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Flood basalts and large igneous provinces from deep mantle plumes: fact, fiction, and fallacy

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Abstract

Flood basalts, large igneous provinces and all intraplate magmatism are almost unanimously ascribed to features called mantle plumes in current geodynamics literature. In the starting plume model, flood basalts and any associated hotspot tracks have been ascribed to the melting of a large plume head and a narrow tail respectively, and the Deccan flood basalt province of India and the Laccadives–Maldives–Chagos–Réunion Island volcanic chain in the Indian Ocean are widely thought to be an excellent example supporting this model. However, the existing geodynamic, thermal, petrological, geochemical and geochronological constraints on Deccan volcanism do not require or support a mantle plume origin. A new interpretation made here rules out any role for the Réunion plume in Deccan volcanism, and even questions the notion that the Réunion 'hotspot' is the manifestation of an underlying Réunion 'plume'. The Laccadives–Réunion hotspot track is viewed here as caused by a southward-propagating fracture, based on the published radiometric age data, and the ultimate cause of Deccan volcanism is argued to be mega-scale lithospheric rifting. The appreciation of the great original areal extent of the Deccan, nearly as far as the southern tip of peninsular India, solves the hitherto unsolved problem of the cause of the uplift of the 1500-km-long Western Ghats escarpment, this cause being combined surface erosion and magmatic underplating. Hypothesized mantle plumes do not appear responsible for most large igneous provinces; instead, their very existence is questionable. No geological evidence of any kind — geochemical, petrological, thermal, topographic — requires mantle plumes. The self-contradictions and special pleading that proliferate in plume explanations are unjustified and unnecessary if one accepts, based on various grounds, that a globe-encircling enriched mantle layer (the 'perisphere') may overlie a deeper depleted mantle, which is the source of MORB. The systematic geochemical variations along the Laccadive–Réunion chain can be explained by this model and are not consistent with a plume tail experiencing entrainment from surrounding MORB mantle, as in the current plume model. Hotspot tracks may not reflect plate motion above stationary deep mantle upwellings but the stress state of the lithosphere, as originally proposed decades ago. The locations of large igneous provinces, hotspots and hotspot tracks are strongly controlled by lithospheric architecture and history: they are not placed randomly on the globe, indifferent to the surface geology, neither are large igneous provinces sudden chance events within the vastness of geological time (which plume theories typically allege).

Keywords: intraplate; volcanism; flood basalt; LIP; Deccan; mantle plume; hotspot

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"Whether or not it is eventually validated by overwhelmingly convincing evidence the 'scientific picture' presented by this sort of theory is inevitably schematic and oversimplified. The danger is that its limitations will not be adequately recognized, and that it will be extrapolated recklessly into an all-embracing dogma."

John Ziman (1978)

1. Introduction

Volcanism, in particular basaltic volcanism, is a fundamental process in the thermal and chemical evolution of all the terrestrial planets of the Solar System (Middlemost, 1985). "Basalts have erupted on Earth throughout known geologic history and on the Moon for more than a billion years before it became thermally quiescent 2.5 to 3 billion years ago. From the Earth today to the small asteroid Vesta 4.5 billion years in the past, on Mars, Venus and Mercury, the generation of basalts has spanned the history of the Solar System and an enormous range of planetary mass" (Basaltic Volcanism Study Project, 1981: xxvii). Basaltic magmas are "probes of chemical compositions and physical conditions in planetary mantles that are hidden from direct observation by depth and time" (op. cit.).

On Earth basaltic magmas are generated in all tectonic settings: convergent and divergent plate margins and intraplate areas. Terrestrial volcanism of the 'flood basalt' type comprises voluminous outpourings of dominantly tholeiitic basalts in intraplate settings, which constitute the so-called 'large igneous provinces' (LIPs). This term itself does not uniquely imply either an intraplate setting or basaltic lavas, but has been usually restricted to them, and not, for example, to huge granitic batholiths at active continental margins. Therefore, in the present paper also, what is implied by LIPs is basaltic, intraplate LIPs. Giant flood basalt fields, such as the Deccan Traps in India, have volumes of 10^5 to 10^7 km^3 (Walker, 1993) often erupted in short time periods of 1–4 million years (MY) (e.g., Courtillot et al., 1986, 1988; Duncan and Pyle, 1988; Venkatesan et al., 1993; Baksi, 1994).

The origin of flood basalts and LIPs is one of the most fascinating and important problems in the Earth sciences. The LIPs, which include continental flood basalts (CFBs) and oceanic plateaus, and some volcanic rifted margins, are now almost unanimously ascribed to features called 'mantle plumes'. Superficially the mantle plume explanation seems attractive and has had a tremendous appeal. However, its numerous built-in fallacies, contradictions and failings are unfortunately little discussed in much of the current literature, and it has acquired the status of an unchallengeable dogma and an obvious fact. I discuss here the three types of evidence that are usually cited for plume involvement in LIP formation: topographic, thermal, and compositional (e.g., Saunders et al., 1992), and show that they are not definitive, even collectively. Having embraced plume models myself earlier, I criticize my own errors wherever appropriate. I also point out what I consider to be far more plausible alternatives, and offer/accept a new interpretation of global hotspots and hotspot tracks that discounts plumes. In this paper, a 'plume' or 'mantle plume' is an uprising diapir of mantle material, usually said to have risen from the core–mantle boundary, and a 'hotspot' is a hot region, usually experiencing magmatic-volcanic activity, at the surface of the Earth, and well within the major plate boundaries.

I discuss the numerous problems with the plume model here with special reference to the Deccan flood basalt province of India, for three simple reasons: (1) it is a major LIP of global importance, extensively studied; (2) it is usually cited as a classic example of a plume-head-produced LIP (which it does appear to be at first sight), and (3) it has been, and remains, my chief research interest, and I feel more competent to talk about it than about any other LIP.

2. Evolution of the mantle plume concept

Since the original suggestion of Wilson (1963) that Hawaiian volcanism, well within the Pacific plate, was due to a fixed melting anomaly over which the plate moved, and which in the process generated a chain of volcanoes showing a systematic age progression, the mantle plume idea has had continuously increasing appeal, so that in virtually all modern works, LIPs, continental or oceanic, are ascribed to mantle plumes by convention. The plume hypothesis has itself constantly evolved. Wilson (1963) thought such intraplate volcanoes to be fed from the interiors of convection cells. Morgan (1971) envisaged plumes as part of the general convection system of the Earth. The modern thinking about plumes is substantially different; plumes are considered to reflect a secondary mode of convection unrelated to (and little affected by) plate-scale convection (e.g., Sleep, 1992).

Fluid dynamical and numerical modelling (e.g., Richards et al., 1989; Campbell and Griffiths, 1990; Griffiths and Campbell, 1990) argues that flood basalts and LIPs must originate from plumes, and these plumes must have risen from the D″ region just above the core–mantle boundary due to thermal and gravitational instabilities, and as a plume moves upwards it entrains material from the surrounding lower mantle, forming a large, bulbous, blob-like 'head'. With continuing ascent and entrainment the head becomes bigger and bigger, and is connected to the source region through a narrow, axial, tube-like conduit, the 'tail', along which primitive plume-source material rises, with little entrainment of the surrounding mantle, at least in principle. This is the model upon which a huge number of geologists, petrologists, geochemists and geodynamicists have based their research. According to a recent numerical modelling study, however (van Keken, 1997), most of the source material in a starting plume would be contained in the plume head and the conduit would be composed largely of entrained material. Therefore, experimental fluid dynamical and numerical modelling appear to be mutually contradictory and to have limited value. The course of action is clear: to construct our 'world view' of intraplate volcanism based on *geological* data, supplemented by geochemical and geophysical data. These three kinds of data I would call real, or factual, data.

3. Laboratory models vs. the actual surface geology

Convection modellers usually assume a uniform idealized boundary condition: no plates, no cracks or fracture zones, no continents, no geology, no lateral temperature variations in the mantle (see criticisms by Anderson, 1996, 1998a, 1999). Anderson (1996) has argued that thermal boundary layers such as at the core–mantle boundary are not stationary, and convection in a high-Rayleigh-number medium such as the mantle is turbulent or unsteady; and he has also correctly observed that the narrow, fixed, tubular conduits of plume theories require special circumstances to form and be maintained, even in the laboratory. Such artificial laboratory conditions are, for example, a non-convecting tank, no phase changes, constant material properties, no plate motions, no internal heating of the plume (all heating of the plume fluid is external), artificial injection of superheated fluid at the base of another fluid, and so on.

As far as the actual surface geology is concerned, several simplistic schemes and all superficial textbooks show pictures of hotspot tracks due to a plume beneath a moving plate, but never do they show the numerous extensive *fracture zones* that traverse the entire plate in question. The bathymetric maps of Smith and Sandwell (1994) show an extensive, pervasive, fracture zone fabric in the ocean basins. Many ocean island chains — with or without any systematic age progression — are along fracture zones, and flood basalt provinces are at the orthogonal intersections of the fracture zones (Smoot, 1997; Smoot and King, 1997; Anderson, 1998b, 1999). Most, if not all, continental flood basalts (CFBs) likewise show a definite lithospheric control on their locations and are associated with/erupted from major lithospheric breaks or rifts at craton–noncraton boundaries (Kazmin, 1991; Anderson, 1994a; King and Anderson, 1995). Most CFBs are built on long-subsiding sedimentary basins, indicating crustal extension (e.g., Deccan, Siberian; see below). These facts emphasize the need for a *historical* approach to the problem of LIP origin, which is utterly lacking in the plume model.

4. The correct approach to investigation of LIP origins

The geographic locations of LIPs are not random, and I opine that LIPs are not chance, random events in geological history either, having no connection with the past and produced by plumes suddenly popping up beneath plates. The past *history* of the lithosphere on which LIPs rest, and the *processes* of LIP formation need equal attention in any logical approach to the investigation of why and how they are emplaced.

Two conditions — an anomalously hot mantle, and an unusually thin lithosphere — have been deemed essential for flood basalt genesis, almost unanimously (e.g., Chandrasekharam and Parthasarathy, 1978; Richards et al., 1989; Carlson, 1991; White, 1992; White and McKenzie, 1989, 1995; Sheth and Chandrasekharam, 1997a), as both should result in huge melt volumes and high eruption rates. The prerequisite of a thin lithosphere is almost certainly true, noting that the parental magmas of many flood basalts such as the Deccan may have originated, or at least last equilibrated, at quite shallow depths in the mantle (∼35–40 km, e.g., Jacques and Green, 1980; Sen, 1988).

The Deccan flood basalt province is associated with at least three major rift zones (Sheth and Chandrasekharam, 1997a,b), which were sites of extensive, voluminous sedimentation well before the ∼65 Ma Deccan event, at least as far back in time as Jurassic or possibly even Late Triassic (∼200 Ma, e.g., Biswas, 1987, 1988; Kaila, 1988; Sheth, 1999). In the Columbia River flood basalt province of northwestern U.S.A. there was substantial crustal thinning and rifting before volcanism began (Catchings and Mooney, 1988). A very widespread idea in current literature, contrary to logic, and anti-historical (blind to the previous history of a flood basalt province), is that all crustal extension, thinning and rifting occurred in these provinces *subsequent to* the eruptive episode (e.g., Richards et al., 1989; Hooper, 1990, 1997). However, it must be appreciated that such extension, thinning and rifting may be *necessary* for flood volcanism itself, and without a thin lithosphere melt volumes may only be small (White, 1992; Sheth and Chandrasekharam, 1997a; Sheth, 1999).

Ideas picturing flood basalt genesis and emplacement without lithospheric thinning or rifting, or at least extension, are self-contradictory; worse, such ideas do not explain why the lithosphere had to thin or rift, and due to what cause, when the volcanism had already occurred and waned. The hypothesized plume is argued to cause this thinning and rifting, but thinning and rifting were already underway tens of millions of years ago, as well reflected in the long sedimentation history of rift zones associated with flood basalt provinces and in episodes of early alkaline magmatism well before the flood basalt event. Noting this, Sheth (1999) concluded that a continuously existing tensional environment before, during, and subsequent to the eruptive episode, is realistic, and satisfies both facts and logic. If seafloor spreading ever begins it does so after the main volcanic episode is over, giving a false impression that rifting occurred after the volcanism (White, 1992). Implicit in all the misinterpretations regarding the relative timing of rifting and volcanism is the premise that rifting is an instantaneous process, and is marked by the event of beginning of seafloor spreading. Since this is usually subsequent to the flood basalt event, this has been taken as strong confirmation that “rifting succeeds flood volcanism and does not precede it”.

This last statement is typical of much flood basalt–mantle plume literature today. For example, Richards et al. (1989), and most recently Courtillot et al. (1999), argue that the Siberian flood basalts, which form the largest CFB province on Earth, have erupted without rifting, and this is evidence that rifting is not necessary for flood basalt events. However, Czamanske et al. (1998) report that the Siberian flood basalts are underlain almost everywhere by terrigenous sedimentary rocks of the Tungusskaya Series, which includes the Tunguska coaliferous basin, thought to be the largest in the world. The Tungusskaya Series, Middle Carboniferous (∼320 Ma) to Late Permian (∼250 Ma) in age, varies in thickness from 100–150 m to 1400 m. No evidence of plume-related uplift can be found in the sedimentary record. (The numerical modelling of Farnetani and Richards, 1994 predicts pre-magmatic lithospheric uplift of a few kilometres.) The Tungusskaya Series accumulated during an environment characterized by mild but continuous subsidence, balanced by sedimentation, and accumulation of the entire volcanic sequence was accompanied by almost perfectly balanced subsidence with the preservation of flat relief, probably near sea level (see also Kamo et al., 1996). Lack of surface uplift accompanying the voluminous volcanism in East Greenland has been documented by Larsen and Marcussen (1992). Sheth (1999) interpreted these field data as suggesting extensive and progressive *conditioning* of the lithosphere that would ultimately facilitate and culminate in the final rapid emplacement of a major LIP, millions or tens of millions of years later. If this sounds like a wild idea, then note that Kent (1991) has actually argued for mantle plume incubation beneath the eastern Gondwana lithosphere for a time period of as much as 150 million years.

5. The basic premises of the plume explanation

The fundamental premises of the plume model are open to what can be called criticisms of a 'conceptual' or 'philosophical' type.

As an example, in the Richards et al. (1989) and Campbell and Griffiths (1990) model, the central conduits of plumes are supposed to be considerably hotter than the surrounding mantle due to a general lack of entrainment (Campbell and Griffiths, 1990), and plume heads also apparently satisfy the volume requirement, which makes them a favourite explanation of many who note the large volumes erupted in LIPs and their apparently short duration, though actually such proposals are illogical because neither huge melt volumes nor high eruption rates constitute (1) any real *evidence* for a plume (/head), (2) evidence for *exclusively* a plume (/head).

A more subtle point, in this connection, is that the large melt volumes and high eruption rates ascribed to plume heads are ascribed to them *by definition*; they are not demonstrable properties but have been assumed, i.e., the reasoning here is that "a plume head would provide large melt volumes at high eruption rates, because it is very large and hotter than normal". However, plumes said to be 'incubating' beneath thick lithosphere only generate small-volume, alkaline, volatile-rich activity at the surface (e.g., Thompson and Gibson, 1991); this is usually ascribed to the large lithospheric thickness.

Clearly, several ad hoc elements form the basis of the whole plume head idea for LIPs, and thus the plume head explanation, which is actually not the most logical one, has so far been the most *convenient* one, effectively shutting out the entire creative thought process of the human mind. Recently, the head–tail model has now been declared by its proponents to require major modifications to explain the rates and volumes of CFB volcanism (Corderey et al., 1997). They abandon the head–tail idea and disregard entrainment or cooling, and state that for plume temperatures in the range 1400–1600ºC and lithosphere ages in the range 6.25–100 Ma, plumes composed solely of pyrolite generally do not melt at all, or produce too little melt to explain CFBs, but similar plumes with 15 wt% eclogite, produced from ancient subducted oceanic crust, and mingled in a pyrolite matrix, are capable of generating CFB melt volumes. However, read on, and it should seem that the total evidence from LIPs, geological, geochemical or geophysical, is against *any* kind of plume that arrives exclusively from the deep lower mantle. It is interesting and instructive to discuss and realize the fallacies and contradictions associated with the plume model, to realize that the price of sacrificing the historical approach, so unique to the science of geology, can be enormous, and disregard of field data, which are so vital to erecting, testing, and choosing between hypotheses, can be disastrous — in the sense that the science and the scientist become completely alienated from the real working laboratory, i.e., the actual, natural world.

