Discussion of

Origins of the plume hypothesis and some of its implications

by

Norman H. Sleep

12th November, 2006, Don L. Anderson

In contrast to Sleep I spent considerable time early in my career attempting to *prove* the existence of plumes and to rationalize the numerous paradoxes and failures of predictions. By 1987, I had found the plume hypothesis to be contrived and wanting (Anderson, 1981-1987) and had started to develop alternative models involving mantle heterogeneity, fertile diapirs ('chemical plumes') and an asthenosphere near or above the solidus. Early plume advocates dismissed alternatives but these were mainly propagating crack models, involving homogeneous cold mantle. The mantle was assumed to be isothermal and to require importation of core heat in order to melt, except at ridges and arcs. A complete theory of mantle magmatism requires treatment of both the lithosphere and the underlying mantle. If the mantle is assumed to be homogeneous and isothermal, and to be well below the melting point (standard assumptions in the plume hypothesis) then one's attention is naturally drawn to the lower mantle and core. But when the effects of pressure are considered (which Sleep ignores) then there are also problems with the deep thermal and bottom-up explanations for surface magmatism. Narrow thermal plumes do not spontaneously form in realistic non-Boussinesq mantles and one is forced to either just assume that they exist, as Sleep does, or to force them to exist by injecting hot fluids into a tank of fluid (in the absence of realistic pressure gradients).

Plumes originally met the minimum requirement for a scientific hypothesis in that they lead to testable predictions (Morgan, 1972). None of the predictions in this classic paper have been confirmed and most of the assumptions have been shown to be wrong or implausible. The predictions include the fixity of hotspots, the parallelism of volcanic chains, the dimensions of plumes, the number of predicted Hawaii-sized features, the global heat and magma fluxes, the temperatures of magmas, independence from surface features, driving forces of plate tectonics, origin of continental break-up forces, and the predicted uplift and local heat flow. The flux estimates in Sleep (2006), in fact, are below Morgan's acceptable range; "…the ridges would close up." Morgan also predicted the style of mantle convection; narrow hot upwellings were compensated by broad diffuse downwellings. This is the exact opposite of our current understanding of mantle convection; narrow downgoing slabs drive the plates and internal heating gives rise to broad diffuse upwellings. We now also know that many midplate volcanic features were formed at plate boundaries and were stranded by later plate reorganizations. The hypothesis has been repeatedly modified to satisfy each new observation and it is no longer possible to test the hypothesis or even to define plumes. Furthermore, linear island chains and age progressions are not a unique prediction of the plume hypothesis. More seriously, Sleep does not even attempt to address the physics, which appears to rule out plumes or to justify the strong heating from below that underlies the hypothesis. In Sleep's essay, the term "shallow fertile anomaly" could replace every use of "plume", with no contradiction with any observation. Where Sleep uses "hot" he could equally well have used "low melting temperature" or "fertile". Low seismic velocities are taken as unambiguous evidence for "plume" and evidence against delamination. In fact, delaminated eclogite, or cold peridotite with H_2O or CO_2 also have low seismic velocities. The fertile blob hypothesis also satisfies the apparent ability of melting anomalies to cross ridges.

It is instructive that recent attempts to identify 'real plumes' (Courtillot et al., 2003) do not use any thermal criteria (magma temperature, heatflow, precursory uplift, lithospheric erosion) or any of the predicted properties of Morgan (1972), but use only proxy criteria that have been assigned to plumes assuming that they are plumes. The criteria used, in fact, also apply to fertile blobs, and some apply to other or all shallow tectonic processes.

Although the original plume hypothesis was testable and falsifiable, the new versions are not. Sleep agrees that many of his selected observations have alternate explanations, that many 'hotspots' are not plumes, and that even his own calculations could apply to low melting point material, rather than thermal plumes. Since many observations such as volume flux, seismic velocity, uplift and composition (including helium isotopes) are satisfied by features that are agreed are not plumes (e.g. Courtillot et al., 2003; Anderson, 2005a) there is the danger that one can pick and choose among the many proposed 'hotspots' to find those that satisfy any particular set of criteria, and then to use these to prove the existence of plumes. Sleep focuses on Iceland and Hawaii, but these also have alternate explanations that involve tectonics, compositional heterogeneity, delamination, ponding and non-peridotite- or upper-mantle sources. There is no tomographic evidence that they extend into the lower mantle.

