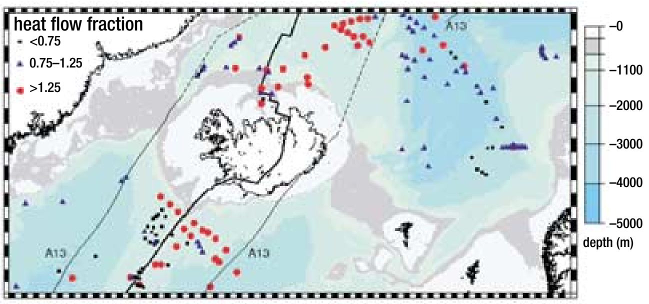
Mantle plumes: heat-flow near Iceland

In the first of four pieces arising from Gill Foulger's challenge to the mantle plume hypothesis (last issue), **Carol Stein** and **Seth Stein** join the debate with some data and comment on heat-flow around Iceland.

Abstract

Seafloor heat-flow near Iceland on the North American side of the Mid-Atlantic Ridge is comparable to that for oceanic lithosphere elsewhere, and thus shows no evidence for significantly higher temperatures associated with a mantle plume. Heat-flow is higher on the Eurasian plate than on the North American plate, an intriguing asymmetry opposite to that expected from models in which Iceland formed over a mantle plume.



1: Bathymetry and heat-flow for the Iceland region. Heat-flow shown as heat-flow fraction, observed values normalized by global average values for that lithospheric age (figure 2). Lithosphere younger than about 35 Myr indicated by the positions of magnetic anomaly 13 (solid line) after Mueller *et al.* (1997) or approximated by dashed line.

Foulger's (2002) paper in the last issue of *Astronomy & Geophysics* (*A&G* **43** 6.19) illustrates the debate over whether 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 (Morgan 1971). In the plume model, plate motion over fixed or slow-moving plumes causes age-progressive linear volcanic chains and topographic swells that identify plumes and yield inferences about their properties. This model has been widely accepted because it gives an elegant explanation of how diverse volcanic regions have similar origins, and an absolute reference frame describing plate motions relative to the deep mantle.

However, many hotspots deviate from the expected behaviour. Some hotspots move significantly relative to each other and the spin axis (Tarduno and Cottrell 1997), changes in some volcanic chain orientations do not correspond to the expected plate motion changes (Norton 1995), and some chains show no clear age progression (Schlanger *et al.* 1984). A view is emerging that at least some hotspots, notably Yellowstone, are not due to deep mantle plumes (Humphreys *et al.* 2000, Christiansen *et al.* 2002), and the entire plume model is being challenged (Anderson 2000, Hamilton 2002).

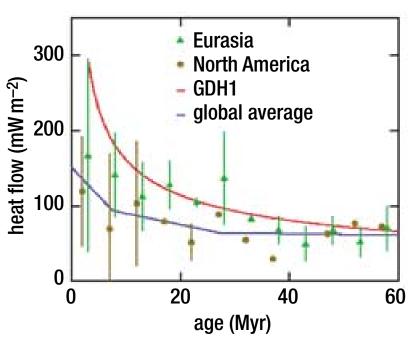
Iceland is a focus of these discussions, as the type example of a hotspot on a mid-ocean ridge. In the plume model, the elevation and thick crust relative to typical mid-ocean ridges result from melting by a hot plume (White 1999), whereas in Foulger's (2002) non-plume model, temperatures are not unusually high but excess melting of more fertile material occurs, consistent with petrologic arguments (Korenaga and Kelemen 2000). Seismological results for the maximum depth of the low velocity anomaly, the strongest discriminant between a deep mantle plume and an upper mantle melting anomaly, are discordant (Foulger *et al.* 2001, Shen *et al.* 2002) because seismometers on Iceland have limited resolution for structure at depth owing to the island's small size.

Seafloor heatflow

Given this interest, we examined seafloor heat-flow data from the Iceland region. The small or absent heat-flow anomalies at other hotspots play a role similar to that of the dog whose failure to bark helped Sherlock Holmes locate the missing racehorse Silver Blaze. Originally, the uplift at Hawaii and similar midplate hotspots was thought to reflect a hot plume causing heating to about 50 km of the surface (Crough 1983, McNutt and Judge 1990). Such heating predicts heat-flow significantly higher than from the usual cooling of oceanic lithosphere as it spreads away from the mid-ocean ridges where it formed. Although anomalously high heat-flow was initially reported, subsequent analysis showed that most, if not all, of the apparent anomalies resulted from comparing data to thermal models that underestimated heat-flow elsewhere (Von Herzen *et al.* 1989, Stein and Abbott 1991, Stein and Stein 1993). Hence subsequent models generally assume that the uplift results from the dynamic effects of rising plumes (Liu and Chase 1989, Sleep 1994) and the associated compositional buoyancy, whose thermal effects are concentrated at the base of the lithosphere and raise surface heat-flow at most slightly, because conduction to the surface takes tens of millions of years.

