Discussion of

The Hawaiian SWELL pilot experiment - evidence for lithosphere rejuvenation from ocean bottom surface wave data

by

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5th January, 2007 James H. Natland and Edward L. Winterer

Laske et al. (this volume) provide evidence for lithospheric rejuvenation of a portion of the Hawaiian swell southwest of Mauna Loa. They evaluate four models and settle on a "hybrid thermal rejuvenation – dynamic thinning" model to explain their data and those of Li et al. (2004) elsewhere along the chain. They reject models of compositional buoyancy and lithospheric fracture (Natland and Winterer, 2005) in favor of one in which the lithosphere and upper asthenosphere are modified by heat arriving from below in a narrow conduit - in short, a plume, or vertical conveyor belt. They prefer such a conduit even though admitting that their own field area did not encompass the region of melt generation beneath Hawaii, and would not include the vertical conduit they drew in the diagram for their model. Following convention, they prefer a deep source for the heat even though the tomographic model does not extend below 200 km. Inclusion of a thermal conduit in their model may be permissible but is not justified from their data set.

Laske et al. (this volume) reject lithospheric fracture, which they say has "no suggestion for the low-velocity anomaly found in the asthenosphere." This is a minimalist interpretation of the nexus of hypotheses offered by Natland and Winterer (2005) to explain Hawaii. These combined not just fracture, but also focusing of asthenospheric counterflow in response to the geometry of the Hawaiian Ridge, and redistribution of mass within the lithosphere as a consequence of large-scale melt production and eruption of lava. Natland and Winterer (2005) suggested a modification of the gravitational-anchor hypothesis of Shaw and Jackson (1973), namely that the Hawaiian Ridge is keeled by dense dunite-wehrlite cumulates that crystallize during differentiation of Hawaiian tholeiite; that these tend to sink more or less by the mechanism of Jull and Kelemen (2001; but originally Daly, 1914, 1933) into warm but refractory mantle that was produced by extraction of abyssal tholeiite near the East Pacific Rise. These cumulates displace and deflect asthenospheric counterflow, triggering melting in the lee of the advancing Hawaiian Ridge.

This is not a simple fracture hypothesis, and it does offer the suggestion that Laske et al. (this volume) seem to have overlooked. In essence, it turns the plume conveyor belt on its side; fertile material is fed in laterally. The experiment described by Laske et al. (this volume) was merely a partial investigation of this downtrend lee, and is insufficient to resolve how warm mantle and/or partial melt became concentrated there. A plume is but one possibility for the convective arrangement southwest of Hawaii. The critical question is whether a vertical conduit is essential to explain the geophysical data.

6th January, 2007 James H. Natland

I question whether a model that assumes that the convective process is entirely thermal is appropriate. Petrology says otherwise. At least two types of compositional heterogeneity will influence density relationships. These are:

1. density contrasts among solid rock – the mantle consists of different lithologies; and
2. the distribution of melt.

These are closely related. Thus the mantle beneath Hawaii does not have merely an identical composition of residual abyssal peridotite left over from the partial melting of mid-ocean ridge basalt at the East Pacific Rise ($\rho = 3.35$ in Appendix Table 1 of Laske et al., this volume). Ultramafic cumulates produced by differentiation of Hawaiian tholeiite, mainly dunite, are also present in great volume beneath the islands (Clague and Denlinger, 1994) and to considerable depth, as borne out by studies of ultramafic xenoliths (e.g., Jackson 1968l; Jackson and Wright, 1970; Sen and Presnall, 1986; Chen et al., 1991; Sen et al., 2005). That such rocks will founder when warm (e.g., > 600˚C; Jull and Kelemen, 2002) can hardly be doubted, since their normative densities (Niu and Batiza, 1991) are dominated by iron-rich olivine (Fo_{65-85}), making them as much as 10% denser than abyssal peridotite (with Fo_{91-92}), which is also present in the xenolith suites. Mafic gabbro associated with ultramafic cumulates will accelerate the sinking when it converts to eclogite.

