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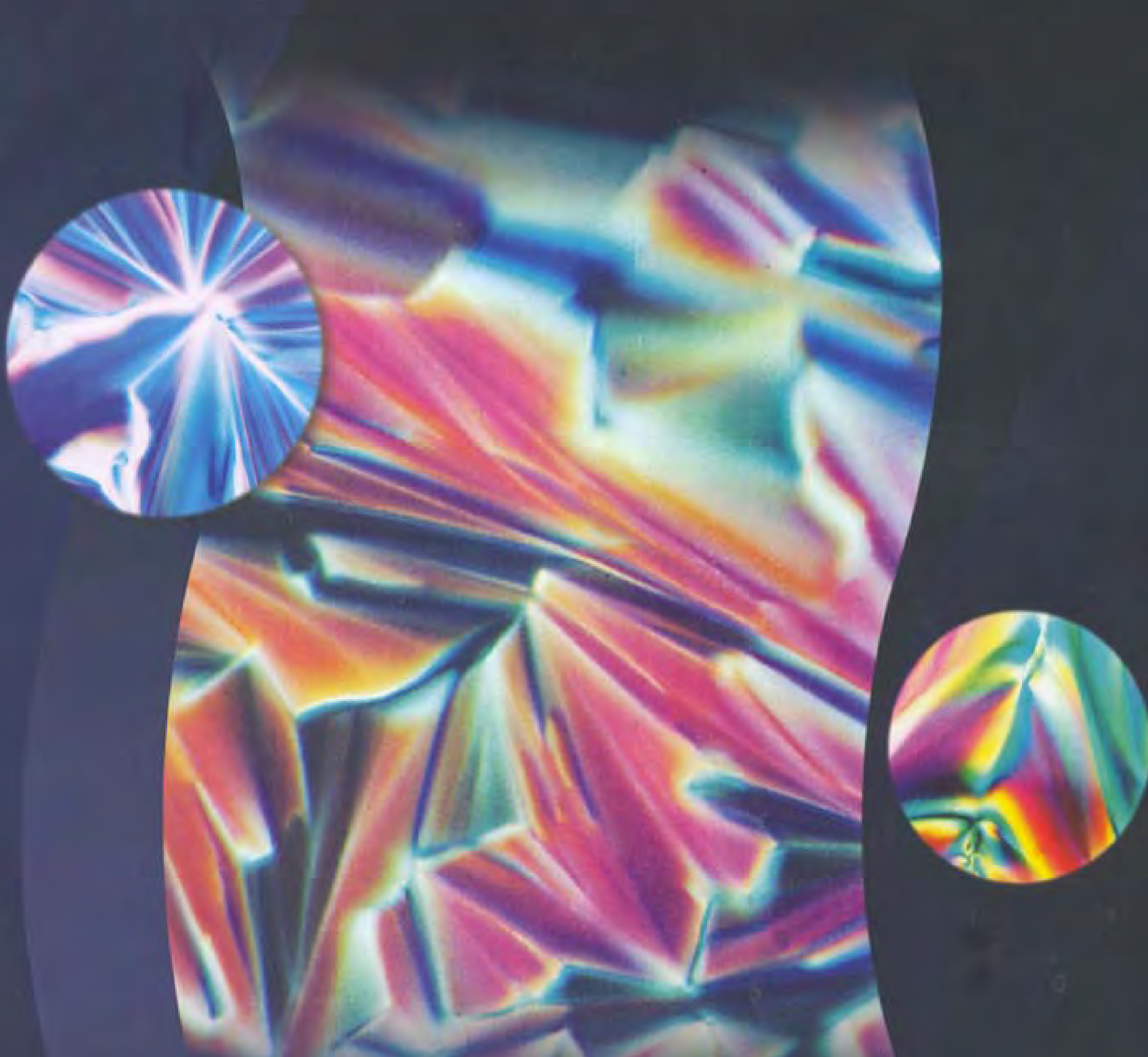

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On the great plume debate

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1 Introductory note

Geological processes are ultimately consequences of Earth's thermal evolution. Plate tectonic theory, which explains geological phenomena along plate boundaries, elegantly illustrates this concept. For example, the origin of oceanic plates at ocean ridges, the movement and growth of these plates, and their ultimate consumption back into the Earth's deep interior through subduction zones provide an efficient mechanism to cool the earth's mantle, leading to large-scale mantle convection. Mantle plumes, which explain another set of global geological phenomena such as within-plate volcanism, cool the earth's deep interior (probably the Earth's core) and represent another mode of Earth's thermal convection. Plate tectonic theory and mantle plume hypothesis thus complement each other to explain much of the whole picture of Earth processes and phenomena.

The above statements represent the mainstream view today on how the Earth works by the majority of Earth scientists, many of whom also recognize that neither plate tectonic theory nor mantle plume hypothesis is perfect, but requires continued developments. However, the mantle plume hypothesis has received great challenges in recent years 'not without justification', and the 'Mantle Plume Debate' is currently rather heated. This controversy is perhaps one of the greatest in the history of solid Earth Sciences. For this reason, and to properly inform the community exactly what this controversy is about, I invited prominent scientists from both schools to express their views on whether mantle plumes exist or not, whether they exist naturally as a result of Earth's cooling or whether their existence is purely required for convenience by scientific interpretations of certain Earth phenomena. Don Anderson[1] basically argued that mantle plumes do not exist in the Earth. In this current issue, Geoff Davies, a strong mantle plume advocate, provides *A case for mantle plumes*[2], whereas Gillian Foulger, a strong sceptic of mantle plumes, explains *Why there is current scepticism of mantle plume hypothesis*[3]. In the following, I briefly introduce these two authors and then provide some background information and basic concepts that may help readers better understand the debate in general and the two papers in particular. I do not wish to make specific comments on any argument for or against mantle plume hypothesis in the two invited papers.

2 About the authors

Geoff F. Davies is currently a Senior Research Fellow of Geophysical Fluid Dynamics at Research School of Earth Sciences, The Australian National University. He received a B.Sc. with honours (1966) and an M.Sc. (1968) from Monash University in Australia, and a Ph.D. from California Institute of Technology (Caltech) in the USA (1973). His PhD thesis dealt with mineral physics titled 'Elasticity of solids at high temperatures and pressures: Theory, measurement and geophysical application'. He held positions at Harvard University, University of Rochester and Washington University in the USA before he took his present post in 1983. He is an expert on mineral physics, very knowledgeable on geology and geochemistry with deep interest in dynamics and evolution of the earth's mantle: plate tectonics, mantle convection and chemical evolution. He is also interested in and researches on crust-mantle interaction, early Earth process and other planets. In the past years, Geoff focuses his research on modelling mantle convetion and its physical and geochemical consequences. He was elected as a Fellow of the American Geophysical Union in 1992 and awarded Augustus Love Medal of the European Geosciences Union as a distinguished scientist in the field of geodynamics in 2005. He published over 100 research papers in leading international journals and a very readable text *Dynamic Earth: Plates, Plumes and Mantle Convection* (Cambridge University Press, 1999; 458pp). Interested readers may view his personal research website where his dynamic models on Earth convection as movies can be found (http://rses.anu.edu.au/gfd/members/davies/index. html).

Gillian R. Foulger is currently a professor in Geophysics at Durham University, UK. She received a B.A (1974) and M.A. (1978) from Cambridge University and an M.Sc. (1976) and Ph.D. (1985) from Durham University. She has held positions at University of Iceland, US Geological Survey and Durham University. Her expertise lies in seismology, using GPS to study deformation at plate boundary zones and researches into earthquakes and hydrothermal activities. Her 'life time' observations and quantitative geophysical modelling on Iceland 'hotspots' encouraged her to raise doubt on 'Iceland hotspots' as a surface expression of deep-rooted mantle plumes. She made important contributions in all these areas and in particular her demonstration of the fundamental importance of non-shear earthquakes that were at odds with popular mechanisms that earthquakes were caused exclusively by shear on faults. This led to classic papers in Nature and other prestigious journals. She is a member of a number of learned societies, was elected as a Fellow of the Royal Astronomical Society, and was recently awarded the Price Medal by the Royal Astronomical Society for her leadership towards a major rethinking of the widely held view that hotspots, regions of long-lived excess volcanism such as Iceland, Hawaii, or Yellowstone, result from plumes of hot material upwelling from great depth in the mantle. Gillian has taken the leadership in a multidisciplinary reassessment of these issues via many publications, summarized in one of her recent papers (Foulger, G. R., Plumes, or plate tectonic processes? Astronomy & Geophysics, 2002, 43: 619—623) and her website www.mantleplumes. org, which provides an unprecedented forum to discuss fundamental earth problems. She published over 100 research papers in leading international journals, and continues to do so and to inspire young scientists to develop independent way of thinking rather than follow the bandwagon.

3 Mantle plume hypothesis: Development, limits and alternatives

The advent of plate tectonic theory almost 40 years ago has revolutionized Earth Science thinking, and provided a solid framework for understanding how the Earth works. By definition, plates are rigid lithospheric blocks that do not deform internally, but move with respect to each other. Hence, the plate tectonic theory explains with simple clarity the occurrences and distributions of earthquakes and volcanic activities along plate boundaries, but fails to explain earthquakes and volcanism occurring within plate interiors. The Hawaii-Emperor seamount chain with age-progression northwestward from Hawaii on the vast Pacific plate is the most prominent within-plate feature on the Earth that cannot be explained by the plate tectonics. Parallel to the development of the Plate tectonic theory[4−7], Wilson[4,5] interpreted within-plate volcanic centres such as Hawaii as 'hotspots' derived from a relatively fixed source in the mantle deeper than, and thus unaffected by, the moving Pacific plate. Morgan[8, 9] advocated further that hotspots were surface expressions of cylindrical plumes derived from the lower mantle, and identified about 20 such hotspots or mantle plumes on the Earth[10]. Thermal plumes may indeed be existing in, and required by, the Earth to cool its deep interior, but the number of plumes must be limited as Davies argues. This contrasts with the suggestion of about 5200 plumes based on analysis and models of heat flow data[11]. The number of plumes has also been raised by numerous plume enthusiasts because of both explicit and implicit reasoning that volcanism that cannot be explained by plate tectonics must be caused by mantle plumes as long as the geochemistry of the volcanism appears to be more enriched in the so-called incompatible elements (e.g., light rare earth elements, volatiles, and large ion lithophile elements) regardless of whether there exists sufficient physical evidence or not in support of plume origin. Foulger counters that substantial evidence for the existence of mantle plumes is lacking, and many cases of the within-plate volcanism do not fit, and cannot be predicted by, the plume hypothesis[8,9,13−15]. There is thus at least a consensus that many of the co-called plumes are not plumes. If not, alternatives are needed to explain these non-plume within-plate phenomena.

3.1 Alternatives and cautions

Davies recognizes the potential significance of 'compositional plumes'[12]. Foulger[3] suggests many alternatives that may explain within-plate volcanic activities without invoking any plume. These alternatives, including 'edge convection', 'melt focusing', 'large scale melt ponding', 'continental lithosphere delamination', 'slab break off', 'rifting' and 'meteorite impact', are in fact familiar terms and concepts that have existed in the literature for some time. The need of these multiple alternatives means that no unified theory yet exists to explain within-plate volcanism. Ideally, all these proposed mechanisms must be testable, but in the event that they cannot yet be tested, we need to analyze their likelihood in terms of available observations, basic physics and logical reasoning. Such analysis should become more productive if we question persistently *why's* and *how's* in anatomizing these alternatives. The latter helps avoid indiscriminately accepting ideas without understanding their validity.

Overall, many aspects of the controversy are well spelled out in the two articles by Davies and Foulger. However, it remains unclear whether the *plume* under debate means the same thing to both schools, to all the plume enthusiasts and to all the plume sceptics. The proposal of 'compositional plumes' and that plumes may 'come from almost any depth', for example, indicate that the plumes referred to here differ from the thermal mantle plumes that originate from a hot thermal boundary layer (TBL)[13−15]. Therefore, the debate would not be fruitful if the debaters innocently compare *apples* with *oranges* or treat different parts of an elephant as the whole of the elephant. The terminologies such as 'hot plumes', 'cold plumes', 'high $^3He/^4He$ plumes', 'low $^3He/^4He$ plumes', 'no head plumes', 'no tail plumes', 'deep plumes', 'shallow plumes', 'small plumes', 'super plumes', etc. that frequently appear in the literature may indeed reflect the diversity of plumes. However, we should be aware of whether our *plume* in mind is comparable to other plumes under debate. Otherwise, the 'Great Plume Debate' would become fruitless. Therefore, a clarification on relevant concepts concerning plume hypothesis is needed.

