoths survived until toward the end of the crust movement. The mammoth of the Beresovka may have lived as late as or later than this, when the climate of Siberia had deteriorated a great deal, though by no means to the present level. The possibility exists that if we could find a mammoth that had died during the earlier phase of the displacement

we would find a combination of plants in its stomach reflecting a much warmer climate, but, of course, it is unlikely that during that warmer period any mammoths would have been preserved.

Just as we assume that North America subsided a thousand feet or more relatively to sea level when the continent was being moved equatorward, this being followed by a later isostatic rebound of the crust, so we must logically assume a progressive uplift of Siberia during its poleward displacement, with subsidence since.

This uplift of Siberia, which I now suggest, is vitally important for the clarification of the facts relating to the mammoths. If we assume that, previous to the displacement, the elevation of the lands in eastern Siberia and the Arctic was about the same as now, we can see in this moderate uplift (which is a necessary corollary of crust displacement) sufficient added elevation to connect both the New Siberian Islands with the Asiatic mainland, and Asia with North America across the Behring Strait. Thus the migrations of the animals to the New Siberian Islands and between Alaska and Siberia are explained, without having to have recourse to the theory of a sea level controlled by glacial melt water. Then, the subsidence of the area by isostatic adjustment after the end of the displacement (let us say, between 10,000 and 3,000 years ago) would have separated America, Asia, and the New Siberian Islands, and it provides us with a clear and sufficient explanation for the reported dredging up of mammoths' tusks from the bottom of the Arctic Ocean, or of their being thrown up, as it is said, upon the beaches of the Arctic Ocean during Arctic storms.

The progressive elevation of Siberia during the displacement provides us, finally, with a very complete explanation of the many curious, enigmatic circumstances surrounding the discovery of the mammoths' remains, and those of other animals. But before these problems can be explained, another circumstance must first be briefly discussed.

I have mentioned that volcanic eruptions would be having

their direct and indirect effects in Asia as in North America, and, indeed, everywhere. The effect of volcanic dust, as we have seen, is to chill the atmosphere. In the last chapter I presented indirect evidence that the volcanism during the displacement was at times massive enough to produce major lowering of the average temperature, and continuous enough to keep it low for long periods. In addition, I presented direct evidence that many volcanoes were active during the period in areas that are now quiet.

I can form no idea as to just how many volcanoes might have been active simultaneously at any time during that displacement of the crust. An educated guess is apparently not possible. I will assume, however, that during any displacement the average quantity of volcanism annually would be considerably greater than at present. This seems a safe assumption.

I will assume, secondly, that at intervals during the 10,000-year period of the displacement, volcanism would reach a higher point than the average for the whole period. This has already been suggested (Chapter VII). Such periods would be likely to occur while the crust was moving at its greatest speed, that is, during the middle part of the period (Chapter XI). These would be periods of readvance of the dwindling continental icecap. They would be of varying length, but some of them would last a long time.

My third supposition will be that occasionally, during periods of very active volcanism, there would occur a conjunction of several major volcanic explosions in the same year. The mathematical probabilities would favor this occurrence. It would be strange indeed, considering the stresses and strains to which the crust of the earth would be subjected during its displacement, if this did not happen now and then.

Let us now look at the consequences of this. Let us suppose five explosions in one year of the magnitude of the explosion of Mt. Katmai (or a larger number of lesser explosions). According to Humphreys, this would produce

enough dust to intercept 100 per cent of the sun's radiation; consequently, the earth's surface would receive no heat at all. It seems very unlikely that things could ever have gone quite as far as this; nevertheless, considering that volcanic dust is circulated around the world in a matter of days, and that the refrigerating effects on the atmosphere may be felt in a matter of weeks or months, there exists the possibility of a sudden and drastic fall in temperature following soon after such a conjunction of volcanic explosions.

The direct effects of this sudden fall in temperature on animals would be serious enough, but the indirect effects concern us more, for the moment. The amount of humidity the atmosphere can hold is proportional to temperature. If a mass of air is heated, it will pick up more moisture. If it is cooled drastically, the precipitation will be drastic, in the form of either rain or snow. At the same time, precipitation will be increased by an increase in the number of dust particles in the air, because raindrops require dust particles to condense around, or so it is thought. Our situation, after a massive outburst of volcanism in a short period, would be that while the air was being drastically cooled, there would at the same time be an enormous increase of the convenient dust particles. This could add up to precipitation of moisture on an enormous scale.

Now let us return to Siberia. Let us suppose that the region is being steadily moved northward, and that its elevation above sea level is increasing. In a certain year, a conjunction of several major volcanic eruptions takes place. Let us come back to our Beresovka mammoth. He is feeding quietly in the grassy meadow, and he has just swallowed a mouthful of buttercups, and has gathered up, with his trunk, a new mouthful of wild beans. The temperature is warm, and there is no sign of what is about to occur. The volcanoes have shot off their dust some time before. Cold air currents are circulating ominously, but unperceived, not far away.

I have seen a situation like this in Canada, during Indian summer. Day after day the sun is warm, although the nights

are cold. The forest still has a summer look. It is still possible to swim in the lake, and then come out and stretch in the sun to get dry and warm. But suddenly, without transition, in the course of a few hours, the northwest wind brings winter. The lake freezes up, and the very next day, as far as the eye can see, there is nothing but ice and snow.

Things moved faster, or at least more drastically, in the grassy meadow. The storm came down early in August, perhaps first with rain, and terrific wind. Humphreys has pointed out that the effect of a large quantity of volcanic dust in the atmosphere must be to increase the temperature gradient between the poles and the equator, resulting in more rapid circulation of the air, in winds of greater velocity. I shall present additional evidence of this in the following pages. Since winds occasionally attain a velocity of 150 miles an hour or more even today, we may conservatively suppose that the storm could have hit the grassy meadow at that speed. At the beginning of the disturbance, the mammoth would have stopped eating—without even bothering to swallow the last few beans in his mouth. He would have left the meadow and, accompanied by his friends, sought shelter in the nearest forest, as, no doubt, he had often done before during storms. Here, in the confusion of the storm, perhaps because of the force of the wind itself, he may have fallen over the cliff at the bottom of which his body was found. He was not killed, but a leg bone and his pelvic bone were broken. He couldn't walk, so he lay there, while the wild hurricane in a very few hours brought down subzero air masses from the polar zone, and the rain turned to snow, and piled up around him. He lifted the fore part of his body as far as he could above the snow, but it piled up, in the lee of the cliff over which he had fallen, until it was above his head. By this time, however, he might already have frozen to death in his half-standing posture. A similar fate would have engulfed millions of animals. During the ensuing months of winter the snow mantle would still thicken, and during the

following summer, because of the continuing effects of the volcanic dust, it would scarcely melt at all.

At this point many animals have been frozen into the snowdrifts; moreover, except for some that may have been dismembered by the force of the wind, they are intact. They are frozen in the ice of a nascent icecap. Since the region moves ever northward, the ice cover tends to remain over most of the area; it does not quite melt away during the summers, and it gets thicker and thicker as Siberia approaches the pole.

But here some impatient reader with some geological knowledge will break in and say, "But there was no icecap in Siberia!" Quite so: no continental icecap finally developed; the icecap that began to grow never came to maturity. Instead, it melted away.

One need not look far for the explanation of this. We have noted that the region was uplifted during the displacement. In the earlier stages, while the climate was still warm, the uplift had been sufficient to open land communications with the New Siberian Islands and Alaska. In climate, a slight increase of elevation may be the equivalent of hundreds of miles of latitude. So, after a movement of the crust had progressed to a certain distance, and the resulting increased elevation of the land had brought lowered temperatures, a thin icecap stretched over northern Siberia.

But when, after the end of the displacement, isostatic adjustment began to correct the elevation, and to bring the area down, it also warmed the climate, and so the nascent icecap melted away, and no doubt this development was furthered by the Climatic Optimum. During this warm period, the ice sheet in Siberia may have melted very rapidly, and torrents of melt water may have borne the bodies of the animals along, after they were disengaged from the ice, and torn their bodies apart, and heaped them up in great numbers as we find them, and buried them in vast seas of freezing mud. In most cases, of course, only the bones remained. Brooks cites a statement by Flint and Dorsey (160:

627) giving evidence of a recent "thin and inactive" ice sheet in Siberia, exactly as the hypothesis demands.

It might frequently occur that an animal would escape being washed out of the melting icecap. It would seem that the Beresovka mammoth was one of these. He seems to have remained frozen in the attitude of his last struggle in the snowdrift. In a succession of thaws, however, the ice encasing his body was apparently washed away and replaced by freezing mud. The temperature of the carcass may have approached the melting point at these times, but though this would have destroyed the edibility of the meat, it would not have disintegrated the body. It would even have been possible for the outside of the body to have thawed briefly during its translation from the icecap to the permafrost with little or no decay, because of the absence of germs in the Arctic climate. It would seem that the Academicians who examined the mammoth *in situ* were not impressed with his edibility; they did not try any mammoth steaks.

It appears that this assumption of a thin, temporary icecap in Siberia gradually transformed into permafrost solves most of the outstanding questions about the mammoths and the other animals whose remains are found in Siberia. It explains, for example, why some of the mammoths have been found on the highest points of the tundra. It explains the configuration of the deposits in which they are found. It explains Tolmachev's remark that "mammoth-bearing drift deposits sometimes have a thickness of tens of feet, sometimes they are spread out in comparatively thin layers" (422:51). The "drift deposits" are water-formed; they can now be explained as the result of the melting of the thin icecap, which must have produced rapidly flowing rivers that picked up and deposited quantities of mud, thicker in some places than in others, and filled with bodies and parts of bodies dropped out of the icecap. The prompt refreezing of these seas of mud, after the thaw, created the permafrost.

But even if these assumptions seem to solve the problems, the reader may still say, "Well—very good. But what if all

this isn't true after all?" All I can then do is to point to some tangible evidence that exactly this sort of thing happened in another part of the earth. But to examine this evidence we must turn aside into one of the byways of science, and examine an unsuspected facet of the Wisconsin glaciation. We must return to North America, and reconstruct the story of the birth of that icecap.

7. *The Mastodons of New York*

About seventy-five years ago, considerable excitement was aroused in scientific and popular circles by the discovery of the remains of extinct animals in various parts of the United States. Perhaps the most sensational of these finds was that of the mastodons. Many of these were found in New York State, and in some cases they were so well preserved that it was still possible to analyze the contents of the animals' stomachs. Some extensive accounts of these mastodons have appeared in print (178, 309, 203). Since science, like clothing, has its fashions, a period of attention to the mastodons was followed by a period of neglect. Neglect did not overtake them, however, before conclusions were reached respecting them. It was noted that nearly all the best-preserved remains were found in bogs and swamps, where, it was assumed, they had been mired and sucked down to their deaths.

This explanation of their deaths was accepted, apparently without any dissent, and it involved, of course, the acceptance also of the opinion that the animals had inhabited New York State after the departure of the ice sheet. It was concluded that the animals were postglacial. The conclusion was inevitable, because the ice sheet would have plowed up all bogs, and the animals' bodies could not have been preserved from destruction by it.

So the matter rested—the mastodons ceased to attract attention. There followed a period during which the general trend of scientific opinion led finally to the view that neither

mammoths nor mastodons, nor any of the other extinct Pleistocene animals, had lived in North America after the ice age. But no one has gone back to dig up the evidence of the mastodons and ask how, if they did not live in New York State after the ice age, they came to be buried in the bogs where they are found. It is now our task to reopen this closed chapter, to drag these ancient beasts once more from their tomb. We shall first look at some of their case histories, and then summarize our findings.

One might imagine two alternative solutions of the problem. Either the mastodons lived in New York State after the ice age, and got themselves mired in the bogs where their bodies are found, or they got mired in the bogs before the ice age, and somehow escaped destruction by the passage over them of the mile-thick ice sheet. In the latter case, the animals would have been found in beds of swampy vegetable stuff below the sand, gravel, and striated stones deposited by the glacier. How, then, are we to explain the fact that the animal remains are not found in this layer but are mixed up with the glacial materials themselves? Since this is usually true, we are driven to the conclusion that the animals were mired in New York State bogs neither before nor after the coming of the ice sheet. Before pressing on to further conclusions, let us consider the details of a few cases.

In August, 1871, a mastodon was discovered one mile north of Jamestown, New York, and the remains were examined *in situ* by Professor S. G. Love and others. Love described the find as follows:

> On the east side of the Fredonia road, about one mile north of Jamestown, is the farm of Joel L. Hoyt. About 500 yards from the road is a sink or slough covering about an acre, possibly more in extent, and varying from two to eight feet in depth, and fed by several living springs. Cattle have been mired and lost there since the farm was first occupied. Mr. Hoyt drained the sink and left the muck to dry, and later commenced an excavation there. The work of excavation had continued a little more than a week, when the workmen began to find (as they supposed) a peculiar kind of wood and roots,

imbedded some six feet beneath the surface. For several days they continued to carry the small pieces into an adjoining field with the muck, and to pile the larger ones with pine roots and stumps to be burned. But Mr. Hoyt discovered unmistakable evidences of the remains of some huge animal. At once there was a change in the procedure, in order to secure specimens and determine their character. It was difficult to determine the precise position of the remains, as they were much disturbed and partially removed before any special notice was taken of them. From the best information I could get, I conclude that the body lay with the head to the east, from four to six feet beneath the surface, and in a partially natural position. Many of the bones were, however, out of place. The lower jaw was about five feet from the head and lay on the side crushed together so that the rows of teeth were very near each other. The tusks extended easterly in nearly a natural position, and, judging from the statements of Mr. Hoyt and the workmen, they must have been from ten to twelve feet in length. After digging into the gravel and clay about ten inches I found traces of a rib, decayed but distinctly marked, over five feet in length. Where the body must have lain were found large quantities of vegetable matter (evidently the contents of the stomach) mostly decayed, in which were innumerable small twigs varying from one half inch to two inches in length. The remains were all in a very forward state of decay; and when I reached the ground I found it possible to do little more than had already been done to preserve them. . . . (203:14–15). [There follows a list of individual parts found.]

Here an important point is the fact that parts of the remains were found mixed with the sand and gravel, which had been deposited by the ice sheet as it retreated. This fact suggests that the carcass may have been dropped or washed out of the icecap into a glacial pool, which later through a process of countless freezings and thawings and accumulation of sediments became a bog. Later Professor Love amplified the foregoing remarks in a very interesting manner, in a paper read before the Chautauqua Society of History and Natural Science, July 16, 1885:

The twigs found in such large quantities where the stomach would naturally be were found, upon microscopical examination and comparison, to be of the same kind (genera and species) as the cone bearing trees (pine and spruce) of the present day. Mingled with the twigs

was a mass of yellowish fetid matter, probably the remains of some vegetation which did not possess the staying qualities of the balsamic cone-bearers (203:15–16).

Several important points are illustrated in these passages. They are all brought out, as well, in numerous other cases. The more significant points appear to be:

a. The rib was found buried in the glacial material under the muck, as already mentioned.

b. Some force crushed the jaw and separated other parts of the body, without completely disturbing its natural position. We shall find many cases of this sort of thing.

c. The stomach was found to contain evidence of vegetation such as now grows in New York State. Since the animal did not live after glacial times, and since the vegetation naturally did not exist in New York State when it was covered by a mile of ice, it follows that the animal lived in the last Interglacial Period, and that New York then had a climate something like the present.

d. The discovery of the remains in a mire tells us nothing of the mode of death, because only in a mire would they have had any chance of being preserved. Animals dying in other situations, or left in dry places by the retreating ice, would have disintegrated completely.

e. The evidence favors the conclusion that the body of the mastodon was preserved within the vast ice sheet itself, and was deposited when the ice withdrew, in a bog where conditions continued to favor its preservation. The power of bogs to preserve animal and vegetable matter for long periods is well known.

The instances cited by Hartnagel and Bishop of mastodons that clearly were not mired in bogs include one mastodon whose remains, a tusk, were found in sand near Fairport; another whose tusk and teeth were found in sand and gravel in the town of Perrinton; one whose remains (ribs, skull, tusk, leg bone) were found "about four feet below the surface in a hollow or water course, lying on and in a very hard body of blue clay, and about two feet above the polished

limestone . . ."; "a rib of a mammoth or mastodon found 12 feet below the surface of the ground in gravel" at Rochester, and other instances (203:35–39).

Dr. Roy L. Moodie, of the New York State Museum, in his *Popular Guide to the Nature and the Environment of the Fossil Vertebrates of New York,* discusses a mastodon that was deposited entire in a glacial pothole:

> . . . The pothole was made by whirling waters grinding the loose stones in a depression, gradually deepening in the post-glacial Mohawk River, and the body of the mammoth came down with the ice and dropped into the hole. . . . (309:103–05).

This particular case provides an excellent illustration of the process by which so many of the animal bodies were torn apart, not only in New York State but also probably in Siberia, where the melting of a thinner ice sheet would still have produced torrents in which the bodies detached from the ice could be dashed against rocks and broken to pieces.

Hartnagel and Bishop, referring to the few remains of mammoths that have been found in New York State, remark:

> There is no doubt that the mammoth remains were imbedded in the sand and gravels laid down during the recession of the ice sheet. Examples of these are best seen in the Lewiston specimens of teeth and bones which were found deeply buried in the spit formed in Lake Iroquois. . . . (203:67).

Sir Charles Lyell visited the site of the discovery of a mastodon near Geneseo, Livingston County, New York, and described his observations in his *Travels in North America,* in a passage quoted by Hartnagel and Bishop:

> I was desirous of knowing whether any shells accompanied the bones, and whether they were of recent species. Mr. Hall and I therefore procured workmen, who were soon joined by some amateurs of Geneseo, and a pit was dug to a depth of about five feet from the surface. Here we came upon a bed of white shell marl and sand, in which lay portions of the skull, ivory tusk and vertebrae, of the extinct quadruped. The shells proved to be all of existing freshwater and land species now common to this district. I had been told that the mastodon's teeth were taken out of *muck,* or the black superficial

> peaty earth of the bog. I was therefore glad to ascertain that it was really buried in the shell-marl below the peat, and therefore agreed in situation with the large fossil elks of Ireland, which, though often said to occur in peat, are in fact met with in subjacent beds of marl (203:31–32).

Let us, in passing, note in this passage the evidence that whatever occurred in Siberia and in North America (though perhaps at different times) seems also to have occurred in Ireland.

If it is now clear that the mastodons were actually contained in the great continental glacier, a question may arise as to their probable numbers. Were these animals rare or did they exist in great herds? Hartnagel and Bishop quote an earlier writer, who remarked:

> . . . I have been particular in stating the relative situations and distances of those places in which bones have been discovered, from a certain point, to show, from the small district in which many discoveries have been made, the great probability that these animals must have been very numerous in this part of the country, for if we compare the small proportion that swamps, in which only they are found, bear to the rest of the surface, and the very small proportion that those parts of such swamps as have yet been explored, bear to the whole of such swamps, the probable conclusion is that they must once have existed here in great numbers (203:62).

It must not be supposed that only mastodons and some mammoths were thus caught in the ice sheet and deposited as it retreated. Hartnagel and Bishop give instances of the finding of remains of foxes, horses, large bears, black bears, giant beaver, small beaver, peccaries, deer, elk, caribou, moose, and bison, forming a mixture of extinct and still living species, all deposited in the same way (203:81–94). Moreover, discoveries were not confined to New York State, but embraced the entire area once covered by the great ice sheet.

We must therefore conclude that in all probability some millions of all these sorts of animals were enclosed in the ice sheet. Now the question must be asked, Did they live in the regions where they are now found, or were they carried

greater or lesser distances by the moving ice? This is an important question. Glaciers often carry vast quantities of debris, even huge boulders, hundreds of miles, even in some cases uphill (63:14–15). Hartnagel and Bishop give an excellent description of the way in which a great ice sheet moves, and provide a partial answer to the question, in the following passage:

> The remains of most of the Canadian animals that were overwhelmed in and by the glacial snows were incorporated in the lower, or ground-contact ice of the southward moving sector of the Quebec (Labradorian) ice cap. The deepest portion of the ice cap was pushed into the deep Ontarian valley and becoming stagnant because of its position and also because of its load of detritus, it served during all the duration of the Quebec glacier as a bridge over which the upper ice, by a shearing flow, passed on south over New York. This element of glacier mechanics is fundamental to the present explanation of the peculiar distribution of the Elephas remains and is believed to describe the behavior of the continental glacier toward deep and capacious valleys, not only those transverse to the ice flow but also longitudinal valleys. . . . (203:69).

This statement should be supplemented by Coleman's remark that a great continental icecap, independently of any valleys, moves only in its upper layers, except at the edges. The layers near the ground are stagnant. This is the reason, he says, that the evidences of glaciation are found mostly on the exterior fringes of the area once occupied by the ice sheet. The central areas escape the grinding and plowing up that leave such evidences in the latter areas.

The usual explanation of this remarkable fact leaves something to be desired. According to the accepted concept, an ice sheet forms first in a small area, and then spreads out, presumably after it has become thick enough to move outward by the force of gravity. That means, it seems to me, that all but a small central area should show signs of having been passed over by the glacier. Yet, according to Coleman, the opposite is the case.

I think our assumption provides a good answer to this problem, for the initial snows, such as we assume may have

occurred in Siberia as the result of dust produced by massive outbreaks of volcanism, would cover a considerable area, enclosing the animal remains. Later, if the ice sheet developed enough to move by gravity, the upper layers would move, leaving a stagnant layer, at least in some places, to protect the animals, and only at the remote fringe or at high points of the area would the now moving icecap containing its load of rocks and pebbles be continuously in contact with the ground. In Siberia the icecap, if there was one, never grew thick enough to move by gravity. In North America, on the other hand (at an earlier time), the icecap did grow, until it began to move.

So we may conclude that the animals contained in the great North American icecap were mostly living, at the time they were overwhelmed by the snow, in the places where they are now found. It would not be wise, of course, to exclude entirely the transportation of animal remains in the ice sheet; very certainly it occurred, and perhaps quite often, but in all probability the vast majority of the animals were in the stagnant ground-contact ice. We have seen that the animals represented in the collection in the glacier indicate a temperate climate, like that prevailing in New York State today. Now the final question is, How did these millions of animals, living in a temperate zone, get caught in the ice sheet?

Note that here there is no dispute about the climate at the time the animals were living. Here the situation is free from the uncertainty surrounding the exact climate in which the Beresovka mammoth lived. The animals listed above are sufficient in themselves to establish the fact of a temperate climate; however, there is an additional piece of evidence, in the form of a quite fascinating botanical analysis of the contents of a mastodon's stomach. Hartnagel and Bishop quote from a report by Dr. J. C. Hunt:

> The remains, both of cryptogams and flowering species, were in abundance. Stems and leaves of mosses were wonderfully distinct in structure, so much so that I could draw every cell. I even readily de-

> tected confervoid filaments, with cells arranged in linear series, resembling species now found in our waters. Numerous black bodies, probably spores of the mosses, were found in abundance. Not a fragment of sphagnum was seen in the deposit. I found, however, one fragment of a water plant, possibly a rush, an inch long, every cell of which was as distinct as though growing but yesterday. Pieces of the woody tissue and bark of herbaceous plants, spiral vessels, etc., were abundant. Carapaces of Entomostraca were present, but no trace of coniferous plants could be detected. It hence appears that the animal ate his last meal from the tender mosses and boughs of flowering plants growing on the banks of the streams and margins of the swamps, rather than fed on submerged plants; and it is probable, moreover, that the pines and cedars, and their allies, formed no part of the mastodon's diet (203:58).

Here we see that everything indicates a climate similar to that of New York today. An interesting point is the difference between the mastodon's diet indicated here and that indicated in the case mentioned earlier. Speculation suggests that perhaps the diet in this second case indicates the animal's preferred diet, or perhaps merely the diet available in the summer, while that in the earlier case, in which twigs were so important, may represent either the winter diet of the mastodon or an emergency diet, the result of the destruction of the normal diet by the events occurring just before the animal's death. In any case, the second diet indicates that whatever happened to that mastodon certainly took place in the summer.

Now it is obvious that the arguments used to explain away the evidence of climatic change in Siberia won't work in New York. Here there was certainly climatic change, with a vengeance. The explanation I have offered for the preservation of the Siberian remains will cover both cases. The great difference between them, aside from the failure of the Siberian ice sheet to develop into a real icecap, consists of the fact that while the melting of the thin Siberian ice sheet left a permafrost in which many remains could be preserved, the melting of the Wisconsin icecap left temperate conditions in which nothing could be preserved except what happened to find

itself in bogs. Thus the great accumulations of bodies, such as are found in Siberia, and such as probably also were piled up by the rushing torrents coming from the melting Wisconsin icecap, simply rotted away and left not a trace behind.

Now that we have satisfactorily established that the mastodons were imprisoned in the Wisconsin icecap itself, it is necessary to add the correction that, despite this, they also survived the ice age, at least in western North America. Radiocarbon dates from 9,600 to 5,300 years ago have been found for some mastodon remains. It must be conceded that they may possibly have survived in North America until a much later date than this. They therefore could have lived in New York State after the ice age and have been caught in bogs. But they must also have lived in New York State before the ice age and been caught in the icecap, for otherwise their remains would not have been found in so many cases intermixed with the glacial materials (434).

8. Storm!

I have referred to the possibility that the extinction of animals and preservation of their bodies may be accounted for in part by violent atmospheric disturbances, and I have offered some evidence that such disturbances did accompany the last displacement of the crust, and therefore, presumably, earlier displacements.

It may be hard to distinguish between the effects on animal life of ice action (that is, of being melted out of glaciers and subjected to the action of glacial streams) and the effects of atmospheric factors. Nevertheless, perhaps some evidence of the operation of the atmospheric factors is available.

The evidence is presented, in part, by Professor Frank C. Hibben, in *The Lost Americans,* and since his description of the evidence is firsthand, and is presented so clearly, I have asked his permission to reproduce the pertinent passages.

He begins with a general description of the Alaskan muck,

in which enormous quantities of bones (and even parts of bodies) are found:

> In many places the Alaskan muck is packed with animal bones and debris in trainload lots. Bones of mammoth, mastodon, several kinds of bison, horses, wolves, bears, and lions tell a story of a faunal population. . . .
>
> The Alaskan muck is like a fine, dark gray sand. . . . Within this mass, frozen solid, lie the twisted parts of animals and trees intermingled with lenses of ice and layers of peat and mosses. It looks as though in the midst of some cataclysmic catastrophe of ten thousand years ago the whole Alaskan world of living animals and plants was suddenly frozen in midmotion in a grim charade. . . .
>
> Throughout the Yukon and its tributaries, the gnawing currents of the river had eaten into many a frozen bank of muck to reveal bones and tusks of these animals protruding at all levels. Whole gravel bars in the muddy river were formed of the jumbled fragments of animal remains. . . . (212:90–92).

In a later chapter Professor Hibben writes:

> The Pleistocene period ended in death. This is no ordinary extinction of a vague geological period which fizzled to an uncertain end. This death was catastrophic and all-inclusive. . . . The large animals that had given their name to the period became extinct. Their death marked the end of an era.
>
> But how did they die? What caused the extinction of forty million animals? This mystery forms one of the oldest detective stories in the world. A good detective story involves humans and death. These conditions are met at the end of the Pleistocene. In this particular case, the death was of such colossal proportions as to be staggering to contemplate. . . .
>
> The "corpus delicti" of the deceased in this mystery may be found almost everywhere . . . the animals of the period wandered into every corner of the New World not actually covered by the ice sheets. Their bones lie bleaching on the sands of Florida and in the gravels of New Jersey. They weather out of the dry terraces of Texas and protrude from the sticky ooze of the tar pits of Wiltshire Boulevard in Los Angeles. Thousands of these remains have been encountered in Mexico and even in South America. The bodies lie as articulated skeletons revealed by dust storms, or as isolated bones and fragments in ditches or canals. The bodies of the victims are everywhere in evidence.
>
> It might at first appear that many of these great animals died

natural deaths; that is, that the remains that we find in the Pleistocene strata over the continent represent the normal death that ends the ordinary life cycle. However, where we can study these animals in some detail, such as in the great bone pits of Nebraska, we find literally thousands of these remains together. The young lie with the old, foal with dam and calf with cow. Whole herds of animals were apparently killed together, overcome by some common power.

We have already seen that the muck pits of Alaska are filled with the evidences of universal death. Mingled in these frozen masses are the remains of many thousands of animals killed in their prime. The best evidence we could have that this Pleistocene death was not simply a case of the bison and the mammoth dying after their normal span of years is found in the Alaskan muck. In this dark gray frozen stuff is preserved, quite commonly, fragments of ligaments, skin, hair, and even flesh. We have gained from the muck pits of the Yukon Valley a picture of quick extinction. The evidences of violence there are as obvious as in the horror camps of Germany. Such piles of bodies of animals or men simply do not occur by any ordinary natural means. . . . (212:168–70).

