**Klaus Hasselmann** has received the Vilhelm Bjerknes Medal "for his pioneering contributions to the theory and modelling of ocean-atmosphere interaction and climate variability, which have improved our ability to predict ocean waves and detect climate change." Hasselmann is an AGU Fellow (Atmospheric Physics and Climate) who joined in 1963.

**Bengt Hultqvist** has received the Hannes Alfvén Medal "in recognition of his important contributions to auroral and magnetospheric physics and his leading role in initiating and supporting space research projects both nationally and internationally." Hultqvist is an AGU Fellow (Magnetospheric Physics) who joined in 1962.

**Philip D. Jones** has received the Hans Oeschger Medal "for his outstanding contribution and tireless effort in reconstructing the climate of the last 250 years at global and regional scales." Jones has been an AGU member (Atmospheric Chemistry) since 1997.

**Peter D. Killworth** has received the Fridtjof Nansen Medal "for his many far-reaching contributions to theoretical oceanography which have significantly enlarged our understanding of the processes determining ocean circulation." Killworth is an AGU Fellow (Physical Oceanography) who joined in 1998.

**Wolfgang Kinzelbach** has received the Henry Darcy Medal "for his outstanding contributions to the sustainable management of groundwater systems." Kinzelbach has been an AGU member (Hydrology) since 1989.

**Christoph Reigber** has received the Vening Meinesz Medal "in recognition of his outstanding contributions in the field of satellite geodesy and the study of the Earth's gravity field." Reigber is an AGU Fellow (Geodesy) who joined in 1987.

**Franco Siccardi** has received the Sergey Soloviev Medal "for his distinguished and pioneering work in the understanding, prediction, and mitigation of natural hazards, and his efforts to promote their interdisciplinary approach." Siccardi has been an AGU member (Hydrology) since 1986.

**Tatiana B. Yanovskaya** has received the Beno Gutenberg Medal "in recognition of her major theoretical achievement in the modelling of surface waves and tsunamis propagation in laterally inhomogeneous media." Yanovskaya is an AGU Fellow (Seismology) who joined in 1993.

# FORUM

## Comment

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The recent summary of the adakite hypothesis by M. Defant and P. Kepezhinskas (*Eos*, 5 February, 2001, p. 65) addresses several important issues that have arisen in regard to interpreting arc rocks during the past decade. Defant and Kepezhinskas imply that slab melting is a common process and that many arc volcanoes are composed dominantly of adakite that has interacted with the mantle wedge on its way to the surface. The interactions are further interpreted as the cause of much compositional (metasomatic) variation in wedge peridotite. I regard many of the statements made by Defant and Kepezhinskas as controversial. The main problems of the adakite model follow:

1) The definition is too broad and ambiguous and does not uniquely identify slab melts. For example, high-Mg andesite made by simple basalt-rhyolite mixing can easily be mistaken for adakite. And Defant and Kepezhinskas regard any silicic melt that is derived from a garnet-bearing source as an adakite. They offer no means of distinguishing slab melts from melts of garnet-bearing lower crust. Defant and Kepezhinskas do not reference geophysical data in their definition, but such data provide a way to discriminate between the two possibilities. In areas where geophysical interpretation strongly suggests lower crustal temperatures high enough for melt production, there is no compelling reason to maintain the adakite hypothesis. For example, in the Cascade Range, high heat flow and shallow Curie isotherms, as well as crustal seismic velocities, indicate that lower crustal melting should be common [*Stanley et al.*, 1990], thus belying Defant and Kepezhinskas' interpretation that Mt. St. Helens dacites are slab melts. "Adakite" at other Cascade volcanoes can also be interpreted as lower crustal melts [*Conrey et al.*, 2001], in agreement with geophysical evidence.

2) Defant and Kepezhinskas list a host of exceptions to their original hypothesis that slab melting is expected only where the downgoing plate is young and therefore hot. The models proposed in these exceptional cases are highly speculative and difficult to test. I propose a much simpler model based on the fact that adakite is consistently associated with high heat flow—for example, in the Taupo Volcanic Zone, Gulf of California, southwestern Arizona, the Cascade Range, Okinawa Trough, and the Komandorsky Basin. This correlation has been noted by Tatsumi and Maruyama [1989]. In areas of high heat flow, melt will be produced at shallow levels; for example, in hydrous wedge or crustal peridotite or lower crustal rocks [*Lagabrielle et al.*, 2000]. Such "adakite" melt will be high-Mg andesite in the case of peridotite melting, and Mg-rich dacite to rhyolite in the case of lower crustal melting, especially in the presence of interlayered peridotite and amphibolite [*Carroll and Wyllie*, 1989]. As far as I am aware, there is no unambiguous case wherein slab melting is the most probable hypothesis because geophysical evidence obviates any simpler model with melt production from shallower sources.

