Plate Tectonics, the Wilson Cycle, and Mantle Plumes: Geodynamics from the Top

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Abstract

By 1968, J. Tuzo Wilson had identified three basic elements of geodynamics: plate tectonics, mantle plumes of deep origin, and the Wilson Cycle of ocean opening and closing, which provides evidence of plate tectonic behavior in times before quantifiable plate rotations. My pre-1968 experience disposed me to try to play a part in testing these ideas. Most recently, with colleagues, I have been able to show that deep-seated plumes of the past $\sim 5.5 \times 10^8$ years have risen only from narrow plume generation zones (PGZs) at the core-mantle boundary (CMB) mostly on the edges of two Large Low Shear wave Velocity Provinces (LLSVPs) that have been stable, antipodal, and equatorial in their present positions for hundreds of millions of years and perhaps much longer. A need now is to develop an understanding of Earth that embodies plate tectonics, deeply subducted slabs, and stable LLSVPs with plumes that rise from PGZs on the CMB.

WHAT WILSON KNEW

J. Tuzo Wilson wrote that "...these features [mountains, mid-ocean ridges, and transform faults]...are connected into a continuous network of mobile belts about the Earth which divide the surface into several large rigid plates" (Wilson 1965a, p. 343). Wilson thus succinctly defined the nature of plates and plate boundaries as well as indicating the continuous motion of rigid plates with respect to one another, but his version of plate tectonics was flawed because it was on a flat Earth (see sidebar, Plate Tectonics). He once expressed his regret to me, joking that if he had spent more effort trying to understand Harold Jeffreys's lectures and less time rowing, he might not have made that error. The power of describing the motions of the rigid plates as rotations across Earth's spherical surface became clear when it was shown that defining poles to describe relative plate motion requires knowing some or all of spreading center azimuths, magnetic anomaly azimuths, and transform fault or fracture zone azimuths (McKenzie & Parker 1967, Morgan 1968, Le Pichon 1968). For that reason, plate motion cannot be analyzed for times older than the age of the world's oldest surviving ocean floor. The question then arises: How can the geodynamic history of Earth be described for times before the oldest preserved ocean floor was formed at ~180 Mya?

Wilson again took the lead. He wrote: "If continental drift has been going on for an appreciable part of geological time...it means that a succession of ocean basins may have been born, diminished and closed again. Since ocean basins are the largest features of the earth's surface and would dominate other features it seems useful to outline the stages in their life cycle in terms of present examples. This makes it apparent that each stage has its own characteristic rock types and structures as outlined in Table 1" (Wilson 1968, p. 312). **Figure 1** is reproduced from Wilson's version of his 1968 table 1 published in Jacobs et al. (1973). It shows the stages that Wilson recognized in what soon came to be known as the Wilson Cycle of ocean opening and closing (Burke & Dewey 1974).

Wilson has lately received some bad press suggesting that he did not recognize the existence of large rigid plates and that his idea of a life cycle for ocean basins envisaged no more than a single continent rupturing and later reuniting. However, neither interpretation is consistent with what Wilson wrote in 1965 (Wilson 1965a), 1968 (Wilson 1968), and 1973 (Jacobs et al. 1973) or with **Figure 1**. There has been interest in a suggested Supercontinent Cycle as an improvement on the Wilson Cycle, but because continental collisions are an inevitable part of the Wilson Cycle, because only one supercontinent can be shown to have existed in the past (Burke 2007), and because in Wilson's words "oceans...would dominate," that does not seem a very good idea. Wilson's (1963) other success in defining the fundamental elements of geodynamics was that he was the first to point to evidence for deep mantle plumes (see sidebar, Mantle Plumes). Current research

PLATE TECTONICS

The word tectonics, from a Greek word for builder, is used to characterize the large-scale evolution of planetary lithospheres. On Earth, that evolution is dominated, and probably has been since the moon-forming event, by plate tectonics involving the rotation of horizontally extensive rigid plates of lithosphere across Earth's surface. Mountain building and igneous activity are concentrated close to narrow plate boundaries in the ocean, but broader plate boundary zones are prominent within continents and volcanic arcs, which are made of material weaker than that forming oceanic lithosphere.

Stage	Examples	Dominant motions	Characteristic features	Typical igneous rocks	Typical sediments	Metamorphism
1. Embryonic	East African Rift Valleys	Uplifts	Rift valleys	Tholeiitic flood basalts, alkalic basalt centers	Sedimentation minor	Negligible
2. Young	Red Sea, Gulf of Aden	Spreading	Narrow seas with parallel coasts and central depression	Tholeiitic flood basalts, alkalic basalt centers	Shelf and basin deposition; evaporites possible	Negligible
3. Mature	Atlantic Ocean	Spreading	Ocean basin with active mid-ocean ridges	Tholeiitic flood basalts, alkalic basalt centers but activity concentrated at center	Abundant shelf deposits (miogeosynclinal)	Minor
4. Declining	Pacific Ocean	Shrinking	Island arcs and adjacent trenches around margins	Andesites, granodiorites at margins	Abundant deposits derived from island arcs (eugeosynclines)	Locally extensive
5. Terminal	Mediterranean Sea	Shrinking and uplifts	Young mountains	Volcanics, granodiorites at margins	Abundant deposits derived from island arcs (eugeosynclines) but evaporites possible	Locally extensive
6. Relic scar (geosuture)	Indus Line in the Himalayas	Shrinking and uplifts	Young mountains	Minor	Red beds	Extensive

Figure 1

The Wilson Cycle of the opening and the closing of the ocean basins. Plate tectonic analysis, which requires rigid-body rotations, is not possible for times before the age of the oldest preserved ocean floor (~180 Mya), but the past operation of plate tectonics can be discerned in associations of rocks and structures representative of various intraoceanic and ocean margin environments. This figure, which identifies those associations, is copied from Wilson's slightly revised version of his originally published (1968) table that appeared as table 15-1 in the second edition of *Physics and Geology* (Jacobs et al. 1973). Copyright of the McGraw-Hill Companies, reproduced with permission.

is confirming the existence of plumes from the core-mantle boundary (CMB) and is revealing a great deal about them.

I have tried to set the record straight because reading Wilson's 1965 paper (Wilson 1965a) and later his 1963 paper (Wilson 1963) gave my career a new and happier direction and because Wilson's ideas have provided a continuing stimulus for me. Here I emphasize how plate tectonic theory and Wilson Cycle concepts, which show that plate tectonics has operated throughout recorded Earth history, have continued to prove fruitful over more than 40 years. In addition, I

MANTLE PLUMES

A mantle plume is a buoyant object in the mantle typically no more than a few degrees across in horizontal dimensions. Mantle plumes have long been considered to exist from evidence at Earth's surface (in hotspot volcanoes, for example) but have only recently been mapped within the mantle by Raffaella Montelli and her colleagues (2004, 2006).

summarize how recent results (e.g., Burke et al. 2008b) call for an integrated model in which the eruption of plumes from a restricted environment at the CMB and the complementary operation of plate tectonics, which sends subducted slabs and slab debris into the deep mantle, have been the phenomena that have overwhelmingly dominated the geodynamics of the solid Earth since the planet settled down after the moon-forming event.

PLATE TECTONICS: A CAREER-CHANGING THEORY

In 1968 I was working at the University of Ibadan in Nigeria, and from my research on the Niger delta (Burke 1972), I had developed an interest in West African coastal structure and its evolution. That led me to a paper by Lynn Sykes about the giant fracture zones of the equatorial Atlantic (Sykes 1967). Reading Sykes's paper changed my life. It dealt, not as I had hoped, with regional coastal structure, but with the mechanisms of earthquakes on the faults that offset the mid-Atlantic ridge. Wilson had recognized faults offsetting oceanic spreading centers as examples of the new class of faults that he termed transform faults because they transform motion with a substantial vertical component—at spreading centers, for example—into predominantly horizontal motion on the ocean floor. Wilson ended his great 1965 paper with this plea: "It is particularly important to [investigate the validity of the concept of transform faults further] because transform faults can only exist if there is crustal displacement and proof of their existence would go far towards establishing the reality of continental drift and showing the nature of the displacements involved" (Wilson 1965a, p. 347). Sykes responded nobly to Wilson's plea by establishing the first motion directions of earthquake mechanisms on the giant transform faults of the Gulf of Guinea (see sidebar, Earthquake First Motion). Sykes's results were the first to demonstrate how transform faults operate. He showed that Wilson had got it right because the alternative idea required first motion directions opposite to those he observed.

I recall being so excited by my first reading of Sykes's paper that I ran about 100 m to the nearest library to check what Wilson had written. Much of what I have attempted in the past 40 years follows from reading Sykes's and Wilson's papers on that day. One immediate result was an interest in the seismicity of the coast of West Africa that I found to be concentrated at Accra in Ghana and at Kribi in Cameroon; both are places where fracture zones reach the continental margin (Burke 1969, 1971).

A more general consequence of reading Wilson's 1965 paper establishing plate tectonic theory was that it showed me that one result of the operation of plate tectonics is that oceans are opening in some parts of the world today and closing in others. I inferred that the history of Earth's surface recorded within continents must, on the grandest scale, be the record of just those processes. It was not until four years later in 1972 that I discovered that Wilson had himself published, admittedly in a rather obscure place, his own elegant and comprehensive way of handling that idea (**Figure 1** and Wilson 1968).

EARTHQUAKE FIRST MOTION

The first wave to reach a seismic station from an earthquake can be treated as a compression or a rarefaction. Compiling first motions from a single, sufficiently large earthquake on the global scale shows which quadrants of the globe first received compressions and which received rarefactions, revealing the sense of movement on the responsible earthquake fault. The sense of motion on the transform faults of the Gulf of Guinea was opposite to that which a fault of the then-familiar transcurrent type would have experienced.

GREENSTONE BELTS

A greenstone belt is typically a body of rock hundreds of kilometers long and tens of kilometers wide, consisting mainly of highly deformed and steeply inclined basalt and serpentinite with associated sedimentary rocks including greywackes and cherts. Many greenstone belts are more than 2.5 Ga in age, and most have been metamorphosed to low grade to form green minerals such as chlorite and actinolite.

CONTINENTAL ASSEMBLY

An early fruit of my realization that the continents of Earth contain a record of oceans opening and closing was that it provided an explanation for the resemblance between the rocks in the Caribbean island arc that I had known while living and working in Jamaica between 1961 and 1965 and those of the greenstone belts of the Archean and Proterozoic ages that I had known from my work in Africa, first at the University of Ghana between 1953 and 1956 and then in Zimbabwe while working with the British Geological Survey from 1956 to 1961 (see sidebar, Greenstone Belts).