No one has ever seen, let alone examined, a physical specimen of any mantle source rock for Hawaiian tholeiite. All presumptions about the typical extent and temperature of partial melting of a uniform Hawaiian source are model dependent (e.g., the original pyrolite model of Green and Ringwood, 1967). However, the source is likely a composite of diverse lithologies with different densities, the proportions of which determine the relative fertility of the mantle. Thus primitive Hawaiian tholeiite may derive in large measure from eclogite (recycled ocean crust) in the mantle source (Lassiter and Hauri, 1998; Sobolev et al., 2000; Sobolev et al., 2005) or from refractory peridotite infused with basaltic melt (Falloon et al., this volume). This adds great complexity to consideration of any geophysical model for Hawaiian volcanism and its thermal effects on the lithosphere.

Eclogite is often considered to be a component in a plume source, but in fact there are no constraints whatsoever on its distribution in the asthenosphere. It could be widespread, in a layer, or unevenly distributed in strips, dipping slabs, small blobs, big blobs, blob clusters, or columns. Experimental studies indicate that eclogite has a lower range of melting temperature than peridotite; that pods or schlieren of it will contain some partial melt even when adjacent peridotite contains none; and that when any adjacent peridotite finally begins to melt, those pods or schlieren will contain a considerable fraction of melt. Fluctuations in melt volume along the Hawaiian chain thus could as well result from fluctuations in the proportion of eclogite in a laterally convecting source in the upper asthenosphere as from thermal, compositional or volumetric perturbations of an ascending plume.

This militates against thermal convective interpretations of lithospheric structure at Hawaii because volcanism proves that melt, whether it is derived from eclogite or peridotite, is always present and it cannot be ignored. As long as the porosity structure allows, it will rise and then break out to the surface whenever and wherever the stress field on the plate allows it. If partial melt is widespread in a low-velocity layer beneath the lithosphere (Anderson and Spetzler, 1970; Presnall et al., 2005), differential ponding will likely result from patterns of flow in the convecting upper mantle and the action of the lithospheric plate itself as an impermeable barrier. Values of 5% or more of partial melt distributed in the mantle near Hawaii as revealed by magnetotelluric experiments (Constable and Heinson, 2004) are not surprising, but do not prove a plume.

Long ago, Daly (1914) described basalt as “the bringer of heat”. The presence of eclogite in the source brings the mechanism of transfer of heat into the domain of temperatures of common basaltic liquids; high potential temperatures acting on a homogeneous peridotite source are not necessary to explain Hawaii (e.g., Anderson and Natland, 2005; Falloon et al., this volume) or the shape of the lithosphere. Thus the main mechanism of rejuvenation of the lithosphere, and of underplating (e.g., McNutt and Bonneville, 2000), is injection of basaltic dikes into the base of the lithosphere.

Xenolith diversity indicates that such basalt is only rarely primitive magma with a high temperature (Sen et al., 2005). The process clearly starts at the “zero-age” end of the chain at Loihi (Clague, 1988) and the South Arch volcanic field. Therefore, “lithospheric erosion” along a portion of the chain is not a matter of convective overturn of homogeneous peridotite in the solid state. Instead, the deep Hawaiian lithosphere transforms into something rheologically different because it contains either basaltic melt, or, where it is cooler, at least the cumulus products of such basaltic melt. These on the average will remain more plastic than abyssal peridotite, still and always being nearer their melting and/or crystallization temperatures, and therefore having lower shear velocities than abyssal peridotite. Perhaps the lithosphere near Kauai is thin (Li et al., 2004) because of sinking of a mixed mass of abyssal peridotite and dense cumulates from the lower lithosphere into the convecting upper mantle.

9th January 2007, Don L. Anderson

The authors are to be commended for mounting this remarkable and successful experiment. The seismological conclusions are well founded and appropriately conservative. The data itself cannot address a lower mantle origin of a proposed mantle plume, as the authors state, but they can test the hypothesis that a plume, as conventionally defined, exists. It can also test alternate hypotheses–including excess fertility–as proposed in the discussions by Winterer and Natland, which are quite different than the ones criticized by Laske et al. (this volume).

