REPLY

Reply

Bruce Marsh

Most of the sills described so far in this chapter [Palisades, Shiant, Dillsburg, Elephant's Head dyke, Black Jack, Tasmanian dolerites, Ferrar dolerites] can be said to exhibit certain features in common. They are sills in which crystal settling has probably taken place, although in most cases this is confined to the earliest stages of crystallization and involves minerals, usually olivine, present in the magma at the time of emplacement.

Wager and Brown (1967).

Introduction

The central point of contention of Latypov et al. (2014; see Conclusion) is: "… it is his [Marsh's] conclusion *that these processes preclude differentiation of phenocryst-free magma in crustal chambers* that we call into question. While we do not doubt the transient existence of crystal suspensions and slurries within magma chambers or that phenocrystic magmas can be emplaced into crustal chambers, it is with Marsh's hypothesis that *layered intrusions are formed by the repeated emplacement of crystal slurries* that we take issue… The hypothesis is in any case no panacea: It simply removes the origin of the phenocrysts to another, conjectural and inaccessible magma chamber while leaving largely unexplained the nature and ultimate fate of the carrier magma. Marsh's hypothesis denies progressive in situ fractional crystallization in any magma chamber and

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hence disallows the derivation of crystal slurries with phenocrysts and melts that change systematically in composition with cooling in any feeder system. The key attribute of the hypothesis essentially prevents the formation of layered intrusions anywhere." [Authors' italics].

A careful reading and in depth understanding of my works illuminates and firmly negates this.

What my work does say is that magmatic systems, especially large, active systems, are vertically highly spatially integrated; this is the definition of a magmatic mush column. The end result, as a pluton, sill, or volcano is a reflection of this overall process and is not simply the result of crystallization of an isolated, encapsulated vat of initially crystal-free magma. And, most assuredly, crystallization does take place in situ, but mainly in marginal solidification fronts within which all the familiar processes of Bowen's reaction series may take place. Inward of these advancing solidification fronts, which is the hottest part of the body, there is generally no crystallization. The one exception is possibly in water-saturated magmas that have a thermal ascent trajectory taking them, especially under adiabatic conditions, straight toward the solidus; crystals will then be generated everywhere, congesting and immobilizing the magma (e.g., Marsh and Coleman 2009). But the ultimate origin of crystals in most magmas is in solidification fronts and also in the associated wall rock widely spread throughout the associated underlying feeder system or mush column. Thus, the disruption of solidification fronts and wall rock during ascension, which is unavoidable, may entrain and transport swarms of crystals that function as slurries, unavoidably chemically react with the melt and go, along with further crystallization, compaction, and porous flow, to produce layered intrusions. To deny that magmatic systems beneath ocean ridges, large volcanic centers, rifts, and other intrusions function like this is untenable.

To be even more clear, the building of a fundamental base in any science stems from sound physical observations. The ten "Magmatic First Principles" I enunciated are based as soundly as possible either on first-order observations, experiments, or simple and rigorous inferences. Moreover, they are quantitatively testable and for the most part have been already thoroughly tested. If our science is to proceed, it is of the utmost importance to appreciate some of the observations on which these principles are based and to realize their deep fundamental strength. A demonstration of the pervasiveness of rigorous phase equilibria in the aftermath product is a necessary, but certainly not a sufficient condition of proof of process. Full-blown, rigorous calculations must also be made of the solidification process, yielding cooling time, crystal size, texture, and local composition as a function of space and time. In most of our studies, like Sudbury (Zieg and Marsh 2005), Ile au Haut (Patwardhan and Marsh 2011), and Beacon Sill (Zieg and Marsh 2012), for example, this has been done in considerable detail and the rock record has been tested against our hypotheses, which include phase equilibria, and the "Magmatic First Principles." But almost without exception, this has not been done in the works of Latypov et al.; the entire proof of process is circumstantial and untested.