The basaltic pillow lavas, andesites, granodiorites, serpentinites, greywackes, and cherts as well as the low-grade metamorphism that I knew in the Greater Antilles closely matched most of the rock types that I knew in the Precambrian greenstone belts that were at least 2 Ga older (see sidebar, Pillow Lavas). I inferred that rocks of the ocean floor, formed at spreading centers, and rocks of island arcs, formed at convergent plate boundaries, are juxtaposed in greenstone belts as a result of the closing of large or small oceans. Greenstone belts lie among and are intruded by granodioritic intrusive rocks with isotopic ages close to those yielded by the volcanic rocks of the belts. I concluded, as did others, that the tectonic environment represented by Archean cratons is that of the assembly of island arcs to form continents (e.g., McKenzie & Weiss 1975). Some students of Archean geology have concluded and continue to conclude, mainly on inferences from perceived compositional differences, that rocks of Archean age (2.5 Ga to 4.0 Ga) differ from later rocks to a degree that warrants the conclusion that plate tectonics did not operate in those ancient times. Others (e.g., Harrison 2009) prefer the idea that plate tectonics has always operated and that the differences of ancient rocks can be explained by recognizing the breadth of conditions and rock types that may develop in the plate tectonic environment. I prefer the latter approach because it lends itself to progress. Plate tectonics can be shown from the geological record to accommodate great diversity and complexity as well as to result in the destruction of much of the record of what has happened (see, for example, Şengör & Natal'in 1996b). Abandoning plate tectonic theory has the drawback that it leads to alternative suggestions for which there is no modern analog, such as the postulated existence during intervals in the past of a "plume-dominated" Earth and the idea that plate tectonic processes were, at times, temporarily suspended (Condie et al. 2009). Tested against observations of the modern world, those ideas fail.

PILLOW LAVAS

Pillow lavas are spherical, tubular, and pillow-shaped bodies of volcanic rock typically a meter in diameter and usually of basaltic composition that form as a result of volcanic eruption underwater. Pillow lavas are widespread on the ocean floor and in volcanic arcs. They are common in greenstone belts.

GREENSTONE BELTS

Applying the ideas of plate tectonics to greenstone belts has engaged my interest intermittently since 1968 (e.g., Burke et al. 1976). I knew, from walking outcrops on 1:100,000 scale maps of the Rhodesian Geological Survey and from detailed maps of the world's great mining camps such as Abitibi and Kolar, that greenstone belts are places of extreme structural complexity, and that because they contain no diagnostic faunas they are without stratigraphic control. It was therefore not surprising to find greenstone belts tectonically emplaced as isolated slivers within vast areas of gray granodioritic gneiss. Nor was I surprised that the ages of those gneisses were younger than the ages of the greenstone belts because I knew Paleocene (~63 Mya) granodiorites of comparable composition that cut Cretaceous (~70 Mya) volcanic arc rocks in Jamaica (Chubb & Burke 1963).

My experience made it easy to apply the concepts of plate tectonics to greenstone belts because I had already satisfied myself in the field that there were no unconformities underneath greenstone belts, and I did not have to worry about how the belts with their huge volumes of pillow lava could have formed within continents. My familiarity with the young intracontinental rifts of Africa had shown me that greenstone belts in no way represent ancient rifts (see sidebar, Rifts). Nor did I try to work out stratigraphy in the highly tectonized belts. There was a widespread idea that all greenstone belts were of Archean age (i.e., >2.5 Ga), but the first examples I had worked on, in the Birrimian of Ghana, had already been shown to be no older than ~2.0 Ga, so I was able to avoid another pitfall. A further advantage for me in trying to apply what I did not yet term Wilson Cycle understanding to the pre-~200-Ma-old Earth was that although there was much discussion in those early days (1965 to 1975) of the question, When in Earth history did plate tectonics start? I did not worry because I had recognized the signatures of plate tectonic processes in the oldest of Archean rocks.

I worked on greenstone belts while on sabbatical in Cambridge, England, during 1969. Southern Rhodesian (i.e., Zimbabwean) Geological Survey memoirs, of which the Sedgwick Museum library kept a set, proved a treasure house because of their detailed maps, which are based on some of the world's best greenstone belt outcrops, and because of their abundant petrological, mineralogical, and chemical data. I worked with John Dewey, whom I had met when he was a graduate student because of a shared interest in the geology of the west of Ireland. Dewey was soon to become my closest collaborator and remains a lifelong friend from whom I continue to learn. He had received an invitation from a leading journal to submit a paper on a topic of his choice. We submitted a paper on our plate tectonic interpretation of greenstone belts with a section on the role of continental collision in Precambrian mountain belts. In spite of the invitation, our paper was rejected. Dewey, who handled the correspondence, told me that the rejection was recommended by a petrologist reviewer who was also an expert Precambrian geologist—although it is not clear to me what that is. When he read our paper, the reviewer was working in an oceanographic institution studying ocean floor basalts. Dewey had compiled analytical data from publications on Rhodesian greenstone belts, but the K_2O content of basalt in his compilation had caused the rejection. The analyses of rocks that we suggested represented ocean floor tholeiites were too potassic. I think now that the analyses reflected the inadequacy of the wet chemical analytical methods used for

RIFTS

A rift is an elongate depression, typically more than 100 km long and tens of kilometers wide, filled with air, water, or rock. It marks a place beneath which the lithosphere has broken in extension.

rocks in the first half of the twentieth century. Our disappointment was that the compilation of chemical analyses was not a very important part of the paper and was not critical to our argument. We would have been delighted to modify our submission. This rejection was the first that I experienced in trying to point out the—as I thought, obvious—relevance of plate tectonic understanding to widely known but poorly understood aspects of the geology, geochemistry, and geophysics of the ancient Earth. I think that rejections of the kind that we then experienced, and that I continue to receive, are a reflection of an understandable reluctance on the part of specialists to accept new and unfamiliar synthetic ideas from outside their own community. Having been involved, as much from circumstances as from choice, with a great variety of Earth and planetary sciences, I am likely to use results from a range of fields and to apply those results to fields in which I have had little or no prior publication record. I have therefore become used to rejection by journals and funding agencies. I do have sympathy for those who reject my work because I have not measured anything either in the field or in the laboratory for many years, and I appreciate that science can hardly afford to accommodate many people who do as little I do. Gerry Wasserburg has told me that he has thought of my approach to science as intuitive. I accept that because I think that intuition plays a part in all scientific research, but I like to think that my approach stems also from breadth of experience, wide reading, friends in many scientific communities who keep me informed, assiduous attendance at seminars and presentations of all kinds, and a retentive—if now increasingly unreliable—memory. One result of my way of working is that I have to warn collaborators that if they choose to work with me, they will certainly have their papers and proposals rejected. Many have chosen, perhaps wisely, to abandon me.

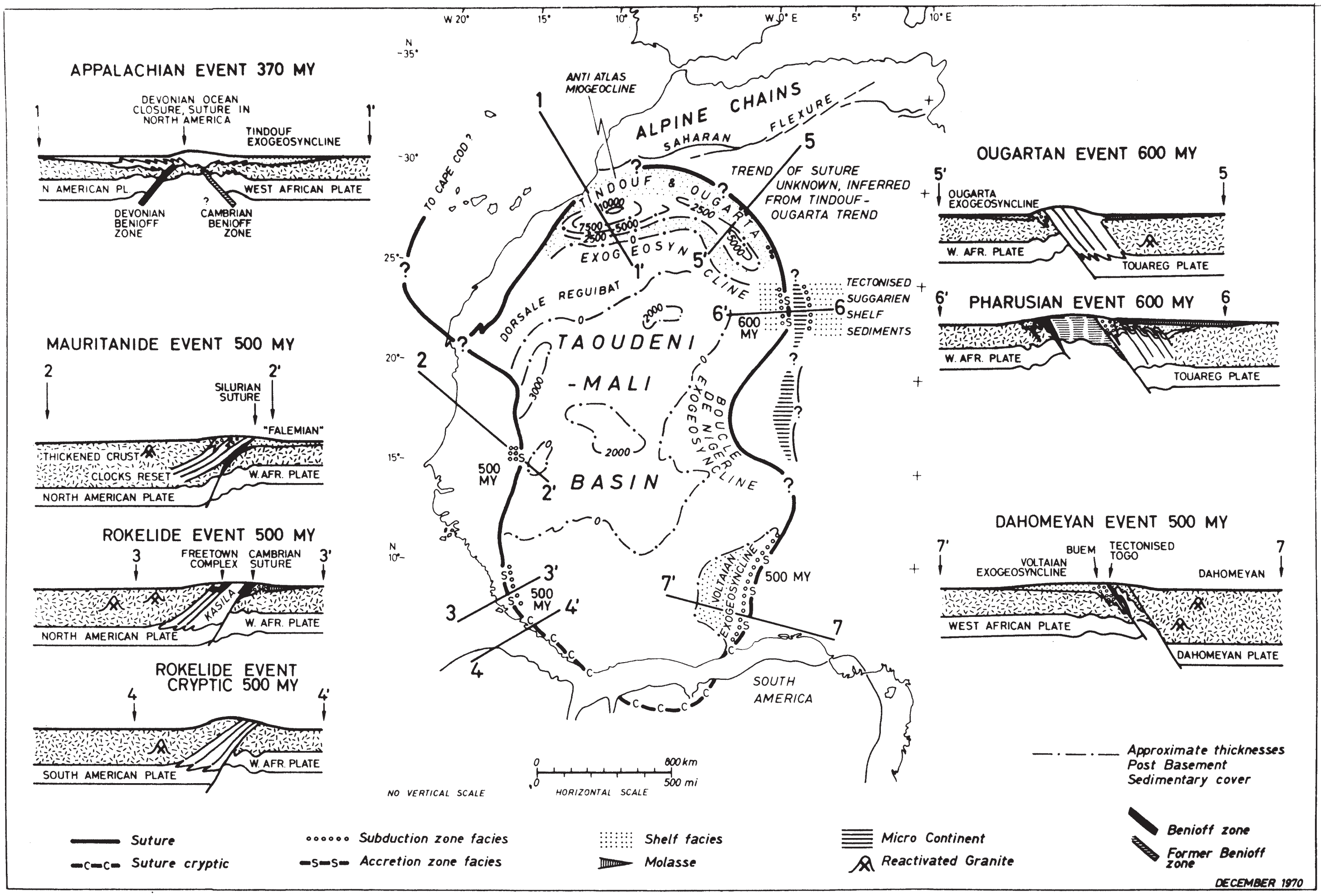
Our rejected paper did appear, as rejected papers generally do. It came out in a book published as the proceedings of a meeting, the first of a continuing series organized by African geologists. The meeting had been set up by my Nigerian colleague Soba Oyawoye as a complement to those organized by former colonial nations, to celebrate the first decade of geological teaching and research at the University of Ibadan (Burke & Dewey 1972). I gladly relinquished departmental chairmanship to Oyawoye, and for the rest of my time in Ibadan I concentrated on research.