3.2 Definitions and concepts relevant to Great Plume Debate

Fig. 1 illustrates some key elements of the plume hypothesis. On the left (Fig. 1(a)) is the probable scenario of whole-mantle convection and on the right (Fig. 1(b)) is the scenario of layered mantle convection. The vertical axis shows the depth of the Earth from the surface to the centre of the Core. The horizontal axis approximates qualitatively the temperature increase to the right.

(1) Scenario of whole-mantle convection. Strictly speaking, there is no direct evidence for or against whole-mantle versus layered mantle convection. If the 660 km seismic discontinuity (660-D), which results from a pres-

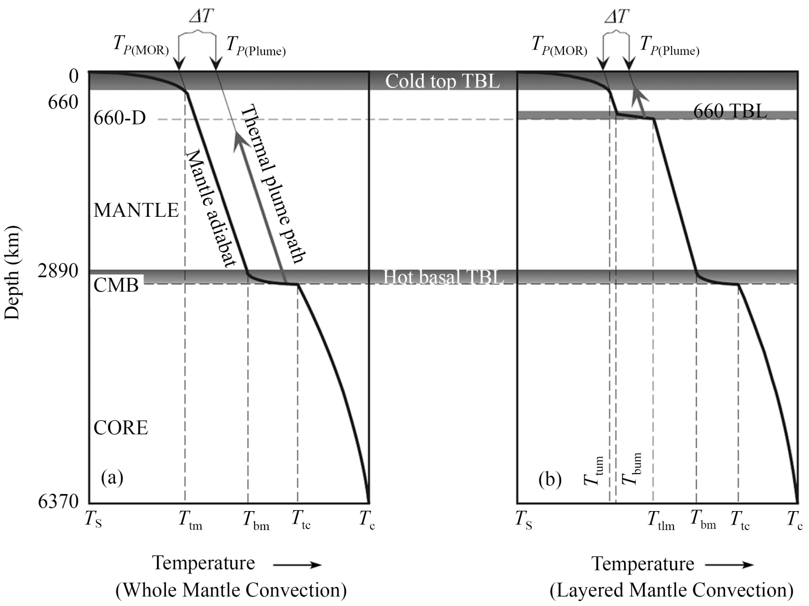

Fig. 1. Cartoon illustrating mantle thermal profile (thick solid line) and derivation of thermal mantle plumes (grey arrowed lines) from a deep hot thermal boundary layer (TBL), which is at the D″ region or core-mantle boundary (CMB) if the mantle convects on a whole-mantle scale (a), but which is at the 660 km seismic discontinuity (660-D) if the upper mantle convects independently from the lower mantle (b). The temperature labels along the horizontal axis stands for the following: earth's surface (s), top of the mantle (tm), bottom of the mantle (bm), top of the core (tc), centre of the earth (c), top of the upper mantle (tup), bottom of the upper mantle (bum), and top of the lower mantle (tlm). $T_{P(Pluime)}$ stands for mantle potential temperature for thermal mantle plumes, and $T_{P(MOR)}$ for normal mantle such as beneath ocean ridges. See text for details.

sure dependent phase change, does not act as a physical barrier to prevent material flow across the discontinuity, then mantle convection must be on the whole-mantle scale as material from the upper mantle can flow to the lower mantle and to balance the mass, material from the lower mantle must flow into the upper mantle. Penetration of the oceanic lithosphere across the 660-D into the lower mantle in many subduction zones as reflected by the seismic tomography[16,17] favours whole-mantle convection. The massive material exchange across the 660-D means effective heat exchange across the 660-D. As a result, the lower mantle would be cooled effectively, and no excess heat can be accumulated in the lower mantle. The straightforward consequence is that the 660-D is not a conductive TBL. Hence, in Fig. 1(a), the mantle has only two TBLs with steep thermal gradient as indicated by the shape of the mantle temperature profile (thick solid line). The top cold TBL results from conductive cooling of the mantle to the surface, and the bottom hot TBL results from conductive heating by the core. The temperature profile in the main portion of the mantle between the two TBLs has been generally accepted as representing the mantle adiabat (or adiabatic thermal gradient). That is, the temperature change along the adiabat is not due to heat loss or gain, but results from the pressure (depth) change: compression (downward) or expansion (upward) under constant entropy. In reality, the temperature profile in this main portion of the mantle may not strictly be adiabatic because of the likely internal radiogenic heating, but treating it as an adiabat is a reasonable approximation for the purpose here. Fig. 1(a) thus says that if mantle upwelling takes the path of adiabatic thermal gradient and if the mantle plume source materials are necessarily hotter (potential temperature of plumes: $T_{P\ (Plume)}$) than the 'normal' ambient mantle such as beneath ocean ridges (i.e., $T_{P\ (MOR)}$), then thermal mantle plumes (thick gray arrowed line sub- parallel to the mantle adiabat at a higher temperature) must come from the bottom hot TBL, i.e., the D″ region or the Core mantle boundary (CMB). This is simply because materials that originate from anywhere else in the mantle away from the TBLs will rise along the adiabat (thick solid line) and will not be hotter than the normal ambient mantle (i.e., $T_{P\ (MOR)}$). Hence, we can say that if thermal plumes do exist in the Earth, they must come from a hot TBL. If so, the inferred $T_{P\ (Plume)}$ from hotspots volcanism must be higher than $T_{P\ (MOR)}$.

On the other hand, if a parcel of material from main portion of the mantle away from the hot TBL is compositionally buoyant with abundant water (thus inferred to have elevated abundances of other incompatible elements), and is large enough in size (Stokes law), it will rise along the path defined by the adiabat (thick solid line), and will partially melt at shallow levels, giving rise to the surface volcanism. If this occurs in the interior of a plate, one may invoke mantle plumes to explain its origin. However, this is conceptually not the same as thermal plumes. If the jury decides to use the word plume, then such within-plate melting anomalies would be products of compositional (vs. thermal) plumes. Because it is not derived from the hot TBL, its potential temperature would be similar to $T_{P\ (MOR)}$, but cooler than $T_{P\ (Plume)}$. This may help resolve the dispute on $T_{P\ (MOR)}$ and $T_{P\ (Plume)}$ inferred from erupted volcanic products[18 – 21]. Note that this parcel of compositionally anomalous mantle could be slightly hotter than $T_{P\ (MOR)}$ because of probable excess heat produced by elevated abundances of heat-producing elements, but is still not so hot as $T_{P\ (Plume)}$.

(2) Scenario of layered mantle convection. If the 660-D is not a physical barrier for material exchange between the upper and lower mantle as discussed above, then layered mantle convection would be unlikely. However, other than the tomographic suggestions of oceanic

lithosphere going down to the lower mantle in many cases, there is no direct evidence for 'free' mass exchange between the upper and lower mantle. The tomographic suggestion that the subducted Pacific oceanic lithosphere lies horizontally in the transition zone beneath east Asia[17] extending for > 2000 km far to the west, and that the subducting slabs kink at the transition zone depths in some other cases means that the 660-D may be locally or temporarily a barrier for mass exchange and may thus probably be a TBL. It is also possible that layered convection may have been important in Earth's history[22,23]. Therefore, the scenario in Fig. 1b is worth considering. In this regard, plumes that originate from different TBLs (e.g., 660-D or CMB) may very well explain hotspot volcanism of varying characteristics[24].

Fig. 1(b) says that the 660-D may be a conductive TBL. Such a thermal boundary layer develops because plate tectonics induced cooling is confined to the upper mantle, and heat transfer from the lower mantle to the upper mantle by conduction is rather inefficient and limited. Therefore, a large temperature contrast develops at the 660-D, thus the TBL. Thermal mantle plumes thus can only develop at the 660-TBL. Mantle plumes derived form the 660-TBL would be less hot than mantle plumes developed from the CMB. Compositional plumes may also develop in the upper mantle, but they cannot be thermally hot with respect to $T_{P\,(MOR)}$. Nevertheless, it remains to be tested and quantified whether a thermal plume (or pulses of diapirs) originated within the upper mantle is capable of triggering large-scale anatexis of thickened lithosphere in a short period.

4 Closing note

The 'Mantle Plume Debate' is currently rather heated. This debate is perhaps one of the greatest in the history of solid Earth Sciences. Undoubtedly, scientific debates provide the momentum towards revelation of the truth, in this particular case, whether mantle plums exist or not in the Earth. What is first important in this debate is that we understand and agree on what a mantle plume is and if the plume we debate on means the same thing to all the enthusiastic debaters. Otherwise, the debate will be fruitless. The articles by Geoff Davies and Gillian Foulger provide an excellent starting point for the debate——the very first time in the literature such controversial views are published simultaneously in the same scientific journal. In this debate, we need to keep in mind that thermal mantle plumes, if existing, can indeed explain some of the within-plate volcanism, but the reasoning that within-plate volcanism must all result from thermal mantle plumes is invalid. If within-plate volcanism cannot be explained by thermal plumes, including the possibility that thermal plumes may not exist in the Earth, then alternative mechanisms must be sought. 'Compositional plumes' mentioned by Geoff Davies is certainly one of the alternatives, but it must be explicitly defined what a 'compositional plume' is. Many other alternative mechanisms suggested by Gillian Foulger must also be tested against observations, physical principles and logical reasoning. Indiscriminate acceptance of any of the alternatives does not do our science any favour. Equally, invoking plume models of any kind in the igneous petrogenesis must also pass critical tests with sound geological, petrological, geochemical and geophysical observations.

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A case for mantle plumes

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Abstract **The existence of at least several plumes in the Earth's mantle can be inferred with few assumptions from well-established observations. As well, thermal mantle plumes can be predicted from well-established and quantified fluid dynamics and a plausible assumption about the Earth's early thermal state. Some additional important observations, especially of flood basalts and rift-related magmatism, have been shown to be plausibly consistent with the physical theory. Recent claims to have detected plumes using seismic tomography may comprise the most direct evidence for plumes, but plume tails are likely to be difficult to resolve definitively and the claims need to be well tested. Although significant questions remain about its viability, the plume hypothesis thus seems to be well worth continued investigation. Nevertheless there are many non-plate-related magmatic phenomena whose association with plumes is unclear or unlikely. Compositional buoyancy has recently been shown potentially to substantially complicate the dynamics of plumes, and this may lead to explanations for a wider range of phenomena, including "headless" hotspot tracks, than purely thermal plumes.**

Keywords: Earth's mantle, plumes, convection, hotspots, flood basalts, rift volcanism.

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Since its proposal by Morgan[1,2] the mantle plume hypothesis has consistently been the subject of some level of controversy. Perhaps because a mature physical theory of plumes developed only rather slowly, over about two decades, plumes were invoked perhaps excessively by some enthusiasts, while sceptics complained, not without justification, that plumes were an ill-defined concept that could neither be tested nor well justified. By now some sceptics have become vociferous, even derisive, and the existence of mantle plumes has become the focus of a controversy that is at times rather heated.

The first purpose of this paper is to argue that the plume hypothesis is relevant enough to observations and supporting knowledge to be a fruitful one to pursue further. The second purpose is to review briefly the status of models and observations, noting some important limitations of both, and to point to future developments, some of which are quite exciting.

With regard to the first purpose, the paper points out to enthusiasts and sceptics alike that there is by now a well-based, quantified and predictive theory of mantle plumes, so the plume concept is much less malleable and more testable than it used to be. The paper also reiterates, for sceptics, that the existence of plumes in the Earth's mantle can be inferred fairly directly from well-known observations and a minimum of assumptions, without the benefit of any theory. There are also robust arguments, relying on few assumptions, that thermal plumes are to be expected in the silicate mantles of planets.

None of the points just summarised would constitute "proof" that mantle plumes exist, but proof in the strict sense belongs to the world of logic and mathematics, not to the world of science. I believe it is more productive to view science as developing quantified stories (i.e. hypotheses, theories or models) that provide useful guidance to the behaviour of the world within a certain context. In this sense my judgement is that the plume hypothesis is useful and well worth pursuing further.

With regard to the second purpose of this paper, my judgement is that there are important examples of non-plate-related magmatism that are also unlikely to be related to plumes, at least within the understanding we have had until very recently. It may be that recent developments bring more of these examples plausibly into the fold of plumes. Even so, plumes are not likely to be the explanation for everything that is not related to plates, and few if any have ever argued for this position.

There has always been a range of opinions on the mechanism, viability and observational support for plumes. This paper therefore necessarily presents one person's point of view, and it does not pretend to speak for any fictitious "plume community", much less to defend all of the plausible or implausible assertions that have ever been uttered about or on behalf of plumes. The view presented here is not identical to the one espoused originally by Morgan. Rather, it is one informed by our better knowledge of the mantle and of plume dynamics than was available to Morgan over three decades ago.