Crystals, mush columns, and I, S, and ± D profiles

A substantial underpinning of the understanding of integrated magmatic systems comes from the mere existence of large crystals in magma and in their spatial variations. Spatial variations in rock composition, texture, and crystal size are the key to correctly understanding magmatic processes. These spatial variations are intimately linked to the physical processes of transport, emplacement, and solidification. The evidence for the existence of large crystals, for example, be they phenocrysts, xenocrysts, or primocrysts, in magma delivered to Earth's surface is *absolutely unequivocal*. These crystals are observed firsthand in eruptions at many localities, especially at Kilauea Volcano, Hawaii. The Kilauea Iki eruption of 1959, for example, was easily accessible, and rapidly quenched samples of spatter from the eruption fountain were frequently made. The olivine in these samples is often 1–2 mm in size and covers a wide range of composition from Fo_{75} to Fo_{90}. Careful, prolonged, and detailed observations over many years by Powers (1955), Murata and Richter (1966), Wright and Fiske (1971), Evans and Moore (1967, 1968), Wright (1971), Wright and Okamura (1977), and many others have established this fact beyond any doubt. There is no question whatsoever of the veracity of these facts. In particular, Murata and Richter have gone to some length to marshal these observations and demonstrate that the size and abundance of these "Tramp" olivines is proportional to the eruptive flux with the overall load being similar to the sediment bed load of a river in flood stage. Slurries and granular flows are an enormously well-researched phenomenon and are entirely reasonable processes given the physical process of volcanism. Lavas the world over offer abundant evidence of this process as also do dikes and sills, not only in the plain existence of the large crystals themselves, but in their transport, sorting, and deposition. Investigations of physical processes of solid particle transport and sorting are vast (papers in the 1,000s). We ourselves have performed many tens of experiments demonstrating this particle sorting process in solidifying fluids, verifying the original hypothesis of T. Simkin in his synthesis of olivine-rich Scottish sills. There is here, also, absolutely no question whatsoever of the importance of crystal entrainment and sorting in magma transport. And once crystal-laden, slurry magma is emplaced, these crystals are deposited, continue chemically reacting with the host magma, and form distinctive compositional deposits, reflecting gravitative fractional crystallization differentiation. The resulting deposits, which have been observed forming small, layered intrusions in real time in Hawaiian lava lakes, take the form of I, S, and ± D vertical chemical spatial profiles, depending on the initial concentration of crystals and the rate of emplacement. As quoted in the fronts-piece, even Wager and Brown (1967) clearly recognized the fundamental importance of this process.

Latypov (2003a, b, 2009; these papers are the root sources of the present Commentary) has gone to great length to ignore these observations by invoking models of in situ crystallization in a magmatic sheet obeying *ad hoc* physical processes and the rules of phase equilibria and crystal settling. It is critically important to examine and address the unsound logic and faulty basic science of these refutations, as it typifies his many accusations concerning my past and present work. To be clear, there is no argument whatsoever here with the veracity of fractional crystallization; the conflict resides in the overall unreasonable and unrealistic employment of the associated conjectured physical processes.

Soret differentiation and thermal convection

Beginning with a crystal-free magmatic sheet, if my Magmatic First Principles are used, crystallization occurs only in the upper and lower solidification fronts. No crystals exist in the center, hottest part of the sheet to settle and fractionate, as any tiny crystals falling from the upper front will be thermally resorbed by the interior magma, which, by definition, is initially at the liquidus temperature. So, nothing happens unless the upper front becomes unstable

and collapses, which is uncommon, and thus the sheet solidifies to a compositionally uniform body. I hasten to add, however, that it is highly unlikely that any batch of crystal-free magma of significant volume could ever be of a truly uniform initial chemical composition.

To sidestep these difficulties and achieve the desired end result, Latypov must invoke *ad hoc* processes, and thus ignore established physics. There are two principal processes invoked, namely vigorous internal convection and highly efficient Soret diffusion. Soret diffusion is the process of chemical fractionation of a liquid due to differential diffusion of chemical species in a strong temperature gradient. Some species diffuse faster than others and, given enough time, the melt can be chemically fractionated, which can then be redistributed by pervasive convection.

Before simply invoking convection, Latypov (2003a, p1604) briefly reviews the results on thermal convection, and sums up with: "Hot debates between Cambridge [wanting vigorous convection] … and Johns Hopkins researchers [wanting little or no convection] … have not clarified the situation pertaining to convection in magma chambers. The key reason for this is that ideas about thermal convection in natural magmas are based mainly on calculations, not any direct observations of magma itself (Marsh 1990). It seems, therefore, that the potential to resolve this conflict of views with the help of numerical modeling and experiments on paraffin and water–isopropanol that are far from the composition of natural magmas may have been already exhausted. In such a situation it appears reasonable to return to the original information recorded in the rocks themselves. Let us reexamine whether the observational data are consistent with theoretical predictions of models involving vigorous or weak convection."

