Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-942-AC1, 2021

Atmospheric Chemistry and Physics
Discussions

ACPD

Interactive comment

Interactive comment on “SO_2 and BrO emissions of Masaya volcano from 2014–2020” *by* Florian Dinger et al.

Florian Dinger et al.

fdinger@iup.uni-heidelberg.de

Received and published: 21 March 2021

We thank Tom Pering for his comprehensive review of our manuscript “SO_2 and BrO emissions of Masaya volcano from 2014–2020". In particular, we welcome his critical checks on our statistical methods and results. In the following, we reply on his specific comments paragraph-wise. If not stated differently, the line numbers and figure numbers refer to the originally submitted manuscript.

Printer-friendly version

Discussion paper

1 General Comments

Tom Pering, *Manuscript Summary*:
This manuscript details the changes in SO2 and BrO emissions at Masaya using an unusually long degassing dataset, which in combination with re-analysis meteorology the authors use to investigate the trends and associations of data, whilst being rigorous in their retrieval methodologies.

Tom Pering, *General Comments* (general part 1):
This is a well written manuscript with a generally high level of presentation throughout. It is logically structured and is easy to read. My one major comment is on the treatment of statistics in this piece, which reads as a little bit muddled (and confusing in places), with several elements lacking. These elements need to be improved before the manuscript can be published, particularly as some of the conclusions and discussion rely on some of the statements made about trends through time or difference between values, yet differences between times (BrO/SO2 ratios for example at the different phases of the lava lake arrival and the activity) are only stated in a qualitative manner. More statistical rigour is needed. I outline this below in other general comments and in the specific comments.
We welcome the reviewer's focus on statistical rigour. Several of his recommendations helped to make our statements more comprehensible. Nevertheless, there might be misunderstanding on the side of the reviewer regarding our notation or methods. In particular, we consider our statistical analysis sufficiently comprehensive as we will argue in detail below.

Tom Pering, *General Comments* (general part 2):
The abstract is very long. I found it difficult to follow exactly what the key purpose but importantly the major conclusions and discoveries were. Needs

shortening. Further comments below.

Change: We shortened the abstract by about 25 %. In particular, we removed most of the qualitative volcanological interpretations because these have not been the major conclusions of this study.

Tom Pering, *General Comments* (general part 3):

A very thorough discussion of methodology and sources of error throughout. Substantial and rigorous. Excellent.

We are glad that this is appreciated.

Tom Pering, *General Comments* (general part 4):

A minor thing, I found it difficult that some Figures and Tables were presented before aspects of the main text, which explain some of the formation of the Figures, some rejigging to make sure that this doesn't happen would be great.

Our original manuscript was prepared and optimised for the final two-column format of ACP (including the correct positions of the figures). We will take care of the correct placing of figures and tables relative to the text in the final type-setting process.

Tom Pering, *General Comments* (Treatment of statistics, Part 1):

Correlation vs. Regression. It is difficult to see what form of analysis has been performed, as frequent reference to 'correlation coefficients' are made, and yet the resultant number is provided as a percentage (with negatives occasionally). Correlation coefficients are presented in the -1 to 1 format (as you do in one point in the manuscript). The use of the percentage here throughout is confusing as we could commonly use the regression coefficient in this manner, i.e., an R^2 of 47% (47% of the variation in one variable can be counted for by another). The correlation would be reported as 0.47. This is where the confusion arises. Have you conducted regression and are providing an R2 and calling it a

correlation coefficient? Or have you conducted correlation and are formatting it incorrectly. If you were using Pearson's correlation, then the actual correlation value for an R^2 of 47% would be 0.69. This is an important distinction, and it is important that the reader has confidence in the actual statistical technique used — correlation or regression.

Our statistical analysis was based almost exclusively on Pearson correlation coefficients (the R^2-values in Figures 5 and 12 — Figures 11 and B1 in the revised manuscript — being the only exceptions). We consistently called these coefficients “correlation coefficients" and never used the term “regression". Nevertheless, we agree with the reviewer that the notation would be less ambiguous when the correlation coefficients are stated as pure decimal numbers (nonetheless because we use the “%" exhaustively elsewhere in the manuscript for “amounts/fractions of data").