King and Anderson (1995) argue that plumes are not necessary for flood volcanism and rapid pull-apart (splitting) of the lithosphere along craton–noncraton boundaries, over extensive hot cells in the mantle, can explain the large erupted volumes, as well as the rapid turn-on and turn-off of the volcanism. In convective partial melting (e.g., Mutter et al., 1988; King and Anderson, 1995), which does not require abnormally hot mantle, flow rates are increased and more material is processed through the melting zone rapidly, resulting in large melt volumes. Sharma (1997), however, expressing a common opinion, considers a plume head a more satisfactory explanation, stating that the amount of melt actually produced in the Siberian Trap event is more than 100 times that calculated by King and Anderson (1995). However, the way the large-volume requirement is fulfilled by the plume model is that the proposed plume head is a very large one, and can therefore easily supply the melt volumes needed. Practically no upper limit or restraint exists in this assumption — the larger the melt volumes seen erupted, the larger must have been the plume head (e.g., the superplume idea of Larson, 1991). This is obviously not a scientific solution at all.

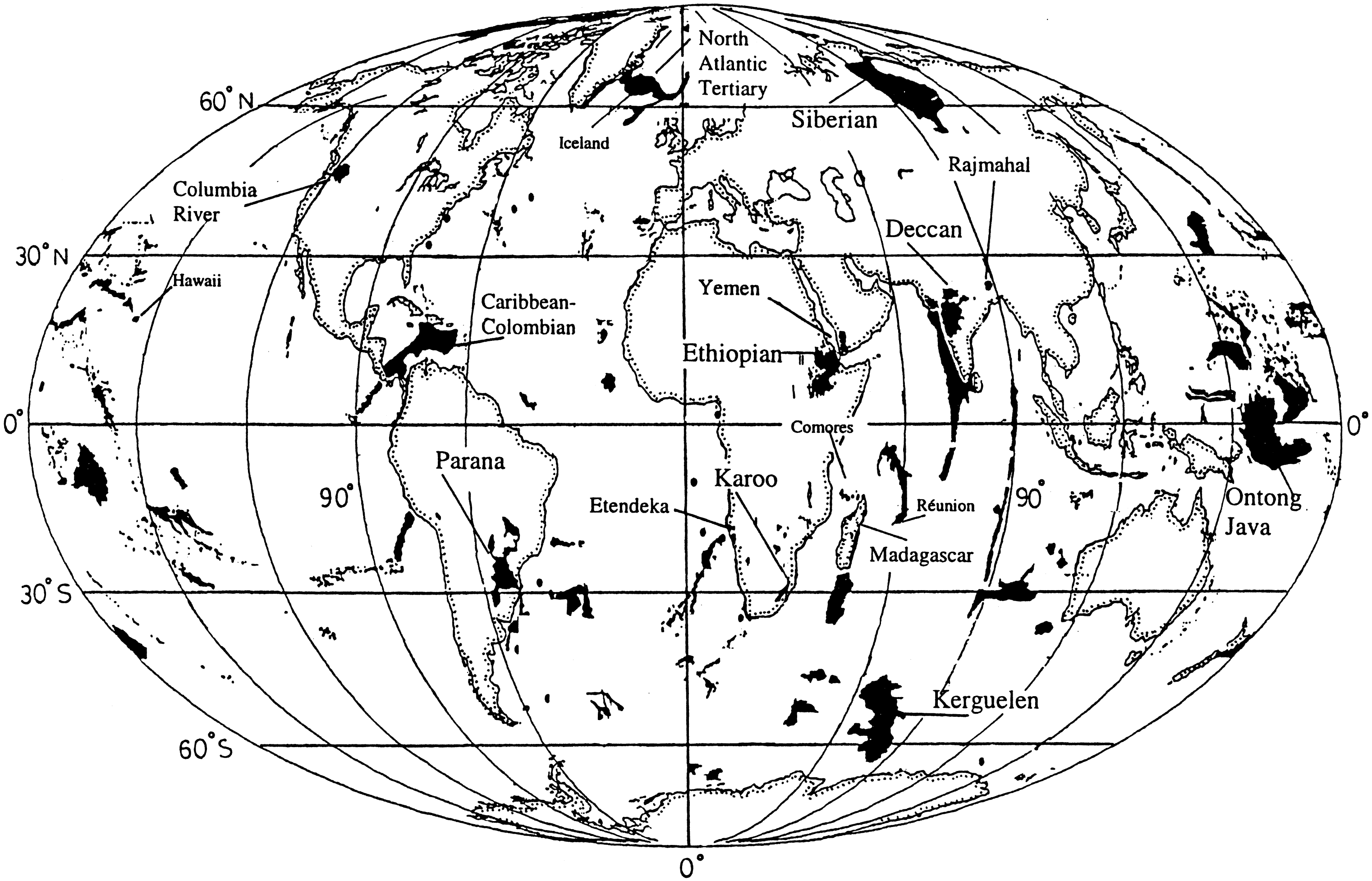

Fig. 1. Global distribution of large igneous provinces (shown in black), modified from Coffin and Eldholm (1992).

Indeed, the rapid turn-on and turn-off of flood volcanism appear to reflect lithospheric stress conditions. The rapid turn-off indicates that the system is one which can be quickly 'switched off', and this is very difficult to reconcile with the idea of a large mass flowage of the mantle, noting its much larger thermal inertia (Bailey, 1983). But the passion for the plume model has been so strong in geodynamics thinking that not only massive flood volcanism, but also low-volume magmatism such as that of kimberlites, has been ascribed to plumes (e.g., Crough et al., 1980), for the sole reason that in many cases kimberlites cannot be related to known lithospheric discontinuities or rifts.

6. CFB–hotspot track pairs: how many are valid?

Richards et al. (1989) listed several pairs of major CFBs and associated hotspot tracks ending at presumed plumes (see Fig. 1 for the global distribution of LIPs): the Deccan CFB and the Réunion hotspot connected through the Laccadives–Maldives–Chagos Ridges, and the Rajmahal CFB of eastern India and the Kerguelen hotspot through the 90ºE Ridge, for example. A hotspot track between the Siberian CFB and its presumed causative plume, the Jan Mayen hotspot (Morgan, 1981), or any other hotspot, has never been found, and it was concluded recently by Haase et al. (1996) that Jan Mayen is not the product of a mantle plume but owes its existence to the unique juxtaposition of a continental fragment, a fracture zone and a spreading axis in that part of the North Atlantic Ocean. Saunders et al. (1992) opine that if a hotspot track is not seen it is not evidence that it does not exist; rather, due to subsequent plate movements it may not have been detected. This conclusion has an element of escapism. The Columbia River province is widely said to be due to initiation of the Yellowstone plume, but the Yellowstone plume track passes some 300 km south of the Columbia River outcrop (see discussion by Dickinson, 1997 and fundamentally different non-plume interpretations by Smith, 1992 and Anderson, 1999).

The Afar plume has no hotspot track, though this is said to be because the plume onset is recent (~30 Ma) and the African plate nearly stationary (V. Courtillot, pers. commun., 1996). On the other hand, the three rifts in the Afar region, namely the East African rift, the Red Sea rift and the Gulf of Aden rift, have propagated *towards* each other, not away (Courtillot, 1980, 1982; Courtillot et al., 1987; V. Courtillot, pers. commun., 1996), unlike as predicted by the plume model (Burke and Dewey, 1973; Burke and Wilson, 1976).

For the Rajmahal CFB, Mahoney et al. (1983) wrote long ago that the Kerguelen plume could have been a heat source, not a source of material. Similar conclusions have been reached by Kent et al. (1997), who do propose an indirect role for the Kerguelen plume in Rajmahal volcanism, but write that the Rajmahal CFB was not directly derived from the Kerguelen plume or, for that matter, the Crozet plume (Curray and Munasinghe, 1991), but from normal Indian MORB mantle. Peate (1997), reviewing our knowledge of the Parana–Etendeka province, completely rules out a role for the Tristan plume, except as a possible source of heat, and Storey et al. (1997) likewise consider a plume head origin of the Madagascar CFB unlikely based on various considerations. Thus, out of the numerous CFB–hotspot pairs cited by Richards et al. (1989), the Siberian–unknown plume, Rajmahal–Kerguelen plume, Columbia River–Yellowstone plume, and Parana–Tristan plume connections have been invalidated by newer insights, though Courtillot et al. (1999) still accept all of them as valid. I show here, however, that the classic Deccan–Réunion connection is as invalid and incorrect as the other pairs.

7. From Deccan to Réunion: no role for a mantle plume?

7.1. General

According to the widely accepted model of Morgan (1972, 1981), the cause of eruption of the Deccan flood basalts was the upwelling of a mantle plume beneath the northerly drifting Indian continent in the Late Cretaceous. This plume, the Réunion plume, subsequently gave rise to the Chagos–Laccadives and Mascarene aseismic ridges, and is now active beneath Réunion Island in the Indian Ocean (Fig. 2). The plume model for Deccan vol-

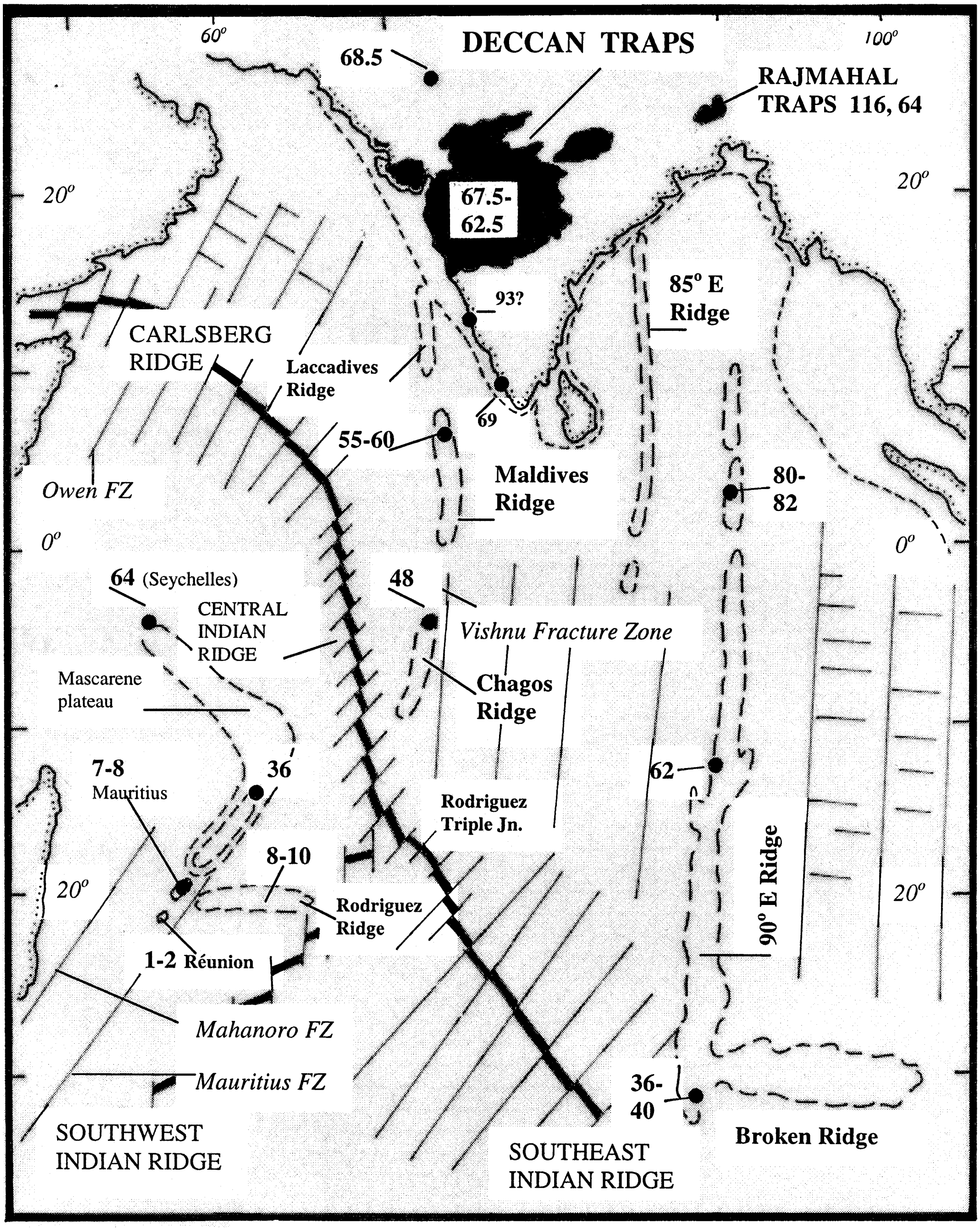

Fig. 2. Map of the Indian subcontinent and the Indian Ocean region, showing the main tectonic and physiographic features of the latter. Modified after Duncan and Pyle (1988) and Duncan (1990). Fracture zones are from Norton and Sclater (1979) and Duncan (1990). The outcrops of the Deccan and Rajmahal Traps are also shown. Numerals are the radiometric dates for various Deccan rocks: early alkaline rocks in the north (68.5 Ma, Ar–Ar, Basu et al., 1993); the Western Ghats sequence (67.5 Ma, Ar–Ar, Duncan and Pyle, 1988; 62.5 Ma, Ar–Ar, Venkatesan et al., 1993); a dike from the Tapi rift (65.5 Ma, Ar–Ar, Sheth et al., 1997); St. Mary's Island volcanics (93 Ma, K–Ar, Valsangkar et al., 1981) and Kerala dikes (69 Ma, Ar–Ar, Radhakrishna et al., 1994).

canism has been embraced by a very large number of workers in the past, including myself, but it has certainly not been without problems, which remain still unanswered, and several which I raise in the present work. One of the yet unexplained anomalies in the Deccan–Réunion link, pointed out by Duncan (1990), and Burke (1996), is the 450-km-long, E–W-trending Rodriguez Ridge (Fig. 2), which is completely made up of basalts with ages of 8–10 Ma, does not show any internal age progression, and which does not lie along the Deccan–Réunion track. I show below that the present picture for the Deccan–Réunion hotspot track is actually inconsistent with the plume model. A new alternative interpretation of the available data that I propose has greater explanatory power, and discounts the idea that the Réunion 'hotspot' is the manifestation of a Réunion 'plume'.

In the Western Ghats region (Fig. 3) of the Deccan Traps the tholeiitic lavas have a stratigraphic thickness of $\sim$3,000 m, and there is no statistically significant age difference between their base and top according to Duncan and Pyle (1988) (mean ^{40}Ar–^{39}Ar age 67.5 $\pm$ 0.3 Ma; but see below). Low-volume, often alkaline, magmatism along the rift zones of the province is however known to have shortly preceded and succeeded the main tholeiite phase (Basu et al., 1993; see Sheth and Chandrasekharam, 1997b). Sheth and Chandrasekharam (1997b), accepting the starting plume model for flood basalt genesis (Richards et al., 1989; Campbell and Griffiths, 1990), accepted the role of the Réunion plume head in the generation of the voluminous Deccan tholeiites, and argued that the varied rift magmatism required a significant role for the Indian continental lithosphere as well. To explain the early alkaline magmatism in the northern part of the province (^{40}Ar–^{39}Ar age $\sim$68.5 Ma, Basu et al., 1993), with ocean-island-basalt (OIB)-like Sr–He isotopic characteristics, Sheth and Chandrasekharam (1997a) argued that the plume incubation model (Kent et al., 1992; Saunders et al., 1992) was more realistic for the Deccan than the plume impact model (Campbell and Griffiths, 1990).