13th November, 2006, Norman H. Sleep

I thank Don Anderson for his comments. The intent of my paper is to lay out some features of modern plume theory and to point out the dearth and poor resolution of available data. That is, I made an argument from partial ignorance that it is premature to jettison the plume concept.

I agree that plume theory has evolved since the paper of Morgan (1972). I also agree that the deep processes that allegedly feed plumes are not well understood. I did not address this issue in my paper; it is partly disjoint from the existence of plumes at mid-mantle and sublithospheric depths. Seismic tomography and petrology provide direct evidence. The plume hypothesis is testable when such data become more reliable and better understood.

Current mid-mantle tomography is fuzzy, but does resolve low-velocity features in expected places. The possibility of small amounts of partial melt from minor components makes it difficult to relate seismic velocity changes to temperature changes. Current work certainly cannot resolve chemical from thermal features on the scale of plumes.

In general, petrology is a potent way to examine the chemistry and temperature of the rock that actually melts. However, petrological work has yet to produce agreement on the composition and source temperature beneath hotspots. Herzberg et al. (2007) provide an update. Lacking this agreement, I concentrated on the depth of melting beneath Hawaii and the volume of melting beneath Iceland. Higher temperatures from plumes have the correct sign and magnitude. My approach delineates the minimal requirements for a chemical alternative.

My main modification to the original plume theory is to concentrate on the implications of plumes as regions of hot buoyant upwelling fluid. Plume material is indeed a modest item in the global heat and mass budget. Flow driven by plates and slabs dominates. Plumes advect in the rest of the flow. Hotspot fixity is not a prediction of my form of the theory. Non-fixity is potentially predictable from fluid dynamics as is the deviation of plume conduits from vertical cylinders.

I concentrated on processes that affect bathymetry in the paper under discussion. Plume material ponds beneath the lithosphere and flows laterally. To be sure, a continuous supply of chemically buoyant material would behave similarly, but not identically. The sign of the slope of the base of the lithosphere is known near ridge axes and along passive margins. Dynamic calculations in three dimensions quantify lateral flow. I chose the Hawaiian swell as a resolved feature. Tomography might resolve the structure near the base of the lithosphere and hence chemical versus thermal differences.

Yes, an advocate of plumes must pick and choose. That is, one needs to use data to tentatively sort out primary hotspots underlain by plumes, secondary hotspots from lateral flow away from plumes, and low-volume volcanoes that tap sources at ambient mantle temperature. Right now, bathymetric tracks and lineations meagerly constrain speculation. Many submarine edifices are still unsampled and undated.

24th December, 2006, Geoffrey F. Davies

A complementary presentation of arguments relating to the existence of mantle plumes is given by Davies (2005). I quote the 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.*"

I have no significant disagreement with Sleep's presentation. I just think the inference of some plumes from observations is more direct and straightforward than is perhaps implied by him. Also the basic physics of thermal plumes is by now well understood and well quantified, with a significant number of predictions being quantitatively confirmed by observations, contrary to a number of claims in the present volume. A summary of important predictions and relevant observations is given by Campbell and Davies (2006).

1st January, 2006, Don L. Anderson

Sleep develops a passive version of the plume hypothesis that parallels in many respects the fertile blob model. Hotspot fixity is not a prediction of his form of the plume theory. Plumes advect in the rest of the flow, pond beneath the lithosphere and flow laterally. These are the same as predictions of the fertile blob model. However, blobs are basically upper mantle features; if they are entrained in a broad asthenospheric counterflow channel, then they will drift counter to plate motions and will appear to define a fixed reference system, for each plate. Plumes have more complex trajectories.