Although the experiment is off-axis from the conjectured plume track it is close enough to see a plume head or lateral flow of a hot plume, if these in fact exist, as the authors apparently believe (http://mahi.ucsd.edu/Gabi/plume.html). Plume theory has been refined for more than 30 years and offers very testable predictions, even if plume tails are too small to resolve. For example, the region around Hawaii should look like the diagrams in Campbell and Davies (2006), which includes the refinements and modifications that have been made to the hypothesis up to this time (http://www.mantleplumes.org/WebDocuments/Episodes06-plumes.pdf). Other authors envision even longer-distance effects away from the plume axis. In the plume model, the travel times of vertically traveling S-waves should be long, and the attenuation should be high. The transition region should be thin, and the plume should also spread out beneath the 650-km discontinuity.

A thermal plume differs from other explanations for Hawaii in being a very strong and hot active upwelling. An active upwelling, in contrast to a passive upwelling, spreads out beneath the plate; an upwelling at a dike, a ridge or in response to delamination (or "lithospheric erosion"), is focused toward the eruption site, or the region of thin or extending lithosphere. A plume will have a broad pancake or mushroom-shaped low-velocity region, concentric about the center of the upwelling or the region of active magmatism. There is no evidence of this in the current data for Hawaii. A passive upwelling will be cone- or wedge-shape, focusing and narrowing as it rises, rather than spreading out, much as is observed at ridges, and ridge-centered Iceland (Wolfe et al., 1997; Foulger et al., 2001). It will also have little impact on downstream heatflow. In the fertile blob-counterflow model, the melting anomaly will come in from the NW and will not be a large hot circular feature centered on Hawaii-Loihi. In the delamination and self-propagating-volcanic-chain models the upwelling will be local, linear, progressive, and will have asthenosphere-like temperatures, slightly higher than average because of the insulating effect of the large and long-lived Pacific plate. It will have an eclogite imprint on the chemistry; upwelling rates and chemistry will be affected by pre-existing features, such as fracture zones (FZ). Normal crust and mantle may exist very close to the eruption site.

Other seismic observations are consistent with shallow and lateral flow mechanisms, and with the absence or smallness of the effects seen by SWELL. Multiple *ScS* waves bouncing between the surface and the core, have normal travel times (Best et al., 1975, Sipkin & Jordan, 1975; Julian, 2005) and attenuation, and the transition zone (410-650-km) thickness shows no thermal thinning (Deuss, this volume). Thus, the breadth, depth and magnitude of the Hawaiian anomaly are, to some extent, already constrained.

It is useful to recall that a purely thermal explanation for the high magma production rate at Hawaii in a small area requires an upwelling velocity of ~50 cm/year, temperature excesses of up to 300˚C, and lateral flow of plume material out to more than 500 km. The upwelling is very narrow in the deep mantle but very broad near the surface.

Removal of the lower part of the plate may trigger upwelling, as in alternative models, rather than the reverse. Fertile material may be brought in laterally ("horizontal conveyor belt"), rather than in a narrow vertical cylinder that spreads out laterally. Low shear velocities are often attributed to hot buoyant plumes but eclogite-bearing blobs, or regions with CO_2, can also have low velocities and cause melting anomalies, even if not particularly hot or buoyant.

There is therefore sufficient motivation to consider alternate mechanisms, and not just restrict attention to the plume and crack models, both of which, in their pure form, assume a homogeneous isothermal reference mantle, which is assuredly not the case. The implication in Laske et al. (this volume) is that a plume origin of some sort is not in dispute, just the depth. The follow-up experiment is named PLUME but one would hope that serious non-plume and non-thermal explanations will be assessed, such as fertile blobs, self-perpetuating volcanism and delamination, not just cartoonish or strawman versions of these ideas.