RIFTS AND SUTURES, CRITICAL FEATURES OF THE WILSON CYCLE

A sad aspect of my time in Nigeria was that it bracketed an interval of political unrest and tragic violence. A trivial side effect of the disastrous Biafran War was that we were unable to run our customary field excursion with senior undergraduates to see a variety of Nigerian geology. Instead we drove them to Ghana, where I was able to visit again a serpentinite-dominated complex, which I had by now realized occupied part of a suture formed in a continental collision at the end of Precambrian times (~550 Mya) (see sidebar, Sutures). I spent three days of the Eid al-Fitr holiday in 1970 with Arthur Whiteman, who had joined us in Ibadan from the University of Khartoum (another British colonial University), using published information and our field experience in North and West Africa to trace that suture all around the West African craton (**Figure 2**). We found that the suture was locally what came to be termed a cryptic suture (Brown & Coleman

SUTURES

A suture is a place where a large or small ancient ocean has closed; sometimes it is marked by pillow lavas, serpentinites, and mylonites. The term is analogous to the surgical term that describes a place where a wound has been sewn up.


APPALACHIAN EVENT 370 MY
DEVONIAN OCEAN CLOSURE, SUTURE IN NORTH AMERICA
TINDOUF EXOGEOSYNCLINE
N AMERICAN PL.
WEST AFRICAN PLATE
DEVONIAN BENIOFF ZONE
CAMBRIAN BENIOFF ZONE
MAURITANIDE EVENT 500 MY
SILURIAN SUTURE
"FALEMIAN"
THICKENED CRUST
CLOCKS RESET
W. AFR. PLATE
NORTH AMERICAN PLATE
ROKELIDE EVENT 500 MY
FREETOWN COMPLEX
CAMBRIAN SUTURE
KASILA
W. AFR. PLATE
NORTH AMERICAN PLATE
ROKELIDE EVENT CRYPTIC 500 MY
W. AFR. PLATE
SOUTH AMERICAN PLATE
ANTI ATLAS MIOGEOCLINE
ALPINE CHAINS
SAHARAN FLEXURE
TO CAPE COD ?
TINDOUF & OUGARTA EXOGEOSYNCLINE
TREND OF SUTURE UNKNOWN, INFERRED FROM TINDOUF-OUGARTA TREND
DORSALE REGUIBAT
TAOUDENI-MALI BASIN
BOUCLE DE NIGER EXOGEOSYNCLINE
TECTONISED SUGGARIEN SHELF SEDIMENTS
600 MY
500 MY
VOLTAIAN EXOGEOSYNCLINE
SOUTH AMERICA
NO VERTICAL SCALE
HORIZONTAL SCALE
800 km
500 mi
OUGARTAN EVENT 600 MY
OUGARTA EXOGEOSYNCLINE
W. AFR. PLATE
TOUAREG PLATE
PHARUSIAN EVENT 600 MY
W. AFR. PLATE
TOUAREG PLATE
DAHOMEYAN EVENT 500 MY
VOLTAIAN EXOGEOSYNCLINE
BUEM
TECTONISED TOGO
DAHOMEYAN
WEST AFRICAN PLATE
DAHOMEYAN PLATE
Approximate thicknesses Post Basement Sedimentary cover
Benioff zone
Former Benioff zone
Suture
Suture cryptic
Subduction zone facies
Accretion zone facies
Shelf facies
Molasse
Micro Continent
Reactivated Granite
DECEMBER 1970

OPHIOLITE

Ophiolite is a term for cherts, pillow basalts, sheeted dykes, gabbros, and ultramafic rocks (usually serpentinite after harzburgite), ideally in that vertical order, exposed on land and interpreted as representing similar suites of rocks that form in a variety of spreading center environments on the ocean floor. The name comes from a Greek word for snake because weathered serpentinite has been considered similar in appearance to snakeskin.

1972) because it was not ornamented with ocean floor rocks for ~1,500 km of its length from Togo through northern Brazil (where Monica Dirac, in her PhD thesis, had already identified a sliver of the West African craton) and Liberia. Only in Sierra Leone did outcrops of ocean floor rocks of Pan-African age reappear in the suture zone. This led us to the idea that suture zones marking the sites of continental collision might be cryptic for distances of hundreds or more kilometers. Cryptic sutures can show outcrops of rocks from two continents in contact with no diagnostic ocean floor or island arc rocks in between. A geologist who had worked in Liberia became incensed that we had traced a cryptic suture zone through his map area. His was a widespread attitude—"plate tectonics works very well, but not in my quadrangle"—that has helped delay the assimilation of Wilson Cycle understanding.

Shortly before I arrived in Nigeria, the 1,000-km-long Benue river valley in eastern Nigeria had been shown, by outstanding Nigerian Geological Survey field work including a gravity survey, to occupy a major Cretaceous (144–65 Mya) intracontinental rift system complete with igneous activity, normal faults, and thick sediment piles. My new Wilson Cycle appreciation of the history of Earth (**Figure 1**) explained rifts of that kind as recording extension in the continental lithosphere that did not develop into ocean formation. They have been termed failed rifts, although I prefer to refer to them as successful rifts but failed oceans. The Benue rift is unusual because it experienced widespread and locally intense folding during Late Cretaceous time (~84 Mya) without apparently having passed through the canonical cyclical stages of ocean opening and closing.

With colleagues I pursued Benue research. After field reconnaissance, geochemical work on Cenozoic alkaline rocks, and photointerpretation, we concluded that there had been a short-lived episode of ocean floor consumption in the lower Benue during the late Cretaceous (Burke et al. 1971). This conclusion was based partly on a description of andesitic volcanic rocks associated with thick, intensely folded, and cleaved black shales of the lower Benue valley, which resemble the rocks of a subduction complex. Today those volcanic rocks are no longer considered andesitic, and a preferred explanation of the folding in the Benue is that it represents part of a continent-wide episode of deformation concentrated in then-existing intracontinental rifts. That deformation episode occurred ~84 Mya as a result of the transmission of stress throughout Afro-Arabia following a major island arc collision with the Afro-Arabian continental margin expressed in ophiolite emplacement in Oman, Iran, Syria, and Turkey (Burke & Dewey 1974, Guiraud & Bosworth 1997) (see sidebar, Ophiolite).

Figure 2

Circum–West African craton suture zone. An early application of Wilson Cycle interpretation to Precambrian geology by Burke and Whiteman in 1970. An old craton was recognized in West Africa, with an area equal to approximately half that of Australia. The craton was shown using various of Wilson's criteria, to be surrounded by a suture zone marking the site of mountain belts formed by continental collision at approximately the end of Precambrian times. Reproduced with permission from Dewey & Burke 1973.

UPLIFT, RIFTING, AND THE BREAKUP OF AFRICA

The northeastern part of the Benue rift system joins another Cretaceous rift. In map view, the upper and middle Benue rifts and the third rift meet so that angles between them are roughly 120°. The pattern reminded me of an observation of Cloos (1939) from the Rhine rift system in Europe. Cloos had suggested that three rifts, meeting near the Eifel mountains, formed to relieve stresses set up in the lithosphere above a rising dome. Şengör (2001) has emphasized that a dome would have to be unrealistically elevated to generate much extension in such a three-rift system. The process is more complex with dome elevation, typically of ~1 km, generating only vertical cracks that accommodate dike-forming magma. The dikes propagate horizontally, and the lithosphere below the dikes soon extends to form rifts.

I knew from studying African geology that there were many intracontinental rifts in Africa, that some appeared related to domes, and that there were places where three rifts met to form patterns similar to that described by Cloos. An observation that linked intracontinental rifts to the new ideas of plate tectonics was that some intracontinental rifts, including the lower Benue rift, extended to the continental margin. If the continental margin had formed from older intracontinental rifts, as I could show had happened in the Afar and the lower Benue, then two rifts of a three-rift set became the site on which ocean floor began to form. I wrote with Whiteman an article embodying these ideas that we titled, to the amusement of our politically minded students, "Uplift, Rifting and the Breakup of Africa" (Burke & Whiteman 1973). That paper was seminal for me. It was the first of an ongoing series that embody much of my research effort, consisting in large part of reviews of existing work that I integrate and interpret in terms of my own ideas. Many make a distinction between review articles and original work, but for me that is unreal. The paper that I wrote with Whiteman involved synthesis of published work on aspects of intracontinental rift data, including observations on gravity, igneous petrology, geological maps, structural geology, stratigraphy, and sedimentology.

In 1969, my long-suffering wife persuaded me that our growing children needed better schools than we had access to in Ibadan, so I began, without success, to apply for jobs. Finally I was offered a job at Erindale College, one of the satellite campuses of the University of Toronto in Canada. The single-year appointment represented a risk because we had a young family, but I decided that the risk was worth taking because the principal of Erindale College was Tuzo Wilson.

WITH TUZO WILSON AT ERINDALE, WORKING MAINLY ON HOTSPOTS

Administrative duties took much of Wilson's time, but he was also preparing a second edition of *Physics and Geology*, a text that he had written with Jacobs and Russell (Jacobs et al. 1973). Chapters 15 and 16 of that revised edition contain a treatment of what we now term the Wilson Cycle, expanded from Wilson's 1968 paper and illustrated by examples from world geological history over the past 600 Ma.

Wilson made working at Erindale easy for me, and he hired Bill Kidd, who was a student of John Dewey's. Teaching loads were not heavy, and Wilson made sure that I was involved, as I was eager to be, in meetings at which I could learn about the Precambrian tectonics of the Canadian shield. However, I also continued to pursue my interest in African tectonics.

MOVEMENT OF THE AFRICAN AND SOUTH AMERICAN PLATES IN THE HOTSPOT REFERENCE FRAME

Working on "Uplift, Rifting and the Breakup of Africa" had made me think about hotspots. Tuzo Wilson and Jason Morgan had both drawn attention to volcanoes such as those of the Hawaiian chain that, unlike most of the igneous activity on Earth, are unrelated to plate margins. They had linked Hawaiian volcanism to an underlying deep-seated plume, and Morgan had suggested that plumes, such as the Hawaiian plume, rise from the CMB. Morgan's suggestion has, after more than 30 years, only lately been shown to be correct. It is now established that mantle plumes rise from the CMB (see, for example, Burke & Torsvik 2004; Torsvik et al. 2006; Burke et al. 2008b; Montelli et al. 2004, 2006; Davaille et al. 2005; and Torsvik et al. 2010).