Change: We formatted all correlation coefficients presented in the manuscript in the -1 to +1 format, where we explicitly added also the “+"-sign in order to minimise further ambiguity.

Tom Pering, *General Comments* (Treatment of statistics, second paragraph): Further, regarding regression. Any trend identified can then come with a p-value, is there a significant trend through time? So, where you identify a trend in the manuscript we also need to see the associated p-value to see whether this is the case.

All reported fits came with total p-values $< 2.22 \cdot 10^{-16}$ (i.e. the machine epsilon of the used computer). The p-values for most regressors were as well $< 2.22 \cdot 10^{-16}$.

On the specific question on a trend through time: We separated our time series in three time intervals (motivated also by the general volcanological observations) and retrieved and reported different trends (including standard errors for the linear trend regressor) for those adjacent time intervals. The purpose was to identify differences between those three time intervals. For the sake of clearness, we did not investigated further trend constituents (beyond a linear and a sinusoidal term) within the specific

Printer-friendly version

Discussion paper

time intervals.
Change: We added the p-values to all statement of fits.

Tom Pering, *General Comments* (Treatment of statistics, Part 2):
In parts of the manuscript where you are comparing significant differences between variables (between different phases for example), you need to statistically test this, i.e., avoiding the qualitative terminology currently used. After determining normality of the data, we can then use a variety of techniques for two-variables (t-test variations / Mann Whitney etc.) and others for three+ variables (Anova / Kruskal-Wallis) dependent on circumstances. This would allow rigorous interpretation of differences and back up the points you make in the manuscript.
A general remark to our statistical analysis and reporting of the statistical result:
We applied linear regression analyses (based on ordinary least squares) to estimate models constituting, e.g., of a linear trend parameter and a sinusoidal part. (The other statistical method applied was the Lomb-Scargle periodicity analysis.) For those linear regressions, we have a large set of statistical test results available, e.g., the standard error, the t-value, the p-value for each regressor, the F-statistics etc. We decided to report the estimated value x and its standard error e, that is $(x \pm e)$, which we consider best practise. In particular, we consider the reported format $(x \pm e)$ as a comprehensive notation of a t-test (where the reader can easily derive the explicit t-value).

Tom Pering, *General Comments* (last paragraph):
Overall, the only major comment being the treatment of statistics I consider that this manuscript would be acceptable after minor revisions. The authors will need to be careful that the results of the additional statistical analysis match with the framing of the discussion.
The analysis has already been carried out with rigour on the statistical significance.

Adjusting the notation of the statistics will thus not change the framing of the discussion.

2 Specific Issues

Tom Pering, *Specific issues, Line 19*: What is an 'extremely significant' annual cyclicity? Do you mean statistically significant? There are no degrees of 'extremeness' beyond this.
We aimed to highlight that the confidence in this observation is basically 1 (false alarm probability of $9 \cdot 10^{-74}$, see Line 484).
Change: We changed from *extremely significant annual cyclicity* to *annual cyclicity*, i.e. omitting on purpose also the redundant word "significant".

Tom Pering, *Specific issues, Line 21*: Correlation is not measured as a percentage; it is a standardized set of values between -1 and 1. So what is the -47% signifying? Is this regression?
It is the Pearson correlation coefficient.
Change: See our reply on (Treatment of statistics, Part 1) above. (Specifically, we replaced $-47\,\%$ by -0.47.)

Tom Pering, *Specific issues, Line 57*: I would say in situ methods are not able to retrieve bulk gas emissions, suggest removing 'may, however'.
Change: We changed the text in the revised manuscript as recommended.

Tom Pering, *Specific issues, Line 65*: Why are chlorine and fluorine compounds 'obvious candidates'? Needs more detail here.
Change: We now state in the revised manuscript: *Other obvious candidates are chlorine and fluorine compounds due to their relatively high abundance*, which we think are obvious reasons.

