Reply to comments on "Eruptive shearing of tube pumice: pure and simple" by D. B. Dingwell et al.

Dear Editor,

Thank you for your patience with this manuscript. Owing to complicated logistical circumstances, it was difficult to carry out this review in a timely fashion.

Please find herein a revised version of our manuscript as well as a point-by-point reply (green) to the comments (black) made by the reviewers. Both reviewers found this manuscript of interest and having answered their queries, we hope the revised manuscript will see to fulfil the requirements for publication at *Solid Earth*.

We thank you for your time and effort with this manuscript.

Kind Regards,

Donald B. Dingwell and co-authors

Reviewer 1

This is a well-written manuscript, which discusses the processes leading to the formation of tube pumices. Tube pumices are considered quenched magma parcels recording its physical properties at the moment of fragmentation and are therefore important to identify the processes responsible for an explosive volcanic eruption. I have to say that, as currently presented, the arguments in support of simple shear are not extremely convincing and I think the authors should clarify the logic sequence leading to their conclusions.

As we discuss in our letter and reply below, this is the first application of bubble geometry analysis onto tube pumice made of a large number of pores imaged by tomography. Simple shear is favoured as, simply put, the geometry obtained cannot be modelled using pure shear constraints (from Rust et al., 2002). We hope to have clarified the text sufficiently to ease understanding of this important conclusion.

I provide here some general comments. More detailed ones are given in an annotated pdf. The authors describe the tube pumices analysed as "highly representative of the eruptive products in these highly homogeneous deposits of Ramadas volcano" (Lines 105-106). The main conclusion of the paper is that the tube pumices are formed by simple shear, which, as indicated in Fig.1 is normally limited to a relatively minor portion of the conduit next to the wall. If I understand correctly the deposit of the eruption investigate is essentially composed of tube pumice. How do the author reconcile the simple shear argument with a deposit in which tube pumices are very abundant (Lines 86-87)? I think the authors should specify in the text the relative proportions of tube pumices and pumices with other textures in the deposit.

The Ramadas deposit consists of tube pumice only. It holds no pumice with "more spherical" vesicles. 40% of the tube pumice however contains overprinting by kink bands. This is stated in the text.
So why is it that only tube pumice erupted? We wish we knew the answer to this; it may be that the eruption occurred along a narrow fissure, prompting dominance in

simple shear; yet, we wish not to speculate about that as the original vent structure through which the eruption took place is buried.

In Figure 8 it is not clear with respect to which direction the angle Θ is measured. This should be clarified either in the figure (may be with an inset) or the associated caption. This is quite important because Θ is the angle between the principal axis of the bubbles and the shear direction and it is measured in the plane containing both the shear direction and the velocity gradient (Rust et al., 2003). How did the authors identify the shear plane in the tube pumices? This should be explained in the text.
The angle is the orientation of the horizontal cross section. It is obtained as one of the fitting parameters for the ellipsoid. The zero angle is chosen arbitrarily (and we made a mention to this in the caption). What this figure shows is that the horizontal cross sections of the bubbles are strongly aligned. This is what we expect for simple shear.

In Figure 9 why the authors do not use the same diagram provided in Fig.2 of Rust et al., 2003?
Because we wished to demonstrate the adequacy of the two fits against our data; This is a simple rearranging of axis which holds no consequence for the conclusions of this work.

Lines 313-320. This part is the fundamental for the conclusions of the paper but I do not find it convincing. I refer in particular to this part: "From the mechanical scenarios that we have envisaged, the bubbles in a pure shear regime would implicitly stretch with the flow direction regardless of their initial size. . .". I do not think this is correct, the initial bubble radius enters in the formulation of the capillary number and therefore bubbles of different radii will react differently to deformation, with the smallest ones potentially not deforming at all.
I suggest the author revised all the text from Lines 313 to 326 and produce a more convincing Figure 9.