7.2. Still older ages in the north

Mahoney et al. (1996) found OIB-like chemical and Nd–Sr–Pb isotopic characteristics for pillow basalts belonging to the Parh Group in southern Pakistan, dated by them at 76 Ma, and suggested that the Parh Group could represent pre-Deccan submarine volcanism of the Réunion plume. These rocks lie along the northern end of the extrapolated Réunion hotspot track in a tectonic melange. If the Parh Group is indeed related to the Réunion hotspot, a far greater incubation time (more than 10 MY) is indicated for the plume, and the OIB-like characteristics of the Parh Group and Basu et al.'s (1993) rocks (thought to be signatures of the Réunion plume axis) are seemingly consistent with northerly movement of the Indian plate over a fixed melting anomaly. In addition, Radhakrishna et al. (1994) reported ^{40}Ar–^{39}Ar ages of $\sim$80 Ma for an alkali gabbro dike and $\sim$69 Ma for numerous doleritic dikes from Kerala in southern India (Fig. 3). They argued that the 80 Ma age for the alkali gabbro dike (which has transitional-OIB characteristics) requires Réunion plume incubation for 15 MY (i.e. 15 MY before the 65 Ma main volcanic event).

It is important to note, before we progress in this discussion, that all the ^{40}Ar–^{39}Ar ages existing in the Deccan literature are not comparable in a straightforward manner, as different experiments have involved variable neutron fluxes, monitors and irradiation parameters, etc. (see Baksi, 1994), and this is the reason why there can be a difference in age of as much as 2 to 3 million years for the *same* flow, measured by different laboratories, and radiometric dates are only strictly comparable if they are on samples from the same run (V. Courtillot, pers. commun., 1997; see Courtillot, 1994 and Courtillot et al., 1996 for discussions). Therefore, it is very important to note that while the entire Western Ghats tholeiite sequence has been dated at $\sim$67.5 Ma by Duncan and Pyle (1988), substantially younger ^{40}Ar–^{39}Ar ages of $\sim$62.5 Ma for the upper part of the sequence have been obtained by Venkatesan et al. (1993), using the *same* monitor standard as used by Duncan and Pyle (1988).

7.3. No systematic age progression within the Deccan

It seems that the emplacement of the Kerala alkali gabbro dike (Fig. 3) at 80 Ma is actually linked to India–Madagascar breakup, ascribed to the Marion

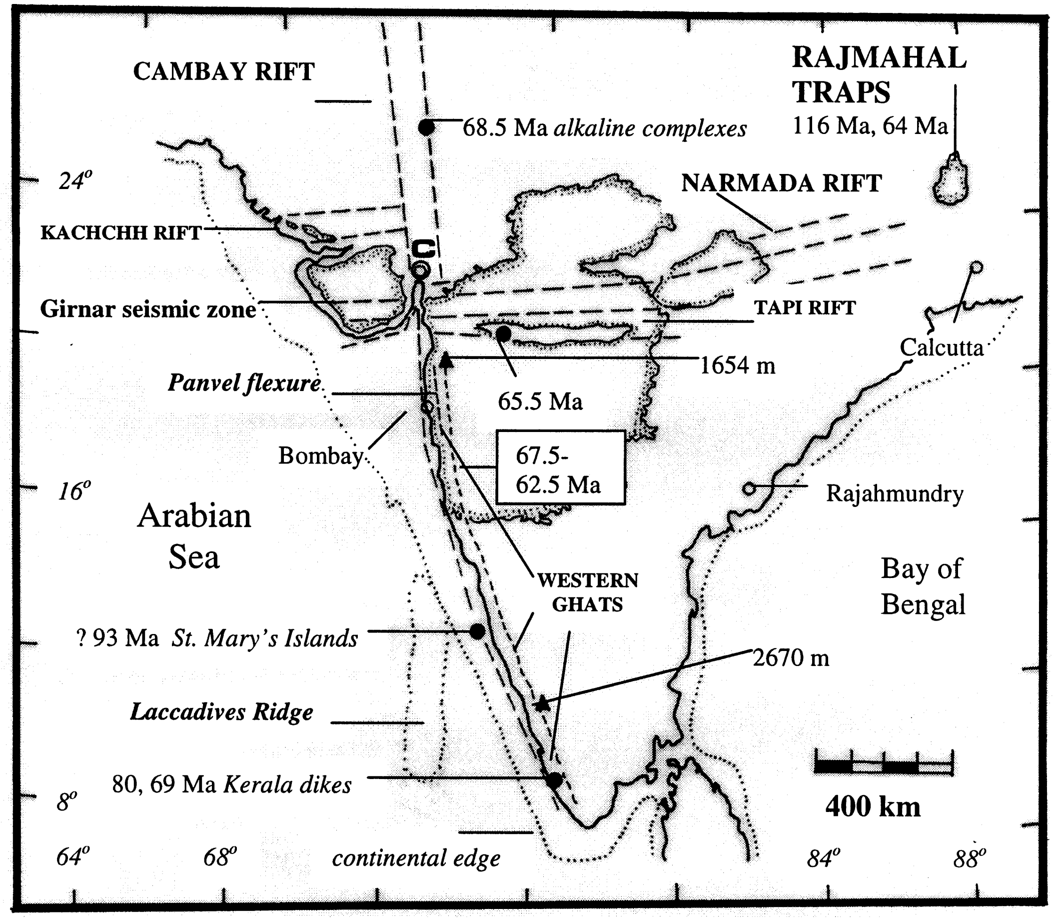

Fig. 3. Map of the Indian subcontinent showing the first-order tectonic-physiographic architecture of western and central India and the Deccan volcanic province. Modified from Mahoney (1988) and Sheth and Chandrasekharam (1997a). The Kachchh rift is from Biswas (1988). The area where the other rifts converge, marked *C*, is the Cambay region, which is not a triple junction, as seen. Note that the Laccadive–Maldive Ridge begins just at the latitude marking the southern periphery of the Deccan proper, but the Kerala dike swarms near the southern tip of India extend the original area of the Deccan substantially. The Western Ghats become progressively higher southwards, due to 'real' and 'apparent' uplifts combined (see text). The Panvel flexure is from Sheth (1998).

plume (the volcanism associated with which continued up to ~84 Ma, Storey et al., 1995, 1997), and before this India experienced an episode of felsic volcanism along its western coast, at ~13° N (93 Ma *K–Ar* age, Valsangkar et al., 1981; Joseph and Nambiar, 1996); these volcanics today constitute the St. Mary's Islands (Fig. 3). Basalts are lacking here but the volcanics comprise rhyolites, rhyodacites and granophyres, and lie 300 km south of the southernmost limit of the Deccan proper outcrop.

In any case, Radhakrishna et al. (1994) have correctly linked their 69 Ma dikes in Kerala to the Deccan event. Their ages appear to be reliable ^{40}Ar–^{39}Ar ages, but this 69 Ma event argues against models of northerly movement of India over a hotspot resulting in a southerly younging stratigraphy (e.g., Cox, 1983; Beane et al., 1986; Devey and Lightfoot, 1986; Subbarao, 1999). Or was it a plume head which had already arrived under India before 76 Ma and incubated and spread to a disc 2000 km across by 69 Ma? But what about the expression of the magmatic activity being confined exclusively to the western Indian coast? It suggests a *hotline*, implying ultimate control by the Indian continental margin.

Also, with the 69 Ma events both in the extreme north and the extreme south of India — early alkaline rocks of the northern part of the Cambay rift, and the dike swarms of the extreme southwestern part of peninsular India — note that the there is no systematic southerly younging within the Deccan province.

Besides, it must be appreciated that the Deccan eruptions covered a far, far greater area, even within continental India, than their present outcrops (cf. Mahoney, 1988; Devey and Stephens, 1991). Deccan-age dikes are found not only in Kerala in the extreme southwestern part of India (ages ~69 Ma), but also within the ~117 Ma Rajmahal Traps of northeastern India (ages ~64 Ma, Kent et al., 1997). Unquestionable Deccan lavas are found, besides their main outcrop in west-central India, in locations like Rajahmundry on the eastern Indian coast (Fig. 3) and Sind and Baluchistan in Pakistan (Wadia, 1975).

7.4. The Cambay 'triple junction': how real?

The Cambay region of the Deccan Traps (Fig. 3) is widely claimed to be a 'triple junction' (innumerable works, following Burke and Dewey, 1973). Burke and Dewey (1973) identified numerous such triple junctions globally, and recently they have been strongly advocated as former plume centres (e.g., Campbell and Griffiths, 1990; Ernst and Buchan, 1997). However, the Cambay 'triple junction' of the Deccan is not a triple junction actually. First of all, there seem to be four rifts forming a cross, namely the N–S-trending Cambay rift and its southern continuation, the West Coast rift belt, and the E–W-trending Narmada rift and its western continuation into the Saurashtra peninsula (the Girnar seismic zone, Chandra, 1977) (Fig. 3). Most works embracing the plume head idea for Deccan volcanism, including my own (Sheth and Chandrasekharam, 1996, 1997a,b) ignore two important facts: (1) the 'Narmada rift' is not just one rift, but actually a double-rift (Narmada in the north, Tapi in the south), with an upraised horst in between (the Satpura range). This additional complication is unlike anything predicted by the simplistic plume head models. (2) More serious, most of these works scarcely mention the Kachchh (Kutch) rift in the northwestern part of the Deccan province. The Kachchh rift is as important as the rest, being a major site of Jurassic–Lower Cretaceous (pre-Deccan) sedimentation along with the Narmada–Tapi rifts, and an extremely important focus zone for extensive Deccan-related volcanic and especially plutonic activity (e.g., Wadia, 1975; Biswas, 1987, 1988; Sheth, 1999), but it has been disregarded by almost all workers claiming the existence of a triple junction. To summarize, the Cambay region is certainly not a triple junction. Rather, the overall distribution pattern of the Deccan rifts seems to follow a regular orthogonal pattern (N–S and E–W; see below), which is unlike anything that a plume head can be imagined to produce.

7.5. Control of Precambrian tectonic trends on the Deccan rifts

It is undisputable that the Deccan rift zones follow the structural trends of the Indian Precambrian lithosphere. The Narmada zone has been a major zone of weakness since Archaean times (Naqvi et al., 1974), and the Cambay graben and the western Indian coast also developed by faulting parallel to the Precambrian Dharwar trend (Raju, 1968; Chandrasekharam, 1985; Biswas, 1987). These rifts were sites of voluminous and protracted sedimentation just before and well before Deccan volcanism (e.g., Biswas, 1987; Sheth, 1999). Kaila (1988) has reported the existence of a hidden Mesozoic basin in the Narmada–Tapi region *below* the Deccan lavas, identified from deep seismic soundings. The northern Narmada graben has a thickness of 1000 m of these sediments, while in the southern Tapi graben the sediments are 1800 m thick. There is complete agreement that the expression of a plume head ultimately is dependent on the pre-existing lithospheric breaks and rifts (e.g., Hill, 1991; Thompson and Gibson, 1991), and if there is absolutely no other evidence for a plume head or a tail as argued above and below, can we not do away with a plume altogether? What is the unquestionable, compelling evidence for a mantle plume in Deccan volcanism?

7.6. Results of seismic tomography

Seismic tomographic evidence (Anderson et al., 1992a) rules out a plume head beneath India (or beneath any other flood basalt province). A hot plume head emplaced beneath the lithosphere cools conductively to 200 km depth in 200 million years, and in the light of the relatively young age of the Deccan episode one should find slow seismic velocities beneath India. However, seismic tomography indicates high velocities under peninsular India proper (the main Deccan region), indicating a thick, cold lithosphere, and a cold asthenosphere (110–200 km depth) (Anderson et al., 1992a). For the southern Indian peninsular shield, the lower crustal shear-wave velocities are higher than those for the Baltic, African and Canadian shields, but the upper mantle velocities are lower, and compared to the southern Indian shield, the Deccan region exhibits marginally *higher* S-wave velocity both in the lower crust and upper mantle (Mohan et al., 1997). P-wave analyses have given similar results (Iyer et al., 1989). Low heat flow is associated with the Deccan Trap terrane, except for the rift zones (Gupta and Gaur, 1984; Ravishankar, 1988; Mahadevan, 1994). Recent identification of a low seismic wavespeed anomaly beneath the Cambay rift region (Kennett and Widiyantoro, 1999) is an important contribution to Deccan studies, but the authors' suggestion that it may represent the Deccan plume head is model-dependent. This means that, far from this low-velocity anomaly being a 'proof' of the plume head model, the authors have, in the framework of that model, suggested that the anomaly could be the plume head. The anomaly may well have had an upper mantle origin. A low-velocity upper mantle cushion beneath the Cambay rift has been described and discussed before, by Mahadevan (1994).

Likewise, although VanDecar et al. (1995) have identified what they conceive to be a fossil plume head beneath the Parana CFB, from seismology, unless it is shown that this came from the core–mantle boundary one cannot deny that this structure may well have had a shallow, upper mantle origin (a few hundred kilometres). The same argument applies to the results of Wolfe et al. (1997) who have seismically identified a low-viscosity upwelling zone, 300 km wide, and extending up to 400 km depth, beneath Iceland. Keller et al. (1997) have interpreted this as a shallow, non-plume upwelling, and Hamilton (1998) has strongly criticized the methods of Wolfe et al. (1997). Seismic tomography does not show any evidence for an unusually hot mantle beneath Jan Mayen too (Zhang and Tanimoto, 1993; see Haase et al., 1996). In fact, the North Atlantic region around the Kolbeinsey Ridge–Iceland–Reykjanes Ridge appears a classic area for encountering contradictions with the various premises of the plume model (see Anderson, 1996).

7.7. Topographic uplift and doming

Dome-flank radial drainage patterns, such as those identified by Cox (1989) for the Deccan and other flood basalt provinces, are based on topographic uplift which is merely consistent with what is expected from a plume head. Cox (1988a) has actually noted that the very striking uplift undergone by the Western Ghats is *post*-volcanic, and Radhakrishna (1965) has in fact argued that the easterly drainage of the Indian peninsula is much older and not a consequence of post-Deccan uplift. In fact, neither is the topographic uplift of the lithosphere before flood volcanism a rule. The already discussed case of the Siberian Traps, the largest CFB on Earth, is a glaring anomaly in the plume-caused thermal uplift idea. Similarly, the emplacement of the largest oceanic plateau, the Ontong Java plateau in the southwestern Pacific, which must be due to a plume head (by convention), was accompanied by little lithospheric uplift and no subaerial emergence, despite the addition of as much as 36 km of new crust (Neal et al., 1997). Its cause just cannot have been a continental-size plume head.

To explain the strongly bimodal age distribution of the basalts forming the Ontong Java plateau ($\sim$122 Ma and $\sim$90 Ma), Bercovici and Mahoney (1994) put forth a double-plume-head model. In this, a plume head detaches itself from its conduit on entering the upper mantle from the lower mantle, and generates the first phase of surface volcanism. The trailing conduit of the plume develops a second plume head, on crossing the 660 km discontinuity, which then rises up to generate the second volcanic phase. However, this explanation runs into several problems. First, it does not explain why *all* flood basalt provinces do not show bimodal age distributions (only a couple do), and therefore has an element of special pleading. Second, it also creates a conceptual problem: one may ask why no flood basalts show *trimodal* age distributions (if two plume heads are possible, three are also possible, logically). The authors note this. Third, as pointed out by Ernst and Buchan (1997), the multiple-plume head explanations do not take into account motion of the overlying plate throughout the intervening period, i.e., the second plume head will arrive at the same location as the first only if the plate had not moved significantly in the time period between the two plume impacts. Fourth, Bercovici and Mahoney (1994) have themselves noted that although the endothermic phase transition at the 660 km discontinuity was ignored by them for the sake of simplicity, actually the effect of this phase transition would be to prevent the penetration of the second smaller plume head into the upper mantle.

