Discussion of

The origin of the Columbia River flood basalt province: Plume versus nonplume models

by

Peter R. Hooper, V. Camp, S. Reidel & M. Ross

7th January, 2007, Alan D. Smith

I would contend that any unifying model for Late Cenozoic volcanism in the Pacific northwest would be better based on a back-arc model, with the origin of the Columbia River Basalt Group (CRBG), Oregon plateau volcanism, and Chilcotin basalts in the Canadian Cordillera linked to asthenospheric upwelling behind the Cascade-Pemberton arc (Fig. 1). The presence of OIB-like compositions in the CRBG does not indicate a plume origin: in non-plume models, subducted oceanic crust is remixed into the convecting mantle such that the recycled components otherwise attributed to plumes are ubiquitous in the asthenosphere (Meibom and Anderson, 2003). Contentions that helium isotopes, melt volumes and eruption rates require a plume origin for large igneous provinces have been addressed by Anderson (2005). Rather, the volume of the CRBG can be ascribed to the coincidence of back-arc upwelling with a region of thin lithosphere in an embayment in the Precambrian basement (Fig. 1). Lithospheric thinning occurred mainly in the Eocene and Oligocene. Thus convective upwelling, not extension, acts as the trigger for volcanism (Smith, 1992).

The eruptive centres of the Imnaha, Grande Ronde, and Wanapum Formations occur in an area approximately 225 by 275 km (Fig. 1). The volume of the CRBG could be met by 6-10% melting of an asthenosphere/continental mantle section 40-60 km thick under such an area in a back-arc model. Hooper et al. (this volume) dismiss continental mantle source components: "*Recent work on Re/Os isotopic ratios (Hart et al., 1997; Chesley and Ruiz, 1998) rules out an SCLM component in the Imnaha basalt….*". The study by Hart et al. (1997) concerned the Oregon plateau and Snake River provinces, whereas Chesley and Ruiz (1998) studied the CRBG. Both modelled generation of Os isotope ratios from sub-continental lithospheric mantle (SCLM) peridotite contaminated with metasomatic components including vein pyroxenites and subduction-derived fluids. However, the mixing lines are controlled not by the composition of the metasomatic components, which was the only parameter varied in both studies, but by the composition of the continental mantle on account of the high Os contents assumed therein (3 to 3.5 ppb Os).

Chesley and Ruiz (1998) also assumed $^{187}Os/^{188}Os = 0.110$ for the continental mantle, which would be appropriate for ancient cratonic mantle, but not young lithosphere beneath accreted terranes. Mantle xenoliths from further north in the Cordillera suggest $^{187}Os/^{188}Os = 0.129$, and

1.37 ppb Os would be more reasonable for continental mantle under the latter. Substituting the xenolith values into the calculation of Chesley and Ruiz (1998), allows generation of the Os isotopic composition of the Imnaha basalts from continental mantle containing 20-30% pyroxenite (Smith, 2003). Adopting the xenolith composition also relaxes the requirement for minimal involvement of continental mantle in the modelling of Hart et al. (1997). The Os isotope systematics of basalts from the Columbia River and Oregon Plateau provinces are consistent with crustal contamination, but they do not preclude continental mantle sources.

The contemporaneous eruption of Columbia River basalt and calc-alkaline volcanism in graben systems (presumably Powder River volcanism; Hooper et al., manuscript page 23) is readily explained as a result of compositional variations in the continental mantle section in a back-arc model. Rather, it is the plume model, which would appear to encounter difficulties in explaining the eruption of different magma types from volcanic centres a few tens of km apart, when according to Camp and Ross (2004), the asthenosphere beneath the eastern half of the Columbia River province had been replaced with plume material by 15 Ma.

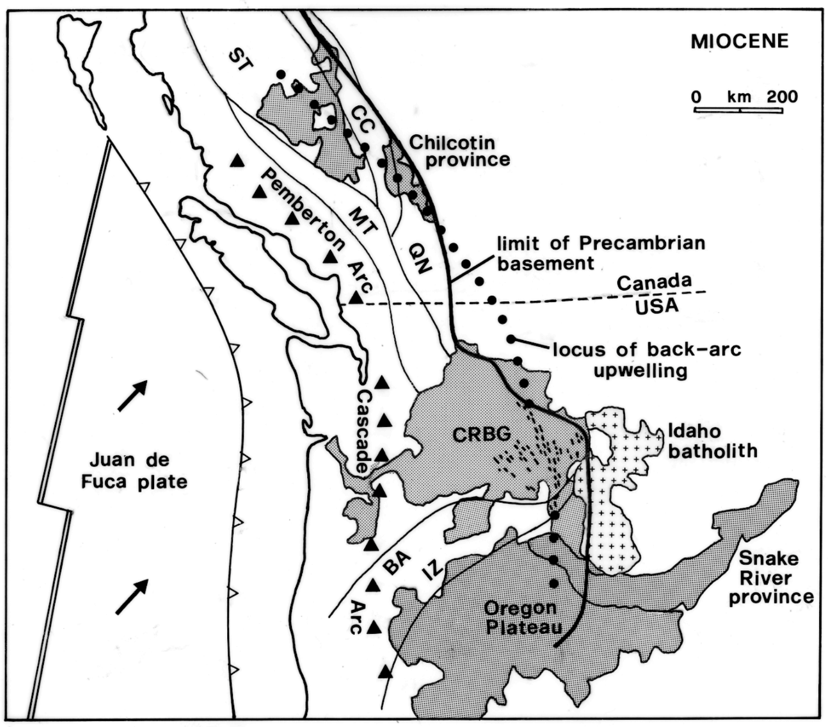

Figure 1. Relationship of CRBG (eruptive centres of the Imnaha, Grande Ronde and Wanapum Formations indicated by dashed lines), Oregon plateau, and Chilcotin volcanism to the Pemberton and Cascade arcs in the Miocene. Note: the absence of volcanism where back-arc upwelling occurred beneath Precambrian basement. Accreted terranes: BA Baker, CC Cache Creek, IZ Izee, MT Methow-Tyaughton, QN Quesnel, ST Stikine.

Before the mantle plume model was widely invoked, intraplate volcanism was attributed to shallow mantle and lithospheric processes. These included small-scale convection and propagating fractures (e.g. Smith, this volume), and a partially molten low-velocity zone. Continental basalts had a variety of shallow explanations involving the crust, lithosphere, and asthenosphere. Geologists sought to understand intraplate volcanism within the framework of geology and plate tectonics. With increasing popularity of the hotspot model, emphasis shifted to the core-mantle boundary and interpreting all intraplate volcanics as the result of mantle plumes. Other chapters and comments in this volume describe alternative models and give excellent descriptions of how normal plate tectonic processes (the plate hypothesis) can explain many features that are routinely attributed to plumes. Plate reorganizations, non-rigid plates, heterogeneous mantle, and abandonment of ridges, trenches and back-arc basins are all involved. These processes are either ignored in the plume hypothesis, or they are attributed to plumes.

