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Publication date:
2011-03

Permanent link:
https://doi.org/10.3929/ethz-b-000031402

Originally published in:
Journal of Petrology 52(3), https://doi.org/10.1093/petrology/egq096

JOURNAL OF PETROLOGY | VOLUME 52 | NUMBER 3 | PAGES 435–438 | 2011 | doi:10.1093/petrology/egq096

Reply to Gualda & Ghiorso Comment on 'A Metamodel for Crustal Magmatism: Phase Equilibria of Giant Ignimbrites'

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RECEIVED NOVEMBER 24, 2010; ACCEPTED DECEMBER 21, 2010
ADVANCE ACCESS PUBLICATION JANUARY 27, 2011

We welcome the opportunity to clarify issues raised by Gualda & Ghiorso (G&G, hereafter) on our 2010 paper, 'A metamodel for crustal magmatism: phase equilibria of giant ignimbrites'. Voluminous (V ~100–1000 km^3) explosive eruption of silicic magma, accompanied by eruption columns that may empty into the stratosphere, has environmental consequences of planetary significance and represents an important mechanism for the growth of continental crust. Understanding the origin and evolution of silicic volcanic systems is hence critically important. We have described quantitatively (Fowler *et al.*, 2007; Fowler & Spera, 2008; Fowler & Spera, 2010) how cooling and crystallization and/or assimilation can lead inherently to destabilization and eruption of several well-known magmatic systems. Fig. 15e (Fowler & Spera, 2010) summarizes the lithostatic pressure, melt fraction, and initial wt. % H_2O conditions that may lead to destabilization. One contentious point raised by G&G concerns our thermal timescale estimate for evolution of the Long Valley magmatic system culminating in the Bishop Tuff eruption (G&G support a shorter timescale). The other deals with the compatibility of our conclusions with data from Bishop Tuff minerals, melt inclusions, and pumice.

Regarding our timescale estimates for development of Long Valley rhyolitic liquids from a mafic liquid parent, we described (Fowler *et al.*, 2007; Fowler & Spera, 2010) a scale analysis aimed at providing a system thermal lifetime estimate for a particular set of initial conditions and configuration: cooling of a fixed mafic magma mass, assuming closed-system behaviour (no thermal or mass recharge), a simple magma body shape, a fixed heat flux, and eruption of some fraction of differentiated magma formed by fractional crystallization. As opposed to a scale analysis, a proper thermal model would necessarily include computation of conjugate heat transfer rates across a magma body–host-rock boundary zone. For a particular system, such a thermal model should account for the three-dimensional magma body geometry (size, shape, and depth), the time history of magma recharge (very difficult to gauge without real-time geodetic data; e.g. Lundgren *et al.*, 2003), host-rock thermophysical properties (notably thermal conductivity and permeability structure, including anisotropy; e.g. Rosenberg & Spera, 1990), the regional stress field (an important feedback to permeability and, hence, heat transfer), and detailed crustal stratigraphy. Sufficient spatial resolution (of the order of 10 m) would be required, based on conjugate heat transfer zone thickness. Initial conditions, generally unknown, would also need to be specified. Computation of such a model is extremely challenging, requiring a great deal of focused effort. We have not made this calculation (neither have G&G) because there are so many uncertain quantities as to render any 'detailed' model as uncertain as the simple scaling estimate embodied in equation (1) of Fowler &

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Spera (2010). Wohletz *et al*. (1999) performed a calculation in the right direction, although still somewhat simplified, for the silicic magmatic system of the Phlegrean Fields, Italy. They computed more than 50 two-dimensional (2-D) thermal models based on diverse system specifications with varying parameters, including those relevant to our thermal scale analysis. In fact, our simple scaling calculation yields timescales for the Campanian Ignimbrite system that bracket their detailed 2-D model results, providing some confidence in the applicability of scale analysis to this rather complicated problem. In the end, however, one must never confuse a scale analysis from results based on a model.