Heat-flow has played little role in the debate about hotspots like Iceland, which are on or near mid-ocean ridges, for two reasons. The first is that predictions for heat-flow have not been offered, because such hotspots are thought to reflect an interaction between upwelling plumes and nearby spreading centers (Ito *et al.* 1996) more complex than at mid-plate hotspots which are generally attributed to a simpler (albeit not yet understood) interaction of a plume with a plate interior. Second, seafloor near on-ridge hotspots is young, less than 40 Myr old. In young seafloor, measured heat-flow is significantly lower than expected purely from conductive cooling of the lithosphere, because some heat is transported by hydrothermal circulation of sea water through the crust (e.g. Stein and Stein 1994). Hence it was unclear how to characterize "normal" heat flow and assess possible perturbations.

Plume models imply that heat-flow should be above the "normal" in several ways. The most important is likely to be an indirect effect of plume material migrating along the Mid-Atlantic Ridge (White 1999). This should raise temperatures along the ridge by up to several



2: Heat-flow data (figure 1) grouped in Myr bins for the Eurasian and North American plates, compared to the predictions of the GDH1 thermal model which does not include the effect of hydrothermal circulation, and a linear fit to the global average values.

hundred degrees, depending on distance from the plume, so that lithosphere formed on either plate would have higher heat-flow.

The plume should also have direct effects on heat flow. First, outward-flowing plume material should heat the base of already-formed lithosphere. This effect would be similar to that at Hawaii, but larger because heat is added at the base of the lithosphere, which is thinner near Iceland because of its relative youth. Hence increased heat-flow should occur on both sides of the Mid-Atlantic Ridge.

A second direct effect could result from the history of relative motion between the plume, Mid-Atlantic Ridge, and the two plates. Modelling this history is more complex than along the Hawaiian–Emperor seamount chain, where the history of volcanism is used to infer the history of the plume. In contrast, the Iceland plume's history cannot be inferred directly from the elevated Iceland–Greenland and Iceland–Faroe plateaus extending westward and eastward from Iceland (figure 1), because models assuming various hotspot sizes and motions "offer non-unique solutions that could be used to explain a plateau of any location, origin, and age progression" (Vink 1984).

To address this ambiguity, Vink (1984) used plate reconstructions assuming that plumes are fixed to predict that the plume presently under Iceland was under Greenland 45 Myr ago. Since then, westward motion of the Mid-Atlantic Ridge relative to the plume has brought Iceland over the plume. During this time, plume material flowed laterally beneath the North American plate to the Mid-Atlantic Ridge. Assuming that plume material flowed to the closest point on the Mid-Atlantic Ridge, where plateaus formed by excess volcanism and were transported away in opposite directions as the two plates spread, this matches the observed trends of the plateaus. Alternatively, White and McKenzie (1989) argued that such lateral flow was not possible. Instead, they proposed that a newly formed plume initiated the rifting of the Greenland margin and the opening of the North Atlantic, such that the paired plateaus formed directly above, via ridge jumps that kept the Mid-Atlantic Ridge above the plume's core (White 1999). Although these plume history models differ, and only the first reflects detailed kinematic modelling, we expect that both predict heat-flow near Iceland higher on the North American (west) plate than for lithosphere of the same age on the Eurasian (east) plate.

We thus examined heat-flow data for sites within 500 km of Iceland to see if they showed either expected effect – abnormally high heat-flow on either side of the ridge, and higher heat-flow to the west. Only good-quality data, by the criteria of Stein and Abbott 1991, were used. As shown in figure 2, we find no evidence for either effect. The North American values, where a plume should raise heat flow, are consistent with the global average for lithosphere of that age including the effect of hydrothermal circulation (Stein *et al.* 1994). We do observe an asymmetry, but in the opposite sense. Out to an age of about 35 Myr, European values are generally about 40% higher than for North America, approaching those of a lithospheric cooling model (Stein and Stein 1992) that does not include hydrothermal effects.