So far, Professor Hibben's description of the evidence in Alaska may be consistent with the solution I have suggested for the evidence in Siberia, and for the area of the former Wisconsin icecap. No doubt Alaska also had a temporary icecap, since it was, in effect, merely an extension of Siberia, and apparently had the same kinds of animals at about the same time. However, it is evident that the animals that were killed far to the south, in Florida, Texas, Mexico, and South America, cannot have been contained in any icecap, whether thin or thick. Professor Hibben suggests that other factors were at work.

One of the most interesting of the theories of the Pleistocene end is that which explains this ancient tragedy by world-wide, earth-shaking volcanic eruptions of catastrophic violence. This bizarre idea, queerly enough, has considerable support, especially in the Alaskan and Siberian regions. Interspersed in the muck depths and sometimes through the very piles of bones and tusks themselves are layers of volcanic ash. There is no doubt that coincidental with the end of the Pleistocene animals, at least in Alaska, there were volcanic eruptions of tremendous proportions. It stands to reason that animals whose flesh is still preserved must have been killed and buried quickly to

be preserved at all. Bodies that die and lie on the surface soon disintegrate and the bones are scattered. A volcanic eruption would explain the end of the Alaskan animals all at one time, and in a manner that would satisfy the evidences there as we know them. The herds would be killed in their tracks either by the blanket of volcanic ash covering them and causing death by heat or suffocation, or, indirectly, by volcanic gases. Toxic clouds of gas from volcanic upheavals could well cause death on a gigantic scale. . . .

Throughout the Alaskan mucks, too, there is evidence of atmospheric disturbances of unparalleled violence. Mammoth and bison alike were torn and twisted as though by a cosmic hand in Godly rage. In one place, we can find the foreleg and shoulder of a mammoth with portions of the flesh and the toenails and the hair still clinging to the blackened bones. Close by is the neck and skull of a bison with the vertebrae clinging together with tendons and ligaments and the chitinous covering of the horns intact. There is no mark of a knife or cutting instrument. The animals were simply torn apart and scattered over the landscape like things of straw and string, even though some of them weighed several tons. Mixed with the piles of bones are trees, also twisted and torn and piled in tangled groups; and the whole is covered with fine sifting muck, then frozen solid.

Storms, too, accompany volcanic disturbances of the proportions indicated here. Differences in temperature and the influence of the cubic miles of ash and pumice thrown into the air by eruptions of this sort might well produce winds and blasts of inconceivable violence. If this is the explanation of the end of all this animal life, the Pleistocene period was terminated by a very exciting time indeed (212:176–78).

In Chapters IV and VII we saw that volcanic eruptions, possibly on a great scale, are a corollary of any displacement of the crust; therefore, our theory strongly supports and reinforces the suggestions advanced by Professor Hibben, and at the same time his evidence strongly supports our theory. But Professor Hibben points out certain consequences that would flow from our theory, which I have not stressed. Wherever volcanism is very intensive, toxic gases could locally be very effective in destroying life. This is also true of violent local windstorms. Massive volcanic eruptions might, of course, occur anywhere on earth during a movement of the crust, and we saw, in Chapter VII, that they apparently oc-

curred in a good many places, some of them far removed from the ice sheets themselves.

Despite the unquestionable importance of these locally acting factors, it seems that we must give much greater importance to the meteorological results of the universally acting volcanic dust. As we have noted, this dust has a powerful effect in reducing the average temperatures of the earth's surface. A sufficient fall in temperature could easily wipe out large numbers of animals, either directly, or by killing their food, or even by favoring the spread of epidemic diseases. Then, the dust could greatly increase rainfall, which, in certain circumstances, would produce extensive floods, thus drowning numbers of animals and perhaps piling their bodies in certain spots. As already mentioned, the dust would also act to increase the temperature differences between the climatic zones (the temperature gradient), thereby increasing, perhaps very noticeably, the average wind velocities everywhere. Violent gales, lasting for days at a time, and recurring frequently throughout the year, might raise great dust storms, in which animals might be caught and killed by thirst or suffocation. It must not be forgotten that, at the same time, changes in land elevations would be in progress, and these also would be affecting the climate and the availability of food supplies. The gradual character of these changes would be punctuated, at times, by the abrupt release of accumulating tensions in the crust, accompanied by terrific earthquakes and by sudden changes of elevation locally amounting perhaps to a good many feet, which also could be the cause of floods either inland (by the sudden damming of rivers) or along the coasts. There is, as a matter of fact, as already mentioned, much evidence of turbulence throughout the world, during the last North American ice age, not only in the air but in the sea.

I have not been able to make a complete survey of this evidence. Nevertheless, a few additional items have come to my attention. Ericson, for example, finds that turbidity currents in the sea were more powerful during the ice age than they

are today (141:217). Kulp found, by radiocarbon dating, that deposition of sediments along the eastern coast of North America occurred at a fast rate prior to about 15,000 years ago (262). Millis cited evidence of very violent winds during the melting phase of the Wisconsin icecap (308:14). Violent storms would seem a very natural explanation for the peculiar finds of many bodies of animals crammed into caverns and fissures, dating from various geological periods, that have been found in various parts of the world. Hibben mentions one of these (212:173–74). It would seem possible that, in storms of the character that may have occurred, caverns may have been the only refuges available for man and beast alike. Dodson refers to the fact that the destruction of animals in dust storms was apparently the cause of the preservation of many fossils (115:77). Volchok and Kulp, in their examination of ionium dating as applied to several Atlantic Ocean deep-sea cores, remarked that "at the close of the Wisconsin [glacial period] the rates of sedimentation for both sediment types [red clay and Globigerina ooze] increased by factors of 2–4" (442a:219). This means that the rate of deposition of sediment in the deep sea at these points was increased by from 200 to 400 per cent. This certainly suggests an unusual turbulence for the climate.

It is little wonder that, faced by all these unpleasant conditions, a good many species in all parts of the world, even very far from the icecaps, gave up the struggle for existence.

In conclusion, it appears to me that the whole mass of the evidence relative to the animal and plant remains in the Siberian tundra, interpreted in the light of the evidence from North America, sufficiently confirms the conclusion that there was a northward displacement of Siberia coincident with the southward displacement of North America at the end of the

IX : EARLIER DISPLACEMENTS OF THE CRUST

1. Introduction

According to the evidence presented in the last two chapters, the Hudson Bay region lay at the North Pole during the period of the Wisconsin ice sheet. It is not possible (with evidence now at hand) to define the geographical position of the pole more exactly; it may have been located in Hudson Bay itself, somewhat to the west in Keewatin, or somewhat to the east in the province of Quebec. Coleman refers to the fact that the earlier advance of the Wisconsin ice sheet entered Michigan from the north—from the direction of Hudson Bay—rather than from Labrador (87:16). Flint remarks:

> . . . It is evident that in Cary time, the ice first entered Minnesota from the Rainy Lake District on the north, later from the northwest, and still later from the northeast via the Lake Superior basin. . . . (375:171).

Flint explains that all the known centers of the Wisconsin glaciation are relatively late; they date from the declining stage, when, according to our theory, the crust was in motion. The evidence of the earlier centers would, he points out, have been destroyed by the ice flow of later times (375: 171). It is evident that the two principal ice sheets in this region—the so-called Keewatin and Labradorean ice sheets—were part of the same glaciation, and were contemporary, although the western center was the first to develop. Coleman mentions that this earlier phase of the ice sheet—the so-called Keewatin—transported boulders from the Laurentian area near Hudson Bay to the foothills of southern Alberta, depositing them at an altitude of 4,500 feet (87:15). This would indicate that the ice was moving westward from Hudson Bay.

It would also indicate that the ice sheet at this time was about a mile thick, while the Labrador ice sheet, at least in its later phases, was not nearly so thick (375:169). The suggestion here is strong that the pole was in Hudson Bay itself, and that the Labrador ice sheet began to develop when the main ice sheet was wasting. It is possible that it took over as the glacial center because the supply of moisture to feed the thinning icecap was better nearer the coast. This might have been the result, in part, of the opening up of water areas by the shrinking of the icecap.

Now, it follows logically that if the Wisconsin ice sheet existed because the Hudson Bay region lay at the pole, and if it disappeared because of a displacement of the crust that moved North America away from the pole, then the Wisconsin ice sheet must have been brought into existence as the result of an earlier displacement. The question, therefore, now arises, Where was the pole situated previous to its location in or near Hudson Bay? It also becomes important to establish as closely as possible the date of this earlier displacement.

We have already discussed the date of the beginning of the climatic change that produced the Wisconsin glaciation. A considerable amount of evidence has now accumulated that there were Wisconsin glacial phases earlier than the Farmdale (133). Furthermore, we must remember that the Farmdale date of 25,000 years ago is only the date of the invasion of Ohio by the ice sheet, which had previously to advance a long way from its center of origin. Evidence to be presented below will strongly support the conclusion that the beginning of the change of climate—that is, of the movement of the crust—that eventually produced the Wisconsin icecap was about 50,000 years ago. This date, as we shall see, is not in conflict with evidence of cold climate in the North Atlantic extending back considerably further.

With the date of the beginning of the Wisconsin glaciation tentatively fixed in this fashion, it is possible, by the use

of certain methods of deduction, to reach an educated guess as to what area lay at the pole during the previous period.

The method of locating a previous polar position is simple in principle, but very complicated in practice. The principle is to find a point on a circle drawn about the last established polar position with a radius of the same order of magnitude as the distance between the present pole and the last position. The assumption underlying this is that while one displacement may move the crust (and therefore shift the poles) farther than another, the chances are against any very great differences. It seems that the last displacement, which brought Hudson Bay down from the pole, amounted to about 2,000 miles on the meridian of maximum displacement. We shall therefore start out with the idea that the previous displacement may have been of about the same magnitude, but it can easily have been half as great or twice as great. This can later be checked with the field evidence.

Our first step is to draw a circle around the hypothetical polar position in Hudson Bay, with a radius of 2,000 miles. Now, with a very liberal margin of error, we can assume that the previous pole lay somewhere near that circle. Our second step is to check the field evidence for past climates for the whole earth to see what position on or near that circle will explain the most facts.

The difficulties encountered in assembling the evidence for a pole in Hudson Bay were very great, but they did not compare with the difficulties of establishing a reasonable case for the position of the previous pole. For this earlier period, embracing about 40,000 years, the evidence was much scantier. The margins of error on all climatic determinations had to be much greater. The numerous lines of evidence had to be examined in the light of the conscious and unconscious assumptions applied to them by previous workers, whose objectives and methods had been influenced by an entirely different set of ideas, and whose interpretations of the evidence might therefore be very different from mine.

The method used was that of trial and error. I selected a possible location, and then searched the available evidence to see whether that location was reasonable. I tried many locations, giving up one after another as facts turned out to conflict with each of them.

After the Hudson Bay location had been settled to my satisfaction, I considered, for a while, that the previous position might have been in Scandinavia. I was forced to abandon that idea. Other positions, investigated in turn, included Spitzbergen, Iceland, and Alberta. There was a great deal of shifting back and forth.

Finally, clarity began to set in; ever more numerous facts began to fall into place, and at last I had reason to feel that my feet were on solid ground. The previous position of the pole was, I concluded, in or near southern Greenland, or between Greenland and Iceland. That is, the Greenland region then lay at the pole.

I repeated the process, with this polar position as the center of my circle, and a rather flexible radius, and came up, to my considerable surprise, with a pole somewhere in or near Alaska, perhaps in the Alaska Peninsula or in the Aleutian Islands. This third pole takes us back to about 130,000 years ago, and of course the evidence for it is much slighter than that for the Greenland pole. Despite the fact that this hypothetical position is hardly more than a suggestion to guide further research, it is highly important because it serves as a point of reference to "box in" the Greenland pole.

It is impossible, with the evidence now at hand, to reconstruct any earlier displacements of the crust. However, evidence of a late Pleistocene continental glaciation in Eurasia suggests the possibility of one or two former polar zones in that land mass. Though this evidence has been known to Russian geologists for many years, it has attracted the attention of Western geologists only since 1946 (219). One of these positions may account for the so-called "Riss" glaciation in Europe.

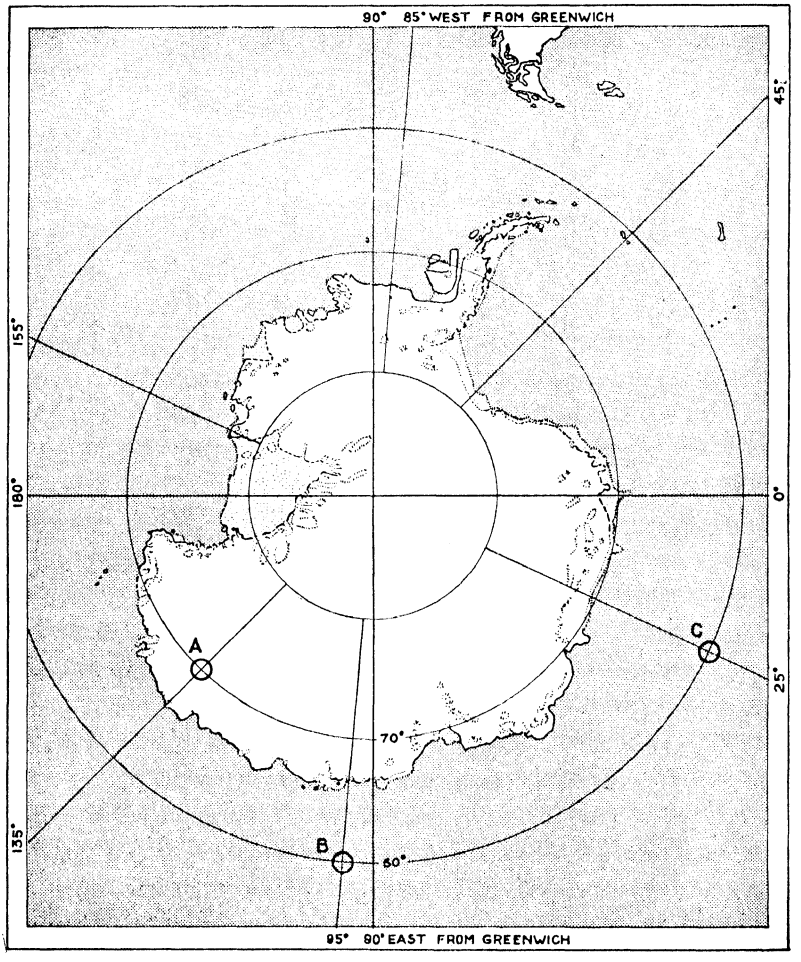

Fig. VII. Antarctica: Three Earlier Locations of the South Pole
A *corresponds to the North Pole in Alaska,* B *to the North Pole in Greenland, and* C *to the North Pole in Hudson Bay. Positions are approximate.*

At a still earlier time, western Canada may have lain at the pole, and this may account for the so-called Illinoisan glaciation—the ice age that preceded the Sangamon Interglacial. However, since the quantity of the evidence declines

by a geometrical progression as we go backwards, clearly these earliest suggested positions for the crust are of value only as guides to research.

I am suggesting three displacements of the crust in the last 130,000 years, the intervals between being of the order of 30,000 or 40,000 years. Considering the fact that the Wisconsin glaciation, if it started about 50,000 years ago, had an over-all span of about 40,000 years, it is not unreasonable to assume similar spans for the earlier periods, though perhaps we should allow for a considerable variation of their lengths. Whether this rapid pace was maintained all through the earth's history is a matter that perhaps cannot be settled at this time; however, I will discuss it briefly further on. So far as the Pleistocene is concerned, Suess and Emiliani, at least, see evidence that major climatic change did take place at that rate (409:357). Their explanation that climatic changes resulted from the cyclical astronomical curve of solar radiation is not convincing, for reasons already made clear.

Much of the evidence that I will use to support this suggested series of displacements is in the form of cross sections of sedimentary deposits, called cores, which often singly embrace very long periods of time. Rather than discuss each core separately, the simplest method will be to assemble the evidence from all the cores, so far as it bears on each suggested polar position in turn. The lines of evidence include marine cores from the Arctic, Antarctic, North Atlantic, Equatorial Atlantic, and South Pacific Oceans, and the Caribbean Sea; many radiocarbon and oxygen isotope findings, pollen studies, and various evidences relating to the interglacial periods. The purpose of the presentation of this evidence will be to explain the known major climatic changes of the last 130,000 years, in terms of displacements of the earth's crust. Before attempting this reconstruction of the glacial history of the late Pleistocene Epoch, however, we must first discuss some of the current ideas in this field.

2. *Weakness of the Accepted Glacial Chronology*

Geologists are used to thinking of four major glaciations during the million-year period of the Pleistocene. They have assumed that each glaciation affected the earth as a whole simultaneously, causing ice sheets in both Northern and Southern Hemispheres, and lowered temperatures generally. Some geologists have questioned this concept of four glaciations; it is at least necessary to recognize several successive phases of advance and retreat for the older glaciations. Whether these interruptions were merely interstadials, like those of the Wisconsin glaciation, or were true interglacials it is increasingly hard to decide the further back in time one goes. According to the accompanying chart of the glacial periods (p. 282), it is evident that the intervals between the different stages of the major glacial periods are in some cases longer than the entire duration of the Wisconsin glaciation. It does not seem reasonable, therefore, to insist that they were merely interstadials, nor, consequently, to insist upon the number of just four glaciations during the Pleistocene.

This becomes more apparent when we consider the implications of the Eurasian continental glaciation mentioned above. This, obviously, makes at least a fifth Pleistocene glaciation, but the matter does not end there. The question must be asked, If European geologists could overlook the evidences of this comparatively recent glaciation until the last decade (and this in spite of the fact that the evidences were spread widely over two continents, and had attracted the attention of Russian geologists as long as seventy-five years ago), how many other glaciations in various parts of the world may not have escaped attention? Flint has pointed out how easily glacial evidence can be destroyed (342:171). Coleman also emphasized the same thing:

> It might be supposed that so important a change would leave behind it evidence that no one could dispute, and that there should be no room for doubt as to what happened in so recent a time of the

earth's history. In reality the proof of the complete disappearance of the ice and its return at a later time is, in the nature of things, a matter of great difficulty and it is not surprising that there are differences of opinion (87:20).

Croll pointed out the ephemeral character of glacial evidence eighty years ago in books that are still eminently readable. After first discussing the accumulations of strata containing plant and animal remains during a period of temperate climate, he comments thus on their subsequent destruction:

. . . We need not wonder that not a single vestige of [these strata] remains; for when the ice sheet again crept over the island [Britain] everything animate and inanimate would be ground down to powder. We are certain that prior to the glacial epoch our island must have been covered with life and vegetation. But not a single vestige of these is now to be found; no, not even of the very soil on which the vegetation grew. The solid rock itself upon which the soil lay has been ground down to mud by the ice sheet, and, to a large extent, as Professor Geikie remarks, swept away into the adjoining seas (91:257).

It is obvious, of course, that whatever could destroy all the surface deposits of a temperate period would also, at the same time, destroy any evidences of former glaciations. Croll goes on to say:

It is on a land surface that the principal traces of the action of ice during a glacial period are left, for it is there that the stones are chiefly striated, the rocks ground down, and the boulder clay formed. But where are all our ancient land surfaces? They are not to be found. The total thickness of the stratified rocks of Great Britain is, according to Professor Ramsay, nearly fourteen miles. But from the bottom to the top of this enormous pile of deposits there is hardly a single land surface to be detected. True, patches of old land surfaces of a local character exist, such, for example, as the dirt beds of Portland; but, with the exception of coal seams, every general formation from top to bottom has been accumulated under water, and none but the under-clays ever existed as a land surface. And it is here, in such a formation, that the geologist has to collect all his information regarding the existence of former glacial periods. . . .

If we examine the matter fully we shall be led to conclude that the transformation of a land surface into a sea-bottom (by erosion and

deposition of the sediments) will probably completely obliterate every trace of glaciation which the land surface may once have presented. . . .

The only evidence of the existence of land ice during former periods which we can reasonably expect to meet with in the stratified rocks, consists of erratic blocks which may have been transported by icebergs and dropped into the sea. But unless the glaciers of such periods reached the sea, we could not possibly possess even this evidence. Traces in the stratified rocks of the effects of land-ice during former epochs must, in the nature of things, be rare indeed (91:267–69).

Croll was interested in pointing out the impermanence of glacial evidence. He continued, therefore, as follows:

The reason why we now have, comparatively speaking, so little direct evidence of former glacial periods will be more forcibly impressed upon the mind, if we reflect on how difficult it would be in a million or so of years hence to find any trace of what we now call the glacial epoch. The striated stones would by that time be all, or nearly all, disintegrated, and the till washed away and deposited in the bottom of the sea as stratified sands and clays. . . . (91:270).

In view of the facts presented by Croll, it would appear to be most unreasonable to insist on any fixed number of Pleistocene glaciations simply because hitherto it has been possible to group, in a very rough way, the comparatively few evidences we have in four glacial periods.

It is a well-known fact that the chronology of four Pleistocene glaciations has been built up on the foundation of the assumption that all glacial epochs were the result of lowered world temperatures. Thus the European glaciations were declared to have been contemporary with the glaciations in America, although, as a matter of fact, no evidence of this existed. The assumption was based solely on astronomical and other theories of the causes of glaciation that we have shown to be inadequate. If the grouping of all European glacial evidences into only four major glaciations is questionable, and if, in addition, there is no good evidence that these glaciations were really contemporary with those in America, then the possibility of a large number of different

glaciations in America and Europe during the Pleistocene must be taken seriously.

If the number of these alleged major Pleistocene glaciations is not satisfactorily established, the attempts at dating them leave even more to be desired. A review of the past and current literature on the subject reveals lack of agreement. Estimates vary widely, and none of them has convincing support. To make this plain, it is only necessary to compare the various estimates. The table on page 282 shows the estimates made by Penck and Bruckner, considered the leading European experts (whose work, however, was done before the development of nuclear techniques of dating), and by Zeuner, whose estimates were endorsed by the climatologist Brooks. The reader will note that Zeuner divides each of the older glaciations into a number of substages, some of which are longer than the entire period covered by the Wisconsin glaciation. The reader will recall that the interstadials and the successive advances of the Wisconsin glaciation had durations of the order of two or three thousand years; he may also note in the various cores shown later on that all the cores show brief climatic changes of the same magnitude. It is therefore impossible to concede that the earlier glaciations could have had interstadials 40,000 or more years long. The only explanation ever advanced for the oscillations of the Wisconsin ice sheet that I know of is the one advanced in this book: massive volcanism caused by displacement of the crust. This explanation cannot, however, be reasonably applied to oscillations 40,000 years in length.

We see that the estimates of Zeuner (52:107) and of Penck and Bruckner (52:107) are in profound disagreement.

It would be easily possible to multiply the number of such contradictory estimates, or, if the reader pleases, he may accumulate authorities who will support one of them; but is it not obvious that if leading professional geologists can differ to such an extent, no real reliance can be placed upon any of their very approximate and very speculative estimates? And

TABLE II

The Pleistocene Glaciations

Zeuner (European)		Penck & Bruckner (European)
Würm Glaciation		40– 18,000
Stage III	25,000	
Stage II	72,000	
Stage I	115,000	
Riss Glaciation		130–100,000
Stage II	187,000	
Stage I	230,000	
Mindel Glaciation		430–370,000
Stage II	435,000	
Stage I	476,000	
Gunz Glaciation		520–490,000
Stage II	550,000	
Stage I	590,000	

when, in addition, we find they have all been wrong as to the number of Pleistocene glaciations—since a fifth one has just turned up—are we not justified in dismissing all these estimates as speculations that are no longer worth discussing?

If there is any doubt as to the reasonableness of this conclusion, it should be put at rest by an entirely new estimate of the glacial chronology just produced by Emiliani. Emiliani, working with marine cores, and applying some of the new techniques of dating, has found that the earliest Pleistocene glaciation occurred only 300,000 years ago, and that all the four recognized European glaciations, and their alleged American counterparts, have to be compressed into that comparatively short period (152). This finding, which there is no rational reason to reject, completes our picture; it

disposes, finally, it seems to me, of the traditional glacial chronology of the Pleistocene.

As a consequence of this breakdown of the old theory, it seems to me that we must now start from the beginning, and build a new glacial chronology of the Pleistocene. Our method can only be the tested method of science: to proceed from the known to the unknown; from the Wisconsin glaciation, where our information is most ample, backwards.

3. The Beginning of the Wisconsin Glaciation

Examination indicates that, with the elimination of some mutually contradictory and evidently disturbed sediments, the three Ross Sea cores show a change from the deposition of glacial sediment to the deposition of temperate sediment on the Ross Sea bottom 40,000 years ago (p. 306). As a first step in establishing an approximate date for the beginning of the climatic change (that is, for the beginning of the movement of the crust) that was to produce, simultaneously, the temperate age in Antarctica and the glacial age in North America, we must interpret the meaning of this change in the sediment.

In the first place, the date itself is subject to some doubt. Two cores show the change at 40,000 years ago; one shows it a few thousand years later, but at the same time it also shows a change at 40,000 years ago from one type of glacial sediment to another. The change in that core is from coarse to fine sediment, which in itself indicates amelioration of climate. Thus, all three cores indicate at least some warming of the climate about 40,000 years ago.

Now, what lapse of time must we allow between the beginning of the change of climate in Antarctica and the resulting end of glacial deposition on the Ross Sea bottom? In the first place, we must remember that the movement of the crust would at first be entirely imperceptible. The speed of the displacement would increase slowly. After several thou-

sand years a warming of the climate would be noticeable in the areas moving equatorward, and a cooling of the climate would occur in areas moving poleward, but these long-term trends of change would often be modified or even reversed by other factors. Massive volcanism would erupt before the crust had moved far, and the effect might be to check the retreat of the icecap, and even to cause its readvance after it began to retreat. We can see that this seems to have happened with the Wisconsin icecap: for thousands of years after it began to grow thinner, and to retreat, it went through phases of readvance.

Before the final disappearance of glacial sediment from the Ross Sea bottom about 40,000 years ago, then, we must assume that the icecap then in Antarctica had gone through an initial phase of very gradual retreat, followed by perhaps several phases of readvance, until finally it withdrew entirely from the Ross Sea coast, and melted sufficiently to permit free-flowing rivers to bring down temperate-type sediment from the interior. It seems obvious, on the analogy of the Wisconsin icecap, that we should allow a period of time of the order of 10,000 years or so for this entire process. This suggests, then, that the movement of the crust began about 50,000 years ago. However, it does not mean that the movement ended 40,000 years ago; it might even have continued for another 10,000 years. The cores contain no evidence on that point. It may have taken much more than 10,000 years to shift the crust. The core evidence, however, may be used to support the thesis that the crust displacement that brought the Hudson Bay region to the pole started about 50,000 years ago. In the following discussion we will tentatively consider that the previous position of the pole was in southern Greenland.

a. The Arctic Cores

In the last few years Soviet expeditions in the Arctic Ocean have taken a number of deep-sea cores, and have dated them

by the ionium method. These cores provide impressive confirmation of the situation of the pole in North America during the Wisconsin glaciation, and, in addition, they furnish evidence of the approximate date of its migration to the Hudson Bay region from its previous position.

The Soviet scientists were much impressed by their discovery that in the comparatively short period of the last 50,000 years there have been no less than six major changes of climate in the Arctic Ocean. They did not find it easy to explain all these changes. They may all be explained, however, by the hypothesis of two displacements of the earth's crust.

The period begins with a very cold phase. Since the cores go back only 50,000 years, we do not know when the cold period began. The scientists remark:

> It seems that in the period in question, a considerable part of the Arctic shelf was dry; there was no, or almost no, communication with the Atlantic. The climate was cold (364:9).

According to the principles already set forth (Chapter IV), a pole in southern Greenland might be expected to have coincided with higher elevation of that general region, including the adjacent sea bottoms. The continental shelves in that part of the Arctic Ocean facing toward Scandinavia might well have been raised above sea level. There might well have been a land connection between Greenland and Iceland, and even across the narrow North Atlantic to Scandinavia. The Soviet scientists themselves strongly suggest that this land connection must have existed; nor are they alone in their suggestion. Years ago Humphreys advanced the idea. More recently Malaise has produced much new evidence for it (291a). Thus it is evident that our theory has started out pretty well, by explaining why there was little communication between the two oceans. The interruption, however, was not simply the result of land masses in the North Atlantic. It could also have been the result of having a polar area lying right across the connections between the

Arctic and the North Atlantic. A glance at the globe will make this clear.

Beginning 50,000 years ago, and lasting for about 5,000 years, there was, according to the Soviet scientists, a brief warm spell in the Arctic Ocean:

> . . . The bottom sediments became finer; argillaceous and highly argillaceous oozes began to be deposited, of a brown or here and there dark brown color, with increased contents of iron oxides, manganese, and foraminiferous micro-fauna (364:9).

