Solid Earth Discuss., 7, C1015–C1018, 2015
www.solid-earth-discuss.net/7/C1015/2015/
© Author(s) 2015. This work is distributed under
the Creative Commons Attribute 3.0 License.

Solid Earth Discussions
Open Access

Interactive comment on "Cyclic fracturing during spine extrusion at Unzen volcano, Japan" *by* O. D. Lamb et al.

Anonymous Referee #1

Received and published: 27 August 2015

General Comments

This manuscript presents some insightful observations on the generation of seismicity during spine growth that will be of interest to the general volcanological community using clearly defined scientific methods. The entire manuscript is concerned with cyclicity that occurs over a four month period identified by analysing over 12 000 events. However, the detailed analysis comprising the bulk of the paper on which the conceptual model and the majority of conclusions are drawn is conducted on two clusters of events consisting of only 668 events. The evidence presented that the same cyclicity is present in this small subset of events is weak and needs to be improved. The final conceptual model is well presented but there are several aspects that I would like to see further discussed (see specific comments). I also feel that a lack of correlation

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

with physical observations of spine growth weakens any conclusions reached. Overall the manuscript is well written, there are however a few minor typographical errors (see technical corrections). Ultimately in its current state I would rate the manuscript as Excellent (1) for Scientific significance, Good (2) for Scientific quality, and Good (2) for Presentation quality but aspects of the conceptual model need clarifying

Specific Comments

P2113 – L10 – Can you sufficiently observe the low frequency behaviour suggested in these events on a 1Hz vertical component seismometer? I would like to see this discussed.

P2121 – L15 I am unconvinced by these key observations, it could be interpreted that the cyclicity "glides" from ∼40h – ∼100h over the full period of October – February, with a second period of cyclicity that glides from ∼100h - ?, it disappears in early December due to the scale of the plotted spectrogram (see technical corrections). Perhaps this is a consequence of the figure scale. Also, why is the previously identified 24hr cycle (P2121 L5) from the MTM spectra (Figure 2) not present (with the exception of possibly the first half of October). This needs to be explained.

P2121 – Focusing on the two largest clusters is sensible, but do these clusters really dominate the data set? Point three states they only make up 11% of events in this period. I would consider re-wording this. It is also unclear to me whether these clusters exhibit the previously identified cyclicity defined by the entire data set, particularly given the events plotted in Fig 3, which shows cluster 1 to be fairly continuous over the first half of its lifetime, not following the patterns of a 40h cycle. This may be a function of the scale at which Figure 3 is plotted, but regardless, the occurrence of "strong cyclicity approximately corresponding to that seen in the STEF analysis" (Point 5) needs to be made more explicit or the use of these events during the remaining manuscript is questionable.

P2129 – I have some comments and queries on the conceptual model that I would

SED

7, C1015–C1018, 2015

CC BY

like to be addressed. What would be an estimate of the increase in normal stress from the densification and cooling of the spine, does this match the required increases in normal stress identified in experimental work required for significant increases in seismic velocity to account for a time lag of 0.1s-0.2s (Figure 4(b)? Or to cause the location of these events to migrate to the shallower level suggested ($\sim$100m). An estimate of the changes in normal stress required to do this would be beneficial. The assumption that the opposite of the above would be true on the western margin of the spine due to "unloading" the contact is reasonable, but rather than the assumptions being that this will only result in a reduction of seismic velocity and/or deepening of the event location, it should also be mentioned that a reduction in the normal stress across this contact could likely also result in aseismic slip. As an aside this may be a possible explanation as to why the events in cluster 2 stop first as observed in Figure 3. It is hard to tell from the conceptual model presented in figure 6 whether the statement that the lower frequency content of cluster 2 can be explained by the presence of fluid is valid. It appears that the source to receiver path from cluster 1 would also pass through this region, causing a similar low frequency component to be present in the coda for cluster 2 events. The location of the FG1 Receiver needs to be further discussed to validate this statement. If these events do represent frictional controlled slip of the spine at the Condit margins (spine extrusion) the gliding cycles from $\sim$40h to $\sim$100h should correlate with an overall decrease in extrusion rate over the same period, unless the extrusion of the spine became more aseismic with time, this is not discussed and this link to physical observations (if available) would greatly improve the manuscript.

Technical Corrections

P2113 – L9 , "< 1k to where" -> "< 1km from where"

P2144, Figure 1(b) I am unsure of the vertical scale on Figure 1, is this Log scale, and does it really plot up to 672h as suggested in the caption? Please make clearer.

P2126 – L27, "this occurs conduit margins" -> "this occurs at conduit margins"

SED

7, C1015–C1018, 2015

CC BY

P2128 – L 16, “damage in and close to volcanic conduit” -> “damage in and close to the volcanic conduit”

Interactive comment on Solid Earth Discuss., 7, 2109, 2015.