Asymmetry

Such striking asymmetry between ridge flanks is unusual. Although significantly more data will be needed to fully understand it, we can ask three questions with what we have:

- Is it real? Heat-flow data in the region are sparse, as are such data elsewhere in the oceans, owing to the cost and difficulty of collection. We lack the ideal distribution of data on opposite sides of the ridge, especially near the plateaus, and so have only a regional comparison. Even so, the asymmetry seems real.
- Is it due to sediments? For a given age, sediments tend to be thicker on the Eurasian side (Talwani *et al.* 1971). The higher heat-flow may thus reflect impermeable sediment suppressing hydrothermal circulation, as observed near the Juan de Fuca ridge (Davis *et al.* 1992). However, this mechanism is thought to require that almost all igneous basement rock be covered, which is not the case here, especially within 10 Myr of the axis. Moreover, on a global basis, sediment thickness rarely has a significant effect on heat-flow (Stein *et al.* 1995). Hence, although sediment effects may contribute, our sense is that they are not the prime cause of the asymmetry.
- If not, what causes it? Because the asymmetry is opposite to that expected from the proposed history of the plume, non-sediment-related effects also seem worth considering.

First, the asymmetry might somehow reflect differences only in mantle temperature between the plates. However, in such a case we expect comparable variations in subsidence, with the hotter Eurasian plate subsiding faster and hence being deeper for a given age. Such an effect has been reported, but the 5% subsidence-rate asymmetry (Johansen *et al.* 1984) is significantly less than that in heat-flow. Second, the asymmetry might somehow reflect westward migration (absolute motion) of the Mid-Atlantic Ridge, which may affect spreading processes (Stein *et al.* 1977, Small and Danyushevsky 2003). However, initial inspection of data suggests that the asymmetry dies off to the north and south. Third, the asymmetry might reflect ridge migration over an unusual part of the mantle, similar to Foulger's (2002) proposal that Iceland results from excess magmatism as the ridge migrates over the Caledonian suture. Such a mechanism needs a process that generates both higher-than-normal heat-flow and much-less-anomalous depths.

In summary, heat-flow data near Iceland show no evidence for either of the regional thermal anomalies that might be expected near a mantle plume – higher overall heat-flow and asymmetry with higher heat-flow on the North American plate. Hence if a plume exists, it is not significantly hotter than typical mid-ocean ridges. Moreover, the heat-flow asymmetry opposite to that expected implies either significant sediment perturbations or other tectonic processes. Hence, whatever the outcome of the debate over the depth of the low velocity anomaly near Iceland, heat-flow should provide useful constraints on models for what is occurring. ●

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Look again

Don Anderson argues that there is abundant evidence against the plume hypothesis, for an objective eye to consider.

In astronomy and physics it is common to challenge and test the major reigning paradigms, including venerable ones such as the Big Bang, expansion, inflation, superstrings, dark matter and even relativity. In Earth sciences it is more common to hang on to cherished beliefs, such as continental and hotspot fixity, long after they have ceased to have any predictive power, and to rationalize discrepant observations by *ad hoc* modifications. The plate-tectonic revolution was strongly resisted but was so successful that the plume model, which followed on its heels, largely escaped criticism. Plumes were devised to overcome certain perceived shortcomings of plate tectonics such as the existence of volcanic chains and continental flood basalt provinces. Despite major shortcomings, the plume idea has been accepted and modified but seldom challenged or tested. Alternatives are often scoffed at rather than being seriously considered. Plumes have become unquestioned dogma rather than a testable idea.

The author and editors are to be complimented for publishing an alternate view for one of the classic textbook hotspots. The plume suggestion of Morgan (1971) and Wilson (1963) attempted to explain long-lived melting anomalies such as Iceland, Yellowstone, Hawaii and about 15 other volcanic provinces in terms of narrow, hot, stationary plumes, jets or thermals from the core–mantle boundary. The list later grew to about 170, the number of volcanic features that for various reasons were not considered plate boundary or incipient boundary features. The original speculation was an elegant idea and gave several specific testable predictions about heat flow, magma volume, fixity, and parallelism of island chains. One prediction was that there had to be about 20 plumes equivalent to Hawaii and these represented narrow upwellings compensated by distributed diffuse downwellings. Geophysical measurements including mantle tomography have shown that these predictions were wrong. The large predicted plume heads, easy to spot in tomographic images (Anderson *et al.* 1992) and uplift data, were not there. Alternative ideas involving crack propagation, mantle heterogeneity and small-scale convection must now be considered. The most serious observational problem with the plume idea is the lack of any evidence for high magma temperatures or high heat flow around hotspots or for thermal uplift (Anderson 1999, 2000). Athermal mechanisms such as magma focusing, magma fracture and corner flow must be entertained to explain regions of excess magmatism without uplift, high magma temperatures or high heat flow. Plate tectonics itself introduces thermal and chemical heterogeneity into the mantle so some regions will have greater or lesser amounts of fusable material and melting as a result.