This warm period may reflect the movement of the crust that resulted in withdrawal of the pole from Greenland and its shift toward Hudson Bay. It is followed, however, by a cold period in the Arctic from 45,000 to 28–32,000 years ago. This following cold period is explained, according to our theory, by the beginning of the subsidence of the land bridge, which may have opened a water connection between the two oceans. The pole was at this time still near the Atlantic, and very cold Atlantic water was thus able to pour into the warmer Arctic. The subsidence of the land bridge as the pole moved away is, of course, a corollary of our theory. Such a subsidence in any area moving away from a pole would positively have to occur, unless counteracted locally by the relocation at particular points under the crust of lighter rock detached from the underside of the crust elsewhere.

Following this cold period in the Arctic, we come to a most remarkable change, one which, in my opinion, provides an unusually impressive confirmation of our assumption of the location of the Hudson Bay region at the pole. About 28–32,000 years ago, a really warm period began in the Arctic, and lasted until 18,000 to 20,000 years ago. The Soviet scientists have found that during this period there was a rich development of temperate-type microfauna in the Arctic, though their explanation is very different from ours:

> . . . The luxuriant development of a foraminiferal fauna of North Atlantic type testifies that during this period warm Atlantic waters were invading the Arctic Basin on a broad front; that is, communication between the Arctic Basin and the Atlantic Ocean, which had

apparently been interrupted in the previous period, was reestablished. The duration of the warm period has been set at approximately ten to twelve thousand years (364:11).

According to our interpretation, this luxuriant development of microorganisms of temperate type in the Arctic Ocean marks the movement of the pole into the interior of North America, and away from the Atlantic. This movement must have initiated a temperate period in all that half of the Arctic Ocean facing Scandinavia and Siberia. Again, a glance at the globe will make this clear. The reader cannot fail to see that with the pole in Hudson Bay, the Arctic and the Atlantic Oceans would lie on the same temperate parallel of latitude.

This indication of temperate conditions in the Arctic gains enormously in significance when considered in connection with the evidence of the Ross Sea cores for the same period in Antarctica. It seems that here we have evidence of warm periods near both the present poles. Yet, obviously, it is impossible to claim that the whole earth was warmer at the time, because of the evidence of widespread glacial conditions in both North America and Europe. It seems to me that a reasonable person is forced to the conclusion that the crust shifted.

b. Earlier Phases of the Wisconsin Glaciation

Let us briefly reconstruct the history of the crust displacement that resulted in the shift of the polar location from Greenland to Hudson Bay. This will explain the earlier, unknown phase of the Wisconsin glaciation. As has been pointed out, the earliest known phase of that glaciation is the Farmdale, only 25,000 years ago. Despite this fact, very many pieces of wood and other remains from glacial deposits in North America have been found to be much older than that. As already mentioned, Flint was led by this evidence to suggest that there must have been earlier glacial advances, the evidences of which were later destroyed. It has been shown

that the Mankato Advance of the ice sheet occurred only 11,000 years ago, and yet some deposits of Mankato age contain pieces of wood that have been found to be more than 30,000 years old. These must have been included originally in older glacial deposits.

Our theory may explain not only the earlier phases of the Wisconsin glaciation but also why most of their traces were destroyed. Let us assume that 50,000 years ago the previous crust displacement started shifting the pole from southern Greenland toward Hudson Bay. This would have started an expansion of glaciation in North America, with the ice moving toward the west and south. We must visualize a gradual lowering of the temperature over a period of several thousand years, with those sudden changes resulting from volcanism with which we are already familiar. In this case, instead of each advance of the icecap falling short, at least in some places, of the preceding advances, so as to leave traces in the form of undisturbed moraines, the contrary happened. As the crust moved, the pole was steadily advancing from the northeast, and therefore each phase of expansion of the ice sheet would naturally carry the ice front farther than the one before, plowing up and destroying the evidences of earlier phases, obliterating the record and incorporating the older glacial material with its own debris. Not until the climax of the glaciation was reached, with the Farmdale Advance, did a change in this situation occur. The date of this advance, therefore, may have been the time when the Hudson Bay region reached the pole and the crust ceased to move.

4. Greenland at the Pole

Several impressive lines of evidence are in accord with the assumption of the location of the general region of southern Greenland at the pole before the time when the Hudson Bay region may have been located there. In considering these lines of evidence, we shall assume that the period during

which Greenland was at the pole was of the order of 30,000 or 40,000 years, or that it was roughly comparable in length to the period when Hudson Bay lay at the pole. This will carry us back, therefore, about 90,000 years.

The assumption of a pole in southern Greenland seems able to solve a remarkable number of climatic problems pertaining to various parts of the world. In the first place, it can explain the evidence that indicates that the glacial period in Europe began earlier than the Wisconsin glaciation. It is consistent with the existence in New York State of the temperate fauna and flora which we saw were enclosed in the Wisconsin ice sheet. It would explain, as nothing else has explained, why the massive European ice sheet advanced southward only as far as the 50th parallel of latitude, while in North America the Wisconsin icecap extended southward to the 40th. A glance at the globe will be sufficient to show the reader the truth of this. In addition, this position of the pole will explain two different problems in Antarctica. On the one hand, in many parts of Antarctica, as already mentioned, there are glacial striations above the present level of the ice, indicating that a thicker ice sheet once existed in those areas; on the other hand, the Ross Sea core N–4 (see Figure XI, p. 306) indicates temperate-type sediment on the bottom of the Ross Sea from about 65,000 to about 80,000 years ago. Now this continuous deposition of temperate sediment on the bottom of the Ross Sea for nearly 20,000 years is very difficult to explain. It quite obviously cannot be explained at all in terms of the present position of the pole.

If the reader will turn to the map of Antarctica (p. 276) on which I have indicated the various positions of the South Pole corresponding to our Hudson Bay, Greenland, and Alaskan North Poles, he will note that a South Pole corresponding to our Greenland North Pole would imply a thicker ice sheet than now in some parts of Antarctica. So far as the Ross Sea is concerned, an interesting situation emerges. It seems probable that the side of the Ross Sea nearest this former South Pole would be glacial, while the other

side may have been nonglacial. Thus sediment of both kinds could be deposited in different parts of the Ross Sea at the same time, depending, perhaps, to some extent, on the local peculiarities of bottom topography and bottom currents. It cannot be assumed, of course, that the outlines of the continent, or the depths of the surrounding oceans, were the same at that time as they are now. However, those who may still be inclined to discount the theory presented in this book must be reminded that all the phenomena discussed in this chapter—and all the principal phenomena discussed in this book—have been hitherto unexplained. It is not a question of choosing between explanations. There is, at the present time, no other explanation than ours of these facts.

a. Cores from the San Augustin Plains

A good deal of evidence from other cores supports this succession of polar positions in Greenland and Hudson Bay. Let us consider first a very interesting study recently made in New Mexico. There a group of scientists have been studying a very long sedimentary core taken from the San Augustin Plains. On these plains, sediments have accumulated to a great depth without consolidating into rock, so it has been possible to bore down about 645 feet and get a cross section of all the deposits to that depth. These sediments contain pollen of various trees and plants, which, as is well known, does not easily disintegrate, but remains preserved in the soil for very long periods. In this core, different kinds of pollen in the different layers indicated changes in the species of trees and plants growing in the region, and of course changes in the kinds of plants growing in a region indicate changes of climate. For the upper and most recent part of the core, it was possible to use the radiocarbon method to date the changes in pollen types, and therefore in climate.

Figure VIII (opposite page), prepared by Drs. Clisby and Sears (84), shows the climatic curve for the upper 330 feet of the core. The radiocarbon date at 27,000 years ago plus

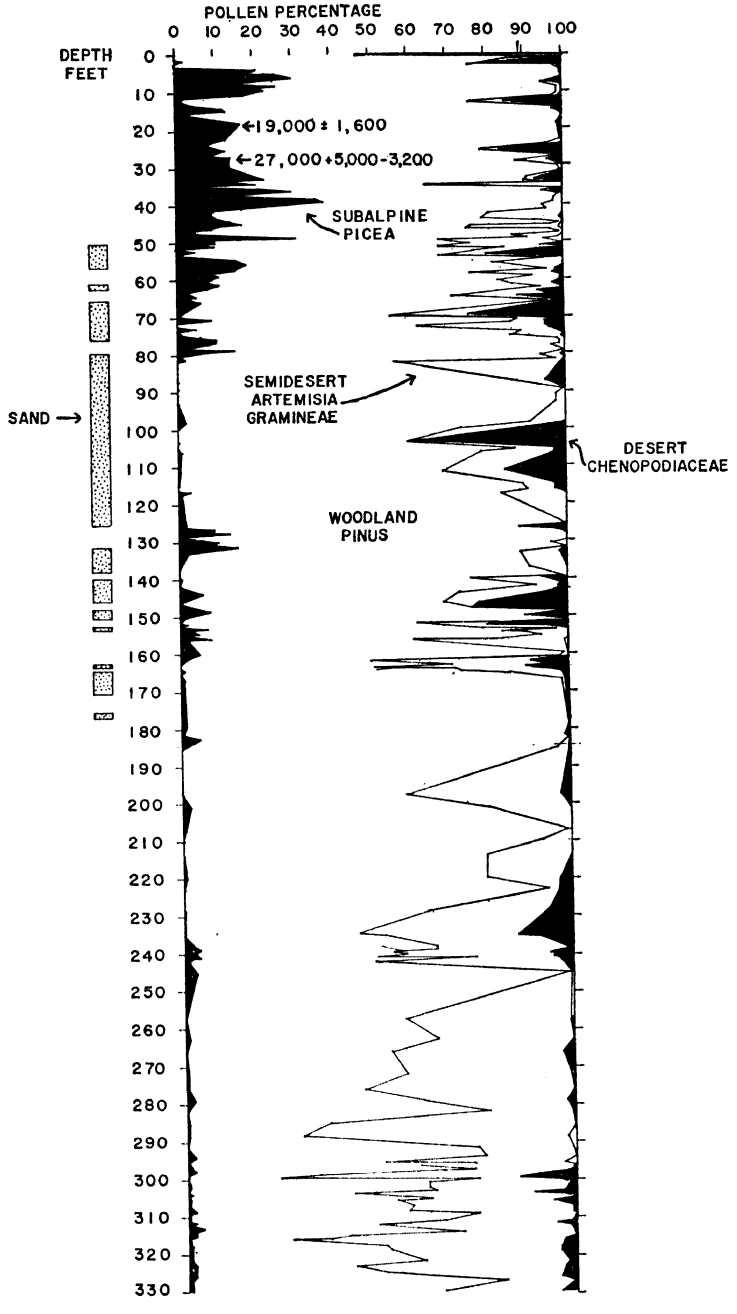

Fig. VIII. Pollen Profile from the San Augustin Plains, New Mexico

5,000 or minus 3,200 years (an unusually large margin of error) indicates glacial conditions at that time, and a rough extrapolation based on the rate of sedimentation indicated for the dated part of the core would indicate that glacial conditions began in New Mexico not more than about 40,000 years ago. A most remarkable thing about this core is that it indicates temperate conditions from this point all the way down to the bottom. When it is considered that the older section dated by radiocarbon, about 27,000 years old, was only 28 feet down in the core, it is evident that the core covers a very long time.

This climatic record has some very important implications for us. It appears to show, for one thing, that the glacial period in North America generally did not extend back as far as the glacial period appears to have done in the North Atlantic and in Europe. It seems to show, unmistakably, that the so-called Sangamon Interglacial continued without a break, down to about 40,000 years ago, at least in western North America. Thus, it implies that the Sangamon Interglacial in North America was contemporary with the earlier part of the Würm glaciation in Europe. This is a remarkable state of affairs, and appears to dispose irrevocably of the doctrine of the simultaneousness of glaciations. Incidentally, it provides confirmation of climatic conditions in New York State suitable for the mastodons and other animals that lived there before the growth of the Wisconsin icecap. It is therefore in agreement with our assumption of a pole in southern Greenland. This core, the data considered above from North America and the Arctic, and the Antarctic cores give us a triangulation that provides extraordinarily strong confirmation of the theory.

There is one factor, however, that may enter into the interpretation of this San Augustin core, which could invalidate our conclusions. The plains are now at an elevation of 7,000 feet above sea level. The earlier chapters of this book have explained how displacements of the earth's crust may cause major changes in land elevations. A change in land elevation

would, of course, affect the climate and therefore the vegetation growing in an area. It is generally considered that 1,000 feet in elevation is the equivalent of about eight hundred miles in latitude, so far as plant habitats are concerned. We therefore have to take into consideration the possible effect of an uplift of the plains during the time that the sediments were accumulating. In view of the fact that the 645 feet of sediments may easily embrace a total period of time exceeding a million years, or the length of the whole Pleistocene Epoch, and since we have evidence that there were a number of ice ages in North America during the Pleistocene, before the Wisconsin glaciation, we may be forced to assume that there was a considerable uplift of the plains during the Pleistocene. However, it is reasonable to suppose that during the comparatively short period we are now discussing—the period from about 40,000 to about 90,000 years ago—the uplift may not have been of major proportions.

Our Greenland pole is supported by other lines of evidence. In evaluating them, however, we shall have to take into consideration the still earlier hypothetical position of Alaska at the pole.

b. Some North Atlantic Cores

A number of cores have been taken in the North Atlantic, and dated by the new methods of absolute dating. Despite the fact that these cores were obtained independently by different scientists, and dated by different methods, we shall find that they agree well, and that they support our basic assumptions.

The cores to be considered include three taken from the North Atlantic in about Lat. 46–49° N., and dated by the ionium method, and three cores from the Caribbean and the Equatorial Atlantic.

The last three cores were prepared by Ericson, who analyzed their enclosed foraminiferal remains for evidence of climatic change. They were subjected to radiocarbon tests by

the United States Geological Survey. They were also studied by Dr. Cesare Emiliani, who used a technique developed by Dr. Harold C. Urey for determining ancient water temperatures. This technique makes use of an isotope of oxygen (O_{18}) the proportion of which in sea water is affected by temperature. The temperature of the water at which ancient shells grew (or, in this case, at which foraminiferal microorganisms grew) can be determined by the proportion of O_{18} in the remains. This determination is independent of time, and the temperature at which a shell grew 200,000,000 years ago (but not the date) can be determined just as well as if the shell grew last year. Emiliani used this technique to establish temperature curves for the sedimentary cores for the period within the range of radiocarbon. Then, using this as a base, he extrapolated to the older parts of the cores. There is reasonably good agreement between temperature curves arrived at in this way and those obtained by the ionium method.

Of the three cores taken from the North Atlantic and dated by the ionium method, one extends back to only 11,800 years ago, one to 24,300, and one to 72,500. The longest core will have the greatest interest for us (Figure IX).

Core P–126(5) was taken in mid-Atlantic, approximately at the latitude of Nova Scotia, in about three miles of water. Since it extends back to 72,500 years ago, it should be able to throw some light on the question of our assumed polar positions, including the Alaskan.

The story of this core, as we recede into the past, is as follows: There is first a layer of volcanic glass shards, dated about 12,800 years ago, then a layer of nonglacial sediment, then glacial sediment from 14,700 to 23,700 years ago, then nonglacial sediment but with evidence of cold water, back to 60,700 years ago. This period of cold water is interrupted by three brief intervals of glacial deposition, by two layers of confused ("anomalous") sediments, and by a layer of volcanic glass shards between 51,400 and 55,400 years ago. Then, between 60,700 and 68,000 years ago, the North Atlantic water

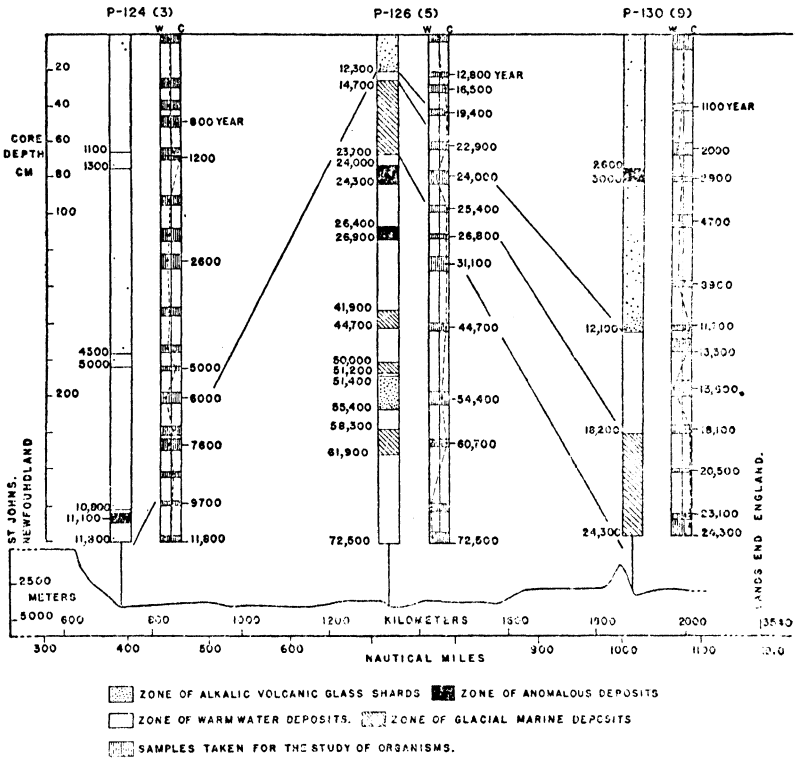

Fig. IX. Chronology of Sediments in the North Atlantic Cores P-124(3), P-126(5), P-130(9)

gets warmer, until it becomes definitely warmer than it is today. The warm period continues to the bottom of the core.

This core raises some enormously interesting questions. In the first place, it indicates very cold conditions, much colder than the present, in the North Atlantic back to about 62,000 years ago, and we must ask, How can this be reconciled with the indications of a climate like the present on the San Augustin Plains for the same period? We shall see that the other cores, from the Caribbean and the Equatorial Atlantic, will intensify this contradiction, for they confirm the cold climate in the Atlantic. These cores constitute evidence for a pole in the Greenland region.

A second vital question raised by the core is the existence of a warm North Atlantic 70,000 years ago: the foraminiferal studies made in connection with this core show that the North Atlantic was warmer then than now. There has been no explanation of this, yet obviously it is enormously important. It may be explained by a polar position in the Alaskan region. We will return to this question in connection with the other evidences for the location at that time of Alaska at the pole.

Deposits of volcanic glass shards are not, from our point of view, something to be passed over without remark, especially when, as in this case, the deposits relate themselves remarkably well to critical phases of our assumed crust displacements. The last such deposit agrees in time with the Cary Advance of the Wisconsin ice sheet, and may be considered as possible evidence of the intense volcanism that may have accompanied and, indeed, caused that glacial advance. The older band of volcanic shards also apparently coincides with a time of great change, as indicated by the Arctic cores already discussed. That was the time when, according to our theory, the displacement started that was to shift Greenland from, and Hudson Bay to, the pole.

It appears to me that the dating of the brief intervals of glacial deposition also has significance. For example, a brief interval of glacial deposition between 50,000 and 51,200 years ago immediately follows the deposition of volcanic glass shards, which according to our theory can have represented a period of massive volcanism, which may have acted as the direct cause of the glacial advance, which, in turn, may have led to the deposition of the glacial sediment in the sea. Of course the finding of volcanic debris and glacial sediment in sequence in the same core is an accident in one sense, for a prolonged episode of massive volcanism anywhere else on the surface of the earth would have produced the advance of glaciers adjacent to the Atlantic. During any movement of the crust, we may assume that volcanism would be very active in many parts of the earth, and not necessarily in the immedi-

ate neighborhoods of the ice sheets. The oldest episode of glacial deposition, about 60,000 years ago, coincides closely with the time that we have assigned hypothetically for the maximum of the Greenland glaciation.

A serious problem is presented by the fact that the sediment deposited in the North Atlantic during the period from 23,700 to about 60,000 years ago is not actually glacial except for brief episodes. How can this be accounted for if we are to assume a pole in Greenland? For that matter, how can it be accounted for if we are to extend the Würm glaciation in Europe back as far as the European geologists seem to demand?

It is obvious, from the brief recurrent episodes of glacial deposition, that somewhere an ice sheet or ice sheets lay near the coasts, so situated that when expanded in the brief periods of greater cold brought on by massive volcanism, one or more ice sheets reached the sea. It may be necessary to consider that the pole in Greenland may not have been very near the sea at that time, and, as I have already suggested, a number of authorities are now receptive to the idea of a land connection across the North Atlantic.

c. A North Atlantic Land Mass?

This, in turn, produces another problem. If there was a land connection across the North Atlantic, and if the pole was in southern Greenland, would not the continental ice sheet have entered the British Isles from the northwest, instead of from the northeast, from the direction of Scandinavia? Strangely enough, a very persuasive book has been written by Forrest (164) to sustain precisely this thesis. I have spent considerable time checking his sources. I have gone through many of the original reports he used, including numerous field reports published by the British Association for the Advancement of Science, the Liverpool Geological Society, and other British geological societies. I have also checked secondary works by various British geologists. As a result I

have found an impressive mass of reliable direct field observation indicating that the original direction of the ice invasion of the British Isles, in the earlier part of the ice age, was from the northwest. In most areas the evidences of the movement were later overlaid or destroyed by the Scandinavian ice sheet, or by local glaciers.

In reviewing the field reports, and later the general geological works that used the reports as source material, I have noticed a most interesting phenomenon. Field observers quite often remarked on the northwest-southeast directions of certain glacial striations, on associated glacial evidences showing clearly that the ice moved from the northwest toward the southeast, and on evidence that the ice sheet swept over the tops of most of the mountains of Ireland, Wales, and Scotland, often across the axes of the valleys, and from the northwest. But as the discussion of the subject was removed from direct contact with the field, and as the field reports were condensed, abstracted, and interpreted, this evidence of ice movement from the northwest became more and more subordinated, and, finally, was lost to view. The reason for this is quite simple. The geologists were firmly convinced that the ice sheet could have come only from Scandinavia. An ice center in the North Atlantic, involving former land areas in that region, was quite unthinkable. This was a natural consequence of the general acceptance of the theory of the permanence of continents. Various ingenious solutions of the problem were suggested. Charlesworth pointed out that a westward-trending valley could turn an ice sheet descending from the British highlands westward, and it might fan out on the coastal plain, so that its northern flank would actually be moving *toward* the northwest. Other geologists have stressed glaciers expanding in all directions from elevated areas in Britain. The awkward thing is that precisely what these people assert happened probably did happen, but in the later part of the ice age, when the North Atlantic ice center was gone, the Scandinavian glacier lay along the east coast of Britain, and local valley glaciers occupied the in-

terior. Nobody, however, has properly examined the data cited by Forrest as his principal line of evidence.

Recently Kolbe and Malaise have produced evidence that may cause many to turn to the work of Forrest with new interest. From the evidence of fresh-water diatoms indicating a former fresh-water lake on the mid-Atlantic ridge, now about two miles below the surface of the ocean, and from the study of the differing sediments deposited on the two sides of the ridge, Malaise reaches the conclusion that the mid-Atlantic range was above sea level until the end of the Pleistocene (291a:207).

Unfortunately, limitations of space must prevent further discussion of this question. I can only refer the reader to the works cited and to Forrest's original sources. Forrest has made a few mistakes in discussing matters dealing with ice sheets, but he himself calls attention to the fact that he was trained as a zoologist, not a geologist. However, his errors are not such as to affect the validity of his argument. The principal weakness of his book is that he does not present the evidence in sufficient detail, so that it is necessary to refer to the original sources.

It is possible that the displacement of the crust that resulted in the movement of the pole from Alaska to Greenland may itself have raised the North Atlantic bottom enough to bring a transatlantic land connection, in the manner suggested in Chapter IV, and that in consequence the Greenland ice sheet may have travelled along the land bridge into Britain. The subsequent displacement that shifted the pole toward Hudson Bay could have brought about the resubmergence of parts of the land bridge, and have resulted in the destruction of the Greenland continental glacier. The final movement of the pole to its present location could have resulted in the submergence of the remnants of the land bridge.

The advantage of our hypothesis is that it seems able to suggest both the cause of the creation of a land connection across the North Atlantic, and the cause of its disappearance. The fact that we have here two successive displacements of

the crust helps us to account for the total amount of the submergence. Each of these hypothetical movements would have shifted the North Atlantic nearer the equator, and therefore, according to the theory, would have favored subsidence of land area relatively to sea level. The evidence of very widespread volcanism in the whole basin of the North Atlantic during the ice age (174) carries the implication, also, that invasion of the lower parts of the crust by molten magma of high density could have weighted and depressed the area, in the manner suggested earlier (Chapter VI).

d. Additional Atlantic Cores

To return to our Atlantic cores, a rather interesting point about Core P–126(5) is that the deposition of glacial sediment ceased about 14,000 years ago. It may be noticed, however, that in another of the Urry cores, P–130(9), which was taken much farther to the east, the deposition of glacial sediment ceased 18,000 years ago. One might at first be inclined to pass this over as an unimportant detail, until one realized that with Hudson Bay at the pole the difference is completely explained. Under those circumstances, the second core, which now lies to the east, would have been due south of the first core, and it would be entirely natural that the warming of the climate at the end of the ice age would be felt first in the more southerly region. This, then, constitutes additional evidence for the location of the Hudson Bay region at the pole during the period of the Wisconsin glaciation.

We have still to consider the Ericson-Suess-Emiliani Atlantic cores. I reproduce, below, Suess's graph of the temperature changes in the Atlantic for the last 100,000 years, as evidenced by these cores.

Now, to begin with, we note that according to these cores the temperature of the Atlantic Ocean was at a peak between 75,000 and about 98,000 years ago; this agrees substantially with the Urry core already discussed, but extends the warm

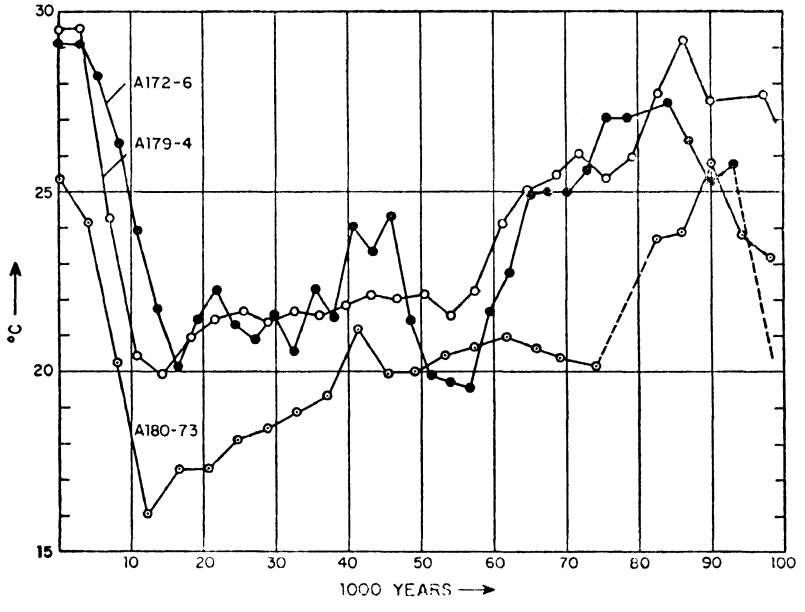

Fig. X. North Atlantic Cores A 172-6, A 199-4, A 180-75

period back to about 100,000 years ago. We will assume that this warm phase in the Atlantic represents the period, or at least the latter part of the period, when Alaska was at the pole.

We note, too, that the temperature in the Atlantic during this warm period was not, at the sites of these cores, as high as the temperatures now prevailing there, except for two very brief spurts in Cores A 180–75 and A 179–4. Yet we have seen that the Urry core P–126(5) indicated clearly that the water was warmer at that time than it is at present. How is this conflict to be solved? Shall we be forced to discredit the reliability of one finding or the other? Not at all. Our theory offers the possibility of eliminating this apparent contradiction in the evidence.

Let us consider the present and past latitudes of these cores. P–126(5) was taken in about the latitude of St. John's, Newfoundland; the others were taken in very low latitudes.

If we assume that Alaska was at the pole during this warm period in the Atlantic, the site of this core would then have been farther from the pole than it is now; that is, it would have been south of its present latitude. Quite naturally the water would have been warmer. A glance at the globe will suffice to make this plain.

On the other hand, if we now consider Core A 180–75, taken in the eastern Equatorial Atlantic, nearly on the equator, the opposite situation is revealed. A pole in Alaska would displace this core southward from the equator, possibly as far as the 20th parallel of South Latitude (depending, of course, on the precise location of the pole in Alaska). Quite probably, the water would be colder then than it is at present, other things being equal.

The two Caribbean cores would, with the Alaskan pole, have had approximately the same latitude as at present; the uncertainty as to the precise location of that pole makes it impossible to draw any reliable conclusions from them.

Both the Caribbean cores indicate a temperature minimum about 55,000 years ago, which would correspond well with the date we have tentatively assigned for the arrival of Greenland at the pole; they both show the temporary warm period that was shown in the Arctic cores, which we have interpreted as marking the breakup of the Greenland continental icecap. They then show a gradual temperature decline from about 40,000 to about 11,000 years ago, which may correspond, first, to the growth of the Wisconsin continental icecap and, finally, to the movement of the ice center of that icecap eastward into Labrador during the declining phases of the glaciation.

