

Discussion of

Implications of lower mantle structural heterogeneity for existence and nature of whole mantle plumes

by

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25th December, 2006, Don L. Anderson

*“If someone points out to you that your pet theory of the universe is in disagreement with Maxwell's equations — then so much the worse for Maxwell's equations. If it is found to be contradicted by observation — well, these experimentalists do bungle things sometimes. But if your theory is found to be against the second law of thermodynamics I can give you no hope; there is nothing for it but to collapse in deepest humiliation.” (Sir Arthur Stanley Eddington, *The Nature of the Physical World*, 1915.)*

Most scientific paradigms survive until a better paradigm comes along. Rarely, a paradigm is abandoned because some overlooked physics shows that it is impossible, or improbable. The concepts of Ptolemy, aether, phlogiston and caloric, the most famous of the classical physics paradigm shifts, occurred when the concepts became so contrived and convoluted that people lost interest. But an idea that violates thermodynamics should be abandoned immediately. Seismological studies paired with fluid injection experiments and Boussinesq simulations cannot answer the question of whether the lowermost mantle is a plausible source for surface hotspots. Low velocities, high velocities, ultraslow velocities and anisotropy have all been used as ‘evidence’ for plumes (see references Garnero et al., this volume). Plumes have been proposed to emanate from the tops of superplumes, rather from D”. We have also been told that lack of clear tomographic evidence for plumes in the lower mantle is due to lack of resolution or coverage. Now we are told that the most likely locations of mantle plumes are over the boundaries between slow and fast regions. There is no way that the plume hypothesis can be falsified with these many and contradictory options. So much for the observational aspect. The commonly used Boussinesq ‘approximation’ for mantle convection is not as soundly based as Maxwell’s equations and it certainly ignores thermodynamics.

A thermal boundary layer (TBL) at the base of the mantle certainly exists, but this is not a sufficient condition to form deep narrow plumes as currently envisaged. A TBL serves to conduct heat out of the core but whether it can form sufficiently buoyant plume heads to break out of the lower mantle, or develop 100-200-km-dimension plume tails in a reasonable amount of time depends on the material parameters, which depend on composition, pressure, core heat, mantle radioactivity and convective vigor. If the local Rayleigh number is less than about 1000,

the TBL will not go unstable. If the coefficient of thermal expansion is low, the TBL may never develop enough buoyancy to escape. When pressure is taken into account, the dimensions of buoyant instabilities are of the order of thousands of km, not hundreds of km. If the intrinsic density of D” is as little as 1% higher than the rest of the mantle, it will be permanently trapped, but this does not mean that it will have a simple structure. It is surprising that none of the discussions in this volume that argue for deep mantle plumes mention the effects of pressure on material properties, and the ability of internal heating and background mantle convection to prevent or destroy the instabilities.

As long as heat is flowing into the base of the mantle a TBL should be present, but the ratio of core heat to mantle heat and the local (pressure dependent) Rayleigh number are the key parameters, not the mere existence of a TBL. Cylindrical plumes in numerical and laboratory experiments usually involve localized basal heating, the instantaneous creation of a hot sphere, or basal injection of fluid; pressure and background convection effects are ignored and thermal effects exaggerated (see, for example, the chapters by Sleep and King, in this volume). The “expected small plume conduit dimension (e.g., < 500 km)” is based on experiments that ignore pressure effects or that impose the dimension (“experimentalists do bungle things sometimes”).

The smoking gun against deep mantle plumes is thermodynamics. All the critical thermodynamic parameters depend on volume, but these are ignored in all calculations that yield narrow whole mantle plumes (the Boussinesq ‘approximation’). None of the fluid dynamic calculations used by the authors to support their view take into account the effect of pressure on thermal expansivity and its role in chemically stratifying the mantle and stabilizing deep thermal structures. The few non-Boussinesq calculations that have been done do not use self-consistent thermodynamic relations, but even so, they do not predict plume-like dimensions and time scales at D” depths. At upper mantle pressures, thermal expansivity is high and chemical stratification is reversible. On the other hand, simple scaling relations show that pressure increases conductivity and viscosity—and spatial and temporal scales—and decreases the coefficient of thermal expansion and the local Rayleigh number. Even slight compositional effects can make deep dense layers permanent, and complex.

27th December, 2006, Geoffrey F. Davies

The factor that invalidates Anderson's claims is the temperature dependence of viscosity. It is the reason plumes form head-and-tail structures, as illustrated in Davies (1999). The effect is actually considerably stronger at higher pressure, because the activation enthalpy may be two or three times larger at the base of the mantle. Plume heads are large mainly because they have to displace high-viscosity surroundings in order to rise, but plume tails can be narrow because the lower-viscosity plume fluid can flow up a pre-existing path. The other factors Anderson mentions have some effect but are not dominant.

The problems with Anderson's arguments were detailed in Davies (2005). Either Davies'

criticisms should be refuted or Anderson's arguments should not be repeated, because they appear to be quite invalid.

By the way, plenty of plume models have been done with uniform basal heating, see for example Davies (1999) and Leitch and Davies (2001).

29th December, 2006, Don L. Anderson

I thank Geoff Davies for providing these references and allowing me to clear up possibly confusing points. He has put his finger on the essence of the issue separating those who think mantle plumes are obvious and inevitable from those who remain skeptical of their physical basis. The issue regarding the temperature dependence of viscosity is not straightforward and is regarded as a paradox (e.g. Nataf, 1991; Lenardic and Kaula, 1994). When this effect is taken into account—in its entirety—the upper TBL becomes thicker, making the upper mantle hotter, and the lower mantle acquires a negative or subadiabatic temperature gradient; the lower TBL becomes *colder*. Melting is more likely to occur in the upper mantle, and to greater depths, than is the case with constant viscosity. Cavity plumes are less likely to form at the base of the system unless the boundary layers interact.

The temperature dependence of viscosity is a two-edged sword. If applied to the lower TBL in isolation it would seem to make cavity plumes more likely. But it also makes the upper TBL stiffer, longer-lived and with a larger temperature drop. When this goes unstable it cools the lower mantle and the lower TBL, making them stiffer (Lenardic and Kaula, 1994). Much of the lower mantle develops a subadiatic or negative temperature gradient because of internal heating and slab cooling, increasing the viscosity with depth. Ironically, it has been argued that hot plumes from the deep TBL will heat and thin the plate, but sinking of the cold surface TBL and cooling of the base of the system, the parallel effect, has been ignored and may be more important, *because of the temperature dependence of viscosity!*

When the possibilities of melting and differentiation are allowed for in a convection calculation, the mantle can become chemically stratified (Tackley and Xie, 2002). The various components, e.g. eclogite and refractory residue in this case, collect or re-collect at levels of neutral buoyancy and survive or regenerate there for billions of years (see also Anderson, this volume). Buoyant material in the shallow mantle also extends the surface TBL, making the upper mantle hotter still. Chemical layering is facilitated by pressure dependence of thermal properties and reduces the Rayleigh number and the vigor of convection, particularly at depth.