Tom Pering, *Specific issues, Line 77-79*: Needs evidence, why is it the best accessible proxy for volcanic processes? References? Examples? The

next paragraph (lines 80-95) then goes on to say that interpretation is difficult, so those two sections don't tie in together. Based on the subsequent paragraph a combined DOAS + MultiGas approach would seem far simpler. If by accessible in line 79, do you just mean that you can just use one instrument? If so, tailor that sentence in that manner.

We never assumed that the BrO/SO_2 molar ratio is per se the "best accessible" proxy for volcanic processes (in fact, we made the potential problems with BrO/SO_2 analysis transparent in the next paragraphs) but we stated that the database for this gas proxy is most likely already the second largest just behind SO_2 emission flux data. Furthermore, we emphasise that this is also a direct consequence of the fact that the BrO/SO_2 molar ratios can be derived rather cost-efficient via remote-sensing (a lot of data for minimum invested resources). Accordingly, we have to disagree that a DOAS + MultiGas approach is simpler as this adds another instrument (the MultiGas) which brings in addition the potential problems of in-situ methods discussed further above in the manuscript.

Change: We now state in the revised manuscript: *In consequence, although BrO is not on the list of the most desired plume constituent species, time series of the BrO/SO_2 molar ratios in volcanic gas plumes are the easiest accessible remote-sensing gas proxy for volcanic processes so far (besides the SO_2 emission fluxes).*

Tom Pering, *Specific issues, Line 170-175*: Where was this data acquired from? Needs a link or detail.

The data sources was given in the original manuscript in Line 128/129 (same paragraph as for the ECMWF data). In the revised manuscript, the statement is now given in the Appendix A in Line 903/04.

Tom Pering, *Specific issues, Line 216*: What does 'hardly affected' mean? Is that the 10% figure at the end of the sentence? If so rephrase to use this value. 'Hardly' isn't quantifiable.

"Hardly" referred to an underestimation of 3% which we consider — while systematic and thus not "insignificant" — minor to the scatter of this comparison (see Figure 5b — Figure B1b in the revised manuscript).
Change: We specified in the revised version of the manuscript: *to be hardly affected by saturation effects up to SO_2 SCDs of $1 \cdot 10^{18} \frac{molec}{cm^2}$ (3% underestimation, see Figure B1b) and still of acceptable accuracy at SO_2 SCDs of $3 \cdot 10^{18} \frac{molec}{cm^2}$ (9% underestimation).*

Tom Pering, *Specific issues, Line 226*: Retrieval of the background SO2 slant column. Content fine, but it might be helpful to the reader to summarise the 'four approaches' into a Table.
Change: We added Table 4 in the revised manuscript.

Tom Pering, *Specific issues, Line 302-304*: Needs more context, why the 'actually'?
In the sentence above we introduced that we used 'a' Gaussian distribution. But 'actually' we used two.
Change: We now state in the revised manuscript: *In order to provide an automated test of the "Gaussian shape assumption", we fitted two Gaussian distributions to the SO_2 VCD distribution, one with a fixed $b = 0$ and one with a free b*

Tom Pering, *Specific issues, Line 351*: Coefficient — this isn't a correlation coefficient, do you mean 0.89? Latter fit suggests regression? Confusing statistical phrasing. See general comment.
We confirm that this is a Pearson correlation coefficient.
Change: See our reply on (Treatment of statistics, Part 1).

Tom Pering, *Specific issues, Figure 8 (d)*: you highlight a relationship between wind speed and plume height. What is the regression coefficient (the

percentage model fit)? What is the p-value? Is it a significant fit? It is also unclear whether the fir is on the grey dots or the black dots.
The fit was based on the black dots. The Pearson correlation coefficient is -0.25 (or $R^2 = 0.06$ and p-value of $2.3 \cdot 10^{-10}$). The fit is thus significant but superposed by a much stronger scatter.
Change: We rephrased the text, see our reply on the next but one reviewer's comment.

