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Solid Earth
Discussions

Interactive comment on "Dynamics and style transition of a moderate, Vulcanian-driven eruption at Tungurahua (Ecuador) on February 2014: pyroclastic deposits and hazard considerations" *by* Jorge Eduardo Romero et al.

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-A reading friendly version of this text is available in the attached pdf supplementary file.-

Dear Editor, Please find here the answers to the comments from J.L. Lepennec regarding the manuscript submitted to Solid Earth by Romero et al. ""Dynamics and style transition of a moderate, Vulcanian-driven eruption at Tungurahua (Ecuador) on February 2014: pyroclastic deposits and hazard considerations"

We are waiting for instructions from Solid Earth regarding a possible revisions, rejection

or further consideration from another reviewer. No information have yet been provided.

The initial text from the reviewer is in italic and answered for each individual remark. The major issue raised by the reviewer is on the confidence in the data. The procedure of field measurements of tephra fallout thicknesses based on direct observation is indeed less reliable than surface weighting or using a network of ashmeter. However, all concerns are answered here to reassure the reviewer about the attention given in the field work. Further, several comments regarding parts of the discussion based on a limited amount of data are justified. When possible, data, citations and explanation will be added, yet it is justified to remove some too speculative parts.

We are very grateful to J-L Lepennec for such an detailed and careful review. This time-consuming work and attention during a review process should be the norm, yet it is not often that well done.

Figure: Additional information documentation of samples most doubted in the review. Locations of the points in the map.

Comments and responses: The introductory section that describes Vulcanian eruptions is quite sketchy and does not point out important issues raised in previous literature, notably regarding the role of magma-water (phreatic) interactions in enhancing the explosivity, with implication on eruption size, and tephra grainsize and componentry. => At Tungurahua, magma-water interactions are marginal, if not absent, from the current eruptive cycle. This was the reason to not develop aspects on phreatic or phreato-magmatic explosions, in order to keep the manuscript condensed. In addition, Vulcanian mechanisms at Tungurahua have been widely described before (e.g. Ruiz et al. 2006; Matoza et al. 2009; Fee et al. 2010; Kumagai et al. 2011; Hall et al. 2015; besides others). The nature of the juvenile material from the February 2014 eruption at Tungurahua also permits to confidently suppose that no phreatic explosion was involved.

The following sections (1.1 historical activity and 1.2 the February 2014 eruption) are quite well done, although the expression and referencing should be improved in places. => Without more specific instructions, we are unable to react on this comment.

The sections 3.2 and 4.5 can be mixed in a single section for a better lecture and interpretation from the readers. => Yes

Similarly, section 3 (deposits of the 1 feb. 2014 eruption in P6) starts with a subsection 3.1 focusing on windflow regime in the Tungurahua area, while the stratigraphy and lithologic description are presented much later in subsection 3.5 in P9. This requires an in-depth restructuration. => A restructuration of section 3 is possible, yet the existing structure is thought as a zoom from large scale to specific features. The question of wind flow regimes at Tungurahua is central, since the unusual dispersal pattern is thought to be largely due to the wind directions, and belongs to the data, yet not in the deposit part and this will be arranged.

In general the manuscript is quite long and descriptive, with some repetitions, and most figures need revisions to display the results more rigorously and to improve legibility.Many useless photos could be removed to keep a selection of really relevant pictures. In order to help the reader understand what the authors mean, the writing should be substantially improved (scientific terminology and phrasing). => These unspecific comments may be addressed. Figures can indeed be condensed by removing several photographs.

There are also many inconsistencies with the use of units (e.g. grainsizes are reported in phi or μm or mm, volumes are quoted in 106 or 107 m3, or in 10-02 or 10-3 or 10-4 km3 etc) and acronyms (e.g. what is a.c.l.?), which all make the ms difficult to read in places. => Units will be homogenized. The acronym a.c.l. (above crater level) has been unintentionally deleted during editing and will be reintroduced.

The ms contains relevant citations but lacks some important references to previous works on Vulcanian-Strombolian issues, notably at Tungurahua volcano. => The bibliography from the reviewer will be considered.

