Discussion of

Plume-related regional pre-volcanic uplift in the Deccan Traps: Absence of evidence, evidence of absence

by

Hetu C. Sheth

5th December, 2006, Peter R. Hooper

In his introductory remarks (p. 3) Sheth quotes a recent paper by Hales et al. (2005) in support of the absence of pre-volcanic uplift in the Miocene Columbia River flood basalt province. This is an unfortunate example of a single and unfounded statement being repeated in the literature to support the author's thesis, despite clear and overwhelming evidence, geological, biological and geophysical, provided by numerous authors that exactly the opposite is true and very easily demonstrated. This evidence is discussed and the relevant authors referenced in Hooper et al. (this volume). Here I simply repeat the most obvious geological evidence for pre- and post-volcanic uplift of the area underlying the main feeder dikes of the Columbia River basalts.

The Chief Joseph dike swarm, the dominant feeder system for the Columbia River basalts, runs NNW through and immediately to the east of the Wallowa Mountains, Oregon. In the Imnaha and all the adjacent Snake and Salmon River valleys to the east of the Wallowa Mountains, the earliest flood basalt eruptions fill very steep sided canyons cut into the older rocks. These areas have been mapped in detail over a large area (e.g. Kleck, 1976; Hooper et al., 1984, see references in Hooper et al., this volume). One does not have to be a rocket scientist, or even a basalt petrologist, to realise that these steep deep canyons formed by river erosion of a rapidly rising landmass and that this uplift must have occurred immediately prior to the basalt eruptions. For Hales et al. (2005) to interpret these deep canyons as ponding in incipient basins and hence suggestive of pre-basalt subsidence is disingenuous and clearly not tenable. Nor is subsidence to the east of the Wallowa Mountains necessary to those authors' interesting thesis that underplating beneath the dramatic uplift of the Wallowa block was a potential factor in the formation of the voluminous Grande Ronde Basalts.

The subsequent development of equivalently shaped canyons through these early basalts, including Hells Canyon, the deepest in the USA, dictates, equally clearly, that this same area continued to rise rapidly after the basalt eruptions. That this rise was specifically along the line of the Chief Joseph dike swarm is illustrated by the steady rise in the Imnaha-Grande Ronde Basalt contact, west to east across this zone from Lewiston (Idaho) to tens of kilometers up the Clearwater River. While the cause of uplift may be open to debate, the occurrence of uplift, both immediately before and after the eruption of the Columbia River basalts, is clearly demonstrated and beyond reasonable dispute.

It is hoped that this simple direct evidence of pre-basalt uplift beneath the Columbia River basalt feeder system will prevent Sheth's statement being repeated through the literature in the future.

6th December, 2006, Hetu C. Sheth

Peter Hooper argues that there was significant pre-volcanic uplift in the Columbia River basalt (CRB) province, reflected in the deep fluvial canyons developed in the pre-basaltic rocks that the basalt lavas filled up. He also states that Hales et al. (2005), whom I have cited as arguing for lack of such uplift, are wrong, and therefore my statement about the lack of such uplift in the CRB province is wrong as well.

This may well be. Having no experience of CRB geology, I reserve judgment on whether Hales et al. (2005) are correct in relating the canyons to incipient basins and pre-eruption subsidence, or whether Hooper (this comment) and Hooper et al. (this volume) are correct in ascribing them to pre-eruption uplift and fluvial erosion. While working on my paper I cited the existing, published literature. In the absence of any published criticism of the work of Hales et al. (2005) I could only quote them as the most recent study I knew of then, relating to the pre-volcanic vertical motion of the CRB.

I thus reserve further judgment regarding my statement about the CRB. In any case, I barely mention the CRB, keeping discussion of other flood basalts to a minimum, and focusing on the Deccan, where I claim sufficient experience. At the same time, it helps to have a brief comparative mention of other flood basalts in one's paper.

It is possible, as Hooper says, that the CRB example doesn't support my thesis. However, I stand by the main thrust of my paper, that there was no pre-eruption uplift (not just absent evidence, but evident absence) in the Deccan.

19th December, 2006, T.C. Hales, J.J. Roering, and D.L. Abt

Dr. Hooper raises a number of interesting questions regarding the nature and timing of uplift in the area surrounding the source of the Columbia River Basalts. His comments highlight the difficulty in determining where (or even if) surface uplift or subsidence occurred prior to eruption of the Columbia River Basalts. Despite these challenges, Dr. Hooper's suggestion that the presence of pre-Imnaha canyons provides evidence of significant uplift in region is simply not supported by the literature. Geoscientists have recently made significant strides in deciphering the coupling between topography and tectonic forcing (e.g. Burbank et al., 1996; Whipple, 2004) and importantly, none of this work suggests that the mere presence of deep canyons can be interpreted as evidence for surface uplift.

Steep and deep canyons form as the result of changes in base level (as first described by Gilbert, 1877; Powell, 1875) which can be controlled by local rock uplift or other processes such as basin reorganization. In some cases, such as the Southern Alps of New Zealand and Taiwan, rapid rock and surface uplift cause rapid base level lowering (e.g. Dadson et al., 2003; Whitehouse, 1988). In contrast, numerous examples demonstrate rapid river incision due to other factors. Among them is another canyon of significance in North America, the Grand Canyon, which cuts through the Colorado plateau as a result of drainage capture or lake overflow (Spencer and Pearthree, 2003 and references therein), not rapid uplift. In fact, we view the Colorado Plateau as a useful analogy for pre-Imnaha northeastern Oregon (Goles, 1986). In our case, Hells Canyon likely formed from the Late Neogene capture of the Snake River by the Columbia River (Alpha and Vallier, 1994; Van Tassel et al., 2001). As such, Hells Canyon is a wonderful example of a canyon of history and circumstance; capture of the Snake River basin increased the Columbia River's drainage area and effectively drained Neogene Lake Idaho resulting in increased local stream power and rapid incision (Van Tassel et al., 2001). Most importantly, this suggests that using canyon incision as evidence for rapid uplift is a fundamentally flawed endeavor, which ignores the principals of landscape evolution (Gilbert, 1877; Powell, 1875).

So how can we determine the pre-eruptive history of the area? As described in Hales et al. (2005), the best markers of uplift are the Columbia River Basalts themselves, because they are well dated and were emplaced as relatively flat layers (Hooper, 1997; Hooper and Camp, 1981; Tolan et al., 1989). The impressive magnitude and detailed nature of lava flow mapping in the area allows us to use them as deformation markers. Furthermore, we argue that the relatively flat orientation of the early Grande Ronde basalt flows lends strong support for the absence of rapid pre-eruptive uplift. If significant pre-eruptive uplift occurred, we would expect significant thinning of the R1 Grande Ronde lava flows from the topographic high created by uplift. As pointed out by Dr. Hooper in his comment, significant thinning of the lava flows does not occur until the last phases of the Grande Ronde eruptions, a point discussed by Hales et al. (2005) as relating to syn-eruptive uplift caused by changes in mantle buoyancy. Another reason we are inclined toward minimal pre-Imnaha topography is the presence of the lava flows themselves. Columbia River basalt flows have been characterized as inflated flows, which can only form on low topographic slopes (typically less than 5˚) (Self et al., 1997). If significant pre-eruptive uplift had occurred in the area one would expect to see channelization, levee development and a range of lava flow forms consistent with basalt emplacement on steep slopes (Hon et al., 1994).