Tom Pering, *Specific issues, Figure 8 (e and f)*: this is unclear, did I miss in the text why you have split this up into 0-5, 5-10, and 10+ ms blocks [I note that I see this stipulated in text following the Figure but question remains for 5-10]? There appears (not tested) to be abroad relationship between flux and wind speed? So why separated? Needs justification. Same comment regarding statistical terminology.
We tested the correlation of the calculated SO_2 emission fluxes and the wind speed for three wind speed regimes. The wind speed intervals were chosen arbitrarily yet comprehensively as regimes of low, intermediate, and high wind speeds (0–5, 5–10, 10+ m/s). We don't understand the reviewer's comment on the "broad relationship between flux and wind speed" — this finding was discussed exhaustively in the manuscript.

Tom Pering, *Specific issues, Line 375*: OK, what does weak mean? What is the regression coefficient (R2 value)? What is the associated p-value? Is this a statistically significant relationship? The scatter plot looks like a smudge of points.
The regression coefficient is $R^2 = 0.06$, thus the 'weak' anti-correlation coefficient is -0.25, and the p-value was $2.3 \cdot 10^{-10}$.
Change: We rephrased and extended the paragraph in the revised manuscript:
The comparison of the triangulated plume height with the wind speed (calibrated as explained above) confirmed such a causal link between the plume height and the wind

speed (correlation coefficient of -0.28 when considering all wind speeds and of -0.25 when considering only wind speeds larger than 5 m/s). We retrieved for the linear relationship of $H_s + A_s = a_0 - a_1 \cdot v_{calibrated}$ a best fit (when H_s and A_s measured in m and $v_{calibrated}$ measured in m/s) for $a_0 = (902 \pm 12)\,m$ and $a_1 = (12.2 \pm 1.5)\,s$ (when all wind speeds were considered, F-statistics of 64.7, p-value = $3.2 \cdot 10^{-15}$) or $a_0 = (909 \pm 18)\,m$ and $a_1 = (13.1 \pm 2.0)\,s$ (when only wind speeds larger than 5 m/s were considered, F-statistics of 41.6, p-value = $2.3 \cdot 10^{-10}$).
As a remark, we retrieved similarly well matching fits also for a quadratic relationship of $H_s + A_s = a_0 - a_1 \cdot (v_{calibrated})^2$ with a best fit for $a_0 = (860 \pm 8)\,m$ and $a_1 = (7.7 \pm 1.0) \cdot 10^{-4}\,s^2/m$ (when all wind speeds were considered, F-statistics of 64.8, p-value = $3.1 \cdot 10^{-15}$) and $a_0 = (850 \pm 10)\,m$ and $a_1 = (6.8 \pm 1.1) \cdot 10^{-4}\,s^2/m$ (when only wind speeds larger than 5 m/s were considered, F-statistics of 38.9, p-value = $8.3 \cdot 10^{-10}$).
We chose to use the linear relationship retrieved for winds speeds larger than 5 m/s for dynamic estimates of the plume height as a function of the wind speed, i.e. we applied a H_s retrieved via $H_s + A_s = 909\,m - 13.1\,s \cdot v_{calibrated}$ as the estimate for the plume height in the calculation of the SO_2 emission fluxes.

Tom Pering, *Specific issues, Line 380*: Its only a best guess if you present the model with some statistical rigour, which it is not currently.
This "best guess" was the significant result of the linear regression described above. The F-statistics of those tests are now given in the text.
Change: See our reply on the previous reviewer comment.

Tom Pering, *Specific issues, Line 384-400*: Explanation here makes broad sense, but I wonder why you did not use a low flux threshold instead of wind speed? For example, omitting below 0.1 x 1000 t/d?
We argued that low wind speeds can cause a significantly wrong estimation of the SO_2 flux — with the amount of deviation being possibly independent of the SO_2 degassing

strength. Accordingly, the interpretation of low wind speeds should be avoided and not the interpretation of low SO_2 emission fluxes.

