Discussion of

Interaction between local magma ocean evolution and mantle dynamics on Mars

by

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27th December, 2006, Joe McCall

This is a most interesting article which requires a very deep knowledge of mathematics and physics (which I do not have) to fully comprehend. It illustrates the two totally contrasting methods of working, both acceptable. As a terrestrial geologist I have always worked from the observed surface geological features, extending what I have observed to conjecture about the deeper origins. There is a contrasting methodology which involves modeling processes at mantle and even core-boundary levels, using seismology, geochemistry and evidence from kimberlites. This is epitomised by "plumologists" such as my former student, Ian Campbell at ANU, Canberra.

The work described by Reese et al. (this volume), comparable with the second approach but applied to Mars, really in no way impinges on my simple comparison between Olympus Mons and terrestrial caldera volcanoes (McCall, 2006a). Olympus Mons shows all the features of a shield volcano with a summit caldera, and a recent discussion with Prof. Lionel Wilson suggests to me that, like many terrestrial shield volcanoes, the huge dome was erected by fissure eruptions; there may have been no summit crater before caldera formation. An important point I made is that there is an active analog on Io. Whether impact was involved in the early mantle-level processes which ultimately gave rise to this activity must remain a matter of conjecture, and indeed, we may never know for certain. At this level of modeling, it seems possible that diverse models will be advanced, all credible. However, the fact that a model is credible does not mean that this is what actually happened.

4th January, 2007, Lionel Wilson

Reese et al. (this volume) mention as issues requiring future work the temporal stability of plumes. There is some evidence on time scales stemming from the observation by my colleague Eve Scott that the presence of cross-cutting but interlocking calderas on the summits of most martian shield volcanoes implies that, in general, any shallow magma reservoir involved in such a caldera-forming event must have a finite life, and must then cease to receive any significant magma supply from the mantle for long enough (which we calculate to mean ~ tens of Ma) for it to cool below the solidus. Only in this way is a new magma reservoir likely to form partly (or completely) offset from the previous one. But if a new shallow magma reservoir is to start growing, each newly-emplaced near-surface early intrusion must be fed by extra magma faster than it cools. The details depend on the size and geometry of the first intrusion but there must be an initial magma flux pulse estimated to be ~100 to 200 m^3 s^{-1} for at least a few weeks. This is followed by a period during which the minimum required flux decreases with time as the initial intrusion swells and becomes more resistant to cooling (because its cooling outer boundary layer only thickens in proportion to the square root of time).

There are uncertainties in timescales due to a range of options in how the geometry of the initial intrusion will develop, but simply to maintain fully-formed reservoirs with the sizes implied by the visible martian calderas requires magma fluxes of ~1 to 10 m^3 s^{-1} (these values correspond to 0.03 to 0.3 km^3 a^{-1}, similar to their mean flux for the last 2 Ga in model C). Overall the implications of our analysis were that, for each volcano, there has to be (a) a way of periodically generating an initial large pulse of magma, and (b) a way of turning off the magma supply for long periods. To satisfy all the constraints, including mean volcano growth rates, averaged over the entire volcano lifetime, estimated to be within a factor of 2 to 3 of ~0.05 m^3 s^{-1}, the simplest solution is for there to be a potentially active phase lasting for ~1 Ma alternating with a quiet phase lasting for ~100 Ma, the Tharsis volcanoes each having experienced 10-20 such cycles.

Considerable variation around these values is possible, but not by orders of magnitude. An explanation for this long-term episodicity is needed. I can imagine that if such a time scale were established, other consequences would follow: perhaps the large initial magma pulse of a new active phase might be related to there being a threshold for the first extraction of melt from new partial melt zones.

6th January, 2007 James H. Natland

The plume hypothesis is often justified by manifestations of large-scale volcanism. The usual refrain for Earth is, “What about Hawaii?” For Mars, it is, “What about Olympus Mons?” The Hawaiian Ridge culminates most actively at two large shield volcanoes, Mauna Loa and Kilauea. Olympus Mons is one of the several Tharsis shield volcanoes of Mars and is by far the largest shield volcano in the solar system. The rationale for plumes at these places is that both Hawaii and Olympus Mons are huge and seemingly require some type of conveyor belt of mantle material to explain them. The conveyor belt has to be hot and its flow rapid in order to explain eruption of great thicknesses of basalt in short periods of time. This in turn is presumed to require a hot source, arguably a thermal boundary layer at great depth, to trigger plumes. However, consider another type of conveyor belt.

Hamilton (2002) and Natland and Winterer (2005) emphasized that plates considered in their entirety influence asthenospheric convection, particularly counterflow away from subduction zones. If the top of the asthenosphere is generally above its solidus (Presnall and Gudfinsson, 2005), buoyant partial melts will tend to collect in general beneath the entire base of the lithosphere. Evidently even very strange, low-degree partial melts, with compositions of potassic olivine nephelinite, are still aggregating today beneath the oldest and coldest portions of the Pacific plate now entering the Japan Trench. These erupt at small volcanoes where the plate begins its flexure (Hirano et al., 2006). Thus, picture the Pacific plate slipping to its destiny on a film of lubricious partial melt where the effective stress is reduced to nil; this is essentially overthrust faulting on an enormous scale. The slippery layer will not be even because the melt fraction and quantity should be greater over fertile spots (patches, schlieren, or blobs of eclogite, etc.), over somewhat warmer spots, and where changes in plate thickness are fairly abrupt (i.e., adjacent to fracture zones). I contend that this obviates the need for deep plume conduits.