11th January 2005 Edward L. Winterer and James H. Natland

Fracture zones and lithosphere structure must be considered in any model of Hawaiian volcanism. The magnetic anomaly offset at the Molokai FZ (MkFZ), one of the longest in the N Pacific, spans a 16-Ma difference in ages; the lithosphere should be thinner by at least 10 km beneath Kauai. The most prominent part of the Hawaiian swell, and the largest volcanoes, are between the MkFZ and the Murray FZ (MrFZ). The volume of basalt along the entire Emperor-Hawaiian chain reaches its peak at the MkFZ (Van Ark and Lin, 2004). These cannot be coincidences; for one thing thin lithosphere allows the asthenosphere at its solidus to well up further and to melt more extensively by adiabatic decompression in this region.

A complex of NE-trending Cretaceous seamounts lies west of the island of Hawaii (Eakins and Robinson, undated chart), suggesting that this part of the plate has long been vulnerable to intrusion and the lithosphere petrologically modified. This is where the SWELL Pilot Experiment was carried out. The data of Li et al. (2004) show steps at places that correspond to the MkFZ and the small-offset Maui FZ between Maui and Hawaii. The evidence for thin lithosphere to the NW of Hawaii is therefore not necessarily evidence for lithospheric thinning. The lithosphere there still isn't very thin and the thinnest parts are far from Hawaii and the chain axis. Besides thermal rejuvenation, thin lithosphere may be inherited (e.g. an effect of lithospheric age, fabric, etc.), or result from athermal thinning/stretching/delamination. Nor is thin lithosphere evidence for rejuvenation unless the prior thickness is known. In this example, a good case can be made that the lithosphere was thin to begin with, and that prior seamount formation made it more vulnerable to current melting. Thus North Arch volcanism (Clague et al., 1990; Frey et al., 2000) indicates very young and widespread melt productivity near Kauai. Perhaps lithospheric "enrichment" or "refertilization" occurred when the Musician seamounts were produced.

The MkFZ is a transtensional band some 300 km wide (Searle et al., 1993), narrowing toward the islands. Changes in lithosphere thickness across it likely ramp up in several smaller steps rather than thickening in one abrupt step. This agrees with Fig. 2 of Li et al. (2004), which shows a long ramp at the base of the lithosphere, shallowing by some 50 km northward, with marked steps of a 10-20 km at the main Molokai FZ between Molokai and Oahu and the smaller Maui FZ. This known blocky architecture of the plate is not treated in Li et al. (2004), and results instead in their depicting a smooth asthenospheric bulge and proposing gradual heating of lithosphere by a plume passing beneath. Control of lithosphere thickness by pre-Hawaii plate architecture could also simplify the history and not require a plume.

The Koolau-Lanai-Kahoolawe (KLK) isotopic anomaly (Basu and Faggart, 1996) and the peak volume of Hawaiian magmatism coincide at the intersection of the MkFZ with the Hawaiian ridge. The basalts of these islands exhibit greater scatter in isotopes than elsewhere along Hawaiian chain. Some of this might result from introduction of seawater into the crust of the fracture zone or the mantle underneath, but it may also indicate susceptibility of fissured and irregularly shaped lithosphere to prior modification by off-axis seamount magmatism. Furthermore, the FZ today may act as a dam or a conduit for magma, which may facilitate removal of material at the base of the plate by partial melting. The volcanoes in the chain to the NW have smaller volumes and more limited isotopic variability (Basu and Faggart, 1996).

These conjectures are consistent with low temperatures for melting and differentiation beneath the islands as revealed by ultramafic xenoliths. The deepest (highest pressure) xenoliths from Hawaii suggest that the in situ temperature near the base of the lithosphere reached a maximum of 1350˚C, or 1260˚C if the effects of volatiles are considered (Sen et al., 2005); this is 50-300˚C lower than predicted by plume models. Estimated temperatures at the lithosphere-asthenosphere transition beneath Oahu are not significantly different from those of normal 90-M-old lithosphere that has not been affected by a hot plume. The Hawaiian lithophere therefore is not unusually hot (e.g., Green et al. 2001; Green and Falloon, 2005; Presnall and Gudfinnsson, 2005). In addition, several studies suggest that low-melting-point eclogite may be involved in the Hawaiian source (Hauri, 1995; Ren et al., 2005; Sobolev et al., 2002; 2005), particularly the Koolau volcano (Hauri, 1995), which is also the most enriched of the KLK anomaly. Eclogite is even necessary for plumes, if they exist, to work (Cordery et al., 1997). However, the distribution of eclogite in the mantle is unknown, and its connection to plumes is not demonstrated; it may simply be distributed in the shallow asthenosphere to begin with (see comment on this paper by Natland [6 January 2007]).

