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Discussion

Reply to Battaglia and Cayol, 2009

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1. Introduction

We thank (Cayol and Battaglia, 2009) (hereafter referred to as CB2009) for commenting on our work (Houlié and Montagner, 2007). We start our response by reviewing our model and then discuss the potential of discrimination of tilt and displacement at a VBB seismometer from seismic records.

There appears to be a misunderstanding of the approach and the interpretation of our results presented in Houlié and Montagner (2007). We do not question the discovery by Battaglia et al. (2000) of transient deformation before volcanic eruptions, as well as the existence of 2 types of signals, but the interpretation is not necessarily as simple as proposed in their paper. We note that the three components in the seismic record of displacement might have different explanations. From a theoretical point of view, any perturbation of displacement can be expressed as follows: $\delta u_i(t) = \sum_j \frac{\partial u_i(t)}{x_j} dx_j$. It means that $\delta u_i(\mathrm{t})$ can be expressed by 9 independent terms. Since $\sigma_{zj} = 0$ at the surface of the Earth, there remain 6 degrees of freedom. Usually, the symmetric part (deformation ε_{ij}) related to the stress tensor (in the elastic case) is separated from the anti-symmetric part that is related to rotation or tilt. Consequently, distinguishing displacement and tilt is a real scientific challenge. Very long period (period $T > 100$ s) transients are interpreted in terms of tilts (Battaglia et al., 2000), but we explore the possibility that a large part of seismic record is related to translational displacement in addition of or rather than tilt (rotation). Following this simple idea, we draw the consequences of this hypothesis and propose a model which differs from previous approaches (Battaglia et al., 2000; Battaglia and Bachèlery, 2003; Battaglia et al., 2005).

There are two extreme models for interpreting the data: the first one is only based on tilt (associated with the dislocation model (Okada, 1992)) and a second one is based on ground displacement generated by a spherical source. CB2009 claim that the whole signal can be explained by tilt, but we claim that a significant part of the signal is related to ground displacement and that actual data cannot yet discriminate between these two models. It is likely that both effects are present, and the next challenge will be to determine where the right loci of the true solution are between these 2 extreme models. The intention of our paper (Houlié and Montagner, 2007) was to draw attention to this fundamental issue, which has important consequences for the understanding of volcano eruption dynamics.

We suggest that the actual data are not sufficient to provide a definitive answer to this issue. Our work shows that an interpretation of the currently available seismic record in terms of translational displacements is perfectly valid and that further work will be needed to discriminate between tilt and displacement models and combinations of the two.

2. Point 1: Clarification on the model proposed by [Houlié and Montagner, 2007]

Our geophysical model, as others, is not unique. Beauducel and Cornet (1999) demonstrated that the same dataset can be modeled by radically different magma reservoir sources. The main interest of our study was to demonstrate that similar signals could be detected coincidently with the start of seismic crisis and this, over decades. This stability of the records does imply the existence of a magma reservoir at Piton de la Fournaise. This hypothesis is supported by the GPS network motion during the 1990s that is consistent with the inflation/deflation of a spherical source located beneath the summit (Houlié,

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2005) and also by the recent collapse of the summit area. This source is the magma reservoir from where the dyke injection starts. The stability of the source opens the opportunity to model the volcanic processes on a long-term basis. We chose not to limit ourselves to monitoring the surface eruptive activity. Indeed, in other volcanoes such a magma source was demonstrated to be responsive in period of eruptive rest (Massonnet et al., 1995; Houlié et al., 2006a). HM2007 proposed to estimate the maximal initial volumes erupted in the magma chamber by modeling such a magma reservoir.

Our model does not intend to track down a dyke injection but focuses on the rupture of the magma chamber itself. The failure of the magma reservoir lasts a few minutes, which provides an upper bound for the volume of magma intruded in the volcano. The dynamics of the magma failure makes it detectable only by a VBB seismometer (not a GPS but maybe a tilt-meter). The volume attributed to this failure relates to the first magma intrusion volume. The signals recorded by the VBB seismometer are thus a powerful indicator of a seismic crisis. Those signals are much more reliable than tilt-meter signals that are not able to provide glitch-free continuous time series of displacement during long period of time (Delorme, 1994).