We thank the reviewer for pointing out at this error in the text: it is true that bubble alignment is bubble size dependent. This, as well as other parts of the paragraph has been rewritten.
As for Figure 9, we believe it is the best way to represent the geometrical data as anyone can grasp bubble length and the concept of bubble undeformed radius, whereas the use of capillary value on the x axis, is not as intuitive for readers not so conversant with the capillary number. Ultimately, this does not change the conclusion of the paper and as such, we wish to retain the use of figure 9, which we deem highly satisfactory.

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<u>Reviewer 2</u>
The main focus of this paper is using tomography data on bubbles in tube pumice to assess the shear regime of magma leading up to fragmentation. This is a worth-while topic and a good approach to address it. However, because there are re-sults/methods/reasoning insufficiently explained, I am unable to assess whether the some of the conclusions are warranted. It may be that only minor revisions are re-quired but I cannot tell without seeing them.
1. Introduction: It isn't clear what is "enigmatic" about the thin bubble walls of tube pumice. (Be more explicit in your reasoning). Also, non-tube pumice often have very thin glass bubble walls - is that enigmatic too or is there something about tube pumice in particular?

We appreciate that the reviewer may deem the wording of this sentence inappropriate. We have removed the word enigmatic, yet we note that the vestige of thin bubble walls in any pyroclasts formed by fragmentation, which is rather violent in these large explosions is somewhat enigmatic; Here we have seen thin bubble wall of the order of 8 microns. From what we know, no one has fully pursued an analysis of stress stored and release at the point of fragmentation, considering their extreme fragility. To term this feature enigmatic is certainly not an overstatement; it likely is an important clue to fragmentation dynamics that ought to be explored by the community.

I do not understand what you have in mind to cause acceleration of magma in a Plinian eruption if not bubble growth. So I don't understand: "For instance, pure shear may be favoured in a regime where the strain induced by magma ascent acceleration exceeds the bubble growth rate associated by volatile exsolution".

Ascending magma does not necessarily require further bubble growth to accelerate; decompression itself may help, magma may be pushed from below and lowering of the fragmentation front will modify the pressure gradient which, we agree, drives bubble growth, but also force flow at a higher rate irrespective of the bubble growth. Here the statement is not about the cause of acceleration, but in explaining how pure shear may dominate: this is possible if the stretching of bubble happens due to direct pull (parallel with flow in conduit), possible if magma ascent accelerates; if magma was static or flow rate linear, bubble growth would be isotropic. Here we have rephrase the sentence to:
"For instance, pure shear may be favoured in a regime where the ascent rate increase, stretching the bubbles in the flow direction."

Section 2.
"The porosity of the undeformed pumice has been estimated at 49–64 %, whereas the tube pumice reach 63–78 % (Marti et al., 1999). ...Up to 40 % of the tube pumice exhibit localised kink bands, or box folds. . ." I found this text confusing: at first it led me to think that that there are pumice clasts that are not tube pumice. You refer to "unde- formed pumice. . . whereas the tube pumice" suggesting that "underformed pumice" are not tube pumice. (Note that you introduce that some pumice have kink bands AFTER writing about "undeformed" pumice.)

This section has been reworded for clarification.

How do you know that the sample is "highly representative"? Perhaps make a less bold statement (or else back it up!).

We have removed the word highly.

I don't think there is an indication of the size of the pumice clast for which the bubbles have been studied in detail. Is the clast photographed in Fig. 2 the one studied here in detail?

Yes, this sample has been analysed and the photograph shows a coin for scale. We added a mention in the text and also added in the caption of Figure 2b that the dimensions of the sample are about 40x40x25 mm^3.

Section 3.
"glass shards require minimal sample preparation" What was the sample preparation? Was any polishing done? Presumably, polishing was needed where the

there was intersection with a second bubble wall, in order to make the maps of Fig 3 (otherwise would need to correct for thickness variations).

We agree that without any further detail, it is difficult to understand the sample preparation. We rephrase this sentence to:
"The perfect plane-parallel nature of tube pumices' bubble walls mean that the fragmented glass shards require minimal sample preparation to meet the requirements to be able to conduct micro-FTIR spectroscopy, as the bubble walls are naturally smooth and perfectly flat; The area where one bubble wall intersected another bubble wall where not considered as it was not possible to ensure a constant thickness for these minuscule samples."