3) Defant and Kepezhinskas state that "hydrous metasomatism is not the only process at work in the arc mantle," a statement with which I am in full agreement. However, I do not accept their implication that adakite metasomatism is primarily responsible for mantle wedge metasomatism. The La-Ta correlation they show is found in other mantle xenolith suites, for example, in the East African rift [*Bedini et al.*, 1997]. Such correlations are suggestive of transport of La and Ta by melt, but the melt is more likely a small degree within-plate-like melt with high concentrations of Ta, not an adakite. Defant and Kepezhinskas propose that adakite metasomatism is Na-rich, but similar Na-enrichment is common in mantle xenoliths recovered from oceanic islands related to plumes. Vein glasses in the Kamchatka xenoliths that they cite as type examples of adakite metasomatism are, in fact, most similar to within-plate-like xenolith glasses with very high Na/K, and not arc-related xenolith glasses [*Schiano and Bourdon*, 1999]. Defant and Kepezhinskas further claim that there is a special class of arc basalt that is enriched in Nb due to the operation of adakite metasomatism. This claim is particularly difficult to accept as they offer no means of distinguishing such Nb-enriched basalt from typical within-plate basalt produced by small degrees of partial melting. It has been apparent for many years that arc basalt grades into within-plate basalt at both the ends of arcs and in back arcs. Furthermore, in time many arc suites are succeeded by within-plate-like basalt suites and vice versa. Arc basalt may be coeval with within-plate basalt, especially in the presence of extension [*Conrey et al.*, 1997]. These facts suggest to me that typical within-plate processes have affected the sub-arc mantle, and the effects may persist in the presence of arc volcanism. In arcs where extensional processes operate—for example, Cascades and Kamchatka—decompression melting may simply yield within-plate-like melt containing typical within-plate-like xenoliths. Defant and Kepezhinskas claim that "nor has there ever been an OIB mantle composition found in arc xenoliths" is belied by their own xenolith collection from Kamchatka, as well as by xenoliths at Simcoe volcano in the Cascade arc.

4) Defant and Kepezhinskas claim that modern adakite is an analogue for Archaean tonalite-trondhjemite suites. This claim is controversial, with both detractors and supporters, and I do not intend to evaluate the older rocks here. However, the adakite interpretation is speculative, and thus any application to former times is suspect.

In summary, my principal objections to the adakite model are its ambiguity of definition and lack of integration with measured geophysical properties. I do not argue that slab melting does not occur, nor that such melts do not metasomatize the mantle wedge. Rather, I find the existing adakite model to be of little utility due to its lack of precision in identifying slab melts and their metasomatic products.

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Peter Hooper kindly reviewed this comment and suggested several significant improvements.

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# Reply

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We are grateful for this opportunity to elaborate upon our ideas. R. Conrey has conveniently numbered his thoughts, and therefore, we will address them using his order. But first a few remarks about his general comments.

We have never implied that slab melting is a "common process" and that many arc volcanoes are composed "dominantly of adakite." In fact, we emphasized that Drummond and Defant [1990] were correct in their interpretation that adakite production has vastly decreased since the Archean because less young crust subducts today. Although there are about 10–15 current adakite-producing regions, this is certainly not a great deal compared with the extent of "regular" arc volcanism on the planet.

1) We agree that the term adakite has been ambiguous. We have always allowed for the possibility of lower crustal melting, and there is little way to differentiate a slab melt from a lower crustal melt. We attended a symposium in China on adakites and discovered that various Chinese researchers have found some fairly strong evidence of lower crustal melting in eastern China. Based on this meeting and a subsequent paper [*Defant et al.*, 2002], we conclude that adakite should be a general term used for those rocks with adakitic geochemical characteristics and should have no "process" implications. But we also must emphasize that adakites are probably predominantly slab melts, because most of the time the crust is not thick enough to produce garnet stability (> 40 km) in the lower crust. Therefore, only a few regions can produce adakites via lower crustal melting; for example, perhaps the Andes or eastern China in the past.