3. Point 2: Vertical displacement

Since there is no vertical (or very small) signal recorded at the VBB seismometer, CB2009 postulate that the record only represents tilt at the station. They propose on this basis to interpret the recorded signals a pure tilt. We think that may overestimate the tilt component on the displacement by using this assumption. Indeed, the location of the VBB station with respect to the source is very specific. The source is located approximately at the same station altitude. The theory suggests that the vertical displacement is small with respect to the signals recorded in the NS direction. We make the same observation for the EW displacement that is also smaller than NS signals. We suggest the installation of another VBB seismometer co-located with a tilt-meter and likely a GPS receiver recording at high-rate is absolutely necessary to distinguish tilt from displacement.

On a more theoretical aspect, the debate here is to know whether or not the tilt-meters are sensitive to displacement and the VBB sensitive to tilt. Before answering this question we need to know whether or not the frequency domain of both instruments can be compared. Indeed, in most of the studies using Blum silicium tilt-meters, the frequency period specified is close to 8–15 s (Beauducel and Cornet, 1999) and is mainly used to constrain long period transients for studies focused on tidal waves (Saleh, 2003). Whatever its sensitivity, the tilt-meter at Piton de la Fournaise is recording data at one sample per 2 min (at least for events 1986, 1992 and 1998 (Battaglia et al., 2000)). The reader will understand that the comparison of tilt-meter records with VBB records in the frequency domain (T>100 s) specified in the study by Houlié and Montagner (2007) on the horizontal plane (tilt has only two horizontal components) is not trivial.

The second argument proposed is related to the amplitudes of signals and to the associated volumes extruded from the magma chamber. First, we encourage CB2009 to read carefully HM2007 and we apologize that the Fig. 2 could led a reader to think that the GPS receiver symbolized on this figure could be site 1B80 (located about 4000 m away from the source/summit as clearly indicated on Fig. 1 of HM2007). Nevertheless, we emphasize that the motion of site 1B80 plotted in Fig. 4 is in the horizontal plane and we are surprised that CB2009 would have thought that a Mogi source located beneath the summit could have generated horizontal displacement at GPS sites located at the summit. We did some new calculations based on the numbers published in page 5 of HM2007 at site 1B80 (different from results in RER station). Assuming a source located beneath the summit, a shear modulus equal to 5 GPa for various radii (from 150 to 650 m), we never reach the vertical estimates proposed by CB2009 (Fig. 1).

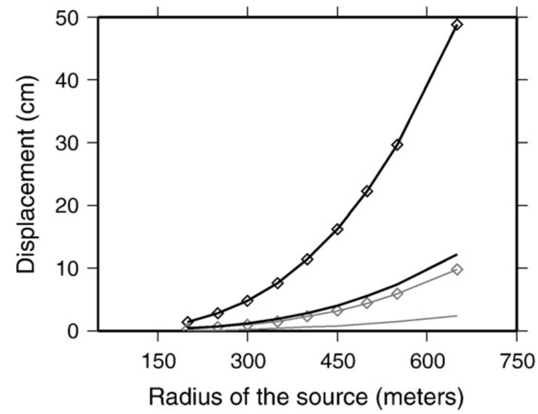

Fig. 1. Vertical (plain curves) and horizontal (Diamonds symbols) displacements predicted at GPS site 1B80 for various source radius (200 to 650 m) and for two values of shear modulus μ. Solutions for $\mu = 2$GPa are plotted in black and $\mu = 10$GPa solutions in gray. The sensitivity of the displacement is very high (>100% for + 150 m change with respect to a 500 m radius chamber.

We plot the vertical and horizontal motions expected at 1B80 for various magma chamber sizes in Fig. 1. The reader will note that maximum vertical displacement never exceeds ~12 cm with 650 m radius magma chamber for $\mu = 2$GPa. We must bear in mind that the Mogi model is a very poor approximation, assuming an elastic medium. It is likely that the medium is not as elastic as expected. We would also expect, (due to the piling of lava flows) that the propagation of displacement is probably different in the horizontal and vertical directions. We add that the sensitivity of the displacement is very high (>100% for + 150 m change with respect to 500 m radius chamber). We reject the argument as saying the vertical component of motions should be larger at 1B80 because the properties of the volcanoes are not constrained enough to be useful in such a debate.