The equations in Davies (2005) regarding the effect of temperature and pressure on viscosity are identical to those in Chapter 7 of *Theory of the Earth* (Anderson, 1989). There is no disagreement there. The standard Arrhenius form and the various terms—the pre-exponential, the activation volume and the activation energy—are derived. It was determined (Anderson, 1989, page 133) that the viscosity should increase by about a factor of 60 to 80, due to *compression*

across the lower mantle, *at constant T*. The total variation across the mantle involves a large decrease due to the temperature rise in the upper mantle and a possibly smaller decrease at the lower TBL. The mid-mantle effect is uncertain. The viscosity jump across discontinuities may be negative.

As Davies points out, there are many calculations in the plume literature of uniform bottom heating; also of localized heating and injections of hot fluid. If we have only bottom heating, no pressure effects, and no radioactivity, then plumes are inevitable if the heating is strong enough; the core is the only heat source in this model and its heat is removed by plumes. But plumes, plates and convection, and D'', should not be treated separately (Lenardic and Kaula, 1994). The papers by Kaula (1983), Lenardic and Kaula (1994), Nataf (1991), Tackley and Xie (2002) and Tozer (1973) collectively make the point that one cannot treat one variable (viscosity), one parameter (temperature), one region of the mantle (D''), one mode of heating/cooling (core heat) or one boundary condition, independently; the whole parameter space and system must be treated together, in a self-consistent way. This is the nature of thermodynamics and far-from-equilibrium systems, such as convection.

All simulations of narrow plumes involve injection of hot fluid through a circular orifice, uniform or localized heating from below, or the instantaneous creation of a hot sphere as the initial condition; plumes and their dimensions are imposed by the investigator, rather than being natural fluid dynamic instabilities in a realistic setup that resembles the mantle. In other studies, a plume is just assumed to exist and its properties are investigated (several papers, this volume).

The five remarkable papers cited above, mostly overlooked, plus *Theory of the Earth* (Anderson, 1989) form the basis of the present discussion. Petrology and self-consistent fluid dynamics appear to explain the thick average TBL thickness (280 km) and the mean mantle potential temperature ($1410 \pm 180^\circ\text{C}$) derived by Kaula (1983) from geophysical and plate tectonic data. These exceed expectations from cooling of a homogeneous fluid or the temperatures of MORB. These temperatures and depths are consistent with 'hotspot' magmas being derived from within or just below the surface TBL, but do not rule out the existence of fertile blobs (Anderson, this volume; Beutel and Anderson, this volume),

30th December, 2006, Geoffrey F. Davies

Anderson's points are either invalid, confused, or make little difference.

Upper thermal boundary layer

A temperature-dependent viscosity can stiffen the top TBL, make it thicker and raise the internal temperature. However this is only true if the TBL becomes immobile (stagnant lid regime). The mantle's top TBL is mobile, because it is broken into moving plates that can subduct. In this case Anderson's arguments do not apply, though they would in any case only change the details, not

the general principles described below. Besides, how could you reconcile a 280-km-thick TBL with seismological constraints on the thickness of the oceanic lithosphere, and with seafloor subsidence and heat flow? I think you can't.

Subadiabatic gradient

It is well known that the vertical temperature gradient between TBLs is subadiabatic, regardless of heating mode or the nature of the top TBL. Recent estimates are that the temperature is 100-300°C lower than adiabatic near the bottom of the mantle. This will raise the viscosity significantly relative to an adiabatic profile, as Anderson says, but it is only part of the uncertainty in deep mantle viscosity, as I will discuss below.

Bottom thermal boundary layer and plume formation

The viscosity and temperature above the bottom TBL are not the only determinants of whether plumes will occur. The other major determinant is the temperature at the core-mantle boundary, which changes only on billion-year timescales. The minimum viscosity in the TBL occurs at this boundary, and is likely to be 2-5 orders of magnitude lower than the overlying ambient mantle, depending on the temperature increase through the boundary layer, commonly estimated to be 1000°C or more. Even a 500°C increase is ample to generate plumes (Leitch, 2001).

The role of the viscosity above the TBL is to control the timing and size of an instability that can begin to rise – the larger the viscosity, the larger the blob must be before it can detach (Griffiths and Campbell, 1990; Davies, 1999). Once a blob is detached and rising the behavior of material following from the TBL depends on its viscosity which, as noted above, depends on the core-mantle boundary temperature, not the temperature above the TBL. Since it is likely to be 2 or more orders of magnitude lower in viscosity, it will form a narrow conduit, as noted in previous discussion and demonstrated by Davies (1999).

The viscosity above the TBL could be changed by 1-2 orders of magnitude without changing this general behavior. Only quantitative details would change, such as the exact dimensions of the initial head and the following tail.

If the lower mantle were cooler and more viscous than we have thought, as Anderson advocates, then the temperature difference between the mantle and core would be greater, which would cause stronger plumes. The higher viscosity would mean the plume heads took longer to develop and would be larger.

Viscosity increase through the lower mantle

Anderson's discussion of the depth dependence of viscosity seems to be confused. Mid-mantle viscosity is constrained by post-glacial rebound and subduction zone geoids to be around 10^{22} Pa s (e.g. Mitrova, 1996). Deep mantle viscosity is unlikely to be much more than an order of

magnitude greater than this, or it would affect the rebound or Earth's rotation noticeably.

I don't know how Anderson gets an isothermal increase of viscosity by only a factor of 60-80 over the lower mantle from his formulas. His $(\partial \ln \eta / \partial \ln \rho) = 40-48$ (η is viscosity and ρ is density) and a density increase from 4400 to 5500 kg/m³ through the lower mantle yields an increase by a factor of 7500-44000. This is larger than the observational constraints seem to permit, so evidently these formulas are not good guides (e.g. Davies 2005).

Anderson's isothermal increase by a factor of 60-80 would seem to imply little increase or even a decrease along the actual (subadiabatic) mantle temperature profile, which would not serve his cause of suppressing mantle plumes.

