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ANNUAL REVIEWS

Annual Review of Earth and Planetary Sciences

A Geologist Reflects on a Long Career

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Annu. Rev. Earth Planet. Sci. 2018. 46:1–20

First published as a Review in Advance on March 12, 2018

The *Annual Review of Earth and Planetary Sciences* is online at earth.annualreviews.org

https://doi.org/10.1146/annurev-earth-082517-010111

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Keywords

plate tectonics, oceanic geotherms, continental geotherms, mantle convection, gravity anomalies

Abstract

Fifty years ago Jason Morgan and I proposed what is now known as the theory of plate tectonics, which brought together the ideas of continental drift and sea floor spreading into what is probably their final form. I was twenty-five and had just finished my PhD. The success of the theory marked the beginning of a change of emphasis in the Earth sciences, which I have spent the rest of my career exploring. Previously geophysicists had principally been concerned with using ideas and techniques from physics to make measurements. But the success of plate tectonics showed that it could also be used to understand and model geological processes. This essay is concerned with a few such efforts in which I have been involved: determining the temperature structure and rheology of the oceanic and continental lithosphere, and with how mantle convection maintains the plate motions and the long-wavelength part of the Earth's gravity field. It is also concerned with how such research is supported.

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1. INTRODUCTION

Unlike most papers I have written this one results from two unusual requests. The first, from Annual Reviews, was to write an account of my career. The second, from the Royal Society of London, was to write something to save whoever writes my obituary from having to do a lot of work, a request which made me feel very old.

I have written elsewhere (M^c^Kenzie 2001) about my experience of being involved in the construction of the theory of plate tectonics at the start of my career. I wrote and published the first paper on the subject with Bob Parker (M^c^Kenzie & Parker 1967) fifty years ago, during my first year as a postdoc when I was twenty-five. Though ours was the first paper on this subject to be published, shortly before this happened I discovered that Jason Morgan had priority and I tried (unsuccessfully) to delay its publication. Frankel (2012) has now published a detailed account of the relevant events, which changed my career almost overnight. I went from being an unknown postdoc who was younger than most US graduate students, to someone who was invited as a guest speaker, with all fares paid, to major international conferences. This was a special time in the history of the Earth sciences, which has not been repeated. I was very lucky to have had the opportunity to be so involved in constructing the theory. It was luck that my parents decided to start a family in 1941, having decided that the United Kingdom would not be conquered by Hitler. If they had been more cautious I would still have been an undergraduate in 1967. My other piece of luck was to be given a fellowship with four years' support by King's College in 1965. The fellowship gave me complete freedom to work on any problem that interested me wherever I wished to do so.

My involvement in plate tectonics led to my being awarded a number of major international prizes. The first was the Balzan Prize in 1981, which I was given with Drum Matthews and Fred Vine when I was thirty-nine. The money associated with the prize made a real difference to my life: I bought my first new car and paid for my son's education. The second was the Wollaston Medal of the Geological Society of London, which I was given in 1983. To me this medal was very special, because it had previously been given to the nineteenth-century geologists, Darwin, Lyell and Geikie, whose work I so much admired. I was also very pleased to be recognized as a geologist, using physics, rather than the hammer, to understand geological problems. Though it is difficult for awards committees to give major prizes to scientists early in their careers, I feel strongly that they should try to do so wherever possible. The theory of plate tectonics is unusual in that it was developed rapidly, between 1963 and 1969, and has required almost no later modifications. Measurements of plate velocity vectors using the Global Positioning System (GPS) have now beautifully confirmed those determined from oceanic magnetic anomalies and earthquake slip vectors, so it seems likely that the theory is essentially complete. Though the new theory attracted a great deal of attention, from both the scientific press and the media, and made its originators (briefly!) famous, for me it was in some ways disappointing. After the challenges and excitement of putting the theory together and of making it as intellectually tight as possible, I did not look forward to spending my career making more and more accurate estimates of the poles of rotation of the plates, even though I thought that new and important results would come from such studies, as Richard Gordon and his colleagues (e.g., Argus et al. 2011) have shown.

My lack of enthusiasm for plate tectonics caused me to become interested in continental tectonics, which clearly has important differences from that of the oceans, and in mantle convection, which alone could provide sufficient mechanical energy to maintain the observed motions. Both problems are much better understood now than they were fifty years ago, and both have caused me to work on a number of interesting problems. But probably the most important research I have done in the last fifty years followed from something I did before I even had a PhD. While I was waiting for my examiners to read my thesis, I carried out a study of the thermal consequences of plate creation on ridges. My first attempt to do this had some important shortcomings. However, over the fifty years since my original paper was published these have steadily been removed, partly as a result of my own research. I thought the history of this effort might be of interest. Another area in which my research has been influential is mantle convection, which has not yet been anything like as successful as plate tectonics or the thermal modeling. I have also made a number of mistakes, for reasons which I find interesting, especially if I could use them to learn how to avoid doing so again (which, sadly, I doubt). The last sections of this essay are concerned with starting and stopping research projects and my own views about how research is supported in the more successful universities in Western democracies. The whole essay is a retrospective attempt at making sense of parts of my research career, which has been quite unplanned and disorganized. My curiosity about how things work has involved me in many parts of geology: continental tectonics, melt generation and movement, organic and inorganic geochemistry, igneous (and most recently metamorphic) petrology, and even in the lithospheric structure and mantle convection within Venus and Mars. I could equally well have chosen to illustrate my arguments with examples from these other fields. The essay is also a plea for the type of undirected research support that has maintained me throughout my career. Such support still exists in a few places but is, I think, endangered, even though it is generally not very expensive.

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2. THE THERMAL STRUCTURE OF THE LITHOSPHERE

My involvement in this problem began when I was still a graduate student and has resulted in more than ten papers with me as author with about 8,000 citations, or an order of magnitude more than those from plate tectonics. Though it began with a paper whose detailed conclusions are (largely) wrong (M^{c}Kenzie 1967), the same is true of Harry Hess's (1962) paper that made the critical suggestion which led to acceptance of sea floor spreading and plate tectonics. As a result of the work that I and others have done, I think we now have rather accurate thermal models of both continental and oceanic lithosphere (Jackson et al. 2008) which can be used to explore other issues (Priestley & M^{c}Kenzie 2013).

My work on this problem began in 1966 when I read a paper by Langseth et al. (1966) which argued that the heat flow and topography associated with ridges were not compatible with Hess's (1962) ideas of sea floor spreading. Their discussion used a numerical model with a local hot upwelling sheet beneath the ridge axis. In the fall of that year I attended a small conference in New York, later published as Phinney (1968), where Fred Vine and Lynn Sykes presented completely convincing evidence that spreading is now occurring on oceanic ridges, and Fred used the magnetic anomalies to estimate the spreading rate. I returned from New York and, while I waited for my examiners to read my PhD, I thought I would look at the problem Langseth et al. (1966) discussed, but in a different way, to see how their observations could be reconciled with sea floor spreading. Instead of solving the problem numerically, I used an approach that I had learned from fluid dynamicists, and especially from Adrian Gill, who I had got to know in 1965 when I was working at Scripps. I used the equations governing the temperature T in a plate of constant thickness l moving horizontally with a constant velocity v in the x direction

$$v\frac{\partial T}{\partial x} = \kappa\left(\frac{\partial^2 T}{\partial x^2} + \frac{\partial^2 T}{\partial z^2}\right) \qquad 1.$$

where κ is the thermal diffusivity and z is positive upwards. I imposed constant-temperature boundary conditions at $x = 0$ and $z = 0, -l$, with $T = 0$ on $z = 0$ and $T = T_1$ on $x = 0$ and $z = -l$. Since the temperature on the base of the plate was constant and the same as that of the ridge axis, the ridge was not underlain by a hot rising sheet. I assumed that horizontal heat diffusion can be ignored compared with vertical diffusion, or $\frac{\partial^2 T}{\partial z^2} \gg \frac{\partial^2 T}{\partial x^2}$. If x is then replaced by t, the age of the plate, Equation 1 can be written in dimensionless form

$$\frac{\partial T'}{\partial t'} = \frac{\partial^2 T'}{\partial z'^2} \qquad 2.$$

with boundary conditions $T' = 0$ on $z' = 0$, $T' = 1$ at $t' = 0$ and on $z' = -1$, where

$$x = vt,\ T = T_1 T',\ z = lz',\ t = (l^2/\kappa)t'.$$

Equation 2 and its boundary conditions are easily solved by standard methods. The great advantage of using dimensionless variables is that the resulting solution is valid for all values of l, κ and T_1. The scaling also immediately shows that the time dependence of the thermal structure is $\propto l^2/\kappa$. **Figure 1** shows the thermal structure I proposed (panel *a*) and the calculated and observed heat flow (panel *b*). Clearly the model could account for the elevated heat flow associated with ridges,