Based on these lines of evidence, we suggest that rapid pre-eruptive uplift of the magnitude proposed by Dr. Hooper is unlikely. Instead, we favor minimal pre-eruptive uplift and possibly pre-eruptive subsidence (Elkins-Tanton, 2005). Granted, the physical evidence for subsidence is relatively poor, mostly because we lack the ability to resolve paleo-topography before emplacement of basalt flows. However, we are able to easily discount rapid pre-eruptive uplift for the reasons outlined above. Determining the pre-eruptive uplift history of this region may be possible via thermochronometry and we welcome any comments that may shed light on this compelling question.

12th January 2007, Don L. Anderson

There are various mechanisms for having large and rapid uplifts concurrent with magmatism. The plume hypothesis is unique in predicting that uplift should precede magmatism by 5 to 20 Ma. The delamination hypothesis predicts a period of subsidence prior to uplift and magmatism. A critical test of the plume vs alternative mechanisms involving delamination, asthenospheric upwelling, slab windows and slab detachment is the timing between subsidence, uplift and magmatism. If large igneous provinces are emplaced on deep sedimentary basins, or at sealevel (or below sealevel if emplaced on an oceanic ridges or triple junctions e.g. Ontong Java Plateau) then this would argue against the plume hypothesis, particularly if there is no heatflow anomaly. Uplift is not the issue; it is the timing of the uplift. Numerical models of plumes do not predict the sequencing of observed uplifts, in spite of repeated claims to the contrary.

20th January 2007, Peter R. Hooper

I reply to the comment of Hales and others, 19th December, 2006.

Whipple (2004) and subsequent work indeed illustrates the significance of factors other than rapid uplift for the formation of deep canyons, but that hardly precludes the established view that uplift triggers river erosion. River capture, the most obvious alternative to rapid uplift, also requires the captured system to be at a much higher elevation than the capturing system. While the present canyon system, including Hells canyon, may well be due in significant part to the capture of the upper Snake River by the Salmon-Columbia River system, that capture occurred long after the CRBG eruptions and was clearly aided by the continued uplift of the area along the Idaho-Oregon border as evidenced by the rise of the Imnaha Grande Ronde Basalt contact from west to east as noted earlier. There is no evidence of an earlier river-capturing event capable of forming the pre-Imnaha Basalt steep-sided canyons. I re-emphasise the very steep-sided nature of the canyons filled by the first CRBG eruptions (Imnaha Basalt), the lack of evidence for any river-capturing event prior to those eruptions, and consider the rapid rise of the area above the CRBG feeder system as the only plausible explanation for the formation of the steep-sided Imnaha Basalt-filled canyons.

Hales et al. (comment 19th December, 2006) are incorrect in stating that significant thickening of Grande Ronde flows from east to west is confined to the last phases of the Grande Ronde eruptions. It is true that the sudden uplift along the NW trending Limekiln Fault prevented anything but small volcanic cones of Grande Ronde N2 lava developing to the southeast of that structure while large volumes of N2 lava erupted on the northwestern down throw side. But it is equally true that all Grande Ronde flows (R1 through N2) thicken westwards. As recorded many years ago, this creates an off-lap effect, reflecting the continuing rise of the area beneath the CRBG feeder system. The thickness of CRBG flows in the central Columbia Plateau is almost three times the thickness of the basalt that accumulated around the feeder dikes (Hooper and Camp, 1981; Tolan et al., 1989; Hooper, 1997).

The argument of Hales et al. (comment 19th December, 2006) that doming prior to eruption would cause channeling rather than growth by inflation is equally unrealistic. First, the Imnaha flows and Grande Ronde flows on the east side of the Wallowa uplift have strikingly different physical properties to the inflated flows of the central Columbia Plateau; their mode of emplacement has yet to be investigated. Second, the implication that doming over the plume head, if it occurred, would have caused flows to be channelled and not fed by inflation fails to recognize the likely slope angle that would result from doming. For instance, if the radius of the dome were 500 km and the central elevation 1-2 km, then the dip on the dome flank would be 0.250 degrees. Even assuming that the surfaces were perfectly smooth, the yield strength of erupted basalt is such that this low slope angle would be unlikely to modify the evolving flow patterns or methods of flow growth. These low angles are likely to be the underlying cause of the massive inflated pahoehoe sheets that are so characteristic of the central Columbia Plateau (Thordarson and Self, 1988) and which are consistent features of many other continental flood basalts (Jerram and Widdowson, 2004).

I conclude by reiterating the belief that the evidence for rapid rise of the area underlying the main feeder dikes for the CRBG along the eastern margin of the Columbia Plateau is compelling and that the arguments advocated by Hales et al. (comment 19th December, 2006) are, at best, equivocal. To this must be added the geophysical and botanical evidence for doming (Parsons et al., 1994; Saltus and Thompson, 1995; Pierce et al., 2002) cited in Hooper et al. (this volume).
All references cited above are listed either in Sheth (this volume) or in Hooper et al. (this volume).

2nd February 2007, Mike Widdowson

Sheth (this volume) offers an interesting, albeit controversial, interpretation to some of the geological and geomorphological data from the Deccan Volcanic Province (DVP). He continues an essentially plume-skeptic theme adopted in previous papers (e.g. Sheth, 1999; 2005). Here, he challenges the "plume model" by attempting to demonstrate a "lack of regional uplift" prior to the Deccan eruptions. There are some excellent background sections, but it would be wrong to conclude that this paper offers either a comprehensive or a balanced review of the available geophysical, geochemical, and geomorphological data. Moreover, the issues regarding plume-related uplift, and associated geophysical data concerning evidence for a Deccan plume, are actually far more complex than admitted (e.g. Kennet and Widyantoro, 1999; Burov et al. 2005).

Sheth correctly focuses on the volcano-sedimentary evolution of the DVP in seeking evidence for, or against, pre-eruptive doming of the region, but provides only those interpretations consistent with a particular viewpoint. Alternative interpretations do exist, and these are more widely accepted. Of more immediate concern is that this paper fails to present us with a viable alternative to the currently preferred plume-based view for the DVP; this is a serious omission. Sheth's assertion, that the Deccan Volcanic Province (DVP) is not the result of a Late Cretaceous mantle plume, might be made more credible if it were tested and explored through application of the available, alternative models (e.g. edge-driven convection, EDC): Instead, only a critique of previous work is offered.

