Solid Earth Discuss., 7, C1262–C1265, 2015
www.solid-earth-discuss.net/7/C1262/2015/

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 #3

Received and published: 29 October 2015

Cyclic fracturing during spine extrusion at Unzen volcano, Japan. Lamb et al.

The paper provides an interesting analysis of cyclicity and characterisitcs of seismic activity associated with spine extrusion in the latter phases of the Mount Unzen lava dome eruption. In particular the findings show gliding in the period of the cycles over a 4 months, as well as clusters of events that appear to show migration of sources in different directions. The authors discuss the results in the context of other published work and provide a conceptual model for seismicity associated with the inclined extrusion of the spine.

Abstract: In two places in the abstract the authors state that the paper is supported by, or combined with, field and experimental work. The field and experimental work are reported elsewhere, and is not part of this paper, although this other work should

be brought to bear in the discussion of the results of this paper – it should not really be reported as part of the same work as it is in a number of places throughout the manuscript.

Section 1.1: The final conceptual model relies heavily on the fact that the spine at the surface was seen to extrude obliquely, and inclined at an angle of 45 degrees. Is there any other surface or near-surface evidence that this inclination continued at depth? Is it possible that as the solid lava approached the surface that it keeled over, or that it lunged sidewards only after some part of it was extruded, but that this pulled lower portions of the spine sidewards ? It would be good if the authors could be make a more convincing case that this angle at the surface was indeed representative of what was going on deeper down.

I would encourage the authors to expand on the chronology of events as observed at the surface during the period of spine extrusion. The current discussion is brief and relates mostly to the eventual textures found on the surface. The data and results of the paper related to how extrusion changed through time, so a chronology of eruptive events at the surface would be helpful to place the observed seismic activity in better context.

Section 2.2: Perhaps clarify in this section if this detection algorithm is for all/any type of seismic events occurring. .... presumably there were different types of events occurring. How were these classified ? where they all used ? There is a discussion in the introduction about different types of seismic events associated with dome extrusion and seen elsewhere – but the authors don't provide an explanation of the types of events occurring here. More information is needed about this period of this eruption to put the analysis of the seismic data undertaken in this work in context.

Section 3.1: The initial results here report number of events but without any explanation about what type of events and whether they were all the same general type of event (I assume that is the case – and for example that this event catalogue does not include

rockfall signals etc. It seems to be taken as a given here that the authors are now dealing with a catalogue of drumbeat or hybrid type events . . .but this point really isn't explicitly made anywhere.

Section 4.1: The observed gliding is interesting – it would merit more discussion on its source/cause. There is lots of general, and knowledgeable, discussion of the previous literature in this section, but little of it is really directly tied to the results found here.

Section 4.2: the new results here are: 1, the two clusters, and 2, the opposing arrival polarities. The great majority of text in this section is a discussion based on previous literature but very little of it is directly relevant to the results actually found here. Its an informative discussion – but really more at home in a review type paper– it doesn't seem to add much to this paper. It should either be cut down or tied much more directly to the results found.

Section 4.3: Put the '100 m displacement' (line 26) into context – what errors could reasonably be expected in such results ?

Section 4.4: The explanation of why the source migration for the clusters is in opposite directions for each side of the spine is not clear to me. Further, discussions base on a spine having two sides (either east or west, upper or lower sides), is also counterintuitive given that the spine margin is continuous – possibly forming an elliptic cylinder shape. The explanations here were hard to follow 'the spine sunk away from the contact, unloading it from below'. . .etc.. just not easy to follow. Perhaps this could be rectified by a good figure. Figure 6 could be improved by trying to represent the situation in 3 dimension, and illustrating approximately the how the stress conditions change with time in different places – perhaps by having 3 versions of the image and showing how it changes with time as the spine is extruded.

Minor issues: The spine dimensions are a bit confusing: 150m long x 30 wide x 60 high (?). . . especially as its said earlier that the spine was 30-20 m diameter.

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