7.8. Quality of radiometric age data

India has experienced essentially continuous magmatism–volcanism–tectonism from 65 Ma, back through 68–69 Ma, 76 Ma, 80 Ma, 88 Ma, 93 Ma (doubtful date, see below), and 116 Ma (Rajmahal Trap volcanism; Baksi, 1995; Kent et al., 1997). Note that most of these are high-quality $^{40}Ar–^{39}Ar$ ages and not K–Ar ages and so inferences may safely be drawn from them. The 93 Ma age for the St. Mary's island volcanics (Valsangkar et al., 1981) is a K–Ar age. In light of the Radhakrishna et al. dikes of Deccan age, there is a definite possibility that these volcanics are also of Deccan age. In fact, they were thought to be of Deccan age originally, but were later dismissed by Valsangkar et al. (1981) as unrelated to the Deccan, based on their K–Ar age data. The K–Ar ages of 93 Ma for them could be overestimates, because of possible inherited argon through contamination by Indian Precambrian basement crust, which effect the conventional K–Ar dating technique cannot identify. Therefore, accurate dating of these volcanics by the $^{40}Ar–^{39}Ar$ method is perhaps the #1 pressing problem in Deccan research today, and work in this direction has already begun in our Ar–Ar dating lab at the Physical Research Laboratory (K. Pande and H.C. Sheth, work in progress).

Some authors have not considered the quality of age data existing in the literature and confused the literature substantially, especially for nonspecialists, with false interpretations (see Mahoney, 1988 for a good discussion of the Deccan case). Baksi (1990) is correct in concluding that "subjective and, in many cases, incorrect use of radiometric data has become endemic in the earth science literature". Kent et al. (1992), for example, used several K–Ar ages for LIPs of the world to support their concept of plume incubation. Meyerhoff (1995) wrote that "the literature existing on the Deccan Traps, especially the speculative papers, is enormous". This is correct, but he also cited the dates of 180 Ma–40 Ma by Alexander (1981) and one age as low as 4 Ma for the Deccan Traps, to argue for a very long duration of volcanism. All ages of Alexander (1981) are K–Ar ages and are almost all doubtful, because of the serious possibilities of substantial argon gain or loss (V. Courtillot, pers. commun., 1996; Baksi, 1987; Mahoney, 1988). The question of the true duration of Deccan volcanism is easily fixed (in a however approximate way), because field evidence clearly shows that interflow sedimentary beds are absent throughout most of the province, and successive lava flows in the Deccan rarely show weathering profiles between them, an unlikely situation if the whole Deccan event were spread out between 180 Ma and 4 Ma, as noted by West (1981). Lastly, Pandey and Negi (1987) compiled numerous ages published in the Deccan literature (137–30 Ma), most of which came from Alexander (1981); recorded the locations of these dated flows, and noted that since the ages did not show any systematic southerly younging they did not support a Réunion hotspot origin for the Deccan, and ascribed the origin of the Deccan and other CFBs to galactic disturbances.

8. Picrites: no evidence for anomalously hot mantle

8.1. Picrites from plume axes?

Campbell and Griffiths (1990), proposing the plume head and tail model, ascribed flood basalts to a relatively cool plume head (a mechanical mixture of primitive plume source material and entrained MORB mantle) and the associated picritic melts to the hotter primitive plume source material in the axis (which supposedly does not entrain surrounding mantle), discussing case studies such as the Deccan. However, Campbell and Griffiths (1991) argued that because of mantle convection plume conduits are likely to become tilted from the vertical and to entrain surrounding cooler mantle, and therefore they may not produce picrites.

Actually, the Deccan picrites are no evidence for anomalously hot mantle. The Deccan picrites of the Western Ghats region are known to be not primitive liquids, but cumulates, and the maximum MgO contents of their parental liquids were 9–10% (Beane et al., 1986, 1988). The Deccan part of the Réunion hotspot track has been drawn based on some presumed primitive (liquid) picrites in the Cambay region, but rather than reflecting high-degree melting of the hot plume axis as postulated by Campbell and Griffiths (1990), they have been argued to reflect *low*-degree, high-pressure melting of Ambenali-type and Réunion-type mantle (Peng and Mahoney, 1995), as they are somewhat alkaline and plot in Nd–Sr–Pb isotopic arrays between these two and continental lithospheric endmembers (see Section 13 for a detailed discussion).

8.2. Picrites from depleted mantle sources?

It has been noted that picrites are rarely erupted in flood basalt provinces, and this has been ascribed to their high density (e.g., Stolper and Walker, 1980). The erupted flood basalts may therefore well be the fractionated daughters of picritic parents (e.g., Cox, 1980), and the fact that picrites are not erupted also does not vitiate the picritic parental magma hypothesis as Mahoney (1988) points out. But the point to be noted is that, from the chemical-isotopic evidence, picrites themselves may well be derived from ordinary MORB mantle at the appropriate depths, and picrites of all flood basalts need not have come from plume axes (see also Anderson, 1994a,b; Anderson et al., 1992a). If melting began at considerable depths (>25 kbar) where phase relationships required olivine as the first phase to be consumed, the resultant melts, even if low-degree, would be highly magnesian picritic melts and at the same time rich in incompatible elements (Arndt et al., 1993). Extensive decompression of uprising mantle (active or passive), conceivably aided by lithospheric rifting, would also result in shallow-level melting, which would produce picritic melts (as the effect of decreasing pressure would be compensated by increasing degrees of melting), although these high-degree picrites would not possess any appreciable enrichment in the incompatible elements (Arndt et al., 1993). In both these cases, therefore, an anomalously hot mantle (which is usually interpreted as a mantle plume) is certainly not a requirement. Indeed, for satisfying any real thermal requirements (e.g., picrites of Skye, Scarrow and Cox, 1995), *hotcells* (Anderson, 1994a, 1998a; Anderson et al., 1992a,b) would serve quite well (K.G. Cox, pers. commun., 1998).

8.3. Picrites from plume peripheries?

Picrites of the West Greenland CFB have been thought to reflect melting at the *periphery* of the proto-Iceland plume, not its axis (Gill et al., 1992; Holm et al., 1993). These workers noted the eruption of large volumes of picrites in West Greenland at a considerable distance (850 km) from the expected plume axis site, and the subordinate proportion of picrites erupted directly above that plume axis in East Greenland. This led them to consider explanations like a shift in the relative position of the plume, two separate plumes, or a non-axisymmetric plume head. The last proposal (e.g., Sleep et al., 1988; White, 1992) comes as the ultimate saviour of any speculative plume model, but Gill et al. correctly noted that "an irregularly-shaped starting plume with an elongated head, initially culminating in a hot diapir beneath West Greenland, and a tabular 'tail' extending under central Greenland could account for voluminous Tertiary volcanism in West Greenland as well as East Greenland, but this involves the same degree of arbitrary special pleading as the proposal of a separate Davis Strait plume". Even tabular plumes have been conveniently proposed (Sleep, 1992), but as aptly put by Hamilton (1998), "the voluminous literature on hypothetical plumes is notable for its ingenuity in the near-total absence of constraints".

8.4. Stirring or mixing?

One more contradiction in the plume head and tail model is as follows. Campbell and Griffiths (1990) and Griffiths and Campbell (1990) show from their fluid dynamical experiments that the entrainment of depleted MORB mantle by a rising diapir results in zoning within the plume head, i.e., the MORB mantle becomes stirred into the plume, producing a concentric layering of primitive plume source material and depleted MORB mantle. So far so good. However, note that the MORB mantle gets stirred into the plume, not mixed. *Stirring* is the intermingling of fluids that still remain distinct, while *mixing* is the merging of distinct fluid types into an intermediate type (Davies and Richards, 1992). Now, if the head contains primitive plume source material and depleted MORB mantle stirred (not mixed) into it, and the former is hot and the latter cooler (an axiom of the model), one should find picrites all over the plume head, not confined to merely its axis as the model postulates. And in this case, the very need and justification for a 'plume tail' vanish. Plume tails were needed to explain the picrites, but if picrites should be forming all over the plume, what is the evidence for a tail? (And, where to place the tail within any LIP outcrop?) Thinking on the same lines, if the flood basalts are produced by the depleted MORB mantle, stirred into the plume head (and, by implication, not changed in temperature or composition), why do we need a zoned plume head at all (using Ockham's Razor)? Without a zoned plume head, these same flood basalts would still be produced from sublithospheric, non-plume, MORB mantle (Anderson, 1994a,b; King and Anderson, 1995).

8.5. Basic fallacy of the purported picrite–plume connection

The whole picrite paradox outlined above is an unnecessary and model-imposed one. It is, first of all, heavily dependent on the wrong contention that all picritic magmas are high-degree melts. This incorrect assumption has been followed up with another, namely that for high-degree melts high temperatures are needed. Next, these high temperatures are thought to prevail in a hypothesized structure, namely a plume tail, and in turn, this is cited as evidence for the lack of entrainment within a plume stem. Still further, these tails are therefore thought to represent the plume source material at the D″ region. The fact, however, is that "picritic magmas represent melts formed at relatively high pressures and temperatures, but they do not necessarily represent high degrees of melting" (Peng and Mahoney, 1995). The Nuanetsi picrites of the Karoo CFB were interpreted as low-degree melts by Cox et al. (1984), and the experimental work of Herzberg (1992) has shown that picritic liquids are the initial melts formed in the 25 kbar range under anhydrous conditions. The picrites found in flood basalt provinces are not all the same: some are true liquid picrites, some are cumulates, and even the true liquid picrites are not always high-degree or high-temperature melts. And when they are, genetic connections with plume axes are obscure at best.

8.6. Archaean komatiites: indicators of Archaean plumes?

The 'successful application' (as claimed by some) of the plume head-plume tail model to Archaean volcanism also appears unfounded. Komatiitic volcanism is almost exclusively confined to the Archaean Eon and indicates very high potential temperatures for the Archaean mantle (Arndt and Nisbet, 1982; Arndt et al., 1998), and the implied high geothermal gradients are not reconcilable with the geothermal gradients apparent from metamorphic mineral assemblages (Bickle, 1978). To reconcile these two conflicting observations Campbell et al. (1989) proposed that Archaean komatiites (the ancient equivalents of modern picrites in their opinion) formed in plume axes and associated basalts in plume heads. This explanation is first of all heavily dependent on their incorrect contention that modern picrites associated with flood basalts come from plume axes. Secondly, Archaean komatiites are geochemically and isotopically MORB-like (Anderson, 1994b). Even the Middle Cretaceous Gorgona komatiites of Colombia, the only komatiites known from the entire global Phanerozoic rock record, are LREE-depleted, and are like Pacific normal-MORB in terms of Nd–Sr–Pb isotopes (Storey et al., 1991). Worst of all, the whole proposal of Campbell et al. (1989) is impossible to reconcile with the observation of Sleep (1992) that the mantle may have cooled faster than the core, and therefore in very early Earth history the temperature contrast between the outer core and the lowermost mantle may have been far less than today, preventing the very development of plumes. There vanishes all the thermal evidence for plume involvement in LIP formation.

9. Where are the ancient and modern plume heads?

Hawaii is thought to be the best, classic example of an intraplate hotspot, and the entire Emperor Seamount–Hawaiian chain is ascribed to the tail of the Hawaiian 'plume', but the glaring problem of the huge flood basalt province that must have been generated by the *head* of the Hawaiian plume is rarely discussed anywhere. For that matter, few if any island chains have an associated LIP (Anderson, 1998a,b, 1999). Modern Hawaiian volcanoes such as Mauna Loa add a 10 km thickness of basaltic crust on top of the Pacific plate, and if they are the products of the Hawaiian 'plume tail', it is not unreasonable to assume that the initial LIP generated by the Hawaiian plume head would have had a minimum thickness of 30 km (and this thickness is reasonable, if the Ontong Java plateau is 36 km thick; Neal et al., 1997). The Hawaii-related LIP is thought to have been presumably subducted along Kamchatka (e.g., Coffin and Head, 1997), or along the Aleutian or Kurile trench (Courtillot et al., 1999), but Cloos (1993) and Saunders et al. (1996) have shown that large oceanic plateaus cannot be subducted. Such thick plateaus resist subduction, jam the trench and accrete to the arc. Only their lower parts may get subducted if they are eclogitized, but in turn the effect of a low-density melt-depleted root would be to increase the buoyancy (see Neal et al., 1997). Condie (1997) goes even further and suggests that these oceanic plateaus evolve into lower continental crust. So where is this initial product of the Hawaiian plume?

Another glaring anomaly in the plume head and tail model is that, out of all modern (presently active) hotspots that have been cited in the literature (120, Burke and Wilson, 1976; 42, Crough and Jurdy, 1980), not a single one can be classified as due to a plume head. This is surprising. Statistically, should at least a couple of these tens of 'plumes' not show a starting plume head stage? "Paradoxes can be viewed as a bother, or as a rich source of information" (Anderson, 1999).

10. Alternatives

"The introduction of black boxes, physics and mathematics into geology has not eliminated the need for keen observation and coherent logic, and more than one idea" (Anderson, 1999). Numerous alternatives to plumes exist, but have simply gone out of fashion or never been given due attention. They seem even more worthy of attention now as the plume hypothesis seems to have led Earth scientists along a blind alley. Importantly, the non-plume interpretations are not all mutually exclusive, and each has numerous good points, only on combining which we could arrive at a complete understanding of all intraplate geodynamics and volcanism.

A credible alternative idea, an old one, is that of propagating rifts (Turcotte and Oxburgh, 1973; Turcotte, 1974; Jackson and Shaw, 1975), in which linear volcanic chains are thought to delineate the *stress* field, not the displacement field, of the lithosphere. Thus volcanic chains may be piezometers, not speedometers (Anderson, 1998b, 1999). Leaky transform faults are another quite plausible explanation (e.g., Smoot, 1997; Anderson, 1998a,b). Still another idea, from the 'surge tectonics' hypothesis (Meyerhoff et al., 1992; Meyerhoff, 1995) is that such linear chains are produced by 'magma surge channels' existing in the lithosphere and connected to the asthenosphere. The surging magma, and therefore the consequent island chains, have a preferred easterly or equatorward age progression, due to Earth's (eastward) rotation. The sphericity and rotation of the Earth should be integral components of any realistic, sensible and complete Earth dynamics hypothesis (e.g., see also 'global wrench tectonics' of Storetvedt, 1990, 1992, 1997; Smith and Lewis, 1999a). In surge tectonics, Benioff zones constitute barriers to eastward magma flow, and so the only places it can move out eastward are the *cusps* between island arcs, as best seen in the Western Pacific, e.g., in the Hawaii–Emperor Seamount chain, the Mid-Pacific mountains, the Carolines chain, and the Louisville Ridge. This observation is a pertinent one, and it is of interest, in the context of the surge idea, that it is the *eastern* salient of the Ontong Java plateau (recall Section 7.7) which is apparently made up of substantially younger basalts (90 Ma vs. 122 Ma). However, in the absence of substantially more coverage of the Ontong Java plateau than available at the moment, it is best not forced into conformity with any model, plume or non-plume, prematurely.

All except a fanatic of this or that model would agree that the method of multiple working hypotheses is strongly needed at the present stage of our knowledge — when numerous hypotheses are available, why confine ourselves to just one and remain blind to its faults? 'Hotcells' and/or melting of the 'perisphere' (Anderson, 1994a; Anderson et al., 1992a) are ideas worth consideration. Smith (1993) argues that mantle metasomatism is prevalent at ancient sutures and rifts and lithospheric discontinuities, that such regions would be most prone to melting, and that hotspot tracks are the result of oblique rifting of such ancient weaknesses. Continued metasomatism and volatile fluxing of such lithospheric breaks over millions of years results in a shallow enriched mantle which is sampled by the volcanism (see e.g., Bailey, 1982; Sheth, 1999). Such a model (of which the perisphere model is a variant) is especially appealing in cases where the only evidence cited for a mantle plume is the enriched chemical-isotopic composition of the erupted basalts, and deserves a detailed discussion.