In the Pacific northwest of North America we have ridges, ridge-trench collisions, young plate subduction, accreted terranes, sutures, slab windows, backarc basins, batholiths, rifts, extension and edges of cratons. We also have the possibilities and suggestions of delamination, fluids from an underlying slab, and melts from the mantle wedge and crust. With all of these processes and materials, why are deep mantle plumes also invoked? The reasons given are similarities to OIB, high $^{3}He/^{4}He$ ratios (R), short duration and uplift during volcanism, and the consensus of workers on the ground (Hooper et al., this volume). The plume model involves taking the material that is found in abundance around ridges, trenches and collisional belts down to the core-mantle boundary and then bringing it back up to the same sorts of places, sometimes through an intervening slab. The plate model focuses on the upper boundary layer and processes in the crust, mantle wedge and asthenosphere.

One has to be careful, when interpreting continental flood basalts (CFBs) in terms of a deep plume, to avoid circular reasoning (http://www.mantleplumes.org/CRB.html). This is particularly true when using chemical arguments, including R ($^{3}He/^{4}He$ ratios; Anderson, 2000a,b; 2001). Hooper et al. (this volume) argue in favor of a plume origin for the Columbia River Basalts (CRB) using a series of oft-repeated assertions, some of them circular (see comments following chapter by Sheth, this volume). Arguments based on helium, large volumes, OIB-type chemistry, uplift concurrent with volcanism, and short duration of magmatism are particularly unfortunate examples. These arguments have been raised and refuted many times (see reviews by Anderson (2005); Anderson and Natland (2005); Smith (1992) and http://www.mantleplumes.org/CRBDelam.html).

The arguments boil down to this: only plumes have rapid uplift, high R and short durations. The CRB region has these characteristics, it is therefore due to a plume. The circularity of the short-duration argument is as follows. All plumes were assigned the property of high eruption rates only because CFBs were assumed to be plumes and these had high eruption rates (Richards et al., 1989). Hooper et al. (this volume), and many others, now argue that because the CRBs are of short duration, they must therefore result from a plume.

OIB-like chemistry and high-R basalts have been found along mid-ocean ridges and in backarc basins. In some cases, these have been attributed to hotspots thousands of kilometers away. Some basalts found along midocean ridges (E-MORB) resemble OIB. The terms E-MORB, OIB and OIB-like are used to describe alkalic olivine basalt on ridges, seamounts, islands, CFBs, and continental rifts (Natland comment 10th January, 2007, on Fitton, this volume). OIB-like signatures occur at ridges and thousands of small seamounts (Natland and Winterer, 2005). Many oceanic hotspots are known to carry continental signatures that are plausibly related to delaminated continental crust or the continental fragment upon which they erupted (Anderson, this volume; Natland, this volume). OIB-like chemistry cannot then be used to argue for a deep mantle plume, or even a hotspot, source.

High R was originally thought to be a proxy for high [^{3}He] content and 'therefore' for an undegassed primordial lower mantle origin; this is the helium paradox or fallacy. High-R basalts were attributed to plumes because Iceland, Yellowstone and Hawaii had high-R magmas or gases, and these were "known to be from plumes". Most hotspots have helium isotope statistics that are indistinguishable from spreading ridges and they have much lower [^{3}He] contents than MORB (Anderson, 2001), consistent with a shallow origin in cumulates or restites.

The rates of CRB magmatism are comparable to the rates of arc magmatism and delamination (per kilometer of length) (Jicha et al., 2006). Asthenosphere from the mantle wedge upwells and displaces the dense foundering continental root at the same rate it is removed. This is in addition to the crustal space made available by spreading or stretching. Uplift accompanies delamination and asthenospheric upwelling. Foundered or delaminated material can be a local source for continental or 'plume' signatures, and of low-melting eclogite. Thus, underplating, uplift, large volumes, eclogite in the source, and the isotopic signatures of the CRB do not require deep mantle sources.

14th January 200,7 James H. Natland

Accumulation of the great volume of Columbia River basalt (CRB) east of the active Cascades arc means that they erupted in some type of backarc setting. Hooper et al. (this volume) are greatly knowledgeable about the CRB, but seem unaware that western Pacific backarc basins have many of the geochemical and other attributes that, for the CRB, they attribute to plumes. In my opinion, combining a plume with a backarc setting simply beggars the imagination.

Figures in Hooper et al. (this volume) suggest that the average thickness of CRB before erosion was ~1.2 km. The Lau Basin west of the Tonga arc covers roughly the same area (Taylor et al., 1996) and consists of ocean crust several km thick that formed over the past ~4 Ma. Although only about a third of this is basalt flows and dikes, the full volume of ocean crust implies a rate of magmatism perhaps three times that of the CRB. The Lau Basin, of course, is intraoceanic and the basalts there erupted at spreading ridges, whereas the CRB erupted through continental crust.

However, this does not negate comparisons of magmatic productivity; it simply says that the forms of magmatic activity, the tectonic controls on it and probably the fertility of the source were different.

Lau Basin basalt geochemistry indicates the influence of distinctive mantle components, viz., 1) a depleted MORB component; 2) an Indian Ocean component; 3) a Samoan component; and 4) a Louisville Ridge component (e.g., Volpe et al., 1988; Hickey-Vargas, 1998; Turner and Hawkesworth, 1998). Other backarc basins are similarly complex (e.g., Stern et al., 1990; Taylor and Fernandez, 2003). The Louisville component is attributed to subduction of volcaniclastic material on the Pacific plate, and the Samoan component to infiltration of Samoan mantle through a tear in the Pacific plate deep beneath the northern end of the Lau Basin. But in neither case is a plume within the basin required. I also believe that Samoa results from lithospheric fracture, not a plume (Natland, 1980; http://www.mantleplumes.org/Samoa; Stuart et al., this volume, Natland and Winterer 12 January comment on Stuart et al., this volume) The Indian Ocean component is a regional signal originally produced by delamination of lower continental crust and dispersal of the Gondwana continents (e.g., Meyzen et al., 2005), but is present in the Lau Basin because asthenosphere is drawn from the west into the convecting regime of the Tonga arc and Lau Basin (Hickey-Vargas, 1998; Turner and Hawkesworth, 1998; Smith et al., 2001). Again, no plume is involved.

Although Hooper et al. (this volume) only briefly mention He isotopes, they assume the opinion of isotope geochemists that $R > 8$ require a plume. One could presume that the geochemists are right, but still ask whether a plume within the basin was necessary. After all, basalt with the Samoan signature in the Lau Basin has such a signature (Poreda and Craig 1992). But here I side with Anderson (1998 and 13 January comment to this paper) that sources with helium isolated from U and Th and with low intrinsic ^{3}He concentrations are likely to be important. This is because the sources of the CRB, with so many of its rocks having a continental isotopic signature, likely include cumulates produced during the ancient magmatism that created continental crust.

