

REVIEW by Referee#2

We thank Referee#2 for the critical and constructive comments. In the new version of the manuscript we have taken into accounts all the received comments and advice. We detail below how each comment was addressed and considered in the revised version (the Referee's comments typed in italics and our responses typed in red).

The authors did a great analytical job and their proposal about the multi-reservoir model is in accordance with recent investigations.

We thank Referee#2 for the appreciation of our job and the constructive comments that have been considered in the revision of the manuscript.

However, I think data could be used in more detail to explore other scenarios. The main idea will remain, the polybaric model seems unrefutable, but the processes involved during the evolution of magmas could be an important factor for the geothermal system. The model proposed by the authors rely in only one differentiation process; crystallization. Whereas mixing, and perhaps assimilation, seems to be important processes.

We thank R#2 to confirm that a polybaric model is the only possible explanation for the Los Humeros magmatic activity during the Holocene post-caldera stage.

We agree that many processes such as fractionation, mixing/recharge and assimilation could generally control the magma evolution in feeding systems. However, in the case of Los Humeros, LHPCS products do not show any evidence of mixing and assimilation processes.

Following the comments of Referee#1, we produced major elements mass balance modelling and demonstrated that all studied samples can be produced through Rayleigh fractional crystallization (FC) of the same fixed mineral assemblage of Pl+Cpx+Ol+Sp (see new section 8.1). Therefore, all LHPCS studied rocks belong to a single line of descent and can be generated without the necessity to invoke assimilation or mixing phenomena.

Furthermore, fabric and textures observed in all samples clearly exclude the possibility of assimilation and mixing/recharge processes.

As we wrote in the response to Referee#1, all Los Humeros samples (from basalts to trachytes) are characterized by unzoned homogeneous or normally growth/zoned phenocrysts.

We explained already in the reply to reviewer # 1 the conditions for the zoned phenocrysts, and the complete lack of magma mixing for the LHPCS.

However, we agree with R#2 that this point must be better discussed in the manuscript as we do in the new version.

Something should be explained about the origin of these magmas. Once explained, you can go further and propose how do you think they evolved.

As we answered to Referee#1, we consider that the origin and the evolution of the Los Humeros post caldera stage (LHPCS) magmatism, requests a deeper petrological approach, which is far beyond the scope of this manuscript. Instead, this paper is focused on the reconstruction of the anatomy of its magmatic plumbing system using the crystal-melt pairs (as in Stroncik et al., 2009; Aulinas et al., 2010; Dahren et al., 2012; Keiding and Sigmarsson, 2012; Coombs and Garner, 2014; Barker et al., 2015).

I think mixing is evident, for example:

1) TAS diagram looks very linear. Find published liquid lines of descent for basalts evolving to trachytes just by crystallization. I think they do not look like the trend displayed by your samples.

We already addressed this point in answer to Referee#1 (see also the answer above)

However, a liquid line of descent where basalts evolve to trachytes could be found in White et al. (2009) (in the reference list of the original manuscript). Linear trends could be found also in Moghadam et al. (2016) and in Lucci et al. (2016) where FC models were used to demonstrate

lines of descent of mafic parents evolve to felsic daughters. Here, we applied the same mathematic model. References added in the revised manuscript.

2) Plagioclase microlites and phenocryst rims show a very wide compositional range An20-63. Is evident this are not in equilibrium.

Phenocrysts and microlites show a wide compositional range as normally expected for progressive crystallization from the reservoir to the surface during the ascent of the magma.

We applied to all phenocrysts and to all microlites, the same Min-Liq test for equilibrium, and we selected for thermobarometry models, only those in equilibrium with the nominal melt. The other were discarded.

3) The presence of Cr-rich titanomagnetites and Cr-poor titanomagnetites in the same sample. In fact, I suggest to remove the ulvospinel data, is of bad quality. Either, the crystals were very small and you excite the surrounding matrix or something failed with the standardization. Although data is not publishable, the relative abundance of Cr is evident and you should explain it.