Because G&G have not computed a thermal model for the Long Valley system, they can only speculate, as we did (Fowler & Spera, 2010), on appropriate parameter values to use in a scale analysis. We make no secret of the origin or uncertainty of the values used. The volume of erupted magma is based on observation. Melt fraction, enthalpy or heat loss, and density are directly linked to our phase equilibria calculations. G&G claim that the heat loss required to explain crystallization is strongly model-dependent. We agree wholeheartedly with this self-evident statement; it is one of the points of our metamodel framework. One parameter, α, the fraction of differentiated melt within the magma reservoir that erupts, is very uncertain. The works by Smith & Shaw (1975, 1979), Smith *et al*. (1978), Crisp (1984) and White *et al*. (2006) are among the studies that attempt to address this issue, through examination of repose times of repeated eruptions from the same large-volume magma body. Fluid dynamic models (e.g. Spera, 1984; Blake & Ivey, 1986; Spera *et al*., 1986; Trial *et al*., 1992) for withdrawal of magma from large crustal reservoirs also inform α estimates. The conclusion is that α varies between unity and 0·1. A factor of 10 variation in α changes the timescale by a factor of ~2. In our scale analysis, we have used α values of 0·8 and 0·4. Like us, G&G do not know the average heat flux in the Long Valley Caldera region over the 'incubation' period for formation of the Bishop Tuff. Where we describe our Bishop Tuff time scaling (Fowler & Spera, 2010), we compare the present-day measured heat fluxes (q) at Long Valley and Yellowstone, and we explicitly state that the estimated thermal timescale is inversely proportional to q. It is evident that using the Yellowstone q value shortens the Bishop Tuff timescale from ~1·5 Myr to 150 kyr. G&G, in their comment, simply repeat what we state. In any case, using a q value from Yellowstone, >1200 km to the NE, does not seem any more appropriate than using an estimate based on Long Valley measured values, especially considering that Yellowstone hosts what is arguably the world's most active hydrothermal system (Husen *et al*., 2004) and has a longer and more voluminous record of magma output (more than a factor of three) than Long Valley. The bottom line is that the scale analysis yields a range of possible minimum timescales—from ~1·5 Myr to 150 kyr. In addition, we have chosen to use heat flow values that are based on observation rather than analogy, because of the significant differences between Yellowstone and Long Valley. The computational models of Wohletz *et al*. (1999) show that surface average heat flux decays rather slowly in shallow geothermal systems, as the rate-limiting step is heat conduction through a conjugate boundary layer separating the magma body from the overlying vigorous hydrothermal system (Spera *et al*., 1982; Nield & Bejan, 1992). Consequently, the rate at Long Valley today may not be 10 times smaller than what it was ~1–2 Myr ago. Without a detailed thermal model, to speculate further on Long Valley heat flow at 1–2 Ma is not profitable.

The thermal timescale issue raises additional, rather more general considerations. An important implication of our metamodel is that the notion of a thermal timescale is meaningless unless system specifics are defined. For example, in an open system undergoing mafic recharge and episodic eruption, the thermal 'timescale' is very different from the characteristic timescale of a closed system such as the ones that we studied. In the limiting case of continuous recharge with episodic eruption and efficient mixing, the 'timescale' is infinite because the integrated system is in a thermal steady state. The timescale G&G allude to, of ~3·8–2 kyr (unpublished data), is based upon closed-system, isothermal–isobaric gas-saturated fractionation of a liquid that is initially rhyolitic. Leaving aside for now the question of how useful MELTS is for elucidating distinct evolutionary paths over limited compositional ranges in silica-rich liquids (except to note that MELTS often cannot return a converged solution close to the solidus), that starting with a rhyolitic liquid to achieve a slightly different rhyolitic liquid results in a short timescale is hardly surprising. Such an exercise trivializes the origin of rhyolite because it does not address the derivation of the starting rhyolite. It is very different, obviously, from starting with a basaltic liquid and deriving, after a great deal of fractional crystallization, a rhyolitic liquid, as we have done. We do not know the history of Long Valley magma chamber recharge, so our approach is to define a simple system (closed-system fractionation from a precursor basaltic parent sample), forward model the characteristics of the rhyolitic magma generated, and then compare predictions and observations. Even more fundamentally, we hypothesize, advocating the unity of nature, that other high-volume silicic ignimbrites might have an evolution that is similar. The question we pose is: to what extent may all six of the great eruptions in western North America within the past ~2 Myr share a common petrogenetic evolution? We find the answer surprising and remarkable. Without any gross inconsistency, they might

very well be derived in the same fashion. Below we address some of G&G's specific comments.

Comments by G&G suggest that, despite our best efforts (concluding remark (6) pages 1817–1819 including Fig. 20 in Fowler & Spera, 2010), we have failed to communicate clearly one of the main outcomes of our broader study. We are not claiming that silica-rich liquids in the systems we have studied were stable over timescales >1 Myr. According to our calculations, the bulk of time spent evolving from the liquidus to the solidus involved more mafic liquids, with high-silica liquids stabilizing over a much shorter period of time, very late in the process. In fact, the very generation of this water-rich, bubble-bearing silicic liquid ensures its eruption owing to dynamical instability. Let us be clear: G&G claim that data from Simon *et al.* (2007) and Crowley *et al.* (2007) are inconsistent with million-year timescales, with Crowley *et al.* (2007) even supporting millennial timescales. We are aware of this work, having cited it in support of our conclusion that distinct phases may have distinct age ranges; millennial timescales are not inconsistent with derivation of rhyolite from a mafic precursor. Based on phase equilibria, one would expect zircon to crystallize from the liquid only upon attainment of sufficiently high Zr concentrations to stabilize zircon. Consistent with our studies, Crowley *et al.* (2007) concluded that zircon saturated immediately prior to eruption and that zircon crystals formed over a 10–20 kyr time period. We would expect zircon to crystallize at the very end stages (last few per cent) of a protracted history. Furthermore, we do not say, as G&G claim, that our thermal timescale suggests sanidine stabilization at >1 Ma. Instead we state 'we maintain that accumulation of the Bishop Tuff magma began earlier, well before sanidine stabilized, at >1 Ma'. The interested reader is invited to examine fig. 20, and the accompanying text and conclusions of our study (Fowler & Spera, 2010), where we state that our minimum model stabilization age for sanidine is >400 ka. We point out that this value and the existence of a difference in mean ages for sanidine and zircon crystals coincide with observations from Simon *et al.* (2007).