The authors explain they collected tephra thickness measurements at least two weeks after the main eruption. At Tungurahua this is fairly late for such a thin tephra layer, because frequent rain falls and wind remobilization may lead to unreliable measurements. => We describe that "The tephra fallout field study was carried out in mid-February, and thus contain the signature of two weeks of activity (between 01 and 14 February)". In terms of tephra reworking, the weather situation in report 729 of IGEPN describes "favourable weather conditions for the observation of the volcano, with clear sky between February 4 and 9", even if light rains were reported around the volcano. On February 10-11, the summit of the edifice was not visible due to cloudy weather. Personal communication from IG staff reported that no rain had occurred around the volcano base before 07 February.

The ms does not fully explain (P5L18) how these measurements were obtained (single measurement at each locality? Or several? how many? => Our isopach map consists of 18 data points of thickness at a distance of <30 km, while 17 of them are at about $\leq$20 km from the vent. These points were measured 5 times in every area and the average presented. The data were produced by cutting open the deposit and measuring thicknesses with a scale. The 0.1 mm thicknesses are visually estimated lower limits.

With what instrument to obtain the 0.1 mm resolution reported in fig. 4a?). => 0.1 mm was used as a value to describe in the field an extremely thin ash blanket covering the surface, it corresponds to the distal isopach of observable fallout. This value is indeed an approximation and the comment needs rigorous consideration since it can greatly influence on the calculated total erupted volume. At this stage, we cannot deliver more precise data on these very thin deposits. The attached figures illustrate several of the observation points taken as 0.1 mm, and should reassure the reviewer about the correctness of the estimation.

Similarly, this section does not explain how the componentry analyses were conducted (number of grains per sample? in which size fraction? how was sample homogenization ensured?). Only four samples were collected for grainsize analyses, this is really a small number. A single scoria fragment was analyzed to determine the composition of the juvenile material. => From each sample, we selected the fraction >500 μm (1 θ) after sieving. Samples B1 and C1 were also analyzed in the fractions 315-500 μm (1.0-1.5 θ) and 200-315 μm (1.5-2.0 θ) and samples C2 and SJ1 were also analyzed in the fractions 250-500 μm (1.0-2.0θ) and >1000 μm (>0.0 θ) due to their coarser grain size. From each grain size fraction, we made aliquots until reaching ca. >100 particles. Particle populations given in average % abundance of each type of particle (e.g. lithics, black scoria, etc.). => That's true, just a single scoria fragment from PDCs was analyzed to determine the composition of the juvenile material.

The size (weight) of PDC samples should be provided, how large were they? => Between 600 and 1200 g were used for each sample, this can be added to the ms as an annexe if required.

What was the largest mesh used for mechanical sieving? From the results I guess that only matrix samples were collected. => For amounts in the range of 1 kg, we considered only the fraction < 2.8 cm to be representative and indeed only the matrix was considered. This will be added to the ms.

The grainsize analyses were carried out using mechanical sieving (which yields mass results) and laser diffraction (<125 mm, volume result) techniques. How were these different results merged to obtain a single grainsize distribution? => We considered that the density was homogeneous between the different grain sizes, so that the volume (surface) fractions measured by laser diffraction were translated linearly into weight fractions. The fractions <0,125 mm from the mechanical sieving was used for laser diffraction and its weight fraction distributed over the range of laser measurements <0,125 mm. Given that two methods are used, both curves are presented separately. For the calculation of sorting and median diameter, a single continuous curve is however needed.

The method and equipment used for thermal analyses should be described somewhere

here, and not in section 3.4 in P9. => This is true and will be changed.

The relevance of the thermal approach to investigate the Vulcanian-Strombolian transition or the size of the eruption is not made clear in the ms. I am not sure these thermal data are useful and they could be removed. => As stated in a further comment from the reviewer: "The "low temperature" of the PDC deposits are not "striking" [...] the PDCs certainly incorporated large amounts of cold material when flowing downslope". These low temperatures are, as pointed by the reviewer himself, an indication that the PDCs incorporated large amounts of cold material by flow bulking from the substrate. As mentioned in the manuscript (P19L7): "The low temperatures measured at PDC lobe deposits are striking in comparison with the simultaneous occurrence of carbonized wood fragments. This can be explained by the fact that there was only a low component of hot juveniles. Wood in contact with hot juvenile was carbonized, yet there was not enough of these clasts to make the whole deposit hot".

