

REVIEW by Referee#1 (Dr. Chiara Maria Petrone)

We thank Dr. C.M. Petrone for the critical and constructive comments that deeply contribute to improve our manuscript. 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).

Major comments

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.

We agree with Dr. C.M. Petrone that defining the line of descent only with Harker major elements is not enough. Therefore, we use major element mass balance models (Bryan et al., 1969) to test the hypothesis of differentiation via Fractional Crystallization (FC) following the scheme proposed in White et al., (2009), Moghadam et al. (2016) and Lucci et al. (2016).

In particular, using the Rayleigh Fractional Crystallization (RFC) model (e.g., White et al., 2009) we demonstrate that a progressive fractionation of Pl+Cpx+Ol+SpI assemblage is able to produce all trachyandesites and trachytes, starting from the most mafic Cpx-bearing ol-basalt (LH5-2).

Mass balance modeling and in particular crystal accumulation ("Cumulate" model in White et al., 2009; Lucci et al., 2016) demonstrate the genetic linkage existing between all LHPCS basalts. A progressive appearance of Cpx and accumulation of Pl+Cpx+Ol+SpI is able to produce Cpx-bearing basalts (LH5-2 and LH27-1) from the LH18 Cpx-free basalt.

The obtained results are in agreement with the improved textural observations and describe LHPCS magmas as melts belonging to a single line of descent and differentiated via fractional crystallization.

The results obtained through FC-Models are presented in the new chapter "8.1 Major-elements mass balance modeling" and in Supplementary Table 6.

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 crystallization? A more in-depth discussion on the origin and evolution of these suite of rocks is necessary.

We agree with the Reviewer's comment. The understanding of the origin and the evolution of the Los Humeros post caldera stage magmatism, would requests a deeper petrological approach, based on the integration of mineral chemistry, whole rock geochemistry (major+trace+REE elements) and Sr-Nd isotope ratios. However, unravelling the petrogenetic evolution of this active caldera is far beyond the scope of this manuscript, that is focused, instead, on the reconstruction of the anatomy of its magmatic plumbing system.

Therefore, we deleted Lines 718-724 (of the original manuscript): "Intra-caldera basalts show assemblages containing Aegirine-rich clinopyroxenes (Fig. 4 c, d), widely considered as one of the most reliable indicator of magma transition to peralkaline conditions (i.e., White et al., 2009; Melluso et al., 2014). However, absence of olivine in the most evolved LHPCS trachyte (LH6) excludes, at this stage, a connection with typical basalt-pantellerite suites where tephroitic-fayalite is commonly found in high-silica rocks (i.e., White et al., 2005; Ronga et al., 2010; Macdonald et al., 2012, Melluso et al., 2014 and references therein)".

We deleted also Lines 253-256 (of the original manuscript): "The Los Humeros felsic (i.e., SiO₂ >63 wt%) lava samples belonging to the post-caldera stage (i) fall in the "Trachyte" fields, (ii) show potassic signature and (iii) are characterized by Agpaitic Index (molecular ratio

[(Na₂O+K₂O)/Al₂O₃] values < 1 (range: 0.7-0.8) thus excluding a peralkaline character of these evolved melts.”

Coherently, we modified the Chapter 4 (Lines 232-251 of the revised manuscript).

Concerning the finding of aegirine-augite cpx and fayalite olivine in LH5-2 and LH27-1 basalts, their presence is in agreement with the transitional- to alkali-basalt character identified in TAS following the criteria proposed by the existing literature (reported in manuscript at Lines 241-243 of the revised manuscript).

The new FC-models (new paragraph 8.1, Lines 654-703 of the revised manuscript), together with mineral chemistry and the improved textural observations, indicate that the magmatic diversity is the result of a magmatic evolution dominated by fractional crystallization processes.

Since the aim of the manuscript is to demonstrate the possibility to rebuilt the anatomy of the feeding system using the crystal-melt pairs (as in Stroncick et al., 2009; Aulinas et al., 2010; Dahren et al., 2012; Keiding and Sigmarsson, 2012; Coombs and Garner, 2014; Barker et al., 2015), we think that a deeper exploration of the magmatic diversity (i.e, petrology and petrogenesis of LHPCS magmas) is far beyond the scope of this work, while it clearly represents the core for a future paper.