Explain further the reasoning for "we infer significantly higher water contents tha[n] this minimum estimate during the shearing flow described here". What water content is used for the melt viscosity calculation later? The "minimum" value or some value that is significantly higher?

We see that this sentence, as written, is somewhat misleading and we removed it. We were simply detailing that the water content would have been at its lowest at fragmentation (assuming that fragmentation was followed by a trivial amount of degassing); but deeper in the conduit, leading to the point of fragmentation, there would have been more water. As this inference is not central to the story, and in fact detracting, we simply removed the statement.

Section 4. "and potentially further aspects of bubble–bubble interactions may ensue at high vesicularities, we see in these samples no evidence of the latter. " The non-elliptical cross-sections of the bubbles look to be evidence of bubble-bubble interactions (deformation related to pushing against each other as they expand).

Yes, that is correct. We have corrected this mishap by rewriting the sentence to:
"Although the experimental calibration of these relations have been performed at lower bulk vesicularity than is exhibited here, the precedent of applying this treatment at similar levels of vesicularities is well-established in the recent literature (Moitra et al., 2013)."

I'm confused by the calculations. (e.g. I do not follow how Ca is calculated based on " The integration of Eq. (2) through (7) provides us with the capillary number of bubbly magmas".) This is the core of the paper and so the steps must be explained clearly. In doing so, I suggest separating the explanation (and equations) for pre-fragmentation analysis before explaining the post-fragmentation analysis. If I have corrected deduced what was done, then to solve for shear rate (for which a value is reported in figure caption of Fig 9) then you needed to know the melt viscosity which required an estimate of the water content. What value was used for w? However, apparently there was fitting for w: "The results of that fitting yield water contents consistent with the discussion above." Please be more specific - what were the water content values resulting from fitting? The figure 9 caption says "The best fit for simple shear requires a characteristic cooling rate of $10^{-4.9}$ s^{-1} and a strain rate of 10^{-2} s^{-1}" That sounds like the characteristic cooling rate is a fitting parameter. However, I think it is not, but rather is the value determined from a laboratory experiment. Rephrase for clarity.
We have revised this section for clarity, separating the pre- and post-fragmentation analysis. We believe the steps are now clearer detailed.

The viscosity estimate was made considering the magmatic water content constrained as 1.0 wt.%. We have also modified figure caption 8 (previously figure 9) to remove this incorrect statement as the characteristic cooling is not fitted.

Is it feasible that the actual cooling rate could be significantly faster than the laboratory experiment? For example because the natural clast was travelling quickly through air?
Perhaps it would have cooled faster; perhaps it would have cooled more slowly (for example if thermally insulated in volcanic plumes. In absence of further constraint, we choose to rely on the cooling rate we can measure in air.

It is argued that the range lf bubble shapes for a given bubble volume can be explained by relaxation during cooling. If that were then case I would expect systematic spatial variation in bubble shapes because (for all else equal) the bubbles near the margin of the clast will cool more quickly than those in the middle. With tomography data it should be straightforward to look for spatial variations.

We looked for such variation early on, and saw no evidence of gradient related to relative cooling. The characteristic cooling rate suggest that cooling through the glass transition (where all structures get locked in) would have been quite rapid, thus preventing from development of strong cooling gradient that would have force the spatial distribution of bubbles morphology across the sample.

“whereas pure shear required non-physical values of strain rates and an infinitely fast cooling rate in order to fit some of the observations.” It would be helpful to indicate what strain rates were found.

Essentially, the data cannot be fitted for pure shear. We rephrased to: “whereas pure shear failed to fit some of the observations.”

I was confused about the angle used to quantify bubble orientation. Theta in Figure 1 is related to the orientation of the longest axis of the bubble (L), which makes sense. However, the theta shown in Figure 8 appears to be related to the orientation of B. It is not clear how theta was measured for the data shown in Figure 8. (In the diagram theta is the angle between the B axis of the bubble and a line but I don’t know how you know where that line is in the sample). For interpretation of orientation data, it would be useful to plot orientation vs. bubble size (a).