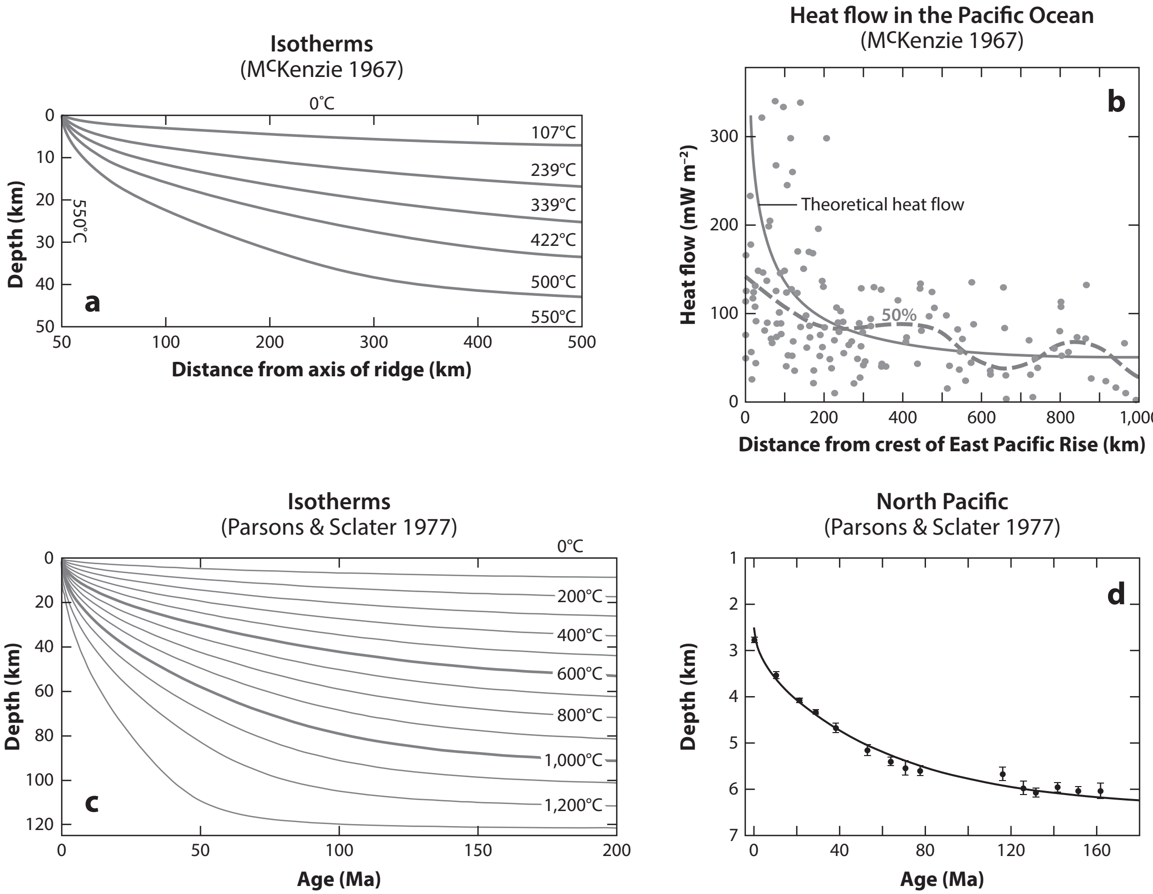

Figure 1

(*a*) Isotherms and (*b*) heat flow (modified from McKenzie 1967), calculated from the analytical solution for the temperature of a plate model with a plate thickness of 50 km and a constant lower temperature of 550°C. (*c*) Isotherms and (*d*) oceanic depth as a function of plate age calculated from the analytical solution using Parsons and Sclater's (1977) values for constant thermal conductivity, a plate thickness of 125 km and a base temperature of 1,333°C. The observed depths in panel *d* are from the North Pacific.

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though the scatter in the observations was large. In 1966 the cause of this scatter was unknown, and panels *a* and *b* in **Figure 1** were little more than sketches illustrating how spreading could in a general way account for the observations. When I got to know Jason Morgan well in 1968 I discovered that he had carried out the same calculation at about the same time as I did (Morgan 1975).

Two events showed that the plate model was much better than a sketch, and could accurately model the thermal structure of the oceanic lithosphere. The first was the discovery of black smokers: vents in the sea floor near ridges from which huge volumes of hot (~300°C) water are pouring. The existence of these hot plumes was first suspected from water temperature observations above the Galapagos Ridge (Williams et al. 1974) and spectacularly confirmed by submersible dives on the East Pacific Rise. These observations showed that important amounts of heat were advected, rather than conducted, across the sea floor and explained the scatter in the conductive measurements. The second was a beautiful paper by Barry Parsons and John Sclater (Parsons & Sclater 1977), showing that the simple plate model I proposed in 1967 could accurately model the increase in depth with the age of the sea floor (**Figure 1*c*,*d***). I was astonished that my simple model worked so well. This success in turn showed that the main features of the model were correct. One of the most important (which I scarcely mentioned in the 1967 paper through inexperience; it was almost the first paper I wrote) was that both the heat flow and the topography of ridges were compatible with an isothermal mantle below the ridges. Neither provided any evidence of the existence of a hot sheet in the upper mantle. Such a sheet was a feature of Hess's (1962) original paper. But his proposal caused major problems in understanding how ridges surrounding Africa and Antarctica could all move away from their respective continents as they spread symmetrically. How did the upwelling sheets know how to move to stay beneath ridge axes? No such difficulties arise if ridges have no deep structure. I was profoundly impressed by the agreement between the theory and the observation. This agreement showed that simple models, based on straightforward physical ideas, could produce quantitative agreement with the observations.

In the forty years since Barry and John published their paper the agreement has actually improved. They used the oceanic bathymetry to estimate the values of T_1 and the thermal expansion coefficient α, as well as the plate thickness l. The value of T_1 is now constrained by the oceanic crustal thickness and the latent heat of melting, and that of α by laboratory experiments. Both values agree well with Parsons and Sclater's estimates. The only remaining adjustable parameter is now l, the plate thickness. If the variation of thermal conductivity with temperature is also taken into account, the best fitting value of l is about 100 km (McKenzie et al. 2005). **Figure 2** shows that the variations of depth and heat flow with age still agree excellently with the observations when volcanic ridges and dynamical topography are removed (Crosby et al. 2006). The thermal model shows that the oceanic seismogenic thickness T_s, the depth of the deepest earthquakes, is controlled by temperature (Jackson et al. 2008) (**Figure 2*a*,*b***), and corresponds to the 600°C isotherm. Accurate estimates of the long-term elastic thickness T_e are sparse, because few areas of the oceans have been accurately mapped in two dimensions. Those that are available are all less than T_s.