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 more in-depth discussion on these points and the implication on magma dynamics.

We are glad that Reviewer Dr. Petrone completely agrees with our findings and interpretations, because this comment is totally in line with our evidence and conclusions.

Concerning the Los Humeros dynamics, this scenario is coherent with other postcaldera eruption behaviors, as it happened in Ischia (see Casalini et al., 2017). We integrated this important observation in the revised manuscript at Lines 969-973 (of the revised manuscript)

We agree with the Reviewer on the necessity to stress this point in the discussion. We better explored this point in the new Chapters 8.4 and 8.5.

We will add these considerations about magma dynamics in discussion, together with answer to comments 4 and 5.

3) The area is characterised by ~30 km of crust, but the role of the thick crust is not explored.

Regarding this comment, we must say that the Trans Mexican Volcanic Belt in the study area shows a crustal thickness of ca. 40-42 km (not 30 km). The few (only) reasonable crustal data come from Crust 1.0 Global Model (Dziewonski and Anderson, 1981; Davies, 2013), which is a list of thicknesses, densities and Moho depths based on seismic information (Lines 735-747 of the original manuscript).

We integrate the thermobaric estimates from our work to the crustal properties as derived from the Crust 1.0 Global Model to convert the obtained pressure estimates into depth values.

We also proposed in the original manuscript (Lines 840-844) that density contrasts between different crustal layers act as a controlling parameter for ascending or stalling magmas. Moreover, it is possible to propose that each of these crust/density boundaries would determine lateral transport and grow of magma stagnation pockets.

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?

The new major elements mass-balance modeling (see new section 8.1) demonstrates that Los Humeros post-caldera magmas are cogenetic and all belong to a single line of descent. Thermobarometry models indicate that all intermediate and felsic melts are generated in a polybaric magmatic system, vertically distributed in the whole crust beneath the caldera. The improved description of the phenocryst zoning textures and magmatic fabric (see new sections 5 to 8) demonstrate that LHPCS mafic and intermediate erupted magmas are characterized by kinetically dominated, fast-growth in a closed system during a rapid ascent up to the shallowest plexus prior to eruptions.

A full comprehension of magma dynamics in the LHPCS plumbing system would request a different research approach (magma-density, magma-viscosity and P-T relationship, degassing rate, ascent rate) that, at this stage, is out-of-the scope of the manuscript.

However, we agree that preliminary implications on magma dynamics could be recovered by textural (phenocrysts morphology, zoning textures, microlites, presence of vesicles) observations. Accordingly, we modified and readapted the new sections 8.2, 8.4 and 8.5.

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?

Concerning the LH geothermal system, the main outcome of our work is that the hypothesis of the single large and voluminous, shallow magmatic chamber, which was proposed for the caldera stage (lasted ca. 130 ka, consisting of two major caldera-forming events: Xaltipan and Zaragoza ignimbrites, 115Km³ and 15Km³ DRE, respectively, Carrasco-Núñez and Branney, 2005; Carrasco-Núñez et al., 2018), is no longer active in the Holocene. We, in contrast, are in favor of a more reliable scenario characterized by a polybaric magmatic system feeding shallow magmatic transient batches of different magmas localized beneath Los Humeros nested caldera, having a high output rate of spatially-distributed small-volumes of mafic to felsic products.

This result is presented at lines 860-875 of the original manuscript. However, we agree with reviewer's comment about the necessity to stress up the main implications for the geothermal system. We improve the Discussion and we explore the resonance of changing scenario in the new sections 8.4 and 8.5.

The manuscript will also benefit from some reorganization. In particular, the thermobarometric 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 hardtop see in Fig. 11. This part should be dedicated to present the new T-P data and discuss the discrepancy between different thermobarometry (olv-liq gives always lower T, plg-liq higher T). The discussion should start at section 7.4.

We agree with the reviewer's suggestion.