The angle is the orientation of the horizontal cross section. It is obtained as one of the fitting parameters for the ellipsoid. The zero angle is chosen arbitrarily (and we made a mention to this in the caption). What this figure shows is that the horizontal cross sections of the bubbles are strongly aligned. This is what we expect for simple shear.

The ellipticity data shown have a mode that looks very similar to the cross-section for bubbles in 2D hyperbolic pure shear of Hinch and Acrivos (1979). Even if this correspondence is a co-incidence, it does also highlight that can have non-circular cross-sections in a type of pure shear.

As pointed out by the reviewer, it is interesting to see the similarity of our bubble section distribution with that of viscous droplet in shear flow by Hinch and Acrivos (1979), and although a comparison is inviting, we refrain to do so without complete consideration of the importance of the viscosity of the object (be it bubble or droplet) relative to the surrounding medium. As such, we wish not to include this reference here. We are certainly open to future discussions about the potential link between the two and the applicability of this study to bubbly magma shearing; the reviewer is welcome to approach us.

There does not seem to be a consideration of types of pure shear flow: axisymmetric extension vs. hyperbolic flow (e.g. Hinch and Acrivos, 1979). Also one can have a combination of pure and simple shear.
True, both pure and simple shear may be acting in the system, hence the title. Yet, in the tube pumice we analysed, only simple shear seems to be responsible for the final porous structure. This does not mean that pure shear did not act earlier in the history of this pumice (i.e, at greater depth than the fragmentation level), or that pure shear did not act elsewhere on other clasts. Constraining the latter would require a large tomographic reconstruction of many pumices. Here, we laid out the method to do so; future studies may be able to answer these questions, but this will require a very large statistically relevant dataset, which will result in an enormous undertaking by several people over several years.

Section 5.
An idea is presented for the origin of kinks in some of the tube pumice. It would be nice try to link this to the fact introduced earlier that kinked pumice are of lower vesicularly than the unkinked tube pumice.

We find the kink fascinating but do not generally know their origin in the pumice; We certainly encourage a targeted study on the topic. Here our analysis does not provide information to speculate about its origin, we simply note the similarity with boxfolds developed after minor strain imposed on layered bodies, to relate its presence to our eruption schematic. The text state that this is a suggestion and we hope it can remain as such, although it is not central to the conclusion of our study. Please also, note that the vesicularity of deformed (kinked) pumice, is higher than that of the undeformed pumice; this is now made clearer in the revised manuscript.

I didn't understand how the following is evident (i.e. what is it based on?): "a shift of the fragmentation level in the magma column to greater depths and higher pressures as has evidently been the case at Ramadas."

Here, we rephrase the sentence, and removed "as has evidently been the case at Ramadas" to leave it as a supposition, as whether this has happened at Ramadas is speculative. The idea behind it is that as simple shear elongates bubble, and increase the resultant (vertical) permeability in the magmatic column, it might cause the deepening of the fragmentation front towards regions where significant pore pressure remains (ie., where the gas may not be effectively bled through the permeable tubes). Yet, this statement was modified to let the reader decide of the potential faith of the permeability evolution in a magma ascending by simple shear.

Abstract: The work on water content and isotopes is interesting and new: a brief sum- mary of these results should be in the abstract even though not the main focus of the paper.

Thanks, this has been amended.

I don't understand what is meant by: ". . . implies that magma ascent is conditioned by a velocity gradient at the point of origin of tube pumice."

It is meant that simple shear implies a velocity gradient, as in, for instance, plug flow.

More on figures:

Figure 6 could be combined with Figure 4.

Done

Figure 7 caption. Where does the 7mm3 come from in: “51.2 µm × 51.2 µm × 51.2 µm (i.e., about 7 mm3)”?

Good question. This is a severe typo. Thanks for spotting this inconsistency. It has been removed as the dimensions are sufficient.