Compositional and melt-induced buoyancy not only substantially complicate the plume hypothesis, but can potentially remove the perceived need for plumes. The melting point of fertile mafic blobs is about 200°C lower than peridotite, about the same magnitude as required plume excess temperatures. Realistic convection simulations with large plates, internal heating and temperature-dependent properties raise the background temperature of the mantle, again lessening the need to import high temperatures from deep in the mantle. The delaminated-lithosphere hypothesis is different from the delaminated-crust hypothesis. Ambient mantle upwells to replace the material that delaminates. Its source temperature is that of the underlying asthenosphere but this need not be the same as MORB temperatures at spreading ridges. Mantle under thick, long-lived plates can be hotter than the shallow mantle under mature ridges.

One source of fertile blobs is crustal delamination. Overthickened basaltic crust does not just form in mountain belts. It can form in island arcs and at ridge-ridge-ridge triple junctions where 3D focusing of magma occurs. Oceanic plateaus, with thick crust, may form at such places. Underplating and ponding may also be involved if extrusion is prohibited by the stress state of the lithosphere. Subduction of seamount chains and aseismic ridges also introduce thick mafic sections into the mantle. By contrast, the plume hypothesis assumes that the upper mantle is homogenous, cold and roughly isothermal so that excessive melting requires importing high absolute temperature. In the delamination- and fertile-blob hypotheses, melting anomalies result from increased fertility or homologous temperature rather than excess absolute temperature. The fertile blob hypothesis makes different predictions about mantle temperature, heatflow, and uplift timing compared with the thermal plume hypothesis, but many of the effects of a buoyant blob are the same as those of a hot plume head. Mafic blobs may be tens of km in dimension and therefore differ from pyroxenite-vein or marble-cake models.

Tomography is unlikely to resolve the hot plume vs. fertile blob issue. Both give negative shear-velocity anomalies and it is agreed that the tails, if any, are unresolvable. Poisson's ratio and attenuation may be able to tell the difference, and there are already some claims in this direction. One needs more than the sign of the velocity anomaly. Delaminated crustal material or subducted aseismic ridges at near-ambient temperatures will also have low seismic velocities.

The excess fertility of blob material can explain voluminous magma production. Changes in lithospheric stress, and the presence of a blob, can explain the sudden outbreak of magmatism in previously quiescent regions. Pre-existing plate boundaries and fracture zones, underlain by subducted seamount chains or delaminated arc crust, explain most hotspot tracks. Others can be attributed to the stress state of the plate or to incipient plate boundaries. The absence of hotspot tracks leading away from most large igneous provinces is then easy to understand.

2nd January 2007, Dean C. Presnall

Sleep mentions that temperature is a critically important variable in the plume debate, and petrology should be able to help. As several major plume candidates (Iceland, Azores, Tristan, Galapagos, Afar, Easter, Bouvet) lie on or very close to ocean ridges, tight constraints on temperatures of MORB generation along ridges would settle at least a major part of the controversy. Unfortunately, petrologists have been debating the temperature and depth ranges of MORB generation for about 40 years, with no significant convergence of conclusions.

The MORB model that is probably the most widely accepted (Klein and Langmuir, 1987) assumes a very wide range of magma-generation temperatures, and this has nourished the concept of hot plumes. However, in a global reexamination of MORB glass compositions, Presnall and Gudfinnsson (submitted) found that the model of Klein and Langmuir is not supported by data from any ridge segment. Instead, the systematics of the Na_8 and Fe_8 parameters used by Klein and Langmuir (1987) are beautifully consistent with solidus phase relations in the CaO-MgO-Al_2O_3-SiO_2-Na_2O-FeO-Fe_2O_3 system at a globally uniform potential temperature of ~1240-1260 ˚C and a pressure range of ~0.9-1.5 GPa (Presnall et al., 2002). As this 7-component system contains all the major minerals in the mantle and about 99% of the composition of both the source (lherzolite or some mixture of lherzolite and basalt) and the extracted basaltic melts, it provides constraints that are particularly robust.

For all ridges, including Iceland, the modeling of Presnall and Gudfinnsson (submitted) replaces large potential temperature variations and hot plumes with mantle heterogeneity and uniformly low temperatures of magma generation.

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