Two papers have been published that claim some of the above statements are incorrect. Hasterok (2013) claimed that the numerical scheme used by McKenzie et al. (2005) is incorrect, and proposed a different one. His statement is not correct, though his alternative scheme is satisfactory if the time step is sufficiently short to satisfy the stability conditions. It then gives results that are indistinguishable from those of McKenzie et al. (2005), whose scheme is unconditionally stable. The other claim, by Hofmeister and Criss (2006), concerns the thermal contraction of the lithosphere. They argued that the thickness of the lithosphere will decrease by $\alpha T/3$ because the contraction is isotropic. Their belief that the contraction is isotropic is correct, and Mishra

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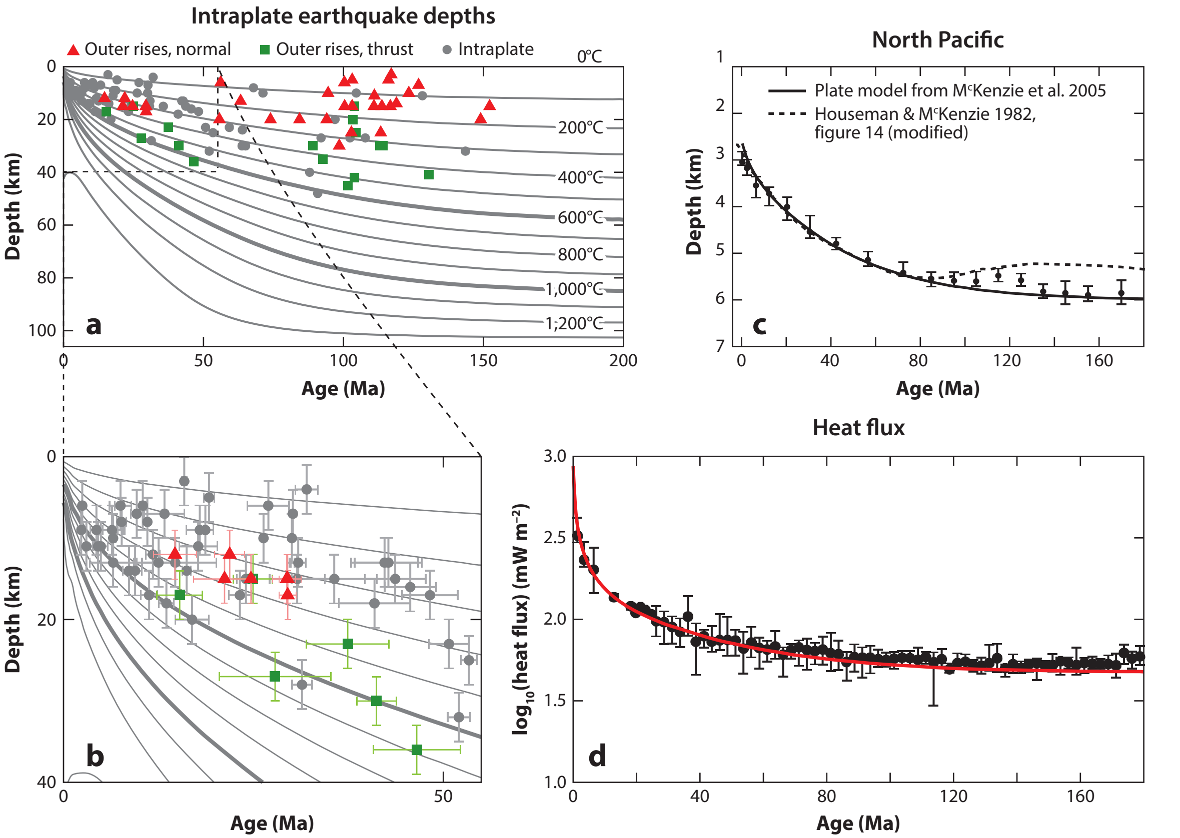

Figure 2

Comparison of (*a*, *b*) depths of oceanic earthquakes (Jackson et al. 2008), (*c*) North Pacific subsidence history (Crosby et al. 2006), and (*d*) oceanic heat flow (Hastorek 2013) with a numerical solution for a cooling plate (M^c^Kenzie et al. 2005) using a temperature-dependent conductivity and a base potential temperature of 1,315°C. The initial temperature of the plate was calculated from isentropic decompression melting using the base potential temperature. Such melting generates a crustal thickness of 7 km. The dashed line in panel *c* is taken from Houseman and McKenzie (1982, figure 14), and shows the effect of the removal of the lower part of the cooling boundary layer by a convective instability.

and Gordon (2016) have recently argued that this effect can explain systematic variations in the azimuths of transform faults. But the subsidence of the sea floor is controlled by isostasy, not vertical contraction. As the lithosphere cools, the density increases. But so does the mass/unit area of the lithosphere, because the contraction is isotropic. This mass increase causes the pressure exerted on the base of the lithosphere to increase with age, resulting in it sinking deeper into the mantle. This effect is illustrated in figure 19 of Sclater and Francheteau (1970). The compensation mechanism is therefore a combination of both Airy and Pratt isostasy. The standard calculation, which involves balancing sections of different ages, implicitly takes this effect into account. So Hofmeister and Criss's objection is incorrect (though see their figure 2*c*, which is correct).

The same thermal model of cooling lithosphere which is so successful in the oceans can also account for the subsidence of sedimentary basins (McKenzie 1978). The important difference between oceanic and continental tectonics is that deformation of continental lithosphere is distributed, rather than occurring on narrow plate boundaries. Distributed stretching of continental

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lithosphere makes it thinner and causes it to subside. When the stretching stops, the lithosphere continues to cool and subside. At the time I made this proposal I was working on tectonics of the Aegean, which consists of continental lithosphere now undergoing extension, and was not interested in sedimentary basin formation. Despite the enormous economic importance of the oil and gas contained in basins like the North Sea, in 1978 no one had a clear understanding of how they were formed. My proposal accounted for their principal geological features, and was immediately accepted by petroleum geologists. Its relevance to the thermal evolution of source rocks in sedimentary basins was obvious, both to me and to geologists working in oil companies. Though the modifications to the oceanic thermal model that were required to deal with distributed deformation are relatively minor, this paper is the most cited ($\sim$3,000 times) of all those I have written, and also has probably been the most influential. At Cambridge two graduate students, Penny Barton and Rosy Wood (Barton & Wood 1984), carried out what is still one of the most careful tests of my proposal, using data from the North Sea. When John Browne was head of BP he remarked to me that understanding the thermal evolution of sedimentary basins had saved the industry more than 10^9 dollars, though I think such estimates can be little more than guesses. What is, I think, more important is that the proposal resulted from studies of the earthquake mechanisms in the Aegean, and not from research directed at understanding the origin of sedimentary basins.

The thermal model of oceanic plates that I proposed has a lower boundary condition of constant temperature. Clearly such a boundary condition is physically impossible, because it requires a discontinuity in heat flux. Some authors continue to use a half-space cooling model, though this results in unrealistically large subsidence of sedimentary basins. Barry Parsons and I (Parsons & McKenzie 1978) proposed that the heat was supplied to the lower part of the cooling lithosphere by a convective instability that occurs when the plate age is $\sim$60 Ma. Greg Houseman (Houseman & McKenzie 1982) carried out detailed numerical modeling of this instability, which can cause the variation of depth with age not to be monotonic (**Figure 2*c***). Alistair Crosby (Crosby et al. 2006) has made a careful study of the variation of depth with age, taking into account both variations in crustal thickness resulting from volcanism and dynamical topography resulting from mantle convection. He found that the depth actually decreases as the age increases from 70 to 100 Ma (**Figure 2*c***). Such a decrease is to be expected if the lower part of the plate is removed by an instability (Houseman & McKenzie 1982), as Barry and I proposed. There is also a suggestion (**Figure 2*d***) that the heat flow may increase with age for the same reason.

The success of the plate model of the thermal structure of the oceanic lithosphere shows that we now have good constraints on oceanic geotherms. In continental regions we also have similarly good constraints from regions of thick lithosphere, from P, T estimates from garnet-peridotite nodules. Priestley and McKenzie (2006, 2013) have used temperature estimates from the plate model of McKenzie et al. (2005) at depths of up to 70 km to determine $V_{sv}(P, T)$, and hence the global variations in lithospheric thickness.