It is apparent that Sheth favours the EDC family of models (e.g. Sheth, 1999; 2005), but fails to consider that these might present even greater inconsistencies if adopted as an alternate explanation of the DVP and the wider pre- syn- and post-rift evolution of the Western Indian margin. For any model to be credible for the DVP, it must adequately explain that,

a) independent of global eustatic changes, offshore sedimentation rates increase significantly around peninsula India during the Late Cretaceous, and then wane again during early Palaegene (most scholars would conclude that this indicates regional uplift of the continental hinterland), and
b) the fact that rift-related diking, the Seychelles rifting event, and associated sea-floor spreading clearly post-date the bulk of the DVP tholeiitic eruptions (e.g. Hooper, 1990; Devey and Stephens, 1991; Widdowson et al., 2000). Another well-documented observation is the fact that
c) the major DVP eruptive units young (both stratigraphically and chronologically), southwards (Cox 1983; Devey and Lightfoot, 1986; Mitchell and Widdowson, 1991; Vandamme et al. 1993).

Most contend that this DVP structural asymmetry is consistent with the rapid drift of peninsular India over a mantle melting anomaly. Without resorting to the petrogenetic arguments rehearsed elsewhere, it remains an effective argument that these stratigraphical, volcanological, geochronological, and palaeomagnetic data are more consistent with the plume model of DVP evolution than any of the available alternatives (Jerram and Widdowson, 2005).

Like many geoscientists, I am not particularly committed to any model of CFBP evolution. Even so, most would agree that any geological/geophysical model is simply a conceptual framework which aids an understanding of a wide range of observations; typically, it is usually a simplification of the natural world, and thus often imperfect. However, we should at least demand equal testing of all available models; and that this testing should first require determining the model outcome, and then its comparison with actual observational data.

A vast body of recent geological and geomorphological data now exists for the DVP, including examples by Sheth (2001a; 2001b), which might allow the first attempts at this approach. However, much of the observation presented here by Sheth regarding uplift, or lack thereof, is either equivocal, or else can equally be explained in a manner consistent with the very plume-rift interaction model he seeks to contend (e.g. Cox, 1978; White et al., 1987; Ernst and Buchan, 2003; Courtillot et al., 1999).

Another serious omission is the lack of reference to Courtillot et al. (2003) (or indeed, to any of the Deccan research published by Courtillot and colleagues). These authors conclude, reasonably, that in order to demonstrate the presence of a plume, five specific and stringent observational criteria must be met. Unsurprisingly, most of the "mantle plume" sites around the globe fail these tests. However, of those examples that do apparently fulfill all the criteria of plume-related volcanism, the Deccan is given as a prime example. Interestingly, Courtillot et al. (2003) do not consider pre-DVP uplift as being a critical factor, and whether such uplift can actually prove that mantle plumes exist, or not, still remains a polemic issue (see response by Anderson, 12th January).

To the credit of the instigators of this debate, we can now accept that the wider plume model needs to be scrutinised, and perhaps used with a more circumspection. However, it is neither logical nor scientific to conclude that simply because not every fact or interpretation fits a particular geological model, the model then becomes redundant or disproved. For any (CFBP) model to be acceptable, it must be capable of explaining most of the collected observations, and also be viable under most of those comparable geological situations found elsewhere (Jerram and Widdowson, 2005). Accordingly, we should now focus on which model best fits the available DVP data, and thereby seek to develop and refine a wider understanding of the geologically complex phenomena that gave rise to this CFBP.

5th February, 2007, Hetu Sheth

My paper was meant to present the geological and geomorphological evidence from the Deccan Traps that has a bearing on the issue of plume-related pre-volcanic uplift. Any model should address this evidence. Widdowson misses the point. I never intended a geophysical-geochemical-geomorphological synthesis or the discussion of a whole family of genetic models that he expected, simply because the Sheth (2005) paper that he quotes offers these, at some length, for the Deccan. Many workers (e.g., papers in this volume and Foulger et al., 2005) evaluate the plume and other models for other specific areas or in general. I have not invoked the EDC model previously or here.

I believe in field data and logic. Widdowson does not call my discussion logical, though it is not logical to continue to defend a plume model for the Deccan traps, that has failed all predictions and tests and has no physical basis. He does not question the field facts given here such as the Indian peninsular drainage being antecedent to the Western Ghats uplift and not related to a hypothetical plume-generated dome, or the planation surfaces in central India over which the Deccan lavas erupted. These observations bear greatly on the plume issue. The Courtillot et al. (2003) paper, Anderson (2005) shows, uses highly subjective criteria and even circular reasoning for picking plumes, but Widdowson considers it authoritative, and a standard. That paper is irrelevant here. Previously, Widdowson (2005) suggested a single conglomerate at Rajpipla to potentially reflect plume-caused uplift. My much more down-to-earth interpretation of it, supported by comparable examples in Skye, does not evoke comment by him. His new suggestion that pre-volcanic uplift is irrelevant, because Courtillot et al. (2003) think so, well illustrates the moving goalposts that plume-skeptics must wrestle with.

Widdowson considers my paper unbalanced in interpretation. The plume view has been abundantly promoted in the existing literature in a way that is far from balanced by criticism (Anderson and Natland, 2005, estimate that some 500 papers published in the last 10 years simply assume the existence of plumes). More seriously, much of the plume-advocate literature distorts or ignores well-documented facts. The high-level contact between the Deccan lavas and the Gondwana sedimentary rocks in the Pachmarhi area cannot be evidence for plume uplift as was used. A plume-generated dome with rivers flowing away from it simply does not exist; the drainage antecedence was known well before that model was proposed, but ignored. Widdowson asserts that other interpretations of the data I have presented are possible. He does not itemise them but I assume flattening of a plume head, a strong lithosphere, or some similar *ad hoc* embellishment of the plume hypothesis would do. These arguments I noted, and this is not my approach.

I answer the three specific points Widdowson raises:

a) Some references would help. There is a huge Tertiary sedimentary pile along the western Indian rifted margin, and several papers in Gunnell and Radhakrishna (2001) discuss major Neogene uplift of the Western Ghats and the peninsula. Widdowson reports major uplift in the late (90-65 Ma?). In fact, much Deccan volcanism had already occurred by the KTB, and any uplift can be well explained with magmatic underplating, or the buoyancy of melt-depleted residues. See also comment by Anderson (above).

b) True, Seychelles-India separation and sea-floor spreading began after much (not all) Deccan volcanism had occurred. This does not preclude rifting (not an instantaneous process) occurring before/with volcanism. Dikes were arguably being emplaced along the future coastline and in the Narmada-Tapi region and elsewhere, were feeding a lava pile that was growing laterally and vertically, and at some point break-up occurred. This is fully consistent with a rift-related convective melting model (see Fig. 13 of Sheth, 2005).

c) A half-truth. There is a southward younging of stratigraphic formations in the Western Ghats. But Widdowson does not cite new knowledge from geochemical-stratigraphic work (Peng et al., 1998; Mahoney et al., 2000; Sheth et al., 2004) that the northern and northeastern Deccan areas expose formations very similar to (or the same as) the younger formations to the south. Because lavas can flow great distances, it is the feeder dikes that really matter. We are only beginning to locate these (Bondre et al., 2006; Vanderkluysen et al., 2006; Ray et al., 2007), and this argument is lost. A simple southward age progression does not exist and is not even expected when a putative plume head 1000 km across melts simultaneously over large areas.