In essence, the proposition defended here is that there exist upwellings in the mantle driven by their own buoyancy and having the form either of a large spherical "plume head" roughly 1000 km in diameter or of a narrower "plume tail" 100—200 km in diameter. The arguments will involve dynamics and some of the key observations that I think are sufficient to establish the viability of the hypothesis. Others are better qualified to discuss the many observations and detailed considerations involved in evaluating many of the less clear candidates for plumehood.

1 Plumes

Morgan's plume hypothesis[1,2] was proposed to supersede Wilson's qualitative idea of a hotspot within the mantle under a moving plate. Wilson's idea had developed, in parallel with his formulation of plate tectonics[3,4], as an explanation of the age-progressive volcanism in the Pacific that was first conceived by Darwin and first identified in contiguous islands by Dana (as recounted by Menard[5]). The reality of the age progression of the Hawaiian-Emperor island and seamount chain has by now been very well established quantitatively[6]. Morgan proposed there are about 20 plumes, and others have argued for 40 or more, although the case becomes less clear as weaker candidates are considered.

Morgan also argued at some length, using plate reconstructions, that the plumes were laterally fixed relative to each other. At the time it was still commonly believed that the lower mantle has an extremely high viscosity that keeps it essentially static, so it made sense that relatively fixed plumes should rise from within the lower mantle. Nevertheless the idea of plume fixity is not essential to the plume hypothesis as presented here, and indeed the rationale for fixity soon weakened as the estimated viscosity of the lower mantle was revised down to levels that permitted active convection[7,8].

1.1 How some mantle plumes can be inferred from observations

The existence of a plume in the shallow mantle under Hawaii can be inferred fairly directly from observations. The active volcanism in the Hawaiian volcanic chain is confined to a "spot" only tens of kilometers across (hence the term "volcanic hotspot"). The islands occur on a swell of the sea floor that is about 1 km high and 1000 km across and that extends back along the volcanic chain to the northwest. This swell is known from seismic profiling to be not due to thickened crust[9], and is too broad to be held up by the strength of the lithosphere[10]. The only other likely explanation is that the swell is supported by buoyant material under the plate. Together, the swell and the localised active volcanism therefore suggest the presence of a narrow column of rising, buoyant material under the volcanic center—a plume.

The upwelling is inferred to be narrow (tens to 100 kilometers across) because the volcanism is so spatially restricted. The upwelling is inferred to be driven by buoyancy because buoyancy is required to elevate the broad swell. The width of the swell is plausibly explained by lateral spreading of the buoyant material under the Pacific plate. This is consistent with the inference of narrow upwelling because the volcanism is plausibly due to decompression melting, which will be concentrated within the main upwelling and minimal where the plume material spreads away horizontally under the plate.

The Hawaii-Emperor seamount chain comprises progressively older volcanic constructions extending to the northwest for thousands of kilometers and recording volcanism for a period of at least 75 Ma (the age at the point where the chain intersects the Kamchatka trench)[11]. As Wilson[4] first argued, the steady age progression argues for a source quite deep in the mantle, deep enough to be not involved in the motion of the Pacific plate. The plume is thus inferred to extend to a depth much greater than its own diameter. The longevity of the volcanism also argues for a persisting or self-renewing source, a point we will return to below.

Hawaii is the outstanding case, but the inference can be extended with reasonable confidence to several other examples of volcanic hotspots with associated swells and age-progressive volcanic chains—— Tristan da Cunha (and the Walvis and Rio Grande Ridges), Reunion (and the Chagos-Laccadive Ridge and Mascarene Plateau), Kerguelen (and the Kerguelen Plateau and Ninety-East Ridge), Iceland (and the Iceland ridge connecting to both continental margins)[12,13]. Cape Verde has persistent volcanism and a large swell but little age progression, but this is expected because the velocity of the African Plate is quite small at this locality. The Louisville Ridge has faded to virtually nothing, but its age progression is well established[14]. Other examples become progressively less definitive, but plausible cases can be mounted for between 20 and 40 plumes.

These observations, and inferences from them, are well known. They are recited again here because the simplicity and force of the inferences that can be made from the observations do not seem to be as well and widely appreciated as they might. No theory of plume structure or dynamics has been required in these inferences.

The above list of inferred plumes by no means exhausts the volcanism that cannot be attributed to plate tectonics. Thus there is certainly no implication that plumes can explain everything.

1.2 Why the Hawaiian plume is probably mainly thermal

The buoyancy inferred above for the Hawaiian plume could be of thermal or compositional origin. Two arguments suggest the buoyancy is thermal (or mainly thermal). One is that thermal plumes are expected in planetary mantles, as will be argued below. The second argument derives from the persistence of Hawaiian volcanism for at least 75 Ma, this being the age of the oldest Emperor seamount[11] (the time of origin of the Hawaiian plume is not known). If the buoyancy is of compositional origin, then it is not clear how the buoyant material could be supplied slowly and relatively steadily. The required total volume of material is large. For example, if the plume material had a density anomaly of 30 kg/m^3 then the volume flow rate of the Hawaiian plume would be about 7.5 km^3/a[15]. To sustain this flow rate for 75 Ma would require a source volume equivalent to a sphere of diameter about 1000 km, similar to the volume inferred for plume heads (below). If it occurred as a large reservoir, then its buoyancy would cause it to rise through the mantle, much as a thermal plume head would, because the "Stokes rise velocity" is proportional to the square of the size of the body[16]. The result would be more like a flood basalt eruption than a hotspot track.

Otherwise no mechanism is evident that would steadily replenish the supply of compositionally buoyant material. Subduction would not seem to suffice, because subducted material is likely to be stirred into the mantle[17]. In the absence of a plausible proposal, fluid dynamics argues fairly strongly against a purely compositional plume. On the other hand if the plume is fed by a thermal boundary layer, as will shortly be argued, this persistence is no problem, since the thermal boundary layer itself would be renewed by heat coming from below.

1.3 Quantitative thermal plume models—heads and tails

The form and behaviour of thermal plumes is by now well understood and well quantified in terms of physics and fluid dynamics. The subject is reviewed, for example, by Loper[18] and Davies[15]. Current understanding is as follows (refer to Fig. 1). Buoyant upwellings in the mantle context preferentially occur as columns rather than sheets. A new plume forms as a spherical blob, termed a "head", that must reach a diameter of perhaps 400 km before it has enough buoyancy to detach from the thermal boundary layer that feeds it and rise through the mantle. Hot material may continue to flow up to the head from the thermal boundary layer via a narrower conduit, termed a "tail". The tail is thinner than the head because the hot plume material has a viscosity roughly 100 times lower than ambient mantle. Whereas the plume head has to be big in order to displace high-viscosity ambient mantle as it rises, the material in the tail follows an already-established path through the surrounding mantle, and a narrow conduit is sufficient for the less viscous plume material to accomplish the required flow rates (which are determined by the supply from the thermal boundary layer).

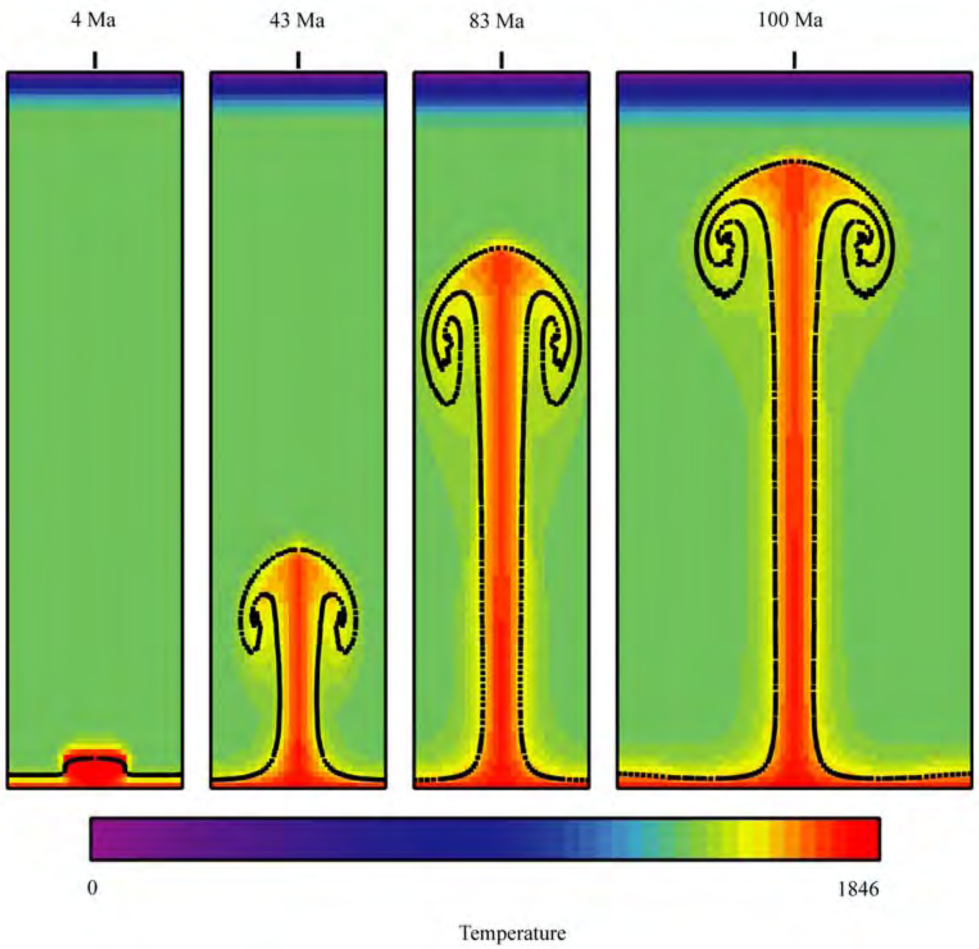

Fig. 1. Growth of a thermal plume from a thermal boundary layer, showing the development of the characteristic "head-and-tail" structure. In this example the ambient mantle viscosity is 10^{22} Pa s; it is independent of depth but a strong function of temperature. A line of tracers delineates material initially within the thermal boundary layer and so reveals the spiral structure that develops in the plume head due to thermal entrainment of ambient mantle. The tail is thinner than the head because it is hotter, has a lower viscosity, and therefore requires only a thin conduit up which to flow. Temperature in ℃. After Ref. [15].

As it rises, the head grows by thermal entrainment, and reaches a diameter robustly estimated to be between 800 and 1200 km. The entrainment occurs because the head heats a thin layer of adjacent ambient mantle, some of which is then buoyant enough to be wrapped into the head and rise with it. This "wrapping" generates a spiral structure within the plume head (Fig. 1) that delineates original from entrained plume material, which may have different compositions reflecting their different original locations in the mantle. This account of plumes is quantified through standard fluid dynamical results and analytical boundary layer theory, confirmed and calibrated by laboratory experiments, and confirmed again by numerical models[19−24].

Recent claims that the plume hypothesis is *ad hoc*, unquantified and untestable[25−29] make no reference to the extensive literature from which this brief summary is drawn, and are plainly contradicted by it.

The basic thermal plume model has been extended to explore, for example, the effects of mantle viscosity stratification[30], background mantle flow[31−34], compositional layering at the source[35], mixing within a plume[36], entrained compositional buoyancy and melting at the top of the mantle[30,37], and investigations continue.

1.4 Why thermal plumes are to be expected in planetary mantles

We should expect thermal plumes in the mantle if there is heat flowing from the core. The latter is virtually unquestioned because the Earth's magnetic field is believed to be generated by the dynamo action of convection in the core, and a flow of heat out of the core is required to maintain core convection. This heat would cross the core-mantle boundary necessarily by conduction, and the conducted heat would form a thermal boundary layer at the base of the mantle. This thermal boundary layer would eventually become unstable and rise, thus driving a form of convection[15]. The physics and material properties of the fluid mantle ensure that such buoyant upwellings take the form of rising columns (rather than sheets), with large spherical "heads" in the lead and thinner "tails" following, as we have just seen. Thus we can predict with some confidence that there ought to be thermal plumes in the mantle.

The above argument applies regardless of how many layers the mantle itself might comprise, because heat must conduct across any persistent density interface, and thus the process of forming a thermal boundary layer that gives rise to buoyant upwellings will occur at every such interface. The argument can be straightforwardly generalised to the silicate mantle of any terrestrial-type planet that begins hot throughout, which is the likely result of accretion and core segregation. The outer mantle layer (or layers) would be the first to cool, being in contact with the cool surface of the planet. As the mantle cooled, heat would begin to flow from the still-hot core into the mantle, and the above argument then applies.