Anderson's formula for viscosity on p. 133 of his book (Anderson, 1989), to which he refers, is not correct: viscosity $\sim (G/\sigma)^n$, $n = 1-3$ (σ is deviatoric stress). If G is the rigidity (shear modulus) as used on the previous page, then this ratio is dimensionless. Presumably Anderson meant: strain rate $\sim (\sigma/G)^n$, which yields: viscosity $\sim (G^n/\sigma^{(n-1)})$. Even so, this is only one factor in the full expression for strain rate (see equation 6.10.3, Davies, 1999). G is a useful scaling factor, not a rigorous predictor. Similarly, Anderson's equation relating activation volume to the depth dependence of G on the previous page is no more than a rough, possibly very rough guide, given the sensitivity of viscosity to activation volume.

Uniform bottom heating

Anderson lumps uniform bottom heating in with other types of boundary condition and repeats his claim that modelers' boundary conditions have pre-determined plumes and their dimensions, rather than leaving the fluid dynamics to determine the outcome. Let it be clear: uniform bottom heating does not determine plume dimensions or other characteristics – it leaves that to the fluid dynamics.

The amount of bottom heating does predetermine the occurrence of plumes, it is true, and that is because the physics requires them under the conditions prescribed. Those conditions are, in the better experiments (laboratory or numerical), tailored to the conditions near the core-mantle boundary, as best we understand them. Anderson has yet to make a persuasive case that those conditions might be very different.

Anderson does a disservice to modelers by claiming or implying that other factors have not been considered, factors such as changes of properties with depth, radioactivity, the existence of another TBL, and so on. They have. If one understands the physics one can sensibly understand the usefulness and relevance of the models.

If the Hawaiian hotspot chain did not exist, and if several hotspot chains did not emerge from flood basalt provinces, there might be some point to the debate, but models of thermal plumes give a good quantitative account of those phenomena. I do not, however, claim that thermal

plumes can explain everything (see Davies, 2005).

Furthermore, it is not true that modelers have not included a vertical viscosity gradient in their lower mantles – they have.

General comment

The physics of thermal convection, and of plumes in particular, is well understood and not too hard to follow. Readers are referred to Davies (1999). The points made here have been made before. The productive debates have moved on to other things, such the role of compositional variations and the influence of subduction.

31st December, 2006, Don L. Anderson

What the general reader of these pages wants to know is; does fluid dynamics prove that mantle plumes must exist, and does it rule out alternative explanations for melting anomalies? Davies and I both agree that it does neither. Are self-consistent, self-organized simulations better than ad hoc parameters and tightly constrained experiments? Yes. Does the newly found complexity in D” imply that the source of plumes has been found? Is the mantle almost entirely heated from below? Of course not. When we look at volcanoes can we ignore the lowermost mantle? Are the early arguments, assumptions, predictions and experiments for plumes still valid? Is it possible that the shallow mantle is hotter and more variable in fertility and melting point than generally assumed? Here, Davies and I deviate.

The best way for the non-specialist to understand these issues is to look at the series of calculations published by Tackley, Bunge, Phillips, Lowman, King, Gurnis and their groups (see references) who demonstrate the extreme sensitivity to initial and boundary conditions that is characteristic of non-linear chaotic systems. These investigators usually start with the simple case discussed by Davies and his colleagues Leitch and Campbell (hereafter DLC) – bottom heating with constant properties. Nice little mushroom forests appear, as expected. Then radioactive heating is introduced, and temperature and depth dependent properties, and then, continents. The mushrooms disappear and the mantle heats up and becomes unsteady. Then melting and differentiation are introduced. Layers and blobs appear. Then large plates and large-aspect-ratio convection cells are allowed. The mantle gets hotter still and fusible blobs melt. Then secular cooling is thrown in. The whole system changes as each new element is introduced, which is the nature of self-organized, far-from-equilibrium or chaotic systems. The system also flips spontaneously from one state to another.

The plates may control mantle temperature. In some cases, the system jams and heats up, and one has to crack the plates to allow subduction, and the whole mantle reorganizes again. The lower TBL is cooled if the plates sink to the bottom. Modelers who have got this far don’t much discuss narrow hot stationary long-lived or radial plumes. The mantle is plenty hot, fertile and

variable so that many alternative explanations of melting anomalies are now on the table. The claims that narrow plumes are an inevitable result of a TBL, that plumes are the way the core gets rid of its heat, and that hotspots are independent of other forms of mantle convection and plate tectonics, have not received numerical validation, although calculations can easily be designed that satisfy these claims.

Delamination of the lower part of the plate, not yet included in any global simulation, can also fertilize and cool the mantle. When delamination occurs, warm asthenosphere rushes up to fill the gap and we get a volcano. When the delaminated blob heats up, it melts, and we can get another volcano or volcanic chain. The more realistic the fluid dynamics gets, the more plausible are layering, delamination, shallow return flow and fertile blob mechanisms. In the extreme case, it is the outer shell that is the regulator, not mantle viscosity or even temperature.

These points are well illustrated in Phillips and Bunge (2005), Tackley and Xie (2002), Parmentier et al. (1994), Grine et al. (2005) and Nakagawa and Tackley (2006). These are Realistic Mantle Simulations (hereafter RMS) performed in wide boxes or spherical shells that can self-organize. These papers show how the various effects (internal heating, plates, continents, melting, layering, secular cooling, 3D) affect the background temperatures and mantle motions that plumes must endure. Not surprisingly, these studies do not validate the results, assumptions and boundary conditions in DLC. Plumes were initially proposed because of perceived shortcomings in the simplified convection and tectonic models existing at the time. It was then thought that the lower mantle was rigid or with very high viscosity, and that plates were rigid.

Bottom heated, axisymmetric, and injection experiments (DLC) have been useful for understanding idealized thermal plumes. Indeed, it was these studies that caused the wider community to embrace the plume idea. Plumes were an elegant and easy-to-understand solution to mid-plate volcanism. However, it is a disservice to the wider community to imply that plumes must always exist, and that they provide the only explanation for Hawaii etc., particularly since more realistic calculations (laboratory experiments cannot cover the appropriate conditions), plus lithologic heterogeneities, provide alternative ways to form hot mantle and melting anomalies. For example, if the mantle is only 100°C hotter than assumed in the plume literature, and the melting point, of blobs, is 200°C lower, then plumes are not needed.

Realistic convection simulations confirm that the mantle runs hotter and more episodically than homogeneous or bottom-heated models (Phillips and Bunge, 2005). Fe-enrichment in D'' means that excess plume temperatures must be much higher than DLC assumes, or otherwise material cannot rise out of the region. Pressure-dependent properties do the same thing. Decreased thermal expansion coefficient, and increased conductivity do not automatically preclude plumes, but the intrinsic density of D'', its thickness, and its buoyancy parameter, can preclude their escape. The required temperature contrasts may become larger than available. Even a 1-2% density excess may stabilize D'', but large structures are required to generate the buoyancy needed to make plumes rise.