Widdowson does not raise one point on which I may be mistaken. This is my usage of "planation surface" for the heavily lateritized surface preserved as erosional remnants at the top of the Deccan Traps in the Western Ghats (Widdowson and Cox, 1996; Widdowson, 1997). These authors consider it a constructional surface and the weathered, lateritized top of the basalt pile, implying little erosion subsequent to the eruptions. Planation surfaces are by definition produced by extensive, advanced erosion (and do often undergo lateritization subsequently). I thank Yanni Gunnell for recently pointing out to me this possible error. Recent email discussions with him and Cliff Ollier are much appreciated. Note that Ollier and Pain (2000) do not believe the high lateritized surface as the top of the lava pile; they suggest relief inversion of a laterite-floored river system. This is another issue.

I do not reject the plume model for the Deccan because, as Widdowson puts it, "just not every fact is consistent with it..." The facts quite inconsistent with the plume model, here and worldwide, now number hundreds, like swarms of midges, flying in the face.

5th February, 2007, Peter Hooper and Mike Widdowson

The assertions by both Sheth and by Anderson (in the comments on the Hooper et al., paper, this volume) that because any one suggested result of the plume model is not obvious, that the plume model is therefore void and to be abandoned, is surely unacceptable. What geological model, even plate tectonics itself, can meet such demands? We are looking for the model that best explains flood basalt volcanism. The melt production models of White and McKenzie (1989) and White (1993) indicate that neither elevated mantle temperatures nor lithospheric extension and associated decompressive melting of the mantle (McKenzie, 1978), would alone be sufficient to generate the volume of magma observed in the large CFBs. These authors argue that it is the combination of both the arrival of a plume head and the associated thermal modification of the overlying lithosphere, with its attendant onset of extension and mantle decompression, which together generate massive mantle melting. It is, therefore, unrealistic to criticise only one or other part of this plume-rift scenario and so conclude that plumes are an inadequate explanation of CFBs. If we accept these models as a basis for understanding mantle melting and CFB evolution, then lithospheric extension, underplating, and ponding (Cox, 1980; 1993), are all explicable consequences of the plume-rift scenario. Most importantly, such models remain consistent with our current knowledge of many CFBs.

For Anderson (comments on Hooper et al., this volume) to describe the western margin of India as a "convergent" margin runs entirely counter to current and conventional geological understanding of the region. Both the structure and geomorphology of the western margin is unequivocally that of extensional passive margin, alternatively described as a "volcanic rifted margin". If, and it is not clear, Anderson is alluding to much more ancient mobile zones between Archean cratons (see Sheth, this volume, Fig. 2) then such an association appears vague in the extreme. In the Karoo, while the northern part of the province appears to have erupted over such a zone, the bulk of the Karoo flood basalts erupted over Lesotho. On the Deccan, the center of the main eruption is Kalsubai Mountain, well within a stable craton.

Perhaps uniquely, the Indian margin has been the site of several, sub-parallel rifting episodes over a considerable period of geological time, all associated with the Mesozoic dismemberment of the Gondwana super continent: the separation of Madagascar and India from Africa at c. 160 – 170 Ma, the separation of Madagascar and India at c. 88 – 90 Ma; and the separation of India from the Seychelles - Mascarene plateau at c. 62 – 64 Ma. Of interest here, is the fact that all these separations apparently followed an inherited structural fabric, which is often described within the India landmass as the "Dharwar trend", and which currently runs NNW-SSE in present-day peninsular India. Indeed, modern earthquake epicentres are often traced to lineaments and structural heterogeneities that together form this "fabric" (Widdowson and Mitchell, 1999). Whilst it may be true that development and spreading along the Carlsberg Ridge has more recently (Paleogene - Neogene?) altered the general stress field in the Arabian Sea to that now comprising a component of a compressive rather than extensional effects (Whiting et al. 1994), there is no indication that Western India ever has developed, or is even likely to develop into, a convergent margin.

By contrast, it is true that the northern part of the Indian plate now constitutes part of a major convergent margin, but even northern India records a change from an essentially Mesozoic extensional regime resulting from the northerly subduction of Tethyan ocean crust beneath Eurasia (during which the E-W trending Narmada and Tapti rifts formed, deepened, and were filled with Jurassic and Cretaceous marine and terrestrial clastic sediments - see Sheth Fig. 2), to a Paleogene compressive regime when the Indian and Eurasian plates actually collided and the Himalayas began to develop. However, even if this essentially N - S orientated convergent tectonics represents the convergent margin that is being described by Anderson, then the timing of the India - Asia collision, and its effect in Northern India, both significantly pre-dates, and is entirely spatially disparate with, the eruption of the Deccan Traps.

Of critical importance to the evolution of the Deccan Traps CFB is the timing of the separation of the Seychelles-Mascarene plateau. Hooper (1990) has argued that rifting largely post-dates eruption, and current paleomagnetic data (e.g. Todal and Eldholm, 1998) reveal that the earliest sea floor spreading between the two continental fragments occurred during Chron 28 (i.e. 62.5 – 64 Ma), further supporting this view. Thus, the timing of Deccan eruption versus Seychelles – Mascarene separation not only places important constraints upon the models promulgated by White (1993), but also presents a serious challenge to the alternative EDC model favoured by Sheth.

7th February 2007, Mike Widdowson

Sheth misreads key aspects of my earlier comment. For the record: I consider "uplift" an issue central to the plume debate. I reiterate; uplift "remains a polemic issue", (i.e. worthy of discussion). Word limits preclude the desired detailed dialogue, but I raise some important issues below.

I entirely agree that concerted fieldwork is necessary, but both this paper and Sheth (2005) offer little new field data, and instead rely largely upon an interpretation of previous authors' information. Such retrospectives do not permit the reader to evaluate the relative merits of plume versus non-plume models, and so cannot materially progress debate.

The argument regarding southward (not eastward, as suggested by Sheth) younging of the main Deccan edifice remains robust. Three independent lines of geological evidence support this interpretation: Two independent lines are usually deemed sufficient to indicate a scientific "truth".

Sheth concludes, logically, that the Western Ghats is the product of post-Deccan denudational processes. This particular interpretation has long been available (e.g. Widdowson and Cox, 1996; Widdowson, 1997; Gunnell and Fleitout, 1998; Widdowson and Mitchell, 1999). Given this issue is not in dispute, why raise it here?

Sheth asserts that the plume-head drainage idea of Cox (1989) is problematic. Perhaps, but the fact that radial drainage patterns do occur in key CFBPs remains a valid, if inexplicable (?) observation. Cox's idea was superseded by arguments provided in Widdowson and Cox (1996), Widdowson (1997; see Fig. 14), and apatite fission track analysis data (Gunnell et al., 2003), and so becomes irrelevant for contending pre-eruptive uplift.