1.5 Anderson's scaling arguments

Anderson[38] has argued to the contrary on the basis of the way some physical properties are claimed to scale with specific volume as pressure or temperature change. The argument is that some properties change in proportion to specific volume whether specific volume changes because of a change of pressure or a change of temperature. The approach was usefully applied by Birch to elastic properties and by Grüneisen to lattice vibrations and related thermal properties. It is worth noting that since temperature induces only small volume changes the conformity of thermally-induced changes to the scaling is not well constrained, nor is it therefore a very accurate guide. This however is not the main problem here. The key claim made by Anderson is that viscosity becomes much less sensitive to temperature under the high pressure of the deep mantle than it is near the surface.

Anderson notes one exception to this scaling, namely radiative heat transfer, but there is another important exception, namely viscosity. Viscosity is the result of a thermally activated process, and its dependence on pressure and temperature therefore takes the form

$$\mu = \mu_0 T \exp\left[\frac{(E^*+PV^*)}{RT}\right],$$

where μ is viscosity, μ_0 is a constant, T is temperature, E^* is the activation energy, P is pressure, V^* is activation volume and R is the gas constant. Ignoring the premultiplying T, which has only a minor effect, this yields

$$\frac{\partial \ln \mu}{\partial \ln T} = -\frac{E^*+PV^*}{RT},$$

which shows that the relative effect of temperature on viscosity increases with pressure, rather than decreasing as Anderson claims. The effect is large. The activation energy is usually estimated to be in the range 200—400 kJ/mol. The activation volume is not well constrained, but a conservative estimate, consistent with Anderson's other scaling, would be 3 cm^3/mol. With a pressure at the base of the mantle of 140 GPa, the term PV^* adds another 400 kJ/mol to the activation energy, giving an activation enthalpy, H^* (= E^*+PV^*), of 600—800 kJ/mol. Using a conservative activation enthalpy of 600 kJ/mol, an increase in temperature from 3000℃ to 3500℃ would decrease the viscosity by a factor of about 250. If anything this is larger than the decrease that plume modellers often use. Thus Anderson's claim regarding viscosity is incorrect: viscosity remains a very strong function of temperature throughout the mantle. Narrow plumes therefore remain plausible.

Anderson is correct that increasing pressure, on its own, increases thermal conductivity and viscosity and decreases thermal expansivity, and that these changes all tend to reduce the vigour of convection in the deep mantle. As

Anderson explains, one manifestation of this is an increase in the size of convective features, and this is actually the reason why plume heads are estimated to be large even in the lower mantle. Note however that even if a buoyant upwelling moves very slowly, it still moves. Eventually it will rise into regions of lower viscosity where its rise will be very rapid because of pent up buoyancy (see later). Anderson avoids this conclusion only by appealing to conjectures about density stratification, which still overlooks the argument made above that plumes are expected regardless of density interfaces.

Anderson's claim that convection is suppressed entirely in the deep mantle is implausible for two other reasons. First, he makes a speculative appeal to radiative transfer to increase the effective thermal conductivity by a factor of eight. More importantly, the increase in viscosity due to pressure implied by his scaling is inconsistent with independent observational constraints. The scaling implies that viscosity varies as specific volume to the power –40 (or –48), or in other words that viscosity varies with density to the power of least 40. Using densities of 3400, 4400 and 5500 kg/m^3 at depths of 50, 700 and 2800 km, respectively, this implies viscosity increases by factors of 3×10^4 and 2×10^8, respectively, relative to the upper mantle. Assuming a relatively low upper mantle viscosity of 3×10^{20} Pa s, Anderson's scaling implies at least 10^{25} Pa s at 700 km and 6×10^{28} Pa s at the bottom of the mantle. However post-glacial rebound constraints and subduction zone geoids permit a viscosity no higher than 10^{23} Pa s at the top of the lower mantle[39−41]. The extreme viscosity predicted for the bottom of the mantle would have implications for Earth's rotational properties, and could also be tested. It seems that laboratory indications of activation volume are inconsistent with observational constraints on mantle viscosity. This issue was discussed by Davies[42] some time ago: part of the resolution may be that activation volume itself decreases under pressure.

1.6 Do all plumes reach the shallow mantle?

It is not infrequently conjectured that although plumes may form, many of them may be swept away by background mantle flow before they reach the top of the mantle. Plume heads are necessarily initially large, as we have seen, and they rise quite fast enough for their dissipation to be unlikely. Once the tail conduit is established it seems to be able to persist even with quite low buoyancy fluxes, as is attested by the fading stages of the Louisville hotspot track. Sufficiently weak plume tails under a fast plate may indeed break up Whitehead[43], and they may not produce observable effects at the surface, but inferred plume buoyancy fluxes range over a factor of 20 so any weaker plumes would contribute little to total heat transport. The rise of plume tails through a background mantle "wind" has by now been studied extensively. Some recent contributions are from Zhong et al.[33], Kerr and Meriaux[34], O'Neill et al.[44]

The claim of many "invisible" plumes has been put most explicitly by Malamud and Turcotte[45]. However their argument is mainly statistical and does not take proper account of relevant fluid dynamics. Plumes originate through a fluid instability, and such instabilities have a characteristic scale. This general principle, along with more specific arguments regarding the growth and detachment of plume heads[22], argue strongly against this possibility. This expectation has been confirmed in the numerical studies of Zhong[46].

1.7 Plume heat flows and core cooling

If plumes originate near the core-mantle boundary, then the heat they carry comes mainly from the core. There are independent estimates of the heat carried by plumes and of the heat budget of the core, so one test of the plume hypothesis is that these quantities should be compatible, within uncertainties.

Davies[47] and Sleep[48] realised independently that hotspot swells can be used to constrain the rate at which buoyancy rises in a plume tail. If hotspot swells are the result of buoyant plume material arriving beneath the lithosphere, then the weight of the resulting topography should balance the buoyancy force of the plume material. The rate of creation of new swell topography then gives the rate of arrival of the plume buoyancy. Buoyancy flux converts directly to heat flux, without the plume temperature being required[15,47]. This approach yields the result that plume tails arriving below the lithosphere carry about 2.3 TW (terawatts, 10^{12} W)[47,48], only about 6% of the Earth's heat budget of 44 TW. An additional contribution would come from the heat carried by new plume heads. Hill et al.[49] argued from the evidence of continental and oceanic flood basalts that new plume heads have arrived on average every 10-20 Ma over the past 250 Ma. This implies they would carry about 1.2 TW of heat, bringing the total heat flow carried by plumes in the upper mantle to about 3.5 TW, still less than 10% of the Earth's rate of heat loss.

This value is unlikely to be increased significantly by inclusion of putative weaker plumes, since it was argued above that large numbers of small plumes are unlikely on fluid dynamical grounds. Nor is the value likely to be increased by taking account of heat loss from plumes as they ascend, because most of the heat that diffuses out of a plume is entrained back into it[24].

However two arguments have been presented recently that could change this estimate. Bunge[1)] has pointed out

1) Bunge, H-P., Effect of mantle non-adiabaticity on excess temperature and fluxes of mantle plumes, Geophys. J. Int., in press.

that since the convecting mantle geotherm away from thermal boundary layers has a sub-adiabatic gradient, the thermal contrast between plumes and ambient mantle will be larger at depth, and this means in turn that the buoyancy flux inferred from hotspot swells underestimates the buoyancy flux of plumes at the base of the mantle. Bunge estimates from numerical models that the non-adiabatic temperature change is between 200 and 300℃, and that the plume heat flux at the base of the mantle could be double, or possibly triple, their flux at the top of the mantle. Bunge's models thus suggest that the heat flux out of the core could be as much as 15—20% of the Earth's heat budget, or 6—9 TW. Related arguments invoke the effects of cold downwellings[46,50], which are the source of the subadiabatic gradient discussed by Bunge. A quite independent possibility is implied by recent modelling of thermal plumes that includes a compositionally dense component[51], namely that some of the dense component, and its heat content, may separate and descend before it reaches the surface.

A minimum core flux can be estimated by combining the adiabatic gradient and thermal conductivity of iron at appropriate pressures, yielding 3.7—5.2 TW[52,53], in plausible agreement with the plume flux. Estimates of the energy required to maintain the core magnetic dynamo involve substantial uncertainty because the spectrum of small-scale and toroidal components is not known. Buffett[54] estimates the energy fed into the dynamo to be of the order of 0.1 TW, possibly as high as 0.5 TW. Because convective energy is converted to dynamo energy with less than 100% efficiency, the required convective core heat flow is larger than this, and Buffett estimates it to be 2—4 TW. These estimates are reasonably consistent with the plume flux estimates, especially if the subadiabatic gradient effect is significant.

Much higher heat flows from the core, up to 15—20 TW, have been assumed in some attempts to reconcile dynamo entropy requirements and inner core growth by assuming radioactive heat production in the core (e.g.[54,55]). However these estimates depend in part on a parameterisation of the heat transferred by the lower mantle thermal boundary layer, and the one used[56] is not necessarily appropriate for a hot boundary layer in a fluid with temperature-dependent viscosity, as it was developed for a cool thermal boundary layer. An arguably more appropriate parameterisation[57,58] yields a relatively low core heat flow throughout Earth history, which may remove the need for core radioactivity and maintain more obvious consistency between core and plume heat flows. This topic requires further investigation.

1.8 Hotspot “fixity”

The relative fixity of hotspots has received a great deal of attention, both because they provided a supposedly “fixed” reference frame against which to measure plate motions and because of attempts to establish whether they are indeed fixed. Neither endeavour took much account of the initial rationale for fixity having disappeared with the lowered estimates of deep mantle viscosity. The hotspots do seem to move more slowly than plates, but for some time there were few attempts to test the upper limit: how fast can the hotspots be moving relative to each other and still be compatible with observational constraints?

By now this question has been addressed more seriously, and it seems that hotspots usually do not move faster than about 2 cm/a relative to each other, whereas relative plate motions are in the range 2—10 cm/a[13]. However some significant episodes of relative motion are now being claimed. Tarduno et al.[59] have argued that the Hawaiian hotspot moved south at a rate of about 4 cm/a during the formation of the Emperor seamounts, 81 to 47 Ma ago, and Tarduno and Gee[60] argued that there was an earlier episode of motion between the Atlantic and Pacific hotspots at 3 cm/a. Relative motions of Indian Ocean hotspots of up to 1 cm/a have also been claimed[44]. On the other hand Gordon[61] has argued that much of this motion is due to true polar wander (all of the hotspots moving relative to the rotation axis), rather than to relative motion between the hotspots.

Detection of relative motion of hotspots would have implications for plate motions and mantle convection, but would not be particularly surprising given recent estimates of deep mantle viscosity[41]. Neither would such results give any reason to question the existence of plumes or the integrity of the physics-based plume hypothesis being defended here[26], since hotspot fixity is not an essential part of this hypothesis.

The relationship between hotspot motion and the horizontal motion of a plume source at depth has been addressed more explicitly in a series of papers by Steinberger and others[31,32], who estimate the trajectory of an ascending plume in the presence of large-scale mantle flow. O'Neill et al.[44] have extended the approach by adding an inversion process, concluding that relative motions of hotspots are discernible for ages greater than 80 Ma. Zhong et al.[33] have presented fully dynamic models of plumes rising under plates and demonstrated the lateral motion of plumes in those models.

1.9 Seismic imaging of mantle plumes

Plumes are likely to be considerably harder to detect than subducted lithospheric slabs, and the latter have been well-resolved by seismic tomography only within the past decade. Factors of geometry, temperature and sign of velocity anomaly conspire to make plumes less visible to seismic tomography. Thus old subducted lithosphere, with a thickness of about 120 km and an average temperature deficit of about 650℃, has an integrated thermal anomaly across its thickness of 8×10^4 km℃. Since the slab is quasi-planar, waves passing obliquely through it will sense a larger integrated anomaly, and all waves in the same vicinity will sense a similar anomaly. On the other hand a plume, with a peak temperature anomaly of only about 250℃ and a diameter of the order of 100 km, will have a smaller integrated thermal anomaly and, because of a plume's cylindrical geometry, only a narrow beam of waves will sense the peak anomaly. Also, because the plume induces a negative velocity anomaly, wavefront healing beyond the plume will reduce the net signature of the plume even more[62].