Conrey contends that adakites can be produced via mixing of a basalt and rhyolite. Adakites have a distinct trace element pattern—for example, high Sr/Y—which cannot be produced via mixing processes unless the silicic end-member has been derived from the slab or lower crust. Normal basalt-rhyolite mixing could never produce an adakite from a trace-element perspective! There is a copious amount of experimental data to substantiate our observations.

There are only a few ways of distinguishing a lower crustal melt in the garnet stability field from a slab melt. Certainly, they can be absolutely identical in composition. However, the association with adakites and young crust is strong evidence that these adakites are derived from the slab. In addition, adakites associated with the plethora of mantle xenoliths, many of which have reacted with a slab-like melt, found associated with Nb-enriched arc basalts (NEAB) are also difficult to explain from melting of the lower crust (where would the mantle xenoliths with adakitic melts come from?). Finally, the high-Mg content of some adakites is difficult to explain via lower crustal melting. These melts have to pass through the mantle, where they interact to give the lower Si and higher Mg concentrations of high-Mg adakites (see *Rapp et al.* [1999]). Kay et al. [1994] proposed that a delamination of the lower crust could cause melting of the top of the sinking crust. It provides an explanation for some of the high-Mg adakites found in some thick-crust arcs.

Conrey suggests that there is no compelling reason to maintain a slab melt hypothesis in the Cascades because "geophysical interpretation strongly suggests lower crustal temperatures high enough for melt production." Defant and Drummond [1993] provided extensive evidence that Mount St. Helens (MSH) is derived from the slab based on geophysical and geothermal evidence, in addition to geochemical evidence. The slab being subducted below MSH is the hottest in the world. The fact that MSH sits in front of the arc where slab melting should take place, based on experimental data, is a very compelling tectonic/geophysical reason for slab melting. Mount Adams, behind MSH, sits in the typical position of an arc volcano and, as far as we know, has no slab melts. Why has lower crustal melting occurred at MSH and not at Mt. Adams?

2) We have no difficulties with Conrey's proposal that adakites are the result of high heat flow in some areas. The glaring problem with this hypothesis is that we have adakites in many arcs where there is no high heat flow and the crust is too thin to derive adakites (e.g., the Philippines). How can an adakite be derived from melting of the lower crust when the crust is as thin as the "Gulf of California" for example? High heat flow can cause melting, but it cannot cause garnet stability. Garnet needs to be stable in the source to produce an adakite and all the high heat flow without the requisite pressure will never succeed. We feel that Conrey's argument is effectively based on trying to make the rest of the geology of arcs conform to what he perceives is taking place in the Cascades.

2) One of the gross mistakes made by Conrey seems to be consideration of only the major-element content of our glasses in xenoliths. But we, and other colleagues [e.g., *Schiano et al.*, 1995], have analyzed trace elements in many glasses from mantle xenoliths in NEAB and found them to be precisely the composition of adakites derived from a garnet amphibolite/eclogite source, for example, Kamchatka, The Philippines, Lihir, Grenada, etc. In each case, the glass is exactly the composition of adakites. In addition, the mantle xenoliths contain precisely the requisite mineral reaction products expected from the interaction of adakites and mantle peridotite such as sodic plagioclases, high-Al and Na clinopyroxenes, pargasitic amphiboles, pyrope-grossularite garnets, and Al-rich spinels [e.g., *Kepezhinskas et al.*, 1995; *Rapp et al.*, 1999]. We are befuddled by Conrey's insistence that these glasses could not be slab melts and must be small partial melts from the mantle. The only conclusion we can reach is that he must be considering our major element data only.

Conrey also claims that we offer no way of distinguishing between NEAB and OIB. We actually have made painstaking effort in our articles to distinguish between them. But don't take our word for it. I suggest Conrey read those papers by Sajona et al. They have also extensively elaborated on the difference between NEAB and OIB. The fact that some NEAB have MORB-like isotopic characteristics provides a clue that they differ from OIB. But perhaps the most conclusive part of our work is the fact that the mantle xenoliths contained within NEAB have, in many cases, interacted with adakitic glass clearly derived from a slab; for example, high in Sr/Y and La/Yb and low in Y and Yb. In fact, in many arcs we have found adakites and NEAB associated with