At this point it is important to consider a contradiction between the Caribbean cores and the core from the eastern Atlantic. It appears that the ocean temperature reached its minimum, after the early warm period, in the eastern Atlantic about 20,000 years before it did in the Caribbean. This is a very important difference. How is it to be explained?

It appears that this anomalous fact may constitute, in itself, one of the most impressive confirmations of the whole theory of displacements of the crust, for, if you look at a globe and visualize the shift of the crust that moved Alaska from, and Greenland to, the pole, and if you use a tape measure to measure the distances from each polar position to the Caribbean and to the equator off the bulge of Africa, you will see that that particular polar shift should make only a comparatively slight change in the latitude of the Caribbean, but a very radical change indeed in the latitude of the eastern Equatorial Atlantic. And since the movement would take the same period of time in both cases, the rate of movement would necessarily be much more rapid in the eastern Atlantic, and this both agrees with and explains the core evidence.

5. *Alaska at the Pole*

We have already seen that the assumption of Alaska's position at the pole between about 80,000 and about 130,000 years ago helps to solve a number of important problems. The idea for this position first occurred to me when I learned of the work of Arrhenius (13a), who took cores from the North Pacific and found that there was a fall of temperature in that area about 100,000 years ago. He decided that that fall of temperature must have marked the beginning of the ice age everywhere. But as we have seen, the evidence shows that this "ice age" began at very different times in different areas. We are forced to develop a theory that will explain why the ice age developed in different areas at different times.

I fully realize that this suggestion of one previous polar location before another in an apparently endless succession may cause some discomfort to the reader. He may be willing to settle for the Hudson Bay pole; after all, the evidence is rather overwhelming. The Greenland pole may be worth

considering, because after all it at least makes sense of the radiocarbon datings and the Atlantic marine cores. But this Alaskan proposition is, he may think, going altogether too far. Where will we end? Is the whole history of the earth to be considered in terms of this hop, skip, and jump of the poles? And is it conceivable that this sort of thing could have kept up for two billion years?

If the reader is having this sort of crisis of belief, I suggest that he consider the matter in this light: one shift of the crust, at the end of the ice age, has been pretty well demonstrated. Another one, at the beginning of the ice age, is necessarily and logically implied. The interval between them seems to have been about 40,000 years. Now if we accept one such unit, why not accept the previous ones also? After all, the laws of nature work continuously: that was the principle laid down by Sir Charles Lyell over a century ago. It is obviously sensible to work from the known to the unknown: if we are fairly sure that the crust did move once or twice, and at a certain rate, then it is but a jump to accepting without a qualm a thousand such movements in the long history of the globe.

The further advantages inherent in the assumption of the Alaskan pole, and the general evidence for it, may be briefly summarized. It explains the cause of the warm Interglacial Period in Europe, when lions and hippopotamuses and elephants romped around in Britain and on the Continent (to which Britain was then probably joined). It allows for the Sangamon Interglacial in the eastern parts of the United States and Canada. As between the two polar positions, in Greenland and Alaska, we have an explanation of the length of the Sangamon Interglacial. We may visualize it as a warm period that began and ended at different times in different parts of North America, and that was warmer in given areas at certain times than at others. The speed of climatic changes was slow enough to permit the gradual migration and adaptation of species, and yet there were also abrupt, disastrous changes due to the effects of volcanism, which, as we have

seen, could account for the widespread Pleistocene extinctions.

Speaking of more specific evidences, if the Alaskan Peninsula were near the North Pole, this would have meant the deglaciation of the Ross Sea and, indeed, of all that half of Antarctica. What have the Ross Sea cores (Figure XI) to say? Core N–4 shows deposition of nonglacial sediment from 110,000 to 130,000 years ago. This by itself provides some confirmation. Core N–5 shows deposition of nonglacial sediment from 130,000 to 180,000 years ago, implying that either the pole was situated in Alaska for a long time or else a still earlier polar position was so situated as to give Antarctica temperate conditions. Core N–3 shows fine glacial sediment from about 120,000 to about 200,000 years ago. This, too, implies a climate warmer than the present, but the evidence is, obviously, inconclusive. Only a great many more cores from the same area can clarify these ambiguities.

As I have mentioned, with each step backward into the past, the evidence decreases geometrically in quantity. The available indications of a pole in Alaska amount to no more than suggestions for research, but, in my opinion, they cannot be disregarded on that account. It might be objected that if Alaska was at the pole only 100,000 years ago, there should be plentiful evidence of a continental glaciation in Alaska. We must not be too easily impressed by this objection. In the first place, there is a possibility that many of the evidences have been destroyed since that time by the glaciers of the Wisconsin period, and by the present glaciers. Then, there is the obvious possibility that evidences have been overlooked or misinterpreted, as seems to have occurred in Britain. Finally, if widespread subsidence of land areas has occurred in the North Atlantic, so may it have occurred in the Pacific. Extensive evidence of a North Pacific land bridge (not to be confused with a Behring Strait connection) has been summarized by Dodson (115:373). How long Dodson's land connection may have lasted is not known, but it could conceivably have lasted until comparatively recent times.

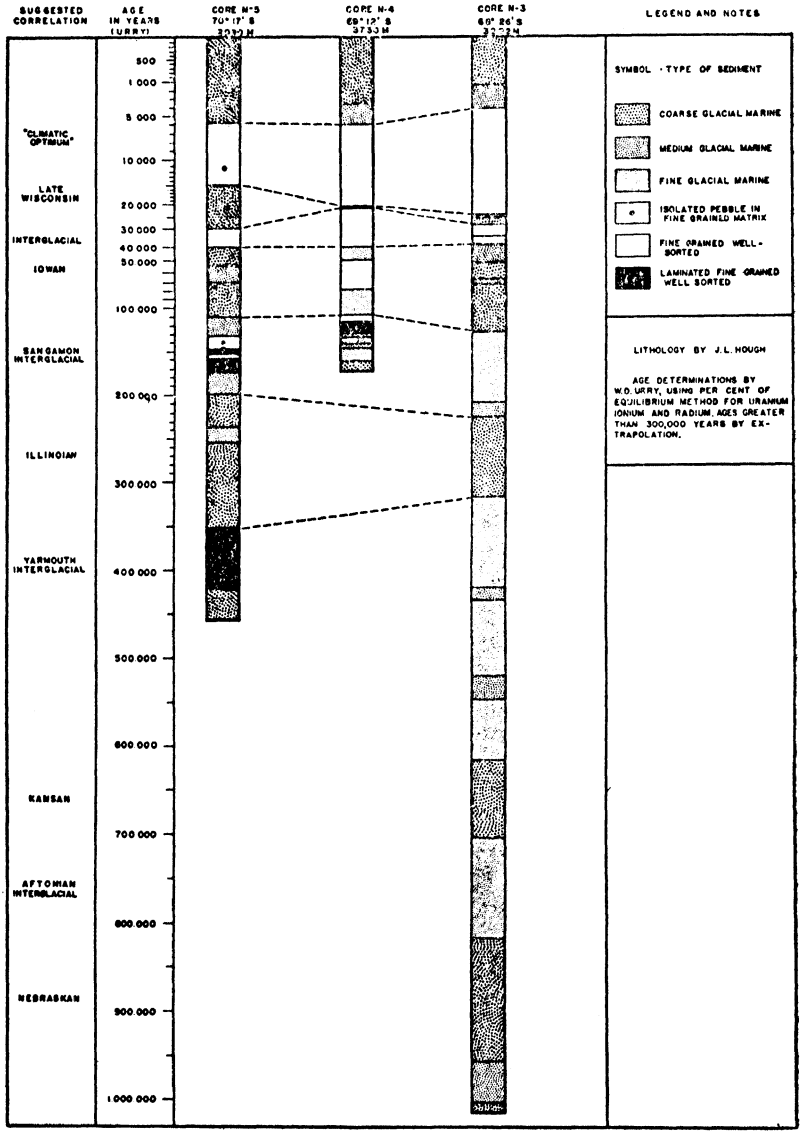

Fig. XI. Lithology of Core Samples, Ross Sea, Antarctica

A small amount of additional evidence for our assumed sequence of climates in Alaska has come to light. Our assumptions call for a frigid climate in Alaska down to about 75,000 years ago, with a warming of the climate coinciding with the refrigeration of the Atlantic as the polar position migrated to Greenland. Then, about 25,000 or 30,000 years ago, the climate was refrigerated (later than in eastern North America) as a consequence of the advance of the Wisconsin ice sheet.

Karlstrom (247) has shown through radiocarbon dating that the oldest glacial stage of the Wisconsin glacial period in Alaska began not earlier than the Farmdale Advance in Ohio, 25,000 years ago, and not later than 19,000 years ago. This is in accord with our supposition that the glacial climate advanced from the east. Before this Alaskan glacial stage, Karlstrom has an interglacial extending back an uncertain distance. This, obviously, is, in terms of current theory, in disaccord with the contemporary climatic trends in the Atlantic. Before the interglacial period Karlstrom notes evidence of a glaciation that he considers to be pre-Wisconsin but post-Illinoisan. Since this glaciation was beyond the range of the radiocarbon method, he had to depend upon the assumption of climatic control by the solar insolation curve in order to estimate its date. He estimates that this older glaciation was at least 47,000 but not more than 87,000 years ago. Presumably these dates mark the estimated time of the end of the glaciation. Even though his basis of calculation may not seem acceptable (for reasons already discussed), it is plain that his date is in pretty good accord with our date for the Alaskan pole. No doubt it is based in part on a lot of stratigraphic studies that prove the antiquity of the glaciation, and at the same time show that it is younger than the Illinoisan ice age.

This climatic reconstruction throws an interesting light on a forgotten item of paleontological research that I chanced upon in the *Smithsonian Miscellaneous Collections* for 1913. It was entitled "Notice of the Occurrence of a Pleistocene

Camel North of the Arctic Circle." The author, James William Gidley, described the discovery in Alaska not only of the camel remains but also of the remains of elephant and of other animals, including the horse and bison. He then remarks that the discovery "adds proof in support of the supposition that milder climatic conditions prevailed in Alaska during probably the greater part of the Pleistocene period" (173:1). We do not have to go all the way with Mr. Gidley. But here indeed is evidence enough of the existence of really temperate conditions very possibly in the very period of time when the assumption of the Greenland pole calls for them. A final consideration relating to the evidence for a pole in Alaska is that this could have caused the Cordilleran glaciation, which, according to Coleman, preceded the Labrador and Keewatin ice sheets (87:10). It may be added also that there is real support from fossil terrestrial magnetism for the polar succession in Alaska and Greenland. The Japanese geophysicist Akimoto and his colleagues have produced magnetic evidence of the migration of the pole from north of North Central Siberia, in the Arctic Ocean, across Alaska to Greenland in the Pleistocene (418:11).

Thus the matter must be left, for the present. If it seems to the reader that I have sometimes based too much on too little data, and that I sometimes attribute too much significance to isolated facts, I agree that I have probably committed this error at times. On the other hand, there is sometimes a tendency to postpone any thinking, in the hope that the necessity of revising basic principles may be removed by additional facts.

6. The Remoter Past

Since, as I have mentioned, with every step backward into time the evidence becomes thinner, it is hardly worth while to attempt, at present, to solve the problem of the earlier positions of the poles that would be required to explain the

climatic history of the Pleistocene. Eventually, perhaps, this can be done. Much more practical questions remain to be considered.

We must consider whether the rate of geological change suggested for the last 130,000 years, by the evidence presented in this book, can be typical for the entire history of the earth. It is plain from the cores that rapid change has characterized the record for the Pleistocene. Radiocarbon dating has established the fact that all the geological processes of glacial growth and decay, precipitation and sedimentation, were enormously accelerated during the Wisconsin ice age. Emiliani has argued, as already mentioned, that all the ice ages of the Pleistocene occurred in the last 300,000 years, which implies a threefold increase in the velocity of geological change, as compared with the older views. Studies of the delta of the Mississippi River suggest numerous important changes at short intervals (165, 276, 349). Blanchard has shown that there were at least twelve major climatic changes in the valley of the Somme since the first glaciation, accompanied by changes in sea levels, fauna and flora, and human cultures. As already mentioned, he argues that only polar change can explain this record (38).

For the older geological periods, there are a number of lines of evidence that suggest rapid change. So insistently, indeed, does this theme occur in the strata that Brooks, in his *Climate Through the Ages,* refers to a 21,000-year cycle of climatic change which he believes operated through the whole Eocene Period, or for about 15,000,000 years. His figure, of course, is only a rough average, and the intervals may have been very unequal in length. With reference to a still older period he remarks, "Alternations in the Cretaceous of U.S.A. suggest a cycle that is estimated at 21,000 years, but there are no annual layers" (52:108).

Irregularities in the cycle are indicated by another study of Eocene beds covering about 5,000,000 to 8,000,000 years. In this case annual varves were present, and they indicated long-term changes at 23,000 and 50,000 years (52:108). Some

scientists have attempted to explain these cycles as the result of the earth's astronomical precession, but, in view of the above-mentioned irregularities, the phenomena seem better explained in terms of crust displacements.

Naturally, such frequent changes in climate have had profound effects on the formation of sedimentary rocks, the chief consequence, perhaps, being the thinness of the individual strata. Very seldom can deposits be found that indicate with any certainty the uninterrupted deposition of more than a few thousand years. On the other hand, innumerable cases of conditions interrupted after a few thousand years can be proved. In addition to the evidence mentioned above, Brooks, for example, refers to a great salt lake or inland sea that existed in Europe in the Permian Period, and says:

> The number of annual layers indicates that the salt lake existed for some 10,000 years, after which the salt deposits were covered by a layer of desert sand (52:25).

Wallace, too, refers to the evidence of sudden changes in climate at short intervals, in his *Island Life:* ". . . the numerous changes in the fossil remains from bed to bed only a few feet and sometimes a few inches apart" (446:204).

Some of the best evidence is provided by coal seams, which are ordinarily thin and interlayered with rock indicating very different climatic conditions. There has developed a considerable literature on the rate of coal formation, and some recent experimentation has thrown light upon it.

Croll devoted considerable attention to the problem. He estimated that it would take about 5,000 years for the formation of one yard (or about a meter) of coal (91:429), and came to the conclusion that the periods of coal formation between changes in climate were about 10,000 years long. It is obvious that any changes that replaced conditions required for coal formation by conditions suitable for the formation of sedimentary deposits beneath the sea (for Croll points out that rock strata between the coal strata are usually of marine origin) (91:424) were indeed radical changes, taking place in

short periods of time. Another writer, Otto Stutzer, after very careful calculation, concluded that a Pittsburgh coal bed seven feet thick could have been formed in no more than 2,100 years (407).

In view of all this evidence, we must not be too much impressed by the very thick layers of rock that are occasionally found. Croll, who was a sound geologist, even if his theory about ice ages was not accepted, pointed out that

> . . . The thickness of a deposit will depend upon a great many circumstances, such as whether the deposition took place near to land or far away in the deep recesses of the ocean, whether it occurred at the mouth of a great river or along the sea-shore, or at a time when the sea-bottom was rising, subsiding or remaining stationary. Stratified formations 10,000 feet in thickness, for example, may under some conditions, have been formed in as many years, while under other conditions it may have required as many centuries (91:338).

It is worth noting that at a number of points the evidence for great and frequent changes in the earth's climatic conditions is linked with evidence of structural changes in the earth's crust, that is, with changes in the elevation of lands, and in the distribution of land and sea. Croll remarked:

> . . . It is worthy of notice that the stratified beds between the coal seams are of marine and not of lacustrine origin. . . . If, for example, there are six coal seams, one above another, this proves that the land must have been at least six times below and six times above sea-level (91:424).

Coleman has emphasized the frequent association of abrupt breaks in the continuity of the strata with extreme changes of elevation above or below sea level. In discussing the Permo-Carboniferous period in India, he says:

> There are the usual cold climate fern leaves in these beds, and above them, without an apparent break, come the Productus limestones with marine fossils (87:102).

Now, it seems altogether reasonable to suppose that if changes of climate were associated with changes of elevation in these different kinds of cases, then the two may have oc-

curred at the same tempo, and have proceeded from the same cause. The hypothesis of periodical shifts of the earth's crust provides both the link and the cause.

An interesting study of repeating geological cycles in a very remote period has been completed by Weller (451). He deals with the so-called "Pennsylvanian" period several hundred million years ago, which had a span of between 35 and 50 million years. He points out that in the study of this period numerous examples have been observed of the deposition of different kinds of sedimentary beds in the same order, at irregular intervals of time. The changes in the composition of the beds imply changes both in climate and in the elevation of the areas above sea level. The cycles are not just local, but can be traced over wide areas (451:110). Furthermore, each complete cycle represents an advance, retreat, and readvance of the sea. Weller accounts for the cycles by diastrophism—that is, by some sort of upheaval in the earth, some activity within the earth's body—but is not able to specify its nature. He recognizes about 42 cycles during the period, with each cycle having a duration of about 400,000 years.

These cycles would appear at first glance to be considerably longer than those that might result from crust displacements. However, there are a number of factors that tend to lessen the apparent difference between them. First, Weller points out that discontinuities in the deposits he is discussing are far more numerous than is generally supposed (451:99–101). This means that a part of the record is missing. Then, we must remember that a complete cycle, involving the retreat and the advance of the sea (probably in a number of stages), would call for several, perhaps quite a few, movements of the crust. At any one point on the earth's surface, several movements might be required to bring the sea level to its lowest point, and several more to bring it to its highest point. We have already discussed this question (Chapter IV). Moreover, Weller points out that in each of his cycles deposition has been interrupted two different times, thus reducing

the average length of the subdivisions of the cycle to periods of the order of 75,000 to 250,000 years. But it must be remembered that we have only averages; the cycles differ greatly and their subdivisions also differ greatly in length. When we consider the fact that the intervals and directions of crust displacements are necessarily irregular, there appears to be a very good agreement between our theory and the facts of the Pennsylvanian cycles. At least, it will hardly be denied that the theory offers the first possibility of understanding the cycles. Moreover, if our recent experience of the shortening of our estimates of geological time in the Pleistocene is a valid basis for extrapolating to earlier periods, it may well be that geologists have exaggerated the length of the Pennsylvanian Period, and that Weller has consequently attributed too great an average duration to his cycles. It appears, therefore, that crust displacements may have been occurring through the whole of one of the major subdivisions of the Paleozoic Era.

It is impossible, however, in the present state of the evidence, to say that displacements of the crust have been going on uninterruptedly all through geological history. It may be that there have been times of quiet, when, for one reason or another, great icecaps failed to develop. The important thing at the moment is that investigators should be willing to undertake further inquiry without preconceptions based on outmoded ideas of gradual change. We may note a serious warning against this bias uttered by no less an authority than Sir Charles Lyell, the greatest geologist of the first half of the nineteenth century, and the father of gradualism in geology. In the course of a discussion of some evidence of recent folding of rock strata on the Danish island of Möen, he remarked:

> It is impossible to behold such effects of reiterated earth movements, all of post-Tertiary date, without reflecting that, but for the accidental presence of the stratified drift, all of which might easily, where there has been so much denudation, have been lacking, even if it had once existed, we might have referred the verticality and flexures and faults of the rocks to an ancient period, such as the era

between the chalk with flints and the Maestricht chalk, or to the time of the latter formation, or to the Eocene, or Miocene or older Pliocene eras. . . . Hence we may be permitted to suspect that in some other regions, where we have no such means at our command for testing the exact date of certain movements, the time of their occurrence may be far more modern than we usually suppose (281: 393–94).

And let us also recall the following words of the greatest geologist of the second half of the nineteenth century, Eduard Suess:

The enthusiasm with which the little polyp building up the coral reef, and the raindrop hollowing out the stone, have been contemplated, has, I fear, introduced into the consideration of important questions concerning the history of the earth a certain element of geological quietism—derived from the peaceable commonplaceness of everyday life—an element which by no means contributes to a just conception of those phenomena which have been and still are of the first consequence in fashioning the face of the earth.

The convulsions which have affected certain parts of the earth's crust, with a frequency far greater than was until recently supposed, show clearly enough how one-sided this point of view is. The earthquakes of today are but faint reminiscences of those telluric movements to which the structure of almost every mountain range bears witness. Numerous examples of great mountain chains suggest by their structure the possibility, and even in some cases the probability, of the occasional intervention in the course of great geological eras of processes of episodal disturbances of such indescribable and overwhelming violence, that the imagination refuses to follow the understanding and to complete the picture of which the outlines are furnished by observations of fact (408:I, 17–18).

The great work from which the foregoing statement was taken is entitled *The Face of the Earth.* The prospect that unfolds before us, as we contemplate the possibility that total displacements of the earth's crust have been a feature of geological history since the formation of the crust itself, is nothing less than the discovery of the formative force, of the shaping factor, that has been responsible not only for ice ages, not only for the mountain ranges, but even possibly for the very history of the continents, and for all the fundamental features of the face of the earth.

X : LIFE

In the preceding chapters we have reviewed a mass of evidence that suggests displacements of the earth's crust, at comparatively short intervals, during the earth's history. We shall now see that this assumption throws some light on the process of evolution.

1. The Cause of Evolution

A century ago, in the *Origin of Species,* Darwin suggested natural selection as the mechanism to account for evolution. The combination of the occurrence of natural variations with elimination of the unfit individuals in the competitive struggle for existence helped to explain a process of unending, gradual change in the forms of life. Darwin did not consider that this was the whole answer. He admitted, for example, that he could not explain the numerous instances of the world-wide extinction of many forms of life simultaneously, especially in those cases where, apparently, there were no competitors and no successors to the extinct forms. Biologists today are in agreement that evolution has occurred, but they also feel that the process has not been satisfactorily explained. Thus Dr. Barghoorn, of Harvard, has recently referred to "our limited understanding of the actual *causes* of evolution," while quoting Dr. George Gaylord Simpson, author of the widely read *Meaning of Evolution,* as remarking, ". . . search for *the* cause of evolution has been abandoned" (375:238). There is a tendency at the present time for specialists to recognize a large number of interacting factors that may, together, conceivably account for evolution, though their relative importance is not agreed upon. This situation does not exclude the possibility that the confusion

may, indeed, arise because one factor is still missing: a factor which, when added, will bring the others into proper focus.

2. *The Problem of Time*

While no biologists since Darwin's time have questioned the basic fact of evolution, numerous difficulties have developed with natural selection. In the first place, while Darwin could present evidence of changes produced in varieties of plants and animals by artificial selective breeding, he was not able to show how, even under artificial conditions, such changes could lead to the establishment of new species. Recently, some progress may have been made in solving this problem, but by the end of the nineteenth century, Darwin's explanation of the mechanism of evolution had been largely abandoned. Natural selection had come to be considered, by many biologists, as chiefly a negative factor, capable of eliminating unadapted variations but not of producing new species.

Around the turn of the century the attention of evolutionists was turned to mutation, the sudden change in hereditary characteristics produced by an alteration of the basic genetic factors, genes and chromosomes. One of the early mutationists, Hugo de Vries, believed that a large-scale mutation might produce a new kind of plant or animal in a single step (115:96). Many evolutionists then adopted mutation, and gave up natural selection as the explanation of evolution.

This did not, however, end the controversy. A neo-Darwinian school, clinging to natural selection, raised damaging objections to the theory of evolution through massive mutations. They insisted, for one thing, that different plants or animals differed by a great many minor traits, rather than by a few major ones. This would mean that a great many mutations would be required, and that these mutations would have to take place in the same individual, or in the same line of descent. The fact that mutation is apparently

an entirely accidental process rendered the mathematical chances against the coincidence of many mutations in one individual or in one line of individuals completely overwhelming.

But this was by no means the only difficulty. The antimutationists could argue that since mutations were purely accidental changes in the hereditary factors, and did not occur in response to needs created by the environment, most mutations would be positively harmful, or at least negative, and would have no effect on the adaptation of the organism to its environment. Only a chance mutation now and then could help an organism to survive. Mutationists were unable to show the existence of any principle by which mutations would be adaptive, that is, brought about as a part of an effort of an organism to adapt to the environment. Some recent experiments indicate that such adaptive mutation may occur, perhaps under special and rare conditions, but it still cannot be shown that adaptation by mutation has been an important factor in evolution.

The mutationists did establish, of course, that minor mutations were of frequent occurrence, and might even be induced artificially; therefore, evolutionists accepted them, but they recognized them as just another way of accounting for the occurrence of variations. The law of natural selection would still be required, in order to eliminate the harmful mutations, which would constitute the great majority of all mutations. For a while it seemed that, in this way, the basic question of evolution was answered.

It soon appeared that this was very far from being the case. The acceptance of mutations by the Darwinians as a factor in evolution did not solve the problem. It became clear, as time passed, that a major difficulty remained. Attention was concentrated on the rate at which mutation and natural selection could be effective in changing life forms. Mathematical studies showed that such changes would take place, according to the theory, at rates so slow that even long

geological eras would provide insufficient time for evolution. Professor Dodson wrote:

> In nature, neither mutation nor selection will ordinarily occur alone, and so the two will act simultaneously, perhaps in the same direction, perhaps in opposite directions. . . . Most frequently, selection will work against mutation, as the majority of possible mutations are deleterious. This will result in very slow change, if any. . . . (115:298).

He emphasized:

> It appears that it is extremely difficult for mild selection pressures, unaided by any other factor, to establish a new dominant gene in a species. . . . (115:298).

By "mild selection pressure," Dodson means those conditions of competition between life forms pointed out by Darwin, that is, the competition that goes on at all times. What he suggests here is that some more drastic influence must have operated to produce evolutionary change.

After discussing Haldane's mathematical calculations indicating the astronomical numbers of generations that might be required to change a plant or animal under the influence of mild selection pressures, Dodson quotes Dobzhansky (the leader of the neo-Darwinian school) on their implications:

> . . . The number of generations needed for the change may, however, be so tremendous that the efficiency of selection alone as an evolutionary agent may be open to doubt, and this even if time on a geological scale is involved (115:298).

Thus the problem is clearly posed: it is the problem of time. It is necessary to find some way of explaining how natural selection can have operated at a sufficiently rapid rate to account for evolution. A factor of acceleration is required.

Some writers, when they saw that evolution could not be explained even with the enormous amounts of time available under the current concepts of the lengths of the geological periods, felt compelled to revert to mystical explanations. Writers such as du Noüy (119) concluded that evolution was

totally inconceivable unless its course had been indicated in advance, by the reigning influence of cosmic purpose. For these writers, the end or final purpose of evolution must be the active controlling force of the whole process. The process, at basis, could be understood only as the direct effect and evidence of the will of God.

Another solution was proposed by Richard Goldschmidt, who became the leader of the anti-Darwinians. He renewed the emphasis on major or macromutations. As Dodson puts it:

> . . . Goldschmidt believes that the neo-Darwinian theory places too great a burden upon natural selection, and hence that the work of selection must be shortened by some other process, namely systematic mutation (115:299).

By "systematic mutation" is meant a mutation that changes not merely an individual trait of an organism but a whole complex of traits, that is, that changes a basic principle of the biological system. The great advantage of Goldschmidt's theory is that it may greatly reduce the number of "genes" required to account for the traits of a single individual. Under present concepts of genetics, for example, from 5,000 to 15,000 "genes" may be called for to account for all the traits of the fruit fly, *Drosophila melanogaster,* while as many as 120,000 may be required for man (115:245). The gene itself, of course, since it has never been identified under the microscope, and since its structure and mode of functioning are entirely unknown, must still be classified as a useful scientific assumption, rather than as a verified entity. The present state of gene theory is roughly analogous to the state of atomic theory before the development of subatomic physics. Then the atomic theory was accepted because it worked in practice, but nobody knew what an atom was. Today, we know only that some sort of unit like a gene seems necessary.

The majority of writers on evolution today seem to feel that Goldschmidt's specific arguments for macromutations have been refuted. I can contribute no opinion on this technical question. But, from my point of view, the most signifi-

cant thing about the Goldschmidt theory is that he produced it in an effort to gain time for the process of evolution, to accelerate it, so that the amount of evolutionary change in life forms could be brought into rough agreement with the available amount of geological time. The rejection of his theory, if the rejection is indeed based upon sound considerations, means that another factor must be found to account for the tempo of evolution.

3. Climate and Evolution

Evolutionists, in general, agree that climatic change must have had a powerful influence on evolution. Geologists have, as I have pointed out, found a correspondence between periods of climatic change and changes in the forms of life. It is evident that as long as the general environment remains roughly the same, there can be only gentle selection pressures such as, apparently, are inadequate to account for evolution. With static environmental conditions, forms of life may continue virtually unchanged for tens or hundreds of millions of years. There are any number of organisms living today whose very similar ancestors lived in remote geological periods. To name merely a few, there is the newly discovered coelacanth, a fish whose ancestors, one hundred or more million years ago, looked as he does today; the recently discovered Dawn Redwood, found growing in China, after having been regarded as extinct since its close relatives disappeared in Alaska about 20,000,000 years ago; the sphenodon, a reptile of New Zealand, whose ancestors, very closely resembling himself, were contemporaries of *Tyrannosaurus rex;* horseshoe crabs, whose time span may amount to half a billion years; palm trees, whose age has just been "jumped" another 10,000,000 years (261); sharks; scorpions, and so on. Sanderson has pointed out that "living fossils" are simply too numerous to list (365). We can take it that, if external conditions are stable, or if animals and plants can migrate around

to find the conditions they are used to, they may continue to exist indefinitely.