There seems to be some uncertainty in the seismological community regarding the likely structure of thermal plumes, but the structure of plumes rising through a mantle with a realistic viscosity profile has been computed by, for example, Davies[15], Leitch and Davies[30] and Farnetani[35], the computations by Leitch and Davies being at the highest resolution. Another example is shown in Fig. 2(a), in this case in a mantle with a vertical viscosity structure comparable to that inferred by Mitrovica and Forte[41] from observational constraints. This plume has a heat flow rate of about 150 GW, a little less than the 200 GW estimated for the Hawaiian plume and about double that estimated for the next-strongest plumes[48,63]. The thermal anomaly of this plume, integrated normal to and through its axis, is shown as a function of depth in Fig. 2(b), along with the axial temperature and the effective radius. The axial temperature anomaly is nearly constant at 250℃. The effective diameter is around 240 km in the lower mantle (1300 km height), and the axial thermal anomaly there is 5.6×10^4 km ℃. In the upper mantle the axial thermal anomaly is about 3.2×10^4 km ℃ and the effective diameter is 140 km.

These values are smaller than assumed or implied in some seismological investigations, though they are comparable to some recently claimed anomalies. For example, in considering the seismic detectability of plumes, Ji and Nataf[64] assume an axial temperature anomaly of 600℃ and a Gaussian horizontal profile with a scale radius of 125 km, which implies a thermal anomaly of 1.3×10^5 km ℃. Ritsema and Allen[65] explore the seismic model S20RTS, which in the upper mantle resolves shear velocity variations of about 1% amplitude at a minimum horizontal resolution of 1000 km. Using a temperature derivative of 5×10^{-4} km/s ℃, the minimum resolvable thermal anomaly is at least 10^5 km ℃, several times larger than the expected plume anomaly, so the model cannot be expected to resolve plumes with confidence. On the other hand, Montelli et al.[66] claim plume-like compressional velocity anomalies of around 0.5% in the lower mantle and 1.5% in the upper mantle. The diameters of these anomalies are

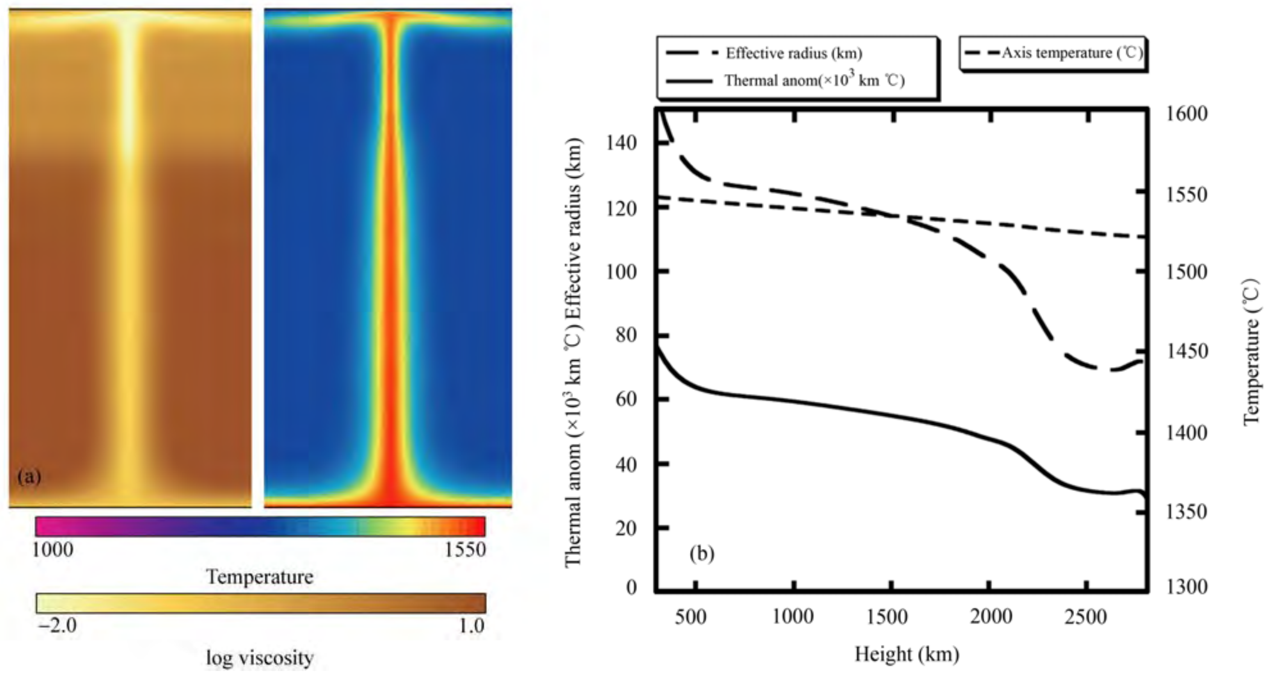

Fig. 2. (a) Thermal and viscosity structure of a plume tail in a more realistic mantle viscosity structure than Fig. 1. Upper mantle viscosity is 3.3×10^{20} Pa s; the viscosity jumps by a factor of 6 at 700 km depth and it also increases exponentially by a factor of 5 through the depth of the mantle, so the viscosity near the base of the mantle is 10^{22} Pa s. Temperature in ℃. (b) Characteristics of the plume in (a) as a function of height in the mantle. The axial temperature is nearly constant. The thermal anomaly, integrated normal to and through the axis, drops from about 5.6×10^4 km ℃ in the lower mantle to about 3.2×10^4 km ℃ in the upper mantle. The effective radius, which is the thermal anomaly divided by the axial temperature, drops from about 120 km in the lower mantle to about 70 km in the upper mantle.

not well resolved, but are said to be around 200 km, within a factor of two. The velocity anomalies corresond to thermal anomalies of roughly 100℃ in the lower mantle and 300℃ in the upper mantle, implying thermal anomalies of roughly $2-6\times10^4$ km ℃, values that have some plausible consistency with the model in Fig. 2, though with considerable uncertainty.

Because plumes are hard to detect, it may be more surprising that there have been any claims of detection than that some expected plumes have not been detected. Nor is controversy amongst seismologists regarding the validity of claimed detections surprising. A conclusion that a plume is not there if it has not yet been detected[25] cannot yet be considered secure.

1.10 Depth of plume source

Courtillot et al.[12] have proposed that there are three distinct types of plumes, and that only seven plumes have an origin in the deepest mantle, the others coming from the mantle transition zone or intermediate depth. They apply five criteria: (1) that there be an age-progressive hotspot track, (2) that there be a flood basalt province, (3) that the plume have a minimum inferred buoyancy flux of at least 10^4 N/s, (4) that the hotspot has a $^3He/^4He$ ratio higher than mid-ocean ridge basalt ratios, and (5) that seismic velocities be low in the nearby region of the mantle. However the relationship between these criteria and source depth is model-dependent and debatable. In particular, there is little reason to suppose a low helium ratio implies shallow depth. Ocean island basalts (of putative plume origin) are distinguished by the large spread of their $^3He/^4He$ ratios, from 3 to over 30 times atmospheric, which are both higher and lower than the much smaller range of 8±1 in mid-ocean ridge basalts[67]. The origin of this heterogeneity is debated, but it is possible that all types of subducted crustal material accumulate in a deep plume source region, along with traces of primitive material[17], so the plume source region could contain all values of the helium ratio and it would therefore not be a useful discriminant of depth of origin.

Neither is there good dynamical reason to believe, at this stage, that a buoyancy flux of 10^4 N/s is the minimum necessary for a plume to ascend from the bottom of the mantle. The references cited by Courtillot et al. use approximations that need to be better tested in fully dynamical numerical models, of the kind presented by Zhong et al.[33].

A potentially more reliable piece of evidence for a plume not extending into the lower mantle is the failure of Montelli et al.[66] to detect a lower mantle extension of the strong anomaly present in the upper mantle under Iceland. If this result persists with improved resolution it could raise a significant question about the Iceland plume, and possibly, though not necessarily, about plumes in general. However initial results from analogous S-wave studies do show evidence of a lower mantle extension of the Iceland plume[68], and anyway the tomographic detection of plumes is in its early stages and its reliability needs to be more thoroughly investigated.

Although it does not reliably discriminate source depth, the compilation by Courtillot et al. provides a useful summary of whether or not a plume can be inferred in any given location. Their remaining criteria (hotspot tracks and flood basalts) will be discussed in the following section.

1.11 Volcanism not obviously related to thermal plumes

Much of the motivation for criticism of the plume hypothesis seems to be drawn from examples of volcanism that do not obviously conform to the predictions of the plume hypothesis, and Clouard and Bonneville[14] and Natland and Winterer[69] provide good summaries of Pacific volcanism that comprise a reasonable case that some volcanism is not related to plumes. However we must be careful not to be too simplistic in dismissing the possibility of a plume source. For example, the lack of a clear age progression in a volcanic ridge or chain (an issue raised also by Foulger[25,26]) must be examined in context: the age progressions of hotspot tracks can be complicated by overprinting of separate episodes, by jumps of adjacent ridge locations (as is documented by Campbell and Davies[1]) for the Ninety East Ridge), by complications engendered by continental crust, and by slow motion combined with large erupted volumes, as in the case of Iceland.

The absence of an identified starting flood basalt province in the type case of Hawaii is not compelling either way, for the simple reason that the hotspot track runs into the Kamchatka subduction zone and the earlier record is obscured. It is plausible that evidence of a Hawaiian flood basalt province resides in the Kamchatka peninsula, but this has not been definitively established because of the region's remoteness. This is obviously an important question to settle.

Even with these cautionary comments, there is a good case that a significant amount of Pacific volcanism does not conform to the classic head-and-tail plume model, particularly by the absence of identifiable head products for many age-progressive tracks[14,69].

Recent models of thermochemical plumes may cause these inferences to be revised substantially. Farnetani and Samuel[51] show plume heads that are more irregular in shape and behaviour than the classic thermal plume heads (Fig. 3). In these examples much of the plume head stalls

1) Campbell, I. H., Davies G. F., Melting rates and efficiencies in mantle plumes, J. Petral., in press.

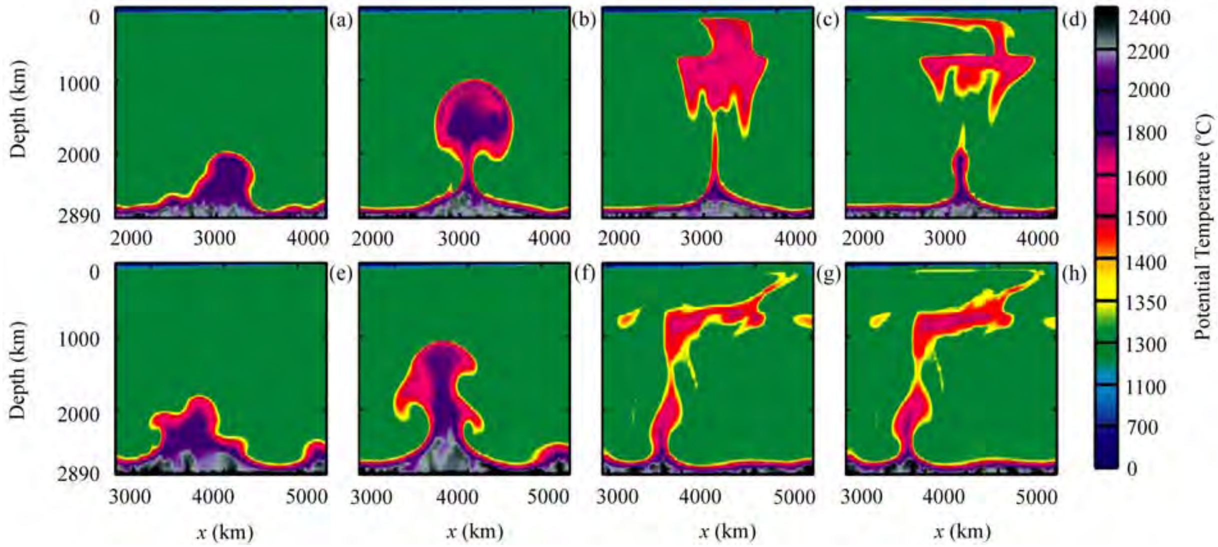

Fig. 3. Thermochemical plumes. Two sequences ((a)-(d), (e)-(h)) of cross-sections through three-dimensional numerical models. The shapes are more irregular than the thermal plume of Fig. 1 and the "tails" are unsteady. Rise of the plumes is interrupted by a phase transformation in the transition zone. Relatively small streamers break through into the upper mantle, which might explain "headless" hotspot tracks. From Ref.[51].

under the transition zone but narrow upwellings break through and rise through the upper mantle. Such models could provide an explanation for "headless" plume tracks[14,69] and a mechanism for small plumes that some have inferred to have arisen within the upper mantle. Lin and van Keken[1)] show that entrainment of compositionally denser material can cause plume tail flows to vary erratically and potentially to yield multiple major eruption episodes. These results take plume models into a new realm with potentially a much richer range of behaviour that may account for a greater proportion of nonplate volcanism.