Plate tectonics involves recycling and the introduction of low-melting point constituents into the mantle, forming layers and blobs. A stiff outer shell prevents magmatism, except where it is permitted by extension or delamination, and it also causes the temperature to rise. In other words, melting anomalies, the very reasons that plumes were introduced in the first place, are potentially explained by models with realistic properties and without plumes. An internally heated mantle, cooled from the top, will have large rising regions due to slowly developing buoyancy and displacement by sinkers, but these are not plumes as conventionally defined; they are the normal convection that plumes were invented to augment.

There are still surprises out there, as one expects in self-organized far-from-equilibrium systems that are allowed to do their own thing. A small change (one crack, one continent) can change everything, as elegantly shown by Gurnis, Bercovici, Phillips, Bunge, Tackley, Lenardic, Lowman, King, Hansen, Conrad, Hager and their collaborators (see references on www.MantlePlumes.org). These authors have repeatedly reminded us that mantle convection is not only a branch of chaos theory but also a branch of thermodynamics. This means that all the parameters and boundary conditions are interconnected and that self-consistency is essential. RMS studies show us that self-consistent models run hot and unsteadily, and have large plates and large aspect ratio convection. A single injection or axisymmetric experiment simply cannot be so generalized; too many degrees of freedom have been removed. Until the RMS calculations were done we did not even ask if the mantle organized the plates or the plates organized the mantle, or if things oscillated. In bottom-heated cylinders, this is not even an issue; one knows what will happen, and where.

It is interesting to note the progression in geodynamic thinking, from lower-boundary control (bottom heating, plumes) to mantle self-regulation (the Tozer effect) to control by the plates (plate bending, top-down tectonics). The top boundary condition, ignored or simplified until recently, may act as a template but may also organize and drive mantle convection, and localize magmatism. Fretting about details of D” may be beside the point for volcanoes if the upper mantle is as hot and variable as realistic simulations suggest.

The RMS papers and associated figures can be found conveniently by inputting the search string `author+convection` into Google or Google Images.

31st December, 2006, Ed J. Garnero, Thorne Lay, Allen McNamara

We thank Don Anderson for his comments on our paper and Geoff Davies for his responses. First, let us reiterate that the purpose of our paper was to consider (not to prove or disprove) the possible connections between recent deep-mantle seismic findings, state-of-the-art numerical geodynamical calculations, and geographical systematics for upwellings and plume initiation. We drew attention to the remarkable seismic evidence for “sharp edges” to the large low-shear-velocity provinces (LLSVPs) in the deepest mantle (note our explicitly dynamically-neutral

terminology), which cannot be attributed solely to temperature gradients. Thus, a distinct chemical component to the LLSVPs is highly likely. This motivates consideration of implications of thermo-chemical boundary layers for plume initiation in contrast to the standard isochemical thermal boundary layer behavior discussed in most plume scenarios.

While we discussed studies that advance the interpretation that LLSVPs are “superplumes” that rise in the mantle due to intrinsic thermal or chemical buoyancy, we do not advocate such models. The fact that LLSVPs are situated away from beneath past/present subduction locations is consistent with subduction-related currents sweeping the LLSVP material into “piles”, and maintaining their strong lateral margins (between LLSVP material and adjacent non-LLSVP mantle). In this scenario, the deep mantle must have significant convection currents. We appeal to a density increase of the LLSVPs, as suggested by a few seismic studies, recognizing that this is an issue of debate. Thus, our paper takes the perspective that LLSVPs are relatively stable deep mantle features with configurations sustained by the past few hundred million years of mantle circulation. Thus, some connection between deep mantle and surface structures is worth examining.

Geographically correlating phenomena at the top and bottom of the mantle certainly leaves one wanting better constraints of structure in between the ends of the mantle. The reduction of seismic resolution in the mid mantle (compared to the upper and lower boundary layers) cannot be used for or against any favored hypothesis. Nonetheless, it is significant that commonly designated hotspots populations are twice as likely to overlie the regions of strongest D” lateral shear velocity gradients than regions with the lowest velocities, and far more likely than for regions with high velocities (Thorne et al., 2004). The strongest shear-velocity gradients in tomographic studies are coincident with LLSVP edges; thus, hot spots are observationally more likely to be situated above LLSVP edges. We recognize the difficult issue of what one calls a hot spot (e.g., Courtillot et al., 2003), but that is beyond the scope of our current paper. The thermo-chemical geodynamic calculations driven by historical subduction patterns result in configurations of large chemical piles in close accord with seismic observations, and predict concentrations of thermal boundary layer instabilities on the margins of the piles, as hot boundary-layer material is swept up onto the pile edges. Thus, the seismic observations, geodynamic models, and crude correlations with surface phenomena give a provocative new perspective on how deep mantle heterogeneity may plausibly influence some surface volcanism.

Anderson points out that the geodynamic calculations we consider involve approximations, in particular, that pressure dependence of the coefficient of thermal expansion is neglected in the Boussinesq approximation. Numerical thermochemical models with compressibility and pressure-dependent thermal expansion have been conducted (e.g. Tackley, 1998; Hansen and Yuen, 2000) which show qualitatively similar results as our calculations; specifically, thermal plumes rising from thermochemical piles. As Anderson notes, the length scales of thermal instabilities tend to be larger in such models than in constant-thermal-expansion calculations. But, for systems at least partially heated from below, decreased thermal expansion coefficient does not intrinsically preclude the development of plumes; the calculations yield fewer, larger

plumes (Davies' commentary on this is also quite relevant). The calculations that we report suggest that deep mantle thermal instabilities will be geographically rooted near LLSVP margins. When it becomes viable to compute fully thermo-chemical spherical convection models with depth-dependent parameters, our expectation is that the configuration of upwellings will remain the same, only with larger scale plume initiation at LLSVP margins. This will need to be explored in the future. The destabilizing effects of post-perovskite phase transitions must also be considered, as this increases the potential for development of boundary layer instabilities, possibly countering any inhibiting effects of pressure dependence. However, at this time, the calculations have not been done, so speculation is appropriate for commentary, not for inclusion in a paper. We do note that some of the speculation in Anderson's comment appears at odds with the experience of actual numerical calculations, so his assertions about the dynamics are also premature.