At the same time, it is equally true that any kind of animal or plant may succumb, in the course of the usual and continuous competition between life forms, and the local or transitory climatic variations that are always occurring. It would distort the picture to forget this fact. Furthermore, recent studies have shown that new varieties of plants and animals can appear within very short periods of time, on the order of a century or less, if they live in conditions of isolation (115:365). But these rapidly produced varieties are not the same, of course, as established species.

A factor which, undeniably, must produce pressure for profound change in the forms of life is major climatic change. Clearly, this will apply what evolutionists call "strong selection pressure." In this case life forms will have but three alternatives: to migrate, to adapt, or to die. Geologists and biologists have never denied the truth of this: Coleman, for example, recognized the importance of the glacial periods in "hastening and intensifying" the process of evolution (87:62). Lull recognizes the importance of basic climatic change, thus:

> . . . For changes of climate react directly upon plant life, and hence both directly and indirectly upon that of animals, while restriction or amplification of habitat and the severance and formation of land-bridges provide the essential isolation, or by the introduction of new forms increase competition, both of which stimulate evolutionary progress (278:84).

The problem has been, until now, that major climatic changes, and concomitant changes in the distribution of land and sea, could not be explained by any acceptable theory. They were inexplicable events in themselves; their coincidence in time was inexplicable. Even more serious, they were assumed to have happened only at such extremely long intervals that the total number of such major climatic "revolutions" was too small to account for more than a very insignificant portion of evolutionary history.

To recapitulate what has already been said, if drastic climatic and geographical change is the most obvious factor to which to look for changes in life forms, then it is to the acceleration of that factor that we must look for the acceleration of evolution. In the previous chapters we have been led again and again by the force of the evidence to the concept of displacements of the earth's crust. There is no reasonable doubt as to the effect that such displacements, at relatively short intervals, would have on the tempo of evolution. They could not fail enormously to accelerate the several aspects of the evolutionary process. Let us now examine some of these special aspects in more detail.

Wright has pointed out that the rate of evolutionary change may have been accelerated at various times through the mass transformation of one kind of plant or animal into another (115:314). This requires that all over the area of distribution of the life form in question there must be strong pressure for change in the same direction. This means that similar new varieties would appear simultaneously and independently in countless localities or that well-adapted new varieties would spread and become established rapidly. Quite obviously this would tend to accelerate evolution.

But how would such mass transformation be brought about? It could only result from profound transformation of the environment. The required change would have to be general and would have to tend in the same direction for a considerable period of time. No short-range fluctuations and, above all, no merely local climatic changes would suffice. A displacement of the crust appears to meet all these requirements. For a period of many thousands of years, some areas, moving toward the equator, would be growing warmer; others, moving toward the poles, would be growing colder. In the areas moving toward the equator (not necessarily reaching the equator, however, or even the tropics) the increase of sunlight would mean more luxuriant life conditions; for many species this might mean increased food supplies and an extended distribution. It would also be likely

to mean increased competition with other forms. Many effects would depend upon whether the displacement carried the area in question into the wet tropics or into the dry horse latitudes, or merely from an arctic into a temperate climate. Meanwhile, of course, in areas displaced poleward, opposite trends would exist; here the forms of life would have to adapt to diminishing light, to increased cold, to decreased food supplies.

What is important is that these changes of climate would apply over great areas of the earth. In one movement of the crust, two opposite quarters of the earth's surface would be moving equatorward while two others were moving poleward. Thus the climatic changes would be in the same direction over very great areas: the entire distribution, perhaps, of many plants and animals. Mass transformation of life forms might therefore be expected to occur; not mass transformations of all life forms, of course, but merely one or two short steps in the mass transformation of one or a few kinds of plants or animals. New varieties might be established in great numbers, during a single movement of the crust; but by this I do not mean to imply that many new "species" would be. The latter may be the end results of a considerable number of displacements of the crust. I hope that the reader will not ask me to define "species." In this book I use the term simply to denote forms of life that are reasonably distinct and relatively permanent.

We must remember that the different areas of the earth's surface would be unequally shifted in a crust displacement. I have explained (Introduction) that the amount of the displacement would depend on whether an area was near to, or distant from, the meridian of displacement. Selection pressures would vary accordingly.

Since we consider displacements to have taken place in short periods of the order of 10 or 20 thousand years, it seems likely that most plants and animals in areas radically displaced by a given movement would be unlikely to succeed in adapting. Some would migrate into areas having climates

similar to their accustomed climates. Some would disappear. Some would develop varieties adapted to changed conditions. Even though there would be no wholesale creation of new plants and animals, the age-long process of change would have received an acceleration.

Another important, generally accepted requirement for evolution, as already suggested, besides climatic change, is geographical isolation to permit the development of new varieties. Geneticists agree that the larger the population of a given sort of plant or animal, the harder it is for a new variety to get established, because crossbreeding tends to destroy the new variety. If, however, populations are cut off from each other, and are reduced in numbers, a new variant has a much better chance to become dominant, and establish itself as a variety in that locality. As already pointed out, crust displacements can account for the alternation of conditions of geographical isolation and intercommunication at the tempo required to account for evolution, because they can account for rapid, recurrent changes of sea level. Let us now visualize the consequences of a displacement of the crust resulting in a subsidence of a continental area displaced equatorwards. Let us suppose a moderate subsidence of a few hundred feet only, over a period of a few thousand years. The result, of course, would be the deep intrusion of the sea into the continent. The sea would invade valleys, cutting off one part of the mainland from another, and creating islands and island groups. Many populations of the same kind of plant or animal would thus be isolated, and left for many thousands of years to develop and establish new variant forms.

Let us suppose many new varieties to have become established in the islands, and in areas of the mainland separated from each other by tongues of the sea. The next requirement of evolution is that these new varieties should be brought into competition and that the best adapted of them should be disseminated into more varied habitats. This might be brought about by a new movement of the crust, such as would displace this area poleward. The area will now be up-

lifted, the sea will withdraw, and the life forms formerly isolated will mingle and enter a phase of competition.

The situation that compels the adaptation of the forms of life to colder, drier climates (poleward displacement) also will adapt the forms of life to higher elevations, to mountain heights. Thus, if we consider all the effects of crust displacement, both toward the equator and toward the poles, we can see that crust displacement constitutes the most powerful engine imaginable for forcing life forms to adapt to all possible habitats.

4. The Distribution of Species

Another important question is the problem of the origin of the present and past distribution of species over the face of the earth. Darwin and Wallace attempted to explain the numerous difficulties in this field, but their explanations have, in general, become less and less satisfactory with the passing years. These are the questions:

a. How did certain species cross wide oceans to become established on different continents?

b. What accounts for the richness of some islands, and the impoverishment of others, with respect to their fauna and flora?

c. How did many kinds of animals and plants get distributed from the north temperate to the south temperate zones, or from one polar zone to another, across the tropics?

d. Why are certain species of fresh-water fish, inhabiting the lakes and rivers of Europe, also found in the lakes and rivers of North America?

Some of the answers to these puzzling questions will already be clear from what has been said about land bridges. Land bridges, or sunken continents, are obviously necessary to explain many of these distributions between continents and between continents and islands. Sunken continents have already been discussed (Chapter V). Here I would like to

discuss the situation that confronts us if we are not allowed to postulate sunken continents or land bridges.

If we cannot find an acceptable mechanism to account for the creation and destruction of land bridges (or sunken continents) we are forced back upon the ingenious "sweepstakes" idea, which has been much overworked, as an explanation of the distribution of species. This idea arose because it was observed that sea birds, or migratory birds, may carry the seeds of plants or the eggs of insects from continent to continent, and that some species manage to cross, by chance, bodies of water on floating objects such as logs or even ice. By conveniently ignoring about nine tenths of the evidence, this idea has been given considerable importance. Even though many species have migrated in this way, the idea is no substitute for land bridges. Nor, it may be added, is one land bridge, at Behring Strait, able to do the work of explaining the infinite number of plant and animal migrations in all climatic zones in all geological periods. Many land bridges are required, and for these an explanation is necessary. The theory presented in this book, however, can explain the creation and destruction of land bridges (and sunken continents), and therefore it can explain the distribution of species across large bodies of water.

The impoverishment of certain island faunas and floras as compared with others may be understood as follows. Some of these islands may have rich faunas and floras because, in recent time, they have had land connections with adjacent continents. This would be true of the Philippines, of Java, of Sumatra, and of numerous other islands in that area, whose former continental connections with either Asia or Australia have been argued for by Wallace (446) and others. It is not a question of showing that the species in these islands came from the continents; it is simply true that there were land connections, and that the species wandered back and forth; we don't know where they originated.

An island like Java can have a rich fauna and flora not only because of having had rather recent connections with the

continent of Asia, but also because it is mountainous. This makes it possible, supposing at some time an equatorward displacement of the island into a warmer latitude, for temperate climate species to ascend into the mountains and so survive. Such variety of climatic conditions, due to differences of altitude of different parts of the island, would favor the preservation of a rich flora and fauna.

Let us contrast with Java the situation of a small island or island group, such as the Bermudas, the Azores, or the Canaries, where, in general, we find the life forms to be impoverished. These islands, often far from the nearest continent, may have been separated from them, of course, for long periods of time. Now let us suppose one of them, say the Azores, to be displaced through about 2,000 miles of latitude in one movement of the crust, in either direction. Where will the indigenous species go? Obviously, there will be no refuge for them; therefore, many of them will succumb. Subsequently, the sea will be an effective barrier to the repopulation of the islands from the mainland.

As to the distribution of life forms across the climatic zones, referred to as "bipolar mirrorism," Darwin proposed an explanation in Chapter 12 of the *Origin of Species* that can no longer be accepted. He supposed, first, that glacial periods alternated in the Northern and Southern Hemispheres. This idea has long since been given up. Then, Darwin reasoned that when there was an ice age in the Northern Hemisphere, the climatic zones would be displaced southward, and the temperate zone species would migrate southward. When that ice age ended, and the climate warmed up, the temperate species that had migrated southward would now ascend into the mountains, where they would survive, in the tropic zone. There are, of course, mountains in the tropics high enough to be snow-capped the year around; on these even arctic plants might exist.

The next step, according to Darwin, would be the onset of an ice age in the Southern Hemisphere. Now the temperature in the southern tropics would fall, and become temper-

ate, and the temperate species would descend from their mountains and migrate across the valleys southward to the south temperate zone. In this way the migration of the species from the northern to the southern temperate zone would be accomplished.

Now this idea of the species clambering up and down the mountainsides in response to the changing weather is a good one, and gives us one key to the problem. Where Darwin went wrong was in his alternating ice age theory; he could hardly be blamed, in view of the prevailing ignorance about ice ages. Darwin, of course, lived at a time when people were first getting used to the idea of ice ages. But if Darwin was wrong, if ice ages do not regularly alternate in the Northern and Southern Hemispheres, how do we explain bipolar mirrorism? For some decades now, glaciologists have been holding grimly to the theory that ice ages were always simultaneous in the two hemispheres. In maintaining this view, they have ignored the fact that they have made mincemeat of Darwin's explanation of bipolar mirrorism. But this does not concern them. They are concerned with explaining ice ages, not with the distribution of species. They have suggested no alternative explanation for the migration of species across the climatic zones. Instead, they have constructed a theory that puts the migration of species, and even the survival of tropical species, into the realm of sheer impossibility.

They insist, we remember, that the temperature of the whole earth was simultaneously lowered in glacial periods. We have seen that at various times in the past great continental icecaps have existed at sea level within the tropics, and even on the equator itself. I have already pointed out that if the world temperature had been lowered enough to permit a continental icecap in the Congo, there would have been no place of refuge for tropical species of plants and animals. Nowhere along the circle of the equator around the earth would any tropical species have survived. This would be equally true of land and sea forms of life.

Bipolar mirrorism, however, presents no problems if we

reconsider it in terms of displacements of the crust. One movement, let us suppose, takes the Rocky Mountains 2,000 miles to the south. The species climb higher. Later, in a series of movements of the crust (not always, of course, in the same direction), the Rockies finally end up south of the equator, in a temperate climate. Now the species climb down, and occupy the temperate valleys of the Southern Hemisphere. The mountain chain has functioned as a ferryboat, simply transporting species back and forth.

At this point it is interesting to reflect on how useful it is to have these high mountain ranges. A low mountain range would never do. It could never ferry a load of species across the tropical zone.

5. *The Periods of Revolutionary Change in Life Forms*

The reader may have gained the impression that, while certain aspects of evolution have escaped satisfactory explanation, at least the process itself has continued evenly through all time. To this reader it may come as a shock, as it did to me, to learn that this is not at all the case. There have been remarkable variations in the rate of evolution. For long periods it has marked time, and then some force, hitherto unidentified, has initiated a phase of rapid change, a revolution changing so many forms of plant and animal life as to alter the general complexion of life on the earth. All paleontologists appear to agree on this point. Dr. Simpson uses the term "Virenzperiod" to define the periods of rapid change. Others refer to "explosive" phases of evolution or to "quantum evolution." It must be understood that development during these periods is rapid only relatively; new forms are still not created overnight.

One phenomenon that frequently occurs during these periods is termed "adaptive radiation." This is a kind of explosion in which one form (or species) rapidly gives rise to dozens, scores, or even hundreds of new forms apparently at

one and the same time. How is this phenomenon accounted for?

We must distinguish between the biological process and the circumstances that cause it to occur. The process is easily explained. Let us suppose that a form of plant or animal is widely spread over a considerable area. Its total population may include some millions of individuals; over its whole distribution there will be local variations in the environment, and consequently there will be selection pressures operating simultaneously but in different directions on different parts of the population in different habitats. New varieties of the plant or animal will tend to appear to take advantage of special opportunities offered by particular local environments. This sort of thing is always going on, but it does not, by itself, produce explosions of adaptive radiation.

Something more is required. Normally, a new variety of any form has to compete with other forms already in possession of the necessary supplies of food, light, and water. The situation that has the particular combination of these things required by a given plant or animal is referred to as its life, or ecological, niche. Naturally, if this niche is already effectively occupied the spread of the new variety is restricted. As an analogy, think of a garden in which you have set out one hundred expensive strawberry plants of a totally new variety, just before being called away for two months on urgent business requiring your presence in a foreign country. What now happens? Weeds immediately take over the niche you had hoped to preserve (artificially) for the spread of the strawberry plants. Their spread is restricted, and their survival may be threatened.

In nature what seems to be required to permit the very rapid dissemination of many new variant forms of the original plant or animal is an absence of competition. Empty life niches are required. The question is, How is an empty life niche produced? Occasionally, of course, it may have been there from the beginning; it may never have been occupied because, presumably, there never was any form of life that

could utilize it, but after two billion or more years of evolution, such primeval biological vacuums are few indeed. Life niches have, in general, been very well occupied for a very long time. Something is required, therefore, to empty them.

This is where our theory comes in. The effects of a displacement can be visualized in two stages. In the first, a movement of a large continental area through many degrees of latitude might well cause a very general extermination of the inhabitants. We have seen how, in several instances, this occurred during the late Pleistocene (Chapter VIII). The consequence of the extermination of many kinds of plants and animals (which is not to say their extinction, for many of them might survive in other areas) would be to leave their life niches empty.

The second stage, initiated by a new movement of the crust, would be marked by the opening up of avenues for the immigration of life forms from other land areas. Life forms entering the continent would now enjoy a field day. They would multiply; they would occupy rapidly a tremendous area and all manner of habitats; they would produce variant forms, and the variant forms would occupy appropriate niches. Thus explosive evolution would take place. The new forms need not always be immigrants; they could equally well be local survivors of the period of depopulation, of the displacement, who had somehow managed to hold their own under unfavorable conditions. It seems highly probable, indeed, that displacements of the earth's crust are the explanation of explosive evolution.

We have already made mention of the fact that an interrelationship between the revolutionary periods in evolution and the critical phases of change in other geological areas has been noted by many observers. Lull, for example, says,

> . . . There are times of quickening, the expression points of evolution, which are almost invariably coincident with some great geological change, and the correspondence is so exact and so frequent that the laws of chance may not be invoked as an explanation (278:687).

Umbgrove mentions two specific examples of this phenomenon:

> The most important point of all, as far as we are concerned, is that the two major periods of strong differentiation of plant life correspond with two major periods of mountain-building and glaciation of the Upper Paleozoic and Pleistocene (429:292).

The same thing is described by Professor Erling Dorf, of Princeton (349:575–91). We need not take too seriously the small number of turning points mentioned by them for the reason that everything, after all, is relative. The turning points mentioned by Umbgrove might turn out to have been, in some respects, the most important turning points in the history of life, and yet there may have been a hundred lesser, but still very important, turning points.

Geologists who have sought an explanation of the relationship between biological and geological change have, in some cases, favored the idea that geological change, such as the formation of new mountain ranges, might have caused both ice ages and biological change. We have seen that this will not account for ice ages. We have also seen that geologists now generally admit their failure to explain mountain building. It is unsatisfactory to attempt to explain the known by the unknown; it will not do to drag in mountain building as the cause of evolution, when the former also is unexplained.

Displacements of the earth's crust appear to be the connecting link between these different processes: they explain, at one and the same time, ice ages, mountain formation, and the significant turning points of evolution.

6. *The Extinction of Species*

It has already been shown (Chapter VIII) that our theory can provide an explanation for the extinction of species. Some further discussion of this problem is, however, required.

It has been suggested that the history of any particular

species can be compared with the life of an individual, with its phases of youth, maturity, and old age. Thus, the explosive period is the youth of a species, the period of quiet and prosperous enjoyment of its life niche is maturity, and its degenerative phase is its old age. Finally, extinction results from the exhaustion of the vital force of the species. This theory assumes an innate cause, and a natural order for the succession of the phases.

This idea has been widely disseminated, and in one form or another it has served to confuse all the issues and obscure the known facts. It is one more of those philosophical abstractions that people resort to who come up against an unsolved problem and cannot stand the psychological tension of persevering in the search for truth. It is important that the essentials of this matter should be made clear.

In the first place, the idea that a species is analogous to an individual, and must go through similar phases, is a modern revival of the Scholastic logic of the Middle Ages, like the microcosm-macrocosm analogy (according to which some people have recently argued that since planets are satellites of the sun, and electrons are satellites of the nucleus of the atom, then planets are exactly like electrons, and must obey the same laws of physics). The alleged vital force, which is supposed to set a preordained limit to the life of a species, completely escapes scientific observation and experiment. It is not only a mere assumption, it is also an unjustified assumption.

The facts of paleontology do not agree with the analogy of the life phases of a species with that of an individual. In very many cases the same phase may be repeated several times in the life of a species, and other phases may be omitted altogether, as we shall see below. For this reason the theory brings caustic comment from Dr. Simpson. After discussing the two phases of adaptive radiation (youth) and "intrazonal adaptation" (establishment in a stable but limited environment), which is analogous to maturity—which often do follow each other in this order—he explains their relationship thus:

> The sequence radiation-intrazonal evolution is usual, simply because radiation does not occur unless there are diverse zones within which evolution will follow. Occasionally, nevertheless, something happens to close the zones so soon that radiation is curtailed, or the intrazonal phase is even shorter than the radiation. The camariate crinoids, for instance, seem to have been in the full swing of a radiation when they all became extinct in the Carboniferous. . . . (390: 232).

We note that Dr. Simpson says, "something happens." What happens? He does not care to suggest what might happen to close the zones, to curtail the radiation, to destroy the species. No one has ever suggested a reasonable explanation of these things, but they can be understood as effects of repeated displacements of the crust.

Dr. Simpson has remarked elsewhere that he does not object to the analogy of the species and the individual, provided it may be allowed that youth may follow maturity, and may occur more than once!

Not only may phases occur in the wrong order, and be repeated, but also some may be omitted altogether. This seems particularly true of the last, or so-called senile, period. No concept has had so wide a currency with so little support in evidence as that of the alleged degeneration of species. The reasoning behind it is essentially specious: if a form of life becomes extinct, and if some "exaggerated" trait can be pointed to, which might have produced this extinction, then it is claimed that the species was degenerate. This is, of course, merely hindsight, because it ignores the fact that some of the oddest creatures in the world have lasted for millions of years and still exist. It is true that some kinds of plants and animals become adapted to very narrowly specialized environments, so that an almost imperceptible change in the environment may destroy them. These forms may, if you like, be called overspecialized, but they cannot be called degenerate. No inner process of decay has taken place in the organism. Its extinction results from the external circumstance that destroys its relationship with its environment. Is the specialist, who has spent his entire life in the study of

the pre-Cambrian, and therefore is incapable of making his living in any field outside of geology, or even outside pre-Cambrian geology, degenerate? If he starves to death, is his extinction due to degeneration? The reasoning is analogous.

But, supposing that we allow a phenomenon of degeneration in species, it is still true that most species disappear without showing any indication whatever of a decline of their "vital force." The majority of them are cut off in the vigor of maturity, or in "youth," as in the case of the camariate crinoids. Moreover, there is no rule as to the relative length of the different periods. Dr. Simpson remarks:

> Diversification may be brief or prolonged, and may be of limited scope or may ramify into the most extraordinarily varied zones covering a breadth of total adaptation that would have been totally unpredictable and incredible if we were aware only of the beginning of the process (390:222–23).

Again, he says,

> . . . Episodes of proliferation may come early, middle or late in the history of a group. This confirms the conclusion that adaptive radiation is episodic but not cyclic (390:235).

We have already noted that Darwin recognized that the ordinary competition of species could not account for the mass extinction of whole groups, of which, even then, there were many known instances in the fossil record. Since his day, paleontologists have found very many more cases of apparently well-adapted species, which in some cases had flourished for tens of millions of years and yet suddenly disappeared, sometimes leaving their life niches empty, and at other times giving way to inferior species as their successors. For the Pleistocene alone, the last million years, as we have seen, the examples of this include the mammoth, the mastodon, the sabertooth cat, the giant beaver, the giant sloth, the giant bison, and countless extinct varieties of still existing forms like horses, deer, camels, peccaries, armadillos, wolves, bears, etc. Dr. Simpson, in discussing the extinction of the dinosaurs, remarks:

> It should be emphasized that these mass extinctions are not instantaneous, or even brief, events. They extend over periods of tens of millions of years. . . . This makes the phenomenon all the more mysterious, because we have to think of environmental changes that not only affected a great many different groups in different environments, but also did so very slowly and very persistently. The only general and true statement that can now be made about, say, the extinction of the dinosaurs is that they all lost adaptation in the course of some long environmental change the nature of which is entirely unknown (390:302).

If the dinosaurs lost adaptation, it was not because they changed. The same is true of the sabertooth cat, which had a life span of 40,000,000 years and, according to Simpson, was apparently as well adapted at the end of that period as at the beginning (392:43–44). The gradual elimination of the dinosaurs can be understood as the result of constant shiftings of the earth's crust, which eliminated these reptiles first in one area and then in another. No doubt, dinosaurs repeatedly reoccupied areas from which they had previously been eliminated, but eventually—perhaps much more recently than some people think—they were destroyed. Being cold-blooded creatures, of course, they would find it quite intolerable to be shifted into the cold zones, but there is not the slightest reason to think they were degenerate. Simpson attacks the entire idea of degeneration of species (392:72, 81). He quotes Rensch:

> In innumerable cases lineages become extinct without there being recognizable in the last forms any sort of morphological or pathological degenerative phenomena (390:292).

Professor Dodson gives a good example of the piecemeal extinction of species. He cites the case of the mastodonts, relatives of the elephants, which became extinct first in the old world and then in the new (115:371). Other examples could be cited from the Pleistocene, when many species became extinct in the Americas, while their close relatives, such as horses, camels, and various kinds of elephants, survived in the Eastern Hemisphere. Now one might ask the question,

If a species becomes extinct on one continent but continues to flourish on another, is it or is it not senile? What stage is it in then? We can understand all these events as the results of piecemeal destructions of animal populations in crust displacements. We can see in them the process of the creation of empty environments, preparing the way for a new stage of explosive evolution. Simpson directly suggests the connection between these two things:

> . . . Opportunity may come as an inheritance from the dead, the extinct, who bequeath adaptive zones free from competitors. Jurassic Virenz for ammonites followed extinction of all but one family, perhaps all but one genus, of Triassic ammonites; early Tertiary mammalian Virenz followed mysterious decimation of the Cretaceous reptiles. . . . (392:73).

There is another question regarding the extinction of species that should be answered. Perhaps it will be asked, If crust displacements killed off the dinosaurs, why did they not eliminate also the very numerous other reptiles that still survive? If the last displacement at the close of the Pleistocene eliminated the mammoth and certain other mammals from America, why did other animals survive? The answer is, essentially, that it is a question of the mathematical chances of survival. It is a question of the numbers of the animals, the geographical extent and variety of their habitats, their particular individual aptitudes, and the ever-present factor of sheer accident. It may be true that size militated against some species, but it may have worked in favor of others. The very largest animal of all—the whale—still survives. Elephants compare favorably with all but the very largest extinct mammals.

7. *The Gaps in the Fossil Record*

One further point remains for our consideration. A feature of the fossil record that greatly impressed Darwin was the curious way in which species appear, full-blown, with no

indication of transition forms, much like the mythical birth of Venus. The paleontologist suddenly comes upon a species, or a whole group of them, which have not been found before. They are all fully evolved; they obviously have had long histories; there must have been hundreds or even thousands of ancestral forms for them; but absolutely no trace of the preceding forms can be found. It happened this way with the dinosaurs, which appeared in Africa, with a great many species already fully developed, at the beginning of the Mesozoic Era. They seem to have come, literally, out of nothing. Sometimes ancestral forms of a particular plant or animal will be found at a great distance—on another continent, perhaps—but always there appear to have existed many intermediary links, which have been lost. Even in the case of the horse, where an unusually good record exists, there are many missing links.

A part of the reason for this situation is, of course, the imperfect preservation of the fossil record. There appear to be several reasons for this. Fossilization itself is a very rare event; very few individuals of any species are preserved, and the great majority of all the species that have existed have disappeared without a trace. Then, of the fossils that were preserved in the rocks since the beginning of the sedimentary record, about 95 per cent have been destroyed, since about that percentage of all the sedimentary rocks of the older periods has been eroded away and redeposited, with consequent destruction of all fossils. Finally, of the fossils that have been preserved, it is very unlikely that paleontologists can have seen and studied more than a very insignificant proportion—let us say, to put the matter as liberally as possible, that they may have seen one millionth of the existing fossils. Many of the latter, of course, are buried deep in the earth or under the numerous shallow seas, and will never be seen.

But true as this is, it does not quite satisfy. Relatively few and scattered as fossils may be, it is still to be wondered at that we do not have a respectable handful of reasonably complete life histories. The light cast on this matter by the theory

of crust displacement is quite startling. We have seen that such movements would necessitate frequent migrations of whole faunas and floras. It would necessarily follow, from the theory of crust displacement, that species would as a rule be separated by considerable geographical distances from the places of their origin. This would be all the more certain since the rate of development of new forms is probably very slow as compared with the rate at which crust displacements may occur. It could, actually, be rather seldom that one plant or animal would complete much of its life history in the same place. The "missing links" would usually have been separated by great geographical distances from the homes of their descendants. Moreover, the successive movements of the crust, with the resulting changes in the distribution of land and sea, would leave much of the fossil record under the present shallow (or even deep) seas, and out of our reach.

8. Summary

To sum up: it would seem that in crust displacements we have the missing factor that can bring all the other evolutionary factors into proper focus and correct perspective. By crust displacements we may accelerate the tempo of natural selection, provide the conditions of isolation and competition required for change in life forms, and account for periods of revolutionary change, for the distribution of species across oceans and climatic zones, and for the extinction of species. We may also account for the significant association of turning points in evolution with geological and climatic changes, presenting them as different results of the same cause. But for crust displacements to have had these effects, and if they are, indeed, to account for the evolution of species, they must have occurred very often throughout the history of the earth.

XI : CAMPBELL'S MECHANISM OF DISPLACEMENT

1. The Logic of the Evidence

Readers of this volume may have reached the conclusion that displacements of the earth's crust have occurred, perhaps frequently, and very recently in the earth's history, and yet they may doubt that the icecaps caused the displacements. It may seem to them that other causes may have brought about these effects, or perhaps that a combination of causes has done so.