11. A perisphere layer in the Indian Ocean mantle?

The Kerguelen 'plume' is now thought to have provided heat, not material, to Rajmahal Trap volcanism (Mahoney et al., 1983; Kent et al., 1997). The enriched character (such as higher-than-normal-MORB $^{87}Sr/^{86}Sr$ ratios) of the Indian Ocean basalts (hotspot basalts as well as Indian MORB) has long been recognized (e.g., Subbarao and Hedge, 1973; Mahoney et al., 1998), and this is often cited to be due to numerous enriched plumes (e.g., Weis et al., 1992). But if we do away with the widespread notion that the upper mantle is depleted and the lower mantle is enriched, and accept, based on various compelling grounds, that the enriched layer must be a shallow-level layer and the MORB source deeper, no plumes are necessary [see the detailed works by Anderson (1985, 1994a, 1999) and Anderson et al. (1992a,b)]. These workers have repeatedly argued that it is incorrect to conceive of the asthenosphere (which is a geophysical–tectonic–rheological–mechanical concept) as 'depleted mantle', or MORB source, the shallow mantle as 'homogeneous' and 'barren', and the deep mantle (the supposed source of plumes) as 'enriched'. Rather, the shallow mantle may be 'enriched' mantle and be distributed all around the Earth (the 'peri'sphere). Such a perisphere layer would be a natural outcome of upward migration of and metasomatism by volatiles and fluids, carrying incompatible elements, which would concentrate in the upper layers (200–400 km depth) and give rise to enriched chemical signatures that are being ascribed to plumes. The perisphere also derives incompatible elements from dehydrating, downgoing slabs. McKenzie (1989) has also suggested the possibility of such a shallow metasomatic layer not only beneath the continents but also partly beneath old ocean floors.

The whole of the Indian Ocean mantle may possess a shallow, enriched perisphere. A pertinent observation in this regard is that of Mahoney et al. (1989), who however did not interpret it in this way: "The rather strong isotopic resemblance of several other Indian oceanic islands, such as Amsterdam and St. Paul, to the Mascarene Islands (e.g., White and Duprè, 1984), suggests that broadly Réunion-like OIB mantle is widely present beneath the Indian Ocean" (see also Mahoney et al., 1998). The Indian Ocean mantle is also hotter than normal, from seismic tomographic evidence, as is the Pacific Ocean upper mantle, which in fact even has widespread partial melt (Anderson et al., 1992a).

12. Mantle plumes from ancient recycled oceanic crust?

The perisphere model is consistent with many observations, satisfies many requirements and does not introduce new paradoxes or invoke assumptions built on assumptions. As an example of the latter, it has been argued (e.g., Hofmann and White, 1982) that subducted oceanic crust goes down to the core–mantle boundary and then erupts back as plumes with enriched signatures. Corderey et al. (1997), who propose a significant eclogite component in plume heads, base their work on the same premise. This explanation contradicts simple requirements of the same subduction process that as the descending slab would melt the incompatible elements should be mobilized and leave the slab. Since they would not remain in the slab, the slab could not develop the observed isotopic heterogeneities, as this in turn requires storage and aging for billions of years, inconsistent with mantle overturn times of a few hundred million years (Davies, 1990). Yet, all these interpretations have been accepted and entrenched in the literature, in order to satisfy the wrong assumption that OIBs are derived from plumes, which in turn come from the core–mantle boundary. In the perisphere model, OIBs and their chemical characteristics can be easily explained with shallow-level derivation, without any need for exotic deep mantle sources: the peridotite layer enriched in incompatible elements is replenished continuously, is near the top, and stays near the top for long periods of time due to its buoyancy.

Another reason why plumes had to be invoked from recycled oceanic crust (Hofmann and White, 1982) was the relatively high Ti–Nb and other such elemental abundances in many OIBs. The explanation was that the high field strength elements such as Nb do not get mobilized but are retained by minerals like rutile in the subducting slab which transforms to eclogite (e.g., Saunders et al., 1992; McDonough, 1991), and this slab, reaching the Earth's surface back as a plume, shows the same Nb enrichment in the erupted products. However, this explanation now becomes unnecessary and invalid as we now know that Nb does leave subducting slabs during melting, after all.

Ionov and Hofmann (1995), in a study of mantle xenoliths from central Asia, show that amphibole is an important, possibly the most important, host mineral for Nb and Ta in mantle xenoliths in alkali basalts, and probably in the shallow peridotite upper mantle in general. Rutile and ilmenite may have high Nb and Ta concentrations as well, but they do not occur in common mantle rocks (peridotites and pyroxenites), being found almost exclusively in eclogites and Ti-rich xenoliths in kimberlites. Amphibole has higher Nb and Ta (and also Ti) contents than coexisting mica. Phlogopite can also be an important host mineral for Nb and Ta in K-enriched mantle rocks. According to Ionov and Hofmann (1995), fluids and melts contributed by subducting slabs rise through the mantle wedge and open-system crystallization of amphibole and phlogopite occurs. The retention of Nb–Ta by these minerals explains the well-known characteristic depletion in Nb–Ta of island arc volcanics.

Indeed, Ionov and Hofmann's results seem to favour a perisphere model. The high Nb abundances of OIBs could be not only due to low degrees of melting, and not due to retention of Nb in a subducted slab now being melted, but due to an enriched, metasomatized source (the perisphere). The specific phase holding Nb in the perisphere, which lies beneath the lithosphere, would depend on depth. Stability considerations show that phlogopite is stable up to 250 km depth, while amphibole breaks down at pressures of 25 kbar (Olafsson and Eggler, 1983; Hawkesworth et al., 1990). This idea gets a strong confirmation from a recent work by Class and Goldstein (1997), who have found evidence for phlogopite and amphibole in the mantle sources of Hawaiian pre-shield volcanoes and the Comores hotspot volcanics. Many so-called 'hotspots' may actually be 'wetspots' (Smith and Lewis, 1999b).

Again, as is too well known many Hawaiian pre-shield lavas have high $^3He/^4He$ ratios, which is almost unanimously taken as an evidence for a primordial, deep mantle, or mantle plume, origin (e.g., Lassiter and DePaolo, 1997). But $^3He/^4He$ ratios do not uniquely indicate high ^3He (which would suggest an undegassed source conceivably in the deep mantle). They might well imply low ^4He (see Anderson, 1998b,c, 1999). Phlogopite has very low U and Th contents (Hawkesworth et al., 1990), and thereby it would generate little ^4He integrated over time (^4He nuclei are the well-known alpha particles), which would keep the $^3He/^4He$ ratios high in that mantle region.

Therefore, OIBs need not come from mantle plumes which in turn arrive from the deep mantle, that too as ancient subducted slabs. OIBs may instead derive from a shallow enriched layer, the perisphere, which probably has widespread phlogopite and/or amphibole (in a peridotite matrix). Contrary to all popular conceptions, it is this enriched layer which is shallow, and the MORB reservoir which is deeper. The perisphere must be removed by melting before the underlying depleted reservoir would begin to be tapped (Anderson, 1994a), and this observation is beautifully consistent with factual evidence from ocean islands, innumerable continental rifts of the world, and new ocean ridges which, before they erupt MORB-like melts, erupt lavas of numerous enriched varieties (see also Bailey, 1983; Sheth, 1999). This simple observation further emphasizes the need to incorporate the *time* factor in geodynamics thought: mere processes are not enough.

13. Back to the Deccan

13.1. A new interpretation of the parental magmas of the Deccan

Since the plume model for flood basalts and LIPs in general and the Deccan basalts in particular, seems rather ill-founded, artificial and invalid as seen above, one can get rid of the constraining contention that the Deccan–Réunion 'hotspot track' is a plume track. In fact, the hotspot tracks in the Indian Ocean are along or parallel to fracture zones. The Laccadives–Maldives–Chagos part of the track is along the Vishnu fracture zone, and the remaining part up to Réunion Island is along the Mauritius fracture zone (Fig. 2; Norton and Sclater, 1979). Therefore, I suggest, based on the published radiometric ages (e.g., Duncan and Pyle, 1988), that the Laccadives–Réunion Island chain has developed along a southerly propagating fracture tapping a shallow, metasomatized, enriched perisphere, and an underlying depleted mantle source region.

It is important to remember that 'fertile' and 'enriched' are not synonymous (see e.g., Anderson, 1989, 1995 for definitions). A 'fertile' source is one capable of producing basalt on melting, while 'enriched' often implies metasomatized and usually has higher-than-MORB $^{87}Sr/^{86}Sr$ ratios, for example. It may or may not be fertile. An 'infertile' (syn. 'barren') source is one deficient in the basalt-forming major elements (Ca, Al, Ti, Na). It should not be confused with 'depleted', which strictly means deficient in the incompatible trace elements. Thus, although the MORB source is 'depleted' (in Rb, U, Th, K and other incompatible elements and their isotopic ratios), it is nevertheless 'fertile' (i.e., it does produce MORB on melting).

Along with the above suggestion, namely that the Laccadives–Réunion Island volcanic chain has been caused by a southerly propagating fracture, I propose a different interpretation of the widespread Ambenali magma type of the Deccan Trap lavas (the apparent parent of the remaining chemical-isotopic types which all show continental lithospheric contamination to various degrees). The close chemical-isotopic similarities of Ambenali lavas to MORB have been noted and discussed by various workers (Chandrasekharam and Parthasarathy, 1978; Macdougall, 1986; Mahoney, 1988; Shrivastava and Pattanayak, 1995). Many Ambenali lavas show little evidence for crustal contamination, and their Nb peaks in mantle-normalized element patterns and other elemental considerations enable classification of the Ambenali magma type as transitional-MORB, which can be closely approximated by mixtures of Réunion hotspot and Central Indian MORB material (Mahoney, 1988). The enrichment in incompatible elements could have resulted from slight amounts of contamination by Indian continental mantle (Macdougall, 1986).

Having come this far, it is important to note that, technically, the 'Réunion component' means merely the composition seen at Réunion Island; it has no *geodynamic* connotation by itself. It is only in the plume head model (wherein the head is a mechanically stirred mixture of primitive plume source material and entrained MORB material, e.g., Campbell and Griffiths, 1990; Griffiths and Campbell, 1990; Duncan and Richards, 1991), that the Ambenali magma type has been depicted as the Réunion plume head composition (e.g., Peng and Mahoney, 1995). Assuming no role for the Réunion hotspot in Deccan volcanism, and no Réunion mantle plume, I reinterpret the transitional-MORB chemical character of the primitive Ambenali magma type as the product of slight amounts of contamination of Indian MORB mantle magmas by the Indian continental mantle (following Macdougall, 1986), or by a shallow, enriched, niobian phlogopite-bearing, perisphere, which itself defines the 'Réunion component'. (Both these contaminants have essentially the same 'enriched' chemical characteristics and would have the same effect.) Thus, the Indian lithospheric mantle can be expected to have phlogopite and amphibole, which both contain Nb. Phlogopite has actually been argued to be the most characteristic mineral of the continental lithospheric mantle and shallow-level enrichment processes (Lightfoot and Hawkesworth, 1988; Hawkesworth et al., 1990), and an actual case study of the role of phlogopite in the genesis of alkali basalts from the Narmada region of the Deccan also stands published (Mahoney et al., 1985).

Peng and Mahoney (1995) found intercalated picritic and basaltic lavas in three boreholes in the Cambay region of the Deccan Traps (Fig. 3). Their important finding was that the picrites and the basalts both formed mixing arrays between the Ambenali magma type and Indian continental lithosphere, and the Réunion composition and Indian continental lithosphere. The picrites were not confined to the Réunion–continental lithosphere mixing array, i.e., not generated exclusively from the presumed plume axis. However, in the framework of the prevalent plume head model, Peng and Mahoney (1995) interpreted these intercalated (and therefore simultaneously erupted) picritic and basaltic flows as representing the melting of a zoned plume head. They also noted that, along the Deccan–Réunion track, the proportion of the Réunion endmember has increased progressively through time (White et al., 1990), such that the lavas presently erupting at Réunion Island show the strongest enriched signature. Although they did note that decreasing degrees of partial melting would lead to this result, they interpreted this in terms of the evidence *required by* the plume model, namely that the plume head stage has been over and only the tail of the Réunion plume has remained which should (as again argued by the plume model), undergo little entrainment of the surrounding mantle and thus exhibit uncontaminated, ‘plume source’ signatures.

Unlike Peng and Mahoney (1995) and White et al. (1990), I propose that the increasing contribution of the Réunion endmember along the track has little to do with a plume head or tail; rather it is simply due to a progressively decreasing degree of partial melting southwards [see Ellam (1992) and Lassiter and DePaolo (1997) for a geochemical corroboration of this idea], the melts being derived from an upper enriched perisphere underlain by a depleted mantle source. My interpretation is easily tested below.

13.2. A big anomaly in the plume head–tail model

Peng and Mahoney (1995) observed that the source regions of the present Réunion island lavas nevertheless appear to contain some amount of (presumed) ‘entrained’ depleted MORB material. This fact, following Campbell and Griffiths (1991), would be taken as strong evidence that the Réunion plume stem had been tilted due to the northerly drag of the Indian plate, and is therefore still undergoing some entrainment of Indian MORB mantle. In this case one would logically expect that the plume stem would become more and more tilted with time due to continuous drag, resulting in more and more entrainment of surrounding mantle. This should lead to continuously *increasing* contribution of the MORB source along the ‘tail’ part of the hotspot track, which is just the opposite of what is observed. What we actually find is that the MORB source endmember has had a continuously *decreasing* contribution. The expected trend of increasing MORB contribution is unaffected by the fact that the older part of the track is on the Indian plate and the younger part on the African plate. Few predictions and requirements of the mantle plume model seem to be fulfilled in the actual geology.

The obvious correct interpretation of the critical observation noted above, a big anomaly in the plume head–tail model, is a progressively decreasing degree of melting of an enriched source in a southerly direction, beneath a southerly propagating fracture. That the Réunion Island alkali basalt source is enriched in the light rare earths has been argued by Albarède and Tamagnan (1988). That the degree of melting (and the eruption rate) are indeed decreasing southwards is indicated by the change in morphology of the hotspot track, from an almost continuous ridge for the early activity to discrete volcanoes over the last 10 million years (Weis et al., 1992; see also Fisk et al., 1989), and the fact that the current active volcano on Réunion Island, Piton da la Fournaise, erupts more alkaline lavas than the dormant, older one, Piton des Neiges (Fisk et al., 1988). Fisk et al. (1988) caution that this phenomenon is linked to the degree of melting only in a general way, and the alkali enrichment may be due to interaction of the new magmas with low-melting components of the earlier erupted lava pile. However, speaking of the whole island chain, the degree of partial melting appears to be a major factor controlling compositional variations, and the minor influence of the MORB endmember seen in present-day Réunion lavas would disappear after a few million years when the degree of melting decreased still further.

13.3. Palaeolatitudes

If measured palaeolatitudes along a hotspot track, which is created by a plume anchored in the deep mantle, are not constant, it is thought by some that the mantle reference frame itself must be moving (‘rolling’) relative to the Earth’s spin axis (true polar wander). Thus, Vandamme and Courtillot (1990) and Duncan (1990) interpret the 10º difference in palaeolatitudes between the Deccan lavas and the Réunion hotspot as indicating a slow (8 mm/yr) northward motion of the Réunion plume since its birth at 66 Ma. The idea of mantle roll is actually based totally on the assumption that global hotspots constitute a fixed reference frame for plate motions (e.g., Wessel and Kroenke, 1997, 1998), which assumption is actually not valid (e.g., Molnar and Atwater, 1973; Norton, 1995; Tarduno and Gee, 1995; Tarduno and Cottrell, 1997; Aslanian et al., 1998).

The palaeolatitude evidence can in principle be the most daring, and an ideal crucial test to decide between the track of a stationary plume and a propagating fracture. However, it cannot be denied that the whole interpretation above, invoking true polar wander, arises from the premise, rooted in other constraints, that the Réunion hotspot was the cause of the Deccan eruptions. Therefore the palaeolatitude evidence is not any independent evidence. If one dismisses the Deccan–Réunion link, as I do, then true polar wander need not be invoked, and there is in fact no systematic palaeolatitude variation in the Site 706, 707 and 715 basalts (see figs. 1 and 7 of Duncan, 1990).