3. MANTLE CONVECTION

The discovery of plate tectonics and the development of the necessary kinematics occupied me and others for three or four years and has not required any important later modifications. Its success showed that most of the surface phenomena that had been attributed to mantle convection instead resulted from plate motions. It quickly became clear that the forces on plate boundaries govern their motion, as Don Forsyth and Seiya Uyeda (Forsyth & Uyeda 1975) convincingly showed. Estimates of the forces involved (McKenzie 1969, Richter & McKenzie 1978) confirmed this conclusion. But it was also obvious that plate motions only represented part of the convective circulation in the mantle. Gravity anomalies with wavelengths greater than about 500 km cannot be supported by elastic forces, and so-called "hot spots" like Hawaii showed that active convection occurs beneath plate interiors. But the existence of rigid plates covering the Earth's surface makes it difficult to "see" through the plates and so image the interior convective circulation. Fluid dynamicists commonly convert the relevant governing equations to dimensionless form, by scaling variables like lengths and temperatures. The resulting equations then contain dimensionless constants which are generally referred to by the names of the scientists who first wrote down the relevant equations. Though this approach is extremely powerful, because the values of these numbers alone control the entire fluid dynamical behavior of the system, the names of the numbers are completely unrelated to the fluid dynamics involved. Fluid dynamicists often use the names of these numbers as a shorthand, which obscures the physical processes at work. For constant-viscosity convection the relevant numbers are the Rayleigh, Prandtl, and Reynolds numbers. The Rayleigh number Ra measures the importance of heat transport by the motion of the fluid, generally referred to as advective or convective transport, to that by thermal conduction. In the mantle it is $\sim 10^6$ and heat transport is dominated by advection. The Prandtl number $Pr = \nu/\kappa$ is the ratio of two diffusivities: that of momentum, ν, the kinematic viscosity, to that of heat, κ, the thermal diffusivity. In the mantle $Pr \sim 10^{23}$. The Reynolds number measures the ratio of momentum advection to that of diffusion, and is $\sim 10^{-19}$. The advective transport of both linear and angular momentum is therefore completely unimportant, which is why mantle convection is not affected by the Earth's daily rotation.

In 1970 rather little was known about the convective behavior of fluids at large Rayleigh and Prandtl numbers, and almost nothing about the surface deformation and gravity anomalies such flow produces. Though convection with appropriate dimensionless numbers can easily be carried out in shallow ($\sim$5 cm) tanks in the laboratory (e.g., Richter & Parsons 1975), such experiments cannot be used to study the relationship between the convection, gravity and surface deformation, which is what was needed to interpret the observations. It was not even clear whether the gravity anomaly associated with a hot upwelling region should be negative, because it resulted from the hot, low-density material, or positive, because the gravity anomaly from the upwardly deformed surface dominated that from the low density. The only method of relating the convective circulation to the observables, then and now, is to solve the governing equations numerically. I combined with Nigel Weiss, whose principal interest is astrophysical fluid dynamics, to carry out such calculations, which had to be restricted to two dimensions, as that was all that was possible on the available computers. These early calculations (M^c^Kenzie et al. 1974) showed that the surface deformation dominated the gravity field, and that positive and negative anomalies corresponded to hot rising and cold sinking regions of the circulation. The effect of the internal density anomalies, which have the opposite sign to those from the surface deformation, is to reduce the magnitude of the gravity anomalies, so that the apparent density of subaerial topographic anomalies is about 1.1 Mg m^{-3}. This difference is diagnostic of convective, rather that elastic, support. Though the maps of gravity anomalies in oceanic regions were not very good in 1974, they clearly showed that depth variations that did not result from plate cooling were, as expected, convectively, not elastically, supported. Our knowledge of gravity anomalies has improved enormously since 1974, and a recent study by Crosby (Crosby et al. 2006) has confirmed the original conclusions.

My graduate students and I carried out a number of numerical studies of convection in the 1970s, but then we suddenly became uncompetitive. When we started, the computers available to us were not as powerful as those in the United States, but UK research policy was that computer time should be free, whereas in the United States, it had to be paid for by grants and cost \$500–\$1,000 an hour. Our runs typically took several days, so we could carry out studies of how the observable parameters varied with Rayleigh number, with the form of heating (whether from below or from within), and with the boundary conditions, which would have been very expensive

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to carry out in the United States. But our advantage suddenly disappeared when NSF installed large computer centers in a number of universities. I then stopped carrying out such research. A number of groups, especially in the United States, then began to dominate this area of research.

I have been surprised and disappointed by what has been done. The observational constraints have improved dramatically over the last thirty-five years, particularly because of the measurements from GOCE (Bruinsma et al. 2014). We now have global maps of the gravity field from satellite data that are essentially error-free at wavelengths longer than about 200 km (**Figure 3**). We also now have excellent maps of the residual bathymetry in the oceans; the bathymetry corrected for the age of the plate (Crosby et al. 2006). The time dependence of mantle convection has also become clear. Richard Gordon and his colleagues (Wang et al. 2017) have shown that the circulation is surprisingly steady, though major changes do occur from time to time when the lower boundary

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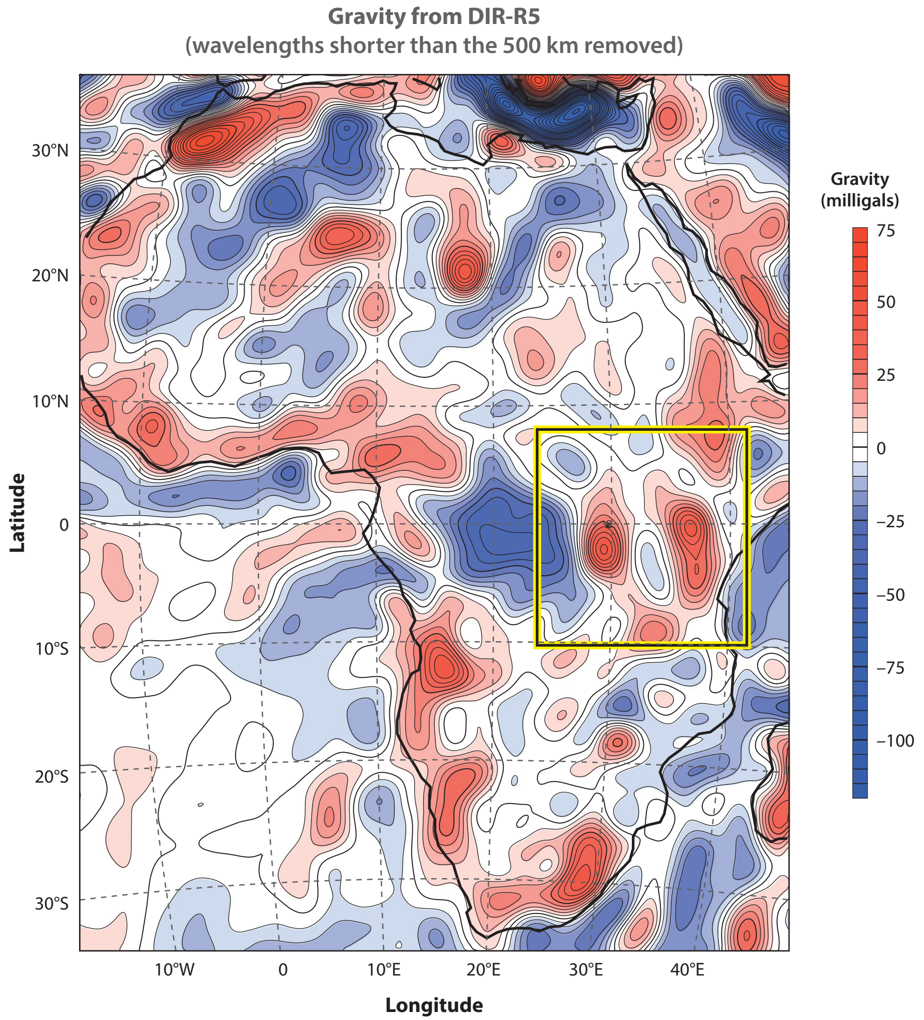

Figure 3

Gravity field for Africa from DIR-R5 filtered as in **Figure 5*c***, whose boundary is shown by the black and yellow box.