Nevertheless, we recall that the deformation predicted by the GPS is the sum of the deformation related to both the magma chamber volume change and the deformation induced by the dyke propagation toward the surface. As this last phenomenon dominates the deformation related to deeper processes, we could miss deformation directly associated with the magma chamber failure. Geodetic studies should take into account all the magma sources involved during the eruptive crisis.

Additionally, this debate on intruded magma volumes is the base of the volcano investigations since their very beginning. This question is thus not new and the discrepancies among various estimates inferred from various techniques have been observed since a long time. At Kilauea, for deep magma intrusions, the discrepancy between the magma volume inferred from seismic network records and modeled volumes can be as large as 90% in some cases for deep area (Aki, 1984). We personally faced this issue in a previous work by comparing our work based on GPS observations (Houlié et al., 2006a) with estimates based on sulfur degassing dataset (Allard, 1997). Indeed, the volume quantified by using GPS would be 90% smaller than the magma volumes degassed into the system and to the surface. At Mt. Etna, such a discrepancy in the signal has been explained by degassing of magmas during the vertical convection of the magma in the main conduit Allard (1997). A concurrent explanation would reside in the lateral intrusion of magma into pre-fractured areas. Indeed, Houlié et al. (2006b) have shown that silent step-wise injection of magma as lateral dikes along structural discontinuities in the flanks of Mt. Etna and Nyiragongo is a process that can take place many months prior to effusive surface activity intrusion (Komorowski et al., 2004).

This process can in fact be detected by remote sensing techniques as it triggers a significant increase in the photosynthetic activity of growing plants above the region of injection, as they are subjected to slightly increased temperatures, soil $C0_2$ concentrations, and soil moisture content associated with dyke injection (Houlié et al., 2006b). Normalized Vegetation Index (NDVI) satellite-derived images reveal these changes in plant photosynthetic activity and show that they are closely correlated with the position of fractures that then became eruptive vents after a few months (Houlié et al., 2006b). Of course, at Piton de la Fournaise, deformation related to external tectonic features cause is non-existent but the large northeast rift zone that has been fractured by last March 1998's event could be a good candidate for being the place of such an observation.

Finally and to end this debate about the ratio of eruptive volume vs. volume extruded from the magma reservoir, we note that HM2007's estimate for December 2005 (16.3 Mm3) is close to the volumes erupted (over 24 days of activity) as published by IPGP estimates (15–20 Mm^3).

We postulate that, with the present available dataset, the independent modeling of single eruptive events does not allow quantifying with certainty the contributions of tilt versus displacement. Moreover we think that the distinction of tilt requires more extensive effort than the work done by Battaglia et al. (2000). An example of what could be done was achieved by Wielandt and Forbriger (1999) at Stromboli. CB2009 argue that we could have done the same work at Piton de la Fournaise by using the VBB seismometer and the tilt-meter. One could say that there is no equivalent to a nicely installed STS-1 seismometer at Piton de la Fournaise and that the comparison of a three components VBB STS-1 and a two components tilt-meter is inappropriate. Such a comparison between two broadband seismometers (at Stromboli: a CMG-3T and a STS-2, two broadband (120 s) seismometers with similar sensitivity frequency domain) cannot be completed at Piton de la Fournaise because such settings are not available at the GEOSCOPE site.

We agree that the process of deconvolution can be unstable. That is the reason why the signal used by HM2007 is filtered between 100 s and 1000 s. CB2009 should have looked at the transfer function of RER, since the station is flat in velocity not in acceleration in the period range between about 1 s and 500 s (see the GEOSCOPE web site). Consequently only one integration is necessary not 3 integrations as claimed in p4 line 112 of CB2009.