Cumulates, especially those with olivine, are mineral aggregates that contain volatiles originally trapped as bubbles during crystallization (e.g., Natland, 2003). Foundering or delamination of such cumulates (e.g., Daly, 1926; Jull and Kelemen, 2001; Anderson, this volume) beneath an oceanic island, an island arc, or an ancient batholith is sufficient to carry the volatiles into the lower lithosphere and convecting upper mantle, and any of these materials, transported into the melt domain by any mechanism, could be present beneath the CRB. The high-R source could even still be intact in the subcontinental lithosphere beneath Nevada near where the CRB originated. High R is simply an indication of the age of the original melting event, carried forward to the present day without affiliated Th and U to modify it in a cumulate time capsule. It is a simple matter of efficient separation of the vapor phase from the liquid and into a solid, having little to do, as Parman et al. (2005) have argued, with partitioning of U and Th into olivine from the melt. As such, it far less a signature of a source in the lower mantle than of a place near the Earth's surface where volatile exsolution can occur. The CRB also includes both low-Ti and higher-Ti basalt (see Natland, this volume), with the latter likely requiring a titanian phase (ilmenite or rutile) in the melt source. That is, this component was not mantle peridotite, but, more likely, eclogite (Takahashi et al., 1998), as agreed by Hooper et al. (this volume). A general link between high R in the CRB and eclogite in the source is indicated, but this is by no means proof of the existence of a mantle plume (Anderson, this volume; Natland, this volume).

17th January, 2007, Alexei V. Ivanov

The Columbia River Flood Basalt Province is located in back-arc tectonic setting, but is interpreted by Hooper et al. (this volume) among many others in framework of the plume model. Two major plume-proponent arguments; the high $^3He/^4He$ and a large volume of lava erupted over a short period of time, have been suggested to be circular arguments rather than pro-plume evidence by Anderson (13th January) and Natland (14th January). I will not considered this point here.

Hooper et al. (this volume) write: “The largest CRBG formations (lower Steens, Imnaha, Grande Ronde, and Wanapum Basalt Formations) suggest a primary mantle source akin to that for OIBs [ocean island basalts], a view consistent with Hawaiian-like trace-element profiles (Hooper and Hawkesworth, 1993)”. This is incorrect, at least in terms of Grande Ronde Formation, and misleading for further evaluation of plume and nonplume models.

In Figure 2, I plot trace-element data for 36 samples of the Grande Ronde basaltic andesites (analyses are taken from Hooper and Hawkesworth, 1993). They are characterized by relatively uniform trace-element patterns, which is significantly different from either modeled primitive OIB composition (Sun and McDonough, 1989) or from a typical Hawaiian basalt sample BHVO-1, which has served for many years as USGS reference material (http://minerals.cr.usgs.gov/geo_chem_stand/basaltbhvo1.html). An important feature of OIB is Nb enrichment relative K and La and Pb depletion relative Ce and Pr on primitive mantle normalized diagrams (Figure 2a). Grande Ronde basaltic andesites show the exact opposite trace-element patterns. Thus, the sentence quoted above from Hooper et al. (this volume) expresses their interpretation that original OIB-like (plume) melts were contaminated by lithospheric material to produce the Grande Ronde basaltic andesites. This interpretation may or may not be correct. I will not evaluate the arguments used in support to this interpretation by Hooper et al. (this volume and earlier papers). The aim of my present comment is to show that the trace-element geochemistry of the voluminous Grande Ronde basaltic andesites can be easily explained by subduction-related processes and does not require a plume.

In Figure 2b, the Grande Ronde basaltic andesites are compared with typical island arc basalts (IAB). Primitive IAB trace element patterns are an average from high-Mg basalts of the most productive volcano of the northern hemisphere; the Klyuchevskoi volcano, Kamchatka, Russia. It may be seen that practically all the troughs and peaks of the primitive IAB are represented in the Grande Ronde basaltic andesites with the exception of the Sr peak (Figure 2b). The Grande

Ronde basaltic andesites have evolved SiO_2 from 53 wt. % to above 57 wt. %, and low MgO from 2.9 to 5.9 wt. % (e.g. Hooper and Hawkesworth, 1993), reflecting their fractionated nature. The major fractionating minerals in the Grande Ronde primary melts were olivine, clinopyroxene and plagioclase (Figure 3). Subtracting these minerals from the primitive-IAB starting composition via equilibrium crystallization (Shaw, 1970) elevates concentrations of all trace elements except Sr, because Sr is compatible in plagioclase (Table 1). Modeled evolved IAB composition is almost identical to the Grande Ronde basaltic andesites (Figure 2b).

It is possible to calculate the primary melt for the Grande Ronde basaltic andesites backwards through addition of olivine, clinopyroxene and plagioclase to the real Grande Ronde compositions. This is not shown here, but the primary melt of the Grande Ronde basaltic andesites will be similar to that of the high-Mg basalts of the Klyuchevskoi volcano, differing by somewhat higher Th and Nb concentrations. Thus, the sentence from Hooper et al. (this issue) that I quote above should be modified to read: “The largest CRBG formations (e.g. Grande Ronde Formations) suggest a primary mantle source akin to that for IABs, a view consistent with island-arc trace element profiles”.

The Columbia River Flood Basalt Province is close to a subduction system and thus IAB mantle source is expected, unlike the enigmatic plume source (Smith, 1992; comment by Smith 7th January, 2007). Subduction may be responsible not only for flood basalts situated near convergent boundaries, but also for flood basalts at distances of 1-2 thousand km away, such as the Siberian Traps (Ivanov, this issue).

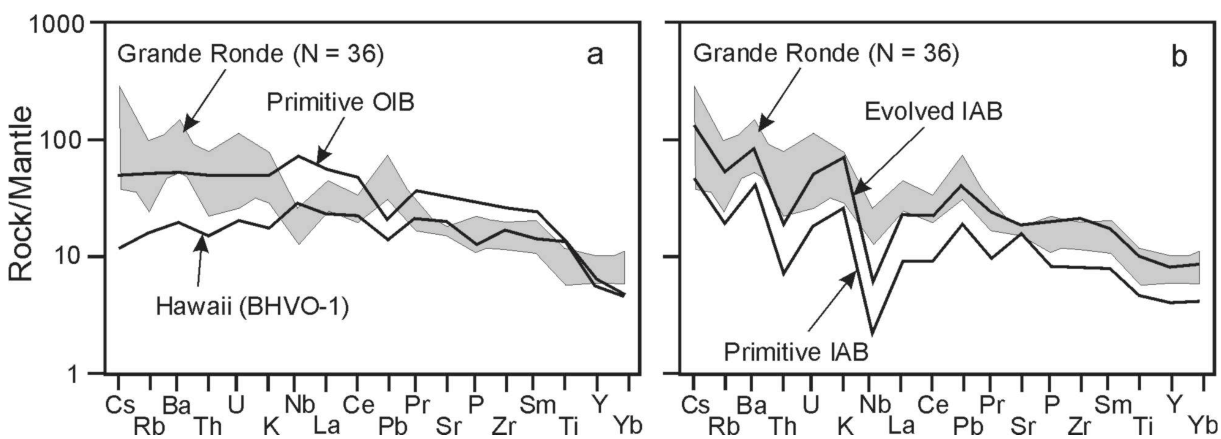

Figure 2. Primitive-mantle-normalized diagram for Grande Ronde basaltic andesites compared with OIB (a) and IAB (b) reference compositions. Grande Ronde basaltic andesites are from Hooper and Hawkesworth (1993). Primitive OIB is from Sun and McDonough (1989). BHVO-1 is from Dulski (2001). Elements not analyzed by Dulski (2001) are from http://minerals.cr.usgs.gov/geo_chem_stand/basaltbhvo1.html. Primitive IAB is the average of high-Mg basalts of Klyuchevskoi volcano, Kamchatka (calculated from Dorendorf et al., 2000). Evolved IAB is a modeled composition calculated using the equilibrium crystallization equation

of Shaw (1970), primitive IAB as starting melt composition, mineral/melt partition coefficients from Table 1 and 65% of fractionated olivine, clinopyroxene and plagioclase in the proportion 0.3:0.45:0.25, respectively.