3. Deposits of the 01 Feb. 2014 eruption 3.1. Dynamic characteristics As pointed out above, this section starts in P6 with an analysis of wind flow regime in the Tungurahua region (subsection 3.1) that should go in a different section, i.e. not in the "deposits" section. Similarly the timing of PDCs emplacement (P6L5-11) should go in the description of the eruption chronology (section 1.2 in P3-4). => Chapter 3.1 can be moved as an independent chapter before the deposit section. However, we believe that the timing of PDCs emplacement does not belong to the introduction, since this part is based on data.

While the ash cloud dispersion pattern reported from VAAC data are consistent with local-regional wind flow patterns, the authors ignore previous works that addressed these issues and documented in detail similar wind/ash dispersion tendencies at Tungurahua (Parra et al., JVGR 2016; Bernard et al., ACI 2013; Le Pennec et al., JVGR 2012), and thus this section adds very little to what was already known. => The research of Parra et al. (2016) and Bernard et al. (2013) were cited, but not in this section. We can add the manuscript by Le Pennec et al. (2012) in the ms and highlight their findings. “Adding little to what is known” can be seen as a good sign if rephrased as “confirming others’ workers interpretations”. This is important in research.

3.2. Distribution and volume Because the 1 feb. 2014 tephra deposits were thin, it is very likely that the ash layer has been partly/largely eroded by rain of removed by winds before measurements and samplings were made. For this reason I disagree that the “fallout distribution is well constrained on land at <20km from the vent” (P7L14). => About 17 thickness measurements were made within <20 km from the vent. Each point was measured 5 times in the area and the average presented. A figure regarding the places mostly doubted by the reviewer is added for confidence about the quality of data presented. The sentence on “well constrained data” deserves to be reworked and detailed.

In fact there are serious problems in the isopach map (fig. 4a, P8). First, the isopach contours (fig 4a) were drawn with a small number of data points (about 10-12 in proximal areas). => The isopach map consist of 18 data points of thickness at a distance of <30 km, while 17 of them are at about $\leq$20 km from the vent.

In addition, the data show poor consistency, supporting the idea that some erosion and/or reworking have taken place before field measurements were made. => From the 18 points, two measurements may be considered as "outsiders" due to their different thickness (in this case thinner deposits in comparison to adjacent data points). One of the points (Bilbao station solar panel) was measured on February 14. We realized that this solar panel was cleaned on February 6 by Mr. Edison Palacios (weekly report 729 for Tungurahua by IGEPN). Thus, this point has been wrongly assigned and will be deleted from the map. It further indicates that the measured amount of 3 mm may be related to the period 6-14 February, and would permit to discuss the data in a revised version of the manuscript . In addition, superposition of tephra lobes from different ash sedimentation phases may be recorded in this small data set (i.e. Vulcanian paroxysm + Strombolian phase). => We describe that "The tephra fallout field study was carried out in mid-February, and thus contain the signature of two weeks of activity (between

01 and 14 February)". Figure 4 is written as "Pyroclastic deposits of the February 2014 eruption of Tungurahua volcano. (a) Isopach maps (blue lines, in mm) of tephra fall deposits associated to the 01-14 February 2014 eruptive period". Please also see the previous comment regarding the Bilbao station cleaned on 06 February.

On one hand, the IG-EPN staff on duty at OVT during the 1 Feb. 2014 eruption reports that a small fraction of the tephra cloud drifted high above the OVT (not visible in VAAC satellite images), but no ash fall was reported in OVT area. It is fairly clear therefore that the tephra deposits reported in fig. 4a near OVT (thickness in the range of 0.1 - 5 mm) do not belong to the main 1 Feb. 2014 eruptive pulse. => Ash fall around OVT probably occurred on February 9, when ash plumes drifting WNW from the volcano were reported in a context of cloudy weather. Again, we stated several time that “the tephra fallout field study was carried out in mid-February, and thus contain the signature of two weeks of activity (between 01 and 14 February)”.

On the other hand, a thickness of 1 mm in Riobamba seems highly unlikely when compared to the short duration of the paroxysmal eruption. The dense permanent ash-meter network installed by the IG-EPN (Bernard et al., JAV 2013; more than 50 data points collected for the whole February 2014 eruption), reveals that the Riobamba-Guano area received a total mass of ash in the range of 90-200 g/m2, which translates into tephra thicknesses that are at least 10 times thinner than the 1 mm reported in Riobamba in fig. 4a of the ms. => Figures of the deposits are given in this comment. The Riobamba observation was made in a cemetery, where all graves where covered with the same layer. Prior eruptions have generally not affected the Riobamba area due to the usual cloud drift to the West. The only eruption that seems to have brought fallout deposits to Riobamba was the August 2006 event (2 mm). It seems unlikely that the graves were not cleaned since 2006. A possible explanation to the discrepancy between our measurements and the ash-meter results from Bernard can be that a distal PDC influence is recorded in the fallout results, as suggested by Eychenne et al. (2013) for the 2006 eruption. Rapid local variations could thus have occurred (See alternative isopach map proposed).