4. Point 3: Spherical model

We agree that the spherical model is a simplified model and we would like to repeat that it is not exclusive of the existence of shallow dykes. The eruption processes are complex and it is likely that many phenomena are acting at the same time. Contrarily to CB2009, we try to put forward some alternative (and partly complementary) explanations of data. As any new model, it is imperfect but it casts a new vision on the volcanic scenarios. We point out in HM2007 that the RER seismometer is not able to detect any signal as soon as the eruptive configuration stabilizes: the dyke is then open, filled by magma and likely still connected to the magma chamber in the case of a large eruptive event. We dismiss then the argument as we were trying to model all eruptive processes by using a unique point source.

5. Point 4: Hidden dykes

Contrary to the assertion of CB2009, our model is neither in contradiction with theoretical calculations nor with basic physics. It is always possible to adjust parameters to get a good agreement between model and data but we cannot adjust the data to the models.

CB2009 suggest that the volume contained in the dyke should be equal to the volume coming from a deeper magma reservoir. This is a very optimistic view on our ability to quantify the respective volumes contained in the dyke, coming from the magma reservoir and erupted at the surface. Telling that the volume contained in the dyke is too small with respect to the total volume extruded, does not make sense if the volume erupted is not discussed.

CB2009 claim that InSAR is able to detect very small volumes and the volumes potentially missed by existing network or remote sensing techniques are not real. We remind that the InSAR is very sensitive to the shallow layer deformation (Bürgmann et al., 2006).

The work completed by Battaglia et al. (2005) on the 1998 seismic dataset could have been even more relevant if the study would have focussed on the focal mechanism of the seismic event detected or dedicated to the study of the LP or VLP signals (Chouet, 1996). Completing such an analysis would have led to better constrain the geometry of the conduit. However, Battaglia et al. (2005) remind that the link between magma arrival and the observed seismicity remains uncertain. We are more than happy to agree on this conclusion.

6. Point 5: Eruptive cycles

We agree with the discussion of point 5 since it addresses real scientific issues. As discovered by Battaglia et al. (2000), there are 2 types of signals (and probably more). These 2 types of signals can have different interpretations and that is the main point of our paper. Our interpretation is in disagreement with the previous interpretation of Battaglia and Bachèlery (2003) and we believe that additional data are necessary to better understand eruptive processes.

According to the most recent work published by Villemant (in press), the deep origin of the magma that erupted at Piton de la Fournaise in 1998 is not as obvious as that suggested by (Bureau, 1999). The 1998 lavas show a slight differentiation that could suggest a short storage of the magma in a reservoir. Thus, the constraints on the source of the magma erupted in 1998 are not based on petrological evidence but only on geophysical observations by Battaglia et al. (2000). This work has shown a linear progression in time of the location of the micro-seismicity crisis during the start of 1998. As both the signal detected at RER and the start of the seismic crisis are close in time, we advance the hypothesis that the opening of the magma chamber induced an upward motion of the magma stored in the magma system beneath the magma reservoir located at the sea level. Such models describing interactions between two magma reservoirs have been explored at least numerically (Aki and Ferrazzini, 2001). This hypothesis would explain why slightly differentiated magma was tapped in the shallow reservoir and erupted in 1998 (Villemant, 2009).

7. Conclusions

The comment by CB2009 raises the important issue on the existence (or not) of a magma reservoir at Piton de la Fournaise. We believe that there is one magma chamber because its influence can be seen on the long-term time series of GPS located close to the caldera limits. CB2009 do not detect such motions because the signal visible on tilt-meters is mainly dominated by the dyke propagation to the surface. However, such debate is common and recurrent in the volcanology community and happened on many volcanoes since the model proposed by Mogi (1958).

Our interpretation is primarily based on Mogis model whereas Battaglia and Bachèlery's (2003) is based on Okadas dislocation model. It is likely that both models are partly right but also wrong, because they are oversimplified models.

Since we have experience on volcanoes in developing countries with many colleagues who have to deal with limited resources, our feeling is that this is our duty to present models that can be applied and reproduced by the largest number of scientists. The formulae here are freely available in the manuscript and are easy to implement with limited computer resources.

We consider as unfair statements by CB2009 that we did not address the problem with enough care or that the modeling is inadequate. In our scientific approach, we only want to demonstrate that alternative models can explain the same data, but more importantly, that new data are absolutely necessary to discriminate models in competition.

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