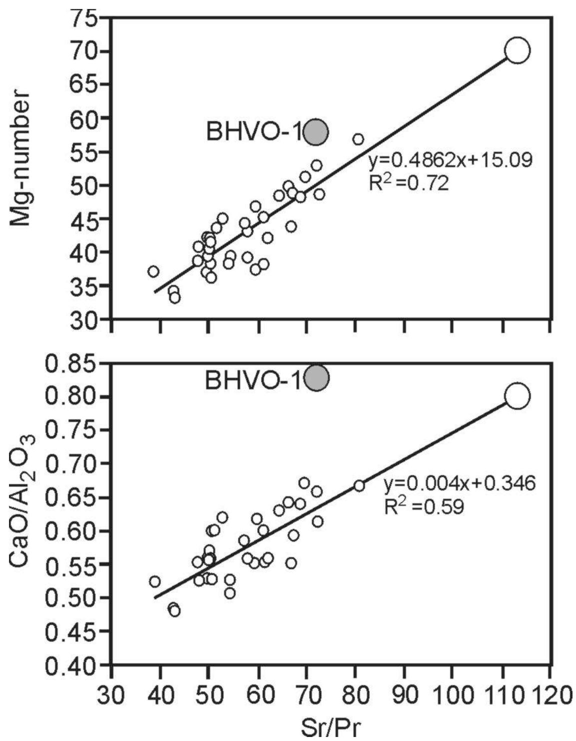

Figure 3. Mg-number and CaO/Al_2O_3 versus Sr/Pr for Grande Ronde basaltic andesites (small open circles). Primitive mantle is after McDonough and Sun (1995). Mg-number, CaO/Al_2O_3 and Sr/Pr are used as proxies for olivine, clinopyroxene and plagioclase fractionation, respectively. Large open circles represent plausible primary melt with high Sr/Pr, which reflects Sr enrichment of the mantle source by subduction-derived water fluid. Typical Hawaiian basalt (BHVO-1) is also marked as large grey circle to show its off-trend position.

Table 1. Mineral/melt partition coefficients for selected elements used for derivation of trace-element patterns of evolved IAB melt (see Figure 2b).

	Plagioclase	Clinopyroxene	Olivine
Cs	0.034	0.001	0.0001*
Rb	0.023	0.0047	0.0085
Ba	0.69	0.0007	0.001*
Th	0.064	0.012	0.01*
U	0.078	0.01	0.01*
K	0.05*	0.05*	0.003*
Nb	0.024	0.0077	0.0035
La	0.12	0.054	0.00001*
Ce	0.097	0.086	0.024
Pb	0.44	0.1*	0.0055
Pr	0.077	0.14	0.031
Sr	2.38	0.13	0.012
P	0.1*	0.125*	0.013
Zr	0.00018	0.12	0.015*
Sm	0.048	0.29	0.016
Ti	0.037	0.38	0.015*
Y	0.012	0.47	0.029
Yb	0.0098	0.43	0.053

Note: Values for plagioclase and olivine are based on Dunn and Sen (1994). Values for clinopyroxene are after the compilation of Zack et al. (1997). Asterisks mark extrapolated values.

20th January 2007, Ajoy K. Baksi

Hooper et al. (this volume), present a model arguing for a hotspot related genesis for the Columbia River Basalt (CRB). Supporting evidence is based on geochronologic data and follows on earlier work (Hooper et al., 2002; Hooper, 2004). All pertinent radiometric data are critically reviewed for statistics and alteration of rocks dated (Baksi, 1999; 2005; this volume). Errors are quoted at the 1σ level.

Two sets of data are cited by Hooper (2004), namely Duncan (pers. comm., 2003) and Hooper et al. (2002). For the former, no data tables are available for inspection. The ages cited appear to be valid from the statistical point of view. It is not possible to quantitatively assess the state of alteration of the rocks dated by Duncan. The very large error (±3.9 Ma) for the isochron age of BUK-5 suggests the rock contained large amounts of atmospheric argon and is altered. The ~15.5 –16.0 Ma ages listed by Hooper (2004) for the Imnaha Basalt cannot be critically evaluated for lack of isotopic data tables. These $^{40}Ar/^{39}Ar$ ages are in general agreement with the K-Ar dates determined in the 1970s, all of which were on altered whole-rock material (see Baksi, 1989). The ages determined by Duncan appear to be on altered material and do not serve as accurate measures of the time of crystallization.

Details of ages determined at the Open University were obtained (P.R. Hooper, pers. comm., 2003). Most of the these ages - listed in Hooper et al. (2002), Table 1 - cannot be treated as accurate estimates of the time of crystallization of CRB rocks. Firstly, the majority of the "ages" are replicate $^{40}Ar/^{39}Ar$ total fusion analyses. These are the equivalent of K-Ar dates, and do not address potential problems of partial $^{40}Ar^{*}$ loss (by alteration?) and/or the presence of excess argon. The alteration index (A.I.) of these samples are 3 - 10 times higher than the cutoff values for freshness (< 0.0006 for basalts, < 0.00006 for plagioclase). Seven samples were analyzed by the $^{40}Ar/^{39}Ar$ stepheating technique (Hooper et al., 2002). Four of these (SK078, KL046, MP028, MP050) yield high A.I. values and are altered. Specimen KL333 appears to be relatively fresh, but its listed age (15.8±2.8 Ma) is of very low precision, and does not help narrow down the age of sections of the CRB. KL276 and SC054 have low A.I. values and gave plateau ages of 16.6±0.1 and 15.9±0.2 Ma, respectively. These two ages were rejected by Hooper et al. (2002), as "invalid because (they are) older than stratigraphic age". It is not clear what is meant by "stratigraphic age" - the rest of the ages listed by these authors? The latter were determined on altered material and are underestimates of the correct age. The ages of KL276 and SC054 appear to be good estimates of the time of crystallization. There is no radiometric evidence that ">90% of the CRB (was) erupted between 16.1 and 15.0 Ma" (Hooper et al., 2002).

Hooper (2004) suggested the ages of Baksi and Farrar (1990) are not in agreement with those determined by three other sets of scientists. This is not the case for the Berkeley laboratory. Swisher et al. (1990) obtained ages of 16.6 – 16.5 Ma for the Steens Basalt. The corresponding age of Baksi and Farrar (1990), when corrected to the calibrations preferred by Renne et al. (1998), is 16.5 Ma. As shown above, few if any of the Open University ages, as well as those determined by Duncan on the CRB, are valid estimates of the crystallization age. There are problems in the interpretation of the Baksi and Farrar (1990) ages with respect to the geomagnetic polarity time scale (see Baksi, 1993; Cande and Kent, 1995; Wilson and Gans, 2003). These may stem from incorrect ages and/or interpretation of the magnetostratigraphy of the CRB. For the latter, it was assumed that all reversals of the geomagnetic field in this time period (~17-15 Ma) are trapped in CRB lavas (see Baksi and Farrar, 1990; Baksi, 1993). The relevant $^{40}Ar/^{39}Ar$ analyses need to be critically evaluated by the A.I. technique. Unfortunately, the primary listing of the isotopic data sets was lost when (floppy) computer disks were damaged; the original charts related to the analyses at Queen's University in 1989 need to be reexamined. Some of the splits analyzed by Baksi and Farrar (1990) may have been altered, since the powdered rocks were not washed in dilute nitric acid prior to analysis (see Baksi, this volume).