There are several reasons for concluding that the centrifugal effects of asymmetrically placed icecaps were, in fact, the cause of the displacements. The first of these is that continental icecaps are the most massive and the most rapidly developed dislocations of mass known to have occurred at any time on the earth's surface. All other known geological processes subject to measurement, such as erosion and volcanic activity, are inadequate in tempo or in quantity to produce equal centrifugal effects. No dislocation of mass within the earth, known or conjectured, can compare quantitatively in equal periods of time with the effect of an icecap of the magnitude of the present icecap in Antarctica. Every theory so far advanced to account for changes at the surface of the earth (such as displacements of the crust) by changes in depth have postulated long periods for the completion of the changes. The geological evidence presented in this book can be understood only in terms of displacements of the crust at very short intervals during at least a large part of the earth's history, with a most recent displacement through no less than 2,000 miles of latitude in a period of about 10,000 years at the end of the North American ice age.

All this evidence calls for a large displacing force that

will overcome the inertia of the crust and its friction with the layer below it, to continue the displacements to distances of the order of several thousand miles. It is essential to have a force that will not be absorbed and exhausted by the work of moving the crust. It is clear that the mechanism of centrifugal effect postulated by Campbell can meet this requirement because the effect increases in proportion as the uncompensated mass of the icecap is moved farther from the axis of rotation. At the same time, as I have already mentioned, a cause of displacement is called for that will cease to exert these centrifugal effects at some distance from the pole, but long before the equator is reached. To accomplish this, the mechanism must provide that the mass responsible for the displacement must itself disappear en route, and, as we have seen, it must disappear rapidly. It seems that the melting of the icecap, as the movement brings it into warmer latitudes, provides not only a sufficient but perhaps the only conceivable method of explaining the facts.

Campbell has made this clear by computing the increase of the centrifugal effect with increasing distance from the axis for the present Antarctic icecap, assuming it to be uncompensated, and assuming its displacement, without melting, as far as the equator. He has shown (Table III, opposite p. 341, and Figure XII, p. 343) that if the icecap should be displaced as far as the 45th parallel of South Latitude, the tangential component of its centrifugal effect would be multiplied about six times. After this point, while the total centrifugal effect operating at right angles to the axis continues to increase until the equator is reached, the tangential component declines, and yet it is clear that the movement, if the uncompensated mass itself remains intact, must continue to the vicinity of the equator itself. The geological evidence already presented shows that this did not occur, but that the movements terminated at points about one third of the distance from the pole to the equator.

In view of the apparently inescapable logic of the geological evidence and of the centrifugal mechanics, we must

TABLE III

The Progression of the Centrifugal Effects as the Icecap Is Displaced toward the Equator

The figures indicate the relative quantities of the total centrifugal effect at right angles to the axis and of the tangential component acting horizontally and tending to displace the crust, for various latitudes, assuming that the icecap would maintain its present estimated weight (without melting) until it reached the equator. The calculations are by James Hunter Campbell.

Latitude	Miles from Axis of Rotation	Linear Velocity *Feet per Second*	Weight of Ice *Short Tons*	Centrifugal Effect *Short Tons*	Centrifugal Effect *Metric Tons*	Tangential Component *Metric Tons*
85°	345	132.47	2.5×10^{16}	7.526×10^{12}	6.827×10^{12}	6.800×10^{12}
65°	1,734	665.81	2.5×10^{16}	3.782×10^{13}	3.431×10^{13}	31.099×10^{12}
45°	2,860	1,098.1	2.5×10^{16}	6.237×10^{13}	5.658×10^{13}	40.006×10^{12}
25°	3,640	1,397.7	2.5×10^{16}	7.940×10^{13}	7.203×10^{13}	30.109×10^{12}
5°	3,985	1,529.4	2.5×10^{16}	8.684×10^{13}	7.878×10^{13}	6.770×10^{12}

examine with some care the mechanism that Campbell proposes as an explanation of displacements.

2. *Calculating the Centrifugal Effect*

I have already mentioned how preliminary calculations were made of the possible centrifugal effect of the Antarctic cap. I have mentioned that the calculation was first made by Buker, and later somewhat revised by Campbell. However, Campbell recognized, early in his examination of the theory, that this effect, since it operated at right angles to the axis of rotation and was not tangential to the surface, would not produce a horizontal movement of the crust, even if the magnitude of the effect was sufficient for the purpose. It would be necessary, he felt, to find the tangential component of the total quantity of the centrifugal effect. He accomplished this by the application of the principle of the parallelogram of forces (Figure XII). However, his use of the

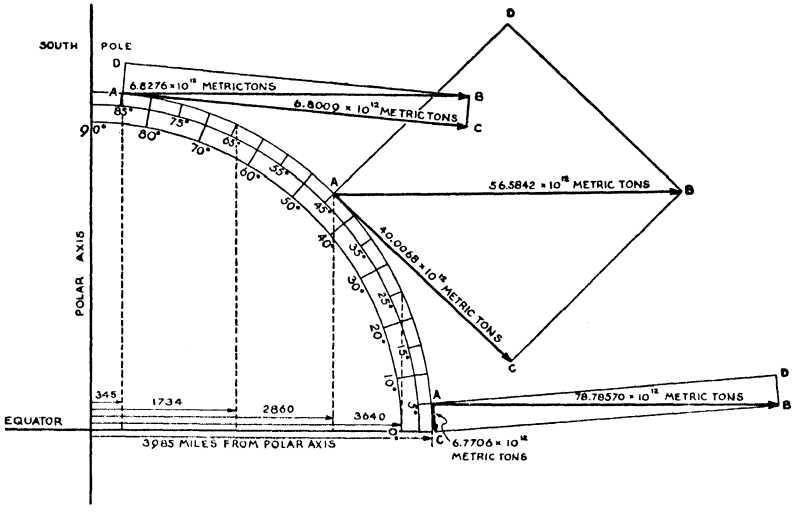

Fig. XII. The Centrifugal Effect of the Icecap. Use of the Parallelogram of Forces to Calculate the Tangential Component

principle is not that usually presented in high school and college textbooks of physics. The definition of the law of the parallelogram of forces is as follows:

> If two forces acting on a point be represented in direction and intensity by the adjacent sides of a parallelogram, their resultant will be represented by that diagonal of the parallelogram which passes through the point (249:489).

In the three parallelograms in Figure XII, the two forces acting on the point *a* are the force of gravity, represented by the line *a–d* (a radial line to the center of the earth), and the tangential component of the centrifugal effect of the icecap, represented by the line *a–c,* while the "resultant" of these forces is the diagonal *a–b,* at right angles to the axis of rotation. The reader will note that Campbell has here inverted the terms of the definition but without changing the quantities of the forces in relationship to each other. The "resultant" in the *definition* is our starting point; it is the estimated total centrifugal effect of the icecap. But it is evident that it is unimportant whether the given quantity is the diagonal or the side of the parallelogram; the parallelogram permits the finding of the unknown quantity from the known quantity, whichever the latter is. The parallelogram therefore permits a finding of the quantity of the tangential component. The rotating effect of this force exerted on the earth's crust is illustrated by the weight shown in Figure XIV.

By definition, the tangential component of the centrifugal effect is exerted horizontally on the earth's crust. Now the question arises as to whether this force will be exerted on the crust itself, or whether it will act on the earth's body as a whole, and thus tend to be dissipated in depth. This is the same as the question whether the icecap will tend to shift the whole planet on its axis or merely to shift the crust. We have already decided that it will tend to shift the crust only, because of the existence of a soft, viscous, and plastic layer under the crust. We may therefore conceive of this force, the tangential component of the total effect, as acting on the

crust alone, while recognizing that the displacement of the crust involves frictional effects with the sublayer. We have seen that a special characteristic of the mechanism under discussion is that it provides a constantly growing force that will overcome this friction, rather than be absorbed by it.

3. *The Wedge Effect*

The problem to be solved by the calculation was to find the quantity of the centrifugal thrust of the icecap in terms of pressure per square inch on the earth's crust, so that this quantity could be compared with the estimated tensile strength of the crust. If these quantities should be found to be of about the same magnitude, it would follow that the icecap had the potentiality of bringing about the fracturing of the crust, which, because of the slightly oblate shape of the earth, was necessary to its displacement over the lower layers. Einstein, in a letter received during an early stage of the investigation, suggested that this necessity for the fracturing of the crust was the only serious hindrance to crust displacements in response to centrifugal effects. He wrote:

> For your theory it is only essential that an excentrically situated mass rising above the mean level of the earth-surface is producing a centrifugal momentum acting on the rigid crust of the earth. The earth-crust would change its position through even a very small centrifugal force if the crust would be of spheric symmetry. The only force that I can see which can prevent such sliding motion of the crust is the ellipsoidic form of the crust (and of the fluid core). This form gives to the crust a certain amount of stability which allows the dislocation of the crust only if the centrifugal momentum has a certain magnitude. The dislocation may then occur and be accompanied by a break of the crust. . . . (128).

Campbell, visualizing the sliding of the crust, perceived that a bursting stress would be caused in the crust when parts of it were displaced toward or across the equatorial region, where the diameter of the earth is greater. It became possible to visualize it in the manner suggested by Campbell in the

lower left-hand drawing of Figure XIV. The drawing shows, in black, two wedge-shaped cross sections representing the earth's equatorial bulge, or that part of it underlying the areas of the crust moving equatorward in the displacement. Half of the bulge is, in fact, involved; the other half underlies the areas of the crust being simultaneously displaced poleward, but these do not affect the point at issue.

The reader will note that the equatorial bulge of the earth is represented in this drawing as lying underneath the crust, so that the crust is not a part of it. This is a new way of visualizing the bulge, introduced by Campbell, and justified by him on the ground that since the earth's crust is of the same general thickness all over the earth regardless of latitude (even though it may be of differing thicknesses from place to

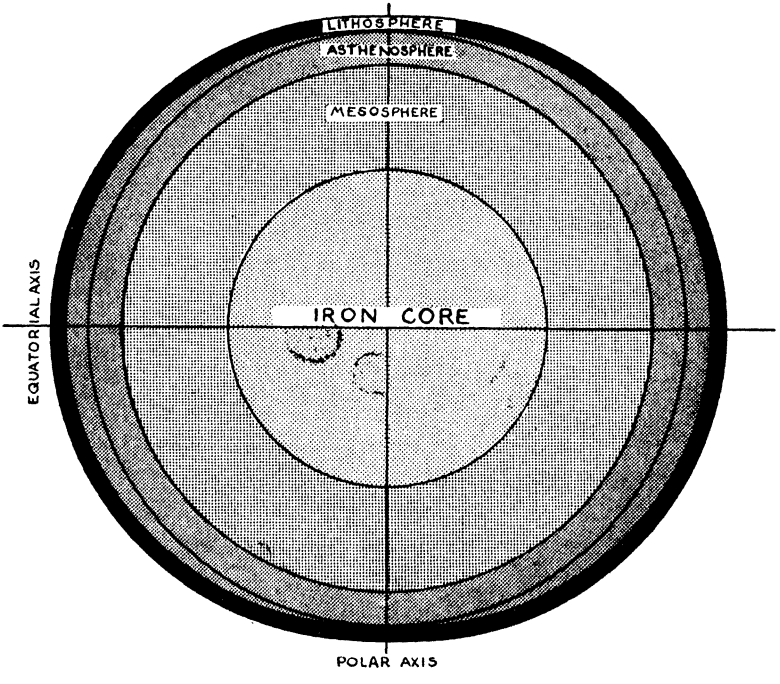

Fig. XIII. A Cross Section of the Earth Showing the Relation Between the Crust and the Equatorial Bulge

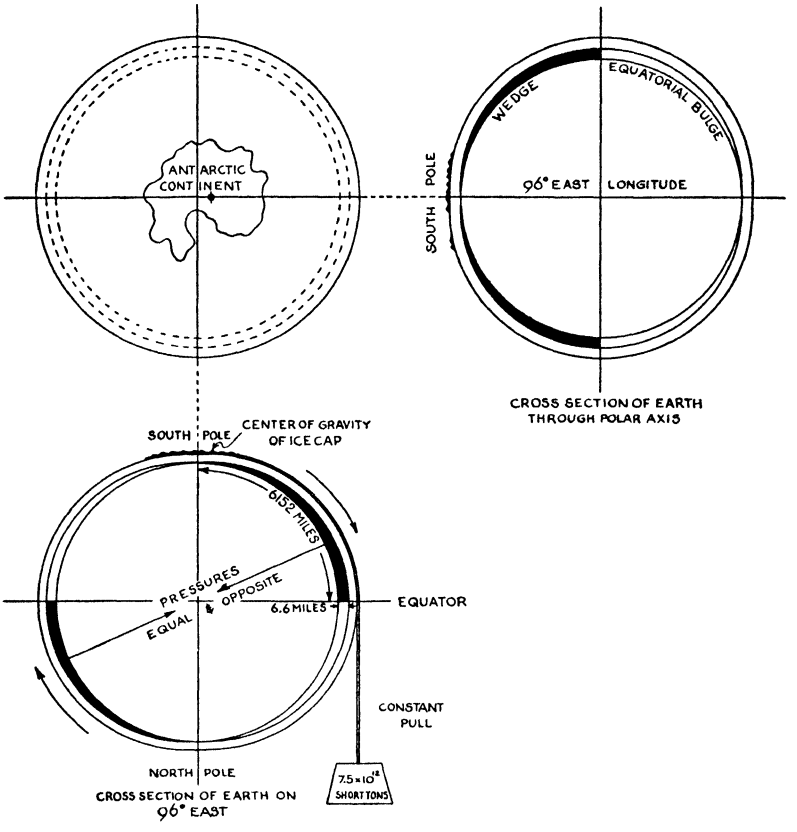

Fig. XIV. Various Aspects of the Wedge Effect

The Antarctic continent is shown, with center of gravity displaced to the right, on a line representing the 96th degree of East Longitude. The figure at the right represents the continuation of the movement of the icecap along this meridian, mounting the bulge, which must be visualized three-dimensionally. The lower left-hand figure shows the vertical cross section of the earth under the icecap, with the two wedges pushing the crust out as it approaches the equator. The proportions of the wedges are shown, and an equilibrium of equal and opposite pressures is indicated. The tangential pull of the icecap is indicated by the suspended weight.

place, and under mountains, continents, and ocean basins), then the differences in the polar and equatorial diameters of

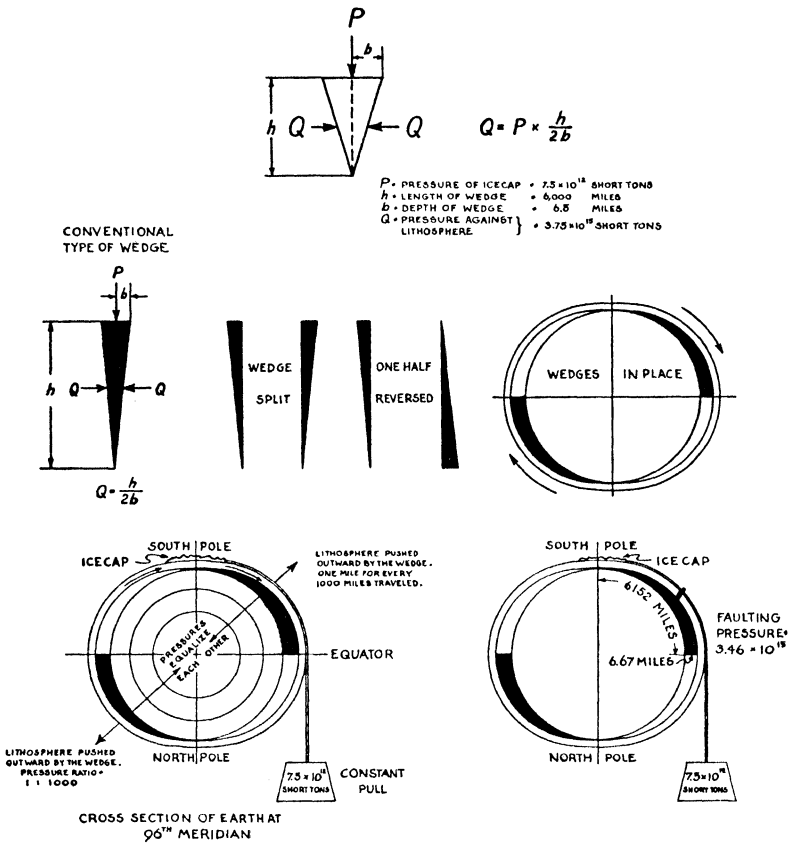

Fig. XV. The Wedge Effect

the earth are accounted for by differences in the thicknesses of the layers underlying the crust, and the bulge itself represents added matter in the subcrustal layers in the equatorial regions.

The bulge, viewed in this way as lying beneath the crust, appeared to form two wedges against which the crust had to be displaced by the centrifugal thrust of the icecap. Campbell reached the conclusion that to estimate the bursting stress produced on the crust in the equatorial region, it would be necessary to apply the mechanical principle of the

wedge, which has the effect of multiplying the effect of an applied force. This principle is usually given as follows:

> The wedge is a pair of inclined planes united by their bases. In the application of pressure to the head or butt end of the wedge, to cause it to penetrate a resisting body, the applied force is to the resistance as the thickness of the wedge is to its length (249:512).

This statement means that a wedge multiplies the splitting power (or bursting stress) produced by an applied force in the proportion of the length of the wedge to its thickness at the butt end. Figure XV shows the application of this principle to the earth. The formula for calculating the wedge effect is presented at the extreme left, where P = pressure (as thrust of the icecap transmitted to the crust), Q = the mutual pressure between crust and bulge, or bursting stress, h = the height (that is, the length) of the wedge, and b = the base or butt end. The bursting stress equals the pressure applied to the butt and multiplied by the ratio of the thickness of the butt end to the length of the wedge. The length of the wedge, in this case, is about 6,000 miles, and its thickness at the butt end is 6.67 miles, so that the ratio is about 1,000:1, and the quantity of the thrust of the icecap should consequently be multiplied by 1,000; however, there are two wedges, one on each side of the earth, and therefore the thrust is multiplied only 500 times. Nevertheless, the significance of such a multiplication of the effect of the icecap is self-evident.

It was not a simple matter to apply the principle of the wedge to the earth. As in the case of the principle of the parallelogram, the formula could not simply be copied from a textbook; it had to be imaginatively applied. For example, in the diagram P, or pressure, is shown exerted on the butt end, like a sledge hammer hitting the butt end of a wedge to split a log. But obviously, the thrust of the icecap is not, in the first instance, applied in this way. It takes an act of the imagination to realize that in effect it amounts to the same thing. The icecap is really pushing or pulling the crust toward the equator on both sides of the earth, but the matter

may just as well be looked at in the opposite way, as if the icecap and crust stood still, or were under no horizontal pressure, but force was being applied to the butt end of the wedge. Either way, the mathematics is the same. Campbell's application of this principle to the problem of estimating the bursting stress on the crust was discussed with physicists, including Frankland, Bridgman, and Einstein (see below), none of whom questioned its soundness.

After finding the quantity of the total stress on the earth's crust produced by the centrifugal effect transmitted from the icecap, Campbell reduced it to pressure per square inch by dividing it into the number of square inches in a cross section of the earth's crust, assuming an average thickness of the crust of about 40 miles. This estimate of the crust's thickness is a liberal one, since some writers, including Umbgrove, suggest that it may be no more than half as much. Since a lesser thickness for the crust would mean a higher figure for the bursting stress per square inch, an error in this direction here may serve to counter the effect of the possible partial isostatic compensation of the icecap, which we have disregarded in the tentative calculation of its centrifugal effect. Thus, if half the icecap is isostatically compensated, but the crust is only 20 miles thick, then Campbell's estimate of the pressure per square inch will be unchanged.

Campbell found that the bursting stress on the crust per square inch amounted to about 1,700 pounds (see p. 361). In comparison with this, I found that the crushing point of basalt at the earth's surface has been estimated, from laboratory experiments, at 2,500 pounds. A number of points must be considered in reaching conclusions regarding the possible significance of these comparative figures. First, the crushing strength of any rock is considered to be higher than its tensile, or breaking strength. Thus, the tensile strength of basalt, the principal constituent of the earth's crust, is probably considerably closer to the estimated quantity of the bursting stress. A second important consideration is that the earth's crust is unequal in thickness and strength from

place to place, and is everywhere penetrated by deep fractures. It would naturally fail at its weakest point. As we shall see below, Einstein, in view of these facts, said that he would be satisfied as to the plausibility of the mechanics of this theory if the ratio of the bursting stress to the strength of the crust was 1:100. The ratio as shown by Campbell is very much closer than the ratio demanded by Einstein. It seems therefore reasonable to suppose that at some point of the future growth of the icecap, which is now, it appears, still growing, the crust may respond to the increasing bursting stress by fracturing. When this occurs, it may be expected that a process will begin of gradual fracturing and folding of the crust, accompanied by the beginning of its displacement over the underlying layers.

Campbell has pointed out that no very great force is required to accomplish a widespread fracturing of the crust during a displacement. At the first local failure of the crust in response to the bursting stress, the stress will be relieved at that point, to become effective immediately at an adjacent point. Thus the fracture will travel through the crust without the application of additional force. From this it is clear that the steady application of a small force would suffice to fracture the crust to a great distance. In his conversation with Einstein, an account of which is given below, Campbell gave a convincing illustration of this principle.

The ability of the crust to resist fracture is slight. Jeffreys found that a strain equal to the weight of a layer of rock 2,200 feet in height would fracture it to its full depth (238:202). It is clear that the tensile strength of the crust does not compare with its crushing strength, which, also according to Jeffreys, is sufficient to enable it to transmit mountain-making stresses to any distance. Campbell visualizes the process of crust displacement not as a continuous movement but as a staged movement resulting from an interaction alternately of the direct thrust of the icecap and of the bursting stress. He writes:

> . . . There are two distinctly separate functions performed by the mass of the icecap. . . . The first is the centrifugal momentum causing the lithosphere to change its position in relation to the poles. . . . When the lithosphere comes to a standstill for want of a sufficient force, the second function of the icecap gets busy and builds up a pressure of tremendous potential, five hundred times the force produced by the icecap, and will continue to add to this pressure at the rate of five hundred times the increasing pressure of the growing icecap, until it finally splits the lithosphere. Then the pressure will drop, and the first function will take hold, and once again start to move the lithosphere. This alternate action will continue to take place until the icecap is destroyed. . . .
>
> . . . The wedge does not multiply the power of the centrifugal momentum, as such. The power disposed for the movement of the lithosphere remains the same as it always has been, but the static pressure that will fracture the lithosphere, thereby permitting the centrifugal momentum of the icecap to start moving the lithosphere, will be multiplied by 500.
>
> The wedge has been functioning ever since the first permanent snow fell on the Antarctic continent; it is functioning today. . . . At the same time, the centrifugal momentum of the icecap is just standing by, waiting for the lithosphere to fracture and be released for the journey toward the equator. The tensile strain will fade away with the faulting of the lithosphere, but should the fractures freeze up again from any cause, the tensile strain will tend to build up again, and the same series of actions will repeat themselves (66).

We saw that the process that destroyed the Wisconsin icecap was several times interrupted and renewed in alternating withdrawals and readvances of the ice, which we have explained as resulting from episodes of massive volcanism. These phases appear to correspond to the alternating phases of the process of displacement as suggested by Campbell, which would naturally have been accompanied by volcanic activity.

4. Some Difficulties

A number of objections have been raised, in the course of consultations with specialists, to the mechanism of displace-

ment suggested by Campbell. We have considered these objections, and they do not appear formidable. I am summarizing some of these in the following pages, in order that the reader may know, if any of these have occurred to him, that they have already been given consideration.

a. The question of friction with the subcrustal layer. It may at first appear that friction would be a powerful brake on any extensive displacement of the crust, and unquestionably it would have an effect. Yet there are several mechanical factors that could aid a displacement. A leading consideration is that the suggested movement is a gliding motion. Gliding is the most economical form of motion. It has been said, in fact, that gliding constitutes an ideal form of motion that utilizes 100 per cent of energy, as opposed to the sphere and the cylinder, which, being round, lose 30 per cent of their energy in rotation, which reduces their speed considerably. Frankland has suggested that a rise of temperature at the interface of the crust and the lower layer, as the result of friction, could facilitate a displacement. Campbell considers that the underlayer, or asthenosphere, would act more like a lubricant than a retardant. He compares the movement to the motion of ice floes: ". . . Observe how vast fields of ice are started in motion just by the friction of the wind on the surface of the ice. . . . Again, you will see the same thing by visiting a pond where they are cutting ice. You will see men pushing around blocks of ice of three or four hundred square feet with the greatest ease as long as the ice is floating in the water. . . ."

b. The question of the extent of the displacement. It has been questioned whether a displacement of the crust might not terminate at an early stage because of the melting of the icecap as it moves into lower latitudes. Campbell, however, has pointed out that as the icecap moves equatorward from a polar region, the ice will continue to accumulate on the rear or poleward side, and that this will have the effect of prolonging the motion of the crust. In some circumstances, if the icecap happened to be situated on a very large land

mass, this might mean a long continuation of the movement. An icecap in Eurasia might move the crust a great distance. It is obvious that a natural point for the termination of the movement will be the arrival of an oceanic area at the pole, so that the rearward build-up of the ice is brought to an end. This would appear to have happened in the last movement, when the southward shift of North America seems to have brought the Arctic Ocean into the polar zone.

c. The question of the possible suspension of movements if both poles should happen to be situated in oceans. This is one of the more important objections that have been raised to Campbell's mechanism of displacement. Yet, it appears that it is much less formidable a difficulty than it seemed at first glance. It might be supposed that the eventuality of having both poles in water areas would be certain to occur; that is, it would have occurred early in the earth's history, and would have stopped crust displacements by putting an end to the formation of great polar icecaps. However, a further examination of this objection reveals weaknesses in it.

In the first place, the very peculiar placing of the continents with respect to the ocean basins renders such an event almost impossible. All the six continents are placed diametrically opposite oceans on the other side of the globe. Ninety-five per cent of all the land on the globe lies opposite water. Moreover, the oceans are surrounded by continental shelves that extend for considerable distances, and there are island areas in the oceans where the water is comparatively shallow. We have seen that, according to Gutenberg, any part of the earth's surface moved poleward by a crust displacement would stand higher relatively to sea level. Any area now near the equator would be raised considerably if moved to a pole, by reason of the variation of gravity alone, while other factors might add to the vertical movement (Chapters IV, V, VI). Displacements could result in raising the continental shelves, shallow seas, and island areas above sea level. The two or three displacements of the crust in the same direction that would be required to move any area from

near the equator to the vicinity of a pole might produce major increases in elevation. Finally, it would be necessary for both poles to be so far away from the nearest land as to prevent the growth of icecaps on any side, for an icecap formed all on one side of a pole and at a considerable distance from it would have a very great centrifugal effect proportionately to its size. Campbell carried out a series of careful measurements of the globe to find out how many possibilities actually exist at the present time for the location of both poles in water. He found that there is only one such position, where one pole would be in the South Atlantic and the other in the North Pacific between the Marshalls and the Carolines. But the latter area, in the course of its displacement from its present latitude to the vicinity of a pole, could easily be raised above sea level.

It cannot be denied, despite this, that there exists a real possibility that at various times during the history of the globe both poles have been, in fact, situated in oceanic areas. Unquestionably, this would have resulted in the temporary cessation of the formation of great polar icecaps, and therefore of displacements of the crust. However, there is no reason to conclude that this would have meant a permanent cessation of crust displacements. We must not forget that Gold has suggested a mechanism by which a shift of 90 degrees in the positions of the poles could occur in periods of a million years. In this way a period without crust displacement could be ended by the gradual movement of a new land area to a pole. It is even quite possible that the accumulation of inequalities of mass within the crust itself, as the result of erosion or of igneous processes, might eventually produce a displacement without the agency of an icecap.

d. The question as to why the postulated centrifugal effect has not prevented the accumulation of the icecap. One commentator has pointed to the well-known fact that ice flows outward in all directions from a central point or points, through the effects of its own weight, and has argued that the centrifugal effect should operate to make the icecap flow off

into the sea rather than to bring about a transfer of centrifugal momentum from the icecap to the crust. This objection has a certain plausibility at first glance, and yet it is invalid for the following reason. We know that according to classical mechanics any mass deposited upon the earth's surface (provided that surface is already in gravitational equilibrium) will be acted upon by the earth's rotation, and will give rise to a centrifugal effect. So much cannot be denied. It is also true that the resulting centrifugal momentum will act upon the ice and can be expected to accelerate to some degree the rate of flow of the ice in the direction of the thrust (not in all directions). In the case of the present Antarctic icecap this might mean a slightly increased rate of flow across the broadest section of the continent, in the direction of the meridian of 96° E. Long. (see the map of Antarctica, Introduction, p. 18). However, the crux of the matter is, obviously, the ratio of the rate of flow to the rate of accumulation of the ice. It is clear, on the one hand, that new ice, until brought within the equilibrium surface of the globe by isostatic adjustment, must give rise to centrifugal effects; and on the other hand it is quite clear that, despite this, the icecap has continued to accumulate. Why does it happen that the centrifugal effect does not produce a flow-off of ice from the continent at a rate equal to the rate of accumulation? The answer to this is, obviously, that ice presents considerable resistance to flow. It flows only under considerable pressure, and is otherwise a solid. Moreover, in Antarctica, and presumably in any polar icecap, the prevailing low temperatures cause the ice to have increased rigidity. It is also thought that the ice below the superficial layers of a great ice sheet is stagnant, and that only the upper layers move. This means that the ice that is in contact with the ground is fixed to the earth's surface. The viscosity, or stiffness, of the ice in turn means that a drag is imparted by the upper layers to the lower layers, and by them to the underlying crust. Thus the centrifugal momentum is transmitted to the crust. Finally, the supposition of the gliding off of the icecap is rendered

more improbable by the uneven topography of the continent and especially by the great fringing mountain ranges. We have cited Einstein's opinion that the flow-off of ice from Antarctica in the form of icebergs is an insignificant percentage of the annual ice accumulation.

e. The question as to whether the centrifugal effect postulated in this theory exists at all. Some commentators have rejected the idea that a large total centrifugal effect may be created by the asymmetric accumulation of an icecap. They have agreed only to the existence of a centrifugal effect that may be created if the center of mass of an object is elevated above the equilibrium surface of the earth. This has been termed the Eötvös effect. It does not depend upon the existence of excess mass at a point. It may be illustrated by an iceberg floating in water. The ice is in gravitational balance, having displaced its weight of water. But one tenth of its weight is above the surface of the water. The center of its mass is therefore higher than the center of the displaced water. If the center of its mass is further from the earth's center, it is now moving around the earth's center at a faster rate. It has been accelerated, and the tangential component of the acceleration will tend to move it toward the equator.