A density of 760 kg/m3 (P7L21) is assigned to the tephra fall layer without any explanation or reference. This value needs to be justified, as previous works report higher values for tephra fall deposits at Tungurahua (Eychenne et al., BV 2012; Bernard et al., ACI 2013). => The value was measured in the lab by filling and weighting a recipient of known volume. As pointed by the reviewer, his student showed that the value varies spatially and recommends assuming 980 to 1000 kg/m3 for deposit bulk density (Eychenne et al. 2013). This comment can be accounted for in the ms.

Moreover, I can't believe that a thickness of 0.1 mm could be confidently measured in the field near OVT and Baños (fig. 4a), or in distal areas between Riobamba and Loja (inset in Fig. 4a). From the text, I understand that most data in distal areas were obtained from reports in social media. In my opinion this is not an appropriate way to obtain reliable thickness data, as inexpert people usually report thickness values that are much higher than real values. To me these distal thickness data are not reliable. => Photographs from Cuenca are presented in the figure. A value of 0,1 has been assigned since the deposit covers completely surfaces and the initial color of surfaces is not visible. Relatively large incertitude brackets should be given to this value (see also previous comment on the 0,1 mm value), yet the reader is welcome to make an estimate based on the photos added to this response.

3.7. Petrography and geochemistry The ms presents modal values for the mineral assemblage observed in thin sections from a cauliflower bomb collected in a PDC deposit. What was the method used to determine these modal values? => The modal estimations were obtained by both a simple visual chart and ImageJ calculations.

With only 16% phenocrysts (P17L2) I am not sure I would write this is a porphyritic bomb. => "Porphyritic" textures do not depend on the mineral percentages, but on the number of grain populations.

At P17L2 what does "trachytic andesite" mean at calc-alkaline Tungurahua volcano?

=> The faint trachytic texture is only observed in thin sections and mineral orientation is not clearly developed, i.e. "faint"

The thin section photos provided in Fig. 11 P17 are of poor quality in my PDF version. => We apologize about the pdf quality created by the Solid Earth format. The final version of the figures will present more defined images.

The presence of zircons is really unexpected in Tungurahua andesites and I question the mineral determination here. => The reference to zircon inclusions has been deleted from the manuscript. Their presence is not relevant to the petrographic description. A more accurate study of the accessory minerals will be further attained.

The ms should provide the complete compositional data set, and not preliminary and partial XRF data. As such these data are useless in the ms. The comparison (P17L10) with magma composition from previous eruptive phases requires citing appropriate references. => We performed major element X-Ray fluorescence geochemistry analyses for two samples collected in the PDCs which correspond to vesicular (M1) and dense (M2) components, bombs and blocks respectively. The results can be reported in the improved version of the ms, if necessary.

4. Discussion

The whole discussion section is confusing, unclear, hard to understand, and in various aspects uncorrelated to the few data provided in the ms.

4.1. Data reliability

The presentation of data reliability is much too optimistic. It's quite clear that ash reworking had occurred before sampling in most places where thickness measurements were collected, and none of the distal data are reliable in my evaluation. => It may be postulated that ash reworking occurred, yet this is far from "clear"... An argument should be given by the reviewer here. The choice of measurement localities was obviously made on flat horizontal surfaces without noticeable reworking prior measurement.

Further, most of the deposit observations were correlated to the oral descriptions from local inhabitants. Images of the distal data are attached and will probably convince or reassure the reader.

In addition I wonder how the thicknesses of 0.1 mm were measured (more than half of the thickness data in fig. 4a). => 0.1 mm was used as a value to describe in the field an extremely thin ash blanket covering the surface. This value shall be bracketed, and is an estimate. The attached figure illustrates several of the observation points taken as 0.1 mm from georeferenced images sent as social media reports.