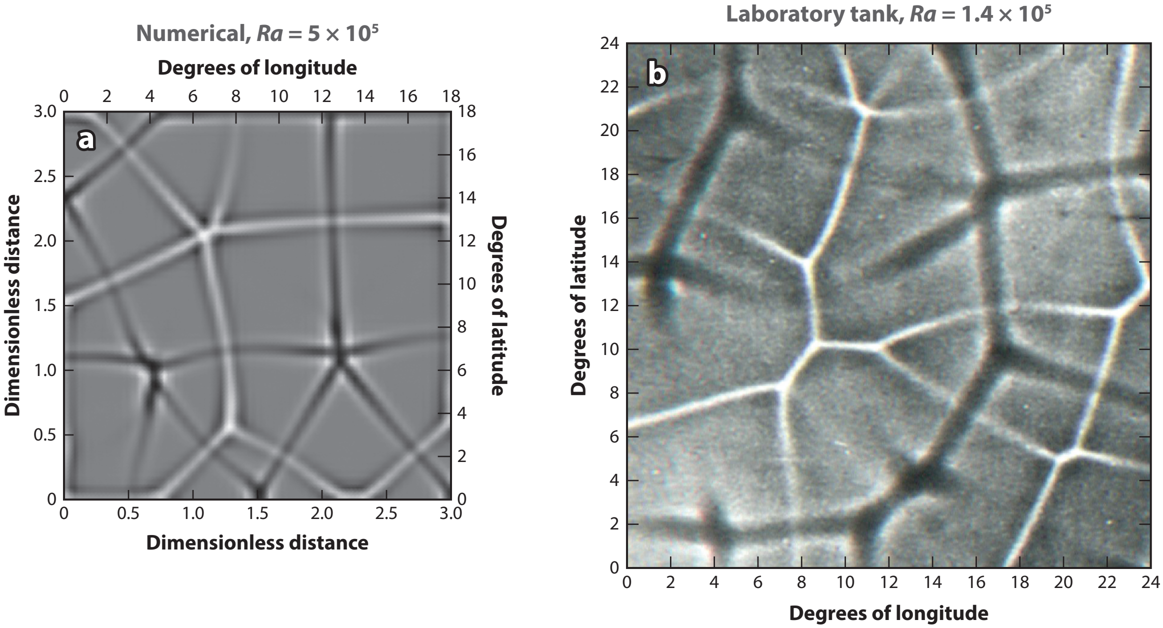

Figure 4

(*a*) Calculated and (*b*) observed shadowgraphs. The dark lines are hot rising regions and the bright ones cold sinking ones. Panel *a* is from a numerical solution using an aspect ratio 3 box with rigid upper and lower boundaries, kindly provided by John Rudge. The Rayleigh number is 5×10^5 and the shadowgraph was generated by applying the Laplace operator to the vertically averaged temperature. Panel *b* is from a tank experiment (see Richter & Parsons 1975, figure 9), using part of a photograph kindly provided by Barry Parsons. Both have been scaled to a layer depth of 670 km.

layer becomes unstable. But little effort has been made to use numerical experiments to discover what constraints the observations impose on models of mantle convection.

The rapid growth of computing power now allows properly resolved high-Rayleigh-number calculations to be carried out on relatively modest clusters of processors. My frustration with the way mantle convection studies have developed has caused me to encourage John Rudge to carry out straightforward three-dimensional calculations in large-aspect-ratio boxes at high Rayleigh numbers. One of the first such calculations he has carried out is illustrated in **Figures 4** and **5**, which show the shadowgraph, gravity field, and surface deformation generated by constant-viscosity convection with a Rayleigh number of 5×10^5 in an aspect ratio 3 box, scaled to a layer depth of 670 km, with rigid boundaries. **Figure 4*b*** also shows a shadowgraph image from a tank experiment carried out by Richter and Parsons (1975) at a Rayleigh number of 1.4×10^5. As expected from the tank experiments, the numerical solution shows the convective planform consists of a spoke pattern, with hot and cold plumes joined by hot and cold sheets. It is straightforward to use the numerical solution to calculate the gravity field on the upper surface of the layer, scaled to a layer depth of 670 km, though both the gravity field and the surface deformation are strongly affected by the presence of the plate on top of the convecting layer. **Figure 5** compares the expected gravity, measured at the surface of a plate whose thickness is 100 km and whose elastic thickness is 30 km, with that from central East Africa. The resulting gravity field now no longer reflects the details of the convective planform. Instead it consists of approximately circular positive and negative anomalies like those in Africa. The horizontal scales of the observed and

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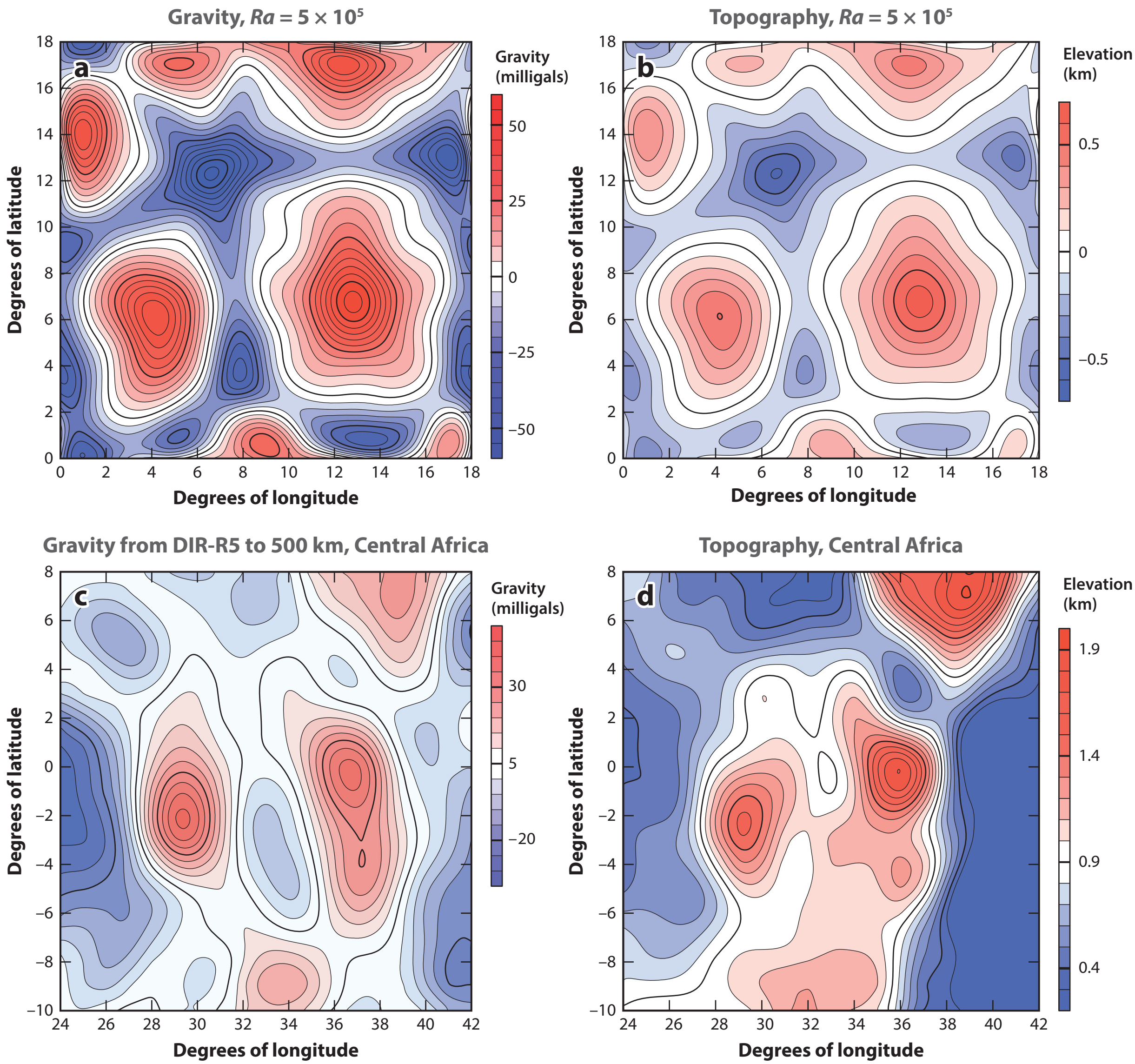