Sheth argues, correctly, that the nature of the pre-Deccan palaeosurface holds important clues regarding pre-eruptive uplift in the DVP (Jerram and Widdowson, 2005). Much of this surface remains buried by the Deccan lavas, and is both inaccessible and unknowable. It only becomes exposed around the northern and eastern periphery of the main lava pile. Such peripheral localities, including many of those described by Sheth, were hundreds of kilometers from the Deccan eruptive loci. If any uplift did occur here, it would have been minimal at such large distances from the focus of putative plume head uplift, and thus consistent with that affecting the Dongargaon basin, for example (Tandon, 2002; Samant and Mohabey 2005).

The pre-eruptive palaeosurface has been significantly modified by the crustal loading of the Deccan edifice, and in its western extensions suppressed far below datum. Thus, the gross form and elevation of this basement – basalt contact is largely an artifact of post-eruptive flexural adjustment. Nevertheless, Sheth argues that this highly modified surface reveals a "peneplain", and that its preservation as such precludes significant fluvial incision. Possibly, but peneplains are the consequence of erosion, and the classical, albeit obsolete, Davisian model requires regional uplift as a trigger for peneplanation to proceed. Etchplanation is more appropriate to the development of the pre-Deccan surface (e.g. Büdel, 1982). Here, thick alteration mantles accumulate through tropical weathering of surfaces exposed during prolonged periods of tectonic stability. If, as Sheth requires, such conditions had characterised the pre-Deccan land surface, then the widespread absence of deep weathering mantle preserved beneath the lava units may instead indicate that this landscape had been thoroughly stripped prior to DVP eruptions. Etchplain stripping may be achieved through widespread fluvial erosion induced by regional uplift (Borger and Widdowson, 2001).

Offshore sedimentary records in the Krishna, Godavari, and Narmada-Tapti basins, all reveal significant increases in Late Cretaceous depositional flux (Halkett et al. 2001): these data are consistent with pre-eruptive regional erosion of peninsular India – starting with the stripping of an easily erodable weathering mantle perhaps.

If pre-eruptive (plume-driven?) uplift had occurred in pre-Deccan peninsular India, what might then be recorded in the erosional and sedimentary chronologies of the DVP peripheral regions? Removal of any easily erodable weathering mantle, perhaps; minimal changes in elevation, possibly; development of shallow basins receiving fine clastic input from the plume-uplift effects hundreds of kilometers away – maybe. This interpretation of the available infra- and intra-trappean sedimentary (i.e. Lameta Beds) data is equally plausible using the same compendium of field evidence provided by Sheth. Accordingly, I offer a modified, précis version of Sheth's own summary:

"Any original flatness and elevation of the pre-Deccan landscape has been significantly modified by syn- and post-eruptive isostatic adjustment deriving, initially, from the loading of the DVP edifice, and subsequently by denudational unloading. The occurrence of a stripped, pre-eruptive etchplain, together with associated offshore sedimentological data, are consistent with those phenomena predicted had a large plume head upwelled beneath India during the Late Cretaceous.

Post-Deccan uplift has elevated both the pre-Deccan, and post-Deccan surfaces. This uplift of the Western Ghats is not related to a putative Deccan plume: it is not domal, occurs beyond the limits off the Deccan lava cover, and represents a later, denudationally-driven, uplift (Widdowson, 1997). Thus, the easterly drainage of the Indian peninsula is not plume-related dome flank drainage, and is largely antecedent to denudational uplift effects".

To summarise, of those observations described by Sheth, most, if not all, can equally and adequately be explained by the passage of India over a static, spatially restricted, mantle melting anomaly during the Late Cretaceous: For want of a better term, and until consensus offers me a better alternative, I will continue to call this anomaly, *sensu lato*, a "mantle plume". I end by reiterating the rationale to my initial comment: The challenge to Sheth remains to deliver us an alternative, "non plume", model that can better explain the Deccan CFBP.

9th February 2007, Hetu C. Sheth

I reiterate that I do not reject the plume model for the Deccan just because only one observation (on uplift) is in conflict with it. I mentioned many others in Sheth (2005), and Kumar et al. (this volume) give us yet another. I am also *not* proposing the EDC model (King and Anderson, 1995, 1998); the repeated assertion that I am is mystifying. Plate tectonics is surely not a hypothesis at the same level as the plume hypothesis.

I propose that the Deccan is related to continental rifting, and invoke a simple, rifting-related convective melting model (Sheth, 2005) that, unlike the plume model, does not require or predict precursory uplift or high temperatures. These are unique to the plume hypothesis. I refer Hooper and Widdowson to recent work (e.g., Van Wijk et al., 2001) showing that mantle plumes (or rather, anomalously high temperatures) are not required for volcanism at rifting continental margins. This work, developed for the North Atlantic, fits the Deccan well.

I completely agree that western India has been an extensional-rifting region for a long time. I presume that Anderson (comment of 28th January, 2007, on Hooper et al., this volume) was referring to early phases of the Greater India-Asia collision and Indian plate flexure that, with ~N-S compression, might have caused the ENE-WSW-trending Narmada-Tapi tectonic zone to experience extension.

I disagree with Hooper (1990) that there was no-prevolcanic extension in the Deccan (Sheth, 2000; Ray et al., in press). Pre-volcanic, and certainly pre-breakup, extension produced many preferentially oriented dike swarms in the Deccan, many of which I take as feeders to the lavas, much of the lava pile erupted, and then the breakup occurred, during which younger dike swarms were emplaced. Thus, continental breakup certainly followed the main phase of volcanism. There is nothing inconsistent here with the rifting-related melting model I endorse.

In response to the comment of Widdowson of 7th February, 2007, I concede that many observations in my paper (this volume) are not new nor my own. My objective was to compile as many of these as possible that are relevant to the plume debate in one place, and give due credit to past works. I do not see this as a shortcoming of my paper.

I am not proposing an eastward age progression for the Deccan in place of southward. I said that no age progression is evident and is not even expected if there were a large plume head supplying melt over extensive areas.

The post-Deccan uplift of the Western Ghats and the drainage issue, which Widdowson says are not new, are discussed at length because some of the greatest champions of the plume model (e.g., Campbell, 2005; Campbell and Davies, 2006) still vigorously defend the plume model, citing the drainage pattern of India as evidence for a Deccan plume.

Widdowson feels that “the fact that radial drainage patterns do occur in key CFBPs remains a valid, if inexplicable (?) observation.” This observation is neither valid nor inexplicable. The drainage patterns are not radial, just as the uplifts, which are substantially younger, are not domal. The key CFBPs have experienced major Neogene uplift (e.g., Bonow et al., 2006; Ollier and Pain, 2000) whose cause(s) we do not fully understand. Doglioni et al. (2003), who propose an active eastward-directed asthenospheric mantle flow since at least 40 Ma, as the cause of ridge depth asymmetries and continental margin uplifts, have an interesting hypothesis. Both the western Indian rifted margin and the West Greenland margin, that have experienced significant Neogene uplift, directly block this proposed eastward mantle flow.

The pre-Deccan planation surfaces around Pachmarhi are not hundreds of kilometers from the Deccan eruptive loci as Widdowson asserts. Huge dikes and intrusions are known from the area, and inferred to be the feeders of lavas nearby (Sen, 1980). Work is currently ongoing on these intrusions and their chemical-isotopic correlations to the lava flows.