1.12 Other objections to plumes

In addition to objections already mentioned, Foulger and Natland[25,26] claim that the lack of a large heat flow anomaly near Iceland is evidence against an Iceland plume, but a large heat flow anomaly will only occur if the lithosphere is substantially thinner than is normal for its age. Lithosphere thinning is not a necessary consequence of a plume[70] and no specific evidence is offered by Foulger and Natland.

Anderson[28] claims that estimates of plume temperatures overlap estimates of normal variations of upper mantle temperatures and therefore the plume hypothesis is not necessary. The latter claim is made without any reference to the key arguments and the quantitative plume literature cited above, while Anderson's catalogue of evidence for mantle temperature variations is so wide-ranging and indiscriminate as to be not compelling. Variations in mantle temperature and composition are not incompatible with plumes, and they are not ignored by plume advocates as Anderson asserts, (e.g., [17, 47, 71]). Evaluations of the temperatures of background mantle and of hotspot sources calls for careful work bearing in mind that the petrology of mantle melts is in a state of flux because of the likely heterogeneity of mantle sources and the complexity of magma migration and disequilibrium processes[72−75].

Foulger and Natland[25,26] collect some of the more extreme claims made about plumes, note that there are inconsistencies between them and claim this as evidence that the plume hypothesis is infinitely adaptable and therefore unscientific. However these differences and inconsistencies arise because plumes are vigorously debated, even among plume advocates. On the other hand both Foulger[25] and Anderson[27−29] claim there is no significant debate about plumes, even as they cite the manifestation of such debate as evidence of the feebleness or demise of the plume hypothesis.

In the category of extreme claims is that of Malamud and Turcotte[45] that there are over 5200 plumes, as discussed earlier. In addition to their failure to take account of fluid dynamical constraints, their estimate is based on an alleged partitioning of oceanic heat flow into a cooling part and a background part, the latter being attributed to plumes, but there is little evidence to support this partitioning.

2 Plume heads, flood basalts and rifts

Plumes have been proposed to explain flood basalts as well as hotspot tracks. In addition, their possible role in anomalous magmatism associated with rifting has gained

1) van Keken, P., Multiple volcanic episodes of flood basalts caused by thermochemical mantle plumes, Nature, in press.

plausibility. So-called large igneous provinces come in two main varieties: as elongate formations up to several tens of kilometers thick flanking the edges of some rifts and rifted continental margins, and as broad, more equant basaltic sequences up to several kilometers thick spread across continental or seafloor surfaces[76]. The former are revealed mainly through reflection seismology as "seaward-dipping reflectors"[77] and the latter have long been known as flood basalt provinces.

Morgan[78] extended his original hypothesis, which concerned what we now call plume tails, by noting that the head-and-tail plume structure demonstrated by Whitehead and Luther[19] might explain the fact that several hotspot tracks seem to emerge from flood basalt provinces. He proposed that flood basalts are caused by the arrival of a plume head below the lithosphere (assumed in general to be moving relative to the plume), and that the hotspot track emerging from the flood basalt province is caused by the continuing arrival of plume tail material under the lithosphere. The realisation that thermal entrainment can cause a plume head to grow to a size sufficient to explain flood basalt provinces[22] boosted the plausibility of this idea[79].

Stimulated by the discovery of rift margin igneous provinces, White and McKenzie[80] proposed that voluminous rift-margin magmatism could be explained by the presence of unusually warm mantle under the rift, so that decompression melting during rifting generates much greater amounts of magma than at mid-ocean ridges or rifts over mantle of normal temperature. They estimated the mantle temperature excess to be up to 200℃. This general model for rift-margin magmatism has been widely adopted.

2.1 Flood basalts and plume heads

White and McKenzie[80] made two other proposals that have been more debated: that flood basalt provinces were also explained by rifting and that the underlying warm mantle accumulated via an upwelling jet or plume confined to the upper mantle. They supposed that flood basalts would flow from the rift eruption sites, which they argued would be elevated, across the adjacent continental surface for large distances. This explanation for flood basalts encounters the difficulty that some flood basalt provinces occur without major rifting, such as the Columbia River and Siberian flood basalts, and that even in cases where rifting occurred, such as the Deccan and Karoo flood basalts, the major phase of the flood basalt eruptions predate rifting[13, 81].

The inference of a plume confined to the upper-mantle encounters the difficulty of requiring a long time for the warm mantle to accumulate, because any upper-mantle plume head would necessarily be relatively small, leaving the plume tail to supply the required volume of warm mantle, which typically would take tens of millions of years[79]. This might not be a problem in the case of the Cape Verde hotspot from where the hypothesis had its genesis[80], because the African plate moves very slowly relative to the hotspot. However, in the case of the Deccan traps the Indian plate was moving north at about 18 cm/a relative to the Reunion hotspot whose track emerges from the Deccan province[13]. The sudden onset and rapid eruption of most of the Deccan flood basalts within less than 1 Ma and before significant rifting[81,82] cannot readily be reconciled with slow accumulation of warm mantle. However if the warm mantle arrives in the form of a new plume head that originates from the base of the mantle, then the plume head grows large enough to provide the required volume of eruption, and it might arrive at and move through the melting zone relatively rapidly[22].

Still, the plume head explanation for flood basalts in turn seemed to have two other difficulties, first that the plume head could not rise far into the shallow melting zone in the absence of rifting, so relatively little pre-rift magma would be generated, and second that the time scale of plume head arrival and spreading under the lithosphere seemed, from early estimates, to be more like 20 Ma than 1 Ma.

More detailed consideration has shown that three factors plausibly overcome these difficulties[30]. The first is that the plume composition is more fertile than normal mantle, as is indicated independently by geochemical arguments[83]. The second factor is the viscosity structure of the mantle, which increases by a factor of roughly 30 downward through the 660-km-depth discontinuity[39,84]. The third factor is that the plume grows from a thermal boundary layer.

High-resolution numerical modelling[30] has shown that as the plume rises through the viscosity drop in the transition zone it accelerates and narrows or "necks" (Fig. 4; a movie of this process can be viewed at[85]). The plume head's smaller horizontal extent then allows it more easily to "punch through" and displace ambient upper mantle and so to rise to shallower levels. The viscosity minimum under the lithosphere allows the head to spread relatively rapidly under the lithosphere. Finally, the thermal structure inherited from the feeding thermal boundary layer ensures that the hottest material is on the plume axis and so reaches the shallowest depths. These factors all work to increase the amount of melt generated. In combination with the more fertile composition inferred from geochemical arguments, the models predict that melt volumes of millions of cubic kilometers can be generated within one or two million years, reasonably approximating the observed range of large flood basalt provinces.

2.2 Plume heads and (some) rift margins

Even in the absence of large pre-rift eruptions, or in the case of delayed rifting[37], plume heads emplace below the lithosphere can provide a straightforward mantle source for large-volume rift margin magmatism (Fig. 5). Keleman

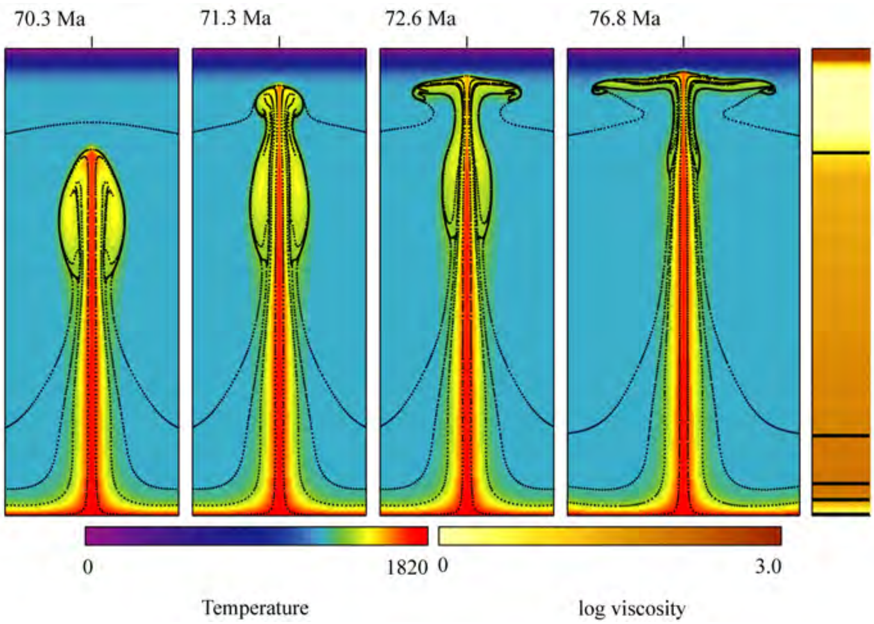

Fig. 4. Rise of a thermal plume head into a lower-viscosity upper mantle. The viscosity (right panel) steps by a factor of 20 at the transition zone and there is a further exponential increase with depth by a factor of 10. Lines of tracers mark fluid from various depths, as in Fig. 1. Temperature in ℃. The plume head "necks down" and is able more easily to displace upper mantle material laterally and thus to reach a shallow depth. The final ascent is also much quicker, as can be seen from the times of each frame.

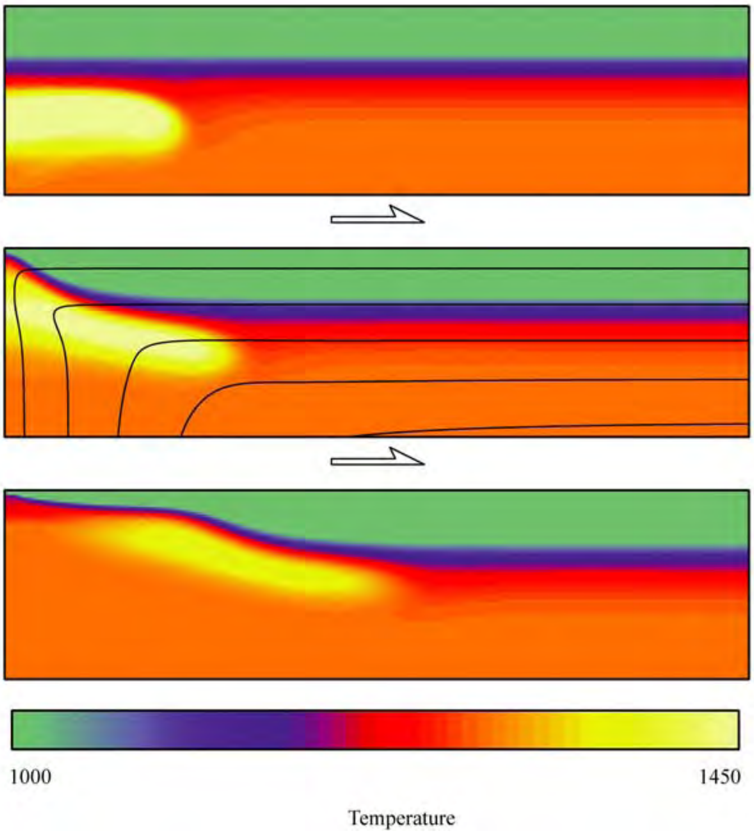

Fig. 5. Vertical sections showing how a plume head previously emplaced under the lithosphere (top panel) will be pulled up through a rift zone (lower panels). The rift margin is initially at the left. Black lines are streamlines. The plume material spreads under the newly-forming oceanic lithosphere, but by the lower panel only normal mantle is being pulled up into the rift. Decompression melting at the rift is initially higher than normal because of the plume's higher temperature, but returns to normal by the last panel. Temperature in ℃. After Ref. [37] and Leitch (personal communication).

and Holbrook[86] question the viability of the plume head model for the case of the United States East Coast Margin Igneous Province (ECMIP) because the ECMIP is very narrow (100 km or less), there were not voluminous surface eruptions on the scale of the North Atlantic igneous province, and because there is no hotspot track obviously associated with it. However the associated hotspot track would plausibly run under the northeast coast of South America and to the presently active Fernando de Noronha hotspot[13]. Moreover the rifting occurred some 15—25 Ma after the inferred arrival of the Newark plume head at 201 Ma, which would have given rise to regional dike swarms and the Palisades sills[87].