Because the vast bulk of igneous activity is associated with plate boundary zones, I referred to the rest as hotspot activity, a term I used nongenetically. Some hotspots, such as Hawaii, I felt able to attribute to underlying plumes of deep origin, but much of the rest I was happy to think of as of unknown origin. Now I think that there are only two major non-plate-margin and non-plume-related kinds of igneous rock sources: (*a*) volcanism associated with intracontinental rifts that forms by decompression melting related to extension in the rifts, and (*b*) volcanism by decompression melting in cracks such as those beneath the younger harrats of Arabia. In those harrats, volcanic cones in lines trending close to north–south indicate a role for underlying cracks that formed in response to the Zagros collision at ~15 Mya (Burke 1996). However, there is still non-plate-margin volcanism that I continue to think of as of unknown origin.

One afternoon in Erindale in 1972 while studying a map of South Atlantic magnetic anomalies, I was surprised to notice that the Tristan hotspot volcano was not, as I had assumed, on the active South Atlantic spreading center but that it instead lay on oceanic crust of the African plate that had formed ~30 Mya. I knew that Tristan had erupted in 1961, so I inferred that the Tristan hotspot was the active volcano at the end of the Walvis Ridge hotspot track. My idea was that volcanism in the Tristan area had begun at ~30 Mya. At that time, the active hotspot at the end of the Walvis Ridge was on the South Atlantic spreading center. Since then, the spreading center has migrated west from the hotspot, leaving it and its underlying mantle plume stationary with respect to Earth's spin axis. Tristan's hotspot volcanic activity has continued episodically and sporadically over an elliptical area of approximately 500 $\times$ 300 km during the past 30 Ma (Burke 1996).

My observation about Tristan and its explanation suggested an answer to a broader question that had been puzzling me. I knew that in contrast to the Pacific plate—on which there are several tracks, each with a hotspot volcano at one end—active African hotspot volcanoes are generally not at the end of a mappable track. I did not know why that was, but I did know that most active African hotspot volcanoes have been active in the same places for ~30 Ma (Burke 1976). Some workers have discerned azimuthal hotspot progression on the African plate during those past 30 Ma. For example, interpretations of observations close to Saint Helena have suggested that African plate motion has "slowed down," but given that African hotspots typically extend over areas ~300 km in diameter, the observations near Saint Helena cover too small an area to show that the Saint Helena hotspot has moved significantly in any reference frame (O'Connor et al. 1999). Moreover, the azimuth derived in that work is not concentric with those of many other mapped short (<400 km) lines of volcano age progression on the African plate during the past 30 Ma. Those lines box the compass. Recognizing that the volcanic activity in African hotspots has remained in the same places for the past 30 Ma solved my worry about the difference between Pacific and African plate hotspots: It was not just Tristan that was at rest with respect to the spin axis and the mantle circulation; it was the whole African plate. It is now clear that in the past 5 Ma, Africa has broken into three plates, although their rotations have as yet been too small to have modified their relationship to underlying mantle structure on the 100-km scale.

I called Wilson, who responded, "Come over to my office at 4:30 and we will have a glass of sherry." Wilson, who had published about gaps in hotspot tracks (Wilson 1965b), was interested in my observation about Tristan and receptive to its implications. Our discussions soon led to a letter to *Nature* titled, "Is the African Plate Stationary?" (Burke & Wilson 1972). I then learned this lesson: "Look for the further implications of your findings." Wilson had realized that if the African plate had stopped in the hotspot reference frame, then, because the spreading rate in the South Atlantic had not changed, South American motion in the same frame had doubled. That implied changes in the Andean convergent boundary at ~30 Mya and more generally enabled volcanic arc margins moving trenchward in the hotspot frame to be distinguished from those retreating from the trench in the same reference frame. We published another letter in *Nature* titled, "Two Types of Mountain Building" (Wilson & Burke 1972). Nearly 20 years later, John Dewey showed that this situation could be analyzed by keeping the trench fixed and distinguishing between situations in which motion toward the trench dominated and situations in which motion away from the trench dominated (Dewey 1980). More recently, Sobolev & Babeyko (2005) revived the idea that the behavior of the Andes changed at 30 Mya. I still think that Wilson's example of making inferences far afield is important. We solid Earth scientists are much better at making observations and reporting the immediate inferences from those observations than at developing the broader implications of our findings.

I had come from Africa with a great interest in learning how plate tectonic concepts were being applied to the Precambrian of Canada. Toronto afforded opportunities to learn about the Precambrian, ranging from the excitement of the prospectors' meeting to Al Goodwin's ideas on volcanism in greenstone belts, but an unexpected highlight came from meeting Paul Hoffman. Hoffman, who had recently returned from his PhD work at Johns Hopkins, gave a talk in Toronto. Afterward, we went to eat and, at Hoffman's suggestion, to hear "Wild" Bill Davison play Chicago jazz. Before Davison played, in case he proved too loud, I sketched on a napkin how I thought Hoffman's observations of two radial rifts extending from the interior of a craton to a curved mountain belt at the craton margin might exemplify the kinds of rifts at high angles to collisional fold belts that Dewey and I were working on (Burke & Dewey 1973). Hoffman applied that idea to his field area and wrote it up in a paper to which he graciously added our names (Hoffman et al. 1974).

Working with Tuzo Wilson was inspiring, and he was keen that I stay in Toronto and continue to work with him. But I needed a job. A solution came when John Dewey called and told me that he and his colleagues wanted to offer me a job at the State University of New York in Albany, although the offer was conditional on my agreeing to chair the department. I was happy to accept, not only because I needed a job, but because there was a lot of work that I wanted to do with Dewey and because I thought—although I was wrong—that my previous experience as a department head in the very different academic environments of Jamaica and Nigeria would be helpful in the job of chairman. Soon Bill Kidd joined us, and in 1976 Celâl Şengör came to Albany to complete the last two years of his BS and his PhD.

ALBANY, 1973 TO 1983: WORKING ON THE WILSON CYCLE

At SUNY Albany, John Dewey, Bill Kidd, Celâl Şengör, and I interacted in research, in which other students were also frequently engaged. Jeff Fox, who had joined us from Lamont, was running a marine geology program on the Cayman Trough spreading center, and Akiho Miyashiro and Steve De Long were also in Albany. Thus six of nine faculty members with skills in a variety of disciplines were working in the exploding field of applying an understanding of plate tectonics to the interpretation of Earth history. Miyashiro worked alone, although his masterly, if barely audible, classes were critical for us. Various combinations of the rest of us published together. The concentration of research interest was unusual, but I justified it to myself because I saw a unique opportunity to develop a discovery that had revolutionized the solid Earth sciences in a way that had not been equaled for more than 100 years.

While in Toronto, I prepared two papers with John Dewey. The first was titled, "Plume-Generated Triple Junctions: Key Indicators in Applying Plate Tectonics to Old Rocks" (Burke & Dewey 1973). That paper grew out of the work I had begun with Whiteman in Ibadan (Burke & Whiteman 1973). The new idea was that if two arms of a triple-rift system developed into an ocean, which later closed to form a mountain belt, a map pattern recognizable on the scale of hundreds to thousands of kilometers would develop. That pattern consists of a curved fold belt and a rift extending away from it into one of the two continents that had been involved in the collision. We recognized, and briefly described, examples of this distinctive geometrical pattern in the geology of fold belts with ages extending back into Precambrian times, helping in that way to demonstrate the long-term history of the Wilson Cycle. Soon Molnar & Tapponnier (1975) showed that the geometric relationship of a rift at a high angle to a fold belt had been established as a result of Himalayan continental collision when the Baikal rift was initiated at ~50 Mya. Discriminating between the two geometrically similar patterns depends on determining, from the stratigraphic ages of rocks within a rift, whether rift initiation was linked to the ocean opening stage or to the ocean closing stage of the Wilson Cycle. We were able to show, from stratigraphy, that the Rhine rift is of the collision-related kind (Şengör et al. 1978).

The second paper that I worked on with Dewey while in Toronto was titled, "Tibetan, Variscan, and Precambrian Basement Reactivation: Products of Continental Collision" (Dewey & Burke 1973). The idea in that paper was that one consequence of the thickening of continental crust during collision is that a wide region on one side of the collisional suture is likely to be characterized by postcollisional igneous activity and metamorphism. In such an area, rocks with ages older than the age of the collision have some or all of their isotopic clocks reset. We introduced this idea on evidence from Africa, where isotopic clocks that close at relatively low temperatures had been reset during continental collision between 600 Mya and 500 Mya in a huge region extending over half the area of the present continent, although higher-temperature (U/Pb) clocks in the same areas have not been reset, so that some rocks yield older ages. The thermochronologically reset areas had been recognized first by Kennedy and Clifford at the Institute of African Geology in Leeds, England. Because of their equant appearance and great extent, the reset areas were considered unlike ordinary mountain belts, which are long and relatively narrow. Kennedy (1965) had therefore characterized them as having experienced what he termed the Pan-African thermo-tectonic event. We recognized the effects as those of continental collision and named the event the Pan-African orogeny. The Pan-African areas that I knew in Nigeria and elsewhere in Africa contained abundant large (>50 km diameter) bodies of granite with close to equal proportions of quartz, plagioclase (~An 30), and alkali feldspar. Rocks of that composition can be generated by partly melting average continental crustal rocks, so I came to think of thickened continental crust formed at continental collision as generating rocks of that type. I became interested in whether the wide, high plateau of Tibet, which is presently the site of active continental collision, might be covered by silicic volcanic rocks generated as a result of the collision. At that time, Tibet was inaccessible to us, but the first Landsat images were available. Bill Kidd put a lot of effort into reviewing papers based on the explorers' rock collections (mainly those of the Swede Sven Hedin) and plotting their traverses on Landsat images. Kidd prepared a map of much of the plateau but decided against publication because neither the ability to characterize rock types on the images nor the ground control was adequate. On Landsat images Kidd and I were able to see volcanoes, some of which had been reported by explorers, and we thought that we could see extensive areas of volcanic rock. Specimens in a Stockholm museum proved to be siliceous tuffs, but fieldwork has since found only sporadic young volcanic rock occurrences in relatively small volumes and with geochemistry indicating a low degree of melting of mantle rock. Molnar & Tapponnier (1975) got a great deal more out of the early Landsat images by studying a much larger area of Asia and by integrating earthquake mechanisms with Landsat observations, but Bill Kidd's efforts were not wasted because he later did fieldwork in Tibet and introduced T. Mark Harrison to the geology of the Tibetan plateau.