Regardless of the detailed nature of the debate, between these two research groups several hundred carefully planned and executed actual laboratory experiments were performed and analyzed along with detailed numerical analyses aimed at fully understanding and extending the experiments (e.g., Hort et al. 1999; and references therein). The numerical codes were benchmarked against one another and a huge amount of new science was revealed on thermal convection in systems undergoing solidification; and a large amount of common ground was revealed. Moreover, the results were found to be compatible with similar experiments in other branches of physics and engineering. Yet no attempt whatsoever was made by Latypov to appreciate any of this extensive work, and, instead, all these results were summarily brushed aside with the surprising phrase: "In such a situation it appears reasonable to return to the original information recorded in the rocks themselves." It is actually that very information in the rocks themselves that these experiments were designed to understand. These results are, admittedly, highly technical and demand a certain level of scientific sophistication to master them, but it is all there for the understanding and is highly worthwhile.

And if these results are deemed incomprehensible or simply not believable, then there is, indeed, direct evidence from active magmatic systems themselves. In numerically modeling the thermal history of Alae lava lake, for example, Peck et al. (1977) found the cooling history to be fully accounted for purely by conductive heat transfer, with no sign of any influence of thermal convection. And in my study (Marsh 1989) of the general theoretical, experimental, and observational evidence for thermal convection, I found this to be similarly true for the much larger Makaopuhi lava lake. Contrary to "…not any direct observations of magma itself…" there are considerable direct observations at hand, and in the paper presently in question I further give even more detailed observations. This is not a close call, for thermal convection causes bodies to cool and solidify rapidly. With vigorous thermal convection, Kilauea Iki would be solid in <10 years, when in fact it is still possibly not completely solid after fifty-five years. Thin lava flows, where thermal convection is completely out of the question, solidify at the same rate as thick lava lakes (Marsh 2013).

The second basic aspect of Latypov's (2003b; p.1619) refutation comes from chemical differentiation by the application of Soret diffusion. That is: "An attempt is made to develop an in situ crystallization model based on the concept of Soret fractionation to explain the origin of commonly observed S-, D-, and I-shaped compositional profiles in sills formed from a single pulse of phenocryst-poor parental magma." Having stated this, nowhere is a detailed model of the physicochemical process itself ever defined in its proper spatial context with well-defined coordinates, initial conditions, and any accompanying governing equations of heat and mass transfer. And thus no actual comprehensive physical calculations are made.

The Soret process operates by differential diffusion within a layer of melt across which a large temperature gradient exists to spur diffusion. There are two possible locations of spatial variations in melt composition. One is in the thin chemical boundary layers existing around growing crystals, the thickness of which, and any possible temperature gradient due to latent heat, is set by the crystal growth rate, which in turn is governed by the local rate of cooling. The second location is within the overall solidification front or sill itself with the temperature gradient coming from the proximity to the cold wall rock. Regardless of the exact position, all aspects of crystallization are governed, directly, or indirectly, by the rate of cooling and solidification. Fresh crystallization takes place in solidification fronts, which are dynamic entities, propagating inward in response to cooling; any process, such

as Soret diffusion, must compete with the rate of local solidification. The relative rate of solidification and Soret diffusion is measured by the dimensionless Lewis number (Le = K/D), the ratio of thermal diffusivity (K) to chemical diffusivity (D), which is typically $10^{-2}/10^{-6}$ (cgs), or Le = ~10^4. Solidification is ten thousand (or more) times faster than chemical diffusion; in the face of rapidly advancing crystallization there is no chance at all for Soret diffusion to be effective. And similarly for crystal growth: It is easy to show that the solidification front grows locally at a velocity given by V_s ~ (K/2)/L, where L is the local thickness of the solidification front measured from the initial contact. The front growth rate decreases as L^{-1}, and this rate can be compared with crystal growth rates of, for example, plagioclase, which is often found in intrusives to be on the order of V_c ~ 10^{-9} cm/s. The question can be asked: How thick will the front have to be to approach a crystal growth rate? Forming the ratio V_s/V_c and solving for L = (K/2)/V_c ~ 50 km. There is not ample time, nor sufficient temperature gradient, for the Soret process to operate effectively.