This concept is well known in geophysics, and consequently it has been the customary mode of considering any question involving centrifugal effects on the earth's surface. But the effects that may arise from an accumulation of ice that has not been brought within the equilibrium surface of the earth by isostatic adjustment, and which therefore constitutes an accumulation of excess mass at a point on the surface, have not received equal attention. I have already mentioned the fact that the idea came to both Bridgman and Daly as a new idea, and one that seemed to them worth investigating. Because of the special importance of this issue, I am presenting a more detailed discussion of it below.

f. The question as to whether the crust is strong enough to transmit the centrifugal momentum of the icecap. A num-

ber of consultants have been in doubt on this point because of their knowledge that the crust is, from certain points of view, very weak. It has little tensile strength, and as a consequence cannot bear heavy loads without fracturing or giving way by plastic flow. However, tensile strength and crushing strength are two very different things. A bar made of a brittle but hard substance will have little tensile strength, but considerable force may be required to crush it. The rocks composing the earth's crust are highly rigid, and therefore, despite the fact that they have little tensile strength, such as would be required to contain vertical stresses, they have enormous strength to resist horizontally applied compressive stresses. They simply cannot be compressed to any extent, and only an enormous force will produce plastic flow. The rocks of the crust are so rigid (despite the fact that they do, of course, possess a certain small degree of elasticity) that the penetration of the crust by fractures does not seriously modify its power to transmit horizontal or tangential stresses. There is geological evidence in the mountain systems, in the planetary fracture systems, in the great globe-encircling canyon system recently discovered by Ewing, that stresses have been applied to the earth's crust as a whole, and various geologists, including Hobbs and Umbgrove, have made statements to that effect.

A proper understanding of this question requires that the magnitude of the stress and its mode of application should be considered. An enormous pressure per square inch, especially if applied all at once, might cause local deformation of the crust by bringing about rock flow, as it has been suggested by Bridgman (p. 189), but the stresses that we suppose to derive from the icecap are not of this order, nor do they reach their maximum intensity until after a period of gradual growth during which they could be transmitted to the crust as a whole. The icecap produces a gentle pressure, slowly growing and steadily applied over a considerable length of time, incapable of radically deforming the crust adjacent to

itself, but capable of exerting a persistent push on the earth's crust.

If it were possible to exhaust the centrifugal effect of the icecap by the absorption of energy in a local deformation of the crust, the matter might wear a different aspect. But we are to remember that, if the yielding of the crust or its displacement as a whole allows the displacement of the ice mass farther from the axis of rotation, the centrifugal effect is thereby multiplied. In these circumstances the centrifugal momentum cannot be absorbed locally, but must be transmitted to the entire crustal shell of the earth.

Jeffreys has remarked that the earth's crust can transmit mountain-building stresses to any distance (238:288).

5. *The Calculations*

The following are the calculations of the centrifugal effect of the present Antarctic icecap, and of the resulting bursting stress on the crust, as worked out by Campbell. The phraseology is in part that of Dr. John M. Frankland, of the Federal Bureau of Standards, who was kind enough to review these calculations.

a. Calculation of the Centrifugal Effect of the Rotation of the Antarctic Icecap:

Assume isostatic adjustment 0, center of gravity of the icecap 345 miles from the polar axis, and volume of the ice equal to 6,000,000 cubic miles.

W = Weight of the icecap = 2.500×10^{16} short tons.

F = Centrifugal effect in pounds = $\frac{Wv^2}{gR}$, where

v = Velocity of revolving icecap, 132 feet per second,

R = Distance from the axis of rotation to the center of gravity of the icecap = 345 miles = 1,821,600 feet,

g = Acceleration due to gravity = 32.

$$F = \frac{Wv^2}{gR} = \frac{2.5 \times 10^{16} \times 132.47^2}{32 \times 1{,}821{,}600} = \frac{43{,}870.75 \times 10^{16}}{58{,}291{,}200} =$$

7.5×10^{12} short tons $= 6.8 \times 10^{12}$ metric tons, total centrifugal effect, 6.8×10^{12} metric tons, radial force tangential to the earth's surface, 6.8×10^{2} metric tons (see p. 343).

This, of course, is an upper estimate, which may be too large by a factor of two or three.

b. Calculation of the Bursting Stresses on the Lithosphere:

An approximation of the bursting stress caused by this centrifugal effect can be reached by simple methods as follows. More elaborate approaches hardly seem justified in view of the uncertainty of the magnitude of the centrifugal force.

It is assumed that the entire resistance to the motion of the lithosphere arises from the fact that the earth is not a perfect sphere, but is an oblate spheroid. The tangential, or shearing, stresses between the lithosphere and the underlying asthenosphere are considered negligible because of the time factor, and because of the assumed viscosity of the asthenosphere. If one considers the great circle passing through the center of gravity of the icecap, at right angles to the meridian of centrifugal thrust of the icecap, it is evident that the circumference of this great circle will be increased if the icecap is displaced away from the pole. Of course, any stress system that arises in this way will be two-dimensional, but one will hardly be in error by a factor of more than two, if one neglects the two-dimensional character of the stresses and assumes instead that they are uniaxial. The only purpose of this computation is, of course, to show the order of magnitude of the effect.

With this kind of approximation, one may view the equatorial bulge as a kind of wedge up which the lithosphere is

being pushed. There are, of course, two wedges, one on each side of the globe.

The bursting stress is the product of the tangential effect of the icecap by the ratio of the gradient of the bulge:

(1) Thickness of bulge (wedge) at its butt end = 6.67 miles.

(2) Ratio of travel to lift, of bulge wedge = 6,152: 6.67

(3) Stress, on cross section of the lithosphere (taken as 40 miles thick) $= \frac{7.5 \times 10^{12} \times 6{,}152}{6.67 \times 2}$

$= 3.4588 \times 10^{15}$ short tons.

$= \frac{3.4588 \times 10^{15}}{990{,}894} = 3.5 \times 10^{7}$

short tons per sq. mi.

= Approximately 1,700 lbs. per sq. in.

6. *Notes of a Conference with Einstein*

In January, 1955, Mr. Campbell and I had the privilege of a conference with Einstein at which a number of important questions relating to the theory were discussed. Subsequently, I prepared the following statement, which I submitted to him for his approval. He approved it as an accurate report of our discussion, but he desired that it should not be interpreted as an official endorsement on his part of Mr. Campbell's calculations in detail, which he had had insufficient opportunity to study. Those present at the meeting included Dr. Einstein, Mr. Campbell, Mrs. Mary G. Grand, and myself.

After the introductory remarks, Mr. Hapgood explained to Dr. Einstein that while, in the development of the theory, he had himself been concerned mainly with the field evidence in geology and paleontology, Mr. Campbell had contributed the basic concepts in mechanics and geophysics.

Mr. Hapgood explained further that Mr. Campbell's calculations had now advanced to a point where he felt that a consultation was necessary. The principal question was

whether the tangential portion of the centrifugal effect resulting from the rotation of the icecap was of the correct order of magnitude to cause fracturing of the earth's rigid crust. Dr. Einstein had stated in a letter to Mr. Hapgood that, owing to the oblate shape of the earth, the crust could not be displaced without fracturing and that the tensile strength of the crust, opposing such fracturing, was the only force he could see that could prevent a displacement of the crust. He had already suggested, therefore, that it would be necessary to compare the bursting stresses proceeding from the icecap with the available data on the strengths of the crustal rocks.

It was this problem that now, through the calculations made by Mr. Campbell, seemed to be solved.

Mr. Campbell explained to Dr. Einstein the principles he had followed in making the calculations. He used photostatic drawings as illustrations. He showed that the crust, in attempting to pass over the equatorial bulge of the earth, would be stretched to a slight degree. A bursting stress would arise that would tend to tear the crust apart. This stress would in all probability exceed the elastic limit of the crustal rocks: that is, they would tend to yield by fracture, if the stress was great enough. Dr. Einstein said, Yes, but he wondered how an equilibrium of force would be created? Mr. Campbell pointed out that two equal and opposite pressures would arise, since, at the same time, on two opposite sides of the globe, two opposite sectors or quadrants of the crust would be attempting to cross the bulge.

Dr. Einstein agreed that this was reasonable, but raised the question of the behavior of the semiliquid underlayer of the bulge, under pressure from the rigid crust. After some discussion it was agreed that this underlayer, despite its lack of strength, would not be displaced, because of the effect of the centrifugal momentum of the earth.

Mr. Campbell then explained the application of a principle by which the tangential stress proceeding from the icecap was greatly magnified. He considered that the bulge of

the earth, starting with zero thickness at the poles, and approaching 6.67 miles in thickness at the equator, behaved physically as a wedge resisting the movement of the crust. Since the distance from pole to equator is about 6,000 miles, the ratio of this wedge was 1,000:1; but the existence of two wedges on opposite sides of the globe reduced the ratio to 500:1. The icecap's tangential effect, multiplied by 500, and divided by the number of square inches of the cross section of the lithospheric shell at the equator (assuming the crust to be 40 miles thick), produced a bursting stress on that shell of 1,738 pounds per square inch. After examining each step in the argument twice Dr. Einstein had the impression that the principles were right, and that the effects were of the right order of magnitude. He stated that he would be satisfied if the bursting stress and the strength of the crust were in the ratio of not more than 1:100, since the crust varied so greatly in strength from place to place, and would undoubtedly yield at its weakest point.

Mr. Campbell explained an effect he had often observed, which illustrated the process by which the crust might yield to fracture. A common method of splitting a block of granite is to drill two small holes, about six inches apart, near the center of the long axis of the granite, and insert and drive home a wedge in each hole. A bursting stress of sufficient magnitude is brought to bear to split the rock. However, the rock is not split all at once. Enough stress is brought to bear to start a fracture, but the fracture does not take place instantaneously. If the wedges are put in place in the evening, it will be found next morning that the whole rock has been split evenly along a line extending through the two holes. The fracture has slowly migrated through the rock during the night. The force required to split rock in this way is but a fraction of that required to split it all at once. So far as the earth's crust is concerned, what is required is not a force sufficient to split it all at once, but simply a force sufficient to initiate a fracture or fractures, which will then gradually

extend themselves during possibly considerable periods of time.

Mr. Hapgood next described the geological evidence of world-wide fracture systems extending through the crust, and weakening it, and the remarkable similarity of these patterns to those which, theoretically, would result from a movement of the crust. Dr. Einstein expressed the keenest interest in this evidence.

Mr. Hapgood referred to the Hough-Urry findings of the dates of climatic change in Antarctica during the Pleistocene. Dr. Einstein stated that the method of radioactive dating developed by W. D. Urry was sound and reliable. As a result, Dr. Einstein was in full agreement that the data from Antarctica, indicating that that continent enjoyed a temperate climate at a time when a continental icecap lay over much of North America, virtually compel the conclusion that a shift of the earth's entire crust must have taken place.

Dr. Einstein asked Mr. Hapgood what objections geologists had been making to the theory. Mr. Hapgood replied that it was principally a question of the number of such movements. Urry's evidence would imply four such displacements at irregular intervals during the last 50,000 years.[1] Dr. Einstein replied that this seemed to be a large number. However, he said, if the evidence could not be explained in any other way, even this large number would have to be accepted. The gradualistic notions common in geology were, in his opinion, merely a habit of mind, and were not necessarily justified by the empirical data.

At this point the discussion turned to astronomy. Mr. Hapgood did not understand why men who would boggle at the rate of change required by the theory of crustal movements thought nothing of accepting the view that the entire universe had been created in half an hour. Dr. Einstein said that, unfortunately, the evidence seemed to point that way. After

[1] This figure was subsequently revised, in the light of much geological evidence (Chs. VII, VIII, IX), to three displacements in the last 130,000 years.

considerable discussion he added that it was not, however, necessary to take the present state of our knowledge very seriously. Future developments might show us how to reach a different conclusion from the evidence. Much that we regard as knowledge today may someday be regarded as error.

Toward the end of the interview Dr. Einstein indicated a number of points where further research would be desirable. He suggested the need for a gravitational study of the Antarctic continent, and for a study of the rates of crustal adjustment to increasing or decreasing loads of ice. He commented upon the difficulties that confront those who wish to introduce new theories, and quoted Planck's remark that theories change not because anybody gets converted but because those who hold the old theories eventually die off.

7. *Isostasy and Centrifugal Effect*

As I have mentioned, there is a possibility of two points of view regarding the particular centrifugal effect postulated by Campbell and myself. It is therefore necessary to provide additional clarification of some of the points at issue. To a certain extent it may be a question of a situation in which new definitions or clearer definitions of accustomed terms are called for, but it also appears to us that in some cases, at least, physicists whom we have consulted in the course of our work are proceeding upon the basis of assumptions that are in conflict with ours. Therefore, it is necessary to re-examine these assumptions. A comprehensive discussion of the matter must begin with a review of the broader questions of the mechanics of rotation already briefly referred to in the Introduction.

The existence of a very common misunderstanding regarding the mechanics of the earth's rotation, particularly related to the problem of the stability of the poles, was made clear to me by a difference of opinion that arose at the beginning of my inquiry. Brown, whose work was the starting

point of my own, was an engineer, and his concepts of the earth's motions were based upon simple mechanics. He understood gyroscopic action, and the stabilizing role of the rim of a rotating flywheel. He also understood the laws of centrifugal effect as applied to weights eccentric to the axes of spin of rotating bodies. It was my good fortune that Campbell, who was to carry the work forward, also was a mechanical engineer.

Brown had made the statement that it was the equatorial bulge of the globe that stabilized it with reference to the axis of rotation; he had compared it to the rim of a flywheel. I found that this statement was disputed by some physicists. The physicists suggested that the stability of the earth on its axis was not owing to the centrifugal effect of the rotation of the equatorial bulge alone, but to that of the rotation of the entire mass of the earth. Later I discovered a passage in Coleman that appeared to express their point of view:

> It may be suggested that the earth is a gyroscope, and, as such, has a very powerful tendency to keep its axis of rotation pointing continuously in the same direction. Any sudden change in the direction would probably wreck the world completely (87:263).

I wished to obtain a clear statement of the rights of this matter. Accordingly, I corresponded with specialists, who eventually referred me to the works of James Clerk Maxwell, in one of whose papers I found the following statement in support of Brown's position:

> . . . The permanence of latitude essentially depends on the inequality of the earth's axes, for if they had all been equal, any alteration of the crust of the earth would have produced new principal axes, and the axis of rotation would travel round about those axes, altering the latitudes of all places, and yet not in the least altering the position of the axis of rotation among the stars (296:261).

For the word "axes" in the second line we may read "diameters," and of course Maxwell is referring to the inequality of the polar and equatorial diameters, that is, to the existence of the equatorial bulge, to which, therefore, he directly at-

tributes the stability of the earth on its axis of rotation.

Maxwell, in the foregoing passage, suggests that in the absence of the equatorial bulge, any change in the crust (meaning, it is clear, the creation of any protuberance or excess weight at any point) would change the position of the planet on the axis of rotation. Even before I located this passage in Maxwell, a peculiar device designed by Brown had made this principle clear to me by observation. This device consisted of a globe mounted on three trunnions in such a way that it could rotate in any direction. The globe was a perfect sphere and had no equatorial bulge. It was suspended by a string to an overhead point. To rotate this sphere, all that was necessary was to wind it up and then let it go. Brown had a weight attached to the South Pole of the sphere, and it was observable that, as soon as the sphere began to rotate rapidly, the weight was flung to the equator, where it stabilized the direction of rotation as long as the speed of rotation was maintained. Later Campbell made a larger model of Brown's device, which I rotated unweighted, and I observed that it had no stable axis of spin. Two motions were observable: a rapid rotation, and a slow, random drifting. It was evident that the mass of the sphere acted as a stabilizer of the speed of rotation, but had no influence on its direction. This experiment, strongly confirming Brown's claim, encouraged me to persist until I could find positive theoretical confirmation of the observation, which eventually I did in the works of Maxwell.

I was amazed and chagrined in this connection to note a phenomenon which, nevertheless, is as old as science itself. The professors—most of them, at any rate—would not come to see the device.

Perhaps I should describe this device in greater detail. A trunnion is like a ring or a hoop, made of metal. A globe is mounted in this trunnion on two pivots set into the ring at points opposite each other (180 degrees apart). Then, if the ring is held (as it often is on a model globe) by a pediment or stand, the globe will rotate. Its axis will be deter-

mined by the fixed positions of the pivots set into the ring.

Now, if, instead of fixing the ring into a stand, or pediment, we set it into another, larger ring, by inserting two pivots into the larger ring at two points at right angles to those of the inner ring, we have an axis within an axis, and the globe can be made to rotate in either direction. If a third ring is used, then the globe has freedom of action in any direction whatever.

There is still the problem of imparting momentum to this globe. Since it has no fixed axis, this is a difficult problem. Brown solved it by suspending the device to the ceiling by a string attached to the outermost trunnion. This string could be wound up by rotating the outermost trunnion in one direction by hand for a while, just as a boy may wind up the rubber bands used to give momentum to a toy airplane. Then, when the trunnion is released, the string unwinds, putting the globe itself in rapid rotation, but a free rotation, one not confined to a fixed axis.

In view of continued skepticism, I could not be entirely satisfied by the Maxwell statement, supported though it might be by the demonstration. Since I am not myself a physicist, I felt it not unlikely that some persons would conclude that, in the first place, I had misunderstood Maxwell, and that, in the second place, I was incapable of interpreting correctly the evidence of my eyes. I therefore wished to obtain an authoritative interpretation of Maxwell's statement, and, accordingly, I wrote Dr. Harlow Shapley, the Director of the Harvard Observatory, as follows:

> After a year of intensive work with a group of people here, I have concluded that the work we are doing is dependent upon a clear answer to the question as to whether the geographical poles are stabilized by the momentum of rotation of the earth, or solely by that of the equatorial bulge. I have had discussions about this with Dr. Adams of the Coast and Geodetic Survey, and with Dr. Clemence of the Naval Observatory. They have given me references to the work of Clerk Maxwell and others, without quite satisfying me. I am not, of course, equipped to understand all of the technicalities, but I am

hoping that you can give me a steer in nontechnical terms on the general concepts.

My hunch is that, contrary to a widespread impression, it is the bulge alone that stabilizes the geographical poles. As I reason it out, if the earth were a perfect sphere, the energy of its rotation, derived from its mass in motion, would "stabilize" the speed of the rotation, but would have no reference to its direction. If we suppose that somebody could reach out from Mars with a pole, and give the earth a strong push at an angle of 90° from the direction of rotation, the earth would be shifted on the axis of rotation to an extent determined by the ratio of the force of the push to the mass of the earth. In fact, if the earth had no bulge, it would never have stable poles, but would rotate every which way. . . .

If my view of the matter is sound, important consequences follow, but I am not quite certain of the validity of my premises.

Dr. Shapley's reply, dated February 2, 1951, was, in part, as follows:

Dr. [Harold] Jeffreys was fortunately here at the Harvard Observatory and I could turn over your inquiry to him. I now have his reply. He says in effect that the fullest discussion of the points mentioned by you is in Routh's Rigid Dynamics, probably in volume I. Most textbooks of rigid dynamics will have something about it. The theory goes back to Euler. Really *both* the rotation and the equatorial bulge are needed to maintain stability. Without rotation the body could be at rest at any position; with rotation but without the equatorial bulge it could rotate permanently about an axis in any direction. . . . (343).

With this statement I decided to rest content. It seemed to me that Brown's position in the matter was correct. Maxwell showed both by the use of his dynamical top and theoretically what Brown showed by his device: that a rotating sphere tends to throw the heaviest weights on its surface to the equator of spin. Maxwell and, after him, George H. Darwin recognized that the equatorial bulge of the globe stabilized the direction of the earth's rotation just as a weight on the surface of a model sphere would do when the sphere was rotated rapidly.

Yet there is a distinct difference between the earth and the model globe. The earth's approximately round shape is

not due to the fact that it is a strong, rigid body, for it is not. Its roundness is due primarily to the force of gravity, which in fact holds the earth together. The earth as a whole is a very weak body, and if it were not for the effect of gravity the centrifugal effect of the rotation would disrupt the earth and send all its component masses hurtling outwards into interstellar space.

There is also a difference between the equatorial bulge of the earth and a weight attached to the surface of a model globe at its equator of spin. This difference consists in the fact that the earth's equatorial bulge and the flattenings at its poles have been produced by the yielding of the earth's body in response to the centrifugal effect of its rotation. The amount of the yielding has been determined by the ratio of the forces of rotation and gravity. The shape of the earth thus represents a balance of these two forces, a balance that is perfect, theoretically, at every point of the earth's surface. It therefore follows that any unit of material in this balanced surface will be at rest. For this reason, such a surface has been called an equipotential surface.

The balance of the forces of rotation and gravity at every point of the earth's surface can be understood also in this way. The shape of the earth, as we have pointed out, is oblate. This means that as you go toward the equator you are getting farther from the earth's center. In a sense, therefore, you are going uphill. Likewise, when you are going toward the poles you are getting closer to the earth's center and therefore you are going downhill. But we can all see that it takes no more energy to move toward the equator than it does toward a pole. Also, water in the ocean does not run downhill toward the poles. The earth's surface acts as if it were perfectly level. The reason for this is that as you go toward the equator, going uphill, the centrifugal effect of the earth's rotation increases just enough to compensate for the gradient, while, if you move toward the poles, the centrifugal effect declines in proportion. The forces of gravity and rotation are therefore balanced, and no centrifugal ef-

fect will tend to propel a mass in this equipotential surface toward the equator, and no gravitational effect will tend to propel it toward the poles. The fact that the force of gravity is absolutely much greater than the centrifugal effect of the rotation is shown by the fact that the flattening of the earth is very slight. The equatorial bulge amounts to 6.7 miles in comparison with the earth's mean radius of 4,000 miles. This is a ratio of only .017 per cent.

The past century has been notable for extensive studies of the effects of gravity at the earth's surface. The theory of isostasy has been developed, and the actually existing state of balance of the surface features of the earth's crust has been measured in various ways and for various purposes. As we have seen, there are various difficulties with the theory of isostasy, some of which may be soluble in terms of the theory presented in this book. At the same time, but independently, studies of centrifugal effects at the earth's surface have been undertaken. Eötvös investigated the centrifugal effects that would arise if a given mass had its center of gravity above the equipotential surface. This could occur even with masses in isostatic equilibrium. To visualize this case, we may take the example of a block of ice floating in water.

Ice is lighter than water. When a block of ice falls into a body of water it displaces its own weight of water, and then floats with a tenth of its mass above the water level. It is now in equilibrium, or in isostatic adjustment, even though its upper part projects a considerable distance up out of the water. This upper tenth, in the meantime, has displaced air, not water. It is a solid mass of far greater density than the air it has displaced. Its center of gravity, midway between its summit and the water surface, is farther from the axis of rotation of the earth than was that of the mass of water it has displaced. Since points move faster with the earth's rotation the farther they are from this axis, this mass has now been given added velocity. Added velocity means an increase in the centrifugal effect, and one not compensated by gravity, since the amount of mass is the same as before,

and therefore the effect of gravity at that point has not been altered. A tangential component of this added centrifugal momentum will tend to move this ice mass toward the equator.

Eötvös applied this same principle to parts of the earth's crust. We have seen that, according to the theory of isostasy, mountains and continents are elevated above the ocean bottoms because they are composed of lighter materials, and they are considered to be "floating" in an approximate gravitational balance with the heavier crustal formations under the oceans. Eötvös considered the centrifugal effects that might arise from the elevations of the centers of gravity of continental formations above those of the oceanic sectors of the crust, and calculated them mathematically. He found that the effects were comparatively slight. Attempts have been made to account for the drift of continents through these effects, but his calculations show they are too small to have considerable effects. Since Eötvös' time, it has been generally assumed that any centrifugal effects that were to be considered in relationship to the earth's crust must be effects resulting from variations in the vertical position of centers of gravity of masses in gravitational balance, that is, elevations of these centers above the equipotential surface, or depressions of them below it, owing to differences in relative density of the masses involved.

Let us now consider, in connection with this, the effect of departures of given masses from the state of isostatic or gravitational equilibrium. We have already seen that there are remarkable departures from isostatic balance, some resulting from deformities of the crust, and some, it seems, from the accumulation of icecaps. In these irregularities in the distribution of matter, resulting from the limited failure of isostatic adjustment, we must recognize the existence of another surface of the earth, in contradistinction to the equipotential or geoidal surface already mentioned. We may call this surface the gravitational surface, or the surface of equal mass. This is a real surface. It is not, however, the visible

surface. A high plateau may represent an area of deficient mass, and an ocean basin may represent an area of excess mass. We have seen that there are many oceanic areas that show positive isostatic anomalies, or the existence of local excesses of mass in the earth's crust. We can easily see the distinction between the level equipotential surface of the geoid, represented by sea level, and the surface of mass that may deviate considerably from the level surface.

The mechanism for crust displacement presented in this book depends upon recognition of the fact that distortions of mass on the earth's surface, of whatever type, if they constitute anomalous additions of mass at points on the earth's surface, will give rise to centrifugal effects like the effect of the mass attached to the surface of Brown's rotating model sphere, in accordance with ordinary principles of mechanics, and measurable by the standard formula for calculating centrifugal effects.

An example may serve to illustrate the difference between the surface of mass, which differs in elevation from place to place, and the equipotential, geoidal surface. Let us take a fictional case of a mass out of isostatic adjustment but with its center of gravity below the surface of the geoid. Let us suppose that under the bottom of the Atlantic Ocean we have a slab of material ten times as dense as basalt, two thousand miles long, one thousand miles wide, and forty miles thick. The excess of mass in this slab, as compared with other sectors of the crust, would be enormous, and gravity would be greater at the surface. Consequently, the ocean level over this area would be affected slightly, but the shape of the geoid would not be significantly changed, and the sea level would still represent an equipotential surface. The center of gravity of the anomalous mass of high density would be depressed far below sea level; it might be fifteen or twenty miles below the geoidal surface. Now if the slab were of average density, the depression of the center of gravity would mean an inverse Eötvös effect, that is, a *poleward* centrifugal effect, the quantity of which, as we have seen, would be slight. But, now,

to counteract this, the rotation of the earth, acting on this mass of ten times normal density, would produce a centrifugal effect ten times as great as the one normally balanced at that point by the effects of gravity. Let us note the fact that the assumption that this slab is not isostatically compensated involves the consequence that the centrifugal momentum resulting from it is not compensated.

The difference between an Eötvös effect and one produced by an uncompensated mass may be illustrated in another way. Let us return to our example of a mass of ice. Campbell has suggested the example of an iceberg before and after its separation from its parent, land-based icecap. It is assumed that the icecap is uncompensated. The iceberg, breaking off from the icecap, falls into the water. Before this event the icecap, by assumption, is outside the equilibrium surface of the geoid; the rotation of the earth acts upon it precisely as the rotation of Brown's model sphere acts upon the weight fixed to its surface.

But let us see what happens when the iceberg falls into the sea. It now reaches gravitational equilibrium. It sinks, and displaces its weight in water. It is now a part of the equipotential surface of the geoid (though the portion projecting above sea level is not, and therefore exerts an Eötvös effect).