Burke (1996) makes exactly the same argument, that it is unnecessary to invoke true polar wander for the Deccan–Réunion case, but based on quite different interpretations than mine. The main thesis of Burke (1996) is that the African plate has come to rest over the underlying mantle circulation for the past 30 MY, and since the plate has not moved with respect to the mantle plumes beneath it for that time, an active manifestation of the Deccan plume source would have to be where it was at 30 Ma. However, there has been no volcanism in the expected area (Saya de Malha Bank, Nazareth Bank or Cargados Carajos Bank) over the past 30 MY. Correctly discounting the possibility that only the Réunion plume has moved south by 700 km while all other plumes beneath Africa have remained where they were, he interprets the gap in volcanic activity between 30 Ma and 7–2 Ma as evidence that the plume which generated the Deccan basalts died out at 30 Ma. Thus his argument against true polar wander is that the Deccan eruptions took place close to 25ºS, and the Réunion plume is separate from the Deccan plume, and therefore the location of the Réunion plume at 21ºS today does not provide evidence for true polar wander. In Burke’s opinion Mauritius, Rodriguez Ridge, Rodriguez Island, and Réunion Island are four separate members of the youthful population of African hotspots, each of which is underlain by its own discrete young plume.

With the last proposition, I naturally disagree, but interpret the whole scenario as evidence for ultimate control on volcanism by lithospheric stress. In particular, the event of sudden “dying out of the plume at 30 Ma”, i.e., the rapid turn-off of volcanism, can have no better explanation (see Section 5). I have already noted the fact that the older part of the Deccan–Réunion track is along the Vishnu fracture zone. Likewise, the younger part on the African plate is along the Mauritius fracture zone, and there is no need (or justification) for one or more plumes. Bailey (1992) has already pointed out this fact for African volcanism, noting that when Africa goes into plate-wide extension there is widespread volcanism; when it goes into compression the magmatism ceases. Likewise, although there are extensive low-seismic velocity regions under the Indian Ocean, Central America, western equatorial Pacific and the western Atlantic, all these are areas of regional compression, and therefore lack midplate volcanism, indicating that hot upper mantle is not a sufficient condition for magmatism (Anderson et al., 1992b). The notable lack of present-day OIB volcanism in the Indian Ocean may be due to this simple fact.

Thus collapses all the alleged geochemical, thermal, topographic, and plate tectonic evidence for plume involvement in the genesis of the Deccan–Réunion hotspot track. The data indicate that the Deccan eruptions may have had no genetic connection with the Réunion hotspot; nor may be the Réunion hotspot underlain by a deep mantle plume.

The ‘tail’ part of the hotspot track (outside the limits of the Deccan and India, i.e., the track beginning in the Laccadives–Maldives Ridge and ending at Réunion Island) may be a southerly propagating fracture tapping a shallow, enriched mantle of the Indian Ocean (though that too is consistent with the published radiometric ages, not really with palaeolatitudes), or a huge shear zone (see Storetvedt, 1990, 1992, 1997), and reflects the *stress field* of the Indian Oceanic lithosphere (following Jackson and Shaw, 1975), not its *displacement history*. Indeed, the current strong seismicity in the Chagos–Laccadives region indicates a complex deformation pattern, with focal mechanisms showing thrust, normal, and strike-slip faulting, and likewise the so-called ‘aseismic’ Ninetyeast Ridge is experiencing left-lateral strike-slip faulting, with a level of seismicity like that of transform faults elsewhere (e.g., Wiens et al., 1986; Neprochnov et al., 1988, 1998; Gordon et al., 1998; Deplus et al., 1998). Therefore, the popular and widespread notion that hotspot tracks are simply the products of one or more plumes beneath moving plates is actually far from reality. And, as we let go the plume model for Deccan volcanism, let us now see how this fetches us a new reward in a completely different, but related, quarter.

13.4. The cause of Western Ghats uplift

The Western Ghats ridge or escarpment is a watershed for most of peninsular India, and extends, almost unbroken, from north of Bombay down to the southern tip of India (Cape Comorin) (Fig. 3). Its impressive length of 1500 km justifies its inclusion in the list of several ‘great escarpments’ of the world (Ollier and Powar, 1985; Ollier, 1990). Devey and Lightfoot (1986) stated that the strong control of the ridge on the drainage of India and the ridge’s present large elevation and its linearity, indicate it to be the product of an uplift event concentrated along the western margin of India. They considered the underplating model of McKenzie (1984) for such epeirogenic uplifts as suitable for the Deccan proper region, but wrote that “problems arise when extrapolating this underplating model to the whole western margin of India, where over a distance of some 1000 km to the south of the lava exposures no evidence of Deccan age magmatism is seen . . . it seems unlikely that such underplating could ever have taken place in the region to the south of the Traps as the Chagos–Laccadive ridge, the postulated trace of the hotspot responsible for the Deccan magmatism, separates from the Indian continental shelf at approximately the same latitude as the present-day southerly limit of basalt exposure. Thus for the moment the generation of the Western Ghats ridge is a major problem.”

This major problem is, however, clearly a model-imposed one, and disappears when the wrong working model is done away with: the Chagos–Laccadives Ridge is not the tail part of a mantle plume, of which the Deccan province is the head. There *was* Deccan-age activity at 69 Ma along the southern tip of India, represented by the Kerala dikes studied by Radhakrishna et al. (1994), like whom I argue that these dikes are feeders to flows long eroded. There must have been substantial underplating under this area as well, and the resulting topographic uplift is not merely a consequence of this underplating (“real” uplift of Cox, 1988b), but also a consequence of great volumes of surface lava flows having been eroded (“apparent” uplift of Cox, 1988a,b), so that even the feeder dikes are exposed. These real and apparent uplifts have combinedly resulted in peaks of majestic heights along the southern part of the Western Ghats, unparalleled anywhere else in the entire Indian peninsula. Mt. Dodabetta in the Nilgiri Hills thus rises 2670 m above sea level, whereas the highest peak within the Deccan region, Mt. Kalsubai, stands at 1654 m (Fig. 3), where the lavas themselves extend to several hundred metres below the Earth’s surface, completely hiding the feeder dikes. Cox (1988a) has made a similar interpretation of the coastal escarpments of the Karoo and Parana provinces.

14. Helium isotopes

Space does not permit a discussion of helium isotope data in detail and only a few comments are possible, and the reader is referred to the following detailed works which show that high $^{3}He/^{4}He$ ratios, unlike as is commonly thought (e.g., Basu et al., 1993; Lassiter and DePaolo, 1997; Sharma, 1997), do not require a mantle plume, or do not necessarily indicate a deep mantle or primordial origin.

There is one to two orders of magnitude *less* ^{3}He in 'plume' magmas than in MORB, which are thought to be from a depleted mantle (Sano and Williams, 1996; Anderson, 1998c, 1999). This big paradox has led to the ad hoc suggestion that ^{3}He must be leaking from the lower mantle into the upper mantle (the MORB source). The chain of inference "High ^{3}He/^{4}He ratios imply excess ^{3}He implying an undegassed reservoir implying a deep mantle source" is incorrect. There are several alternative explanations, and the primordial mantle explanation disregards all of them.

Another self-contradiction is that if the inferred high ^{3}He contents of OIB magmas are due to a primordial mantle source, then the abundances of U and Th must be high in that source (as the source has not contributed to continental crustal formation). Then, by the radioactive decay of U and Th, ^{4}He would increase, and the ^{3}He/^{4}He ratio would be actually *low*, not high.

Still another problem is that high ^{3}He/^{4}He basalts which have been inferred to have excess ^{3}He also show severe atmospheric or seawater contamination (e.g., Fisher, 1985). The explanation by Anderson (1993), which involves deep recycling of oceanic crust with sediments that contain interplanetary dust particles, has found little favour with geochemists for various reasons. However, high ^{3}He/^{4}He ratios in hotspot basalts may in fact reflect low ^{4}He, not excess ^{3}He, and could come from U–Th-depleted refractory lithosphere containing CO_2-rich fluid inclusions which trap ^{3}He (Anderson, 1998c, 1999), or from a phlogopite-rich perisphere. In conclusion, the last possible argument for deep mantle plumes — high ^{3}He/^{4}He ratios — fails.

15. Epilogue

It is clear that the original limits of the Deccan province were far more extensive than its main outcrop in west-central India, and the cause of the uplift of the 1500-km-long Western Ghats ridge was Deccan-related magmatic underplating all along it. The Laccadive–Chagos ridge cannot represent the 'tail' part of a hypothetical mantle plume, nor do the Deccan eruptions represent its 'head'. The Laccadive–Chagos Ridge and the entire seamount chain from there up to Réunion Island, is a southerly propagating fracture zone (accepting the published radiometric dates). This seamount chain reflects the stress conditions of the Indian Oceanic lithosphere and not its displacement history, and the existing data, which have been used to support the plume model in a superficial way, and often forced into conformity with this model, undermine the same model when reinterpreted without any bias, pre-conceived ideas or special pleading. The grand Deccan volcanic episode is not the product of any hypothetical mantle plume but the result of protracted processes of lithospheric rifting and early low-volume alkaline magmatism, ending in a catastrophic culmination due to lithospheric splitting. Such rifting, thinning and extension of the lithosphere are necessary for flood volcanism itself and ideas picturing these essential prerequisites to flood volcanism as subsequent to volcanism are highly misleading and mistaken.

Not only the Deccan, but most LIPs of the world (if not all), despite widespread and popular conceptions, seem inconsistent with plumes. All the evidence that has been used so far to support the plume model — geochemical, petrological, thermal, topographic — is equivocal at best, if indeed not contrary. The plume idea is ad hoc, artificial, unnecessary, inadequate, and in some cases even self-defeating, and should be abandoned. Numerous alternatives, some of them demonstrably with a far greater explanatory power and a far stronger factual basis, exist.

The search for *the* theory of the Earth continues, and we would do well to have a multitude of working hypotheses and a critical as well as objective approach towards all of them. New problems will continue to dare us in the future, only to be solved in turn, and to generate still newer questions in the process. This should lead to a continuous improvement in our understanding of the workings of this dynamic Earth, our one home in this immeasurably vast Cosmos. To cite Davis (1984), opportunities for discovery are limitless.

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References

Albarède, F., Tamagnan, V., 1988. Modelling the recent geochemical evolution of the Piton de la Fournaise Volcano, Réunion Island. J. Petrol. 29, 997–1030.

Alexander, P.O., 1981. Age and duration of Deccan volcanism: K–Ar evidence. In: Subbarao, K.V., Sukheswala, R.N. (Eds.), Deccan Volcanism. Mem. Geol. Soc. Ind. 3, 224–258.

Anderson, D.L., 1985. Hotspot magmas can form by fractionation and contamination of MORB. Nature 318, 145–149.

Anderson, D.L., 1989. Theory of the Earth. Blackwell, Boston, MA, 366 pp.

Anderson, D.L., 1993. Helium-3 from the mantle: Primordial signal or cosmic dust? Science 261, 170–176.

Anderson, D.L., 1994a. The sublithospheric mantle as the source of continental flood basalts: the case against the continental lithosphere and plume head reservoirs. Earth Planet. Sci. Lett. 123, 269–280.

Anderson, D.L., 1994b. Komatiites and picrites: evidence that the 'plume' source is depleted. Earth Planet. Sci. Lett. 128, 303–311.

Anderson, D.L., 1995. Lithosphere, asthenosphere, and perisphere. Rev. Geophys. 33, 125–149.

Anderson, D.L., 1996. Enriched asthenosphere and depleted plumes. Int. Geol. Rev. 38, 1–21.

Anderson, D.L., 1998a. The scales of mantle convection. Tectonophysics 284, 1–17.

Anderson, D.L., 1998b. The EDGES of the mantle. In: The Core–Mantle Boundary Region. AGU Geodynamics Ser. 28, 255–271.

Anderson, D.L., 1998c. The helium paradoxes. Proc. Natl. Acad. Sci. USA (Geophysics) 95, 4822–4827.

Anderson, D.L., 1999. A Theory of the Earth: Hutton and Humpty-Dumpty and Holmes. In: Geol. Soc. London Hutton Conf. Spec. Publ. (in press).

Anderson, D.L., Tanimoto, T., Zhang, Y.-S., 1992a. Plate tectonics and hotspots: the third dimension. Science 256, 1645–1651.

Anderson, D.L., Zhang, Y.-S., Tanimoto, T., 1992b. Plume heads, continental lithosphere, flood basalts and tomography. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. London Spec. Publ. 68, 99–124.

Arndt, N.T., Nisbet, E.G. (Eds.), 1982. Komatiites. George Allen and Unwin, London.

Arndt, N.T., Czamanske, G.K., Wooden, J.L., Fedorenko, V.A., 1993. Mantle and crustal contributions to continental flood volcanism. Tectonophysics 223, 39–52.

Arndt, N.T., Ginibre, C., Chauvel, C., Albarède, F., Cheadle, M., Herzberg, C., Jenner, G., Lahaye, Y., 1998. Were komatiites wet? Geology 26, 739–742.

Aslanian, D., Géli, L., Olivet, J.-L., 1998. Hotspotting called into question. Nature 396, 127.

Bailey, D.K., 1982. Mantle metasomatism — continuing chemical change within the Earth. Nature 296, 525–530.

Bailey, D.K., 1983. The chemical and thermal evolution of rifts. Tectonophysics 94, 585–597.

Bailey, D.K., 1992. Episodic alkaline igneous activity across Africa: implications for the causes of continental breakup. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. London Spec. Publ. 68, 91–92.

Baksi, A.K., 1987. Critical evaluation of the age of the Deccan Traps, India: implications for flood basalt volcanism and faunal extinctions. Geology 15, 147–150.

Baksi, A.K., 1990. Search for periodicity in global events in the geologic record: Quo vadimus? Geology 18, 983–986.

Baksi, A.K., 1994. Geochronological studies on whole-rock basalts, Deccan Traps, India: evaluation of the timing of volcanism relative to the K–T boundary. Earth Planet. Sci. Lett. 121, 43–56.

Baksi, A.K., 1995. Petrogenesis and timing of volcanism in the Rajmahal flood basalt province, northeastern India. Chem. Geol. 121, 73–90.

Basaltic Volcanism Study Project, 1981. Basaltic Volcanism on the Terrestrial Planets. Pergamon, New York, 1286 pp.

Basu, A.R., Renne, P.R., Dasgupta, D.K., Teichmann, F., Poreda, R.J., 1993. Early and late alkali igneous pulses and a high-^{3}He plume origin for the Deccan flood basalts. Science 261, 902–906.

Beane, J.E., Hooper, P.R., 1988. A note on the picrite basalts of the Western Ghats, Deccan Traps, India. In: Subbarao, K.V. (Ed.), Deccan Flood Basalts. Mem. Geol. Soc. Ind. 10, 117–134.

Beane, J.E., Turner, C.A., Hooper, P.R., Subbarao, K.V., Walsh, J.N., 1986. Stratigraphy, composition and form of the Deccan basalts, Western Ghats, India. Bull. Volcanol. 48, 61–83.

Bercovici, D., Mahoney, J., 1994. Double flood-basalt events and the separation of mantle plume heads at the 660 km discontinuity. Science 266, 1367–1369.

Bickle, M.J., 1978. Heat loss from the earth: a constraint on Archean tectonics from the relation between geothermal gradients and the rate of plate production. Earth Planet. Sci. Lett. 40, 301–315.

Biswas, S.K., 1987. Regional tectonic framework, structure and evolution of the western marginal basins of India. Tectonophysics 135, 307–327.

Biswas, S.K., 1988. Structure of the western continental margin of India and related igneous activity. In: Subbarao, K.V. (Ed.), Deccan Flood Basalts. Mem. Geol. Soc. Ind. 10, 371–390.

Burke, K.C., 1996. The African plate. S. Afr. J. Geol. 99, 341–409.

Burke, K.C., Dewey, J.F., 1973. Plume-generated triple junctions: key indicators in applying plate tectonics to old rocks. J. Geol. 81, 406–433.

Burke, K.C., Wilson, J.T., 1976. Hotspots on the Earth's surface. Sci. Am. 235, 46–57.

Campbell, I.H., Griffiths, R.W., 1990. Implications of mantle plume structure for the evolution of flood basalts. Earth Planet. Sci. Lett. 99, 79–93.

Campbell, I.H., Griffiths, R.W., 1991. On the dynamics of long-lived plume conduits in the convecting mantle. Earth Planet. Sci. Lett. 103, 214–227.