We disagree with this comment. As fig 4 (BSE images) shows, analysed specimens were big enough to shot: Cr-spinels phenocrysts are up to 200 μm in diameter (4a). Ilmenite crystals in the groundmass are usually 20-30 μm width and up to 100 μm length (4c, 4d). Concerning Ti-magnetites we always shot on phenocrysts with up to 50-100 μm in diameter (4g) or on crystals in groundmass with ca 20 μm in diameter (4f).

We reported dimension of spinel in the revised section 6.5, entitled "Spinel and Opaque Minerals". The used spot sizes were 1-10 μm , depending on the phases analysed (see appendix A methods of the submitted manuscript).

Concerning the possibility to excite the surrounding matrix, spinels are in a groundmass of Cpx+Ol+Pl (and they are Cr-free, see the EMPA Supplementary tables) or they are included in Cpx phenos and Ol-phenos (with $\text{Cr}_2\text{O}_3 < 0.06 \text{ wt}\%$). Therefore, the only source for Cr is the spinel itself.

Concerning the hypothesis of failed standardization, in such case all elements should be wrong not only Cr. Our analyses close very nice (Total 90-97 wt%) and recalculated formulas are coherent with those expected for the Spinel group. Therefore, there is no evidence to say that they are bad analyses.

We are confident to have demonstrated that our analyses are good, and we will keep them as it is. In the light of thermobarometry, and since no spinel-liquid model was used, discussing the chemistry of spinel is out of scope of the manuscript.

Thermobarometric results should be used with caution. Its hard to match the results reported on the text with the supplementary data.

We do not understand what the Referee#2 means for "thermobarometric results should be used with caution". In our work we produced a dataset of ca. 2400 EMPA spots and we obtained, through the application of opportune tests, more than 1200 mineral-liquid equilibrium pairs for the application of thermobarometric models.

We discussed the results following a statistical approach (for every population the internal error and the MSWD were calculated). The results were then graphically presented in a constrained P-T space.

We think we have been overcautious.

We disagree with the statement that it is hard to match the results with supplementary tables. Every table is divided for phenocrysts and groundmass (as for olivine, orthopyroxene and spinel) or by clustered population (as for Pl and Cpx). For every rock it is used a conventional abbreviation AB (basalt), TA (Trachyandesite) and TR (Trachyte).

In text (chapter 7.3) P-T results are discussed for rock and for mineral, therefore the reader just needs to enter the supplementary material and search for the right mineral phases and rocks. It is important to understand that we are handling a voluminous dataset of 2400 EMPA analyses, so it requires time to fully explore it. Therefore, we appreciate a lot that R#2 went through the dataset.

But for example, plagioclase data of almost the same composition found in different WR samples is used to calculate temperature and pressure. These models almost always will yield a number, the idea is to generate a good interpretation for the results, the best possible approximation.

We analysed the mineral chemistry of a specific sample and then we compared it to the hosting magmatic rock. We used opportune test (as recommended) and then we applied the mineral-liquid thermobarometry model and we discussed statistically the results. As declared in the previous point we have been overcautious, we made our best to propose a model that is strictly based on data without any unconstrained speculation.

Even if is not evident on their tests, the authors should incorporate explanations on how mixing could affect their thermobarometry results. Protracted heating-mixing could be the driving force for convection-conduction in a geothermal reservoir.

As we already explained in the response to Referee#1's comments, there is no evidence of mixing in the studied samples.

However, we would like to stress here that Mineral-Liquid thermobarometry models, as defined in all the existing literature, are based on the compositional equilibrium existing between a mineral and its hosting liquid, no matter the origin of the liquid.

Figure captions should be more descriptive and informative. The manuscript is overall well written. Captions should be improved.

We thank Referee#2 for appreciating our work. When necessary, figure captions were modified

Detailed comments

L351-L353. Here you highlight the dissolution on pyroxene. You should do the same for plagioclase.

Growth mantle is not only due to dissolution of pre-existing grains. It can be achieved because pre-existing grains are more energetically favourable than nucleating new crystals, as explained in the existing literature (e.g. Streck, 2008; Coombs and Gardner, 2004).

L368-L374. Aegirine is an index mineral on peralkaline rocks. The same for anorthoclase. You reported non peralkaline rocks occur in the studied area. Mixing processes should be explored. The wide compositional range on "microlites" reported above could suggest the same mixing process.