Section 7 (up to section 7.3) now are entitled as "Thermobarometric Estimates". The Discussion section (new section 8) starts now from the original section 7.4. Furthermore, section 8 is now divided in the following sub-sections: (i) 8.1 Major-elements mass balance modelling; (ii) 8.2 Magma evolution beneath Los Humeros; (iii) 8.3 The magma plumbing system; (iv) 8.4 "Standard" vs. multilayered magmatic plumbing system; and (v) 8.5 Implications for the active geothermal systems.

We concur that Fig.11 is too rich of information. We modified it and we propose a new Fig. 11 as follow: (i) Fig 11a is the original diagram with all the thermobarometric results plotted; and (ii) we

add Figs 11b, 11c, 11d, and 11e, for cpx-free basalts, cpx-bearing basalt, trachyandesites, and trachytes, respectively.

Concerning the discrepancy between thermometers, we must clarify that OI-Liq gives not always temperatures lower than PI-Liq models. At lines 572-576 of the original manuscript, OI-Liq and PI-Liq models applied to basalt produce comparable results.

Concerning trachyandesites and trachytes we improved thermo-baric data presentation and discussion (see lines 627-631 and 643-649 of the revised typescript), respectively.

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 through-out, which in literature is commonly referred to as mineral-liquid thermobarometry not as “inverse”. A clarification on this point is necessary.

We agree, the Reviewer is right. We now use the correct term “mineral-liquid thermobarometry”.

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.

We agree, we stressed too much the sentence. We modified the text accordingly.

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 subalkaline/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.

We do not completely agree with this comment.

Major elements geochemistry is a very short chapter of ca. 20 lines (Lines 232-251 in the revised manuscript). We are just describing the whole rock chemistry (of the studied rocks) that is used as nominal liquid in the mineral-liquid thermobarometry models, and now (in the revised manuscript) in the new FC-models.

Diagram 3c and 3d are simplified representative Harker diagrams. X axis is SiO₂ content as differentiation index. Diagram 3c is CaO vs SiO₂. The blu vector in diagram 3c represent the trend of Na₂O with respect to SiO₂. Diagram 3d is Mg# vs SiO₂, it represents the evolution of Mg# i.e. the evolution of the existing relationship between MgO and FeO*.

Concerning the subalkaline/alkaline division line, it was a mistake, but we correct this. It is missing due a wrong export from CoreIDraw to JPG.

Concerning the choice of symbols, there was a precise strategy that can be followed along all the manuscript: (i) we chose the three different colors to help the reader to visualise the different magmas (red= basalts, green= trachyandesites and yellow= trachytes) as depicted Fig. 3a of the submitted typescript; (ii) we chose three different symbols (circle for basalts, diamond for trachyandesite, triangle for trachytes) as a code for all the following diagrams. In mineral chemistry diagrams (Fig. 5, 6, 7), in all the P-T model diagrams (Fig. 8, 9, 10), and in the final P-T model (Fig. 11), the reader can always recognize basalts (circle), trachyandesites (diamonds) and trachytes (triangle).

Modifying Fig. 3 would imply to modify the graphic code of all the other diagrams.

We agree to the reorganization of the description of the major element geochemistry, but we therefore prefer to maintain Fig. 3 as it is.

Petrography – it is not clear why the Texcal 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.

Concerning the Texcal lava flow, “olivine-basalt” is from the existing literature, and in particular in the already published geological map (Carrasco-Nunez et al., 2017b). However, we agree that the name is imprecise, therefore, we modified along the whole text in Cpx-free Ol-basalt (Texcal) and Cpx-bearing Ol-basalt (intracaldera basalts).

With respect to Fig. 1, the names and abbreviations of volcanic unit follow the geological map of Carrasco et al. (2017b). We specified that in the original caption.

Concerning the only 2 trachytes, we collected many different samples from the two trachytic lavas (LH5-1 and Lh6-1), all samples from the first trachyte show cpx+ol-bearing assemblages, all samples from the second trachyte are cpx+ol-free. We selected the two most representative samples, one for each trachyte.

Concerning the number of samples, we collected in the field more than 60 samples (as indicated by sample names such as LH27-2...). For every sample we collected a fresh block of 3-5 kg. Petrographic characterization was produced for near all collected samples. We concentrated then our efforts on the most fresh, preserved and representative samples, one for every major magmatic products of the post caldera stage.