Campbell, I.H., Griffiths, R.W., Hill, R.I., 1989. Melting in an Archaean mantle plume: heads it's basalts, tails it's komatiites. Nature 339, 697–699.

Carlson, R.W., 1991. Physical and chemical evidence on the cause and source characteristics of flood basalt volcanism. Austr. J. Earth Sci. 38, 525–544.

Catchings, R.D., Mooney, W.D., 1988. Crustal structure of the Columbia plateau: Evidence for continental rifting. J. Geophys. Res. 93, 459–474.

Chandra, U., 1977. Earthquakes of peninsular India — a seismotectonic study. Bull. Seism. Soc. Am. 67, 1387–1413.

Chandrasekharam, D., 1985. Structure and evolution of the western continental margin of India deduced from gravity, seismic, geomagnetic and geochronological studies. Phys. Earth Planet. Inter. 41, 186–198.

Chandrasekharam, D., Parthasarathy, A., 1978. Geochemical and tectonic studies on the coastal and inland Deccan Trap volcanics and a model for the evolution of Deccan Trap volcanism. N. Jb. Miner. Abh. 132, 214–229.

Class, C., Goldstein, S.L., 1997. Plume–lithosphere interactions in the ocean basins: constraints from the source mineralogy. Earth Planet. Sci. Lett. 150, 245–260.

Cloos, M., 1993. Lithospheric buoyancy and collisional orogenesis: subduction of oceanic plateaus, continental margins, island arcs, spreading ridges, and seamounts. Geol. Soc. Am. Bull. 105, 715–737.

Coffin, M.F., Eldholm, O., 1992. Volcanism and continental break-up: a global compilation of large igneous provinces. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. London Spec. Publ. 68, 17–30.

Coffin, M.F., Head, J.W., 1997. Large igneous provinces: a planetary perspective. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 411–438.

Condie, K.C., 1997. Contrasting sources for upper and lower continental crust: The greenstone connection. J. Geol. 105, 729–736.

Corderey, M.J., Davies, G.F., Campbell, I.H., 1997. Genesis of flood basalts from eclogite-bearing mantle plumes. J. Geophys. Res. 102, 20179–20197.

Courtillot, V., 1980. Opening of the Gulf of Aden and Afar by progressive tearing. Phys. Earth Planet. Inter. 21, 343–350.

Courtillot, V., 1982. Propagating rifts and continental breakup. Tectonics 1, 239–250.

Courtillot, V., 1994. Mass extinctions in the last 300 million years: One impact and seven flood basalts? Isr. J. Earth Sci. 43, 255–266.

Courtillot, V., Besse, J., Vandamme, D., Montigny, R., Jaeger, J.-J., Cappetta, H., 1986. Deccan flood basalts at the Cretaceous/Tertiary boundary? Earth Planet. Sci. Lett. 80, 361–374.

Courtillot, V., Armijo, R., Tapponnier, P., 1987. Kinematics of the Sinai triple junction and a two phase model of Arabia–Africa rifting. In: Coward, M.P., Dewey, J.F., Hancock, P.L. (Eds.), Continental Extensional Tectonics. Geol. Soc. London Spec. Publ. 28, 559–573.

Courtillot, V., Féraud, G., Maluski, H., Vandamme, D., Moreau, M.G., Besse, J., 1988. Deccan flood basalts and the Cretaceous/Tertiary boundary. Nature 333, 843–846.

Courtillot, V., Jaeger, J.-J., Yang, Z., Féraud, G., Hofmann, C., 1996. The influence of continental flood basalts on mass extinctions: Where do we stand? In: Ryder, G., Fastovsky, D., Gartner, S. (Eds.), The Cretaceous–Tertiary Event and Other Catastrophes in Earth History. Geol. Soc. Am. Spec. Pap. 307, 513–524.

Courtillot, V., Manighetti, I., Tapponnier, P., Jaupart, C., Besse, J., 1999. On causal links between flood basalts and continental breakup: a mixed 'active–passive' scenario based on the case of the Afar plume and a review of other trap–rift associations. Earth Planet. Sci. Lett. 166, 177–195.

Cox, K.G., 1980. A model for flood basalt vulcanism. J. Petrol. 21, 629–650.

Cox, K.G., 1983. The Deccan Traps and the Karoo: stratigraphic implications of possible hot-spot origins. IAVCEI Hamburg Meeting Abstracts, 9.

Cox, K.G., 1988a. The Karoo province. In: Macdougall, J.D. (Ed.), Continental Flood Basalts. Kluwer, Dordrecht, pp. 239–271.

Cox, K.G., 1988b. Inaugural address. In: Subbarao, K.V. (Ed.), Deccan Flood Basalts. Mem. Geol. Soc. Ind. 10, xv–xxii.

Cox, K.G., 1989. The role of mantle plumes in the development of continental drainage patterns. Nature 342, 873–877.

Cox, K.G., Duncan, A.R., Bristow, J.W., Taylor, S.R., Erlank, A.J., 1984. Petrogenesis of the basic rocks of the Lebombo. Geol. Soc. S. Afr. Spec. Publ. 13, 149–169.

Crough, S.T., Jurdy, D.M., 1980. Subducted lithosphere, hotspots, and the geoid. Earth Planet. Sci. Lett. 44, 224–230.

Crough, S.T., Morgan, W.J., Hargraves, R.B., 1980. Kimberlites: their relation to mantle hotspots. Earth Planet. Sci. Lett. 50, 260–274.

Curray, J.R., Munasinghe, T., 1991. Origin of the Rajmahal Traps and 85ºE Ridge: Preliminary reconstructions of the trace of the Crozet hotspot. Geology 19, 1237–1240.

Czamanske, G.K., Gurevitch, A.B., Fedorenko, V., Simonov, O., 1998. Demise of the Siberian plume: Palaeogeographic and palaeotectonic reconstruction from the prevolcanic and volcanic record, North-Central Siberia. Int. Geol. Rev. 40, 95–115.

Davis, G.H., 1984. Structural Geology of Rocks and Regions. Wiley, New York, 492 pp.

Davies, G.F., 1990. Mantle plumes, mantle stirring and hotspot chemistry. Earth Planet. Sci. Lett. 99, 94–109.

Davies, G.F., Richards, M.A., 1992. Mantle convection. J. Geol. 100, 151–206.

Deplus, C., Diament, M., Hébert, H., Bertrand, G., Dominguez, S., Dubois, J., Malod, J., Patriat, P., Pontoise, B., Sibilla, J.-J., 1998. Direct evidence of active deformation in the eastern Indian oceanic plate. Geology 26, 131–134.

Devey, C.W., Lightfoot, P.C., 1986. Volcanological and tectonic control of stratigraphy and structure in the western Deccan Traps. Bull. Volcanol. 48, 195–207.

Devey, C.W., Stephens, W.E., 1991. Tholeiitic dykes in the Seychelles and the original spatial extent of the Deccan. J. Geol. Soc. London 148, 979–983.

Dickinson, W.R., 1997. Tectonic implications of Cenozoic volcanism in coastal California. Geol. Soc. Am. Bull. 109, 936–954.

Duncan, R.A., 1990. The volcanic record of the Réunion hotspot. Proc. ODP, Sci. Res. 115, 3–10.

Duncan, R.A., Pyle, D.G., 1988. Rapid eruption of the Deccan flood basalts at the Cretaceous/Tertiary boundary. Nature 333, 841–843.

Duncan, R.A., Richards, M.A., 1991. Hotspots, mantle plumes, flood basalts, and true polar wander. Rev. Geophys. 29, 31–50.

Ellam, R.M., 1992. Lithospheric thickness as a control on basalt geochemistry. Geology 20, 153–156.

Ernst, R.E., Buchan, K.L., 1997. Giant radiating dyke swarms: Their use in identifying pre-Mesozoic large igneous provinces and mantle plumes. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 297–333.

Farnetani, C.G., Richards, M.A., 1994. Numerical investigations of the mantle plume initiation model for flood basalt events. J. Geophys. Res. 99, 13813–13833.

Fisher, R.L., 1985. Noble gases from oceanic island basalts do not require an undepleted mantle source. Nature 316, 716–718.

Fisk, M.R., Upton, B.G.J., Ford, C.E., White, W.M., 1988. Geochemical and experiemental study of the genesis of magmas of Réunion Island, Indian Ocean. J. Geophys. Res. 93, 4933–4950.

Fisk, M.R., Duncan, R.A., Baxter, A.N., Greenough, J.D., Hargraves, R.B., Tatsumi, Y., Shipboard Scientific Party, 1989. Réunion hotspot magma chemistry over the past 65 m.y.: Results from Leg 115 of the Ocean Drilling Program. Geology 17, 934–937.

Gill, R.C.O., Pedersen, A.K., Larsen, J.G., 1992. Tertiary picrites in West Greenland: melting at the periphery of a plume?. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. London Spec. Publ. 68, 335–348.

Gordon, R.G., DeMets, C., Royer, J.-Y., 1998. Evidence for long-term diffuse deformation of the lithosphere of the equatorial Indian Ocean. Nature 395, 370–374.

Griffiths, R.W., Campbell, I.H., 1990. Stirring and structure in mantle starting plumes. Earth Planet. Sci. Lett. 99, 66–78.

Gupta, M.L., Gaur, V.K., 1984. Surface heat flow and probable evolution of Deccan volcanism. Tectonophysics 105, 309–318.

Haase, K.M., Devey, C.W., Mertz, D.F., Stoffers, P., Garbe-Schönberg, D., 1996. Geochemistry of lavas from Mohns Ridge, Norwegian–Greenland Sea: implications for melting conditions and magma sources near Jan Mayen. Contrib. Mineral. Petrol. 123, 223–237.

Hamilton, W.B., 1998. Archean magmatism and deformation were not products of plate tectonics. Precambrian Res. 91, 143–179.

Hawkesworth, C.J., Kempton, P.D., Rogers, N.W., Ellam, R.M., van Calsteren, P.W., 1990. Continental mantle lithosphere, and shallow level enrichment processes in the Earth's mantle. Earth Planet. Sci. Lett. 96, 256–268.

Herzberg, C., 1992. Depth and degree of melting of komatiites. J. Geophys. Res. 97, 4521–4540.

Hill, R.I., 1991. Starting plumes and continental break-up. Earth Planet. Sci. Lett. 104, 398–416.

Hofmann, A.W., White, W.M., 1982. Mantle plumes from ancient oceanic crust. Earth Planet. Sci. Lett. 57, 421–436.

Holm, P.M., Gill, R.C.O., Pedersen, A.K., Larsen, J.G., 1993. The Tertiary picrites of West Greenland: contributions from 'Icelandic' and other sources. Earth Planet. Sci. Lett. 115, 227–244.

Hooper, P.R., 1990. The timing of crustal extension and the eruption of continental flood basalts. Nature 349, 246–249.

Hooper, P.R., 1997. The Columbia River flood basalt province: current status. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 1–27.

Ionov, D.A., Hofmann, A.W., 1995. Nb–Ta-rich mantle amphiboles and micas: implications for subduction-related metasomatic trace element fractionations. Earth Planet. Sci. Lett. 131, 341–356.

Iyer, H.M., Gaur, V.K., Rai, S.S., Ramesh, D.S., Rao, C.V.R., Srinagesh, D., Suryaprakasam, K., 1989. High velocity anomaly beneath the Deccan volcanic province: evidence from seismic tomography. Proc. Ind. Acad. Sci. (Earth Planet. Sci.) 98, 31–60.

Jackson, E.D., Shaw, H.R., 1975. Stress fields in central portions of the Pacific plate: delineated in time by linear volcanic chains. J. Geophys. Res. 80, 1861–1874.

Jacques, A.L., Green, D.H., 1980. Anhydrous melting of peridotite at 0–15 kb pressure and the genesis of tholeiitic basalts. Contrib. Mineral. Petrol. 73, 287–310.

Joseph, S., Nambiar, C.G., 1996. Alkaline nature and taphrogenetic affinity of felsic volcanic rocks of St. Mary Islands, off Mangalore coast. Curr. Sci. 70, 858–860.

Kaila, K.L., 1988. Mapping the thickness of Deccan Trap flows in India from DSS studies and inferences about a hidden Mesozoic basin in the Narmada–Tapti region. In: Subbarao, K.V. (Ed.), Deccan Flood Basalts. Mem. Geol. Soc. Ind. 10, 91–116.

Kamo, S.L., Czamanske, G.K., Krogh, T.E., 1996. A minimum U–Pb age for Siberian flood basalt volcanism. Geochim. Cosmochim. Acta 60, 3505–3511.

Kazmin, V.G., 1991. The position of continental flood basalts in rift zones and its bearing on models of rifting. Tectonophysics 199, 375–387.

Keller, W.R., Anderson, D.L., Clayton, R.W., 1997. Resolution of seismic structure in the mantle beneath Iceland. Eos Trans. AGU 78, F500.

Kennett, B.L.N., Widiyantoro, S., 1999. A low seismic wavespeed anomaly beneath northwestern India: a seismic signature of the Deccan plume? Earth Planet. Sci. Lett. 165, 145–155.

Kent, R.W., 1991. Lithospheric uplift in eastern Gondwana: evidence for a long-lived mantle plume system? Geology 19, 19–23.

Kent, R.W., Storey, M., Saunders, A.D., 1992. Large igneous provinces: sites of plume impact or plume incubation? Geology 20, 891–894.

Kent, R.W., Saunders, A.D., Kempton, P.D., Ghose, N.C. 1997. Rajmahal basalts, eastern India: mantle sources and melt distribution at a volcanic rifted margin. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 145–182.

King, S.D., Anderson, D.L., 1995. An alternative mechanism of flood basalt formation. Earth Planet. Sci. Lett. 136, 269–279.

Larsen, H.C., Marcussen, C., 1992. Sill-intrusion, flood basalt emplacement and deep crustal structure of the Scoresby Sund region, East Greenland. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. Spec. Publ. 68, 365–386.

Larson, R.L., 1991. Latest pulse of Earth: Evidence for a mid-Cretaceous superplume. Geology 19, 547–550.

Lassiter, J.C., DePaolo, D.J., 1997. Plume/lithosphere interactions in the genesis of continental and oceanic flood basalts: chemical and isotopic constraints. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 335–356.

Lightfoot, P., Hawkesworth, C., 1988. Origin of Deccan Trap lavas: evidence from combined trace element and Sr-, Nd- and Pb-isotopic studies. Earth Planet. Sci. Lett. 91, 89–104.

Macdougall, J.D., 1986. Isotopic composition of Deccan and ocean ridge basalts: implications for their mantle sources. J. Geol. Soc. Ind. 27, 38–46.

Mahadevan, T.M., 1994. Deep Continental Structure of India: A Review. Mem. Geol. Soc. Ind. 28. xv +562 pp.

Mahoney, J.J., 1988. Deccan Traps. In: Macdougall, J.D. (Ed.), Continental Flood Basalts. Kluwer, Dordrecht, pp. 151–194.

Mahoney, J.J., Macdougall, J.D., Lugmair, G.W., Gopalan, K., 1983. Kerguelen hot spot source for Rajmahal traps and Ninetyeast Ridge? Nature 303, 385–389.

Mahoney, J.J., Macdougall, J.D., Lugmair, G.W., Gopalan, K., Krishnamurthy, P., 1985. Origin of cotemporaneous tholeiitic and K-rich alkalic lavas: a case study from the northern Deccan Plateau, India. Earth Planet. Sci. Lett. 73, 39–53.

Mahoney, J.J., Natland, J.H., White, W.M., Poreda, R., Bloomer, S.H., Fisher, R.L., Baxter, A.N., 1989. Isotopic and geochemical provinces of the Western Indian Ocean spreading centers. J. Geophys. Res. 94, 4033–4052.

Mahoney, J.J., Duncan, R.A., McCormick, G.R., 1996. The Parh Group of Pakistan: Pre-Deccan submarine volcanism of the Réunion hotspot? In: Deccan'96 Conf. Abstracts, 16.

Mahoney, J.J., Frei, R., Tejada, M.L.G., Mo, X.X., Leat, P.T., Nägler, T.F., 1998. Tracing the Indian Ocean mantle domain through time: isotope results from old West Indian, East Tethyan, and South Pacific seafloor. J. Petrol. 39, 1285–1306.