The ECMIP can be explained by delayed rifting of a plume head that has "pancaked" under the lithosphere and is less than 100 km thick. The detailed numerical modelling by Leitch et al.[37] (Fig. 5) has shown that an earlier-emplaced plume head can give rise to excess magmatism at the onset of rifting. Then, once the warmer head material is pulled up through the rift melting zone, underlying normal mantle is pulled up after it and the basaltic thickness reverts to that of normal oceanic crust. The result can be a rift-margin zone of thickened crust tapering to normal within a width of about 100 km, much as observed. White and McKenzie[80] also questioned the viability of the plume model in the case of the North Atlantic igneous province, evidently because of the contrast in volume and extent of the initial magmatism compared with the Iceland ridge hotspot track, but this contrast just reflects the head-tail contrast of a plume that has risen from the base of the mantle[87].

Thus the anomalously large magma volumes of some rift margin igeous events are plausibly explained by rifting over anomalously warm mantle, as proposed by White and McKenzie[80]. The most straightforward source of hotter fluid in a convecting system is a hot, lower thermal boundary layer, which in the mantle is expected to rise as a plume. The arrival of a new plume head provides a straightforward explanation for the typically sudden onset of non-rift or pre-rift eruption, which no other proposed mechanism seems to do. Voluminous rift-phase magmatism is also plausibly explained by the presence of a plume head. Important questions remain to be investigated, especially how the volume and composition of erupted or emplaced magma relates to the volume of melt generated in the source, which is all that has been calculated in the numerical models. This involves the processes of melt migration in the mantle and of the melting of compositionally heterogeneous source regions, both of which are complex topics and the subjects of vigorous debate[72—74].

3 Conclusion

Thermal plumes are to be expected in planetary mantles, their physical behaviour is well-quantified, and there is straightforward evidence for their presence in the Earth's upper mantle. Detection of plumes by seismic tomography is difficult, so current controversies are not surprising, but it can potentially provide the most direct test of and constraints on the plume model. Plume tails remain the most straightforward explanation for age-progressive volcanic chains. Plume heads offer a straightforward explanation for flood basalt eruptions, especially now that the timescale and volume of eruptions have been plausibly modelled.

Rifting over anomalously warm mantle provides a straightforward explanation for the thick, elongate large igneous provinces of many rift margins. The lower, hot thermal boundary layer of the convecting mantle is the most straightforward potential source of warm mantle, and the delivery of such warm material via a plume head has been shown to be a plausible model, once the kinematic interactions of a plume head and a rift are understood.

Some volcanic ridges do not show clear age progressions. In some cases this might be due to local ridge jumps or other complications, or it might be due to insufficient age data on representative samples, but we must remain open to the possibility that some such ridges are not plume-related. Other volcanic ridges with age progressions have no flood basalt province associated with them. These observations challenge the classical thermal plume model, but thermochemical plumes may have a richer range of behaviour that is capable of explaining a greater proportion of nonplate volcanism.

The effect on mantle melting of compositional variation, in both major element and volatile content, has become a vexed question, for spreading centers as well as for plumes. The potential complexities involved with melting a hetorogeneous, multicomponent source region, melt migration (via pore flow, channels or dikes), continuous but variable and incomplete reaction with surrounding material, near-surface fractionation, and the dependence of all these processes on local vagaries and timescales mandate careful work and cautious conclusions[73,75,88,89]. As a result, the source composition and source temperature will not be easy to resolve definitively.

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Mantle plumes: Why the current skepticism?

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Abstract **The present reappraisal of the mantle plume hypothesis is perhaps the most exciting current debate in Earth science. Nevertheless, the fundamental reasons for why it has arisen are often not well understood. They are that 1) many observations do not agree with the predictions of the original model, 2) it is possible that convection of the sort required to generate thermal plumes in the Earth's mantle does not occur, 3) so many variants of the original model have been invoked to accommodate conflicting data that the plume hypthesis is in practice no longer testable, and 4) alternative models are viable, though these have been largely neglected by researchers. Regardless of the final outcome, the present vigorous debate is to be welcomed since it is likely to stimulate new discoveries in a way that unquestioning acceptance of the conventional plume model will not.**

Keywords: plume, volcanism, hotspots, convection, mantle, plate tectonics.

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The current vigorous re-appraisal of the mantle plume hypothesis[1] has been described as potentially the most radical development in Earth Science since the advent of the plate tectonic theory in the 1960s. The foundation of mantle plume theory was laid in 1963, when Wilson[2] suggested that Hawaii and the time-progressive island/seamount trail northwest of it could be explained by passage of the Pacific ocean floor over a hot region in the mantle, which he termed a "hot spot". The mantle plume hypothesis proper was born in 1971 when W. Jason Morgan proposed that there were approximately 20 such "hot spots" and that the source material rose convectively in structures resembling "pipes to the deep mantle"[3]. He hypothesized that these "pipes" were rooted in the deep mantle, assumed to be relatively immobile, in order to explain the apparent relative fixity of surface "hot spots". Despite the lack of radiometric dates at that time, Morgan presumed many volcanic chains to be time-progressive like the Hawaiian and Emperor chains.

For the first two decades following the original hypothesis, interest in mantle plumes was slight (Fig. 1)[4]. However, their popularity exploded about 1990 following the publication of papers describing laboratory simulations of plume-mode convection in fluid-filled tanks[5], and proposing that mantle plumes deliver a high flux of ^{3}He which comprises a primordial-mantle tracer[6]. The

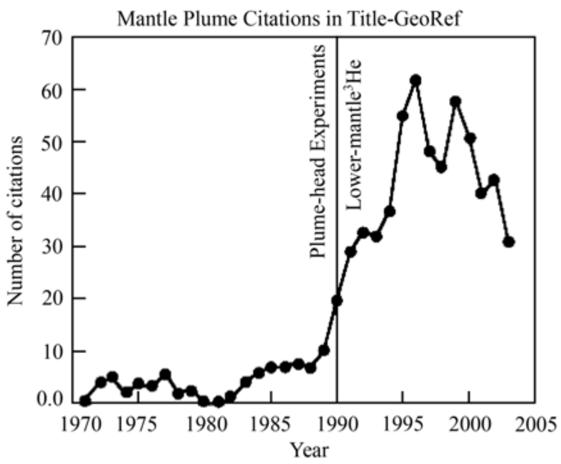

Fig. 1. Number of citations with the word "plume" in the title, in reference to mantle plumes, by year since 1971, listed in GeoRef, the online data base of the American Geological Institute. The vertical line gives the year of publication of a paper by Campbell and Griffiths[5] depicting plume heads and tails. The same year saw publication of a paper by Kellogg and Wasserburg[6] proposing a contribution from the lower mantle to ^{3}He flux via mantle plumes. Following these papers, the plume hypothesis attained a great degree of acceptance (reproduced from Anderson and Natland, 2005).

rate of publication of papers advocating mantle plumes leapt by almost an order of magnitude as a result, and subsequently remained high.

Nevertheless, dissenting voices were never entirely absent, and included some who had been influential contributors to the development of plate tectonics. During the 1990s, skeptics were in the minority. Most papers published about mantle plumes assumed the hypothesis to be correct and sought to validate it rather than to test it. The task at hand was to find more plumes, not to look critically at existing ones.

The present decade has ushered in a vigorous upsurge in skepticism, however. Why did this occur, when the hypothesis had moved from embryonic status through vigorous research and on to general acceptance? There are four primary reasons for this, as detailed below.

1 Observations do not agree with the predictions of the classical plume model

The basic, classical mantle plume model makes a number of fundamental predictions. However, for many of the 19 plume locations originally proposed[3], and the much larger number subsequently added to that list[7], confirmation of these predictions by observation has remained elusive:

(1) Volcanic tracks are predicted to extend away from the present-day locus of active volcanism (the "hot spot") and to be time-progressive. This is not observed at many locations, e.g., Iceland and Ascension. Furthermore, the reliability of many ages used to define "hot spot tracks" has recently come under criticism[8,9].

(2) "Hot spots" are predicted to have been fixed relative to one another through time. Their degree of relative fixity is variable, however, e.g., Atlantic "hot spots" were not fixed relative to Pacific ones prior to about 50 Ma[10].

(3) Active "hot spots" should be underlain by vertical, quasi-cylindrical bodies of anomalously hot rock that extend from the core-mantle boundary to the Earth's surface. Seismology has failed to image convincingly and consistently such structures, despite over 30 years of experiments of increasing sophistication. For example, the seismic anomalies beneath Iceland, Tristan and Afar are consistently found to be confined to the upper mantle only, and no anomalies at all are found beneath many other "hot spots", e.g., Reunion and Hoggar.

(4) Lavas at "hot spots" should reflect sources that are hotter than those elsewhere, e.g., beneath mid-ocean ridges. Petrology provides little unambiguous evidence for this, however. Hawaii is the only currently active "hot spot" where picrite glass been found, suggesting high temperature, and the spatial extent of this is unknown. The mantle source of Icelandic basalts may be a few tens of degrees warmer than typical ridges, but such an anomaly is probably too weak for a mantle plume and may be of regional extent rather than only local[11,12]. This might also apply to Hawaii. At most other "hot spots" there is no petrological evidence at all for elevated temperature and even voluminous tholeiitic basalts, which suggest high heat flux, are absent[13,14].

(5) Some proposed plumes lack the large igneous provinces (LIPs) assumed to represent the "plume head", e.g., Hawaii. Other LIPs lack the time-progressive volcanic track associated with the "plume tail", e.g. the Ontong Java Plateau and the Siberian Traps.

In addition to these difficulties, many common geological associations must be attributed to coincidence in the classical plume model. For example, the Yellowstone "track" follows the northern boundary of the Basin & Range province, and the Azores "hot spot" is located on a ridge-ridge-ridge triple junction.

In some cases, the observations conflict so acutely with the plume hypothesis that they cannot be ignored or attributed to incomplete sampling. For example, there is no evidence that the Ontong Java Plateau, the largest LIP on Earth with a volume of 60×10^6 km^3, was preceded by the uplift predicted by the plume hypothesis[15]. For the Siberian Traps, the continental sister of the Ontong Java Plateau, geological evidence suggests pre-emplacement subsidence[16,17]. Although these are only two of the many LIPs on Earth, if the plume hypothesis fails there, and an alternative mechanism is required for them, it naturally follows that the alternative is a candidate for other LIPs also.

It is not the case that no observations at all are consistent with the plume hypothesis-some are[18]. Nevertheless, many scientists find the predictive power of the classical plume hypothesis unsatisfactory.

2 Convection of the kind required to generate classical mantle plumes may be precluded by the physical properties of the mantle

All regions of the mantle probably convect in some way. However, given the physics of the interior of the Earth it is questionable whether convective upwellings from the deep mantle rise to the surface and produce the local volcanic features known as "hot spots"[19]. It is even more questionable whether deep upwellings could produce the regular behaviour of some of these volcanic features, which occurs on spatial scales of the order of kilometers and timescales of the order of millions of years. It has also been pointed out that the hypothesis requires mutually exclusive assumptions-plumes were proposed to be rooted deeper than the convecting upper mantle in order to explain the relative fixity of surface "hot spots", but a convecting upper mantle is not consistent with relative hot-spot fixity[20].

The effect of high pressure on convection in the deep mantle is important. Pressure has a strong, non-linear effect on thermal expansion, conductivity and viscosity. At high pressure, temperature has less effect on density and less buoyancy is imparted to material warmed, for example, by heat transfer from the core. Similarly, thermal conductivity increases with pressure, reducing the tendency for heat to be removed by convection. Viscosity increases by 1—2 orders of magnitude with depth in the mantle, further hindering convection. The effects of pressure on material properties further suggest that the lower mantle may be chemically stratified. Plausible temperature variations in the deep mantle may then cause density variations that are smaller than those across the chemical interfaces, hindering or precluding the rising of warmed material from the deep mantle.

These variations in physical properties within the Earth suggest that, whereas in the upper mantle convective features have characteristic dimensions of hundreds of kilometers and lifetimes of the order of hundreds of millions of years, the deep mantle, in contrast, may convect only slowly and on a vast scale, with timescales of billions of years and spatial scales of thousands of kilometers.

