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Interactive comment on "Anatomy of the magmatic plumbing system of Los Humeros Caldera (Mexico): implications for geothermal systems" *by* F. Lucci et al.

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The authors present some new whole rock major elements and mineral chemistry data on a suite of rocks (from basalts to trachytes) of the post caldera activity of Los Humeros. Using a large amount of new mineral chemistry data, they calculate thermobarometric conditions for the Holocene plumbing system which allow them to put forward a new model in line with the recent idea of a trans-crustal plumbing system (e.g. Cashman et al., 2017).

I like the proposed model and how the thermobarometric constrains are put together with petrographic and mineralogical observations. However, there are aspects that

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Discussion paper

should be discussed in more details. In fact, the manuscript will benefit from a more in-depth discussion particularly regarding the implications on magma dynamics and the geothermal system, but also on other aspects such as the absence of magma mixing, the role of the crust and fractional crystallization. Some suggestions along these lines are: 1) A single liquid line of descent is inferred only from few major elements, namely SiO2, Alk and CaO on a limited suite of rocks. This is a key aspect of magma evolution at Los Humeros, but it is not discussed thoroughly. Even when the connection with typical basalt-pantellerite is questioned, no further explanation is offered. For example, how do you explain the presence of aegirine augite given the lack of clear peralkaline composition? Magmatic diversity is really the results of simple fractional crystallisation? A more in-depth discussion on the origin an evolution of these suite of rocks is necessary. 2) The almost complete lack of magma mixing is remarkable, pointing to single batches of magma evolving in closed system. I assume these are completely evacuated during eruption and presumably not reactivated(?). This is a quite unusual dynamics for a caldera and different from previous activity with large ignimbrite. It can also have important implication on the geothermal system. It would be good to see a more in-depth discussion on these points and the implication on magma dynamics. 3) The area is characterised by $\sim$ 30 km of crust, but the role of the thick crust is not explored. 4) From the model it is seems that trachyandesites and trachytes form at 30-15 km in the second stagnation level (L774-790). Here it is claimed that pl+cpx2 + microlites form. If so it means that the liquid is not changing composition at shallower level. Therefore, the liquid must pond at the third stagnation level (15-10 km, cpx3) and at shallow level (7-3 km, cpx4, cpx5, Fe-olv and opx) for very short time and possibly shortly before the eruption. What are the implications for magma dynamics and for the geothermal system? 5) The paragraph on the implication for the Los Humeros geothermal system is a repetition of the proposed model of the plumbing system rather than a real discussion of the implication of the model. This part should be used to actually discuss how the geothermal system can be sustained in the light of the new model of small single-charge of magmas at different level (from 30 to 15 km upward) and ephemeral pockets at shallow level. Is the current geothermal system different from the pre-caldera stage? If so, is there any inference that can be drawn?

The manuscript will also benefit from some reorganization. In particular, the thermbarometric section should be separate from the discussion. In fact, at the moment it is quite a long section, not extremely clear, with a long list of P-T conditions that are quite hard to see in Fig. 11. This part should be dedicated to present the new T-P data and discuss the discrepancy between different themobarometry (olv-liq gives always lower T, plg-liq higher T). The discussion should start at section 7.4.

The authors talk about "inverse thermobarometric modelling" (in the abstract and in various part of the text). However, the standard mineral-liquid approach is used throughout, which in literature is commonly referred to as mineral-liquid thermobarometry not as "inverse". A clarification on this point is necessary.

It is stated that the "this study attempts to emphasize the importance to integrate field-petrography, texture observations and mineral chemistry of primary minerals to unravel the pre-eruptive dynamics". However, the field evidence is quite sparse and not used to support the data and conclusions.

Overall, I recommend the manuscript for publication pending moderate reviews.

Kind regards, Chiara Maria Petrone

Specific comments along the text.

Major elements – the description of the major elements characteristics is quite redundant and so are the presented diagrams in Fig. 3. The TAS diagram with the subalkaine/alkaline division line (which is missing, why?) would be totally sufficient to inform the reader of the chemical composition of these rocks. Fig. 3c and d don't add any further information. The text is unnecessarily wordy accounting for the range of variability of SiO2, NaO2, K2O etc that we can clearly see in the diagram. It would be more useful to show the harker diagrams alongside the TAS. In addition, I find the choice of symbols quite confusing. In fact, the information on the SiO2 contents is already shown in the x-axis, therefore the symbols are a simple repetition of the same information.

Petrography – it is not clear why the Texacal lava flow (Fig. 3) is called olivine basaltic lavas (Fig. 1) when olivine is always present as phenocrysts. Only 3 basalts and 2 trachytes are described which makes quite hard to attach too much meaning to the presence or absence of cpx (basalts) and the "substantial" difference in mineral assemblages of the trachytes. I suggest to be more careful in drawing conclusion from such a small number of samples.

Mineral chemistry. I find the section on cpx section very confusing and unnecessarily complicated with 5 different categories of cpx. The zoning pattern is completely lost. In fact, cpx 3 overlaps cpx1 & 2, therefore it is difficult to see if cpx in basalts and trachytes are actually zoned. If so how? The other interesting feature, the presence of microlite and outer rims of aegirine-augite composition, is hard to correlate to specific composition and/or location. Are the cpx microlites in basalts all cpx5? It is well expected that Fo and Mg# decrease from basalts to trachytes, so this is not really a good criterion for discriminating crystal population. How about the behaviour of trace elements such as Cr and Ni?

7.3.2 this part is very difficult to follow, it is a list of P-T condition very difficult to visualise. How do the reader will figure out the difference between opx-free and opx-bearing trachytes if they are not distinguishable in Fig. 11? Please rewrite this section.

Fig. 11 - The difference between basalts, trachyteandesites and trachytes is almost lost in this diagram, particularly for the trachytes. Maybe Fig. 11 can be redrawn and composed of 4 separates diagrams showing the inferred P-T condition for basalts, trachyandesites and trachytes. All the results can be shown together in the last diagram colour coded for composition. It would be easier to see where the different rock types crystallise and this should also be used in the discussion.

L64-68 - The recent work of Jackson et al Nature 2018 show a numerical modelling for the network of melt and mush.

L261-262 – It is not clear what do you mean with a lava flows being free of lithics.

L366 – It is quite interesting that plagioclase cores are more mafic in trachyandesites than in basalts. Why?

L432-441 – No need of this introduction

L472 - you haven't presented Fig2. 7-10 yet

L482-484 – please revise the English.

L485-487 – Strongly oscillatory zoning can be due to different causes mainly magma mixing and/or P-T-H2O fluctuation, it reflects the dynamics of magmatic processes and might be not an indication of equilibrium. On the contrary the low-amplitude oscillatory zoning is kinetically controlled (see Streck 2008 for further details). Therefore, it is important to stress which type of oscillatory zoning the authors refer to. In addition, the fact that the zoning is oscillatory precludes that core and rim formed in evolving liquids with progressively different compositions as argued.

L488-489. The use of WR composition instead of glass is problematic especially when dealing with microlite.

L502-503 – On which basis this additional criterion has been chosen? The test for equilibrium according to Putirka 2008 is satisfied when KD(Fe-Mg)opx-liq = 0.29$\pm$0.06.

L504-514 – This part is quite confusing. The Rhodes diagram (Fig. 8) are for olv and opx, eq. 17 of Putirka is for olivine, how the cpx-liq fits with this?

Appendix – what is the error on EPMA data? How Actlabs and XRF data compare?

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