All data utilized to support hypotheses should be made available for critical examination. In particular, they should not be listed as "personal communication" or refer to "numbers" in abstracts.

28th January, 2007, Peter Hooper, Vic Camp, Steve Reidel, and Marty Ross

We wish to thank Smith, Anderson, Natland, and Ivanov for their comments on our paper. All authors comment on the ambiguity of geochemical interpretations, asserting that OIB signatures and high $^3He/^4He$ ratios are the result of shallow-mantle processes unrelated to a plume origin. Smith and Anderson note that that the presence of OIB-like compositions does not indicate a plume origin. The largest CRBG formations do indeed have trace-element and isotopic ratios consistent with an original OIB-like source. However, we did not and do not claim that the chemical similarity of the Imnaha Basalt to those of Hawaii proves a plume origin; only that such a similarity is necessary to such an origin.

We do not "dismiss continental mantle source components" as claimed by Smith. Rather we emphasize the role of a lithospheric component in mixing arrays for both the Picture Gorge and the Grande Ronde Basalts and state "the variability of ratios such as Ba/Nb are best explained by a mixing array between the original mantle component and a component with a lithospheric geochemical signature. The lithospheric component is characterized by a conspicuous negative Nb-Ta anomaly (Hooper and Hawkesworth, 1993) and could represent either an enriched sub-continental lithospheric mantle (SCLM) or mafic lower crust" (see section "Other Source Components" in Hooper et al., this volume). We excepted the Imnaha Basalt from having a lithospheric component, both because Chesley and Ruiz (1998) rightly or wrongly had used $^{187}Os/^{188}Os$ to discount SCLM as a component for these early flows and because Imnaha chemical trends lack the Ba/Nb enrichment, so conforming to normal partial melting and crystal fractionation trends. Hooper and Hawkesworth (1993) had demonstrated that the enrichment in the Grande Ronde Basalt could not be due to normal crystal fractionation; a conclusion previously reached by Wright et al. (1989) based on their chemical and experimental studies.

Ivanov argues that the Grande Ronde lavas could have been derived from subduction-related processes. While the Imnaha Basalt chemical trends conform to normal partial melting and crystal fractionation processes (Hooper et al., this volume, Fig. 10), those of the Grande Ronde Basalt do not (Hooper et al., this volume, Figs. 8a,b; Wright et al., 1989; Hooper and Hawkesworth, 1993). We have always been conscious of an important lithospheric component akin to calc-alkaline material probably derived from a sub-continental mantle enriched in the earlier subduction process (Hooper and Hawkesworth, 1993) and reactivated by the flood basalt event. To imply that we regard the Grande Ronde Basalt as derived entirely from a single source similar to the OIBs is to misinterpret our present and previous papers.

28th January, 2007, Vic Camp, Peter Hooper, Steve Reidel, and Marty Ross

We thank Smith, Anderson, and Natland for their comments. Smith reiterates his back-arc model (Smith, 1992) for the genesis of the Columbia River Basalt Group (CRBG) as an alternative to the plume model advocated in our paper. A back-arc origin is also promoted by Natland who points out the similarity of the CRBG to oceanic basalts in western Pacific back-arc basins which have high magma supply rates and similarly diverse geochemistry. Anderson advocates normal plate-tectonic and shallow-mantle processes for the generation of all continental flood basalts, and cautions against the use of oft-repeated assertions and circular reasoning to support a plume genesis for the CRBG.

We maintain that the geological and field data for the CRBG are markedly consistent with the predictions inherent in models of plume impingement, and just as strikingly inconsistent with all proposed alternative models. Here, we expand on some of the broader problems with the non-plume interpretations as supported by Anderson, and some specific problems associated with the back-arc model advocated by Smith and Natland.

1) ***Magma supply rates and duration of magmatism.*** Smith, Anderson and Natland suggest that the high eruption rates of the CRBG main-phase eruptions are no greater than those generated by normal plate-tectonic processes. We agree that the rates of magma accumulation at mid-oceanic ridges, island arcs, and in oceanic back-arc settings may also be considerable, although relatively difficult to estimate with any accuracy. In the Aleutian arc, the eruption rates referred to by Anderson (Jicha et al., 2006) are 60-180 km^3/km/Ma. Equivalent calculations for the CRBG lavas erupted from the Chief Joseph dike swarm are 500-600 km^3/km/Ma, significantly greater than the Aleutian-arc eruption rate. If Jarboe et al. (2006) are correct and the CRBG main phase was erupted in only 0.75 Ma, then the CRBG eruption rate is doubled. However, the real argument lies in high magma supply rates over *exceedingly short periods of geologic time*. On-going back-arc extension, which has continued behind the Cascade arc since Eocene times, cannot by itself account for the dramatic, short-lived burst of basalt accumulation at ~16 Ma.

2) ***Flood-basalt volcanism in a continental back-arc setting.*** Volcanic arcs on continental margins are very well documented. It seems unlikely that the Cascade arc is the only one capable of generating a flood-basalt province behind it by back-arc extension. It is clear that the southern Cascade back-arc region was subjected to an unusual mantle melting event, distinct in size, character and short duration when compared to all other Phanerozoic examples in continental back-arc settings.

3) ***The relationship between extension and magmatism.*** The degree of Basin-and-Range extension behind the Cascade arc decreases from south to north, in marked contrast to the volume of the flood basalts which increases in the same direction. This inverse relationship suggests that back-arc extension cannot have been the controlling factor to trigger flood-basalt volcanism. One must conclude that it was triggered by the abrupt arrival of a thermal and/or fertile anomaly at mantle depths.

4) ***Concept of a unifying model***. Smith contends that any unifying model for Late Cenozoic volcanism in the Pacific Northwest would be better based on a back-arc model than on a plume model. How then does a back-arc model explain the extraordinary spatial and temporal connection of the CRBG flood-basalt province to the Snake River Plain (SRP) hotspot track (Fig. 4)? There is no unifying mechanism in the back-arc model consistent with this relationship. Magmatism along the SRP emanates from the region of flood-basalt initiation in southeastern Oregon at ~16.6 Ma, along a series of calderas that progressively decrease in age from 16.5 Ma near the Nevada-Oregon border to 0.6 Ma at Yellowstone National Park. At Yellowstone, a mantle plume has been seismically resolved to a depth of at

least 500 km (Yuan and Dueker, 2005; Waite et al., 2006). Projecting back in time, places the Yellowstone plume in the vicinity of flood-basalt initiation at ~16.6 Ma. The geophysical data for the Yellowstone plume seems irrefutable, and consistent with the geophysical data of both Parsons et al. (1994) and Saltus and Thompson (1995) demonstrating the existence of a broad mass of hot, low-density mantle in southeastern Oregon and Northern Nevada which, they argue, is the Yellowstone mantle-plume head. The field and geophysical data are consistent with the connection of a plume head/tail pair as predicted in traditional plume models. To believe that flood-basalt volcanism was generated by normal back-arc processes requires one to ignore this important relationship.