The field evidence from Dongargaon shows that the uplift was purely local and of a few meters (Tandon, 2002), which is not evidence for a plume. I have not claimed that local, pre-eruptive subsidence refutes the plume model. Nor have I stated that the pre-Deccan eruptive surface in the Western Ghats, which is not visible at all, was a peneplain. Nothing can be said about it.

Widdowson states that “the obsolete Davisian model requires regional uplift as a trigger for peneplanation to proceed”. On the contrary, Ollier and Pain (2000) write that peneplains necessarily develop at or near the base level of erosion, and because there is no geomorphic process capable of producing a peneplain at high elevation, high-level peneplains indicate relatively recent uplift.

Widespread lateritization of the sub-Trap planation surfaces around Pachmarhi and in central India is indeed apparent (Venkatakrishnan, 1984, 1987).

The point about the Lameta sediments, which Widdowson considers consistent with fine clastic input from a far-away plume-uplifted area, was that several of these sediments contain volcanic detritus, and thus eruptions were already underway in the source areas. This proves the uplift to be syn- or post-eruption, perfectly consistent with emplacement of hot intrusions in the crust or buoyancy of the melt-depleted residue in the shallow mantle.

Widdowson offers a re-written summary of the thesis of my paper. I agree with the second half in full but with the first only in part. Lastly, he states that “of those observations described by Sheth, most, if not all, can equally and adequately be explained by the passage of India over a static, spatially restricted, mantle melting anomaly during the Late Cretaceous”. My point is that they are equally well, if not better, explained without such an anomaly. Widdowson now redefines as a plume any static, spatially restricted, mantle melting anomaly. He thus drops uplift, helium, even temperature as criteria for plumes. Others argue that even fixity is not a criterion. Frequent redefining of "plume" in this way also does not materially progress debate.

There is an observation in my paper that, for the sake of accuracy, I wish to set right. Venkatakrishnan (1987) correlates a lava flow at the top of Mt. Dhupgarh, Pachmarhi, with lava flows capping the Gondwanas at Tamia (Fig. 10b), and considers the base of the Dhupgarh flow as the sub-Trap planation surface. I visited the Pachmarhi-Tamia area (Fig. 10a). The Dhupgarh “lava flow” apparently refers to the spheroidally weathered basaltic/doleritic rock outcropping at the western edge of the mountain, at Sunset Point, some 75 m below the summit itself. This is a N-S-trending doleritic dike that is seen clearly in the road cut about 50 m below Sunset Point. The dike does not quite reach the summit, which shows no lava cover, and the whole mountain comprises Gondwana sandstone intruded by dikes. Three lava flows occur at Tamia (Sen, 1980; Venkatakrishnan, 1987), and it remains to be seen whether their base corresponds to the Dhupgarh Surface or the Pachmarhi Surface. The “basalt cliffs” shown in the section of Venkatakrishnan (1987) (Fig. 10b) are not cliffs–the section greatly exaggerates the basalt thickness. The imposing near-vertical Tamia scarp comprises the Gondwanas intruded by a sill complex seen at lower elevations (Sen, 1980), and the three basalt lava flows simply cap it.

"Basalt cliffs" in Fig. 10b should thus read "basalt-capped cliffs". However, this correction does not affect the validity of the main observations and interpretations, including those by Venkatakrishnan (1984, 1987), about planation surfaces and relatively recent uplift.

I conclude with some general remarks. The uplift issue is very relevant to the plume debate; the prediction of prevolcanic regional uplift is unique to the plume hypothesis. Field data are unambiguous and should be utilized. They were not available to the originators of the plume hypothesis nearly forty years ago. The originators and early enthusiasts had little direct experience with particular provinces, resulting in a large number of claims about things that do not exist (e.g., the Cambay triple junction story, see Sheth, 2005). Critical tests of the Deccan plume should have come from regional experts, either from India or abroad. The purported pre-volcanic doming in the Emeishan province has been made much of, e.g., by Campbell (2005) and Campbell and Davies (2006), who even confidently cite such doming in the Deccan as fact. Even the claimed Emeishan case is suspect (see comments by myself and Hamilton on the chapter by Xu et al., this volume).

Finally, to call a plume viable, one should appeal to evidence, not uncertainties. To say that plumes are possible because there are too many unknowns is not a logical approach. However, that it is used by many plume theoreticians is clear from several chapters in this volume and the discussion following the chapter by Garnero et al. (this volume). Dark matter, baby universes, superstrings and eleven dimensions are also all theoretically possible, without physical evidence, and some would like to have them. Plumes are similar. A plume proponent a couple years ago compared the plume to an electron, by nature invisible but beyond doubt. The more meaningful questions to me are: Do plumes have to exist, is there *evidence* for them, and can known, well-understood, non-plume upper mantle processes explain the observations instead?

I thank Editor Gillian Foulger for ably coordinating this Discussion, and for the opportunity to clarify my objectives and position.

References

Alpha, T.R., and Vallier, T.L., 1994, Physiography of the Seven Devils Mountains and Adjacent Hells Canyon of the Snake River, Idaho and Oregon: USGS Professional Paper, v. 1439, p. 91-100.

Anderson, D. L., 2005, Scoring hotspots: The plume and plate paradigms, in Foulger, G. R., Natland, J. H., Presnall, D. C., and Anderson, D. L., eds., Plates, plumes, and paradigms: Boulder, Colorado, Geological Society of America Special Paper 388, p. 31-54.

Anderson, D. L., and Natland, J. H., 2005, A brief history of the plume hypothesis and its competitors: Concept and controversy, in Foulger, G. R., Natland, J. H., Presnall, D. C., and Anderson, D. L., eds., Plates, plumes, and paradigms: Boulder, Colorado, Geological Society of America Special Paper 388, p. 119-145.

Bondre, N. R., Hart, W. K., and Sheth, H. C., 2006, Geology and geochemistry of the Sangamner mafic dike swarm, western Deccan volcanic province, India: Implications for regional stratigraphy: Journal of Geology, v. 114, p. 155-170.

Bonow, J. M., Lidmar-Bergström, K., and Japsen, P., 2006, Palaeosurfaces in central West Greenland as reference for identification of tectonic movements and estimation of erosion: Global and Planetary Change, v. 50, p. 161-183.

Borger, H., and Widdowson, M., 2001. Indian laterites, and lateritious residues of southern Germany: A petrographic, mineralogical, and geochemical comparison: Zeitschrift fur Geomorphologie N. F., v. 45, p.177-200.