McDonough, W.F., 1991. Partial melting of subducted oceanic crust and isolation of its residual eclogitic lithology. Philos. Trans. R. Soc. London A 335, 407–418.

McKenzie, D., 1984. A possible mechanism for epeirogenic uplift. Nature 307, 616–618.

McKenzie, D., 1989. Some remarks on the movement of small melt fractions in the mantle. Earth Planet. Sci. Lett. 95, 53–72.

Meyerhoff, A.A., 1995. Surge-tectonic evolution of southeastern Asia: a geohydrodynamics approach. J. SE Asian Earth Sci. 12, 145–247.

Meyerhoff, A.A., Taner, I., Morris, A.E.L., Martin, B.D., Agocs, W.B., Meyerhoff, H.A., 1992. Surge Tectonics: a new hypothesis of Earth dynamics. In: Chatterjee, S., Hotton III, N. (Eds.), New Concepts in Global Tectonics. Texas Tech Univ. Press, Lubbock, TX, pp. 309–409.

Middlemost, E.A.K., 1985. Magmas and Magmatic Rocks. Longman, London, 265 pp.

Mohan, G., Rai, S.S., Panza, G.F., 1997. Shear velocity structure of the laterally heterogeneous crust and uppermost mantle beneath the Indian region. Tectonophysics 277, 259–270.

Molnar, P., Atwater, T., 1973. Relative motion of hotspots in the mantle. Nature 230, 42–43.

Morgan, W.J., 1971. Convection plumes in the lower mantle. Nature 230, 42–43.

Morgan, W.J., 1972. Deep mantle convection plumes and plate motions. Am. Assoc. Pet. Geol. Bull. 56, 203–213.

Morgan, W.J., 1981. Hotspot tracks and the opening of the Atlantic and Indian Oceans. In: Emiliani, C. (Ed.), The Sea, vol. 7. Wiley Interscience, New York, pp. 443–475.

Mutter, J.C., Buck, S.R., Zehnder, C.M., 1988. Convective partial melting, 1. A model for the formation of thick basaltic sequences during the initiation of spreading. J. Geophys. Res. 93, 1031–1048.

Naqvi, S.M., Rao, V.D., Narain, H., 1974. The protocontinental growth of the Indian shield and the antiquity of its rift valleys. Precambrian Res. 1, 345–398.

Neal, C.R., Mahoney, J.J., Kroenke, L.W., Duncan, R.A., Petterson, M.G., 1997. The Ontong Java Plateau. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 183–216.

Neprochnov, Y.P., Levchenko, O.V., Merklin, L.R., Sedov, V.V., 1988. The structure and tectonics of the intraplate deformation area in the Indian Ocean. Tectonophysics 156, 89–106.

Neprochnov, Yu. P., Gopala Rao, D., Subrahmanyam, C., Murthy, K.S.R., 1998. Intraplate Deformation in the Central Indian Ocean Basin. Mem. Geol. Soc. Ind. 39, xiii +250 pp.
Norton, I.O., 1995. Plate motions in the North Pacific: the 43 Ma nonevent. Tectonics 14, 1080–1094.
Norton, I.O., Sclater, J.G., 1979. A model for the evolution of the Indian Ocean and the breakup of Gondwanaland. J. Geophys. Res. 84, 6803–6830.
Olafsson, M., Eggler, D.M., 1983. Phase relations of amphibole, amphibole-carbonate and phlogopite-carbonate peridotite: petrologic constraints on the asthenosphere. Earth Planet. Sci. Lett. 64, 305–315.
Ollier, C.D., 1990. Mountains. In: Barto-Kyriakidis, A. (Ed.), Critical Aspects of the Plate Tectonics Theory, Vol. II. Theophrastus Publications, Athens, Greece, pp. 211–236.
Ollier, C.D., Powar, K.B., 1985. The Western Ghats and the morphotectonics of peninsular India. Z. Geomorph., Suppl. 54, 57–70.
Pandey, O.P., Negi, J.G., 1987. A new theory of the origin and evolution of the Deccan Traps (India). Tectonophysics 142, 329–335.
Peate, D., 1997. The Parana–Etendeka province. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 217–246.
Peng, Z.X., Mahoney, J.J., 1995. Drillhole lavas from the northwestern Deccan Traps, and the evolution of Réunion hotspot mantle. Earth Planet. Sci. Lett. 134, 169–185.
Radhakrishna, B.P., 1965. The Western Ghats of the Indian peninsula. Sagar Symp., pp. 4–14.
Radhakrishna, T., Dallmeyer, R.D., Joseph, M., 1994. Palaeomagnetism and $^{36}Ar/^{40}Ar$ vs. $^{39}Ar/^{40}Ar$ isotope correlation ages of dyke swarms in central Kerala, India: Tectonic implications . Earth Planet. Sci. Lett. 121, 213–226.
Raju, A.T.R., 1968. Geological evolution of Assam and Cambay Tertiary basins of India. Am. Assoc. Pet. Geol. Bull. 52, 2422–2437.
Ravishankar, 1988. Heat flow map of India and discussions on its geological and economic significance. Ind. Miner. 42, 89–110.
Richards, M.A., Duncan, R.A., Courtillot, V.E., 1989. Flood basalts and hotspot tracks: plume heads and tails. Science 246, 103–107.
Sano, Y., Williams, S.N., 1996. Fluxes of mantle and subducted carbon along convergent plate boundaries. Geophys. Res. Lett. 19, 1053–1056.
Saunders, A.D., Storey, M., Kent, R.W., Norry, M.J., 1992. Consequences of plume–lithosphere interactions. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. London Spec. Publ. 68, 41–60.
Saunders, A.D., Tarney, J., Kerr, A.C., Kent, R.W., 1996. The formation and fate of large oceanic igneous provinces. Lithos 37, 81–95.
Scarrow, J.H., Cox, K.G., 1995. Basalts generated by decompressive melting of a mantle plume: a case study from the Isle of Skye, N.W. Scotland. J. Petrol. 36, 3–22.
Sen, G., 1988. Possible depth of origin of primary Deccan tholeiite magma. In: K.V. Subbarao (Editor), Deccan Flood Basalts. Mem. Geol. Soc. Ind. 10, 35–51.
Sharma, M., 1997. Siberian Traps. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 273–295.
Sheth, H.C., 1998. A reappraisal of the coastal Panvel flexure, Deccan Traps, as a listric-fault-controlled reverse drag structure. Tectonophysics 294, 143–149.
Sheth, H.C., 1999. A historical approach to continental flood basalt volcanism: insights into pre-volcanic rifting, sedimentation, and early alkaline magmatism. Earth Planet. Sci. Lett. 168, 19–26.
Sheth, H.C., Chandrasekharam, D., 1996. Deccan volcanism: the work of a plume, a triple junction, and three propagating rifts. 30th Int. Geol. Congr., Beijing, Abstr. vol., 348.
Sheth, H.C., Chandrasekharam, D., 1997a. Early alkaline magmatism in the Deccan Traps: implications for plume incubation and lithospheric rifting. Phys. Earth Planet. Inter. 104, 371–376.
Sheth, H.C., Chandrasekharam, D., 1997b. Plume–rift interaction in the Deccan volcanic province. Phys. Earth Planet. Inter. 99, 179–187.
Sheth, H.C., Duncan, R.A., Chandrasekharam, D., Mahoney, J.J., 1997. Deccan Trap dioritic gabbros from the western Satpura–Tapi region. Curr. Sci. 72, 755–757.
Shrivastava, J.P., Pattanayak, S.K., 1995. Nb–Zr–Y discrimination diagram: a testimony to MORB nature of Deccan Trap basalt. In: Srivastava, R.K., Chandra, R. (Eds.), Magmatism in Relation to Diverse Tectonic Settings, Oxford and IBH, New Delhi, pp. 429–438.
Sleep, N.J., 1992. Hotspot volcanism and mantle plumes. Annu. Rev. Earth Planet. Sci. 20, 19–43.
Sleep, N.H., Richards, M.A., Hager, B.H., 1988. Onset of mantle plumes in the presence of pre-existing convection. J. Geophys. Res. 93, 7672–7689.
Smith, A.D., 1992. Back-arc convection model for Columbia River basalt genesis. Tectonophysics 207, 269–285.
Smith, A.D., 1993. The continental mantle as a source for hotspot volcanism. Terra Nova 5, 452–460.
Smith, A.D., Lewis, C., 1999a. Differential rotation of lithosphere and mantle and the driving forces of plate tectonics. J. Geodyn. (in press).
Smith, A.D., Lewis, C., 1999b. The evolution of scientific thinking in the mantle plume concept: criticism and non-plume synthesis. J. Geol. Soc. China 40, 1–40.
Smith, W.H.F., Sandwell, D.T., 1994. Bathymetric prediction from dense satellite altimetry and sparse shipboard altimetry. J. Geophys. Res. Solid Earth 99, 21803–21824.
Smoot, N.C., 1997. Magma floods, microplates, and orthogonal intersections. In: Choi, D.R., Dickins, J.M. (Eds.). New Concepts in Global Tectonics Newsletter 5, 8–13.
Smoot, N.C., King, R.E., 1997. The Darwin Rise demise: the western Pacific guyot heights trace the trans-Pacific Mendocino fracture zone. Geomorphology 18, 223–235.
Stolper, E.M., Walker, D., 1980. Melt density and the average composition of basalt. Contrib. Mineral. Petrol. 74, 7–12.

Storetvedt, K.M., 1990. The Tethys Sea and the Alpine–Himalayan orogenic belt; mega-elements in a new global tectonic system. Phys. Earth Planet. Inter. 62, 141–184.

Storetvedt, K.M., 1992. Rotating plates: new concept in global tectonics. In: Chatterjee, S., Hotton III, N. (Eds.), New Concepts in Global Tectonics. Texas Tech Univ. Press, Lubbock, TX, pp. 203–220.

Storetvedt, K.M., 1997. Our Evolving Planet: Earth History in New Perspective. Alma Mater, Bergen, 456 pp.

Storey, M., Mahoney, J.J., Kroenke, L.W., Saunders, A.D., 1991. Are oceanic plateaus sites of komatiite formation? Geology 19, 376–379.

Storey, M., Mahoney, J.J., Saunders, A.D., Duncan, R.A., Kelley, S.P., Coffin, M., 1995. Timing of hotspot-related volcanism and the breakup of Madagascar and India. Science 267, 852–855.

Storey, M., Mahoney, J.J., Saunders, A.D., 1997. Cretaceous basalts in Madagascar and the transition between plume and continental lithospheric mantle sources. In: Mahoney, J.J., Coffin, M.F. (Eds.), Large Igneous Provinces: Continental, Oceanic, and Planetary Flood Volcanism. AGU Geophys. Monogr. 100, 95–122.

Subbarao, K.V. (Ed.), 1999. Winds of Change. W.D. West Mem. Vol., Mem. Geol. Soc. Ind. (in press).

Subbarao, K.V., Hedge, C.E., 1973. K, Rb, Sr and $^{87}Sr/^{86}Sr$ in rocks from the Mid-Indian Ocean ridge. Earth Planet. Sci. Lett. 18, 223–234.

Tarduno, J.A., Cottrell, R.D., 1997. Paleomagnetic evidence for motion of the Hawaiian hotspot during formation of the Emperor seamounts. Earth Planet. Sci. Lett. 153, 171–180.

Tarduno, J.A., Gee, J., 1995. Large scale motion between Pacific and Atlantic hotspots. Nature 378, 477–480.

Thompson, R.N., Gibson, S.A., 1991. Subcontinental mantle plumes, hotspots, and pre-existing thinspots. J. Geol. Soc. London 148, 973–977.

Turcotte, D.L., 1974. Membrane tectonics. Geophys. J.R. Astron. Soc. 36, 33–42.

Turcotte, D.L., Oxburgh, E.R., 1973. Mid-plate tectonics. Nature 244, 337–339.

Valsangkar, A.B., Radhakrishnamurty, C., Subbarao, K.V., Beckinsale, R.D., 1981. Palaeomagnetism and potassium–argon age studies of acid igneous rocks from the St. Mary Islands. In: Subbarao, K.V., Sukheswala, R.N. (Eds.), Deccan Volcanism. Mem. Geol. Soc. Ind. 3, 265–276.

Vandamme, D., Courtillot, V., 1990. Palaeomagnetism of Leg 115 basement rocks and latitudinal evolution of the Réunion hotspot. Proc. ODP Sci. Results 115, 111–117.

VanDecar, J.C., James, D.E., Assumpção, M., 1995. Seismic evidence for a fossil mantle plume beneath South America and implications for plate driving forces. Nature 378, 25–31.

van Keken, P., 1997. Evolution of starting mantle plumes: a comparison between numerical and laboratory models. Earth Planet. Sci. Lett. 148, 1–11.

Venkatesan, T.R., Pande, K., Gopalan, K., 1993. Did Deccan volcanism pre-date the Cretaceous/Tertiary transition? Earth Planet. Sci. Lett. 119, 181–189.

Wadia, D.N., 1975. Geology of India. 4th Edn. Tata–McGraw Hill, New Delhi, 508 pp.

Walker, G.P.L., 1993. Basaltic-volcano systems. In: Prichard, H.M., Alabaster, T., Harris, N.B.W., Neary, C.R. (Eds.), Magmatic Processes and Plate Tectonics. Geol. Soc. Spec. Publ. 76, 3–38.

Weis, D., White, W.M., Frey, F.A., Duncan, R.A., Fisk, M.R., Dehn, J., Ludden, J., Saunders, A., Storey, M., 1992. The influence of mantle plumes in generation of Indian oceanic crust. In: Duncan, R.A., Rea, D.K., Kidd, R.B., von Rad, U., Weissel, J.K. (Eds.), Synthesis of Results from Scientific Drilling in the Indian Ocean. Am. Geophys. Union, Geophys. Monogr. 70, 57–89.

Wessel, P., Kroenke, L., 1997. A geometric technique for relocating hotspots and refining absolute plate motions. Nature 387, 365–369.

Wessel, P., Kroenke, L.W., 1998. The geometric relationship between hot spots and seamounts: implications for Pacific hot spots. Earth Planet. Sci. Lett. 158, 1–18.

West, W.D., 1981. The duration of Deccan Trap volcanicity. In: Subbarao, K.V., Sukheswala, R.N. (Eds.), Deccan Volcanism. Mem. Geol. Soc. Ind. 3, 277–278.

White, R.S., 1992. Magmatism during and after continental breakup. In: Storey, B.C., Alabaster, T., Pankhurst, R.J. (Eds.), Magmatism and the Causes of Continental Break-up. Geol. Soc. Spec. Publ. 68, 1–16.

White, R.S., McKenzie, D., 1989. Magmatism at rift zones: the generation of volcanic continental margins and flood basalts. J. Geophys. Res. 94, 7685–7729.

White, R.S., McKenzie, D.P., 1995. Mantle plumes and flood basalts. J. Geophys. Res. 100, 17543–17585.

White, W.M., Duprè, B., 1984. Isotope geochemistry of the mantle beneath the Indian Ocean. Eos Trans. Am. Geophys. Union 65, 1139.

White, W.M., Cheatham, M.M., Duncan, R.A., 1990. Isotope geochemistry of Leg 115 basalts and inferences on the history of the Réunion plume. Proc. ODP, Sci. Res. 115, 53–62.

Wiens, D.A., Stein, S., Demets, C., Gordon, R.G., Stein, C., 1986. Plate tectonic models for Indian Ocean 'intraplate' deformation. Tectonophysics 132, 37–48.

Wilson, J.T., 1963. A possible origin of the Hawaiian Islands. Can. J. Phys. 41, 863–868.

Wolfe, C.J., Bjarnason, I.Th., VanDecar, J.C., Solomon, S.C., 1997. Seismic structure of the Iceland mantle plume. Nature 385, 245–247.

Zhang, Y.-S., Tanimoto, T., 1993. High-resolution global upper mantle structure and plate tectonics. J. Geophys. Res. 98, 9793–9823.

Ziman, J., 1978. Reliable Knowledge. Cambridge Univ. Press, Cambridge.