Solid Earth Discuss.,
doi:10.5194/se-2016-159-RC2, 2017


Solid Earth
Discussions
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# *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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Received and published: 13 January 2017

Review of "Dynamics and style transition of a moderate, Vulcanian-driven eruption at Tungurahua (Ecuador) on February 2014: pyroclastic deposits and hazard considerations" by J.E. Romero et al. – submitted to Solid Earth

The manuscript (hereafter ms) by J.E. Romero et al. submitted to Solid Earth focuses on the February 2014 eruption of Tungurahua volcano, Ecuador. It aims at determining the size and eruptive style of the explosive event, and makes some comparisons with other eruptive phases. The study is based on field observations, tephra thickness



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Discussion paper

measurements, grainsize analyses of tephra fall and pyroclastic density current (PDC) deposits, componentry analyses, and petrographic observations. It also includes atmospheric data to document the dispersion pattern of ash clouds produced by the 1 Feb. 2014 eruption, and reports temperature data from cooling PDC deposits. The ms highlights the transition from the initial Vulcanian onset to later Strombolian activity in the course of the Feb. 2014 phase, and elaborates on various aspects of the eruption phenomenology (ash dispersal pattern, PDC emplacement) and dynamics (conduit processes, magnitude, mass discharge rate). The eruptive dynamics and products of the Feb. 2014 Tungurahua eruption have been partly studied in previous works (the PDC deposits were considered by Hall et al., JVGR 2015), but the tephra fall layer described in the ms was not fully documented before. In this aspect, the ms provides some new insights on this particular eruptive phase, which contrasts with pre-2008 events of the 1999-ongoing activity. On the other hand, the submitted version shows serious weaknesses that render most results and interpretations unconvincing. Instead of making a synthetic comment, I follow below the structure of the submitted ms, after summarizing some general points in the first paragraph. The comments are given with page and line numbers using the abbreviation PXLY (for example page 6 line 10 is noted as P6L10). Below OVT is the Spanish acronym of Tungurahua Volcano Observatory, while IG-EPN is the acronym of Instituto Geofísico de la Escuela Politécnica Nacional (Quito).

1. Objective and structure of the manuscript

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. 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. In contrast, the main sections including the methodology, deposit descriptions, volume/mass estimations, petrology, and discussion, are confusing and inaccurate, with a tendency to mix locally methodological aspects, results, and interpretations. For example, section 3.2 at page 7 (P7) addresses tephra volume calculations that are later reexamined in section 4.5 at P22. These issues should be considered consistently in a single section. 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. 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). There are also many inconsistencies with the use of units (e.g. grainsizes are reported in phi or $\mu$m 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. The ms contains relevant citations but lacks some important references to previous works on Vulcanian-Strombolian issues, notably at Tungurahua volcano.

2. Sampling and analytical methods

The study of the tephra fall and PDC deposits has been carried out using a collection of methods that comprises thickness measurements in the field, grainsize analyses, temperature determinations, and petrographic observations. This section lacks rigorousness and precision. 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. The ms does not fully explain (P5L18) how these measurements were obtained (single measurement at each locality? or several? how many? With what instrument to obtain the 0.1 mm resolution reported in fig. 4a?). 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. The size (weight) of PDC samples should be provided, how large were they? What was the largest mesh used for mechanical sieving? From the results I guess that only matrix samples were collected. The grainsize analyses were carried out using mechanical sieving (which yields mass results) and laser diffraction (<125 mm, volume results) techniques. How were these different results merged to obtain a single grainsize distribution? The method and equipment used for thermal analyses should be described somewhere here, and not in section 3.4 in P9. In fact, 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.

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). 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.

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 <20 km from the vent" (P7L14). 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). In addition, the data show poor consistency, supporting the idea that some erosion and/or reworking have taken place before field measurements were made. 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). 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. 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. 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). 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. At the end, in my evaluation the isopach map in Fig. 4a is too poorly constrained to document the tephra fall distribution and thickness with a reasonable degree of resolution. This has important implications on tephra volume estimates, and thus on volume/mass discharge rate determinations. The authors used 6 isopach (P5L24) to calculate the tephra volume but show only 5 isopach contours in fig. 4a, whilst fig. 13 in P22 shows only 4 isopach data. This needs to be clarified.

3.3. Morphology of PDCs This paragraph contains some pertinent observations (yet most were known from Hall et al., JVGR 2015), but remains highly descriptive and deserves some rewriting. The slope in the Juive-Viejo Minero area is < 25 degrees, in contrary to what is written at P9L3.

3.4. Temperature and disturbance from PDCs The methodology section should explain how temperatures were measured: what instrument was used? What was the accuracy of the temperature determination? The goal of these temperature measurements is not made clear here. Because this part is very descriptive and of little relevance to the objective of the study, I would recommend removing this section as it brings little information to resolve the problem of the ms.