Given trench geometry in the western Pacific, the strongest counterflow working its way back toward the East Pacific Rise should lie approximately beneath the Hawaiian Ridge and parallel to it. This is necessarily coincident with the most favorable location of propagation of a tensional fracture based on thermoelastic cooling of the Pacific Plate (see Natland and Winterer comment on Stuart et al., this volume). The "bow" on the thermoelastic stress field of the cooling plate of Stuart et al. (this volume), produces a line of weakness that coincides precisely with an upward bow in the pattern of asthenospheric counterflow. The conveyor belt is not a plume but is turned on its side in such a way that lots of melt aggregates continually from a large volume of mantle to supply Hawaii. No other volcanic chains are active in the vicinity to dissipate or weaken this flow. We may even surmise that the melt layer is squeezed or overpressured by the action of the plate sliding above it, like the fluids that lubricate thrust faults and decollements at accretionary prisms, and that this contributes to the volume and elevation of Mauna Loa. Of course this is supremely speculative, but then, I think we should admit, so are plumes.

Reese et al. (this volume) argue that formation of Olympus Mons and the other large Tharsis volcanoes early in the history of the planet is incompatible with an origin by means of mantle plumes. They advocate instead that large-scale impact melting produced local magma oceans, from which these volcanoes derive. They develop an evolutionary model to show how long-term thermal effects from a deep thermal boundary layer at the Martian core-mantle transition may have permitted types of follow-up plumes to develop and persist, allowing volcanism to cover up crater fields for long times thereafter.

But was an impact-produced local magma ocean even necessary? Suppose that the Martian surface once was effectively a single lithospheric plate capped with a thick primordial crust that, perhaps because of the relatively small size and fairly rapid cooling of the surface of the planet, was never dynamically unstable. The crust never foundered back into the planetary interior, like the presumed proto-crust of Earth, and it never split up into separate plates. But when the whole planet was hotter, that one large surface plate must at some time have overlain a mantle that was at or above its solidus temperature regionally all around the planet. These are the same conditions I have postulated above for beneath the Pacific plate: an asthenosphere refulgent with partial melt, concentrated near the top, but in this case global; an asthenosphere weakest and with least viscosity near the top (Zhong and Zuber, 2001); and eruption only occurring where concatenation of stresses on the lithosphere permitted. I see no reason why a widespread layer, perhaps the entire shallow asthenosphere, could not have provided the magma. I cannot say whether forces acting within the lithosphere produced the stress points, or whether they were punctures produced by impacts. But it seems to me that a broad comparison can be drawn between the mechanisms of large-scale "mid-plate" shield volcanism on planets Earth and Mars that is tied to the general direction necessarily taken by cooling of both bodies, but without mantle plumes.

8th January, 2007 Joe McCall

A global view of Mars magnetic stripes from a geological viewpoint: no plate tectonics?

The magnetic stripes recorded by Mars Global Surveyor from ~400 km altitude (Bowler, 2005; Connerney et al., 2005) are a major surprise. It is not the intention here to question the geophysical statement or the detailed relationships with Mars surface features. However, as a geologist, involved for many years with terrestrial mapping in the East African Rift Valleys and a major plate convergence zone in Iran as well as long concern with meteoritcs and planetology, I question the attribution to early Plate Tectonics on Mars.for the pattern revealed

The pattern displayed is of a panglobal striping traversing the planet's surface from east to west. There are swirls, widenings, constrictions to cutting out, and even circular patterns, but one cannot escape the conclusion that this patttern is panglobal and almost certainly a single impress on the planets surface and not a conglomeration of separate spreading patterns imposed at different times comparable with the patterns of magnetic stripes imposed by spreading on the Earth's oceans (of which the latest cycle only is now preserved).

It is important to consider exactly what the terrestrial magnetic stripes are. They are a feature of oceanic crust only. Each successive stripe represents a newer age of formation (spreading), recording a magnetic polar reversal. The oceans are mega-rifts, opening up from a median volcanic ridge. The East African Rift is in effect an aborted ocean, where eruptive distension occurred but because of its unique situation between the passive margined Atlantic and Indian Ocean systems, arose in a situation where compression was acting on both sides of the continent, and it could not spread out to form an ocean (McCall, 2006b). The rifts associated with ocean spreading initially developed on Earth amid supercontinents, breaking them up (e.g. Gondwana).

The problem with equating the Mars magnetic field with terrestrial Plate Tectonics is that we have apparently a panglobal assemblage of stripes affecting only the Martian analog of 'oceanic crust' and later erasures of the stripes where volcanic provinces and inferred impacts have been superimposed. What is lacking is the 'foreland' continental marginal masses. A resolution of this problem bordering on the absurd would be the dictum that 'on Mars continents vanish, on Earth oceans vanish'. However, the pattern has a regularity of average stripe width, and magnetic reversals on Earth vary widely in duration and so width. A relationship to a number of successive openings at different magnetic polarities can thus surely be ruled out? Magnetic stripes on Earth also have a wide variation in trend within the single Jurassic-Present cycle, because of the diverse length of polarity episodes. The pattern on Mars suggests an early pan-planetary crust, with this early crust splitting along linear zones and a new crust forming between the separations in a time of reversed polarity throughout the planet.

What seems evident is that this is not analogous to terrestrial Plate Tectonics, it cannot relate to a system of rifting of continents and spreading provinces – the surface areas (continent analogues) on which the initial spreading occurred are absent, the comparison with terrestrial magnetic stripes is far fetched. Plate Tectonics involves a movement of lithospheric plates geographically, that is across the planetary surface. Where is the evidence of this?

This is, surely, a surprise peculiarity of Mars – sui generic? The final question that comes to mind is "should we look other planets and satellites for evidence of similar stripes?".

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