Orogenic Andesites and Crustal Growth

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Orogenic Andesites and Crustal Growth

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Foreword

The editors of this volume invited a brief retrospective about how its 16 chapters reflect progress since my 1981 book *Orogenic Andesites and Plate Tectonics* (*OAPT*) on a similar topic. The epilogue of *OAPT* concluded that the most frequent mechanism, and therefore the most fundamental process, of andesite genesis is low-pressure crystal fractionation of anhydrous phenocryst phases from primary basalt that resulted from partial melting of peridotite mantle that had been metasomatized by fluids or melts from the subducted slab. That is, Bowen, Kuno and Osborne got most of it right before plate tectonics. The last page of *OAPT* concluded that the chief 'andesite problems' for the future were the relative proportion of slab-derived components in the mantle source, the details of the crystal fractionation process, the frequency of other differentiation mechanisms, and the spatial and temporal variations in these parameters. It may be instructive to compare the results of the 16 papers of the present volume with those conclusions.

In the introduction, the editors discuss six topics and especially the relative proportion of slab-derived components v. peridotite in the mantle source of arc magmas (their Fig. 1). They summarize two end-member options as (1) the 'basalt-input model', in which andesites are crustal-level differentiates of partial melts of mantle peridotite (attributed to *OAPT* by the editors as 'an elegant explanation that linked old-school petrology with the conceptual revolution of plate tectonics'), v. (2) 'slab/mantle models', in which andesites are primary melts of sub-crustal (slab-derived and peridotite) mantle sources. To me, these two options lie within a continuum of sources in which the percentage of slab-derived input to the mantle wedge ranges upward from $<1\%$.

The present volume shows that there is indeed no consensus about the mass fraction of slab-derived sources: no one number applies to all subduction zones and all times in Earth history. I note, however, that even the maximum amount invoked in this volume is $<18\%$ and applies to 'hot-subduction zones', such as Mexico and SW Japan. The slab surface temperature beneath the volcanic front of such arcs is estimated to be $>900\,^{\circ}C$ using the '80 km decoupling' model of Syracuse *et al.* (2007). The only papers in this volume that deal with the more usual case are about Guatemala and the Marianas, and they discuss rocks more felsic than andesite. In both, the felsic rocks are interpreted as direct or indirect differentiates of mafic melts of peridotite (Singer *et al.* and Stern *et al.* respectively). Most current papers about these more typical subduction zones debate whether the mass fraction of slab-derived material is closer to 1 or 5%; whether the slab component is added to the mantle wedge as a solid, dilute low-temperature fluid, high-temperature silicate melt or something in between; how the mass fraction of slab component relates to the percentage of mantle flux melting; and the mass fraction of basaltic crust v. sediment in the slab source.

It may therefore be concluded that few if any orogenic andesites are primary melts of purely mantle peridotite plus water, the possible exceptions bearing names like boninite and high-magnesium andesite. In this volume, adakite seems to be distinguished from the above by lower Mg–Ni–Cr contents, and three papers regard the most mafic of them as primary partial melts of young pyroxenitic mantle formed by recent reaction between slab melts and mantle peridotite (i.e. modified mantle wedge). All three cases are from hot subduction zones. To me, this topic reprises the evolution of the topic from the seminal papers by T. H. Green & Ringwood (1968) to Ringwood (1974), both from the Australian National University (ANU). The first argued that typical calcalkaline andesite is a primary melt of dry subducted eclogite whereas the latter argued that parental arc magmas are mafic melts of modified mantle wedge. My PhD at the ANU lay between the two papers. As part of my thesis, I created a forward model of arc magma genesis to test the ability of the Green and Ringwood hypothesis to explain the suite of rocks from which Green selected the andesite for his experimental study. I concluded that the suite could not be explained as a product of partial melting of the slab (Gill 1974), and presented this to Ringwood in his office. He quickly understood the implications and modified his story to emphasize the reaction products between peridotite and felsic melts at 2–3 GPa. Others soon explored the implications further (e.g. Kelemen 1990), but Ringwood (1974) presented the first heuristic version and cartoon image of the 'slab/mantle model' of this volume.

It appears that this model is a reasonable interpretation of some high-magnesium andesites in hot subduction zones, even when an obvious garnet signature is absent. Indeed, there is a continuum between high-magnesium andesite, more typical calcalkaline basalt and arc tholeiitic basalt as the mass fraction and character of slab-derived component in the mantle source changes, and how this affects the degree of flux melting. The most fundamental modification of the 'basalt-input model' since *OAPT* is that current geodynamic models of subduction zones (e.g. Kelemen *et al.* 2003; Syracuse *et al.* 2007) and current understanding of trace element partitioning between eclogite, fluids and melts (e.g. Johnson & Plank 1999; Kessel *et al.* 2005) agree that at least the sediments, and often some of the mafic rocks, of subducted ocean crust *melt* beneath most and perhaps all arcs. Reaction between that melt and the overlying mantle results in flux melting of the modified mantle wedge in which the percentage melting of the wedge is proportional to, and greater than or equal to, the mass fraction of slab flux added (e.g. Kimura *et al.* 2010). Consequently, arc magmas come from recently modified mantle as Ringwood foresaw. This implies that slab melting usually occurs at the water-saturated solidus of sediment and basalt, which in turn often requires an external source of water and some degree of focused fluid flow through the slab. It also opens the potential for relatively small degree slab melts in which accessory minerals might be stable and buffer trace elements.