Another measure of the effectiveness of this process is to estimate the rate of flow of the interstitial melt necessary to dominate any Soret effect. This estimate can be made by forming the ratio of advective to diffusive chemical transfer in the full diffusion equation, which is measured by a chemical Peclet number, VL/D. For D as above and L ~ 10 m, any velocity greater than a few millimeters per year will overwhelm Soret diffusion.

This is no surprise to anyone who has spent much time looking at chilled margins in the field. The ineffectiveness of crystal growth and/or chemical diffusion is reflected in tiny, hard-quenched crystals slowly giving away with distance to increasing crystal sizes that can be exactly matched with rational models of cooling and crystal growth (e.g., Zieg and Marsh 2002, 2012); regardless of how the Soret process is argued to be effective or at all competitive, it fails; just exactly as Bowen originally pointed out.

The quantitative fundamental underpinnings on which Latypov (2003a, b, 2009) are founded are untenable.

The three postulates

For the sake of argument, Latypov (2009) ascribes three so-called postulates to me and continues with them here: (1) "…intrusions are not differentiated because they lack residual granitic rocks." No, I say that large magmatic systems, like Kilauea and the ocean ridges, show little to no overall tendency to produce viable granitic differentiates, and this, of course, relates without saying only to tholeiitic-type systems that have the potential to do so. The origin of the strongly bimodal rhyolitic suites exhibited by, for example, Iceland's Torfajokull and Thingmuli volcanic centers are explained in chemical and physical detail by Gunnarsson et al. (1998). (2) "The ubiquity of homogeneous intrusions is due to crystallization from phenocryst-free basaltic magmas….." Yes; basically true, except for second-order internal processes such as, among others, solidification front gravitational instability (Marsh 2002), and basal compaction (Zieg and Marsh 2012). (3) "Layered intrusions with pronounced phase and cryptic layering are attributed to the repeated emplacement of batches of magma with phenocrysts in different combinations and with varied chemical compositions. These phenocryst-laden magmas are supposedly derived from an underlying magmatic mush column." Yes, mainly true, except for the impression that the inputs may perhaps be so "varied" as to be chemically chaotic. To be specific: Each magmatic system has a marked and unavoidable chemical pedigree, characterizing in a broad way all its final products; given any reasonably related mass of crystals and melt and enough time, equilibrium will prevail making the assemblage appear delicately prescribed.

The notable specifics mentioned to disprove these postulates involve, in several places, evidence in the Sudbury impact melt sheet, which will be discussed separately below, but it is important to single out the suggested "convincing observations" for disproof of postulate 2, namely: "The return of this liquid to the residual magma *must invariably have led to* differentiation, especially when the magma in the chamber *was being stirred by thermal, compositional or other forms of convection* [italics added]." The stringing together of normal fractional crystallization with adjoining unsubstantiated physical processes makes this overall assertion groundless; this style of analysis, as already mentioned with regard to thermal convection, commonly characterize each of the adversarial arguments.

Apparently having doubts of the viability of thermal convection, Latypov (2003b) similarly invoked, *ad hoc*, compositional convection to provide the stirring necessary to remix residual melt escaping from basal cumulates. Although compositional convection of the style needed is observed in some experiments, it is mainly in aqueous systems where the crystals are unusually large, fast growing, and produce large buoyant chemical boundary layers. But in systems where crystal size and growth more closely matches silicate systems, like high molecular weight paraffins, this does not occur. Upward migrating residual melt is clearly observed in vertical pipes in the basal cumulate pile of Shonkin Sag Laccolith (Marsh et al. 1991), but these formed in response to the sudden deposition of a load of pyroxene phenocrysts entrained in the initial emplacement magma, thereby expelling the interstitial melt during sedimentation. And this melt did not mix with the overlying melt but, instead, formed its own distinctive layer, which

as I mention can also be taken for evidence of lack of any convection, thermal, or other (Marsh 2013).