The plume hypothesis has proven resistant to falsifications because rationalizations have been adopted for all discrepant data. It was fixity that convinced most workers that plumes were more appropriate than crack or stress-based hypotheses, but now we are told that fixity is not expected and is, on the contrary, an argument in support of plumes. Other rationalizations include large radius of influence, large distance lateral flow and explanations for the absence of uplift, heat flow and expected geochemical anomalies. Instabilities originating at the 650 km phase boundary have been proposed, even though this is a mineralogical phase transition and not a thermal or chemical boundary and such shallow plumes do not have the strength to do what they were originally proposed to do, such as breaking up continents, keeping ridges open and providing massive amounts of basalt through thick lithosphere. Finally, the pick-and-choose technique has been used to cull the official hotspot list down to between 7 and 10 as detailed studies eliminate plumes as credible explanations for the data. This leaves most "hotspots" unexplained. It is unlikely that Foulger's important observations will change many minds. Persuasive evidence against the plume hypothesis and accessible deep mantle reservoirs has been available for decades.

The most serious problems underlying the plume hypothesis involve unrealistic assumptions about the physics and thermodynamics, the normal background temperature, melting temperature and homogeneity of the upper mantle. In plume calculations the upper mantle is unrealistically assumed to be cold, dry and subsolidus and more or less isothermal at a given depth. Large volumes of magma are assumed to reflect locally elevated temperatures imported from great depth, rather than differences in fertility, upper mantle temperature, melting point or focusing. The average mantle potential temperature is more likely closer to 1350 °C than to 1200 °C and the melting point is likely to be lower than dry pyrolite (Korenaga and Kelemen 2000, Anderson 2000). This makes an enormous difference. If normal upper mantle is mainly close to or above the solidus, plus or minus normal fluctuations, then the plume hypothesis is unnecessary. The asthenosphere has low viscosity and can flow towards regions of thin lithosphere without a plume. The long-distance lateral transport of plume-head material recently proposed (Sleep 1997) is an *ad hoc* adjustment to the deep plume hypothesis and brings it closer to alternative views regarding shallow distributed sources of heat and magma. Plumes are point sources of pollution and require large lateral transport to service the widespread volcanism attributed to them. A partially molten asthenosphere provides a widespread and readily available source of magma, needing only lithospheric extension to localize magmatism. Asthenospheric material and chemical heterogeneity need not originate at the core–mantle boundary or any deep thermal boundary layer.

The basic geochemical assumption behind plumes is that of a chemically homogeneous upper mantle. It is assumed that if mid-ocean ridge basalts come from the upper mantle then chemically different basalts must come from the lower mantle. This is not only a logical fallacy but is likely to be false (Anderson 1989, 1999). Sampling theory and the central limit theorem, however, show that large volume integrators such as oceanic ridges should be more homogenous and should exhibit less extreme values than smaller scale samplers such as oceanic islands. Thus, it is to be expected that ocean island basalts should be more geochemically diverse than mid-ocean ridge basalts, but have a similar mean. This is borne out by observation – many seamounts and oceanic islands have average isotopic ratios similar to ridge basalts, but greater variance. In the presence of mantle inhomogeneity, it is thus unnecessary to invoke a separate, isolated yet accessible, ocean-island basalt "reservoir" (Anderson 2001b).

Many fluid dynamic plume simulations adopt the so-called Boussinesq approximation, Cartesian geometry and large core heat flows, meaning there is a symmetry between the upper and lower thermal boundary layers. Great pressure suppresses thermal expansion and the local Rayleigh number so thermal upwellings in the deep mantle are large, weak, sluggish and long-lived (Anderson 2001a). Most of the buoyancy, heat flow, conductive cooling and radioactive heating are concentrated in the outer layers of the Earth. The result is plate tectonics. The active surface boundary layer and associated mantle convection certainly overwhelm contributions from any deep thermal boundary layers which must be weaker. The fundamental physics of Earth is much more consistent with plate tectonics, mantle convection and magmatism being driven from the surface, and not by the deep interior. Plate tectonic forces not only drive the plates but can also break them, as can buoyant magma from below. This is an alternative to so-called hotspot tracks. This option is not available if the plates are rigid and permanent and the shallow mantle is isothermal and well below the melting point, assumptions of the plume hypothesis (Anderson 2001b).