L389. Fayalite is present, same comment as above. Fayalite occur in peralkaline rhyolites.

We know that aegirine and fayalite are considered a clear indication of peralkaline magmatism when they are found together also with sodic amphiboles (i.e., Afverdsonite) in felsic melts (rhyolite and trachytes).

However, we have found aegirine and fayalite compositions only in the groundmass of cpx-bearing ol-basalts, not in felsic melts.

Concerning aegirine, they are outer rims on major cpx phenocrysts and microlites in groundmass. Concerning olivine, basalts show invariably suites of olivines with maximum forsterite (Fo) contents in equilibrium with the respective whole rocks, and vertical trends consistent with closed-system melt differentiation (Roeder and Emslie, 1970; Rhodes et al., 1979; Putirka, 2008; Melluso et al., 2014).

No need to invoke at this stage a peralkaline felsic melt. The evolving fractional crystallization process in an alkali-basaltic closed system could produce both aegirine and fayalitic-olivine. By the

fact, at lines 245-247 of the original manuscript we wrote: “on the total alkali versus silica (TAS) diagram (Le Maitre et al., 2002) LHPCS lavas span from basalts to basaltic trachyandesites, trachyandesites and trachytes (Fig. 3b). Los Humeros mafic rocks fall in the “Basalt” field and, according to Bellieni et al. (1983), Le Maitre et al. (2002) and Giordano et al. (2012), can be classified as alkali-basalts”.

Concerning anorthoclase, it is found as phenocrysts and microlites in little amount in all studied rocks. Anorthoclase in alkali basalt could be generated by crystallization due to local high activity of fluids (e.g., Morten and De Francesco, 1993; Upton et al., 2009).

Since all the LHPCS belong to the same line of descent, and the parental melts are transitional- and alkali-basalt there is no need to claim assimilation for anorthoclase presence. This mineral phase is part of the same line of descent.

L446. It's correct to use the WR; however, you need first to define why this is valid if mixing could modified some magmas. The whole rock would simply be an integrated result of all magmatic processes (mixing+assimilation) that occurred just before the eruption.

We thank Referee#2 for confirming that use of WR is correct in our approach. Furthermore, thanks to the comments of both Referee#1 and Referee#2, we demonstrated in the revised manuscript, that no mixing nor assimilation processes occurred prior to eruption. We believe that this definitively validate our approach for unravel the anatomy of the plumbing system of the LHPCS magmatic activity.

L484. This sentence is not clear. A crystal could have a patchy zone at the core and then be in equilibrium with the melt (rim). In your sentence you should clarify or have a reference to one of your figures.

The sentence is a general postulate. However, we agree with reviewer Referee#2 that the sentence must be rephrased. We correct all the paragraph (lines 493-505 of the revised manuscript).

L485-L487. Not necessarily; just changes in temperature would record extreme compositional changes in crystals, without any mixing involved (mass exchange).

We want to clarify that at lines 485-487 of the original manuscript, we were not talking about mixing or mass exchange. We are just declaring that as general rule for the application of mineral-liquid thermobarometry euhedral crystals should be selected, whereas those showing morphological evidence of disequilibrium should be discarded. Hereafter the original sentence:

The predominant euhedral to subhedral habit of crystals is generally considered an evidence of equilibrium with the surrounding melt (e.g., Keiding and Sigmarsson, 2012). Accordingly, in the first step of our analysis we discarded from the analyzed dataset the minor cluster of crystals presenting morphological evidence of disequilibrium such as patchy chemical zoning (from BSE images). All the microprobe analyses related to the very rare crystals presenting resorbed rims and patchy zoning, were discarded.

As we declared in the previous comments, all the paragraph has been improved in the revised manuscript (see new section 7.1).