Finally, realistic simulation of mantle circulation is a long-term research objective in geodynamics: all current models make some simplifying assumptions based on either computational considerations or lack of constraint on various thermodynamic parameters. There is often a resulting disconnect between practitioners who compute numerical models and geophysicists who speculate on plausible complexities not incorporated into those models. Similarly, there are large uncertainties in relating findings from different disciplines, as in our case of exploring interpretations of seismic observations by geodynamic models constrained by plate tectonic histories. Any claims of uniqueness would be laughable, but arguments by assertion without computational validation are of little merit as well.

31st December, 2006, Norman H. Sleep

I wish to comment on two of Anderson's points. (1) The thermal expansion coefficient in the deep mantle is so low that convection heated from below would be sluggish, and (2) any chemical density stratification would overwhelm thermal expansion.

With regard to the thermal expansion coefficient, it useful to review the parameterized convection equation. The convective heat flow from a thin boundary layer is

$$q = Ak\Delta T \left[\frac{\rho g \alpha \Delta T}{\kappa \eta} \right]^{1/3} . \quad (1)$$

where k is thermal conductivity, ρ is density, g is the acceleration of gravity, α is the thermal expansion coefficient, and κ is the thermal diffusivity. The dimensionless multiplicative constant A is of the order of 1; it depends on the boundary condition at the core-mantle boundary or the interface between "dregs" (chemically dense regions at the base of the mantle with no geometry or origin implied) and normal mantle. ΔT is the temperature contrast that actually drives convection and η is the viscosity (a weighted average between the low-viscosity hot boundary layer and the conducting interior) (Davaille and Jaupart. 1993a, 1993b, 1994; Solomatov, 1995;

Solomatov and Moresi, 2000). The product $\rho\alpha$ rather than just α occurs in the equation, which partly offsets the effect of the decrease of thermal expansion with pressure. In addition, this product is raised to the 1/3 power making the heat flow insensitive to its value, provided it does not become zero or negative. To obtain the long-term thermal expansion coefficient, one needs to include the effect of multiphase systems where the partition of components between phases depends on temperature rather than just the expansion of isochemical phases.

Anderson is correct that compositional variations can overwhelm thermal expansion. The Earth's core-mantle boundary and the rock-air interface are obvious extreme cases. The evolution of the lowermost mantle over time is relevant to plume convection heated from below. The core likely formed hot in the wake of the moon-forming impact. Conduction from the core thus has heated the lowermost mantle over geological time. In the absence of convection, the hot region at the base of the mantle would be quite thick, scaling to $\sqrt{kt_E}$ where t_E is the age of the Earth. The heat flow from the core would scale crudely with the square root of the age of the Earth. One needs to account for the cooling of the core over time to obtain a better conductive model.

Overall, chemical stratification in the lower mantle leads to predictable but complex behavior. Anderson's "dregs" layer is gradually stratified so it does not convect internally. It is quite thick so that a vigorous thermal boundary layer does not form above it. However, seismologists and petrologists have yet to constrain strongly the properties of conceivable chemically dense regions at the base of the mantle. We do not even know if the "dregs" layer is thin, that is, "D", or thick as in a lava lamp. We do not know how dregs formed to begin with and whether current mantle processes enhance or disrupt stratification. Given this state of ignorance and physical complexity, observable manifestations of deep processes remain relevant. This includes hotspot tracks and the possible detection of plumes by tomography.

2nd January, 2007, Don L. Anderson

The other extreme from a vigorously convecting, well-stirred, high-Rayleigh-number homogeneous mantle – as usually modeled – is a mantle stratified by intrinsic density. The possibility that the mantle is chemically stratified is usually dismissed outright, particularly by modelers. Compositional stratification is plausible, and merits more attention (Anderson, this volume).

In his comment of 31st December (above) Sleep has added insight into this issue and the formation of "dregs" layers at chemical and viscosity boundaries. Much of the chemical layering was probably contemporaneous with accretion and Moon formation, but subsequent cooling can also create stratification, by the "dregs mechanism", e.g. light material leaving the core and newly dense basalt-eclogite in the proto-crust returning to the transition region. At some point in Earth evolution, presumably as a result of cooling, the deep subduction mode of plate tectonics kicked in and this returns surface material back to the mantle and displaces deeper mantle upwards. But there are ways to form a chemically distinct "D" layer without importing upper-

crustal material from the surface.

Although the core-mantle boundary is the most obvious place (apart from the surface) to collect dregs from the mantle and dross from the core, it is not the only candidate boundary for collecting debris from Earth accretion and differentiation. As the mantle cools, the thick basaltic crust converts to eclogite and this sinks and collects at the 410- or 650-km discontinuity, depending on its temperature and major-element chemistry. Very cold SiO₂-rich MORB eclogite may sink into the deeper mantle, until it reaches a density or viscosity barrier. Mantle viscosity may jump by 2 or 3 orders of magnitude at depths of ~1000 km and ~2000 km (Forte and Mitrova, 2001). Big chunks of delaminite are not easily stirred back down to meter- and centimeter-sized pieces. Olivine-rich cumulates and restites are buoyant and collect under the crust, as a perisphere. The crust and proto-crust are the dross of terrestrial differentiation and the core is the dregs. These are just a few examples of possible chemical layers.

Plausible chemical differences between mantle lithologies give huge density differences compared to thermal expansion. Overall, chemical stratification in the mantle leads to complex behavior that is not necessarily predictable. The delamination scenario is particularly interesting. Delaminated blobs are fertile, fusible, and initially dense. They will form dregs in the mantle where they become neutrally buoyant. They do not necessarily form a continuous dense layer. They then heat up and approach ambient mantle temperature, melt and become buoyant. Their fates depend on their sizes. This has not yet received numerical validation.

Sleep is correct in pointing out the need to generalize the expansion coefficient in a multiphase rock. One example is the gradual heating of garnet-majorite that may reside in the transition region as a result of the delamination of the lower part of the mafic crust of an overlying continent. The conversions of majorite back to garnet+pyroxene and then of garnet to magma give large density reductions. These effects, and compositional variations can overwhelm thermal expansion.

The interpretation of tomography is more ambiguous than usually appreciated. Cold peridotite with CO₂ (Presnall and Gudfinnsson, 2005, submitted) can be dense yet have low seismic velocities. Eclogite has low shear-wave velocities compared to dry peridotite of the same density; dense sinkers of eclogite can have low shear-wave velocities and may be mistaken for hot rising plumes. Refractory peridotite has high seismic velocity but low density; in tomographic images it appears blue but is not sinking. Tomography is not a thermometer.