On their refutation of postulate 3 appears a fundamental fallacy pervading this entire critique, namely that if extensive differentiation is precluded in initially crystal-free magmas, it must also be true everywhere in the underlying magmatic mush column and is "…both unfounded and excluded by the hypothesis itself." What is misunderstood here is the critical fundamental link between magma ascent dynamics and magma composition (Marsh 1996, 2007a, b, 2013). That is, slowly ascending magma is unable to sustain entrainment of crystals and arrives, indeed, essentially free of larger crystals, but with increasing rate of ascension, crystal entrainment is increasingly effective, just as observed by Murata and Richter (1966), and the 500,000-year Hawaiian lava record shows clearly how this dynamic feature can vary in time. So, the bulk chemical nature of the emplaced magma depends critically on its ascent dynamics; see Fig. 8 and associated text in Marsh (2013). The ensuing body made from such deliveries can be, in the end, as varied, in terms of layering, cryptic and otherwise, as commonly observed, which emphasizes the important concept of Initial Conditions (Principle 1). And this goes also for primocryst-free deliveries, each of which may have experienced during ascension differing degrees of chemical interaction with solids and melts in the mush column conduits.

The common assumption of a precise knowledge of initial conditions of formation of any magmatic body may seem harmless, but it is fundamental to the true evolutionary understanding of the body. Initial melt composition can be highly varied depending on the previous history of the magma at depth. The durations of rates of transport govern not only the entrained crystal content, but also how long the melt had to equilibrate with various crystal assemblages. So, a series of crystal-free emplacements, while being broadly similar in bulk composition, may differ significantly in chemical detail. The emplaced magma, then, is the collection of a time series of physical processes buttressed always by a relentless drive toward chemical equilibrium. The fact that small bodies like Shonkin Sag (70 m) and Basistoppen (650 m) show excellent cumulates and equilibrium assemblages reflects exactly this process. The building materials have bathed intimately together protractedly prior to emplacement, which is not similarly possible for a magma at rest.

In sum, I say here and previously that even an apparently haphazard train of deliveries of crystal-laden slurries and crystal-poor magmas will, unavoidably, give rise to good, clean equilibrium assemblages if given enough time. And, yes, the interstitial melt, once the magma is no longer active in reaching 50 vol. % crystals, may play an important role in the final texture and local composition.

The other thirteen items

The string of thirteen items following the postulates essentially emphasizes what has already been said, but some specific comments are warranted.

Skaergaard initial conditions

I clearly do not attribute the initial conditions of "…instantaneous emplacement of crystal-free magma for the origin of Skaergaard, Stillwater, and Bushveld to Wager and Brown (1967)." Although it is abundantly clear that this was their assumption for Skaergaard and it caught on and was adopted by many other workers, even to this day. Here's what they said: "The uniformity of the chilled margin and its continuity round the intrusion together give grounds for believing that the intrusion was produced by a single, grand episode in which basic magma, estimated at 500 km^3 in volume, was emplaced rapidly in the upper crust. Wager and Brown (1967, p. 20)" and McBirney (1993, p.196) reads this as: "The magma seems to have been emplaced in a single surge…." To deconstruct exactly what time interval is assumed is immaterial. What is clear geologically is that the body was not assumed to have been emplaced serially over hundreds of years. And, in their defense, at the time it seemed like a harmless assumption.

Mineral-melt reactions

Yes. The drive to equilibrium, as already discussed, is relentless regardless of the mineral assemblage present, and Bowen's reaction series tells in what direction equilibrium broadly proceeds. When magma is in motion, containing an entrained assemblage, this process (i.e., reactions among crystals and melt) operates, and when the magma tarries or stalls, the crystal assemblage is gravitationally shed; this is fractional crystallization, which defines punctuated differentiation. Even though the overall mineral assemblage may be highly varied or heterogeneous, in large volume, well-used systems due to the overall recurrent nature of the physical process itself, much of the materials involved will commonly be of a related pedigree.

An extreme example of this process whereby an initially heterogeneous suite of minerals comes to a common equilibrium can be found in metamorphic rocks. That is, consider a clastic sediment made through erosion, transport, and deposition. The resulting assemblage of minerals and lithics has been largely mechanically made and assembled, and at sufficiently high pressure and temperature the assemblage tends toward thermodynamic equilibrium. In the end, having started in perfect chemical disequilibrium, the assemblage may show prefect equilibrium; the resulting crystals are clean and unzoned, the texture shows prefect

geometric equilibrium, and even the isotopic record may be homogeneous. Given the final product, it is impossible to decipher the exact initial state of this rock. This, too, is commonly an overriding process in the history of igneous rocks.