Figure 5

(*a*) Surface gravity field and (*b*) deformation calculated from **Figure 4*a*** when the convecting region is overlain by a plate of thickness 100 km with an elastic thickness of 30 km (McKenzie et al. 2014). (*c*) Gravity field over Central Africa from DIR-R5 (Bruinsma et al. 2014), band-pass filtered to remove wavelengths shorter than 500 km and longer than 4,000 km. (*d*) Topography of Central Africa low-pass filtered to remove wavelengths shorter than 300 km.

calculated anomalies in **Figure 5** are similar, and suggest that spoke pattern is the planform of mantle convection. The comparison of **Figure 4*a*** with **Figure 5*a*** shows how difficult it is to "see" through the plates without help of numerical experiments. John and I plan to carry out a number of three-dimensional numerical experiments to examine what constraints the observations impose on the nature of mantle convection.

4. MISTAKES (OF MINE AND OTHERS)

The type of research that interests me is at the edge of what we understand. Though I try to avoid mistakes, and setting off in wrong directions, it is inevitable that this will sometimes happen. In general, mistakes by myself and others divide into two groups. Some are caused by ignorance: something that is understood by others but not by you. Mistakes of this type are the most embarrassing. The other type of mistake is one that only becomes clear when more relevant data become available. It is this type of mistake that I think is unavoidable. However, if one makes a mistake of this type it is in my view important to admit to having done so in print, especially if the relevant paper has been influential. Three examples of such mistakes, two by me and one by Don Forsyth, illustrate the issues involved.

My first example is a paper by myself and John Sclater (McKenzie & Sclater 1968), on the heat flow through back-arc basins. John made a number of measurements in the back-arc basins of the western Pacific, in regions where the age of the sea floor was reasonably well known. The resulting heat flow was considerably greater than that through normal oceanic lithosphere of the same age, where there were already a large number of measurements. We therefore argued that there was some mechanism that increased the heat flow through back-arc basins. Though this proposal seemed obvious, it was wrong. The explanation of the difference was that the estimates of the heat flow through normal oceanic lithosphere were wrong, even though they were based on hundreds of observations, because they did not include advective heat transport by water circulation through the sea floor. Because of the sediment blanket, such transport is less important in back-arc basins. So it was the low heat flow through the sea floor of normal oceans that needed an explanation, not the high heat flow behind island arcs. Our mistake became obvious when black smokers were discovered, and caused no serious trouble. Sadly the same is not true of the other two mistakes.

The first of these concerns a paper Greg Houseman, myself, and Peter Molnar (Houseman et al. 1981) wrote on convective removal of lithosphere. We argued that this process occurs after continental shortening. This paper has been cited more than 700 times, but I, though not, I think, my two coauthors, now believe it is quite wrong. We argued that the Tibetan Plateau was formed by shortening and that it has elevated heat flow (I still believe both statements). We then carried out some numerical experiments on the evolution of a convective boundary layer when it is suddenly shortened. As expected, we found that it is unstable and is rapidly removed by convection. This result I also still believe. But I no longer think that this experiment is relevant to the present thermal structure of Tibet, because surface-wave tomography (a technique that did not exist in 1981) now shows that the lithosphere beneath Tibet is 200–250 km thick and has not been convectively removed. The composition of mafic magmas erupted on the plateau shows that their source regions do not consist of ordinary upper mantle material, but material that has had $\sim$25% removed by melting. Like surface-wave tomography, the method of obtaining the source density from the melt composition had not been developed in 1981. The residue is considerably less dense than ordinary upper mantle material. Therefore the explanation of Houseman et al.'s (1981) mistake is that they assumed the density was controlled by temperature alone, whereas beneath Tibet the density is also strongly affected by the composition. As a result the lithosphere is less dense than the underlying undepleted upper mantle, even though it is colder. The process that Houseman et al. modeled may well occur elsewhere where lithosphere is shortened, but I do not believe it is responsible for the structure of Tibet (McKenzie & Priestley 2016).

In the last example the mistake was not mine but Don Forsyth's, and concerns his and his colleagues' estimates of the elastic thickness, T_e, in old continental shields. I think it is a particularly interesting example of the trouble mistakes can cause, and how difficult it is to correct them. In oceanic regions the usual method of estimating T_e is by using the relationship between the free-air

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gravity anomalies, now generally obtained from satellite altimetry (Sandwell & Smith 2009) and bathymetry. The limitation of this approach is that there are few areas of the oceans where there is good two-dimensional coverage of the bathymetry. Where there is, for ridges and for intraplate regions like Hawaii, the estimated values of T_e are less than the seismogenic thickness of the lithosphere. This result is to be expected, because the elastic stresses associated with bathymetry must be supported for Ma, whereas those associated with earthquakes need only to be supported for the length of the earthquake cycle, or ka. Furthermore estimates of both T_e and T_s are less than the depth of the 600°C isotherm (see Section 2). In continental regions the gravity field is commonly reported as a Bouguer, rather than a free-air, anomaly. This convention has a long history, and is in my view unfortunate, because Bouguer gravity anomalies are dominated by variations in crustal thickness, whereas free-air anomalies are largely controlled by variations in T_e, which depends on the elastic properties of the lithosphere. The standard approach that is widely used to estimate T_e in continental regions was proposed by Don Forsyth (1985) and uses Bouguer anomalies. It depends on the correlation between topography h and Bouguer gravity g_b in the spectral domain. At short wavelengths topography is uncompensated. Therefore, if the gravitational attraction of the topography is removed from the observed gravity anomalies by calculating the Bouguer anomaly, g_b should be independent of h and the two should be incoherent. However at long wavelengths compensation occurs, and the free-air gravity anomaly is approximately zero. The resulting Bouguer anomaly is then the inverse of the topography and is clearly coherent with it. Forsyth used this idea to obtain the relationship between the coherence γ_b^2 and T_e in the spectral domain, where γ_b^2 is the coherence between the Bouguer gravity and topography. When he and his colleagues applied this approach to North America they obtained values of T_e of 100 km or more for regions underlain by shields. I found such values puzzling for a number of reasons. Earthquakes beneath shields are confined to the crust almost everywhere, and have depths of less that about 50 km. Values of T_e of $\sim$100 km were therefore much greater than T_s. Furthermore temperature estimates from peridotite nodules brought up by kimberlites through shields show that the temperatures at depths corresponding to the estimated values of T_e are $\sim$1,000°C, or about twice those at the base of the elastic layer in oceanic regions. A final problem was that people I knew, like David Kohlstedt, who carried out laboratory experiments on peridotites, told me that it only took about half an hour for elastic effects to be relaxed at temperatures of $\sim$1,000°C. So clearly there was a problem somewhere.

When I was a graduate student my supervisor Teddy Bullard warned me never to get involved in a scientific controversy. He told me it never did any good, and that it was impossible to convince the scientific community that you were correct. In his view the only result was that no one believed what either of the scientists involved wrote. Though I feared this is what would happen if I tried to sort out the problem with T_e in the continents, I thought the whole issue was so fundamental to our understanding of continental rheology that I should try, at least for my own understanding, even if I could not convince others that I was correct. Though I suspected that the problem lay with Forsyth's method, I could not be sure without understanding exactly why his estimates of T_e were so large. If his estimates were correct, the problem then had to be with our understanding of solid-state rheology, and especially with the role of homologous temperature $\theta = T/T_s$. Materials scientists like Mike Ashby have used the idea of homologous temperature to show that the rheological behavior of most materials can be described using deformation maps based on θ (Frost & Ashby 1982). I had made extensive use of this work in thinking about the rheology of the lithosphere and the convecting mantle. If Forsyth's estimates were correct, there was something fundamentally wrong with the way I, and many others, were thinking, and the ideas which worked so well for most other materials, both metallic and nonmetallic, did not work in the mantle. I quickly became convinced that Forsyth's method only provides an estimate of T_e when

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the topography is rough. The problem with shields is that they generally have little topography, though they often have large gravity anomalies. When this is the case Forsyth's method provides an upper bound, not an estimate, of T_e. There is therefore no conflict between the values of T_e from Bouguer coherence and the other constraints we have on crustal and mantle rheology. What I thought was even more important was that there was no reason to believe that the mantle behaved differently from other materials.