It is the physics and the invalid assumptions, as much as observations, that make the plume hypothesis untenable. A more consistent hypothesis will have lithospheric and stress components and a heterogeneous, non-isothermal mantle, as expected from plate tectonics, and recycling of crust and lithosphere. When pressure is correctly taken into account, it is likely that mantle dynamics will prove to be a top-down system, organized by the tectonic plates and cooling lithosphere, rather than by plumes and core heat (Anderson 2001a). Magmatism, both at current and incipient plate boundaries, is a natural result of plate tectonics on an Earth-sized planet with a warm volatile-rich interior and a thin outer shell. ●

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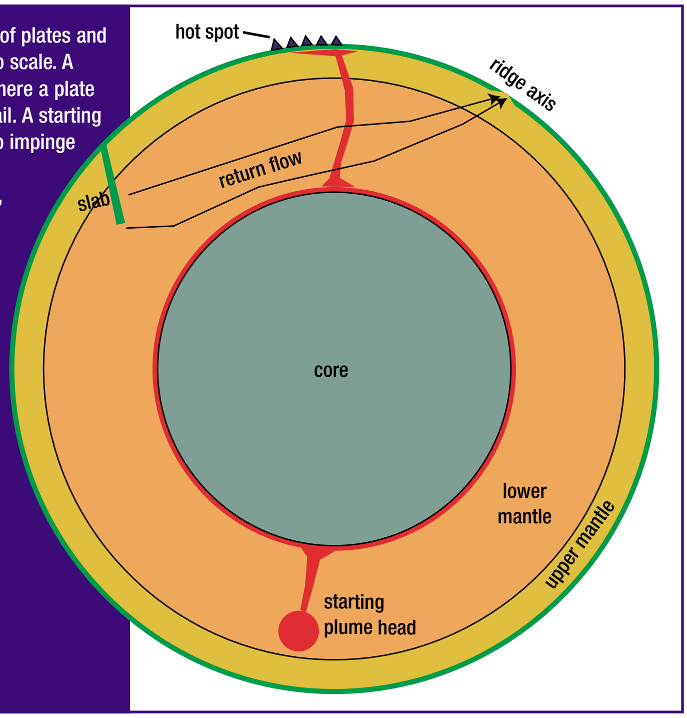
Mantle plumes?

Norm Sleep suggests that it is premature to toss the concept of mantle plumes into the dustbin. The hypothesis yields testable predictions about the geological phenomena of hotspots.



1: Schematic diagram of plates and plumes more or less to scale. A hotspot track forms where a plate moves over a plume tail. A starting plume head is about to impinge on the upper mantle. Currents in the mantle, shown simply as the return flow between a slab and a ridge, deflect the plume tail and its source, causing hotspots to move.

Prof. Gill Foulger is well justified in questioning the mantle plume hypothesis (*A&G* 43 6.19). Yet hotspots like Hawaii, Yellowstone and Iceland exist and Earth scientists need to find physical mechanisms for their presence. Compositional heterogeneities, complicated mantle flow patterns, and cracks through the lithosphere that let magma out are the usual non-plume suspects. I tried to find such alternatives early in my career (Sleep 1974, 1984, Solomon and Sleep 1974). The ad hoc nature of the initial plume hypothesis was my major motivation. The plume hypothesis can be modified in ways that it untestably represents any given hotspot. Conversely, local non-plume alternatives can be found one hotspot at a time. I praise Prof. Foulger for her tomographic work as the plume hypothesis here gives clear testable predictions.

I address her concerns by first summarizing the current plume hypothesis. I then answer a specific concern for which a physical explanation is available: the cold crust beneath Iceland.