Büdel, J., 1982, Climatic Geomorphology (transl. L. Fischer & D. Busche): Princeton University Press, Princeton.
Burbank, D.W., Leland, J., Fielding, E., Anderson, R.S., Brozovic, N., Reid, M.R., and Duncan, C., 1996, Bedrock incision, rock uplift and threshold hillslopes in the northwestern Himalayas: Nature, v. 379, p. 505-510.
Burov, E. and Guillou-Frottier, L., 2005, The plume head–continental lithosphere interaction using a tectonically realistic formulation for the lithosphere: Geophysical Journal International, v.161 (2), p. 469-490.
Campbell, I. H., 2005, Large igneous provinces and the mantle plume hypothesis: Elements, v. 1, p. 265-269.
Campbell, I. H., and G. F. Davies, 2006, Do mantle plumes exist?: Episodes, v. 29, p. 162-168.
Courtillot, V., Davaille, A., Besse, J., and Stock, J., 2003, Three distinct types of hotspots in the Earth's mantle: Earth and Planetary Science Letters, v. 205, p. 295 – 308.
Courtillot V., Jaupart C., Manighetti I., Tapponnier P. and Besse J., 1999, On causal links between flood basalts and continental breakup: Earth and Planetary Science Letters, v.166, p.177-195.
Cox, K.G., 1978, Flood basalts and the breakup of Gondwanaland. Nature, v. 274, p. 47-49.
Cox, K.G., 1983, The Deccan Traps and the Karoo: Stratigraphic implications of possible hot-spot origins: IAVCEI, programme and abstracts of XVIII General Assembly, Hamburg, Germany, 96.
Cox, K.G., 1993, Continental magmatic underplating, Philosophical Transactions of the Royal Society of London (A), 342, 155-166.
Cox, K.G., 1980, A model for flood basalt volcanism: Journal of Petrology, v. 21, p. 629 – 650.
Cox, K.G., 1989, The role of mantle plumes in the development of continental drainage patterns: Nature, v. 342, p. 873-877.
Dadson, S.J., Hovius, N., Chen, H., Dade, B., Hsieh, M.-L., Willett, S.D., Hu, J.-C., Horng, M.-J., Chen, M.-C., Stark, C.p., Lague, D., and Lin, J.-C., 2003, Links between erosion, runoff variability and seismicity in the Taiwan orogen: Nature, v. 426, p. 648-651.
Devey, C.W., and Stephens, W.E., 1991, Tholeiitic dikes in the Seychelles and the original extent of the Deccan: Journal of the Geological Society of London, v. 148, p. 979--983.
Doglioni, C., Carminati, E., and Bonatti, E., 2003, Rift asymmetry and continental uplift: Tectonics, v. 22, doi:10.1029/2002TC001459.
Elkins-Tanton, L.T., 2005, Continental magmatism caused by lithospheric delamination, in Foulger, G.R., Natland, J.H., Presnall, D.C., and Anderson, D.L., eds., Melting anomalies: Their Nature and Origins, p. in prep.
Ernst, R.E., and Buchan, K.L., 2001, Large mafic events through time and links to mantle-plume heads: in Ernst, R.E., and Buchan, K.L., eds., Mantle Plumes: Their Identification Through Time: Boulder Colorado, Geological Society of America Special Paper, 352, p. 483-575.
Gilbert, G.K., 1877, Geology of the Henry Mountains (Utah): Washington D.C., United States Government Printing Office, 172 p.
Goles, G.G., 1986, Miocene Basalts of the Blue Mountains Province in Oregon. I: Compositional Types and their Geological Settings: Journal of Petrology, v. 27, p. 495-520.
Gunnell, Y., and Radhakrishna, B. P., eds., 2001, Sahyadri: The great escarpment of the Indian subcontinent: Bangalore, Geological Society of India Memoir 47 (1-2), 1054 p.
Gunnell Y., and Fleitout L., 1998, Shoulder uplift of the Western Ghats passive margin, India: a denudational model: Earth Surface Processes and Landforms, v. 23, p. 391-404.
Gunnell, Y., Gallagher, K., Carter, A., Widdowson, M. and Hurford, A.J., 2003, Denudation history of the continental margin of western peninsular India since the early Mesozoic – reconciling apatite fission track data with geomorphology: Earth and Planetary Science Letters, v. 215, p. 187 - 201.
Hales, T.C., Abt, D.L., Humphreys, E.D., and Roering, J.J., 2005, Lithospheric instability as an Origin for Columbia River Flood Basalts and Wallowa Mountains Uplift in NE Oregon, USA: Nature, v. 432, p. 842-845.
Halkett, A., White, N., Chandra, K. and Lal, N.K., 2001, Dynamic uplift of the Indian Peninsula and the Réunion Plume: AGU, Fall Meeting, Abstract #T11A-0845.
Hon, K., Kauahikaua, J., Denlinger, R.P., and Mackay, K., 1994, Emplacement and inflation of pahoehoe sheet flows: Observations and measurements of active lava flows on Kilauea Volcano, Hawaii: Geological Society of America Bulletin, v. 106, p. 351-370.
Hooper, P. R., 1990, The timing of crustal extension and the eruption of continental flood basalts: Nature, v. 349, p. 246-249.
Hooper, P.R., 1997, The Columbia River Flood Basalt Province: Current Status: Geophysical Monograph, v. 100, p. 1-27.
Hooper, P.R., and Camp, V.E., 1981, Deformation of the southeast part of the Columbia Plateau: Geology, v. 9, p. 323-328.