The authors do not explain how the values of 7 to 30% uncertainties (P18L11) on isopach areas were calculated, and I think some isopachs may have much larger uncertainties. => As cited, these values refer to the work of Klawonn et al. (2014). It can be precised that this "does not include uncertainty in the measurement itself. The uncertainty in isopach areas is 7 % across the well-sampled deposit but increases to over 30 % for isopachs that are governed by the largest and smallest thickness measurements". Following the comment of the reviewer, a sensitivity analysis will be included with the most proximal and the most distal isopachs.

The possible elliptical shape of the isopach contours (P18L15) is not fully supported by the ash cloud dispersion pattern reported by VAAC in P6 fig. 3C. The quality of the data set provided in the ms is too poor to reasonably infer the tephra distribution pattern. => We agree that the elliptic shape of our isopach map is not well-constrained. An alternate end-member map would be to postulate the influence of co-PDC clouds in the tephra fallout signature, and limit the size of the isopach to the absolute minimum following PDC deposit pathways (see figure).

4.2. Origin of fallout deposits I don't understand the correlation of Layer 1 to the 2013 eruption. If correlated to the July 2013 event, why this horizon is termed Layer 1? In addition, if it belongs to the July 2013 event, how was it preserved from the strong erosion that takes place in the Tungurahua region? (This is particularly questionable

for the Huambalo section shown in fig. 8, where layer 1 is shown as an extremely thin horizon). This correlation to the July 2013 event made from the color argument is not convincing, actually the July 2013 eruption left a thin dark-toned tephra layer (B. Bernard, pers. comm.) that was rapidly removed. I couldn't understand the sentences in P18L22-32. The "fully open-vent system" described at P19L3 is not fully consistent with the presence of "abundant altered lithics". This section is highly confusing. => Layer 1 has been the subject of many internal discussions. This will be reconsidered given the new information provided by the reviewer. Our interpretation was based on the report from Hall et al. (2015) which interpreted for fine grained co-PDC material that "The inner older layer had slightly coarser particles (77 vol% of $\leq$0.25 mm size fraction) and its reddish color suggested that its source was the 14 July eruption. The ash of the outer gray layer (90 vol% of $\leq$0.25 mm size) corresponds to the recently deposited 1 Feb PDC-surge." => Altered lithics are here interpreted to be eroded from the conduit walls by the fragmented magma, but also remnants from the plug, indicating the transitional behaviour between Vulcanian and Strombolian styles, thus it is accompanied by free crystals and large amounts (>50 vol.%) of juvenile volcanic glass. Maybe, the expression of "fully open-vent" is not adequate in this case.

4.3. PDC material P19L7: The "low temperature" of the PDC deposits are not "striking" as the measurements were made 2-3 weeks after the eruption, and the PDCs certainly incorporated large amounts of cold material when flowing downslope, as demonstrated – not "suggested" – by Bernard et al. (2014, 2016) for the August 2006 event. => It is great to agree that the temperature measurements show that large amounts of cold material was incorporated, this is exactly why those measurements are included. The reviewer comment thus shows the usefulness of this data. It is also a good sign that the manuscript confirms (a synonym of repeat!) tendencies observed by other workers for other eruptions at Tungurahua. Finally, our interpretation relate to substrate entrainment, whereas the previous interpretation by Hall et al. (2015) stresses that the distance travelled with air entrainment is the cause of the cold temperatures.

I did not understand the next sentence, but the end of that paragraph essentially repeats findings of previous studies (Kelfoun et al., BV 2009; Bernard et al., BV 2014, GEOLOGY 2016; Hall et al., JVGR 2015). The expression should be revised. => It is again great to confirm (or repeat for another eruption) the findings from other workers. This is an encouraging sign for all the authors.

P19L16: Who interpreted the ash cover as representing two stages of the eruption? References should be provided. P19L20: the location of sample T110 is not reported on the map in fig. 4b. => T110 was taken on the outermost PDC levees from figure 7g. A marker will be added.

P19L20-21: Grainsize data are required to support these interpretations, but I am not sure to understand the distinction between PDC and co-PDC here. => PDCs refers to the main body of pyroclastic material driven by gravity (as a density current made of pyroclasts). Co-PDC clouds are those secondary clouds that are not emitted from the crater, but at local points on the pathway of the main PDC, and are thus related to the PDCs rather than the main plume. Sample T110 thus consist of a basal PDC deposit, yet three other PDC pulses are documented from their sedimentary signature made of successive levees and lobes. Those were certainly accompanied by co-PDC clouds, which must have passed over the location of T110.