Sizes of crystals

Yes. Small bodies with large crystals are highly suspicious. The question is: Do the crystals fit the body? The unusually large crystal sizes in Skaergaard and in many other bodies, like the Basement Sill, are clear indicators of these crystals having been carried in as slurries. And the assertion that: "Marsh (2013) proposes a simple model of kinetic crystal growth that does not take into account the role of equilibration (coarsening) that is so important in the postcumulus textural development of most plutonic rocks (Higgins 2011)." This is true in my 2013 paper because I was presenting a generalized crystallization formalism for intrusions. But, I hasten to add: the linear growth law stated there is perfectly capable of describing an array of growth styles, including coarsening. From the very beginning of my introduction of CSD methodology (Marsh 1988), I have emphasized a variety of models of crystal growth, and I have discussed at length the role of coarsening starting at the earliest stages of crystallization (Marsh 1998, p. 574–579, see Fig. 14; Hersum and Marsh 2007) and these insights have been largely confirmed experimentally (e.g., Schiavi et al. 2009). Nevertheless, the introduction of more complex growth laws is commonly not warranted or justifiable.

In a curious turn of logic is the phrase (Latypov et al.): "In addition, *the data show* that crystals that grow in sills during the middle and late stages of magma differentiation can attain lengths of 10–15 mm. [italics added]" There is no actual data that show this. In his review of crystal sizes in sills, Latypov (2003b) simply asserts that these crystals grew in situ, and no calculations whatsoever were presented linking crystal growth to a full thermal model of cooling and solidification. Mention is made of the exceedingly long (>1 m) tabular harrisitic crystals at Rum, which are, to me, due to local end-to-end coarsening or annealing within a dense pile of slurry-delivered olivine. And the huge size of oikocrysts is clearly a result of crystallization of the interstitial melt at late stages (>55 vol. % crystals) of solidification.

Closure in crystal size distributions

The fundamental relationship between CSD slope and intercept discovered by Zieg and Marsh (2002) is as important to CSD methodology as conservation of mass, momentum, and energy is to general physics (Marsh 2007a). In the

right hands, when coupled with a model of heat transfer it allows the entire CSD to be predicted spatially throughout any body. Moreover, with knowledge of CSD fanning in evolving solidification fronts, the CSDs recorded in a stack of lavas allows the spatial source locations of the lavas to be identified within the underlying mush column or magma chamber.

Kilauea Iki lava lake

The history of filling from eyewitness accounts and its subsequent history of cooling, solidification, and olivine settling as recorded in direct drilling is clear and is as I have described (see references in Marsh 2013).

Evidence for internal fractional crystallization

Kilauea Iki lava lake provides an excellent example of internal fractional crystallization and all that goes with it. Large olivine (~Fo_{80-90}) crystals delivered in a slurry settled to form a basal cumulate that continued to react with the attending interstitial melt, homogenizing the assemblage, and redistributing both major and minor elements (see references in Marsh 2013). Trains of slurries arriving in magma chambers do not wait for the previous input to solidify before the next arrival, but as in volcanic eruptions the cadence may be periodic or haphazard, perhaps even sometimes driven by fortnightly tides. Once emplaced the ongoing resultant processes are in some regards, in the end, indistinguishable from assumptions of pure in situ crystallization. There is a pervasive and mistaken belief among Latypov et al. that somehow the concept of slurry deliveries to build a magmatic body necessitates "…conjectural processes that took place in an underlying (and possibly even more) voluminous chamber." Not at all; all the same processes proven to take place after emplacement in sills, dikes, lavas, and plutons also take place in the mush column prior to emplacement. Yet, these authors state definitively in describing the Cr budget in magnetite in the Sept Ile layered intrusion that there were repeated emplacements of "…primitive (high-Cr) crystal-free magma. During injection, each batch of new magma thoroughly mixed with the resident magma, explaining the gradual increase in Cr in magnetite at the base of each cycle. The subsequent decrease in Cr content is evidence for fractional crystallization within the chamber." This is pure conjecture based on a single line of reasoning to attain the necessary end result. Although such process may occasionally operate, because of the need for repeated highly specialized dynamics, it cannot possibly be a general process at any single location. And this conjecture is certainly no definitive proof of process. Magmatic processes are commonly much more messy and chaotic.