Now what is the quantitative relationship between the Eötvös effect and the original centrifugal effect of the iceberg? It is plain that now nine tenths of the ice is within the equilibrium surface. For this nine tenths of the mass the equatorward centrifugal momentum produced by the earth's rotation is precisely cancelled by the poleward component of the force of gravity at that point, so that there is no net centrifugal effect. Only one tenth of the ice remains to exert an effect, and the quantity of this effect, furthermore, is determined by the elevation of the center of gravity of this tenth of the iceberg above sea level. But the elevation has been enormously reduced. It has, in fact, been reduced to one tenth of the elevation of the center of gravity before the fall of the iceberg into the sea. Campbell has pointed out that,

as a result, the centrifugal momentum not compensated by gravity has now been reduced to one one hundredth of the quantity of the effect of the ice mass when it was totally uncompensated.

It appears, therefore, that the question as to whether a mass is in isostatic adjustment or not is the essence of the matter. The icecap, if totally uncompensated, may produce a centrifugal effect one hundred times the Eötvös effect for the same mass; furthermore, it may be calculated by the formula used by Campbell, with the reservation that a small poleward component of gravity caused by the oblateness of the earth and proportional to the degree of the oblateness must be taken into consideration.

Let us attempt to define and clarify this poleward component of the force of gravity, and to estimate its probable relative magnitude. It applies both to masses in equilibrium but with elevated centers of gravity, and to any mass resting on the earth's surface but uncompensated. Its effect will be greater in the latter case than in the former. In both cases it will tend to counteract the equatorward component of the centrifugal effect of the icecap.

The poleward component of the force of gravity results from the oblateness of the earth. It may be visualized as follows: if you should place a marble at the equator, and if the rotation of the earth should be interrupted so that the earth would be at rest, then the marble would tend to roll toward one of the poles, because the poles are closer to the center of the earth, and therefore downhill. As I have mentioned, this applies both to masses out of isostatic equilibrium, and to those in equilibrium, but with elevated centers of gravity (that is, to masses standing higher because of their lesser average density). However, as I have pointed out, there will be a quantitative difference between the poleward effects of gravity in these two cases of about 100:1.

In both cases these effects would tend to counterbalance the equatorward component of the total centrifugal effect of the icecap. The question is: What proportion of the equator-

ward effect would be thus counterbalanced? This is the crux of the matter.

The answer to this problem may be found in the following consideration. The force with which any object rolls downhill is proportionate not only to its weight but to the gradient of the slope. On a flat surface the marble is at rest. It would develop maximum momentum if it could fall straight down toward the earth's center (if the surface were vertical). Between these extremes of zero and maximum momentum there must be an even curve of increasing momentum with increasing gradient. (It would follow, of course, that a sled would develop twice the momentum if going down a hill twice as steep.)

To apply this principle to the icecap, we may observe that if there were no oblateness to the earth, there would be no poleward component of gravity. If, on the other hand, the oblateness were increased to the point where the icecap could fall straight down, it would develop maximum momentum, the product of its velocity and of its weight. Between these extremes, the poleward momentum would be proportional to the gradient. We have seen, however, that this gradient amounts to only .017 per cent. It follows from this that the poleward component of gravity acting on the icecap will be .017 per cent of the tangential component of the centrifugal effect of the icecap. This of course is a relatively negligible quantity.

It may be objected that in this discussion we have offered no mathematical calculations in support of the positions taken, and that therefore we have no quantitative basis for our theory. This is, however, a misunderstanding. It is essential, before mathematical computations are made, to understand the assumptions on which they are based. In our correspondence we have more than once received communications in which the authors have indirectly or directly stated that the question as to whether a given mass was or was not isostatically compensated was irrelevant. It has seemed to us, on the other hand, that the actual balanced surface or shape of the earth as de-

termined by the balance of gravity and the centrifugal effect of the rotation—that is, the geoid, or the equipotential surfaces —while perfectly valid as an assumption for many calculations, was irrelevant for our problem. We feel it must be conceded that if the conformity of the earth's materials in general to the balance of the two forces of gravity and rotation, so as to create the oblate shape of the earth—the geoid—is important, the failure of some of the materials to conform to this shape is also important. By definition, a mass that is not isostatically compensated fails to conform to this shape. Thus the real surface of mass differs from the geoid, and cannot be called an equipotential surface. We feel that the real "surface of mass" of the earth cannot be disregarded.

In this situation equations are of no use. They will not help us attain clarity. What is needed instead is a re-examination of the assumptions on which equations have been made. This is an intellectual problem of the logical development of ideas, and corresponds to the process advocated by Maxwell as superior, in some situations, to calculations. Discussing the intricacies of the mechanics of rotation before the Royal Society, Maxwell remarked:

> . . . If any further progress is to be made in simplifying and arranging the theory, it must be by the method that Poinsot has repeatedly pointed out as the only one that can lead to a true knowledge of the subject—that of proceeding from one distinct idea to another, instead of trusting to symbols and equations (296:248ff).

Let us remember that the author of this remark was one of the greatest mathematical physicists of all time. As such, he understood the limitations of mathematics, of which the most essential is that all calculations must be based in the last analysis on assumptions that consist of clear ideas, logically expressible in words.

I do not wish to have it seem, however, that I am conceding the point that Mr. Campbell and I have not provided quantitative solutions. On the contrary, I believe that, on the basis of the assumptions discussed above, Mr. Campbell has provided sound and adequate (though approximate) quanti-

tative estimates for the equatorward component of the centrifugal effect of the icecap, and furthermore, that he has indicated the correct order of magnitude of the bursting stresses that may be produced in the crust.

XII : CONCLUSION

1. Looking Forward

It is said that a sound scientific hypothesis should have the character of predictability. "Predictability" is said to apply to a hypothesis if the hypothesis predicts the discovery of new facts that later are actually discovered. An example of predictability of this sort was the discovery some years ago of Pluto as the result of calculations based on the theory of gravitation. Our theory has repeatedly shown that it possesses this sort of predictability. On one occasion Campbell worked out, from purely theoretical considerations, the patterns of crustal fractures that would be formed by a displacement. At the same time, in a different city and entirely independently of him, I was discovering, in the works of W. H. Hobbs, geological evidence showing that fracture patterns of precisely this kind actually existed in the rocks. Only later did we compare results. If I had started with his drawings and used them to guide my research in the field, I would have found approximately the fracture patterns that he predicted, and I would have found them sooner. On another occasion when, in 1951, radiocarbon dates showed the very recent end of the North American ice sheet, I reached the conclusion from the theory that the beginning of that glaciation must have been quite recent, and much more recent than generally believed. At that time this conclusion could not be tested, because the range of the radiocarbon method was not great enough. However, I was aware of the fact that several scientists were working on the problem of extending the range, and I confidently looked forward to a confirmation of the theory when and if the range was extended. I had to wait only until 1954, when Horberg, as already mentioned, published results showing

that the icecap had entered Ohio only 25,000 years ago. Again and again we have had experiences similar to this. Campbell has, in fact, suggested that the theory may have economic importance because of the fact that it may give us a tool through which we may attain more reliable information about the hidden structures of the earth's crust, and thus be able to locate valuable minerals. It seems to me quite possible that his hope will eventually be realized.

Our theory appears to have another kind of predictability. It is possible that it can tell us something about the relatively near future of the earth. The evidence appears to suggest that displacements have occurred at short intervals. Since what has happened in the past may be expected to happen in the future, it is quite reasonable to ask when another movement may be expected. There are a number of factors that bear on this, and they are worth discussing even though, when we get through, we may carry away the feeling that our speculations may contain more imagination than substance.

It would appear from the evidence I have presented that the intervals between the beginnings of the last three displacements were about 40,000 years in length. It seems, also, that the last movement began between 26,000 and 17,000 years ago. If these assumptions are correct, and if the average of these movements holds for the future, it seems that the next displacement of the crust should not be expected for another 10,000 or 15,000 years. While this is a reassuring thought, it should be kept in mind, however, that there are a number of unknown factors in the situation, and that there is no reason to believe that the average of the last three displacements tells us anything about the limits of variation in the periods between displacements. On the contrary, the conclusion to be reached from the investigation completed in this book is that the periods between displacements may vary considerably.

A number of factors favor a movement somewhat sooner than the time indicated by the average of the last three displacements. Among these, I may mention the fact, empha-

sized by Brown, that the present Antarctic icecap is larger than the last North American icecap. If Campbell's calculations are close to the truth, it seems that the bursting stresses in the crust may now be close to the critical point, from this source alone. Yet there is also a possibility that the centrifugal effect of the Antarctic icecap may at the present time be supplemented by another significant centrifugal effect created by the icecap in Greenland. It is true that the Greenland cap is much smaller than the one in Antarctica, but, on the other hand, its center is much farther from the pole. For this reason it could conceivably have an important centrifugal effect. Its position on the meridian is such that any uncompensated mass would add to rather than counteract the effect of the Antarctic cap; the two icecaps are, so to speak, in tandem.

In recent years a French polar expedition has taken many gravity readings across the top of the Greenland icecap. The purpose of these readings was to assemble data for a determination of the state of isostatic adjustment of the Greenland cap. The results of this piece of research are instructive for the whole subject of isostasy. They can be read two ways. On the one hand, the gravity data when reduced according to one of the formulas in common use—the Faye-Bouguer—showed an enormous excess of mass in Greenland (441:60–61); on the other hand, the same data, when reinterpreted differently, resulted in a finding of good isostatic adjustment. It is important to realize that the different methods of reducing gravity data are based on varying assumptions regarding the deeper structure of the earth's crust, and that these assumptions are not subject to direct confirmation. Daly at one time remarked that he did not believe that any of the different methods of evaluating gravity data came very near the actual truth. It is reasonable to think that the selection of assumptions in this field may be influenced by a general belief in the soundness of isostasy, and that this general belief will lead to a preference for those assumptions that result in findings of close isostatic adjustment. It is perhaps

on account of this that Einstein regarded the theory of isostasy as itself unreasonable (128). According to our theory, Greenland possibly was deglaciated during the period of the Wisconsin icecap in North America, and therefore the icecap there may be recent, and isostatic adjustment poor.

There are a few indications that the pressure of the Antarctic cap (possibly reinforced by the Greenland cap) has already begun to disturb the stability of the earth's crust. These consist of recent seismic movements.

In the study of earthquakes, specialists have distinguished between them not only according to their scale but also by a qualitative difference that appears to exist between those of minor and those of major magnitudes. Minor earthquakes, which occur daily in considerable numbers, are considered to be of local origin. They are merely episodes in the perpetual process of adjustment of strains in the earth's crust arising from local causes.

Some of the major earthquakes, on the other hand, are considered to be qualitatively different. Benioff, for example, suggests that these major earthquakes are not related to local causes of any sort, but result from the operation of what he calls "world-wide stress systems" (29)—in other words, from pressures applied to the earth's crust *as a whole.* So far as I know, no geologist has advanced an explanation for these world-wide pressures; it seems quite possible that they may be related to the icecap pressure that, according to our view, has been increasingly exerted on the crust for thousands of years.

Benioff draws attention to a fact that appears to confirm this supposition. He points out that in recent decades there has been an increasing tempo of major earthquakes; they appear to be coming closer together and increasing in violence. He cites especially the great quakes of 1904, 1924, 1935, 1940, and 1950 (29:335). If the Antarctic icecap is the major cause of these quakes, we can understand the increase in their frequency and intensity, which may result either from the increase of the quantity of Antarctic ice or from

the progressive weakening of the crust under the repeated major shocks.

Some further confirmation of this suggestion may be found in some specific features of the two greatest of the major earthquakes, those of eastern India in 1897 and in 1950. The earlier of these was a cataclysm that involved hundreds of thousands of square miles. The later one was still more violent, in line with the observation made by Benioff. The reader may note, by glancing at the globe, that the area in which these two quakes occurred lies almost on the meridian of 96° E. Long., which, it appears, is the meridian of direct thrust of the Antarctic icecap. According to our theory, this is the meridian along which the gradually increasing thrust of the Antarctic icecap has been exerted for thousands of years. Let us note the fact that Assam lies across the equator from the South Pole, and that the thrust of the icecap would tend to push the area toward the north, or poleward, so that, because of the shape of the earth, the result would be compression of the crust in that area. Now it is not unreasonable to suppose that during several millennia the pressure from Antarctica may have resulted in some elastic yielding of the crust along the meridian, with consequent concentration of compressive stresses in that area.

I cannot say that the constant pressure of the Antarctic icecap, operating on the crust in the same direction for ten thousand years or more, and amounting to several times a million times a million tons, definitely did cause some yielding of the crust in that direction, because I do not know, but I know what would have happened if there were some yielding. Yielding, at the latitude of Assam, to a pressure directed from the south would mean compression of the crustal material between lateral pressures, because of the lesser circumference of the globe as one goes north. If we suppose only a very slight yielding of the crust along the meridian (amounting to only a few feet) the pressures so produced would be very great. An explosive situation would exist because rock is not very compressible. The forces would

have to express themselves somehow. There would be no place to go but up. A striking confirmation of this may be found in the extraordinary fact that Mt. Everest and possibly much of the Himalayan range appear to have been raised from 100 to 200 feet by the gigantic earthquake of 1950 (74:63).

Now we learn from Daly that gravitationally the Himalayas are already too high. They are, or were before the earthquake, over 700 feet higher than they should have been for good isostatic adjustment. Earthquakes are generally supposed to perform the function of enabling the crust to adjust to the force of gravity. If an area stands too high, earthquakes occur during a process of settling down to equilibrium. If an area is too low, earthquakes may occur as it is rising. But what shall we say about an earthquake that finds an area already too high, and shoves it up further? This earthquake is not behaving according to the rules. It is not tending to establish the stability of the crust, but rather is exposing the crust to a situation of increased strain after the quake.

But most important of all, where could the compressive, horizontally directed force have come from to cause this earthquake? The best reason for putting forth the claims of the Antarctic icecap is that, so far, no one has produced a more reasonable suggestion. It is interesting that the editors of *Life,* in their illustrated account of the great quake in Assam, called it "the most mysterious earthquake of modern times."

There are a few additional items that appear to fit into this picture. In the very year in which the Assam earthquake occurred (1950) another great earthquake on the same meridian, but on the opposite side of the earth, virtually destroyed the city of Cuzco. Also near the same meridian, in Mexico, we have recently seen the rapid creation of the great new volcanic mountain of Paricutín, an event that Campbell ascribes to the effects of the increasing bursting stress on the crust in the earth's equatorial bulge. Finally, it may be worth

while to mention Ewing's recent discovery of a world-wide system of great submarine canyons, along which the crust is tectonically active. Observers have suggested that these canyons are still widening. Breen has pointed out that their pattern (not yet fully established) is consistent with the effects of a force attempting to pull the Western Hemisphere southward (44).

I am aware that the items that I have mentioned above may all be explained someday according to other principles, and may in fact have nothing to do with centrifugal effects from Antarctica. However, I feel that a definite chance exists that the phenomena may be related, and that they may indicate that beginning of a crust displacement is not remote. The question therefore arises as to whether, in case of another displacement, it is possible to predict anything about it in detail. Among the questions to which answers may be sought are those of the precise direction the displacement may take, and the total distance it may cover.

As to the first question, our theory offers us the basis for a reasonable estimate. If our finding of the location of the center of mass of the Antarctic icecap is correct, and if we assume that no other centrifugal effects from anomalies in the crust will be acting to deflect the motion, we may expect the next displacement to be in the direction of 96° E. Long. from the South Pole. This would involve another southward displacement of the Western Hemisphere, together with another northward displacement of East Asia.

A guess as to the magnitude of the next displacement requires the correct assessment of a number of rather imponderable factors. We should expect the crust to continue to move until the Antarctic icecap was largely destroyed. This might take longer than it did in the case of the North American icecap, because the Antarctic icecap is larger. On the other hand, it could take less long, because in the case of Antarctica there is no land available for the rearward build-up of the icecap as it moves into the lower latitudes, such as seems to have occurred in North America. Perhaps we shall

have to be satisfied, for the present, with the guess that the next displacement may be of roughly the same magnitude as the last one.

If these guesses turn out to be correct, the next North Pole will be in the vicinity of Lake Baikal, in Siberia. North America, moving southward into the tropics, will subside some hundreds of feet relatively to sea level, and the ocean will occupy the river valleys and will divide the continent into several land areas. India will move northward out of the tropics, and since there will be no land to the south to provide a refuge for the fauna and flora, we shall have to expect the extinction of many species of animals and plants now confined to that country. Many other consequences may be unpredictable. The gradual climatic changes due to changing latitude will be accompanied by numerous sudden, violent, and destructive climatic changes due to volcanism.

2. *A General Summary*

In this book I have presented a highly detailed mass of material, and I have sought to relate it to a single, essentially simple hypothesis. It now remains to summarize the evidence, and the argument for the hypothesis.

We have seen that the problem of the geographical stability of the poles has long been a vexatious matter for science. From time to time theories of polar shift have been advanced, supported by large quantities of evidence, but the proposed mechanisms have been found defective, and in consequence the theories have been rejected. The failure of the theories has led, in the following years, to neglect of the evidence, or to its analysis in accordance with theories conforming to the doctrine of the permanence of the poles. Although all the older theories of polar change, including that of Wegener, have been discredited, the evidence in favor of polar change has constantly increased. As a consequence, many writers at the

present time are discussing polar shift, but none of them has as yet suggested an acceptable mechanism.

The general evidence for displacements of the crust is exceedingly rich. In turn, the assumption of such displacements serves to solve a wide range of problems, such as the causes of ice ages, warm polar climates, mountain building; it provides a mechanism that may account for changes in the elevations of land areas and in the topography of the ocean floors; it also provides a basis for the resolution of conflicts in isostatic theory. For the period of the late Pleistocene, the theory permits the construction of a chronology of polar shifts, with three successive tentative polar positions in Alaska, Greenland, and Hudson Bay preceding the present position of the pole. The evidence for the location of the Hudson Bay region at the pole during the last North American ice age is overwhelming, and this fact in itself provides the principal support for the assumption of the earlier shifts. The tempo of change indicated for the late Pleistocene is reflected in evidence from earlier geological periods.

The theory is able to explain not only the general succession of climatic changes in various parts of the world in the late Pleistocene; it can account also for the detailed history of the last North American icecap. It can explain the fluctuations of that icecap, its repeated retreats and readvances. It shows that the effects of volcanism were directly responsible for the oscillations. It shows also that these same effects, added to the effects of gradual climatic change, were responsible for the widespread extinctions of species at the end of the Pleistocene, and from this we may assume that the same cause was responsible for numerous extinctions in earlier geological periods. By providing a reasonable basis for the assumptions of rapid climatic change and rapid topographical change (including the existence of former continents and land bridges), the theory provides solutions for many problems in the evolution and distribution of species.

Our theory of displacement depends upon two assumptions, and on two only. One of these is that a continental ice-

cap is largely or entirely uncompensated isostatically. The other assumption is that at some point below the crust a weak layer exists that will permit the displacement of the crust over it. The first assumption is capable of verification, and may even be verified in the course of the current Geophysical Year. This will depend, however, on whether the new gravity data that are to be collected during this year are reduced by formulas based on correct assumptions. There is no present prospect of direct verification of the second assumption. However, the body of geological evidence presented in this book provides very strong indirect support for both these assumptions.

As to the mechanics of crust displacements, Campbell has provided the necessary constructions. To some, the simplicity of his thought may be unnerving, but I feel assured that in the end this simplicity itself will be the justification for reposing wide confidence in this theory. For it appears that no recondite principle can vitiate it. Who can argue with formulas so simple that a high school student can, and usually does, master them?

In addition to the support provided by evidence from the field, our theory receives support from logic. It has been recognized that one characteristic of sound new theories is the simplicity of their basic assumptions, and another is their capacity to explain a greater number of facts or a greater range of problems than previous theories. It was the simplicity of this theory that first aroused the interest of Einstein, in whose philosophy of science simplicity was a prime consideration. It appeared to him also that it might explain a far greater number of facts than were explainable by the various theories that have been produced to explain the leading problems of the earth separately.

I shall have to admit that the full development of the implications of crust displacements, for all the affected fields, has carried me much further than I originally expected. When I resurvey the structure that has now been erected on the basis of the simple basic theory, I feel exactly as Sir James

Frazer, author of the *Golden Bough,* felt at the end of his protracted labors, and I cannot do better than conclude this volume with his words:

> Now that the theory, which necessarily presented itself to me at first in outline, has been worked out in detail, I cannot but feel that in some places I may have pushed it too far. If this should prove to have been the case, I will readily acknowledge and retract my error as soon as it is brought home to me. Meanwhile, my essay may serve its purpose as a first attempt to solve a difficult problem, and to bring a variety of scattered facts into some sort of order and system. (*The Magic Art*)

> "But when you have the truth, everything fits. I think that's the main test of truth. It fits, it makes a harmony, one pattern all through. . . ."
>
> E. R. Punshon, *Information Received*
> (Penguin Books, 1955)

APPENDIX

Letters from Albert Einstein and George Sarton

A. Einstein
112 Mercer Street
Princeton, N. J.

November 24, 1952

Mr. Charles Hapgood
2 Allerton Street
Provincetown, Mass.

Dear Sir:

I have read already some years ago in a popular article about the idea that excentric masses of ice, accumulated near a pole, could produce from time to time considerable dislocations of the floating rigid crust of the earth. I have never occupied myself with this problem but my impression is that a careful study of this hypothesis is really desirable.

I think that our factual knowledge of the underlying facts is at present not precise enough for a reliable answer based exclusively on calculations. Knowledge of geological and paleontological facts may be of decisive importance in the matter. In any case, it would not be justified to discard the idea a priori as adventurous.

The question whether high pressure may not be able to produce fusion of nuclei is also quite justified. It is not known to me if a quantitative theory has been worked out by astrophysicists. The action of pressure would not be a static effect as classical mechanics would suggest, but a kinetic effect corresponding not to temperature but to degeneracy of gases of high density. You should correspond about this with an astrophysicist experienced in quantum theory, f.i. Dr. M. Schwarzschild at the Princeton University Observatory.

Sincerely yours,
(Signed) A. Einstein

Princeton,
May 8, 1953

Dear Mr. Hapgood,

I thank you very much for the manuscript that you sent me on May 3rd. I find your arguments very impressive and have the impression that your hypothesis is correct. One can hardly doubt that significant shifts of the crust of the earth have taken place repeatedly and within a short time. The empirical material you have compiled would hardly permit another interpretation.

It is certainly true, too, that ice is continually deposited in the polar regions. These deposits must lead to instability of the crust when it is sufficiently strong not to constantly keep in balance by the adjustment of the polar regions.

The thickness of the icecap at the polar regions must, if this is the case, constantly increase, at least where a foundation of rock is present. One should be able to estimate empirically the annual increase of the polar icecaps. If there exists at least in one part of the polar regions a rock foundation for the icecap, one should be able to calculate how much time was needed to deposit the whole of the icecap. The amount of the ice that flowed off should be negligible in this calculation. In this way one could almost prove your hypothesis.

Another striking circumstance appears in connection with the ellipticity of the meridians. If according to your hypothesis an approximate folding of the meridional volume takes place, that is, folding of a meridional volume within an equatorial volume (which is considerably larger), this event will have to be accompanied by a fracture of the hard crust of the earth. This also fits in very well with the existing phenomena of the volcanic coastal regions with their mainly north-south extension and the narrowness in the east-west direction. Without your hypothesis one could hardly find a halfway reasonable explanation for these weak spots of the present-day crust of the earth.

Excuse me for not writing in English. My secretary has been away for some time, and "spelling" makes frightful difficulties for me.

With sincere respect and kind regards,

Yours,
(Signed) A. Einstein

(Translated by Ilse Politzer)

5 Channing Place,
Cambridge, Mass.,
Tuesday '55 06.07

Dear Mr. Hapgood,

I have read your lecture at the AMNH, the discussion which followed and the Einstein documents, with *deep* interest. I really think that you are on the right track but have no authority to express a more definite opinion. It is clear that the only opinions which matter are those which are the results of independent studies by competent specialists.

The combination of ideas is so new that the history of science has nothing to contribute to its understanding. The fact that there have been earlier theories like those of Wegener, Kreichgauer and Vening Meinesz simply proves that some meteorologic and geologic problems had to be solved, and exercised the minds of men of science. What you need is not historical facts, but physical ones, and mathematical developments.

With every good wish
(Signed)
George Sarton

GLOSSARY

ANOMALY, Positive: An excess of mass at a point on the crust, as compared with the average distribution of mass.

Negative: A similar but opposite condition, in which there is a local deficiency of mass.

ANTICLINE: An archlike folding of rocks or rock strata so that the lower beds or strata are enclosed in the upper.

ARCHAEOCYATHIDAE: A fossil sponge, at one time supposed to be a fossil coral.

ASTHENOSPHERE: A layer of material below the crust; assumed to be weak because of heat and pressure.

BASEMENT ROCKS: Rocks of great obscurity and complexity lying beneath the upper rock layers; thus, the lowermost rocks of the known series.

CENTRIFUGAL FORCE: The force tending to throw a body away from the center, in a straight-line direction of flight.

DECIDUOUS TREES: Trees having seasonally falling foliage (oak, elm, etc.) as contrasted with evergreens, having constantly renewed foliage (pine, etc.).

DIASTROPHISM: The process, or processes, by which major features of the crust are formed through deformation, such as faults, plateaus, etc.

ECOLOGICAL: Pertaining to the mutual relationship between organisms and their environment. An ecology is the place-relationship of life forms in their environment.

EPEIROGENESIS (EPEIROGENY): A grander form of diastrophism, forming the broader features of crustal relief, such as continents, ocean beds, etc.

EPICONTINENTAL: Pertaining to regions along the continental shelf.

EUSTATIC: Pertaining to a land area which has not undergone elevation or depression.

GEOID: The figure of the earth (an oblate sphere) with the average sea level conceived of as extending throughout the continents.

GEOSYNCLINE: A great downward flexing of the crust.

GRAVITY, Center of: An imaginary point at which, for reasons of computation, the entire weight or mass of a body is imagined to be concentrated.

HORSE LATITUDES: A belt in the neighborhood of 30° N. or S. Lat., characterized by high pressure, calms, and baffling winds.

HYDROSTATIC: Pertaining to pressure and equilibrium of liquids.

IGNEOUS ROCKS: Rocks which have cooled and solidified from a molten state.

INSOLATION: Solar radiation received by the earth. The *insolation curve* represents the combined effects on mean world-wide temperatures of various astronomical factors, such as precession and variations of the orbit of the earth about the sun.

ISOSTASY: The definition is discussed in the text: Chapter VI.

LITHOSPHERE: The outer shell of the planet. It is composed of rocks and the products derived from them by erosion, etc., such as gravels, soils, and the like. The crystalline, solid lithosphere is assumed to be between 20 and 40 miles thick. (Also called *crust.*)

LOAD, Negative: A deficiency of matter at a given point on the crust; considered *negative* because it results in a pressure from within, as a result of the tendency to achieve hydrostatic balance.

Positive: A local excess of mass.

MAGMA: Molten rock material within the earth. When cooled and crystallized into solid form it yields the so-called igneous rocks (*q.v.*).

MASS: A measure of the amount of matter in a body.

MASS, Center of: Point at which the mass is assumed to be concentrated; for computational purposes.

METAMORPHIC: Altered; applied to rocks or rock strata that have been physically changed by heat or other means.

MILLIGAL: A thickness of ten meters of granite; the effects (gravitational) produced by such mass.

MUTATION: A sudden variation in the characteristics of a life form as compared to those of its progenitors.

NEBULAR THEORY: A theory of the origin of the solar system, according to which a gaseous nebula coalesced and cooled to form compacted centers which then further contracted to form the planets.

OOZE: A soft deep-sea deposit composed of shells, debris, meteoric dust, etc. *Argillaceous* ooze is a clayey type.

PLANETESIMAL: A small, solid planetary body having an individual orbit about the sun.

PLANETESIMAL HYPOTHESIS: A theory of the origin of the solar system supposing that the planets were formed by collision and coalescence of planetesimals and thus have never been wholly molten.

PLICATION: Folding into layers or strata.

PRECESSION: The wobbling of the axis of the earth, making the pole describe a circle as the planet spins.

RADIATION, Adaptive: The production of a diversified fauna as the result of the availability of new ecological spaces. New faunas re-

sult from the adaptation of an original stock to the new environmental opportunities for living space.

SIAL: Silicon-aluminum rocks.

SIMA: Silicon-magnesium rocks.

STRANDLINE: A line marking a fossil seashore.

STRATIGRAPHIC: Pertaining to the arrangement of rock strata.

TECTONIC: Pertaining to rock structures resulting from deformation of the crust.

TURBIDITY CURRENTS: Submarine currents caused by slumping of deposits along continental margins. These currents carry sediments with them.

UNCOMPENSATED WEIGHT: The weight of a crustal formation not isostatically adjusted. It thus constitutes a positive anomaly (*q.v.*).

UNIAXIAL: The condition of having only one axis, as a sphere.

VARVES: Annual deposits of sediment. These can be counted to determine annual shorelines of old lakes.

VIRENZPERIOD: A period in the history of a life form in which it experiences an explosive evolution and proliferation.

VISCOSITY: The quality of being able to yield to stress or of being able to flow; the measure of such a property.

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