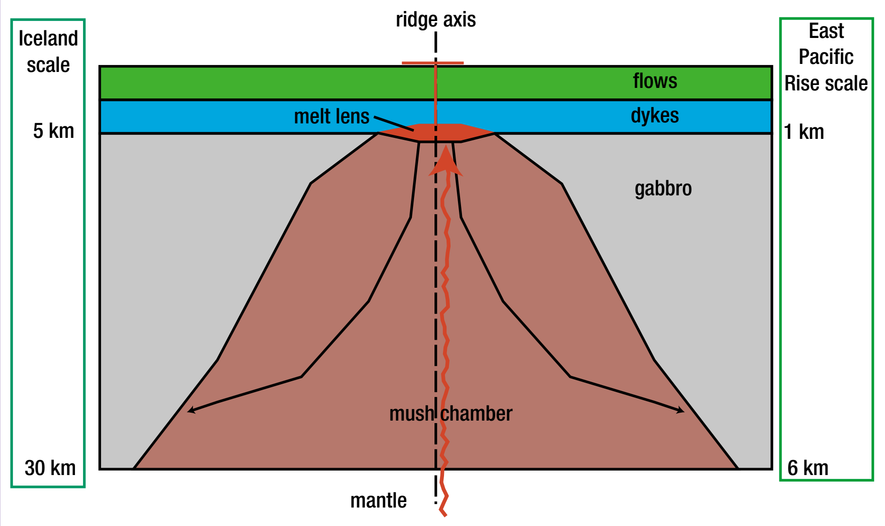
The mantle plume hypothesis

The plume hypothesis involves shallow and deep processes. I begin with figure 1, which shows mantle plumes and plate tectonics more or less to scale. Plumes arise as cylindrical conduits from the basal thermal boundary layer in the mantle.

The shallow part of the hypothesis is complicated by the lateral flow of plume material towards thin lithosphere, including the interaction of the plume material with ridge axes. The analogy of a hotspot track to the series of burns one would get slowly moving one's hand over a candle is initially helpful but in the end misleading. Plumes are a source of hot, buoyant material. Still, the thickness of the lithosphere and the presence of plume material beneath it are potentially measurable quantities. The fluid dynamics of the lateral flow, though complicated, can be modelled.

In particular, plumes begin with instabilities of the basal boundary layer and ascend as more-or-less spherical heads followed by tail conduits. The head ponds beneath the lithosphere and the buoyant hot material within the head spreads out beneath the lithosphere. This results in vast amounts of pressure-release melting over a geologically brief period of time.

Specifically, the arrival of a plume head is an attractive hypothesis for the North Atlantic igneous province at ~65 Ma (e.g. Sleep 1997). Once ponded, this plume material flowed buoyantly to zones of thin lithosphere. The kinematics and dynamics were similar to those of oil ponded beneath pack ice of various thick-



2: Schematic diagram shows the melt lens and gabbro glacier model of mid-oceanic ridge axes. Iceland (left side) and the East Pacific Rise (right side) differ mainly in scale. Melt ascends into the magma lens from the mantle. Some of it ascends to form dykes and flows (the detailed kinematics of this is not shown). The rest of the melt crystallizes at the base of the lens, forming a nearly solid mush. Flow lines go from the melt lens to the sides of the mush chamber.

nesses. For example, the Mesozoic thin, rifted lithosphere of the North Sea acted as a channel. The locally thin lithosphere of the Irish Sea acted as a catchment. The uplift centred on the present Irish Sea above this buoyant material produced a radial drainage pattern away from the Sea, including the Thames. Plume material flowed westward from the thick cratonal lithosphere beneath Greenland to the thin oceanic lithosphere beneath Baffin Bay. Once seafloor spreading started in the North Atlantic, plume material drained towards newly formed thin lithosphere, producing a volcanic passive margin.

After the arrival of the initial plume head, the plume tail continued to supply buoyant plume material at a modest rate. In general, the passage of plates over plume tails produces hotspot tracks just as Hawaii. The buoyant plume material ponds at the base of the lithosphere and tends to flow towards regions of thin lithosphere such as the ridge axis. In the case of Iceland, plume material now flows along the corridor of thin lithosphere at the ridge axis (Albers and Christensen 2001).

On-axis hotspots like Iceland, continental hotspots like Yellowstone, and oceanic hotspots like Hawaii, all result from the same underlying phenomena. One type of hotspot appears to evolve into another. The best example is that the on-land Montregian and New England track became the New England Seamount track after crossing the passive margin (Sleep 1992). Then it became the axial Corner Seamounts and the off-axis Great Meteor Seamounts.

Not part of the plume hypothesis

Foulger rightly doubts aspects to various properties that have traditionally been linked with plumes. Actually, plumes are not well understood and current thinking includes partly disjoint hypotheses as well as some excess baggage. That is, the hypothesis may be only partly right and is unlikely to be applicable to every site of feeble midplate volcanism.