The two papers on plume-generated triple junctions and on basement reactivation appeared. John Dewey had again received an invitation to submit a paper of his choice, this time from Peter Wyllie, who was editing the *Journal of Geology*. That both papers were soon published (Burke & Dewey 1973, Dewey & Burke 1973) may reflect tribal preference because Wyllie, Dewey, and I were all born in London within a few kilometers of one another.

My interest in the Wilson Cycle as the integrating concept for addressing global regional geology on the timescale of the history of Earth continued to grow, and we formally introduced the term in a paper in an early issue of *Geology* (Burke & Dewey 1974). Ted Flinn, who was managing a program at NASA, funded us to produce maps and catalogs of the world distribution of intracontinental rifts and sutures as well as a catalog of hotspots (Burke et al. 1978). A version of the suture map appeared (Burke et al. 1977), and Şengör has updated the rift and hotspot catalogs (most recently in Şengör & Natal'in 2001). Dewey taught plate tectonics, so I taught a graduate course titled World Historical Geology that enabled me to use illustrative examples of Wilson Cycle phenomena from many places and times. Art Green of Exxon, who for more than 30 years has been a friend in petroleum industry research, asked me to generate a rift catalog more focused on the Phanerozoic and on topics such as stratigraphy than the catalog that we were making for NASA. In addition, Jim Hays at Lamont asked me to work with him on reports about regional geology related to the evolution of the world's ocean basins. My taste for regional geology was further satisfied between 1983 and 2003, when I served as a member of an ocean drilling safety panel.

A fertile development in regional geology and Wilson Cycle research in which we at Albany were only peripherally involved came from the collaboration of tectonicians with paleontologists, stratigraphers, and students of paleolatitudinal indicators, especially paleomagnetists. Results showed the geology for various times during the Phanerozoic on maps that also indicated the then distribution of the continents and smaller objects. Stuart McKerrow—who had worked in the west of Ireland, as had John Dewey and I—was a stratigrapher who led in this research, and today strong groups in Chicago, Norway, and Texas can trace their heritage directly to him. A limitation of this work has been that, particularly for times before the assembly of Pangea, longitude has been hard to constrain, although a new way of estimating longitudes for Large Igneous Provinces (LIPs) as old as 510 Ma is helping resolve that difficulty (Torsvik et al. 2008).

In 1976, during my time at Albany, Tuzo Wilson visited Caltech and prepared a book for publication by *Scientific American*. Wilson was keen that he and I have a paper on hotspots in that book, so he arranged for me to be a visiting professor (Wilson 1976, Burke & Wilson 1976). The quarter I spent at Caltech was fertile. I completed work on three papers and I taught a class, but more important, I was welcomed by students and faculty and introduced to exciting ideas that were new to me in geology, geophysics, geochemistry, cosmochemistry, and planetary science. I assumed, although I now realize that the assumption was wrong, that by 1976 all graduate students entering Caltech would have had a full course on plate tectonics during their undergraduate careers, so I taught a well-attended Wilson Cycle course quite similar to the one I taught in Albany.

My interest in hotspots and mantle plumes declined while I was at Albany, mainly because of other interests. Bill Kidd and I wrote two more short papers with Tuzo Wilson (Burke et al. 1973a,b) and one with a graduate student on the significance of the spacing of African hotspot volcanism and swell crests for underlying shallow convection (Thiessen et al. 1979). The idea that Africa's distinctive basin and swell topography reflects shallow convection under a stationary plate (Burke & Wilson 1972) had been taken up and modeled (McKenzie & Weiss 1975) and was soon to be analyzed with improved computing capability (England & Houseman 1984). I was not unhappy to leave hotspot research, at least for a while, because advances depended on improved dating of volcanic rocks, which was happening only slowly and because published research on hotspots and plumes became very confused.

My interest in understanding how plate tectonics had operated during the Precambrian persisted. With John Dewey and Bill Kidd, I published a discussion in a book titled *The Early History of the Earth* (Burke et al. 1976) of how horizontal movements (a code name for plate-scale movements), arcs, and microcontinental collisions had operated. Dewey had persuaded himself that plate tectonics was not operating in those early times, so, to accommodate his opinion, we added the phrase "During the Later Permobile Regime" to the title (Burke et al. 1976). However, my own preference has been that because the ambient temperature at Earth's surface has never been much hotter than now (there have always been pillow lavas) and because silicate rocks do not flow much below ~550°C, the outer surface of Earth has always been rigid. Therefore, there have been plates since Earth settled down after the moon-forming impact and before the oldest preserved rocks and mineral specimens were formed (cf. Harrison 2009).

We had begun to see the appearance of papers that suggested steeper conductive gradients on Earth in early times. Bill Kidd and I did not see evidence for that, so, having been chided for lingering over our group lunch, we spent the next few lunches writing a paper pointing out that the Superior Province of Canada, a block of continent in excess of 1 M km^2 in area, had been assembled from island arcs by 2.5 Gya but showed no evidence of postassembly regional melting. That permitted a crude estimate of a maximum conductive thermal gradient for continental crust at 2.5 Gya that is ~30% greater than that of today. We knew that much more heat was being generated within the mantle by radioactive decay at 2.5 Gya, so we inferred that the extra heat was being removed from the interior by plate tectonics in the world's oceans involving numerous small plates, or faster plate motion, or both (Burke & Kidd 1978).

It had also been suggested that if the convecting mantle were hotter in the early Earth, less heat would have been crossing the CMB, and that would have compensated for extra heat generated in the mantle by nuclide decay. However, the geological record shows that basalts have been erupted at Earth's surface throughout Earth history, as they are today at spreading centers. I interpret those basalts as showing that decompression melting of the shallow mantle throughout Earth history has buffered the average temperature of the convecting mantle, keeping it the same as it is now. Grove & Parman (2004) have shown, on the basis of quite different evidence, that the mantle is unlikely to have ever been more than ~50°C hotter than it is now.

We were also able to show that, at least in one region, the elastic thickness of the Late Archean continental lithosphere was similar to that of today (see sidebar, Elastic Thickness). The Witwatersrand sedimentary basin of South Africa, which was initiated at ~3 Gya, is a well-known basin

ELASTIC THICKNESS

The response of the lithosphere to loading can be represented by a model in which the weight of topographic features is supported by stresses within a flexed elastic plate overlying an inviscid asthenosphere, and by buoyancy forces associated with the deflection of density interfaces in response to flexure. Estimates of the apparent thicknesses of the elastic plates within continents are based on the dimensions of sedimentary basins. Those basins range up to more than 100 km in width.

because it contains the world's greatest concentration of major gold deposits. I had been familiar with the geology of the basin for a long time because when I worked in the British Geological Survey, it was the source of our uranium concentrates. In Albany, we were able to show that the Witwatersrand basin is a typical foreland basin. Foreland basins form by flexural subsidence at the side of a rising mountain belt. The basins subside because the mountains impose a load on the underlying lithosphere. The Witwatersrand basin is the oldest well-preserved basin of that kind. It is 100 km to 200 km in width, which is similar to the width of typical foreland basins today. That width was governed, when the Witwatersrand basin formed, by the then elastic thickness of the underlying lithosphere. We inferred that the elastic thickness of continents does not appear to have changed much through time (Burke et al. 1986).

My interest in intracontinental rifts prompted the editors of the *Annual Review of Earth and Planetary Sciences* to ask me to review how these rifts fit into a plate tectonic view of the world (Burke 1977). I was able to pay tribute to the contribution of Shatsky, who, in searching for new sources of oil while Hitler's armies were invading the Soviet Union in 1941, had recognized the existence of a largely subsurface set of rifts radial to the Russian platform. Those rifts were of the kind Dewey and I had identified as extending into a continent from collisional fold belts (Burke & Dewey 1973). Shatsky termed his rifts aulacogens (from the Greek word *aulax*, meaning furrow). Now that so many kinds of rifts are recognized, that word has been largely abandoned, but Shatsky's was a seminal contribution to continental tectonics.

While in Albany, I took the opportunity of working again on Caribbean problems. We first considered the ocean floor beneath the Caribbean Sea and found that it lies at an average depth of 4.5 km below sea level. My then colleague Jeff Fox and others had shown that Caribbean ocean floor was Late Cretaceous (~90 Ma) in age. Ocean floor forms at 2.8 km below sea level at spreading centers and subsides as the square root of its age. Cretaceous ocean floor is expected to lie at a depth of ~6 km. The Caribbean ocean floor therefore appears, as I too have been accused of appearing, too shallow for its age. We explained the anomaly by suggesting that the Caribbean is occupied by an oceanic plateau with crust made of thicker basalt accumulations than normal ocean floor. Several plateaus of that kind had already been recognized on the floor of the Pacific Ocean (Burke et al. 1978). We now know that the Caribbean is largely occupied by an oceanic plateau, the rocks of which represent a Large Igneous Province or LIP (Burke 1988).

I had known that the Greater Antillean islands were underlain by rocks of an island arc of Cretaceous age from the time I lived in Jamaica during the 1960s. We now used fieldwork to discover something about how that arc had evolved. In the west of Jamaica, we mapped a part of the arc's subduction complex that had been metamorphosed in zeolite facies. The fill of a submarine canyon including blocks of basalt as much as 0.5 km across was a striking feature (Grippi & Burke 1980), but regional structure was hard to establish because the arc-related rocks in Jamaica are exposed only in inliers that have been cut by strike-slip faults of the active Cayman Trough transform system. The continuation of one of the faults, the Fat Hog Quarters fault, into shallow water a few kilometers to the west of where we had mapped it was later shown by side-look sonar to be an active fault (Rosencrantz & Mann 1991).

Farther east in Jamaica, careful mapping by Paul Mann, the only PhD student who worked with me during my time in Albany, showed that the final stages of island arc activity in the island, between ~60 Mya and ~50 Mya, involved the development of an arc-crestal rift (cf. Dalmayrac & Molnar 1981). Although rifts of that kind are well-known—in the Andes, for example—and have been suggested to commonly evolve into back-arc basins (e.g., Şengör 1995), few ancient occurrences of arc-crestal rifts, similar to that preserved in the Wagwater Trough of Jamaica, have been described (Mann & Burke 1990). Mann was able to work out the history of the Wagwater Trough because active tectonics in the plate boundary zone of the northern margin of the Caribbean plate have raised the Early Cenozoic rocks of the trough to outcrop. Mann and I followed the active structures eastward and under the sea into Hispaniola, where he has subsequently found much of interest, including a unique coral reef that is only 10,000 years old and that is exposed in a dry sub-sea-level valley that has been blocked from the sea by an alluvial fan (Mann et al. 1984, Taylor et al. 1985).