5) ***The focal point of southeastern Oregon.*** Plume-skeptics also ignore the implications of the field data demonstrating the migration of flood-basalt eruptions along radial trends (Fig. 4). CRBG volcanism began in southeastern Oregon and adjacent Nevada with the eruption of Steens basalts at ~16.6 Ma. These initial eruptions were soon followed by a short period of rhyolitic volcanism in this same localized region from ~16.5-15.5 Ma (Fig. 4). This focused burst of bimodal magmatism occurs at the western edge of the SRP hotspot track. Stratigraphic relationships demonstrate that volcanism rapidly migrated away from this focus along three radiating trends, forming the Chief Joseph and Monument dike swarms to the north and the NNR to the south (Fig. 4). These radial trends are consistent with propagating volcanism above an expanding mantle-plume head (Camp and Ross, 2004), but are markedly inconsistent with the eruptive history expected from back-arc extension or any proposed non-plume model. Propagating trends along the SRP and the High Lava Plains (HLP) after ~15 Ma emanate from the same area of southeastern Oregon, as one would expect from the westward expansion of the plume head beneath weakened lithosphere (the Brothers fault zone) (e.g., Jordan et al., 2004), and from the east-northeast migration of the plume tail along the SRP hotspot track due to the relative motion of the North American plate (Pierce and Morgan, 1992).

Based on an unusually comprehensive interdisciplinary database, we maintain that the plume model for the CRBG-SRP magmatic system explains better the available evidence than any of the suggested alternative models.

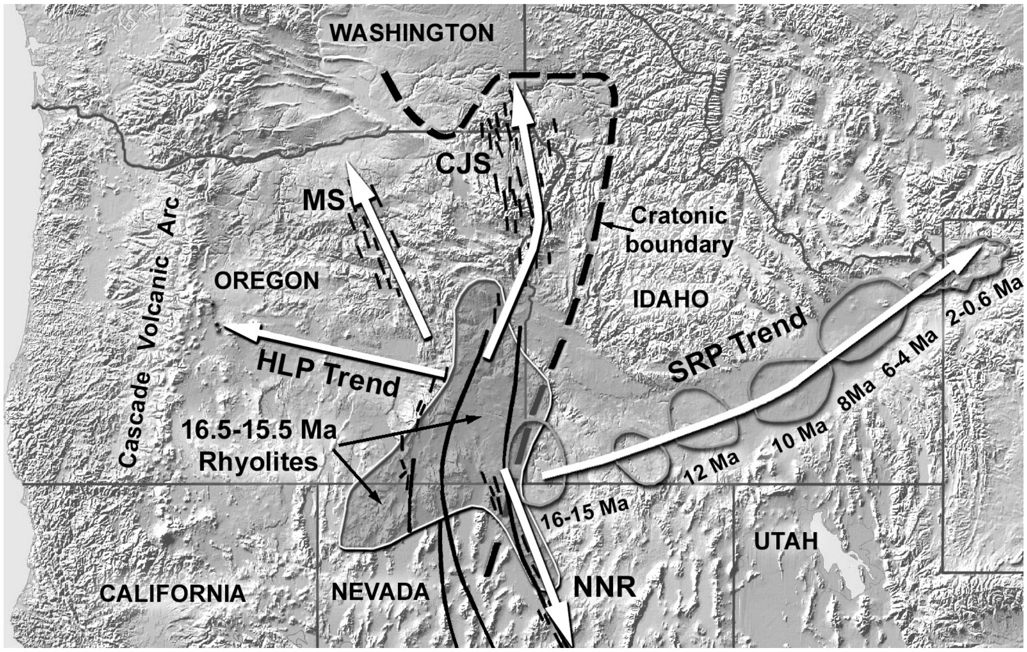

Figure 4. Rhyolite magmatism and migrating volcanic trends associated with the CRBG-SRP magmatic system. HLP, High Lava Plains; SRP, Snake River Plain; CJS, Chief Joseph dike swarm; MS, Monument dike swarm; NNR, Northern Nevada Rift. Circular features along the SRP are age-progressive calderas and rhyolite centers from Pierce and Morgan (1992). Dark lines through the NNR and into southeastern Oregon are curvilinear magnetic anomalies (Glen and Ponce, 2002).

28th January, 2007, Peter R. Hooper

Baksi's remarks of 20th January appear to be aimed primarily at previous publications and are only marginally to do with the CRBG flood basalt as recounted in this volume. All but one of the dated samples he cites are of andesitic and rhyolitic rocks associated with the post-flood-basalt rifting in the Vale-Malheur Gorge area of eastern Oregon.

His criticisms of some of those dates must be answered by the chronological laboratories concerned. As non-specialist geologists we must accept what these labs provide. We did make the effort to have some of the many Open University dates duplicated by the Corvallis laboratory and these showed satisfactory agreement. These ages also tied in well with that of the Steens Basalt from the Berkeley Laboratory and, most importantly, were consistent with the stratigraphy as mapped independently in the field. That these ages are robust is implied by a more recent set of age data; Jarboe et al. (2006) pin down the Steens Basalt magnetic reversal (R0-N0) to c.16.6 Ma and conclude that "the NO of the Columbia River Basalt Group is the C5Cn.3n chron and that the bulk of the of the CRBG (the NO-R1-N1-R2-N2 members) erupted in a short period of time: 0.75 Ma".

Thus, both the old and new dates appear to confirm that the main pulse of the CRBG on the

Columbia Plateau erupted between 16.1 and 15.0 Ma as stated in earlier papers (Hooper et al., 2002, 2004), while, when the earlier Steens Basalt is added, the whole flood basalt eruption excepting the small volumes of late Saddle Mountains Basalt occurred between 16.6 and 15.0 Ma as stated in this and previous papers.

28th January, 2007, Don L. Anderson

Camp et al. (comment of 28th January, 2007; hereafter CHRR) maintain that all the data for the CRBG are consistent with plume impingement, and inconsistent with all proposed alternative models; all non-plume models are viewed as inadequate. But the plume model, taken alone, is also inadequate, as well as implausible. Interpreting tomographic data is not as straightforward or unique as implied by CHRR (e.g. Yuan and Dueker, 2005). Crustal extension, delamination, hydration, eclogite components, mantle wind, focusing, deflection by a slab, edge effects, multiple plumes and so on are required in order to make the plume model viable and to explain why similar tomographic structures elsewhere do not make Yellowstones. There is also the issue of why a deep mantle plume hit this place at this time, and similar questions regarding the other back-arc-plume coincidences. Although the association of plumes with continental break-up had some plausibility, the repeated attempted association of plumes with back-arcs, sutures, accretion and collisions does not.

“The real argument”, according to CHRR, lies in high magma rates over short periods of time. This is not a prediction of the plume model or of any thermal model; it is an after-the-fact rationalization of an observation. This argument is more consistent with a lithospheric stress-valve or extension mechanism, coupled with underplating or ponding; these do not require a plume. Likewise for progressive caldera development.