L488-489. First you need to clarify and yield some confidence to the reader about the origin of the melts and the magmatic processes that modified each magma. If mixing occurred, then the WR is a mixture of xenocrysts and phenocrysts. How does this affect the equilibrium between the phenocrysts and the WR? You should clarify the phenocrysts you used, is not clear and not proven the criteria to choose them. “.....pristine liquids in equilibrium with early crystallized phenocrysts and microlites”? This is confusing; What do you mean as microlites??, decompression induced crystals grown during decompression-cooling? Or these are microphenocrysts?? Do you mean all melts were tapped as they were formed?, I mean, how is possible that a liquid is in equilibrium with an early crystallized phenocryst and a microlite??

As already written earlier, the aim of this work is not to discuss the origin and the petrogenesis of Los Humeros magma. Aim of this work is reconstruct the anatomy of the plumbing system through a thermobarometric approach. We have also demonstrated that all LHPCS studied rocks belong to the same line of descent and derives from the same basaltic parental melt. Textural observations together with FC modelling discard any possibilities of mixing processes.

Regarding the comment “you should clarify the phenocrysts you used and not proven the criteria to choose them”, we want to make some comments: Chapters 7 and section 7.1 and 7.2 are dedicated to the presentation of all the tests required for the correct identifications of Mineral-Liquid pairs and how to apply them. All the existing literatures relative to the tests and to the parameters are fully presented in the manuscript.

Phenocrysts and microlites are related to textural characterization, now improved in the revised version of the manuscript. Tests for equilibrium were applied to all minerals. Only those passing the opportune test were then considered for thermobarometry.

Concerning the last point, not all microlites are in equilibrium with the nominal liquid. Those that crystallised during the rapid ascent (see Rutherford, 2008; reference added in the revised manuscript) are not in equilibrium and were not selected for thermobarometry modelling.

L502-503. You need to explain why this value was chosen.

This second Opx-test is redundant. We delete it.

L550. You should delete the negative values. Have no petrological meaning, no matter other authors have interpreted them as anhydrous. All models will yield data, the job of the petrologist is to evaluate which are usable.

Following interpretations of the existing literature (Keiding and Sigmarsson, 2012; Putirka, 2008), which suggest that the negative values represent a general anhydrous melt condition for the basalts (2010 Eyjafjallajökull eruption), we are just considering our negative values obtained from the same hygrometer in the same way. An anhydrous character corresponds to H₂O < 1wt% content (e.g. Webster et al., 1999). We agree with Referee#2, the job of the petrologist is to present all the obtained data, evaluate them and discuss them, also in the light of the existing literature.

L551-563. Does these water contents match the pressure calculated with other methods used in this investigation? 1.40 wt.% is very shallow. If these are subduction related magmas how is possible they are anhydrous? I know is dictated by the model, but what do you think, the model approach well the problem?? Moreover, if basalt are required to be anhydrous, then what is the origin of the water required for the evolved LHPCS?? Have you try other hygrometer, different to Lhur and Housh 1991?

As we have stated earlier and, in the manuscript, we are not dealing with the origin of the LHPCS magmas, instead our aim is to rebuild the anatomy plumbing system of a volcano using a thermobarometry-based approach.

Concerning anhydrous basalts, we addressed it in the previous point. Concerning the felsic rocks, we demonstrated that they are daughter melt of the same line of descent. As written in one of the previous comments, the term “anhydrous basalt” indicates a basaltic melt containing H₂O < 1 wt%. The new FC modelling indicates that trachytes are the ca. 25 wt% of residual melt from the basaltic parental melt. Trachytes, following the hygrometer of Putirka (2008) show a water content of 2 wt%.

A water content of 2 wt% in the residual fractionated liquid, corresponds to H₂O: 0.5 wt% in the basaltic parental melt.

Here it is demonstrated how to obtain water in felsic rocks, via fractional crystallization starting from a nominal anhydrous melt.

Concerning the hygrometer, we tested that of Putirka (2008) and that of Lange et al. (2009). The choice of the hygrometer of Putirka (2008) is due to the fact that this hygrometer is a parameter of the PI-Liq barometry model (Putirka, 2008) used in this manuscript.

L567-569. *You have a great amount of xenocrysts with felsic compositions. Many of the cores where temperature was calculated have the same composition as other cores measured in TA. The temperature is different because the WR composition in which are supposedly in equilibrium varies. So, which one is the system in equilibrium??*

We would like to clarify that we do not have any xenocrysts of felsic composition in our studied samples. Maybe some plagioclase antecryst cores (see fig. 10), and very rare olivine xenocrystals (Fig. 8a).