After several attempts I finally found a simple way of looking at this problem in the spectral domain (McKenzie 2016). The power spectrum of topography in a wavenumber band from k to $k + \Delta k$, where $k = 2\pi/\lambda$ and λ is the wavelength, is $< \overline{h}^{\star}\overline{h} >$, where the angle brackets denote the averaging over the annulus k to $k + \Delta k$, the bars the Fourier transform and * the complex conjugate. In many shields the free-air gravity g_f is incoherent with the topography, because the surface is so flat. When this is the case

$$< \overline{g}_f^{\star}\overline{h} >= 0. \qquad 3.$$

The Bouguer anomaly is approximately

$$g_b = g_f - Ah \qquad 4.$$

where A is a constant. When the topography is measured in meters and the gravity in milligals $A = 0.11194$. The Bouguer coherence $\gamma_b^2(k)$ is given by

$$\gamma_b^2(k) = \frac{< \overline{g}_b^{\star}\overline{h} >^2}{< \overline{g}_b^{\star}\overline{g}_b >< \overline{h}^{\star}\overline{h} >}. \qquad 5.$$

If g_f is incoherent with h, substitution of Equations 3 and 4 into Equation 5 then gives

$$\gamma_b^2(k) = \frac{1}{1 + R(k)} \qquad 6.$$

where

$$R(k) = \frac{< \overline{g}_f^{\star}\overline{g}_f >}{< A\overline{h}^{\star} A\overline{h} >} \qquad 7.$$

is the ratio of the power of the free-air gravity to that, Ah, of the uncompensated topography. Clearly, if the shield is flat, $R(k)$ is large at short wavelengths and γ_b^2 is small. At long enough wavelengths the topography is always compensated, when g_f tends to zero though h does not. Therefore, as $\lambda \to \infty$, $R(k) \to 0$ and $\gamma_b^2 \to 1$, even though there is no relationship between the gravity and topography. **Figure 6*a*** shows the observed values of γ_f^2 for a box covering central and Western Australia. There is essentially no coherence between the free-air gravity and the topography (the bands where $\gamma_f^2(k) > 0$ actually have elevated topography correlating with negative free-air gravity anomalies). **Figure 6*b*** shows observed values of $\gamma_b^2(k)$ compared with those calculated from Forsyth's approach with $T_e = 90$ km. Though the free-air gravity and topography are incoherent and unrelated to each other, Forsyth's method generates a large value of T_e of 90 km. As **Figure 6*c*** demonstrates, this estimate is meaningless. This argument shows that it is not possible to use gravity and topography to estimate T_e when the free-air gravity is incoherent with the topography, as it is in the shield regions that give large estimates of T_e. So there is no difficulty in reconciling the results from Forsyth's approach with our understanding of rheology. For shields his method simply provides an upper bound, not an estimate, of T_e.

I was pleased to have found such a simple way of understanding what had happened, and submitted a paper explaining this argument to *Earth and Planetary Science Letters* (EPSL). I suggested to the editor, Peter Shearer, that he should send the paper to Don Forsyth as a referee, since I thought my argument would finally bring the controversy to an end.

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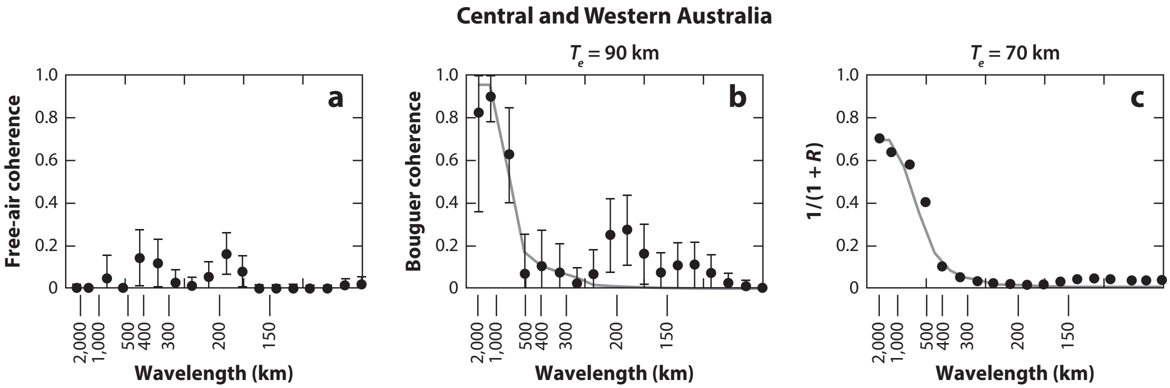

Figure 6

(*a*) The coherence between the free-air gravity anomaly and topography for a box covering parts of central and Western Australia (McKenzie 2016). (*b*) The same plot for the Bouguer anomaly. The line shows the values of the Bouguer coherence calculated for an elastic thickness of 90 km using Forsyth's (1985) expressions. (*c*) The same as for panel *b* but assuming that the free-air gravity and topography are unrelated, and that their coherence is zero.

How wrong I was! Forsyth recommended that the paper should be rejected, and the editor accepted his view on the grounds that Forsyth's paper had originally been published in the *Journal of Geophysical Research* (JGR) and therefore I should submit my paper to JGR and not EPSL. Fortunately the other referee, Frederik Simons, was the editor of the *International Journal on Geomathematics*, and suggested I submit it there. I did so, and the paper has now been published (McKenzie 2016), though I doubt it will be widely read by geophysicists. So the controversy is (sadly) likely to continue.

These three examples strongly confirm what Teddy told me. In the case of heat flow behind island arcs the paper was ignored and therefore did little harm. The same is not true of Houseman et al. (1981) which is still being widely referenced. Even in retrospect it is not clear to me how we could have known that the idea was wrong when we did the work in 1981, so I am embarrassed but don't think I was stupid. The continental T_e controversy is more complicated. It took me a long time to find the answer (I thought at first that it was a signal processing issue), and clearly remember the first seminar I gave on the subject at Cambridge, when James Jackson introduced me by saying that, other than my colleagues in Cambridge, I was the only person who believed what I was going to say. Despite my failure, I still think it is important to attempt to sort out mistakes, and to acknowledge them when they are my own.

5. STARTING AND STOPPING

My undergraduate training was largely in mathematical physics, quantum mechanics and statistical physics. I was taught a little geology in my first year; mostly stratigraphy, sedimentology and paleontology, none of which I have used professionally. What instead I needed was fluid dynamics, earthquake seismology, petrology and geochemistry. When I became a graduate student in 1963 Teddy suggested that I should work on the equation of state of the mantle, and especially of the lower mantle. His idea was to use the velocities of P and S waves, V_p and V_s, as the two thermodynamic variables, and write the pressure P and temperature T as $P(V_p, V_s)$ and $T(V_p, V_s)$. This now seems to me a very ambitious and difficult initial project for a beginning graduate student. I used a Lennard-Jones type potential, and determined the relevant constants

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using laboratory experiments. The approach worked quite well for the pressure, but not for the temperature. I wrote up what I had done as an application for a fellowship at King's College, who used Don Anderson as a referee. King's gave me a fellowship, and I have always been grateful to them for doing so. But I did not think the work was publishable. Curiously I have recently returned to this problem, and have produced empirical expressions that can calculate temperature from the shear wave velocity (Priestley & M^{c}Kenzie 2006, 2013). However, the approach I now use is quite different from that which I tried in 1964.