Concerning basalts there are only three basaltic lava flows (Texcal, Xalapasco and Los Potreros). We agree with reviewer these samples could be a few for a good petrogenetic study based on whole rock geochemistry and isotopes. However, our aim is the thermobarometry modeling, and we think that selected representative samples for mineral investigation on primary assemblages are quite enough to build up a working model.

However, reading the text in the original form, we agree with reviewer: it seems we worked on just 13 samples. We rewrote the first part of Materials and Methods (Lines 201-209 of the revised manuscript), and better presented our work, and the number of samples we worked over.

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. Infact, cpx 3 overlaps cpx1 & 2, therefore it is difficult to see if cpx in basalts and trachytes are actually zoned. If so how?

We improved the textural description of clinopyroxene, following the nomenclature proposed by Streck (2008) as suggested by the reviewer in another comment. We agree that is necessary to improve the text to better present the five cpx-clusters discriminated through texture observations and mineral chemistry. However, we consider it is not possible to reduce the clusters for the following reasons:

(i) cpx1 and cpx2 are cores of phenocrysts in basalt and trachyandesite+trachyte, respectively. As it can be observed in Fig. 6a, 6d and 6f, Cpx1 and Cpx2 show a completely different chemistry with consequence on thermobarometry models.

(ii) cpx3 is the wider group of all, it represents the chemistry of LOA and monotonous zoning outer rims of cpx1 and cpx2, and also the chemistry of homogeneous unzoned phenocrysts in all studied sample. Looking to the diagrams 6d and 6f, it is possible to recognize some differences (Ti, Ca+Mg+Fe, Na) in the Cpx3 group and therefore, to be overprecise, we should further split it in Cpx3a (basalt) and Cpx3b (trachytes and trachyandesites).

(iii) cpx4 represents microlites and micropheocrystals in groundmass of all studied samples. Their composition partly overlaps that of cpx3. However textural characteristics are completely different (phenos vs. microlites).

(iv) cpx5 represents Na-rich pyroxenes as microlites/microphenocrystals in basalts (LH5-2, LH27-1) and thin external rims of major phenocrysts from few trachyandesites (LH15, LH17, LH26-2).

We agree that splitting too much the cpx populations (as for example creating Cpx3a and Cpx3b subgroup) will be unnecessary in the light of a thermobarometric approach.

However, at this stage it is not possible to reduce the subgroup lower than five clusters.

Putting together Cpx1 and Cpx2 should imply that cores are all equal in composition and it is not true. Putting together Cpx4 and Cpx5 would imply that microlites are all equal and the Na-rich signature would be lost.

Concerning the partially overlaps between cpx1 cpx2 and cpx3, it is related to the fact that we are observing LOA oscillatory zoning and normal monotonous zoning. As we reported addressing one of the previous comments: the similarity of compositions between phenocryst and groundmass microlites suggests that there was essentially no major change in the temperature of any of these magmas during ascent (Rutherford, 2008).

However, we agree that original chapter 6.2 was not in shape for publication. We deeply improved it in the revised version.

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?

As we wrote in the previous comment: “cpx5 represents Na-rich pyroxenes as microlites/microphenocrystals in basalts (LH5-2, LH27-1) and thin external rims of major phenocrysts from few trachyandesites (LH15, LH17, LH26-2)”. Na-rich rims are found diffused in basalts and rare in trachyandesites. Na-rich microlites are found just in basalts.

In the revised chapter 6.2, the description of the five cpx-groups is improved and now clear.

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?

We agree with reviewer that Fo and Mg# usually decrease from basalts to trachytes (or more felsic rocks) due to the progressive fractionation/extraction of MgO phases. However, concerning our samples, we observed that phenocrysts describe a three-clusters distribution: (i) basalts with Mg# 79-87; (ii) trachyandesites with Mg# 67-80 and (iii) trachytes with Mg#58-63. These three groups represent the ol-compositions that is entered in the chosen Ol-Liq model of Putirka (2008). Additionally, the use of CaO (wt%) allowed us to graphically (Fig. 7a) improve the discrimination between the three populations, with ol-cores in basalt that discriminated well from all the other olivines.