First, the fixed hotspot hypothesis, like rigid plates, is a useful approximation, but one that has to be wrong. (For analogy, one does not refute plate tectonics by finding one earthquake in Sussex.) Dynamically, plume conduits and the plume source at depth advect with the rest of the flow in the mantle. This results in a well-posed fluid dynamic problem with testable predictions as the geometry of the "mantle wind" associated with the return flow from trenches to ridges are constrained (Steinberger 2002).

The depth from which plumes ascend is unknown and disjoint from evidence that plumes traverse the upper mantle. The D″ layer at the base of the mantle is the obvious suspect. The core accreted hot after the Moon-forming impact and the mantle has cooled over geological time. One thus expects that a significant boundary layer exists at the base of the convecting mantle. However, it is conceivable that a thick, chemically dense region exists in the mantle and that plumes nucleate above it (e.g. Davaille 1999, Kellogg *et al.* 1999). It is also conceivable that some plumes nucleate from or interact with the 660 km discontinuity between the upper and lower mantle.

Geochemistry, including He isotopes, is at best a local empirical tracer of plume material. The mantle is heterogeneous, composed of the remnants of subducted oceanic lithosphere, the residuum from previous melting events, old "primordial" regions, and entrained continental crust and sediments. The details are poorly understood and samples from modern lavas are poor indicators of the deep geometry.

Overall, the plume hypothesis involves deep dynamics near the limit of what can be resolved with currently tractable numerical models. It involves interaction with crustal processes, as described by Foulger in and around Iceland. Modelling of the self-organization of even normal plate boundaries is in its infancy (e.g. Tackley 2000a, 2000b) and it is premature to assume that this physics is inapplicable to on-ridge hotspots and plumes.

The probable position and flux of all but the strongest plumes is uncertain. Tomography provides a difficult way of finding plume conduits. Even in Iceland the geometry is not ideal. Few recorded seismic waves passed through the probable conduit below 400 km. Although frustrating, it is too early to claim that a conduit does not extend below that depth. The situation in the deep mantle is not sanguine. The plume conduit should be a low-velocity region and rays through it should not be first arrivals. The conduit is potentially detectable using waveforms including late scattered arrivals.

Cold Icelandic deep crust

I discuss the high seismic velocities and apparently cold crust beneath Iceland in some detail as its physical explanation is not in the literature and the data are reliable. To do this, I modify the standard model for fast ridge axes for the thicker oceanic crust beneath Iceland.

Normal oceanic crust is about 6 km thick (figure 2). The upper two layers are lava flows and sheet dykes. The deeper gabbro layer is of interest here. Countless multichannel seismic surveys have confirmed a conceptual model proposed independently by Sleep (1975) and Dewey and Kidd (1977). The only fully molten region is a thin (~10s of metres) lens between the gabbro layer and the dykes (e.g. Sinton and Detrick 1992). The gabbroic layer is near-solid mush.

The kinematics involves solid-state flow in the mush layer. Melt ascends from the mantle through the mush and into the lens. There some of the melt ascends to form dykes and flows and the bulk of the melt crystallizes to form cumulate gabbro at the base of the lens. The snow-like recharge of the mush layer at the base of the lens balances the material extracted from the sides of the lens by plate motions, similarly to the snow recharge area of a glacier. That is, the gabbroic mush moves deeper as it moves away from the ridge axis.

The Icelandic situation differs from fast ridges in that the lid is thicker, but more importantly that the crust and mush layer are thicker because of the vast amount of melt supplied by the plume. The gabbroic mush is near its solidus when it starts down away from the lens. It descends adiabatically and is about 100 K cooler than its solidus by the time it is deep in the mush layer. It thus appears as a cool, solid region to seismic waves.

In 3-D this process is more complicated, especially since there are ridge jumps and ill-defined transform faults. The processes that led to the current crustal thicknesses under Iceland are neither obvious nor incompatible with flow in gabbroic mush.

Summary

The plume hypothesis is somewhat vague because the underlying processes are still poorly understood and the deep structure of the Earth is poorly resolved. I am uncomfortable in having to argue partly from ignorance. Overall, plumes provide an explanation for hotspots and lead to quantitative testable predictions. A major difficulty is that surface processes obscure deeper ones. On one end, shallow hydrothermal circulation renders heat flow measurements virtually useless for mantle processes. On the other, seismic studies – especially those that use the full wave field – and fluid dynamic studies are most promising. The seismic methods resolve the testable predictions of the fluid dynamics.