L634-637. *Are these megacrysts or resorbed crystals?, fragmented crystals??. Is there any possibility these are intrusive xenocrystals??. Give a reference for subsolidus equilibration of groundmass after eruption or explore an alternate possibility.*

We presented petrography in section 5. Alkali feldspar are phenocrystals and microcrystals with euhedral to subhedral habit. It exists a wide literature related to feldspar re-equilibration in subsolvus and subsolidus conditions. We improved the manuscript (Lines 643-649 in the revised manuscript) and added reference for feldspar subsolidus re-equilibration (Nekvasil, 1992; Kontonikas-Charos et al., 2017; Plumper and Putnis, 2009, Brown and Parsons, 1994) and for groundmass subsolidus re-equilibration (Latutrie et al., 2017).

L650-653. *This is confusing, or at least, with not enough information for the reader in order to understand the author's point of view. Are these basalts the product of differentiation of the former basalts mentioned just before? These evolution is recorded along the crystallization of the Fe-rich olivine, albites and aegirines? Report the textura of the borders, are they in equilibrium. Is this alkaline-low oxygen fugacity mineral assemblage the result of mixing with more alkaline melts? Mixing could occurred at depth or at shallow pressures??*

Cpx-bearing basalts derives to the LH18 basalt through Cpx crystallization and crystal accumulation. We demonstrated it with the new FC-Cumulus modelling (see answer to R#1 comments).

We improved the textural description of mineral phases in petrography and mineral chemistry chapters (see new section 6).

As addressed earlier in this comment, there is no evidence mixing and assimilation processes in the LHPCS studied samples.

L657-658. *Then, what happened to the proposal that basalts should be anhydrous?*

We already addressed the anhydrous character of the LH basalts earlier

L670-673. *I suggest a brief proposal about the origin of these magmas should be explained. If mafic and intermediate are tapped almost straight from deep reservoirs, then what is the origin for felsic magmas?, do they arrived already evolved to shallow levels? Moreover, the intermediate melts do not have any traces from mixing? Mixing has been reported as one of the mechanisms to produce andesite-dacite melts.*

As already written earlier in this comment, in the LHPCS studied samples there is no evidences of magma mixing. The new FC model demonstrates that all the studied rocks belong from the same line of descent and derive from a progressive fractionation of the same mineral assemblages from the same parental basalt (see new section 8.1). As already declared here (and in the manuscript), our focus is the geometry of the plumbing system in the light of geothermal system evolution. Petrogenesis is out of scope. We agree that magma-mixing is one of the mechanisms invoked to produce andesite-dacite melts. However, (i) you can produce andesite-dacite melt also just with

Rayleigh FC (see Moghadam et al., 2016; Lucci et al., 2016), and (ii) we do not have no andesite nor dacite at Los Humeros.

L674-683. *I disagree. You need to explain why some TA and AB have a mixture of restitic phases as ulvospinel (titanomagnetite)? What I mean is that some samples contain evolved titanomagnetites and Cr-rich titanomagnetites. This is a strong evidence for mixing.*

As we wrote earlier, mixing processes would lead to particular textures (fine-sieve, resorption surface, glomerocrysts, reverse zoning, reaction rims, dissolution, breakdown mantle and crystal clots). Restitic phases due to mixing processes should be characterized by disequilibrium textures. None of these textures, nor restitic crystals are observed in our sample. Even if this point has been already addressed earlier, we would like to outline again that Cr-spinel is found as big crystal inclusion in major Mg-rich olivine. Other spinels phenocrysts show very low content of Cr, whereas Cr content dramatically fall to 0 in groundmass microcrystals. This is compatible with a progressive fractional crystallization, in which Cr is controlled by spinel. Again, there is no need to invoke mixing processes.

L682-L683. *I do not understand what you try to explain. Rephrase.*

We reworded that sentence

L694-695. *These are not the only evidence for mixing.*

We rephrase the sentence.