Sleep, however, is optimistic about the possible detection of plumes and the relationship of volcanic chains to D". In bottom-heated, but otherwise realistic convection calculations, plumes, when they exist, are more like wandering strands of cooked spaghetti than rigid, upright rods, as often illustrated in cartoons, or axisymmetric cylinders, as they appear when modeled in isolation from mantle flow. They would be invisible to tomography. On the other hand, plumes should spread out below the 650-km discontinuity and the lithosphere, and be detectable. However, there is no seismic evidence for this (Deuss, this volume). If plumes exceed ~1000 km in

dimension they would overlap the normal scale of mantle convection and the broad upwellings that are intrinsic to an internally heated mantle in the absence of plumes.

2nd January, 2007, Scott D. King

Unwary readers should take warning that ordinary language undergoes modification to a high-pressure form when applied to the interior of the Earth. A few examples of equivalents follow:

<i>High Pressure Form</i>	<i>Ordinary Meaning</i>
<i>Certain</i>	<i>Dubious</i>
<i>Undoubtedly</i>	<i>Perhaps</i>
<i>Positive proof</i>	<i>Vague suggestion</i>
<i>Unanswerable argument</i>	<i>Trivial objection</i>
<i>Pure iron</i>	<i>Uncertain mixture of all the elements</i>

The discussion points by Anderson and Davies on the paper by Garnero et al. (this volume) remind me of the quote above from Birch (1952). I refer readers to my own paper in this volume where I present calculations that illustrate many of the effects mentioned in this discussion thread.

Let me begin with the rather obvious observation that the Earth is round. The surface area of the core mantle boundary is approximately one quarter of the surface, favoring a larger thermal boundary layer at the base of the mantle than at the surface, all other things being equal. As Anderson reminds us, all things are not equal.

Anderson and Davies both appeal to temperature-dependent mantle rheology to support their views. Kellogg and King (1997), van Keken (1997) and Davies (1999) show that the large plume head, narrow tail structure is a natural consequence of convection heated from below with an Arrhenius form of rheology and otherwise uniform properties (e.g., Boussinesq convection). Few if any papers in the last decade have used a constant viscosity, so this objection begs the question. I demonstrate the effects of internal heating (uniform), pressure-dependent coefficient of thermal expansion (the major effect of compressible convection, c.f., Ita and King, 1994), upper-mantle phase transformations, and a deep stabilized layer at the base of the mantle (phase change or compositional) with a strong temperature-dependent viscosity including an increase in viscosity with depth. These reduce the peak geoid, topographic, and heat flow anomalies, bringing the calculations closer to the observations, but produce deep plumes. It is worth noting that many other calculations have used temperature-dependent rheology with basal heating and in many cases internal heating and phase changes (c.f., Kiefer and Hager, 1992; Farnetani and Richards, 1994; 1995; Davies, 1995; Farnetani et al., 1996; Farnetani, 1997; Kellogg and King,

1997; King, 1997; van Keken, 1997; Leich et al., 1998; Leitch and Davies, 2001; Goes et al., 2004; Davies, 2005; Lin and van Keken, 2006a; 2006b; Zhong, 2006) and it seems past time to put that objection to rest. A significant amount of work has been done.

Anderson reminds us that in a complex system such as the mantle, it may be dangerous to make simplifying approximations that do not allow for a self-consistent formulation. However, his arguments beg the question as he proceeds to base his own arguments on at best the same inconsistent calculations or at worst simple theory that does not account for non-linear feedback. As an example, any effect that decreases the lower mantle Rayleigh number will cause the lower mantle to heat up, lowering the viscosity and increasing the flow. It is exactly because the mantle is a complex, self-organizing system, that such thought experiments, which do not consider feedback mechanisms, are every bit as dangerous (if not more so) as inconsistent calculations. A model as complex as reality is likely to yield very little understanding because it will be as unwieldy as reality (c.f., Oreskes et al., 1994).

While Anderson points out the effect of the increasing adiabatic temperature on viscosity he doesn't mention the pressure-dependent effect (activation volume) that trades off with the adiabatic temperature and is not well constrained experimentally. Viscosity models from the Arrhenius form can be compared with other models of mantle viscosity (e.g., King and Masters, 1992). The argument presented by Anderson on the temperature effect of rheology has been used by many of us to explain the geoid and topography inversions for the last two decades and is consistent with what he often calls the standard model. This has been included in many plume calculations, including most of the ones cited above, so it also begs the question. The lower mantle is perovskite and ferropericlaase and most laboratory rheology measurements are made on olivine or olivine analogues. So self-consistency is in the eye of the beholder.

Ironically, the discussion has little to do with the observations in the original paper. Seismic observations at the base of the mantle are complex and not obviously consistent or inconsistent with any of the models in their original form. They are leading to interesting new ideas (e.g., Ishii and Trump, 1999; Gurnis et al., 2000; Schubert et al., 2004; Le Bars and Davaille, 2004; Farnetani and Samuel, 2005) that one could envision being reconciled with Anderson's, Davies', or a hybrid view of the mantle. As for Anderson's arguments regarding the modification of the plume theory, remember Kuhn's work (1970)—scientific theories are always modified when presented with new observations. It seems to me that the proliferation of observations and ideas, including the chapter by Garnero et al. in this volume, show that the scientific process is working well.

3rd January, 2007, Geoffrey F. Davies

Anderson, in his 31 December comment, shifts from debating specifics to a general discussion of mantle dynamics. His representation of my views is inaccurate. I have never made such unqualified claims as "plumes must always exist, and they provide the only explanation for

Hawaii etc." I therefore summarise here my actual views, for the record.

In my book (Davies, 1999) I argue that mantle convection can be usefully viewed as driven by two thermal boundary layers, with rather different dynamical styles, that interact to a substantial degree. Clearly plates, from the top TBL, are a major driving force, and clearly they affect plumes. Nevertheless some phenomena, Hawaii being the outstanding example, are quite well explained, quantitatively, by the thermal plume model, apparently with only secondary effects from the rest of the mantle system. That is why models of isolated plumes are a useful approximation to some aspects of the mantle system.

Sleep and I were among the first to conclude, from inferred plume fluxes, that the mantle is only secondarily heated from below, and therefore mainly heated by internal radioactivity (with some secular cooling). Indeed, for many years I have modeled the role of plates in the mantle system using only internal radioactive heating, and excluding bottom heating and plumes. The point, of course, is to isolate those parts of the system that can be usefully isolated. It is therefore incorrect to claim that I don't account for radioactivity in the mantle on the basis of plume models tailored to isolate the plume phenomenon.

The mantle system clearly has complications, but that does not mean parts of it cannot be usefully approximated by simpler models. Our understanding of the system has advanced considerably using this standard scientific approach.