Concerning the use of Cr and Ni. We didn't analyzed Ni at microprobe, because it wasn't allowed in the selected microprobe configuration. We analyzed Cr (as Cr₂O₃ wt%, see table S3), it is always below detection limit (0.04 wt%) in all the analyzed olivines in trachytes and trachyandesites.

In basalts most of the olivines show Cr below detection limit and very few spots produce values in the range 0.04-0.06 wt%. Information about Cr-content is too fragile to be used for a possible discrimination between population.

We have no other choice at this stage to work on the classical Mg# index plus CaO, as suggested by Melluso et al. (2014).

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.

We improved the 7.3.2 section. See Lines 627-631 of the revised manuscript.

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.

We agree with reviewer's suggestion. As declared earlier, we prepared 4 new diagrams for different rocks. See the new Fig. 11.

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

We thank the reviewer with this very interesting suggestion. In addition to the suggested work of Jackson et al (2018), we also consider these: Solano et al. (2014) and Cashman et al. (2017 to improve the manuscript.

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

Right, lava flows commonly doesn't contain lithic clasts. However, we just want to point out that in the LH studied lava samples no fragments from host rocks or from previous magmatic rocks were observed. Thus, we use the terms lithic-free as proposed by many works such as Geshi and Oikawa (2014).

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

We thank reviewer for this interesting observation. We consider two possible explanations for the trachyandesite An-rich plagioclase core (An 75-85%). The first scenario is related to H₂O behavior in magma. The second one implies that these An-rich plagioclases tap a more primitive stage of basalt segregation. We believe that both scenarios are compatible with Los Humeros data, and probably concurred together with the crystallization of An-rich phenocrysts in intermediate and felsic rocks.

We explored these two scenarios in discussion at lines 796-811 of the revised manuscript.

We also checked the dataset, and we verified the existence of a limited low quality analyses. We decided to delete them and correct the manuscript coherently.

L432-441 – No need of this introduction

Following the reviewer's suggestion, we delete this paragraph.

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

We checked carefully the text. All figures are presented in the correct order.

L482-484 – please revise the English.

We rephrase the sentence as follow (Lines 515-520 of the revised manuscript):

“Following the textural criteria previous defined, all microprobe analyses related to those rare crystals presenting morphological evidences of disequilibrium, such as patchy zoning (from BSE images), were discarded”.

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.

We agree with the reviewer that a more precise definition of zoning is necessary, since different zoning patterns are indicative of different crystal-melt conditions. Therefore, we made a careful revision of types of zoning and textures.

As declared at Lines 228-229 of the revised manuscript, now the nomenclature of types of zoning and textures follow Ginibre et al. (2002), Streck (2008) and Renjith (2014).

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

Yes, we agree with the reviewer comments about the problems related to the use of WR composition as nominal liquid in equilibrium with microlites/microphenocrystals.

As written in the original manuscript (lines 451-454), we are aware that such procedure put the focus on the early (at depth) steps of the crystallization history and increase the uncertainties on the final stage of crystallization.

However, since the paucity/absence of glass in the groundmass, we had no other choice than use the WR also for microphenocrysts and microlites.

We applied the same tests and rules for all other minerals. We accepted (very few) results only from those minerals in equilibrium with the WR composition (nominal liquid).

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

We concur with the Reviewer that the additional test for orthopyroxene is redundant. We deleted it from both text and Opx supplementary table.

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?

We agree that the paragraph is quite confusing. Then, we rephrased it (Lines 532-538 revised manuscript).

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

We now add the analytical error to the methodologies chapter.

(XRF) analyses for major elements compositions: the analytical uncertainties were <1% for SiO₂ and Al₂O₃, <5% for the other major elements.

(ICP-OES) analyses for major elements compositions: the precision is estimated better than 2% for values higher than 5 wt% and better than 5% in the range 0.1–5 wt%. Data are strongly comparable.

(EMPA) the major-elements analyses of mineral phases show an error of < 2 % for SiO₂ and always < 1 % for the other major elements.

Federico Lucci
(on behalf of the coauthors)