L715. *If I remember well this is the first time alkali-basalt is mentioned. Should be pointed before, this would explain the origin of aegirine and fayalite???*

We presented alkali-basalts at Line 244-247 in chapter 4 of the original manuscript, when we presented the WR chemistry in TAS diagram. We are glad that Reviewer R#2 agrees with us, fayalite and aegirine belongs to the normal evolution of an alkali-basalt.

L717-718. *Harker diagrams are not the best option to explain fractional crystallization. In fact, it seems that the trends are very linear typical of binary mixing-assimilation. Lets think about the mixture of Fe-Ti oxides you have, mixing occurred. A very least you should show trace element evidence. Trace elements are very sensitive to mixing and assimilation, so you could adjust your model.*

We agree with reviewer that Harker diagrams are not the best to sustain FC hypothesis. Therefore, following also the comment by Referee#1, we produced major element mass balance model coupled with Rayleigh fractional crystallization model (see section 8.1 of the revised typescript). We demonstrate that pure FC is able to describe the single line of descent of Los Humeros post caldera stage products. The results obtained through FC-models are in agreement with textural observations and petrography of studied rocks. We agree with reviewer that trace and REE elements are very sensitive to FC-AFC-Mixing processes, however they would request a deep approach that should be comprehensive of the discussion of petrogenesis of magmas. This is far beyond the scope of this manuscript, focused on the possibility to unravel the geometry of a plumbing system using the relationship between mineral assemblages and hosting melts.

L721-724. *Then, how does this support crystallization acting alone to form the trachytes??*

We agree with the reviewer. Since the petrogenesis of Los Humeros would request a deeper discussion of whole rock geochemistry (major, trace and REE) and Sr-Nd isotope. We delete the sentence. And we reorganized the paragraph coherently.

L750. *So what is the origin of these anhydrous basalts? If these evolved to more felsic melts what is the origin of water in those?*

As said earlier, it is not the aim of this work to discuss the origin (petrogenesis) of Los Humeros magma. Concerning the felsic rocks and their water content, we already addressed this point earlier.

L806-813 This paragraph is very confusing. Do you mean this alkaline basalt will arrive almost at its liquidus at shallow pressure, and only then aegirine and fayalite would crystallize?? What is the origin of this alkaline basalt? He basalt has ilmenite-ulvospinel?, calculate the oxygen fugacity and discover is is enough reduced to crystallize fayalite-aegirine.

LH18 basalts (Texcal) made of Ol+Pl+Sp, shows olivines with dendritic, skeletal and spinifex textures, associated with plagioclase with swallow-tailed textures. The only way to achieve these textures is a rapid ascent from the deep reservoir to the surface (see references in the revised manuscript).

Concerning Cpx-bearing basalts (LH5-2; LH27-1), if we see them erupted, it means that they ascent to the surface at their liquidus conditions. It is hard to imagine a solidified basalt ascent to the surface. Concerning aegirine and fayalite in basalts, the thermobarometric results (discussed at line 806-813 of the original manuscript) indicate that these two phases crystallized in the final ascent of the basaltic magma prior to eruption. As we already explained, the petrogenesis of the LHPCS magmas is far beyond the scope of this work. Knowing the origin of the basalts, it will not change the P-T results or the geometry of the feeding system. Also, oxygen fugacity and discovery the thermodynamic conditions for fayalite-aegirine crystallization are far beyond the scope of this manuscript.

Figure 3. There are multiple attempts to recreate liquid line of descents for melts evolving by fractional crystallization from basalts with these compositions. Search and try to fit your data. To me look very linear and probably related to mass addition processes.

As we answered earlier, we demonstrated with mass balance modelling and Rayleigh fractional crystallization that all LHPCS studied melts belong to the same line of descent and are all produced from FC processes starting from the same parental basaltic melt. No need to invoke further assimilation or mixing processes.

Figure 5. Basalts, trachyandesites and trachytes contain anorthoclase as phenocrysts and in the groundmass. I do not remember an explanation about this in the text. Could be mixing with alkaline melts??

As we explained earlier, the parental melt is an alkali basalt. Aegirine, fayalite and anorthoclase are mineral phases compatible with the melt composition.

Federico Lucci
(on behalf of the coauthors)