I then wanted to work on a more topical problem. In a monograph on the rotation of the Earth, Walter Munk and Gordon MacDonald (Munk & MacDonald 1960) argued that the equatorial bulge of the Earth exceeds that expected for its present rotation rate (which is true), and that it was essentially a fossil bulge resulting from tidal deceleration. They used this argument to estimate the viscosity of the mantle, which they found was too great to allow convection. I disagreed, and believed that a high-viscosity shell somewhere in the mantle was all that was required. I taught myself enough materials science to know that all materials at high enough temperatures creep, and that such creep can be described by a linear relationship between stress and stain rate. I also learned enough fluid mechanics to tackle the problem, which I wrote up for a PhD. I now think the whole enterprise is likely to be incorrect, because there is nothing special about the nonhydrostatic bulge; it is simply part of the Earth's gravity field that is maintained by mantle convection. But the materials science and fluid dynamics I learned provided exactly the background I needed later. This was the first time I changed fields. Whenever I have done so later I have found it is straightforward to learn what I need from various excellent undergraduate textbooks. What is harder, and I think is absolutely essential, is to learn how to think in the same way as do the scientists who work in these fields. They generally think in the way they do partly because of problems they wish to solve and partly because of the available technology. Once I became well known I found that I was able to work closely with people with the relevant backgrounds, who then taught me how to think as they did. I also always tried to go into the field (or to sea) with them when they were making the primary observations, and when they also had time to teach me to think as they did. But there was only any point in my becoming involved if I wanted to do something new that was not already understood.

My research career has been governed by observations, and mostly those made by others. I divide them into four groups. Some are simply wrong: not because they disagree with my expectations, but because they are simply incorrect. Some are concerned with problems I think I already understand. Others are concerned with phenomena that are so complicated that I am never going to understand them. But at any time there are a small number of observations that I am sure I cannot understand with our existing ideas, and they are the ones that interest me. Typically such problems become tractable because of advances in technology, which often suddenly open up areas of great interest. During the last fifty years many of those people who have made important advances in our understanding have done so by being early exploiters of new technological methods of making measurements. Sadly much research effort falls into my second group. Drum Matthews used to dismiss this as "me-too science," meaning that all they did was to find something in their own data or field area that was already well known to others. He was also very rude about people who spent their whole career working on their PhD problem. He often used to tease me by saying that I was one of these people, which is certainly true but (in my view anyway) a little unfair. Modern computers have led to another sort of research which I refer to as "what if": If I assume *a* is true what can I then say about *b*, *c*, ... The problem with this approach is, I think, well illustrated by plate tectonics. No one would believe that spherical caps as large as 10,000 km, whose elastic thickness is only about 20 km, could move as rigid caps on the Earth without internal deformation

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unless there was excellent observational evidence that they do so. Nor did anyone suggest that the elements with which we are so familiar were synthesized inside stars, or by huge stellar explosions, before the observational evidence became overwhelming. Many important advances, especially in the physical sciences, involve ideas that are so fundamentally improbable and bizarre that no one thinks of exploring their consequences until forced to do so by observations. The other type of modeling that has become popular as a result of the great advances in computing is "kitchen sink modeling," which involves repeating a calculation with yet another complication included in the code and writing yet another paper describing the results, irrespective of whether there is any observational evidence that the particular new complication has important effects.

6. RESEARCH SUPPORT

About fifteen years ago the departments of physical sciences in Cambridge decided to increase the length of the undergraduate courses from three to four years, and to allow a research project in the fourth year to count for up to 40% of the final degree marks. The success of this change was dramatic: Bright students given a worthwhile and well thought out project sometimes produce a project report in eight weeks that is as good as many PhD theses, and many develop a profound liking for scientific research. The clever ones then go on to take a PhD, often followed by one or more periods as a postdoc. Scientific research is enormously attractive to the bright young. But scientific research has to be paid for by taxes. When I was a graduate student the financial support for scientific research both in the United Kingdom and the United States was increasing rapidly, so rapidly that Teddy Bullard once remarked to me that it would consume the whole gross national product of both countries in ten or twenty years' time if the rate of increase continued. Obviously it could not and did not. The effect of the great expansion of postdoctoral support, with no similar expansion in the number of tenured staff at research universities, has led to a number of unintended consequences. Many postdocs have to take one such job after another, but have little chance of ever obtaining a permanent academic position. They are often very able, and would have had excellent nonacademic careers (I particularly wish they would go into politics). When they finally, and miserably, accept their fate it is often too late for them to change career successfully. The other problem is that postdocs have the time and opportunity to write endless grant proposals, often to carry out "me-too science." When grants committees can only support a small fraction of the grant applications, they (understandably) become very conservative and risk-averse. They then have a strong tendency to support "me-too science" rather than more risky and more innovative proposals. There is no way I could have supported the true cost of my research by applying for grants. All I could have written in the proposals would have been "I know nothing about this problem, but I think I may be able to do something interesting if you support me." I would never have been funded. Fortunately my job at the University of Cambridge paid my (twelve month) salary while I learned new subjects, and the Department of Earth Sciences has a policy of using its studentships to support the best applicants. So, in exchange for teaching undergraduate courses to carefully selected clever undergraduates, almost the entire cost of my research was paid for by others. I often felt like the eighteenth-century naturalist the Rev. Gilbert White of Selborne or the nineteenth-century geophysicists the Rev. Osmond Fisher and Archdeacon Pratt, all of whom were supported by the Church. I am profoundly grateful that a similar support structure still exists (and that it is no longer tied to the Church!). To me the important feature of such support is that it does not require the recipients to justify what they intend to do. If they are successful, everyone is happy to leave the system unchanged. It works because of the competitive nature of scientists, which is striking even among undergraduate scientists.

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7. CONCLUSIONS

Like many well-known astronomers in the twentieth century, I have made my career by applying physics and new technology to an existing area of science. When I became a graduate student in 1963 the application of physics to the Earth sciences was principally the concern of geophysics, and involved using ideas from physics to make measurements. What has changed is that physics now lies at the heart of our understanding of geological processes. The structure of modern research universities and the economics of modern Western states have supported me throughout my career. As a graduate student I thought I was a good enough Earth scientist to be able to make my career in the subject. The impact of the two papers I published in 1967, which were almost the first two papers I wrote, has astonished me. My early work on plate tectonics is now little referenced; it has become textbook material. But about three people a day are now referencing papers of which I am one of the authors, a number that I find extraordinary. As a young scientist I never dreamed of having such an impact.

The curtain will soon fall after fifty years, which makes me sad. I am not likely to be around for the next major advances, one of which I think is going to involve using technological advances in geochemistry and astronomy to understand how planetary systems form. There has already been important progress (Burkhardt et al. 2016, Bouvier & Boyer 2016). The arguments in these two papers clearly illustrate the importance not just of understanding the geochemical arguments involved, but also of thinking like a geochemist!

DISCLOSURE STATEMENT

"If the author has nothing to disclose, the following statement will be used: 'The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.'" I very much doubt whether I can honestly make this statement. I don't believe that my views have remained uninfluenced by having been a faculty member of the University of Cambridge for fifty years, and expect those who read what I written, and whose careers have not been in research universities, will agree that my membership of this university has affected the objectivity of this review, even though I have tried not to let it do so. But what you see is what you get: There is nothing I need to declare which is not completely obvious from my affiliation and what I have written.

ACKNOWLEDGMENTS

Scientists do not work well in isolation, and I am no exception. My career has been profoundly affected by being surrounded by able colleagues and students, many of whom have become close friends, at Cambridge and at the US universities where I have spent sabbaticals. The principal financial support for my research came initially from King's College and the US Air Force, and later from the University of Cambridge, the Natural Environment Research Council and the Royal Society, and now from the Universities Superannuation Scheme.

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RELATED RESOURCES

The Geological Society of London has recently set up an archive concerned with the discovery of plate tectonics, which among other things contains Dan McKenzie's personal correspondence, scientific notes and calculations (see **https://www.mckenziearchive.org/**).

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Annual Review of Earth and Planetary Sciences

Volume 46, 2018

Contents

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