Many CFB provinces lie along convergent margins–presumably above slabs–and adjacent to cratons. Deccan, Karoo, Keweenawan, Emeishan and Siberia are examples of CFB associated with accretion and convergence, rather than continental breakup; these are all on mobile belts, adjacent to one or more cratons. In these areas, water from the underlying slab and delamination and fertilization from above are the most plausible explanations for low velocities and excess melting. Extensional stress, however, is a pre-requisite for extrusion, as it is for the plume hypothesis.

CHRR argue that at Yellowstone, a mantle plume has been seismically resolved to a depth of at least 500 km. This is not quite accurate (Fig. 5). What has been mapped is a heterogeneous upper mantle, including low-velocity zones (LVZ) near Yellowstone and similar ones elsewhere. It is not known if these features are hot and upwelling. Yuan and Ducker (2005) conclude that the Yellowstone feature extends, at an angle, to 500 km depth (but no more). This rules out a conventional deep mantle thermal plume but is consistent with the fertile blob, top-down, and slab dewatering models, or with water solubility effects (Mierdel et al., 2007). The LVZ decreases in amplitude from 3.2% at 100 km to 0.9% at 450 km. The deep anomaly is offset by

200 km. Other hotspot related anomalies also appear to terminate at 400 km or shallower (Anderson, this volume; Deuss, this volume), consistent with a shallow or top-down explanation.

LVZs elsewhere in western North America are similar in magnitude, volume and depth extent to the Yellowstone anomaly; the volcanic output of these other places is very small, suggesting that the dominant control on volcanic output is strain. This probably also controls rates of magmatism.

The mapping of velocity into temperature is difficult, non-unique and likely impossible, e.g. there is not a one-to-one mapping or even necessarily a correlation between temperature, velocity and density. Dense, cold eclogite sinkers can be LVZs (Anderson, this volume) as can hydrated peridotite. The tomography has been explained by small plumes rising beneath Yellowstone, leaving similar anomalies elsewhere unexplained. If the tomography is due to excess temperature, a ΔT of 200-400˚C is required. Alternatively, fluids from the slab, or low-solidus crustal and lithosphere material, weakened and eclogitized by Laramide hydration promoted destabilization and removal, placing low-velocity fertile bodies into the mantle (Yuan and Ducker, 2005). This latter is the unified explanation for magmatism and LVZs in regions that involve sutures, accreted terranes, arcs, slab windows, over-thickened crust and fluids from underlying slabs. This explanation is not just a crack or a back-arc model; it is a general tectonic model that, by the way, explains the magmatism–or lack thereof–and uplift history (Hales et al., 2005).

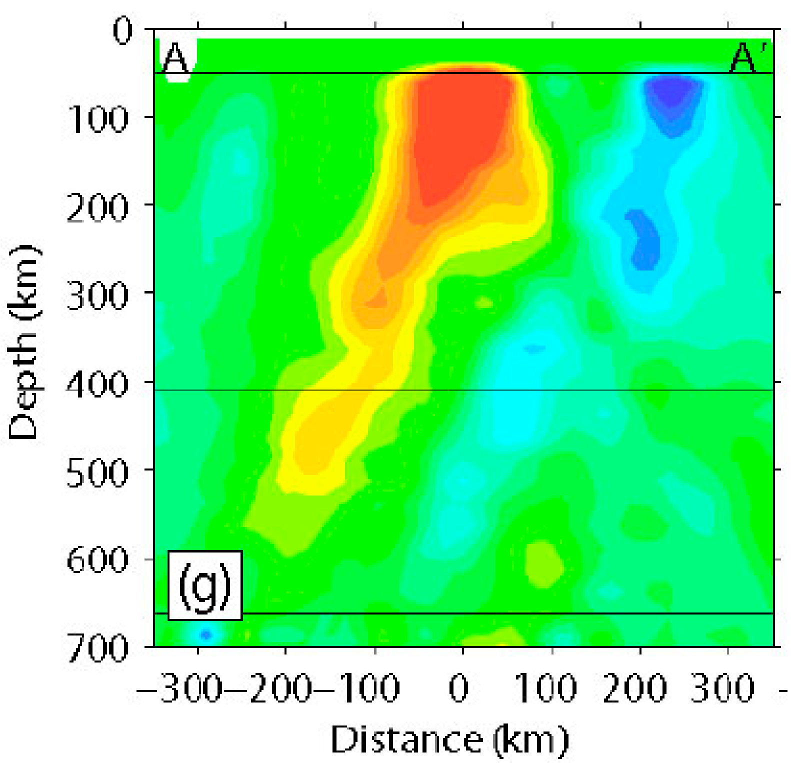

Figure 5. (after Yuan & Dueker, 2005). Tomographic cross-section across Yellowstone. This is the most plume-like section. An active plume upwelling would spread out laterally beneath the plate. The appearance of the feature depends on the orientation, cropping and color scale. Red is low velocity but one cannot infer temperature or upwelling from this kind of data alone. See also Hales et al. (2005), Dueker and Yuan (2004) and Google Images Yellowstone tomography.

28th January, 2007, Alan D. Smith

In reply to the comment of Camp et al. of 28th January, I would like to clarify that it is convection, not extension that serves as the trigger for CRBG volcanism in the back-arc model of Smith (1992). Back-arc upwelling is a consequence of plate interactions, and thus the interpretation offered for CRBG volcanism fits within the "plate model" for the origin of intraplate volcanism. The back-arc model is thus compatible with explanations (e.g. Christiansen and McKee, 1978; Christiansen and Lageson, 2003) which emphasize the role of large-scale plate interactions and lithospheric structure in generation of the Eastern Snake River Plain (ESRP).

Rhyolitic volcanism along the ESRP may show a linear age progression (Pierce and Morgan, 1992), but basaltic volcanism does not, and the plume tail model does not explain its persistence along the length of ESRP after migration of rhyolitic volcanism to the Yellowstone plateau (Christiansen et al., 2002). The low-velocity zone imaged by Yuan and Dueker (2005) also extends at least 350 km along the strike of the ESRP, such that the southwestern part of the anomaly currently underlies rhyolitic centres of 6 Ma age. If the low-velocity zone results from mantle upwelling, the length of this feature and the non-linearity of ages in the basaltic volcanism would be more compatible with shallow non-plume convection mechanisms such as proposed by Humphreys et al. (2000).

31st January, 2007, Alexei Ivanov

In their comment of 28th January 2007, Hooper et al. did not reply to the key point of my comment of 17th January 2007, which is not ambiguity in the geochemical interpretation of OIB signatures. The key point is that the most voluminous Grande Ronde formation shows no sign of OIB signatures at all. Reference to Fig. 8 of Hooper et al. (this issue) does not solve the problem. For example, the caption to the Fig. 8 states that "The proximity of some formations (..., Grande Ronde,...) to OIB source compositions is used as evidence that these formations were derived from an enriched, OIB-like, mantle source." Ironically, OIB is outside the limits of the Ba/Nb vs Nb/Y diagram (Fig. 8b of Hooper et al., this volume). Ba/Nb and Nb/Y in OIB are 7.3 and 1.7, respectively (Sun, McDonough, 1989).