Tom Pering, *Specific issues, Table 4 — Part 1*: How is daily variation measured? Is this a standard deviation? Or range? Or iqr? And in each case how is the error determined? Is this 1 standard deviation? Particularly important to clarify.
The daily variations are based on the standard deviations of the individual days. All given errors are standard deviations.
Change: We state now in the caption of Table 5: *Main statistical properties of the spectroscopic results for Caracol station. Early BrO/SO_2 NOVAC observations between 2007–2009 are listed for completeness. The daily variations are based on the standard deviations of the single days. The given errors are standard deviations, except for the annual trend and the amplitude of the annual cycle for the BrO/SO_2 molar ratios were the errors refer to the standard regression error.*

Tom Pering, *Specific issues, Table 4 — Part 2*: annual trend and amplitude of cyclicity. How did you measured the trend? Can you have a significant trend of -0.1 with an error the size of the trend itself? P-values? Significance? How did you calculate the amplitude of the cyclicity?
The trend and the amplitude were determined simultaneously by a linear regression (see Figure 9c — Figure 8c in the revised manuscript). We consider a trend of (-0.1 ± 0.2) as not significant (based, e.g., on the results a t-test). We see this statistical finding as equally important as the significant trends for the other periods.
Change: We added the standard errors of the amplitude (as derived via the linear regression), and removed the $\pm$ sign. Remark: we had added the $\pm$ in order to clarify that this is a proper amplitude and not a peak-to-peak value but this should be clear already by the term “amplitude”.

Tom Pering, *Specific issues, Line 461-462*: Correlation coefficients listed here. Confirm that this is indeed what you have.
Confirmed.

Tom Pering, *Specific issues, Line 470-473*: OK, significant variability and different averages. You need to test this statistically, see general comments details.
See our reply on (Treatment of statistics, Part 2).

Tom Pering, *Specific issues, Figure 9c*: Orange label is for linear trend. But none is indicated? One would expect obvious linear trend in d (your residual plot) therefore. But it isn't obvious. Is this correct? Also how was your annual cycle determined?
The orange lines give the results of the model $y(t) + y_0 + \alpha \cdot t + \beta \cdot \sin(2 \cdot \pi \cdot t/(364.24days) + \gamma)$. The regressors for the linear trends are given in the text. We did not plot the linear trend by a separate line, in order to keep the plot more tidy.

Tom Pering, *Specific issues, Line 479-481*: See general comment on significant difference.
See our reply on (Treatment of statistics, Part2).

Tom Pering, *Specific issues, Line 482*: Remove the word "extremely".
Change: We changed the text in the revised manuscript as recommended.

Tom Pering, *Specific issues, Line 486*: What does 'basically the same' mean? The values afterwards look different to me.
This sentence deals with the phase parameter of the annual cyclicity. The phase did not changed between the 3 time intervals, that is the sinusoidal always peaks in mid

February (see Figure 9c– Figure 8c in the revised manuscript).
Change: We specified this by replacing the term by *timing of the annual cycle* by *phase of the annual cycle*.

Tom Pering, *Specific issues, Line 490*: Significance of the trend?
The significance can be derived directly from the reported standard errors.
Change: We added the standard errors for the estimated amplitude.

Tom Pering, *Specific issues, Line 493*: How did you determine outliers?
For this qualitative statement: by eye. This statement was just meant to give an overview of the data and its variations. Please note, that we did not treat these “outliers” differently than other data points in the analysis.

Tom Pering, *Specific issues, Line 556*: Rephrase to remove ’basically vanished’.
We consider “basically vanished” a matching qualitative description of a correlation coefficient of +0.19, in particular in contrast to the “former” correlation coefficient of +0.69. The sentence reads: *This correlation was lower for the calibrated data (correlation coefficient of +0.69 when all wind speeds are considered) and in particular basically vanished for wind speeds larger than 10 m/s (correlation coefficient of +0.19, Figure 8f)*.
Remark: We are aware of the fact that we call elsewhere a correlation coefficient of 0.25 “weak” — i.e. there is some arbitrariness in qualitative scales, what we consider nevertheless appropriate.

Tom Pering, *Specific issues, Line 553-558*: See general comments on correlation.
See our reply on (Treatment of statistics, Part 1).

Tom Pering, *Specific issues, Line 580*: see previous comment, anti-correlation needs proof.
We confirm that this refers to a negative Pearson correlation coefficient.