By the early 1980s, Şengör had returned to Turkey and Dewey had gone back to England. I had begun to develop an interest in a broader understanding of the terrestrial planets through involvement in a NASA project named Basaltic Volcanism on the Terrestrial Planets, and in 1983 I left for Houston to become director of the Lunar and Planetary Institute—but not before Mark Harrison had arrived in Albany and had introduced me to modern isotope geochemistry and particularly to his developing ideas in what came to be known as thermochronology.

FIVE YEARS AT THE LUNAR AND PLANETARY INSTITUTE

The Lunar and Planetary Institute (LPI) provided a stimulating environment. It housed a small number of mostly postdoctoral researchers, many of whom worked in the splendid laboratories of the adjacent Johnson Space Center. A steady stream of scientists visited us to use those facilities. There were also service activities such as running review panels and the annual Lunar and Planetary Science conference, the latter held jointly with our Johnson Space Center neighbors.

The LPI organized projects and conferences, and I found participation in them particularly stimulating. About the time I arrived, two meetings proved memorable. One was on the origin of the moon. The main alternatives at the time were capture, growth close to Earth, and formation from Earth as the result of a massive impact. Over the course of a three-day meeting, it became clear to most of us, perhaps mainly because the other processes were shown to be unlikely to work, that the best idea was impact by a Mars-sized object within a few tens of millions of years of the first formation of Earth. Origin by impact is still the preferred hypothesis, although it continues to be refined through advances in planetary dynamic modeling and isotope geochemistry. The other meeting, jointly organized with the National Academy of Sciences, was on the great biological extinction at the K/T boundary. That was a much bigger affair involving scientists from physics, astronomy, and biology as well as Earth scientists. The finding of an Ir anomaly related to impact in rocks at the K/T boundary by the father-and-son Alvarez team and their colleagues Asaro and Michel (Alvarez et al. 1980) was revolutionary and persuasive not just to me, but to most of those attending the meeting.

Those two meetings made me receptive to the idea of catastrophic change in the Earth system. I had always been a disciple of Lyell, partly because I had found that the uniformitarian approach worked well in explaining the phenomena that interested me, but also because I thought that the general criticism of Lyell for adopting too extreme a position was unfair. In my view, he was forced to take the stance he did because he realized that almost all of his geologist contemporaries had yet to be dragged away from accepting unscientific explanations of some things that had happened in the past, in particular at least parts of the Judeo-Christian creation myth.

Unusual rocks named anorthosites, consisting mainly of highly calcic plagioclase, occur both on the moon and in Archean rocks on Earth. Our colleagues at the Johnson Space Center developed a NASA-funded project known as Early Crustal Genesis that was related to anorthosites. We at the LPI participated by holding workshops on very old rocks, including field workshops in Canada, Greenland, and Southern India. The latter two were logistically challenging but were successful because the leading local experts showed tens of specialists from all over the world

FREE-FACE REGION

In blasting, a free face is the rock-against-air interface toward which products are driven by explosion. In tectonics, the borrowed term is applied to the region toward which large blocks of lithosphere move to escape the effects of being caught by the collision of two continents or the collision of a continent and another buoyant object such as an island arc or an oceanic plateau.

remarkable geology, and new collaborations were established (e.g., Leelanandam et al. 2006). Lew Ashwal, who was at the time writing the definitive book on anorthosites (Ashwal 1993), was the LPI anchorman. He and Maarten de Wit, who spent six months with us at the LPI, also edited a comprehensive book on greenstone belts (de Wit & Ashwal 1997).

During my time at the LPI, my interest in the operation of the Wilson Cycle continued to develop, and with Şengör I wrote a paper on the role of tectonic escape in the evolution of the continental crust (Burke & Şengör 1985). The idea, which came from Molnar & Tapponnier (1975) in their work on the Himalayan collision, is that, at continental collision, large blocks of one of the collided continents respond by moving laterally away from the collision zone. Strike-slip faults, which, in at least some cases, are transform faults, bound the laterally moving blocks. Tapponnier termed the process *extrusion tectonique*, but we preferred to write of tectonic escape because we thought that the term extrusion had a narrower meaning in English than in French.

We described examples of tectonic escape to show that the process of tectonic escape could be recognized in old mountain belts. Geologists are reluctant to identify major strike-slip faults in old belts because estimating long offsets of old faults has proved difficult. We suggested that criteria for identifying tectonic escape in ancient mountain belts include (*a*) identification of a free-face region, usually underlain by ocean floor, toward which the escaping blocks were moving (see sidebar, Free-Face Region), and (*b*) the timing of movements on the bounding faults as falling within the duration of the timing of the related collision.

When I started working at the LPI, I became a professor at the University of Houston. That has been my job for 27 years, although I have been generously released by the university to do other things for parts of that time, as I was when I worked at the LPI. For many years, I taught undergraduate historical geology to large classes. Few, if any, students in those classes were likely to pursue a career in science, but I appreciated the opportunity to illustrate how scientists work as well as to try to explain some of the amazing things that have been found out about Earth. I also taught, and still teach, graduate courses in the evenings on tectonics and sedimentary basins. I cycle, semester by semester, through Asia, Africa, and Latin America. Experienced oil geologists have participated in those classes so that I, and the students, have learned from them.

THE GREAT ARC OF THE CARIBBEAN AND RIBBON CONTINENTS

An invitation to write "Tectonic Evolution of the Caribbean" from the *Annual Review of Earth and Planetary Sciences* was a further stimulus (Burke 1988). The Caribbean poses a challenge to interpreting plate tectonics, although Tuzo Wilson (1966) made a good start when he showed that the Caribbean plate had come out of the Pacific Ocean by transform motion with respect to North and South America. However, it is not possible to do rigid Caribbean plate rotations for times before 50 Mya, the time when the spreading center in the Cayman Trough formed. My

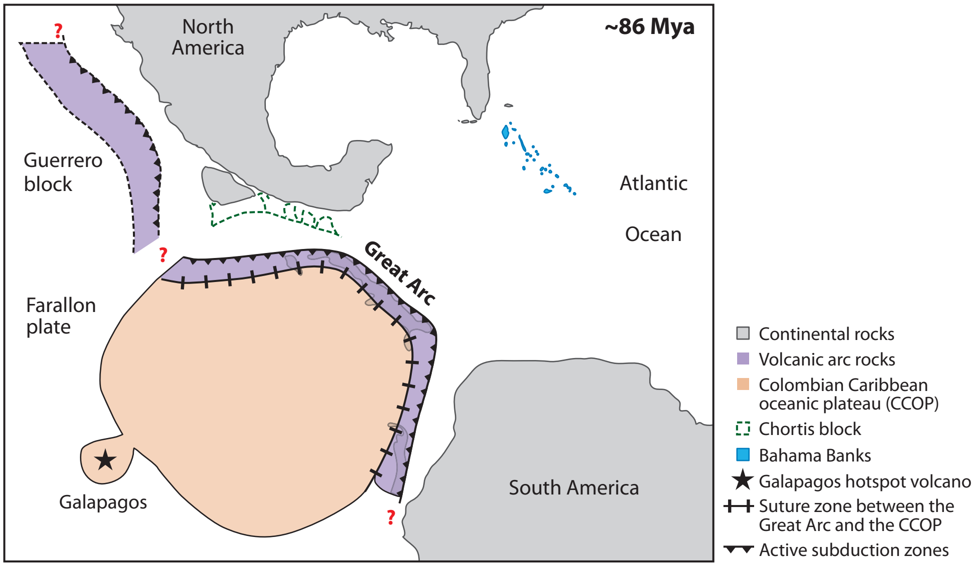


Figure 3

The Great Arc of the Caribbean. A change in emphasis in Wilson Cycle interpretations of regional geology in recent decades has been that island arcs and ribbon continents that have collided with larger continents, as much of the Great Arc is shown here to have done, are increasingly being recognized to have been typically thousands of kilometers in length. Reproduced and modified with permission from Altamira-Areyan 2009.

former student Jim Pindell had drawn plate reconstructions for earlier times on the basis of various bold assumptions, and he kindly gave me maps from a paper that he was preparing on Caribbean plate evolution. I found his maps helpful, but I was really surprised when I found out for myself that the Cretaceous island arc rocks of Ecuador, Colombia, Venezuela, offshore islands, Trinidad and Tobago, the volcanically active Lesser Antilles, the Greater Antilles, and Guatemala appeared to be parts of a $\geq$2,000-km-long island arc that had formed in the Pacific Ocean and had later collided with both North and South America or, in the case of the Greater and Lesser Antilles, had entered the Atlantic Ocean (**Figure 3**). I feared that researchers who had studied small lengths of Cretaceous volcanic arc rocks in various places might be unsympathetic to the idea of a single arc, so in a preemptive step I named my new long arc the Great Arc of the Caribbean, and that idea has survived quite well.

Identification of the Great Arc of the Caribbean proved to be consistent with a growing number of observations showing that island arcs that have collided with continents have been thousands of kilometers long. Furthermore, because the colliding objects have been so large, relatively few individual island arcs have contributed to the growth of the continents during the past 2 Ga. For example, Şengör & Natal'in (1996a,b) showed that the 5-M-km^2 Altaid continental area in Central Asia was assembled from two very long island arcs that were disrupted by large-scale bending, slicing, and shearing in the course of continental assembly. Their review of the growth of Asia (1996a) also showed that arcs involved in Manchuride and Tethyside evolution had been long and few in number. That idea seems obvious because both island arcs and continental margins typically extend for thousands of kilometers, but we students of tectonics have been slow to move beyond considering arc and continental collisions along lengths of only a few hundred kilometers. The concept of the involvement of long arcs in building and modifying continental structure is being further developed in new ribbon-continent models that are beginning to be applied to the

SEISMIC TOMOGRAPHY

Seismic tomography indicates the structure of Earth's interior from estimates of properties such as the propagating velocities of compressional waves (P waves) and shear waves (S waves). On the global scale, seismic tomography has indicated the distribution of heterogeneities within Earth's deep mantle, including (*a*) seismologically fast volumes indicative of the distribution of subducted slabs of lithosphere and (*b*) two prominent, Large Low Shear wave Velocity Provinces (LLSVPs) that form antipodal objects centered close to the equator just above the CMB.

North American Cordillera in Canada (Johnston 2008), Central America (Centeno-Garcia et al. 2008), and the United States (Hildebrand 2009) as well as to the Cordillera of northwestern South America (Altamira-Areyan 2009).

