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

Anonymous Referee #2

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Overview This paper analyses and describes the seismicity associated with a 5-month phase of spine extrusion at the end of a four-year dome forming eruption at Unzen volcano, Japan. The work consists of the application of a comprehensive range of seismological and statistical techniques to the dataset, including: spectral analysis to identify apparent periodicities in seismic event rate; waveform correlation and clustering algorithms to identify repeating earthquakes and their families; singular value decomposition to determine relative event amplitudes; and coda wave interferometry to identify relative source locations and offsets. The results of these analysis reveal an apparent 40h (evolving to 100h) periodicity in the seismic event rate, largely associated with the activity of two dominant families located on either side of the ascending spine. The SVD and CWI analyses suggest that there are subtle changes in both seismic velocity and event locations through the extrusion episode. The authors present

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a conceptual model for the origin of seismicity during this phase of activity at Unzen, involving the progressive extrusion of an inclined spine. Different stress conditions on the upper and lower margins of the densifying plug result in a deepening and shallowing of the failure horizon, respectively, and an increase in seismic velocity on the lower margin.

Overall, I think that this is a good paper, utilizing a range of techniques that have been thoroughly and thoughtfully applied. It is generally well-written, and the results are well-illustrated. Although the new insights provided by the analysis are relatively modest, and partly build on previously reported findings on this eruption, I feel that they are interesting and important for our understanding of spine extrusion processes. I have two main comments that I feel need to be addressed further before publication, and some additional minor comments. My ratings according to the 'Principal criteria' are: Scientific significance: Good (2) Scientific quality: Good (2) Presentation quality: Good (2)

Main comments

Periodicity in event rate: The ∼40h periodicity at the end of October and start of November 1994 is evident in the raw counts data (e.g. Fig 3d), as well as in the results of the STFT and MTM analyses. It appears to persist for in the order of 10 cycles. I feel that evidence for a 'gliding' of periodicity to lower frequencies is less strong, and mainly emerges from the spectrogram in Fig 1b. It is not really evident in the MTM analysis for February 1995. Inspection of the hourly rates suggests a periodicity of around 50h might emerge in mid-December, but after that the periodicity appears rather weak. In section 4.1, there is an extensive review of possible mechanisms for cyclicity during dome-growth eruptions, but this is not really linked to the possible processes at Unzen (the authors fairly comment that it is difficult to attribute cyclicity to different mechanisms with the data available). I would like to see either strengthened evidence for the gliding and 100h periodicity (e.g. correlation with other data sets), or a more measured discussion. I'm not sure that this finding is central to the paper, and I feel that an

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emergence and break-down of periodicity is as interesting as a change in frequency. Could the authors discuss what combination of processes might be responsible for strengthening and weakening of periodicity in the evolving extrusion? For comparison purposes, I would like to see Figure 1b where each window is not normalised by the maximum amplitude, as this may also be informative.

Clustering: In section 3.3, the authors state that only 11% of events are part of clusters. However, they choose to use a high threshold value (0.8) to define these clusters 'based on visual inspection of the cluster tree'. I am interested as to how the number and size of clusters change with this threshold. Does lowering the threshold to 0.7 (typical in some other studies), increase the number of events in these two main clusters, merge these two clusters, or result in an increased number of clusters? I feel that this analysis could benefit from more detailed discussion, as the subsequent work does not consider the remaining 89% of events. Is 11% typical of this type of analysis at other volcanoes? If the P-T-t path of the magma is responsible for controlling the earthquake location and mechanism, and is somewhat stable, why are not more of the events occurring at repeating sources (presumably along some evolving inclined horizon at the margin of the plug)? Do the clustered events have some difference in frequency content suggestive of a different source mechanism, is the signal:noise ratio higher, or is there some other explanation?

Minor comments

Page 2111 – line 9: 'stress variations during magma movement' – I think there are more reasons than this for VT earthquakes, and I don't think there is a need to be so specific.

Page 2114 – section 2.3: I think the difference between the STFT and MTM methods, and when you choose to use them, could be explained a little more clearly here.

Page 2114 – line 15: 'obscure the true signals' – I'm not sure 'true' is the most appropriate word here. Perhaps something along the lines of 'make it difficult to separate

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individual signals'?

Page 2117 – line 1: You might also want to consider referencing 'Rodgers et al., Seismicity accompanying the 1999 eruptive episode at Telica Volcano, Nicaragua, JVGR, 2013', which discusses several aspects of cluster analysis.

Page 2117 – line 17: I think this section would benefit from a brief 1-2 sentence lay-summary of what SVD is and tries to achieve, before beginning the mathematical description.

Page 2123-2124: The second paragraph in section 4.1 reads to me as more as a review than discussion. I feel that some of it could be moved into a brief introductory section on periodicity in dome extrusion eruptions, and the remainder be more explicitly focused on the process

Page 2126 – line 15: 'exclude the movement of fluids as a source for the repetitive seismicity' – I would agree that as the gas flux does not correlate with the occurrence of repetitive seismicity, then gas flux alone cannot be the factor determining when repetitive seismicity occurs. However I do not follow the logic that excludes the movement of fluids from a role in the repeating source. It's very likely that magma failure plays a major (controlling?) role, but is it not still possible that it is an interplay between failure and gas movement that results in the seismic source?

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