Tom Pering, *Specific issues, Line 718*: What does 'basically not correlated' mean in quantifiable terms?
Change: We simplifed the sentence by removing *basically*.

Tom Pering, *Specific issues, Line 715 onwards*: Correlation terminology, see general comments.
See our reply on (Treatment of statistics, Part 1).

Tom Pering, *Specific issues, Line 787*: Reference should probably be made to the Aiuppa paper here (already cited in this manuscript), which talks about this subject exactly.
As stated in the abstract, we focussed on the retrieval method and description of the time series rather than volcanological interpretations. Nevertheless, we agree with the reviewer that a somewhat deeper comparison with the volcanological model presented by Aiuppa et al. (2018) is appropriate. We therefore extended our discussion on volcanological findings.
Change: We extended and reformulated the paragraphs on volcanology in the discussion which reads now (Lines 800–837 in the revised manuscript):
BrO/SO_2 and SO_2 emission fluxes and magmatic processes
Aiuppa et al. (2018) suggested a model, based on their data and past studies, that the (re)appearance of the lava lake on the surface was most likely caused by the enhanced magma convection supplying CO_2-rich gas bubbles from minimum equivalent depths of 0.36–1.4 km. They proposed that this elevated gas bubble supply destabilised Masaya's shallow magma reservoir ($<$1 km depth). The model is not completely new, already Rymer et al. (1998) and Williams-Jones et al. (2003) proposed that Masaya's

cyclic degassing crises are caused by convective replacement of dense, degassed magma by gas-rich vesicular magma in the shallow plumbing system (<1 km depth). Their ideas were based on results of periodic gravity surveys and they also argued such convective overturning is not necessarily triggered by intrusion of fresh (gas-rich) magma but may simply be initiated by degassing/crystallisation (and consequent sinking) of shallow resident magma. The data from Aiuppa et al. (2018) seem to confirm this model.

Our BrO/SO_2 data are characterised by a pronounced annual cycling but in addition we observed further changes in our gas data, which might be linked to the magma dynamics connected to the lava lake. As stated already in Aiuppa et al. (2018) and confirmed with the data presented here, no significant long-term changes in the SO_2 emissions fluxes were observed when the lava lake became visible at the surface. But a step increase in the BrO/SO_2 molar ratios can be noted after September 2015 (happening somewhen between September–November 2015, covered by a data gap). This change in the gas composition was thus caused by variations in the volcanic bromine emissions rather than in the sulphur emissions, similar to the change in CO_2/SO_2 molar ratios noted by Aiuppa et al. (2018), which respectively was caused mainly by the variation of the CO_2 emission flux. Those authors interpret these observations as evidence for supply of CO_2-rich gas bubbles, sourced by enhanced magma transport and degassing at a depth >(0.36–1.4) km. Following their interpretation and assuming that BrO is somehow an indicator for bromine emissions, that would mean that also bromine is degassing below that depth or something, which leads to an enhanced transformation of HBr into BrO.

The increasing BrO/SO_2 molar ratios would thus indicate that bromine degasses together or is enhanced/driven by CO_2 degassing. Unfortunately, there are to our knowledge no studies (apart from conceptual models) to prove or disprove the counter-intuitive early degassing of halogens, specifically bromine. However, also Bobrowski et al. (2017) describe a similar behaviour between CO_2/SO_2 and BrO/SO_2 in connection with a lava lake level change.

Aiuppa et al. (2018) further observed an increase in the SO_2 degassing after the appearance of the lava lake at the surface, which is a further argument on their hypothesis for a faster shallow magma convection. Our data confirms an enhancement of the mean SO_2 emission fluxes by 30 % for the period from December 2015 to February 2016 when compared with the previous and subsequent degassing behaviour. The described observation of Aiuppa et al. (2018) ends with March 2017. The decrease in the lava lake activity in mid 2018 is therefore not described by those authors. We here report a significant decrease in the SO_2 emission fluxes after March 2018 (happening somewhen between March–June 2018, covered by a data gap), while the BrO/SO_2 molar ratios hardly changed. This decrease of the SO_2 emission fluxes in time in connection with the decrease in the lava lake activity is consistent to the interpretation that the convection of the magma inside the conduit below the upper reservoir has slowed down again after 2018 and an important further indicator to sustain this hypothesis could be additional CO_2/SO_2 molar ratios. Unfortunately no CO_2/SO_2 molar ratios are available to the authors by the time of writing of the manuscript.
An unchanged BrO/SO_2 ratio and a lower SO_2 emission flux would lead to lower bromine emission as well, if we assume a correlation of bromine emissions and amount of BrO. We might further speculate that the bromine emission and carbon emission are characterised again by a similar pattern, which would mean that we also see a decrease in the CO_2 emission flux.