Of course ultimately we would like to include all the main phenomena in one model. This is not yet possible, because models including both plates and plumes must be three dimensional, but 3D models do not yet have the high resolution necessary for accurate modeling of plumes, although they are steadily approaching this goal.

Although Anderson refers to some recent models as being more 'realistic', it is important to appreciate that there are still important aspects that are not well understood or well constrained. This applies particularly to the top TBL, mainly because the rheology of the lithosphere is complicated and still not well characterized. Also no model can accurately predict absolute mantle temperatures, because of uncertainties in mantle rheology. Just because there is a relative progression of temperatures among some models, this does not provide a strong criterion for accepting or rejecting particular models.

Regarding alternatives to plumes, any model that involves a passive upper mantle responding to lithospheric changes would need to account for uplift preceding some flood basalt eruptions (e.g. Hooper et al., this volume, and references therein). Some other aspects of the mantle system, such as the role of plates in organizing mantle flow and the role of chemical heterogeneities, both as passive tracers and as active influences on buoyancy and melting, have long been discussed and modeled by practitioners. It has also long been appreciated that the mantle system is interconnected, self-organising, self-regulating, far from equilibrium and unsteady, and that thermodynamics and the role of the plates are important. I refer readers to my book for a broad

summary of the long history of work conducted by many people on these subjects.

The art of modeling the mantle system, as with any complicated system, is to construct models that are instructive because they include important physics, while not depending strongly on parts that are not well understood. This involves judgment and is therefore legitimately a matter of debate. There has always been active debate about plumes and alternatives to, or elaborations of them. The useful role of this volume is to continue that traditional debate.

Scott King (2 January) points out in more detail than I did (30 December) that many models of plumes take account of things Anderson (31 December) claims have been disregarded. Anderson further claims (2 January) that "The possibility that the mantle is chemically stratified is usually dismissed outright, particularly by modelers." This assertion is incorrect and potentially misleading and damaging to forward progress. Possible compositional stratification, either in D" (Christensen and Hofmann, 1994) or in the lower third of the mantle (Kellogg et al., 1999), has been a major theme of debate and modelling in recent years. Possible present or past compositional differences between the upper and lower mantle have also emerged from recent modeling (e.g., Ogawa, 2003; Xie and Tackley, 2004; Davies, 2006). The rest of Anderson's comment is unquantified speculation.

5th January, 2007, Don L. Anderson

"There has been a tendency to regard plumes as a distinct, secondary mode of convection... such a mode of flow has never been observed in any self-consistent numerical or laboratory experiment." (Larsen and Yuen, 1997)

King's comments are valid, but he and King and Redmond (this volume) are discussing and modeling *normal mantle convection*, albeit with enforced axisymmetry. Mantle plumes were invented as an alternate, or addition, to this broad-scale convection, which is driven by internal radioactivity, surface cooling, and plate tectonics. Calculations of mantle convection are indeed getting more realistic, but do they confirm the *mantle plume hypothesis*, i.e. an independent, narrow, plume-mode of convection that is responsible for hotspots?

The question of whether deep mantle upwellings can be independent, narrow, hot and fast—as required in the mantle plume hypothesis—is at the core of more fundamental questions which are; *Are the locations and dimensions of volcanic chains controlled by lithosphere and mantle heterogeneity, or by localized high absolute temperature and rapid upwelling? Do hotspots require a deep source of heat and material?*

Larsen and Yuen (1997) addressed this problem; *"The enigma of... nearly stationary plumes... in mantle convection arises in the hotspot hypothesis ... separation of time scales between the fast plume and adjacent mantle is necessary and, in fact, was invoked by Morgan in his original concept of plumes.... plume studies have usually modeled a plume in isolation from the rest of the*

mantle. ...Upwelling plumes always occur as part of the main convecting system (rather than independently). In particular, there is a problem of obtaining hotspot-like plumes, which must satisfy the requirements of being fast as compared to the ambient mantle circulation and fairly thin... Mantle plumes have peak ascending velocities of 20 meters/yr."

What distinguishes *mantle plumes*, as conventionally defined—and as widely perceived outside of the geodynamics/convection community—from *normal convection upwellings* is higher temperatures, higher ascent velocities and much smaller dimensions. Initially, mantle plumes were also thought to be stationary. Mantle plumes differ from alternative mechanisms in being entirely thermal in nature and in having a source deep in the mantle (although this idea is continually being modified in the face of observation, e.g. Courtillot et al., 2003). Starting plumes differ in uplift history from alternative mechanisms. It is these characteristics that stimulated the question “Do *mantle plumes* exist?” In this debate, no one is challenging the existence of convection or of broad upwellings or of small-scale features that can be attributed to extension and fertile blob scales. Kuhn noted this tendency to talk past one another when a paradigm is challenged.

My comments were specifically addressed **only** to those studies that isolate the lower TBL from the rest of the system, or that argue for narrow plumes or that neglect effects such as feedback. I wrote;

“...these (effects) are ignored in all calculations **that yield narrow whole mantle plumes**. None of the fluid dynamic calculations used ... **to support their view** take (these) into account ... (more realistic calculations) do not predict plume-like dimensions and time-scales at D” depths.” (emphasis is new)

I do not suggest that all studies have ignored pressure, layering or internal heating. I referred to studies that allowed self-organization and did not support the widely held narrow plume assumption. These studies, however, do not address the small-scale, and other characteristics, of hotspots and volcanic chains that motivated the plume hypothesis.

Recent fluid dynamic simulations (Zhong, 2006; King and Redmond, this volume) and most of the above comments and references refer to *normal mantle convection* or *superplumes*, not to the original *mantle plume hypothesis*. Broad plumes are not what Sleep, Larsen, Yuen, Olsen, for example, are modeling. Serious attempts have been made to rescue the original narrow plume hypothesis (Larsen and Yuen, 1997) but these violate other constraints. Although many workers have abandoned the small-scale stationary plume idea, many others attribute the difficulty in observing plumes to their very small size.

Although there are exceptions, most models assume whole mantle convection. The more interesting calculations allow for chemical stratification (e.g. Tackley and Xie, 2002). Quite often scaling between shear velocity and density is assumed, instead of an appropriate thermodynamic scaling via volume (Birch, 1952). In seismology, low shear velocity is still

usually attributed to high temperature and low density.

There are various mechanisms for causing uplift before and concurrent with volcanism, e.g. delamination. The plume hypothesis is unique in requiring major uplift many millions of years before the eruptions. Models that involve fertile blobs do not require such precursory uplift.