In 1988 I left the LPI, which has since grown greatly in stature, and became for the first time a full-time professor at the University of Houston. In about a year, however, I was persuaded to move temporarily to Washington, DC, to work in the National Research Council (NRC) preparing a report on the future of the solid Earth sciences. Specialist groups had put together draft sections of the report that together made a pile of paper more than a meter high. My job was to work mainly with Peter Wyllie, who was chairing the study, and Catherine (Kitsy) McMullen, a science writer, in boiling that bulk down into a coherent document. Scientists are always writing reports of this kind. These reports have a typical influential half-life of about a year, and ours was, I think, no worse than average. During the three years that I was in Washington, I was also given minor administrative responsibility in the NRC; I sat on the then National Academy Committee on Global Change and on a subcommittee of the Space Studies Board; and I edited *Tectonics*, a journal published by the American Geophysical Union (AGU). My time in Washington made me appreciate the conscientious and industrious efforts of scientists who work in and with government organizations as well as the infectious atmosphere of synthetic excitement in which they live.

Soon after I returned from Washington to Houston, I received an invitation to give the Geological Society of South Africa's du Toit memorial lecture. That is a great honor for students of African geology, so I thought for a long time before choosing a topic. I decided to talk about the past 30-million-year history of the African plate because I could address a broad range of different kinds of geology (Burke 1996). I realized that most of a South African audience would not be itching to learn about that subject, but I thought I could make it interesting. Preparing and writing up the lecture brought me back to hotspots, mantle plumes, and topography. I was able to provide a newly coherent picture of what had happened, especially by integrating published oil-industry observations of offshore deposition and the record of changing climate, in relation to Southern Hemisphere glaciation from ~34 Mya and Northern Hemisphere glaciation from ~3 Mya. I was able to elaborate on the idea of Burke & Wilson (1972) that the topographic relief of the African continent is <30 Ma old, which is a concept that I returned to in a later study with Yanni Gunnell (Burke & Gunnell 2008). Although I tried to be comprehensive in my du Toit lecture, I found myself unable to successfully integrate what I knew from surface geology with a geophysical understanding of the mantle structure beneath the African plate. I knew that seismic tomography showed a low-velocity volume near the base of the mantle under parts of Africa and the Indian Ocean (Dziewonski 1984), but its shape did not resemble anything I could see near the surface (see sidebar, Seismic Tomography). Therefore, I did not say anything about relations between the deep and shallow mantle, although others at the time did claim to have perceived a link (e.g., Lithgow-Bertelloni & Silver 1998).

CARBONATITES AND NEPHELINE SYENITES IN THE WILSON CYCLE

Repeating my du Toit lecture in nine places enabled me to see some of South Africa's classic geological localities and, on long drives, to get a feel for the country's distinctive geomorphology and spectacular scenery. Not surprisingly, I have since returned to South Africa several times and have developed a collaboration with Lew Ashwal, who had been a colleague at the LPI. The trigger for our new work was the publication of a catalog of African alkaline igneous rocks and carbonatites (Woolley 2000). From my study of an occurrence of nepheline syenite and carbonatite gneisses on the Circum–West African craton suture near Accra in Ghana (**Figure 2**), I knew this: Although both nepheline syenites and carbonatites typically occur in intracontinental rifts, Wilson Cycle theory predicts that such occurrences that become involved in ocean opening and that occupy a rifted continental margin will later become deformed, as had the gneissic rocks in Ghana during a cycle-ending continental collision.

Carbonatite: an intrusive or extrusive igneous rock, more than 50% of which is composed of carbonate minerals

Ashwal used his convalescence from an attack of pneumonia to put Woolley's catalog data into a spreadsheet. I drew Africa's Proterozoic suture zones on a map of the continent, and Sue Webb showed, by plotting the nepheline syenite gneiss occurrences on the suture map, that ~90% of the 32 catalog entries for deformed alkaline rocks lie in suture zones, as Wilson Cycle theory predicted (Burke et al. 2003).

We have since tested our model on deformed nepheline syenites in Europe, Asia, North America, and more locally in Africa in Malawi. In Malawi, Ashwal acquired an ~750-Mya date for initial eruption, presumably in a rift, of a nepheline syenite and an ~525-Ma age for gneissification of that nepheline syenite body in a Pan-African suturing event (Ashwal et al. 2007). Recently, we demonstrated the rift and suture relationships of the classic Grenville Province nepheline syenite gneisses and carbonatites of Ontario, Canada, and also established their relationship to the Monteregian Hills nepheline syenites and carbonatites of neighboring Quebec (Burke et al. 2008a).

Geologists describing Precambrian tectonic evolution have, with a few outstanding exceptions [such as Hoffman (1988)], seldom made full use of Wilson Cycle understanding. That is probably because they commonly consider areas that are too small to reveal the big picture. The recognition of the occurrence of nepheline and carbonatite gneisses in sutures indicates new opportunities for improving suture mapping and helping construct the Wilson Cycle picture.

PLUMES FROM THE CORE-MANTLE BOUNDARY

By the year 2000, deployments and networks of broadband seismometers and teleseismic studies had begun to reveal more about the structure of the deep mantle, but I was still disappointed to see little under Africa that I could relate to surface features. That changed after I read a paper on LIPs of the past 250 Ma (Eldholm & Coffin 2000); it showed a map of present worldwide LIP distribution as fairly uniform and mainly within a few tens of degrees of the equator. A second map indicated an earlier LIP distribution at ~110 Mya, before much of present plate structure was established. LIPs on that map were concentrated in two clusters near the equator. I immediately thought of the two equatorial bodies just above the CMB that had been discerned using seismic tomography (Dziewonski 1984). Those objects have been named Large Low Shear wave Velocity Provinces (LLSVPs) (Garnero et al. 2007), but I find that name cumbersome and prefer to refer to them as Tuzo (**T**he **U**nmoved **Z**one **O**f Earth's deep mantle) for the body under Africa, and Jason (**J**ust **As** **S**table **ON** the opposite meridian) for the body under the Pacific Ocean.

Eldholm & Coffin's (2000) 110-Mya map made me wonder whether plumes from the CMB of the kinds that Jason Morgan had hypothesized might have generated LIPs, and if so, whether they could be shown to have risen from Jason and Tuzo. I plotted the extent of Jason and Tuzo

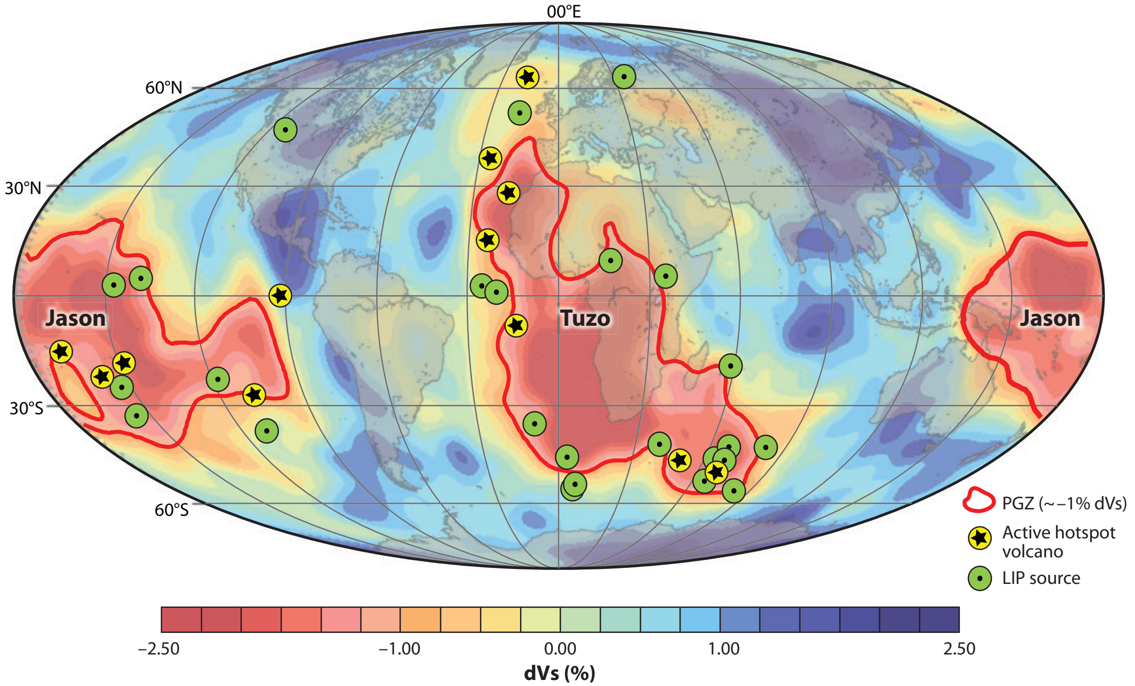


Figure 4

Shear wave velocity map at the core-mantle boundary (CMB) (from Becker & Boschi 2002). Thick lines are the plume generation zones (PGZs) that lie on 1% slow shear wave velocity contours on the CMB at the edges of Tuzo and Jason, the two Large Low Shear wave Velocity Provinces (LLSVPs) of the mantle (modified from Torsvik et al. 2006). Stars in circles are 12 active hotspot volcanoes recognized by Montelli et al. (2006) to have been generated by plumes that rose from the CMB. Hotspot volcano locations have been projected vertically down to the CMB, where they lie on or close to one or other of the two PGZs. Circles with dots show the locations on the CMB of 25 Large Igneous Province (LIP) sources of the past ~300 Ma. LIP source locations were mapped by rotating the LIPs using a hybrid reference frame to their sites at the time of their eruptions and dropping perpendiculars to the CMB from those sites. The concentration of the sources of both LIPs and hotspot volcanoes in the PGZs is striking, but the reason for the generation of the deep-seated plumes only from the PGZs is not fully understood (Burke et al. 2008b). Abbreviation: dVs, gradient of shear wave velocity.

projected radially from the CMB to the surface of Earth on a map of the world. On the same map, I then plotted the locations of Eldholm and Coffin's LIPs—not in the positions they are in today, but where they were at the times of their individual eruptions. To do that, I used maps in a report on Phanerozoic plate reconstructions (Scotese et al. 1987) that showed longitudes for the past 200 Ma as well as latitudes. My resulting map showed that LIP locations at their eruptions were within the upward projected areas of Tuzo and Jason. I inferred that LIP sources were (*a*) close to the CMB and (*b*) within Tuzo and Jason. I was delighted because I had found evidence supporting Morgan's suggestion that plumes rose from the CMB (Morgan 1971), so I tried, unsuccessfully, to publish my findings. My failure turned out to be a good thing because I soon had the opportunity to show my results to Trond Torsvik, whom I had met when we were both working with Ashwal and Webb in Johannesburg and who invited me to visit his group in Trondheim, Norway.