P19L22: The ms offers no quantitative data regarding the componentry of PDC deposits and this paragraph is unclear and sounds highly speculative. => Yes, the word should be rephrased as “surface blocks componentry, granulometry, and aspect” (e.g. Fig 5-6).

L19L30: Grainsize signatures are not adequately documented in the ms, and from the componentry observation it’s fairly obvious that the flows incorporated a lot of accessory material on the slope of the edifice, and not only from the conduit. I failed to understand the end of that paragraph. => 18 samples in PDC deposit were presented. Does the reviewer consider this to be not enough? Here we return the previous comment,

which componentry is evoked? The sentence should be rephrased to say that the grain size signature being invariant, this must be inherited from the explosion mechanism, rather than path dependent. This observation comments on interpretation in Hall et al. (2015) that “The mobility and distance traveled by the PDCs appear to be related to the volume of fines available from the explosion and fountain collapses, as well as that generated by clast-clast abrasion during transport.“

P20L1: I don’t understand why a “lateral blast” would be expected here, I surmise that the authors mean a “surge” accompanying the dense PDCs (?) => We agree that no lateral blast was involved, this is our point! We do not expect, observe, or interpret a lateral blast. Previous work (Hall et al. 2015) used this term for the 2014 eruption, yet probably in an overestimated or inadequate use: “The 1 February eruption generated medium-size PDCs, chiefly by fountain collapses and a small lateral blast“, “The velocities of the 1 Feb PDCs varied between 11 and 18 m/s, in addition to a lateral blast of ∼33 m/s.“. After discussion, it seems that “lateral blast” as used in Hall et al. (2015) refers to PDCs being faster in some of the valleys. Further, we did not see any evidence of real “surges” accompanying, or being elutriated from dense PDCs. Some local, co-PDC clouds formation is documented in relation to topography, yet they largely lack lateral velocities that define surges. Only thin (<20 cm) co-PDCs bed with a ripple surface were encountered, indicating low lateral velocities. Hall et al. (2015) did interpret that fluidized ash surges occurred, yet no signs of fluidization was encountered and instead only saltation-related sediment structures (ripples).

P20L5-6: The description of PDCs volume/mass distribution is correct but the point here is that, if the authors disagree with the volume assessment of Hall et al (JVGR 2015), they propose no other volume estimate for the PDC deposits (which may affect their VEI determination). => PDC deposit morphologies are extremely variable, and no simple rule can be found in regard to slope of substrate. Our observation points that in many parts, almost no deposit occurred, and that no simple way permits to confidently make a volume estimate. Thus, the volume estimate by Hall et al. (2015) based on thickness values from 1 to 5 m seems overestimated. We refrain on making any PDC deposit volume estimate, and will not include these in a following version of the ms. For such valley-confined small volume PDC, we believe that only the use of precise and digital elevation models made briefly after the eruption (e.g. drone-derived) permit a trustful estimation.

P20L9. The assertion that "field work [at least one week after 1 Feb. 2014] was carried out before any rain occurred" conflicts with reports from IG-EPN staff at OVT who witnessed repeated rain falls in the area in days after the main 1 Feb. 2014 event. => The figures of PDC morphologies are self-demonstrating that these deposits are pristine. Light rains had no influence on PDC morphologies during the 3 weeks of fieldwork. Cloudy weather, or rain in proximal areas may have occurred, but the distal areas were not reworked by the time of the first field days. The IG-EPN staff told us that no rain had occurred before the beginning of investigation of PDC deposits.

I note in caption of fig. 5h P10 that the photo of PDC deposits in "Romero proximal" was taken "after first rain". => Yes, this is what is written in the figure caption. The field campaign lasted 3 weeks, and rain occurred several times during these 3 weeks. Not all PDC lobes were observed before rain, yet morphologies are pristine.

P20L12. Yes, this is a good observation. Actually the first PDCs in July 2006 took time to make their path in the forested area of the upper cone. Recent small PDCs since 2010 showed their ability to travel on occasion down to the elevation of a subtle break in slope between the lower and intermediate cone. => The response to this observation of 2014 comparing with July 2006 is very relevant and interesting. Synergies are building!

4.4. A plug driven onset evolving into an open conduit eruption The initial statements at lines P20L17-22 are incorrect. As this is not a petrologic study, I wonder if the observations made in this work were sufficient to claim that there was "no reaction or disequilibrium" in the system. No data are presented to support this important inference. => We agree that this is not a petrologic study, and we cannot release more

data on the reaction or disequilibrium in the system in the present form. Thus, this part would be removed of a future version.