3.5. Stratigraphy and lithologic components These descriptions should come much earlier in the "deposits" section. The associated figure 8 is not suited, and is almost illegible in my PDF version. A figure with clearly drawn stratigraphic sections (instead of poor-quality photos) and correlations should be provided along with appropriate lithological characterizations and explanations. I don't understand why the thickness data indicated in fig. 8 for Penipe, Huambalo, Cuenca (1 mm?) are not reported in the isopach map of fig. 4a. The componentry data (P9L32) are quoted at the first figure, but the methodology section says nothing on how these data were obtained. The authors mention that "pumice is also abundant (15.5-15.9 vol.%)". However, this component (which is an high-vesicularity scoria, and not a typical pumice) represents a small mass fraction in the assemblage, because of the high porosity of these clasts. This should be said somewhere. How was the distinction between volcanic glass and pumice made? P11L10: The ms should provide data of the componentry at Penipe and Pillate, instead of qualitative descriptions. The characterization of the PDC deposits is as well very descriptive and brings little new information in comparison to the work of Hall et al. (JVGR 2015).

3.6. Grain size P13L16: The layer in which the four samples were collected should be precisely specified somewhere (could be shown in a revised fig. 8). B1 and C1 are not unimodal (P13L16) as they show some bimodality. Fig. 9 should show histogram bars instead of curves, to allow distinguishing real data from extrapolated curves. This figure is really hard to interpret. First, the abscissa (size) should be converted to Phi units, for consistency with the main text. Secondly, I don't understand how the laser diffraction results were converted to weight data to obtain these smoothed curves. Thirdly, I don't understand how/why all grainsize distributions end at 0 wt.% in the same point between 10 and 100 $\mu$m. This is very unlikely and has never been documented before in similar deposits. I wonder about the amount of material that has been analyzed there, as for example B1 was collected in an area where the deposit thickness is near zero (0.1 mm according to fig. 4a). Fourthly, I don't understand why samples C1 and C2 have so different grainsize distributions, although they were collected at very close localities (fig. 4a). The multimodal shape of the curves rather supports mixing of different layers in the samples (explaining the moderate to poor sorting), or wind shift effects (unlikely for C1 and C2), or may be some contamination by co-PDCs ash in the small grainsize range of the SW samples. In P15 Table 4 should provide the dry mass of each bulk sample before grainsize analyses. AP15L11: The modes identified in PDCs' grainsize distributions are biased by the fact that only the matrix has been analyzed and not the whole granular material of the PDC deposits. In fact fig. 10 (abscissa should be equally homogenized in phi units) shows that the analyses were not performed for sizes larger than 2.8 mm, while the authors indicate bomb/block as large as 40 cm in the deposits. This is why the selected material appears so well sorted (better than the tephra fall deposits!) and why the data plot in the "fall" field of Walker, a situation that is certainly not expected for bulk PDC deposits of the 1 Feb. 2014 eruption.

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? With only 16% phenocrysts (P17L2) I am not sure I would write this is a porphyritic bomb. At P17L2 what does “trachytic andesite” mean at calc-alkaline Tungurahua volcano? The thin section photos provided in Fig. 11 P17 are of poor quality in my PDF version. The presence of zircons is really unexpected in Tungurahua andesites and I question the mineral determination here. 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.

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. In addition I wonder how the thicknesses of 0.1 mm were measured (more than half of the thickness data in fig. 4a). 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. 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.

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.

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. 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. 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. 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. P19L22: The ms offers no quantitative data regarding the componentry of PDC deposits and this paragraph is unclear and sounds highly speculative. 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. 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 (?) 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). 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. I note in caption of fig. 5h P10 that the photo of PDC deposits in "Romero proximal" was taken "after first rain". 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.

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. 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? The componentry data (P20L31) provided in the ms are really limited to document this “Vulcanian-Strombolian” transition. P21L26-29: This stable compositional trend can be extended to the 2015-16 period. The caption in fig. 12 P21 describes a “major Vulcanian eruption with formation of a Subplinian column”, which sounds quite contradictory.

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. 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 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. 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. 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”. 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. From the data presented in the manuscript I think this comparison with the 2013 event remains speculative.

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.

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. 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”? P24L5: 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. 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.

Summary The manuscript by J.E. Romero et al. aims at documenting the Feb. 2014 eruption of Tungurahua volcano, Ecuador, through evaluating the size and eruptive style of the explosive phase. However, the amount and quality of data presented in this version is low, leading to unpersuasive results and conclusions (notably regarding the size of the event, the pre-eruptive context of the eruption, and the characterization of the Vulcanian-Strombolian styles). The data documenting tephra distribution and thickness are definitely unconvincing to infer the volume and mass of ash-lapilli fall deposits with a reasonable degree of accuracy. More grainsize and componentry analyses should be carried out, while those provided in the ms should be revised. Similarly, a re-evaluation of the volume and mass of the PDC deposits should be proposed if the authors disagree with previous estimates of the literature. From the information and data provided in this ms I wonder if submitting a revised version is feasible. Beyond the serious scientific concerns raised above, the ms also requires ample writing alterations: the structure, terminology, vocabulary and phrasing need substantial revisions to make the text clearer and easier to read. Finally, in my evaluation the ms in its present form does not reach the international standards expected in a journal with an IF around 2, as is the case of Solid Earth.

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