On the other hand, I found no evidence in papers of this volume that whole suites of rocks from one volcano or one arc are related to one another by partial melting of the modified mantle wedge. Rather, I found evidence in most papers that the suites are related by differentiation processes within the crust (i.e. within the stability field of plagioclase or hornblende or both). Even parental andesitic magmas with $>5\%$ slab components seem restricted to hot subduction zones.

Modern micro-analytical techniques indicate that not all the crystals in some (and perhaps most) orogenic andesite crystallized shortly before eruption from a magma with the composition of the host rock. The 'basalt-input model' is not often that simple, but was never claimed to be. To me, the most fundamental point about this topic is that most arc crust, and most of the diversity in trace element and isotope ratios of arc magmas, come from the modified mantle wedge and not old unrelated crust. That arc magmas contain crystals or melts or both from multiple magma batches over 10^2–10^5 years has no consequence for these two conclusions. Cannibalizing the products of earlier differentiation events, or recharging the system with heterogeneous younger magma just adds scatter to differentiation trends.

The introductory chapter concludes that papers favouring the origin of orogenic andesite by fractional crystallization of parental basalt may be qualitatively convincing but lack quantitative models of purported liquid lines of descent even for major elements. Three reasons are given for this failure of the 'classical model': difficulty finding rocks that are strictly related to one another even in one volcano; difficulty for even current quantitative models to handle the range of likely variables (pressure, temperature, f_{O2}, volatile content) during magma evolution; and the potential for the 'crystal cargo' of minerals to be unrelated to the host magma. *OAPT* discussed the first two of these, and micro-analyses of minerals and their inclusions increasingly demonstrate the third. To me, this is a question of the glass being half-full or half-empty. What mass fraction of crystals in a rock lies outside the range that can be related to the host by varying the magmatic variables? What mass fraction is assimilated from much older and unrelated crustal rocks, or mixed from partial melts thereof? From my own experience with arc basalts, basaltic andesites and even most andesites, the answer to both questions usually is 'small' (minority and perhaps $<10\%$) – especially in oceanic arcs or where continental crust is <30 km thick. If so, then most of the mass of arc crust, and most of the diversity in trace element and isotope ratios in arc magmas, comes from the modified mantle wedge.

To me, the greatest difference from the *OAPT* viewpoint is in the chapter by Kent in which he, following Reubi & Blundy (2009), shows a paucity of andesitic melt inclusions in arc volcanoes despite the abundance of andesitic lavas. Although this may be biased by selection of the host minerals, he argues that eruptible andesite melt is rare, and that andesite lavas are instead physical mixtures of more mafic recharging magma plus older more felsic magma or mush that stalled in the crust. Although this idea still requires differentiation of basalt to andesite melt in arc crust, it adds that erupted andesite magma usually is not this melt. An example might be the 1968–2010 eruption of Volcan Arenal in Costa Rica that is the subject of a journal special issue. Its crystal-rich basaltic andesite lavas contain few andesitic melt inclusions but the temporal change in the lavas can be interpreted quantitatively as being magmas along a liquid line of descent that was a largely closed system for the first half of the eruption by volume and a recharging open system for the second half. The mass fraction of 'unrelated' crystals and melts (i.e. those >500 years older, or that could not precipitate from host magma compositions) may be small: see the Introduction to that issue (Gill *et al.* 2006).

Four other major topics are raised in the chapters of this book: the timescale of slab recycling through the mantle and the residence time of magma in the crust, the temporal evolution of arcs over tens of millions of years, and the need to lose mafic cumulates from the crust if primary arc magmas are basaltic. These were also discussed in *OAPT* and much has been learned about all of them since then. To me, the principal change has been the lack of confirmation about *OAPT*'s model of temporal evolution from tholeiitic to calcalkaline $\pm$ shoshonitic suites.

The concluding chapter of *OAPT* posed several other problems and questions for future research, such as the following: are the differences between tholeiitic and calcalkaline suites due to differences in primary magma composition, water content and f_{O2} control, or are the latter just more affected by magma mixing or crustal assimilation? By what processes do melt and crystals separate in arcs and how are the crystals in rocks related to the fractionating phases during chemical evolution of erupted magmas? How can one distinguish between effects of crustal-level 'assimilation' v. mantle-level 'metasomatism'? These and other topics continue to be active research areas in a field that remains vigorous three decades on because of its fundamental importance for understanding what makes our planet so unique and its volcanoes so hazardous.

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