Torsvik asked me, What poles did you use to rotate these objects to their eruption locations? I explained that I had used maps in a report that did not specify poles. He responded that reporting poles is essential and that he would do it. Our conversation was at about 10:00 AM. At 6:00 PM he walked out of his office carrying a map that he said confirmed my findings. In fact, it did more because it showed that the plume sources were not just within Jason and Tuzo but close to their edges

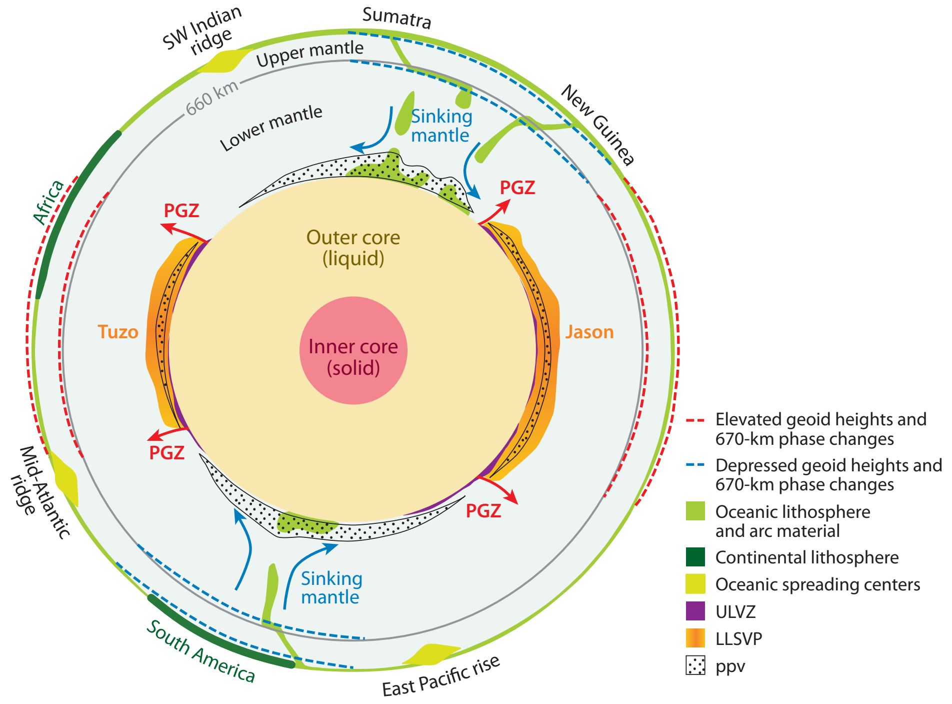


Figure 5

Equatorial cross section of Earth showing mantle circulation dominated by sinking mantle, which consists of lithospheric slabs and slab debris (*shown between blue arrows*), and rising mantle only in vertical plumes from plume generation zones (PGZs). Large Igneous Provinces (LIPs) and hotspots of deep origin, such as Hawaii (see **Figure 4**), have been generated by the vertical plumes. The PGZs (*red arrows*) are narrow and confined to the edges on the core-mantle boundary of Tuzo and Jason, the two Large Low Shear wave Velocity Provinces (LLSVPs) of Earth's mantle. Stippled areas indicate volumes within which post-perovskite mineralogy (ppv) may occur; these may form in the deepest mantle both inside and outside the LLSVPs. Ultralow-velocity zones (ULVZs) are also indicated. Tuzo and Jason are suggested to have been stable in their present antipodal positions on the CMB and close to the equator for hundreds of millions of years, and perhaps for much longer. They may also be chemically isolated, in which case all the igneous rocks of Earth have been derived from the remaining 98% of the mantle mass. Modified and simplified from Trønnes 2009.

on the CMB. After the customary rejections, we succeeded in publishing two papers: one on our initial findings (Burke & Torsvik 2004) and the other after Bernhard Steinberger joined Torsvik. That was a more complete study showing that hotspots (such as Hawaii) that had been suggested to be of deep mantle origin also lay vertically above the edges of Tuzo and Jason (**Figure 4** and Torsvik et al. 2006). At the same time, Raffaella Montelli and her colleagues were showing evidence, using a new teleseismic approach, that plumes underlying hotspots rose from the deep mantle (Montelli et al. 2004, 2006). The occurrence of a population of plumes rising from the CMB and the generation of that population almost entirely from the edges of Tuzo and Jason is now well established (**Figures 4** and **5**).

LONG-TERM STABILITY OF JASON AND TUZO

Seismology is a powerful tool for determining the structure of the interior of Earth, but generally it provides only limited and indirect evidence about what has happened in the past. However, in an innovative paper, Dziewonski et al. (2010) do recognize the long-term stability of the LLSVPs in the deep mantle on the basis of seismological evidence. Our finding that LIPs dating back to 300 Mya, irrespective of age, were erupted from PGZs on the margins of Jason and Tuzo at the CMB adds something to the seismic results by showing that those two large deep structures, which each contain ~1% of the mantle mass, have long been where they are now with respect to the spin axis. Each is also (*a*) antipodal and (*b*) centered close to the equator. For Tuzo, we demonstrated stability for at least 300 Ma; for Jason, for at least 150 Ma (Burke et al. 2008b). More recently, we have shown the longer-term stability of Tuzo and Jason by finding that ~80% of all kimberlites erupted during the past 550 Ma have come from the lithospheric mantle above plumes from the margins of Tuzo and Jason on the CMB (Torsvik et al. 2010).

Long-term stability in the deep mantle comes as something of a surprise. Geodynamic modelers commonly treat everything below the rigid lithosphere of Earth and above the rigid inner core as flowing (e.g., van Keken et al. 2002), although some geochemists have suggested that isolated reservoirs exist within the flowing mantle. Implications of long-term stability for Tuzo and Jason were considered in Burke et al. (2008b). They relate to results such as the finding that the +10-m contour of the residual geoid (Hager 1984) matches closely the footprints of Tuzo and Jason projected upward from the CMB to Earth's surface. That observation indicates that the residual geoid is as stable in its position with respect to the spin axis as are Tuzo and Jason. The length of time before ~550 Mya that Jason and Tuzo have been where they are now, the way they formed, and their time of origin are unknown.

Zhong et al. (2007) suggested that Tuzo formed beneath Pangea when Earth's only supercontinent (Burke 2007) was assembled at ~310 Mya. The idea was that Tuzo consists of "a thermochemical pile" of material driven together by converging subducted slabs of lithosphere. Jason, on Zhong's interpretation, has been where it is beneath the Pacific Ocean for longer. However, findings from our kimberlite study that Tuzo has been stable in its present position with respect to Jason and the spin axis since ~550 Mya are incompatible with Zhong's suggestion. Evidence from the planet Mars may provide a better indication of the time of origin of Tuzo and Jason (Burke et al. 2008c). The Martian areoid is analogous to the geoid in representing mass that is not accommodated by the hydrostatic figure of the planet. It similarly displays two positively elevated parts that are antipodal and lie on the equator. One of those parts closely matches the Tharsis region at the planet's surface, which is apparently made of basalt and is Late Noachian, perhaps ~4.00 Ga in age (Lapen et al. 2010, Nimmo & Tanaka 2005). Tharsis appears to represent a response to a catastrophic event. A similar long-ago event on Earth associated with the formation of Jason and Tuzo would have left no objects at Earth's surface comparable with Tharsis, because plate tectonic processes would have ensured their rapid destruction but would have left dense material, such as Jason and Tuzo, at the mantle's base.

A possible scenario for the geodynamic evolution of Earth could then involve establishment of Jason and Tuzo, each constituting ~1% of mantle mass on the CMB in the aftermath of a catastrophic event. The last known catastrophe of an appropriate scale on Earth was the moon-forming event a few tens of millions of years after Earth formed. Isolation of stable Jason and Tuzo would then represent the final phase of Earth's convective response to the moon-forming event. After that, the geodynamics of Earth's mantle and crust settled into the present regime in which plate tectonics has dominated (Harrison 2009). Under that regime, only lithospheric slabs, any associated material entrained with those slabs, and slab debris have descended to the CMB. On the evidence of the past ~550 Ma, complementary material rising from the CMB to balance the material budget has come from the PGZs at the edges of Tuzo and Jason, and also from the three small PGZs responsible in the past 300 Ma for the Columbia River, Ethiopian, and Siberian LIPs (Burke et al. 2008b).

The current geodynamic behavior of Earth and that of the past >4 Ga can then be envisaged as sketched in **Figure 5** (simplified from Trønnes 2009):

i. Plate tectonics has dominated the behavior of the shallow mantle as recorded in the Wilson Cycle record for times before ∼180 Mya (e.g., Harrison 2009) and in the plate tectonic record since. LIPs, generated by plumes from the edges of Jason and Tuzo, are an essential complement to the plate tectonic process because they represent the budgetary reimbursement from the deep mantle that balances the withdrawals from the shallow plate circuit represented by slabs of lithosphere in the deep mantle.
ii. Plate tectonic processes have been accompanied by the descent of lithospheric slabs and slab-related material to the CMB.
iii. Plumes, some of which have generated LIPs at Earth's surface, have risen from PGZs on the edges of Tuzo and Jason, and in a few cases (probably <10%), from the edges of the three low-velocity bodies responsible for the Siberian, Ethiopian, and Columbia River LIPs. The plumes from PGZs on the CMB have balanced the material budget of the deep mantle. Various topographic, bathymetric, and igneous objects at and near Earth's surface have been suggested to be related to material rising from the deep mantle. Where those objects are in areas remote from the PGZs, they can generally be better explained as of shallow mantle origin, although some surface topographic features such as the Roraima highlands and the Bermuda Rise remain presently unexplained.

NO CONCLUSIONS, ONLY A SUGGESTION

I suggest that it is now time to integrate the concepts of plate tectonics, the Wilson Cycle, and plumes from the CMB into an improved geodynamic model of mantle behavior over Earth history by assimilating the observations that have been summarized here on (*a*) the bodies that I have named Tuzo and Jason and (*b*) the restriction of plumes from the CMB to sources within the PGZs, mainly on the margins of Tuzo and Jason. Current geodynamic models have not yet accommodated these observations. I am keen that my ideas should be tested in new models, but I must warn that my suggested explanations of many of the phenomena that I have discussed in this review have later been proved wrong, or at least in need of modification. My suggestions of (*a*) the long-term stability of Tuzo and Jason and (*b*) the idea that the descent of slab material from the upper mantle and the ascent of plumes from the PGZs account for the material budget of the deep Earth are equally at risk.

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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