Evidence for circulation of interstitial melt in cumulates

It is stated by these authors: "…the way an interstitial melt behaves in a mush depends on the physical properties (mostly density and viscosity) of the crystal matrix." And Marsh (2013; p. 686); see also, Marsh 2002) says: "And the wholesale flow of the remaining interstitial melt is, in essence, a catalyst to these processes, chemically connecting vast areas of the body never before connected. The final chemical outcome of this extensive, long-lived communication is that of homogenization, bringing closer to equilibrium what was initially a physically juxtaposed collection of solids and melt." Latypov et al. confuse the behavior of static melts in growing solidification fronts with possible widespread interstitial flow once the body has reached critical crystallinity (i.e., >55 vol. %). How an interstitial melt behaves is certainly a function of the physical properties of the porous medium and the melt itself, especially its density. That is, if the system in question evolves toward iron enrichment, which likely happened at Skaergaard, interstitial melt may drain from the upper solidification front as long as the front growth is slow enough to allow it. This melt, per conservation of mass, must then be replaced by melt drawn in from the leading edge of the front, and this may lead to significant chemical and heat transfer, perhaps even slightly burning back the leading edge of the front and retarding crystal growth. Each situation must be modeled in detail to understand the outcome. In the floor solidification front similar process may occur with additional enhancement from compaction.

Evidence for silicate liquid immiscibility

Whatever phase equilibria prevail in any interstitial melt does so regardless of the mode of delivery of the adjoining crystals. This is no test, whatsoever, for the existence or nonexistence of slurries.

Evidence from lateral variations

It is stated: "…emplacement of crystal slurries is not compatible with those layered intrusions that show striking lateral compositional variations." This exactly describes a central feature of flow-differentiated slurries, the Simkin Sequence, which I have described in some detail in theory, experiment, and field observation (e.g., Marsh 2013, 2007a, 2006, 2004 1996). Bowen (1928, especially p. 146–150) himself perceptively describes this sequence in his chapter on "Rocks Whose Composition is Determined by Crystal Sorting." The assertion is also advanced that: "The high viscosity of basic magma argues against slurries spreading over huge areas to deposit layers of even thickness." Basic magmas, by which I assume Latypov et al. mean basaltic,

are some of the lowest viscosity magmas we have. And as for understanding the dynamics of slurries, although I give plenty of references in Marsh (2013), there exists a readily accessible vast literature.

Evidence of episodic replenishment of magma chambers

This whole section describes almost perfectly the action of periodic slurry and/or crystal-free magma emplacement and subsequent equilibration process as already described. As for the worry that this requires "….an unrealistic fortuitous sequence of appropriate crystal slurries.": No, not at all. For in most of these situations, like the olivine cycles at Muskox, once the pedigree pattern of delivery is established, it is simply a reflection of the periodicity of the process; effectively, simply starting and stopping of the Simkin Sequence of transport and emplacement.

Evidence of closed-system fractionation

In essence, similar to above but with different examples, summing up with: "A very similar pattern of repeated ultramafic cyclic units overlain by a thick and continuous sequence of gabbroic cumulates *resulting from closed-system fractional crystallization*…[italics added]" A conjecture, based surely on an abundance of solid chemical and textural information, but with a conventional physical process in mind. The same end result can also come from slurry trains.

Isotopic evidence for internal processes

The core of this evidence is summed up in: "…these antipathetic relationships [between $^{87}Sr/^{86}Sr$ and An %] *are interpreted* as being a consequence of coupled assimilation and fractional crystallization due to a thermal link between latent heat of crystallization and heat consumed in heating and melting country rock. [italics added]" Once chambered and confined within a chilled margin, it is exceedingly difficult for any passively cooling magma to repeatedly melt its own wall rock (e.g., Marsh 1989) and extract the melt inward, and it is also likely physically impossible to establish a direct link between latent heat output and wall rock melting. Latent heat is just what the term "latent" implies: hidden or concealed heat appearing only in response to cooling, and not at all appearing as bursts of heat, especially in large, slow-cooling systems. Latent heat is physically exactly like increasing the effective melt heat capacity (e.g., Jaeger 1964). Moreover, any process of wall rock interaction and contamination is far easier to effect, physically, thermally, and chemically during ascent in the underlying delivery system, which for a 1.6-km-thick body must have been extensive with many periodic deliveries.