5th February, 2007, Peter Hooper, Vic Camp, Steve Reidel and Marty Ross

Truth, like beauty we suppose, is in the eye of the beholder; so how best to interpret our Fig. 8? Ivanov is correct in noting that the Ba/Nb and Nb/Y ratios of both the Imnaha and Grande Ronde Basalts are outside the average values quoted by Sun and McDonough (1989) for OIBs. We would interpret this as evidence of some contamination of the early magmas with mafic components in the lower crust as advocated by Chesley and Ruiz (1998) on the basis of their Re/Os isotope data.

The logic of our interpretation of Figs. 8a,b (Hooper et al., this volume) is that:

1. the trace element profiles of early Imnaha Basalt, together with their isotopic ratios, closely match Hawaiian Basalt and are distinct from subduction-related magmas (Hooper and Hawkesworth, 1993, Fig. 3),
2. that the separate Imnaha and Grande Basalt trends on Fig. 8 overlap at their more primitive ends and thus are probably derived from a similar source,
3. that the Imnaha trend may be explained by a combination of partial melting and crystal fractionation processes (Figs. 8 and 10),
4. the Grande Basalt cannot be explained by crystal fractionation (Rb/Zr rises with Rb/Sr (Hooper and Hawkesworth, 1993, Fig. 9) and so is interpreted as a mixing array between the original OIB-like magma and a lithospheric component.

Smith has clarified his concept of the upwelling mantle in the back-arc area, which we appreciate, and we re-emphasise the distinction between the magmatism directly associated with back-arc extension in eastern Oregon and the flood basalt eruption. This difference, so obvious in the field, we perceive as critical evidence against interpreting the flood basalt as a product of Smith's back-arc model.

Back-arc extension down the length of the "Inland Empire" of the NW USA, separating the Cascade and Idaho mountains, has been apparent since the subduction zone jumped west in the Eocene. It is seen in the Eocene Republic and related grabens of NE Washington State and has continued until the present. The extensional features are directly associated by calc-alkali to alkalic magmatism, which, erupting along the graben walls, fill the grabens (Hooper et al., 1995; Morris et al., 2000). In eastern Oregon this small-scale, very local, extension-related volcanism both pre- and post-dates the sudden eruption of the huge volumes of monotonously tholeiitic basalt of Steens Mountain and Malheur Gorge (Hooper, 2002; Camp et al., 2003). The extensional-related volcanism (basalt, andesite, rhyolite and siliceous tuffs) erupted along the developing graben walls and fills the 50 km wide Oregon-Idaho graben. Thus while back-arc extension was a factor before and after the flood basalt eruption, it created its own very distinctive type of volcanism and cannot easily be held responsible for the flood basalts for which, we maintain, a thermal anomaly at the base of the lithosphere is the most obvious explanation.

7th February, 2007, Vic Camp, Peter Hooper, Steve Reidel, and Marty Ross

Anderson (comment of 28 January) states that "the plume model, taken alone, is also inadequate," in explaining the genesis of the CRBG. We do not argue that the plume model needs to be "taken alone." Numerous other factors are inevitably involved, as is apparent in our paper. Lithospheric thickness, for example, clearly influenced the location of volcanism, and extension provided a control on the orientation of feeder dikes. The highest eruption rates occurred in the region of least extension (the Chief Joseph dike swarm), which lies contrary to Anderson's claim that the short duration of magmatism is "more consistent with a lithospheric stress-valve or extension mechanism, coupled with underplating or ponding." We agree instead with Smith (comment of 28 January) that mantle upwelling, not extension, was the trigger for CRBG volcanism, but we disagree with his assertion that the short-lived burst of mantle upwelling at ~16 Ma was a consequence of plate interactions. Extension and underplating may be factors, however vaguely expressed, but we assert once again that the eruption of such large volumes in such a brief period of time, during minimal extension, is surely better explained by the abrupt arrival of a large thermal and/or fertile anomaly. The persistence of this anomaly from SE Oregon to the present Yellowstone center, mirroring precisely the westward drift of the North American plate, is too obvious to be so lightly dismissed.

Anderson implies that our preferred model for CRBG genesis is similar to "other back-arc-plume coincidences," and to "the repeated attempted association of plumes with back-arcs." We agree with Anderson and other plume skeptics who see little rationale in applying a plume genesis to explain the style of volcanism common to most all areas of back-arc extension. However, flood-basalt magmatism is markedly uncommon in back-arc settings, the CRBG being the only obvious example. This unusual occurrence surely requires an additional mechanism distinct from the long-lived plate-tectonic processes inherent in all continental back-arc regions.

The failure of seismic studies to identify a mantle anomaly extending >200 km beneath Yellowstone has been the primary rationale for dismissing a plume genesis for the SRP and CRBG (e.g., Christiansen et al., 2002), until such an anomaly was resolved by Yuan and Dueker (2005) to a depth of ~500 km. We bow to Anderson's expertise in seismology, and his argument that the low-velocity anomaly could also be interpreted as a fertile blob of cold eclogite or hydrated peridotite. We must point out, however, that many other geophysicists have interpreted the available data as supporting plume emplacement beneath southeastern Oregon and adjacent Nevada at ~16 Ma, and beneath the Yellowstone caldera today (Parsons et al., 1994; Saltus and Thompson, 1995; Bijwaard et al., 1998; Montelli et al., 2004; Yuan and Dueker, 2005; Waite et al., 2006). Anderson describes the deeper part of the Yellowstone anomaly as being offset by 200 km, and therefore he relates it to "other hotspot related anomalies [which] also appear to terminate at 400 km or shallower,... consistent with a shallow or top-down explanation." Instead, Yuan and Dueker (2005) state, "Our most important result is the resolution of a tilted low velocity plume that extends from beneath the Yellowstone caldera to 500 km depth. Further support for this conclusion derives from the observation of a localized 12 km depression in the depth of the 410 km discontinuity where the Yellowstone plume crosses this phase transition."

Much of this discussion on the chapter by Hooper et al. (this volume) revolves around a single question: What model can best explain:

1. the OIB-like end-member source and high $^{3}He/^{4}He$ ratios found in the CRBG lavas,
2. the short duration and high eruption rates of CRBG volcanism,
3. the radial, migrating trends of basalt eruption from a focused center,
4. the subsequent age-propagation trend along the SRP hotspot track, and
5. the 500-km-deep low-velocity conduit currently centered beneath the Yellowstone caldera?

All contributors to this discussion appear to agree that back-arc extension played a role in the evolution of the CRBG, but they disagree on the extent of that role. In our view, the burst of flood-basalt volcanism at ~16 Ma must have been triggered by an additional mechanism. Non-plume mechanisms suggested by the contributors include:

1. mantle upwelling associated with plate interactions and exploitation of lithospheric structures (Smith, comment of January 28; Christiansen and Lageson, 2003),
2. mantle flow around a residuum body (Smith, comment of January 28; Humphreys et al., 2000),
3. subduction-related magmatism (Ivanov, comment of January 17), and
4. top-down models involving mantle hydration, delamination, and/or fertilization (Anderson, comment of January 28; Anderson, this volume).

Although these non-plume models have scientific merit, we contend that they cannot explain adequately all of the five constraints listed above. We maintain that a plume origin is not only consistent with each of these constraints, but is also the only interpretation capable of combining the genesis the CRBG flood-basalts and the SRP hotspot-track into a single unifying model.

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