Hooper, P. R., 1990, The timing of crustal extension and the eruption of continental flood basalts: Nature, v. 345, p. 246-249.
Jerram, D.W., and Widdowson, M., 2005, The anatomy of Continental Flood Basalt Provinces: geological constraints on the processes and products of flood volcanism: Lithos, v. 79, p. 385 - 405.
Kennett, B. L. N., and Widiyantoro, S., 1999, A low seismic wavespeed anomaly beneath northwestern India: a seismic signature of the Deccan plume?: Earth and Planetary Science Letters, v. 165, p. 145-155.
King, S. D., and Anderson, D. L., 1995, An alternative mechanism of flood basalt formation: Earth and Planetary Science Letters, v. 136, p. 269-279.
King, S. D., and Anderson, D. L., 1998, Edge-driven convection: Earth and Planetary Science Letters, v. 160, p. 289-296.
Mahoney, J. J., Sheth, H. C., Chandrasekharam, D., and Peng, Z. X., 2000, Geochemistry of flood basalts of the Toranmal section, northern Deccan Traps, India: Implications for regional Deccan stratigraphy: Journal of Petrology, v. 41, p. 1099-1120.
McKenzie, D.P.,1978, Some remarks on the development of sedimentary basins: Earth and Planetary Science Letters, v. 40, p. 25 - 32
Mitchell, C., and Widdowson, M., 1991, A geological map of the Southern Deccan Traps, India and its structural implications: Journal of the Geological Society of London, v. 148, p. 495-505.
Ollier, C. D., and Pain, C. F., 2000, The origin of mountains: London, Routledge, 368 p.
Peng, Z. X., Mahoney, J. J., Hooper, P. R., Macdougall, J. D., and Krishnamurthy, P., 1998, Basalts of the northeastern Deccan Traps, India: Isotopic and elemental geochemistry and relation to southwestern Deccan stratigraphy: Journal of Geophysical Research, v. 103, p. 29843-29865.
Powell, J.W., 1875, Exploration of the Colorado river of the West, and its tributaries.: Washington, Government Print Office.
Ray, R., Sheth, H. C., Mallik, J., 2007, Structure and emplacement of the Nandurbar-Dhule dike swarm, Deccan Traps, and the tectonomagmatic evolution of flood basalts. Bulletin of Volcanology, in press.
Samant, B., and Mohabey, D.M., 2005, Response of flora to Deccan volcanism: A case study from Nand – Dongargaon basin of Maharashtra, implications to environment and climate: Gondwana Geological Magazine, Spl. v. 8, p. 151-164.
Self, S., Thordarson, T., and Keszthelyi, L., 1997, Emplacement of Continenetal Flood Basalt Lava Flows: Geophysical Monograph, v. 100, p. 381-409.
Sen, G., 1980, Mineralogical variations in the Delakhari sill, Deccan Trap intrusion, central India: Contributions to Mineralogy and Petrology, v. 75, p. 71-78.
Spencer, J.E., and Pearthree, P.A., 2003, Headward erosion versus closed-basin spillover as alternative causes of Neogene capture of the ancestral Colorado River by the Gulf of California, in Young, R.A., and Spamer, E.E., eds., Colorado River: Origin and Evolution: Grand Canyon, AZ, Grand Canyon Association, p. 215-219.
Sheth, H.C., 1999, Flood basalts and large igneous provinces from deep mantle plumes:fact, fiction and fallacy: Tectonophysics, v. 311, p. 1-29.
Sheth, H. C., 2000, The timing of crustal extension, dyking, and the eruption of the Deccan flood basalts: International Geology Review, v. 42, p. 1007-1016.
Sheth, H. C., 2005., From Deccan to Reunion: No trace of a mantle plume: in Plates, Plumes and Paradigms, edited by G. R: Foulger et al., Geological Society of America Special Paper 388, p. 477-501.
Sheth, H.C., Pande, K., and Bhutani, R., 2001a, 40Ar-39Ar Ages of Bombay trachytes: evidence for a Palaeocene phase of Deccan volcanism: Geophysical Research Letters, v. 28, p. 3513-3516.
Sheth, H. C., Pande, K. and Bhutani, R., 2001b, 40Ar-39Ar age of a national geological monument: The Gilbert Hill basalt, Deccan Traps, Bombay: Current Science, v. 80, p. 1437-1440.
Sheth, H. C., Mahoney, J. J., and Chandrasekharam, D., 2004. Geochemical stratigraphy of flood basalts of the Bijasan Ghat section, Satpura Range, India. Journal of Asian Earth Sciences, v. 23, p. 127-139.
Tandon, S.K., 2002, Records of the influence of Deccan volcanism on contemporary sedimentary environments in central India: Sedimentary Geology, v. 147, p. 177-192.
Todal, A. and Eldholm, O., 1998. Continental margin of western India and Deccan large igneous province: Marine Geophysical Research, v. 20, p. 273-291.
Tolan, T.L., Reidel, S.P., Beeson, M.V., Anderson, J.L., Fecht, K.R., and Swanson, F.J., 1989, Revisions to the estimates of the areal extent and volume of the Columbia River Basalt Group: Geological Society of America Special Paper, v. 239, p. 1-20.

Vanderkluysen, L., Mahoney, J. J., Hooper, P. R., and Sheth, H. C., 2006, Location and geometry of the Deccan Traps feeder system inferred from dike geochemistry: Eos Trans. AGU 87 (52), Fall Mtg. Suppl., abstract V13B-0681.

Van Tassel, J., Ferns, M.L., McConnell, V., and Smith, G.R., 2001, The mid-Pliocene Imbler fish fossils, Grande Ronde Valley, Union County, Oregon, and the connection between Lake Idaho and the Columbia River: Oregon Geology, v. 63, p. 77-96.

Vandamme, D., and Courtillot, V.,1992, Paleomagnetic constraints on the structure of the Deccan Traps: Physics of the Earth and Planetary Interiors, v. 74, p. 241-261.

Van Wijk, J. W., Huismans, R. S., Ter Voorde, M., and Cloetingh, S., 2001, Melt generation at volcanic continental margins: No need for a mantle plume? Geophysical Research Letters, v. 28, p. 3995-3998.

Whipple, K.X., 2004, Bedrock Rivers and the Geomorphology of Active Orogens: Annual Review of Earth and Plantetary Science, v. 32, p. 151-185.

White, R.S., Spence, D. Fowler, S.R., Mckenzie, D.P., Westbrook, G.K., and Bowen, E.N., 1987, Magmatism at rifted continental margins: Nature, v. 330, p. 439-444.

White, R.S. 1993, Melt production rates in mantle plumes: Philosophical Transactions of the Royal Society of London (A), v. 342, p. 137-153

White, R.S., and McKenzie, D.P., 1989, Magmatism at rift zones: the generation of volcanic continental margins and flood basalts: Journal of Geophysical Research, v. 94, p. 7685 – 7729.

Whitehouse, I.E., 1988, Geomorphology of the central Southern Alps, New Zealand: the interaction of plate collision and atmospheric circulation: Zeitschrift fur Geomorphologie Supplementeband, v. 69, p. 105-116.

Whiting, B.M., Karner, G.D. and Driscoll, N.W., 1994, Flexural and stratigraphic development of the West Indian continental margin: Journal of Geophysical Research, v. 99, p. 13 791 - 13 811.

Widdowson, M., Pringle, M. S., and Fernandez, O. A., 2000, A post K-T boundary (Early Palaeocene) age for Deccan-type feeder dikes, Goa, India. Journal of Petrology, v. 41, p. 1177-1194.

Widdowson, M., 1997, Tertiary palaeosurfaces of the SW Deccan, western India: Implications for passive margin uplift, in Widdowson, M., ed., Palaeosurfaces: Recognition, reconstruction, and palaeoenvironmental interpretation: London, Geological Society Special Publication, v. 120, p. 221-248.

Widdowson, M., 2005, The Deccan basalt – basement contact: Evidence for a plume-head generated CFBP? American Geophysical Union Chapman Conference “The Great Plume Debate”, Scotland, p. 69-70 (abstract).

Widdowson, M., and Cox, K. G., 1996, Uplift and erosional history of the Deccan Traps, India: Evidence from laterites and drainage patterns of the Western Ghats and Konkan coast: Earth and Planetary Science Letters, v. 137, p. 57-69.

Widdowson, M., and Mitchell, C., 1999, Large-scale stratigraphical, structural and geomorphological constraints for earthquakes in the southern Deccan Traps, India: The case for denudationally-driven seismicity. In: Subbarao K.V. (ed.) The Deccan Province: Geological Society of India Memoir v. 43(1), p. 219-232.

Venkatakrishnan, R., 1984, Parallel scarp retreat and drainage evolution, Pachmarhi area, Madhya Pradesh, central India: Journal of Geological Society of India, v. 25, p. 401-413.

Venkatakrishnan, R., 1987, Correlation of cave levels and planation surfaces in the Pachmarhi area, Madhya Pradesh: A case for base level control: Journal of Geological Society of India, v. 29, p. 240-249.