P20L33: The scarcity of geochemical data in the ms makes the discussion at line P20L33 highly speculative. P20L26: If there was a plug in the conduit on 1 Feb. 2014, how can the authors explain the (clearly) magmatic explosions witnessed on January 29-30? => If one conclusion is shared between all studies about this eruption, it is the presence of a plug. But to answer the issue, a plug does not necessarily disintegrate all at once. The chronology of the eruption presented in reports of IG-EPN ((IGEPN, 2014b) describe that "there was not seismic activity and/or fumaroles on 29 January, neither thermal anomalies were not detected in the volcano"; "Since 2 am on 30 January, the monitoring network at Tungurahua detected an increasing number of earthquakes and the occurrence of small explosions.....the ash columns reached 2 km above the crater....A increase of the number and size of explosions occurred during the night of 30 January, then followed by an abrupt decrease of the activity, characterized by a very low seismic activity until January 31 at 17.01 (local time)". This is not counter-arguing against a plug, rather showing pre-paroxysm explosions resulting from a partial plug failure subsequently blocked before the major event.

The componentry data (P20L31) provided in the ms are really limited to document this "Vulcanian-Strombolian" transition. => Yes, although no more data are available at this stage.

P21L26-29: This stable compositional trend can be extended to the 2015-16 period. => Thank you for this observation.

The caption in fig. 12 P21 describes a "major Vulcanian eruption with formation of a Subplinian column", which sounds quite contradictory. => The term sub-plinian is indeed not adequate and will be removed.

4.5. Volume and style Volume estimates were considered earlier in the ms, so part of this section should be reorganized or moved to the "distribution and volume" subsection

3.2 in P7. => The manuscript is organized with separated "data" and "interpretation" parts, volume estimates relate to interpretative parts. The suggestion will be tried though.

Fig. 13 shows only 4 isopach data for the Feb. 2014 eruption, while the text says that volume calculations were based on 6 isopach. It's important to show more distal isopachs, as distal deposits have significant impact on volume estimates. The figure does not show the fit to Weibull/exponential/power law distributions, this would be useful to check the goodness of fit. => The isopachs will be included, accompanied by their respective fits to each volume calculation method to check their goodness.

The shape of the dashed line to separate VEI2-3 from VEI4 fields is really unexpected and at odds with the data. I have no idea where this curious separation was taken from. The plot is presented with the "area" axis in log scale, while the caption says "thickness vs. square root area". In addition, the plot includes data from the literature that are not properly referred to. This is the case e.g. for the 16 August 2006 eruption (Eychenne et al., BV 2012), and August 2001 eruption (erroneously assigned to 1999-2001 in inset of Fig. 13, Le Pennec et al., JVGR 2012). This figure deserves complete reshaping. => Yes, the figure shall be reshaped. The dashed line to separate VEI 2-3 / VEI 4 will be deleted; the mistaken captions of the plot corrected and a plot of thickness (cm) vs square root area (km), as is the typical case for tephra fall deposits. All references will be added.

P22L10. Because of strong uncertainties on isopach data and tephra density, these estimates are subject to large uncertainties which should be highlighted in the ms. The total tephra volume is not given here, and I am still not fully convinced by the VEI3 size of the eruption. => A detailed description of the uncertainty in this case will be added, including two possible isopach maps, and the estimations of uncertainty considering the distal deposits. For each case, a VEI will be calculated, probably releasing a range of probable values.

Here (P22L13) the eruption is described as subplinian, while it is described as vulcanian or strombolian in the rest of the text. This is highly confusing. The componentry data are too scarce to thoroughly interpret the erpution dynamics. Many previous studies have pointed out the role of phreatic water in the development of Vulcanian eruptive styles, but this is considered nowhere in the discussion. P22L14. It is written that the "deposits of the February 2014 eruption are similar to the 14 July 2006 eruption in terms of erupted tephra volume". => Yes, Subplinian was uncorrectly used to describe the eruption size here The componentry data, even few, are helpful for describing the eruptive mechanism, while the eruption dynamics are documented from other parameters such as the eruption duration, column height, tephra fall volume, volume and mass discharge rates, etc. => The point on the phreatic eruptive mechanism has been addressed before (first comment). => In P22L14 the comparison of the February 2014 with the July 2006 eruption will be deleted, because no data on PDCs from the July 2006 exists.