Alternatives to plumes need to be subjected to similar scrutiny. In the case of Foulger's hypothesis, it is inevitable that the Iceland plume that impinged beneath a continent affected some region with a long and illustrious geological history. Several physical issues arise with respect to her non-plume hypothesis: can a hot buoyant zone persist since the Caledonian Orogeny without rising to the surface and spreading or being dispersed by plate-driven flow in the mantle? Are there similar nascent zones present beneath other orogens waiting to trigger hotspots? Can such a region supply enough material for the existing hotspot? How do such features relate to long-lived midplate hotspots, like Hawaii? ●

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In response…

Gill Foulger, author of "Plumes, or plate tectonic processes?", comments.

Prof. Sleep (p1.11) starts by encouraging scepticism of the plume hypothesis, and admitting that it is vulnerable to modifications that make it untestable. However, in common with the majority of literature on "hotspots" (which evidently are not hot; Stein and Stein 2003), his article in general implies a complete absence of doubt that plumes exist. Such an approach has been a major deterrent to fundamental questioning of the hypothesis for over three decades.

Sleep hypothesizes that hot, buoyant material rises in plumes and then flows laterally, guided by basal lithospheric topography into areas where the lithosphere is thin. He quotes uplift of the Irish Sea as an example. However, this interpretation is non-unique. When lithosphere is thinned, material must flow laterally to fill the space created. However, distant (~1500 km in the Iceland case), vertical conduits from the deep mantle are not required to deliver it.

The interpretation of geochemistry, including high helium isotope ratios ($^3He/^4He$), as an "empirical tracer of plume material" is also non-unique. At Yellowstone, where there is strong seismic and geological evidence against a plume (Christiansen *et al.* 2002), $^3He/^4He$ is also high. Viable alternative theories for upper mantle sources for high $^3He/^4He$ are available (Anderson 1998, Natland 2003). And high $^3He/^4He$ was originally attributed to plumes because it was observed at Hawaii, which was assumed to be underlain by a plume (still unobserved). The reasoning is circular. So firmly rooted has this model become that today plumes are proposed on $^3He/^4He$ values less than 1-σ above the mean, with essentially no other supporting data (Anderson 2001).

The gabbro mush model proposed by Sleep to explain why the Icelandic crust is relatively cold is also unsupported by observations. A pervasive layer of melt beneath the dykes would be a bright acoustic reflector, but no such reflector has been found (Foulger *et al.* 2002). Furthermore, gabbro containing a low degree of partial melt would have a high compressional-to-shear wave-speed ratio and high anelastic attenuation. Neither are observed (Menke and Levin 1994). Importantly, this model cannot account for the fact that primitive Icelandic basalts erupt at temperatures similar to those at spreading ridges where the crust is much thinner (Breddam 2002, Korenaga and Kelemen 2000). Neither can it account for the absence of high heat-flow in the ocean north and south of Iceland (Stein and Stein 2003). The Iceland hotspot is not hot, and no geophysical model, however sophisticated, can make it hot.

The only explanations offered for the lack of high temperatures, a time-progressive volcanic track, and seismic structure in the deep mantle (Foulger 2002) are *ad hoc* adaptations of the hypothesis. It is curious that the best defence of plumes, after over 30 years of study, is that they are not understood, cannot be seen, and have unobservable consequences. The lack of evidence for hot plumes is matched only by the lack of doubt that they exist. We are told that the hypothesis yields "quantitative testable predictions", but what exactly are these? Plumes have been proposed to come from almost any depth, to be stationary or move, to be long- or short-lived, to rise vertically or tilt, to have narrow or broad conduits, to have no plume head, a single head or multiple heads, to produce steady or variable flow, and to have high or low $^3He/^4He$. In stark contrast to the plate tectonic model, none of the original predictions of the plume model has been found to be true. What observations could conceivably cause the plume hypothesis to be rejected? If the answer is "none" then it is not a hypothesis, but a data-independent, *a priori* assumption.

Prof. Sleep concludes by highlighting major new questions implied by the shallow, plate-driven model that I proposed in my earlier paper. I welcome these remarks, and those of Prof. Anderson (2003) who explains the physical necessity for a shallow origin for Earth's volcanism. Significant, fundamental advances can be made only if new theories are applied and tested, rather than old ones progressively elaborated to match each new observation. ●

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