Whole-mantle tomography supports this picture, showing that the lower third of the mantle is characterised by global-scale sized bodies[21]. How should these bodies be interpreted, and are the "superplumes" observed by seismic tomography beneath the south Pacific and the south Atlantic thermal upwellings? Shear velocity is affected by temperature, density, and composition, but is a poor proxy any one of these alone. Temperature and chemical composition affect shear velocity only weakly, especially in the deep mantle, and correlations between velocity and density may be positive or negative[22]. The most recent seismic studies of the "superplumes" suggest that they are probably ancient, slowly-developing structures and may be dense and not buoyant[23]. Thermal plumes of the sort postulated to fuel surface "hot spots" must almost certainly rise from a thermal boundary layer clearly visible seismically, and given the physical properties of the very deep mantle it would seem that such a layer would have to lie higher up. However, the major seismic discontinuities are known to result from mineralogical phase changes, not temperature or composition changes. There is no evidence for strong thermal boundary layers anywhere in the Earth except at the surface and the core-mantle boundary.

This view is not at odds with the requirement to get heat out of the core in order to power the dynamo. The lowermost mantle heats up only slowly and this, coupled with its inferred low thermal buoyancy, results in large sluggish upwellings that carry away any heat not conducted or radiated away. It does not follow that classical mantle plumes of the sort proposed by Morgan[3] exist or that the upwellings cause the surface features popularly assigned to plumes. It has been suggested further that heat loss from the core may have been overestimated, and much of the heat lost from the surface of the Earth may be radiogenically generated in the mid- and upper mantle[24]. McKenzie & Weiss[20] have also pointed out that the plume mode of convection is inconsistent with the behaviour of an internally heated fluid, which is expected, on the contrary, to exhibit narrow downwellings and diffuse upwellings.

No laboratory, and few numerical demonstrations of plume-mode convection model the Earth realistically and many do not include all of the critical factors described above. The laboratory convection models that were influential in popularising the plume model in the early 1990s[25] involved injecting low-density fluids into tanks containing higher-density fluid. The plumes produced were not self-sustaining, and the apparatus did not simulate the effects of pressure within the Earth. The future development of numerical convection models that include the effects of temperature and pressure on all the relevant physical properties, along with the variation in thermal expansivity and increase in conductivity and viscosity with depth in the mantle, will be of great interest.

In summary, it is not disputed that some form of convection probably occurs at all levels in the mantle. What is questioned is that the mantle can produce coherent, narrow convective structures that traverse its entire thickness and deliver samples of the core-mantle boundary layer to the Earth's surface. If the thermal plumes postulated to feed "hot spots" do not rise from the only strong thermal boundary layer known to exist in the interior of the Earth, it is then not clear whence they can rise. The conclusion that such thermal plumes possibly may not occur at all in the Earth then becomes a natural corollary.

3 The contemporary plume hypothesis is so flexible that it cannot be disproved

I make the distinction between the original, classical plume hypothesis and its modern, contemporary form. A plume is a well-defined term in fluid dynamics, and Morgan's original meaning was clear[3]. However, subsequently the term "mantle plume" has been applied to such a diversity of phenomena that in many cases it signifies little more than whatever lies beneath a volcanic area[26]. In practice, it has become the case that no observation or absence thereof is considered sufficient to disprove the hypothesis.

Plumes have been suggested to come from almost any depth, including the core-mantle boundary, chemical discontinuities in the lower mantle, the tops of the lower-mantle "superplumes", the mineralogical phase-change boundaries at 410 and 650 km depth, the base of the lithosphere or from arbitrary levels in the mantle[20,27]. They have been suggested to be vertical or to tilt, and for some "hot spots" multiple papers suggest different tilts. For example, the postulated Iceland plume has been variously suggested to tilt to the west[28], south[29] and southeast[30]. Some melting anomalies are very localised, e.g., Hawaii. Nevertheless, scattered volcanic production has been explained by lateral flow for distances of up to thousands of kilometres, e.g., at Iceland[31] or multiple plumes in close proximity e.g., in the Azores region. Different authors have varying perceptions of the width of mantle plumes. Widths of the order of 1000 kilometres have been assigned to plumes on the basis of seismic tomography experiments[32] but single volcanoes only a few kilometres in diameter have been suggested to represent the plume centre at "hot spots" such as Iceland, Hawaii and Yellowstone. Stable or unstable flow on all timescales is considered plausible. Volcanic production at Hawaii has increased by an order of magnitude during the last 5 Ma. Cyclic pulsing behaviour in a plume beneath Iceland has been suggested to account for diachronous bathymetric ridges to the south and north of Iceland[33]. The measurement of ages of 120 Ma and 90 Ma for lavas from the Ontong Java plateau led to the suggestion that this LIP resulted from a two-headed plume[34], but the recent demonstration that the latter ages were in error[35] led to a return to a single-headed plume model.

Relative fixity was one of the original, fundamental properties attributed to mantle plumes, but the subsequent discovery that this did not occur for many pairs of "hot spots" was not considered to be an impediment, but explained by deflection by convection currents in the mantle ("mantle wind"), lateral flow, or "plume capture" by ridges. For example, the Hawaiian "hot spot" is interpreted to have migrated south by ~ 800 km with respect to the Earth's magnetic pole between emplacement of the oldest Emperor seamount (the Detroit seamount, 75.8 Ma) and the Hawaiian-Emperor bend at 47 Ma[36]. Some, but not all of this has been explained as deflection by flow in the mantle. The persistence of the Iceland melting anomaly at the mid-Atlantic ridge has been attributed to lateral flow from a plume centre further west, beneath Greenland or the Greenland-Iceland-Faeroe ridge[37].

The postulated longevity of plumes varies from about 80 Ma (e.g., Hawaii) to only a few Ma, e.g., the Caroline chain in the Pacific ocean. The plume head-tail model, which arose from laboratory convection experiments, has been applied to some melting anomalies e.g., the Deccan Traps—Laccadive-Chagos ridge-Reunion system, which appears to fit the model well. Many LIPs without chains, and chains without LIPs, have also been attributed to plumes, however. In addition, the predicted precursory kilometre-scale uplift is observed at some localities[18] but not at others[17]. Recently it has even been suggested that simple domal uplift accompanying the arrival of a plume head at the base of the lithosphere is not required[38].

The discovery in the early 1970s that geochemistry different from that of MORB characterized “hot spots” and island and seamount chains[39] was attributed to plumes tapping a chemically distinct source. Nevertheless, the discovery that many “hot spot” lavas have compositions that overlap with MORB was explained by entrainment of upper-mantle MORB source into plumes. The discovery that high maximum $^3He/^4He$ ratios occur at Hawaii led to the suggestion that the lower mantle plume source is high in primordial ^3He. However, the failure to find basalts with high $^3He/^4He$ ratios at some “hot spots” e.g., Tristan da Cunha, was explained as contamination by helium high in radiogenic ^4He of crustal origin, or incomplete sampling Petrology and other methods have also been applied to seek evidence for locally elevated temperature beneath “hot spots”. Evidence has been cited from a small subset of currently proposed plume localities, but its lack at others is explained by incomplete sampling or fundamental inaccessibility of the expected rocks.

Few scientists would continue to defend the classical plume hypothesis in its pure, original form, just as few are ready to abandon the model altogether. It is reasonable that an original hypothesis evolves and is amended as new data accumulate. Nevertheless, all scientific hypotheses must remain fundamentally disprovable, or they cease to be hypotheses and become a priori assumptions. If wrong, they may then prevent further progress. Many feel that the plume hypothesis has become, in practice and in its contemporary flexible form, not disprovable[26]. A clear definition of a plume agreed upon by all is a necessary prerequisite for focused discussion of whether they exist or not and if meaningful tests are to be designed and performed.

4 Alternative models are viable

Much work on melting anomalies has focused on adapting the plume hypothesis to account for new observations, but relatively little has been done on developing alternative models. As a consequence, many have remained qualitative only. Quantification of alternative theories is a new and rapidly developing subject. Models include:

4.1 EDGE convection

When continents split, linear volcanic margins generally form, followed by anomalous magmatism in some parts of the new ocean, e.g. the north Atlantic. The theory of EDGE convection is based on the observation that where thick, cold continental lithosphere is juxtaposed against hot, oceanic asthenosphere, small-scale convection may develop at the continental edge and cause vigorous, time-dependent magmatism[40].

4.2 Plate-tectonic processes

Ocean-island basalt geochemistry has long been linked to subducting slabs, including the crustal and mantle lithosphere sections. Furthermore, fusible materials such as these are required to account for the relatively large volume of eruptives that is the primary feature of all melting anomalies. The deep-mantle plume hypothesis requires that this fusible material is transported to the core-mantle boundary and back again. The plate-tectonic processes model (also called “the plate model”) suggests that it is instead circulated at much shallower depths. The model suggests that “anomalous” volcanism occurs where plates are in extension, either in their interiors or near their boundaries, and that the volume of magma produced is a function of the fertility and fusibility of the source material being tapped. If old subducted slab material in the shallow mantle is tapped, volcanism will have ocean-island basalt geochemistry and be more voluminous than if mantle peridotite only is available in the source region[41].

4.3 Melt focusing

It is relatively common for melting anomalies to lie at complicated tectonic junctions such as ridge-ridge-ridge triple junctions, ridge-transform intersections and microplates, e.g., the Azores, the Bouvet triple junction, the Easter microplate and at Iceland. Melt focusing has long been assumed to occur beneath mid-ocean ridges, within a two-dimensional region triangular in cross section perpendicular to the ridge. Quantitative modeling predicts three-dimensional focusing of melt from a cone-shaped region beneath some plate boundary junctions e.g, ridge-transform and ridge-ridge-ridge triple junctions, increasing the amount of melt expected[42, 43].

4.4 Large-scale melt ponding

Numerical modeling has been unable to simulate the vast melt volumes and eruption rates associated with large LIPs such as the Ontong Java Plateau, even if a fusible source is assumed[44]. It seems inevitable that if the volumes and rates have been correctly estimated, the melt must have formed over a longer period than the eruption time. This suggests that large-volume ponding might be possible, despite the usual assumption that melt is extracted from its source region as it forms, at a relatively low degree of melting[45]. In support of this, recent work has shown that non-texturally equilibrated rocks may retain melt fractions of up to 11%[46]. Melt might pond at the base of the lithosphere and be retained there if the lower lithosphere were in compression.

4.5 Continental lithospheric delamination and slab break-off

In addition to the large eruption rates, the lack of uplift prior to LIP emplacement reported from some localities must be explained[15,16]. Lithospheric delamination can potentially fit the observations for continental LIPs. Delamination can occur if the continental lithosphere becomes thickened, transforms to dense phases such as eclogite, and catastrophically sinks and detaches. Numerical modeling predicts that preliminary surface subsidence is followed by extensive magmatism[47]. An analogous process is slab breakoff, which may rapidly alter the pattern of flow in the mantle in collision zones and lead to bursts of magmatism[48].

4.6 Rifting decompression melting

Numerical modelling of the rifting that accompanies continental breakup suggests that the volume, timing and distribution of decompressional melting is related to lithosphere thickness and composition and pre-existing structures. The volumes calculated are sufficient to explain those observed at LIPs and volcanic passive margins, suggesting that plumes are not required to generate these melting anomalies[49].

4.7 Meteorite impacts

The possibility that impacts could generate the large volumes of magma observed in LIPs has recently been revisited, since such a mechanism could elegantly explain the very short timescales over which LIP formation is thought to occur. The potential of pressure-release (decompression) melting was overlooked in early modeling and recent work has demonstrated that it is capable of triggering the volumes and rates observed in LIPs[50].

It has been suggested that such diversification of mechanisms amounts to increased model complexity and is thus moving in the wrong direction. However, there is great diversity in the nature of melting anomalies, which vary from small-volume, short-lived, intraplate alkalic chains such as the Caroline Islands to large-volume, long-lived, ridge-centred tholeiitic features such as Iceland. It seems unlikely, given such diversity, that all are caused by the same process. For few if any melting anomalies can it be claimed that any one theory, plume or alternative, fits the observations without residual problems. For this reason it is essential to consider multiple hypotheses and not to assume one model a priori to the exclusion of all others.

5 Closing remarks

In this short essay I have attempted to describe why many scientists have recently begun seeking alternative explanations for the origin of “hot spot” magmatism, either for individual localities or in general. It is difficult to adequately convey the atmosphere of excitement and enthusiasm that has gripped the many practitioners who feel that their own work and the subject have been unexpectedly invigorated by the new questions being posed. The explosion of critical, innovative thinking in the field owes its thanks largely to the huge expansion of Earth Science data available, and to the advent of new internet-based data-distribution and communication tools, which have transformed the way we all work. Not least does the new subject owe its thanks to the generous and unselfish mentoring of the many newcomers to the field by the few who kept the torch burning during the long decades when interest was relatively low. The subject has now flowered to a state of enthusiastic global debate. Whatever the outcome, it is this debate that is important; only if theories are criticised and tested will new discoveries and real progress be made.

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