This statement contradicts the data presented in fig. 13. Furthermore, the 14 July 2006 eruption produced PDC deposits whose volume is essentially unknown. The comparison is thus difficult to make. P23L1. => This is true, and the July 2006 PDCs also entrained lots of material on the slope so that no volume can be retrieved confidently.

From the data presented in the manuscript I think this comparison with the 2013 event remains speculative. => Yes, this comparison, already made in Hall et al. (2015), is not fundamental for this manuscript and will be deleted.

4.6. Comparison with the 04 April 2014 eruption

The authors offer no data of the April 2014 event to compare with the 1 Feb. 2014 eruption, and MDR estimates are thus highly speculative. This section should be removed. => This is true. Even with apparently similar onsets for both 01 Feb. 2014 and 04 April 2014, we don't have data on the last one to do more evolved comparisons. The section can be removed.

5. Conclusions

From examination of fig. 13 it seems that the size of the Feb. 2014 eruption was somewhat similar to that of August 2001, though durations were different. => We will include a text pointing to the possible similarities between the Feb. 2014 and August 2001 eruptions.

There are no data in the ms that allow discussing the “interaction between juvenile andesitic magma and host mush” (P23L32). What is the evidence for the presence of a “host mush” beneath Tungurahua”? => Host much has been wrongly written and should be hot mush. This will be corrected.

This is a good observation, the March 2016 event sent quite a lot of ash to the S-SE of the volcano as well. The large April 5, 1918 eruption also sent a major tephra cloud to the E-SE. => We agree on this! We can add a paragraph with a mention to the 1918 eruption tephra dispersal, and also make reference to the March 2016 event.

P24L13: Again I don’t understand the sentence “the 01 February 2014 eruption signed the evolution of the eruptive style of Tungurahua from a Subplinian to a Vulcanian-dominated eruptive behavior”, while the ms argues for a Vulcanian-Strombolian transition for this event. =>The sentence is probably unclear and will be rephrased. It is a transition on the scale of the 2006-present period: “The 01 February 2014 eruption signed the style transition from an open-vent dominated activity, which was crowned in 2006 with a major Subplinian eruption, to a Vulcanian-dominated eruptive behavior until 2014".

Best regards, Jorge Romero & Guilhem A. Douillet on behalf of co-authors

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/se-2016-159/se-2016-159-AC1-supplement.pdf

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-159, 2016.

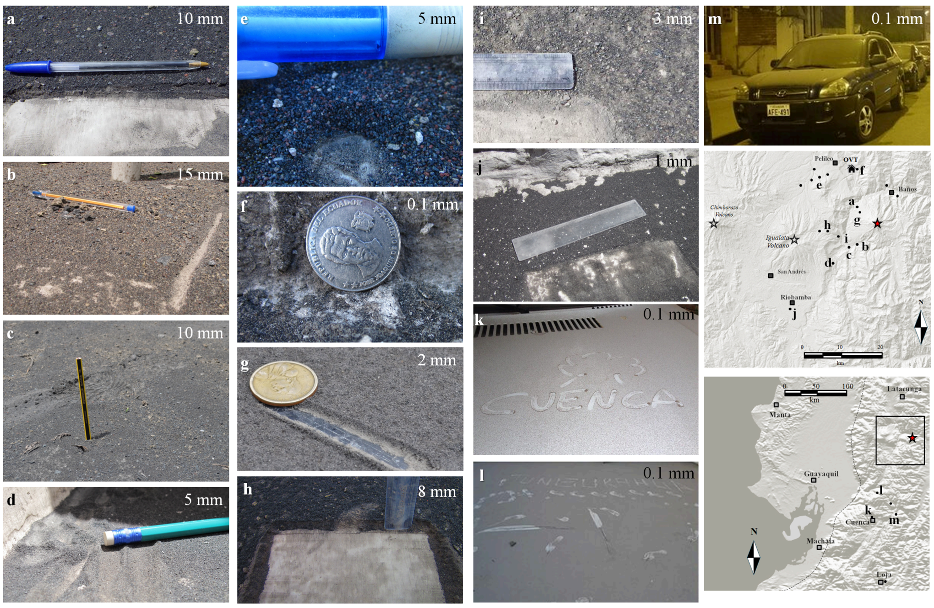

Fig. 1.