We are confused as to the significance of the G&G conclusion that gas-saturated, closed-system crystallization under nearly isothermal conditions is the dominant process, because our calculations subsume that very same process. That is, generation of a rhyolite by closed-system fractional crystallization of a mafic precursor leads to the same result. At the very end stage of evolution where rhyolite is stable, alkali feldspar and quartz precipitate over a small temperature interval, essentially isothermal, within uncertainty (Fowler & Spera, 2010, fig. 16). This phenomenon is highlighted in our earlier studies and is a prominent conclusion of Fowler & Spera (2010).

G&G suggest that the lack of chemical zoning in phenocrysts is incompatible with fractional crystallization. However, we do not find this argument persuasive. First, the timescale for crystal-scale diffusive homogenization is short. For example, Na/K zoning in alkali feldspar phenocrysts could be wiped out rather quickly at magmatic temperatures. Inter-diffusion of Na and K in alkali feldspar at ~900°C is characterized by a mutual diffusion coefficient of $10^{-16}\,m^2\,s^{-1}$ (Christoffersen *et al.*, 1983). For a 3 mm phenocryst, the diffusion homogenization time is ~3 kyr. Evidence for an earlier history of fractional crystallization could be wiped out over longer time frames. Also, as we showed previously (Fowler *et al.*, 2007), the time required for physical fractionation (sinking) is likely to be short relative to the thermal timescale, especially at temperatures higher than the pseudoinvariant temperature. Therefore, crystals sampled in an ignimbrite deposit very probably do not preserve a long record of magma evolution. The crystals that are present in Bishop Tuff samples are largely consistent with those that our modelling predicts to be in equilibrium with liquids near the end stages of crystallization. The bottom line is that zoned crystals and heterogeneous melt inclusions in eruptive deposits are not an inevitable outcome of fractional crystallization. A point that we have made is that there are limitations in inferring an entire evolutionary scenario from crystals within sampled large-volume eruptive deposits.

Concerning the comments made by G&G about the metamodel, we conclude that they misunderstand its function. The concept of the metamodel stands alone. By definition, it is not an explanatory model for any given system. And it is not appropriate only in geochemical space. Instead, its purpose is essentially taxonomic. Any kind of data may be used in conjunction with the metamodel, and it is not tied to the methods we have used or the evolutionary paths that we have investigated. Like all of the scenarios we have investigated, our model fits into the metamodel. We believe that it provides a useful and timely framework for the plethora of ideas, both modern and historical, based on a variety of techniques, concerning silicic melt evolution.

Regarding our particular modelling approach, G&G write 'the postulate of single-step crystal fractionation from mafic–intermediate parental magma is just that, a postulate. And it is, at best, only as likely as many other conceivable "just-so" scenarios in the language of Fowler & Spera (2010).' In fact, our approach is to hypothesize the absolutely simplest model of rhyolite origin (closed-system fractionation of a mafic precursor) that is consistent with geological evidence, and then examine the consequences of this assumption (i.e. test the hypothesis) for a number of spatially unrelated systems. We tested a hypothesis quantitatively using consistent reasoning. We did not invent or call on any exotic or quantitatively ill-defined phenomena. We find that most of the data for each system are consistent with closed-system crystal fractionation of a water-rich

precursor mafic magma at middle to upper crustal depths, and that evolution led inherently to eruption. Was the reality so simple? Probably not. But at the same time, this simple scenario cannot be ruled out. We believe that a simple hypothesis should be favoured over a complex one when both are approximately consistent with observation. We are confused as to what aspect of our reasoning G&G consider 'extreme'.

They state 'to the extent that fractionation of basalt in the lower and upper crust leads to similar results, it really does not matter, from the geochemical standpoint, whether fractionation is step-wise or in a single step. But to infer the spatio-temporal properties relevant for the generation and evolution of silicic magmas, it is necessary to use a model that correctly captures the physical properties of the specific magma bodies that may have existed in the crust (the "just-so" scenarios).' First, based on our conclusions, the extent that fractionation of basalt in the lower and upper crust leads to similar results is relevant only for the first ~25% of crystallization. Continued crystallization at distinct crustal depths in fact does lead to divergent results—a phenomenon that leads us to reject the Hildreth & Wilson (2007) step-wise hypothesis for Bishop Tuff petrogenesis. So in this case, it does matter whether fractionation is step-wise or not. Also, we do not define a 'just-so' model as one that 'correctly captures the physical properties' of magma bodies; nor are we entirely sure what G&G have in mind as far as the nature of 'physical' properties is concerned. Unfortunately, we cannot turn for guidance to G&G's published work on this issue, because as far as we know, none exists. We would like to point out that our modelling is self-consistent and it incorporates as constraints a wide variety of available geochemical, laboratory-based, and field data.

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