Laske and coworkers need to consider these factors in developing models to explain their data; at this stage it is premature to claim that their results are inconsistent with the hypothesis of a propagating fracture at Hawaii and instead are evidence for a mantle plume.

8th February, 2005, Gabriele Laske & John A. Orcutt

Natland and Winterer, in their comments of 5th and 11th January, 2007, feel that we took a minimalist approach to reconcile our seismic model with their model of a propagating crack in the lithosphere. They point out that we may have overlooked the fact that counterflows in the asthenosphere also play a role in their model. Natland also questions whether it is appropriate to assume a purely thermal model for our seismic anomalies.

Addressing the second point first, Anderson (comment of 9th January, 2007) points out that our interpretation is appropriately conservative. Recall that we find low seismic anomalies in the lower lithosphere as well as in the asthenosphere. We believe that these anomalies are sufficiently well constrained to search for possible causes. Perhaps the best understood cause for seismic velocity anomalies are thermal effects, where anelastic effects are the next perturbation to this most simplistic idea (Karato, 1993). Partial melt indeed changes seismic velocity dramatically, perhaps more so than temperature anomalies do (e.g. Sato et al., 1989). Compositional changes, such as the abundance of eclogite suggested by Natland and Winterer also influence seismic velocity. As they emphasize, eclogite may or may not be very abundant in the asthenosphere. Of these causes, the change in composition is probably the most speculative. This leaves temperature and melt fraction. Lacking enough constraints, seismologists usually try to reconcile their data with temperature variations alone and it turns out that T does not have to change unrealistically to fit our data. Our results are supported by the electromagnetic study of Constable and Heinson (2004). At this point, it is difficult to reconcile melt fractions of more than 1% with electromagnetic and seismic data, further than 300 km from the islands.

This alone may or may not speak against a mechanical erosion or an injection of the base of the lithosphere with basaltic dikes but our tests of the seismic model against bathymetry and the geoid support our hypothesis that the lithosphere is not mechanically eroded. We agree that a thin lithosphere is not necessarily a result of rejuvenation, as Winterer and Natland point out. However, a mechanically thin lithosphere is inconsistent with our model, at least for the part of the swell covered by the pilot deployment which includes the South Arch volcanoes. This argument holds only if the greater area is isostatically compensated. As we pointed out, we find some inconsistency with the geoid in the deep ocean that remains to be explained. Winterer and Natland's definition of “thin” may actually agree with ours, if we allow the “thin” to be “altered at the base but not asthenosphere-like”.

We find a pronounced anomaly in the asthenosphere. Any model to explain the Hawaiian swell has to involve at least this region, i.e. models with sources confined to the lithosphere do not work. As Anderson points out, this is not necessarily refuting the specific model of Natland and Winterer (2005), which also predicts some changes in the asthenosphere through the horizontal supply of fertile material, which we have omitted in Figure 1 in our chapter. Perhaps, we have used Natland and Winterer's reference in the wrong context but the figure caption does not say that panel d) describes their model. If a horizontal conveyor supplies fertile material, then the accumulated material in the asthenosphere has to cause a seismic anomaly of 8%, 300 km away from the islands. Sobolev et al. (2005) argue that the enrichment from recycled crust is found only near the proposed plume center, which has undergone melting, but is insignificant near the plume edge where our array is located. Recall that the electromagnetic study supports significant melt fractions near the islands.

As Anderson points out, our pilot study is not appropriate to search for a deep mantle plume and we never say it is. Our pilot experiment is appropriate to search for causes of the Hawaiian Swell as we have discussed in this paper.

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