Tom Pering, *Specific issues, Line 787-806*: Interesting analysis, but framing of this will depend on the reassessment of statistics in the manuscript. We don't consider a reassessment of the statistical methods/results required. Thus no change of the framing is required.

3 Technical Comments

Tom Pering, *Technical Comments, Line 10*: Correct 'We make plausible'. Sentence needs shortening and clarification.
Change: We removed the corresponding sentence from the abstract (along with other shortenings of the abstract).

Tom Pering, *Technical Comments, Line 15-18*: Shorten, too much distance between the mention of 'former periods' and what those periods are.
We consider the original sentences as a rather condensed form of the specific content. We don't see how the text regarding this content can be further shortened. Nevertheless, we moved these sentences to the first paragraph of the abstract because this is where the abstract deals the first time with the time interval(s).
Change: The sentences were moved to the first paragraph of the abstract.

Tom Pering, *Technical Comments, Line 253 and 293 and 531 and 744*: Don't use 'w.r.t' use with regards to.
Change: We replaced *w.r.t.* by *with respect to* at all 10 positions in the manuscript.

Tom Pering, *Technical Comments, Line 295*: 'There is also a significant number of scans'
Change: Adjusted as recommended.

Tom Pering, *Technical Comments, Line 299*: 'Gaussian distribution' singular.
Change: Adjusted as recommended.

Tom Pering, *Technical Comments, Line 571-572*: Rephrase needed.
Change: We changed the text to read now: *[The larger the wind speed, the higher*

is the atmospheric turbulence and thus the lower is the accumulation.] Accordingly, over-proportionally much volcanic gas could effectively get released from the volcanic edifice to the atmosphere during peaks in the wind speed (if the wind speed is subject to significant short-term fluctuations).

Tom Pering, *Technical Comments, Line 577*: There are a number of possibilities.
Change: Adjusted as recommended.

Tom Pering, *Technical Comments, Line 718*: Remove 'basically'.
Change: Adjusted as recommended.

Tom Pering, *Technical Comments, Line 741*: Alter phrasing away from 'basically vanishing'
Change: We changed the text to read: *The correlation became insignificant (+0.16) when the wind speeds are calibrated and only wind speeds larger than 10 m/s are considered (see Figure 7e+f).*

Tom Pering, *Technical Comments, all graphs*: check the 2 is subscripted in SO_2.
Change: We took care of this issue.

4 Additional Comment by the authors

There has been a major update from GNU R 3.6 to GNU R 4.0. Besides many other changes, GNU R 4.0 contains now an improved algorithm for rounding to decimals (see a description of the issue on https://cran.r-project.org/web/packages/round/vignettes/Rounding.html).
The statistical analysis in our manuscript was performed with GNU R 3.6 for the originally submitted manuscript but with GNU R 4.0 for the current version of the manuscript. As expected, most numerical results are identical although whenever there were numerical thresholds applied as data filter, some data points are now no more rejected by the filter while other data points are now no more rejected. In consequence, some data points appears now (or are missing now, respectively) in the plots (and analyses) of the time series. Nevertheless, these minor numerical changes did not changed any of our major findings.
You may see this behaviour prominently in Figure 13 which looks as in the original manuscript except that there is now a “blue" correlation coefficient of -0.2004209 for pressure vs. H_2O which was originally (absolutely) smaller than $|0.2|$ and thus was “grey".

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-942, 2020.