Birch (1952) developed the machinery for self-consistent treatments of mantle dynamics. Kuhn (1970) did argue that scientific theories can be modified but this was prelude to his discussion of why the concepts of Ptolemy, aether, and phlogiston failed; those concepts became so contrived and amended, and had so many versions, that people lost interest. The existence of a chemically layered mantle with large dense thermochemical features (piles) at the base, is not being disputed; in fact it was predicted. It is the association of these with surface volcanism that might be regarded as “unquantified speculation”. The shallow mantle and the transition region also have suggestive correlations with tectonics. It is interesting that the plume hypothesis—motivated by the idea of a buoyant hot D”—now requires importation of cold dense downwellings from the surface. This may be true, but it does make the plume hypothesis immune to new observations and theory.

6th January, 2007, Alexei Ivanov

Davies (3rd January) writes “Regarding alternatives to plumes, any model that involves a passive upper mantle responding to lithospheric changes would need to account for uplift preceding some flood basalt eruptions (e.g. Hooper et al., this volume, and references therein).” The Siberian Traps were not preceded by uplift (e.g. Ivanov, this volume, and references therein).

Davies refers to uplift preceding some flood basalts but neglects the absence of uplift preceding others. The Siberian Traps and Columbia River flood basalts are $4 \times 10^6 \text{ km}^3$ and $0.2 \times 10^6 \text{ km}^3$ in volume, respectively. Which example would seem more important to the debate regarding plumes and alternatives?

There is a mathematical rule that in a complex system evidence can always be found in support of an hypothesis. Probably we all take advantage of this.

6th January, 2007, Geoffrey F. Davies

Anderson's latest comment (5 January) reiterates his opinion but adds little to the continuing debate. However it does illustrate the current level of debate and disagreement among modelers, which is sufficiently diverse to allow Anderson to choose studies that support his contentions. This diversity is just the current manifestation of a debate that has always been vigorous, despite charges to the contrary.

The debates about plumes, and the accumulation of new observations, have led many to consider

variations on Morgan's initial proposals. For example, in my opinion there was never a good rationale, or need, for plumes to be rigorously fixed, rather than simply slow-moving (see Davies, 2005). However this learning and modifying process has been portrayed by some as rendering plumes arbitrarily adaptable and therefore untestable and unscientific. There have certainly been many poorly-motivated proposals invoking plumes, which I would join in criticising, but there has been a core of quantitative work that makes quantitative predictions. A summary of some significant predictions and relevant observations has recently been given by Campbell and Davies (2006).

An irony here is that two charges that have been made - lack of consideration of alternatives and arbitrarily malleable hypotheses - are mutually contradictory. They cannot both be true.

Science never ties up every last loose end of observation, especially in studies of very complicated subjects like Earth. Thus there is always some level of uncertainty to nourish dissenters. Ultimately it's a matter of judgment when to consider the issue decided and to move on, though all conclusions are conditional and subject to later modification or replacement.

22nd January, 2007, final comment by Ed J. Garnero, Thorne Lay & Allen McNamara

Debate about the nature of hotspot volcanism intrinsically raises the question of what the large-scale configuration of mantle convection is. As perhaps the foremost problem in global geophysics, it comes as no surprise that strongly held and conflicting perspectives of this issue persist despite extensive recent advances of our understanding of Earth's internal structure and processes. Enthusiasm for end-member scenarios of whole-mantle or layered-mantle convection ebbs and flows, and there is, as yet, no consensus other than that the most likely scenario involves a more complex thermo-chemical system than either end-member. We think it is fair to state that many deep-Earth geophysicists find the evidence favoring large-scale mixing of the mantle more compelling than evidence for strongly layered convection, but probably the strongest statement we would defend is that no line of evidence yet precludes significant flow between the upper and lower mantles.

Given that perspective, our chapter highlights some of the exciting deep-mantle high-resolution seismic findings in the context of state-of-the-art numerical geodynamical calculations that do allow upper mantle flow to influence the deep mantle. Simply put, we addressed this question: if plumes originate from the deep mantle, what are their possible geographic systematics given the recent seismic and geodynamic analyses? The extensive debate spawned by our paper raised several important factors, most of which were beyond the paper's intended scope, but all of which bear upon the fundamental question of what the configuration of the mantle dynamic system is.

Our chapter highlighted 3D spherical numerical calculations of McNamara and Zhong (2005) which explore thermo-chemical dynamics. These calculations assumed an initially dense,

chemically distinct layer in the lowermost several hundred km of the mantle, adopted the Boussinesq approximation, incorporated temperature dependent viscosity, and used a roughly equal ratio of basal to internal heating. The past 119 Ma of plate motions from Lithgow-Bertelloni and Richards (1998) were imposed as a surface flow boundary condition such that upper-mantle down-wellings spatially control the deep mantle flow that interacts with the chemically stratified layer. The hot, dense material of that layer is swept into large piles under upwelling return flow, yielding a configuration of large piles of chemically distinct material that have strong spatial affinity to large low-shear-velocity provinces (LLSVPs) in the deep mantle observed by seismology.

Our intention was not to predict plumes or deep mantle plume-hotspot connections, but the computations yield boundary-layer instabilities on the edges of the chemical piles that rise as plumes in the 3D flow (admittedly, the detailed character of the instabilities is not fully resolved and does depend on the Boussinesq approximation, although the physics of compressibility will likely not change the results in general). The simulations show that the dense-pile material influences the plume distribution. This configuration is in general agreement with the empirically observed tendency for hot spot volcanism to overlay lateral margins of deep-mantle, low-velocity provinces (Thorne et al., 2004). Thus, plumes are a consequence of our calculations, not an input design. There is no injection of material at the base of the model, both internal and bottom-heating are present, and unlike most earlier models, we explicitly incorporated initial chemical stratification in the system. The debate should not confuse the readers about what is actually in our paper. Even if the descent of slab material is more inhibited or limited than in the calculations, the general flow pattern and implications for where plume instabilities might arise is unlikely to change.

The take-home message is that the presence of dense thermo-chemical piles in the deep mantle can influence the location of boundary-layer up-wellings, providing a geometric distribution of boundary-layer instabilities that is absent in mantle flow models without piles. Thus, while vertical continuity of flow to the surface is not directly constrained, it is attractive to consider the possible connection between upwellings on pile margins (whether these involve continuous plume conduits or fragmented plumes or blobs) and hotspots at Earth's surface. Passive rifting at ridges may cause separate, relatively shallow upwellings that have no direct connection to lower boundary layer instabilities, so we focus on the possible linkage to hotspots. The hotspot research community may find it valuable to keep an eye on developments in deep mantle research as the plume debate progresses.

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