Evidence from experimental phase equilibria

Similar to above. Yes, phase equilibria, as long ascertained, always, in the end, holds true. And thus given any co-genetic mass of crystals and melt, even with foreign crystals, phase equilibria will eventually prevail. The larger Hawaiian lava lakes are excellent examples of this, but to infer an extensive history of physical process from this end equilibrium result is not reliable.

Sudbury

Our analysis of this impact melt sheet is comprehensive in every detail (Zieg and Marsh 2005): From the determination of the degree of superheat, to the recognition of the high-temperature breccia from cratering being a viscous emulsion whose dynamics of separation produces both large-scale differentiation into norite and granophyre and small-scale isotopic homogeneity, to the role of thermal convection during the superheat phase and associated ore deposition, to the understanding of the dense central quartz gabbro transition zone, to deciphering the textures in light of these dynamics, and finally to the overall cooling and solidification time. The crystal sizes calculated independently from this cooling model match perfectly in every detail the observed CSDs of the 1-km-thick basal norite (Zieg and Marsh 2002).

In the face of this, Latypov et al. suggest instead that "… fractional crystallization of a single [apparently homogeneous] sheet of andesitic magma…..reproduces all the geochemical features of the complex…" That is, 1 km of basal norite and 2 km of granite, separated by dense quartz gabbro are each produced by simple cooling and fractionation of a homogeneous andesitic magma. Extraordinary. For this model to operate, this impact melt sheet, which formed in 5 min with materials from the whole crust, had to be homogenized over this same time span, which is a physical impossibility. Moreover, fractionation had to occur inward of the solidification fronts, invoking *ad hoc* vigorous thermal convection, which cools the body too quickly and produces the wrong crystal sizes. And all this occurred leaving no final record of the massive (~35,000 km^3) initial andestic magma.

I firmly stand by our results, and am confident that any additional, apparently irreconcilable chemical information can, when properly analyzed, be reconciled with our basic model.

Conclusion

We are in the midst of a major magmatic paradigm shift. The magma chambers beneath ocean ridges were once thought to function like that described by Latypov et al.,

vast vats of melt, kilometers thick, undergoing in situ fractional crystallization leading to thick cumulate sequences. But direct seismic evidence found the magma chambers to be modest sills capping extensive mush columns, periodically replenished in response to plate spreading (Sinton and Detrick 1992). The random aliquots of magma extracted as sheeted dike complexes the world over offer overwhelming evidence that the magma inward of the solidification fronts is normally essentially crystal free and the layered cumulates found in ophiolites gives direct evidence of the concomitant delivery of slurries from the underlying mush column (e.g., Dilek and Furnes 2014). Extensive piles of lava the world over similarly provide abundant evidence of what goes into building magma chambers: magmatic trains of slurries interspersed with near primocryst-free melts. From the largest scale to the smallest scale of the crystals themselves this process is readily apparent (e.g., Davidson et al. 2007). Magmas consist of a "carrier melt" containing, in response to the dynamics of ascent and emplacement, nearly any combination of indigenous and exotic crystals.

The ten Magmatic First Principles I have set forth have been tested time and again and should be continued to be tested and improved. If adhered to, these principles can be used to solve actual petrologic problems. I stand firmly with them and with the overview that physical processes buttressed by chemistry govern magmatic processes. The concepts of slurries being central to the origin of layered intrusions and the null hypothesis, of little differentiation coming from non-primocryst-bearing magma, remain intact.

Although magma is perhaps the most challenging material known to mankind, enough is now known of magma physics and chemistry to make quantitative models relating, for example, cooling, solidification, and crystal size in space and time. I have attempted to do this, beginning with the simpler of natural systems and working up to those much more complicated. The results stand for themselves, but a certain amount of technical sophistication and scholarship is necessary to appreciate, employ, and enjoy them, and simply to rest a negative argument for physical process on the final results of phase equilibria is insufficient. If carefully justified comprehensive calculations are presented, there is little need for words; if calculations cannot be made or understood, words will not help.

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