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Atmospheric Measurement Techniques
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Discussions

Interactive comment on “Retrieval of volcanic SO_2 from HIRS/2 using optimal estimation” *by* Georgina M. Miles et al.

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article graphicx

Authors' Response to Reviewer 1

May 16, 2017

Referee comments in italics.

We would like to thank Reviewer 1 for his detailed comments which have improved the paper. *The manuscript presents a new scheme for the retrieval of atmospheric SO_2 total column amounts after volcanic eruptions from HIRS/2 observations. An optimal estimation retrieval using radiances from three HIRS/2 channels in the mid-infrared region is presented. Retrieved parameters are cloud top and the total column amounts of water vapour and SO_2. Major error sources, which are identified by synthetic observations, are cloud/ash interference and the assumptions on the altitude and vertical extend of the SO_2 plume. Through simulations it is further demonstrated that the new scheme is superior compared to simple brightness difference methods. The method has been applied to the case of the eruption of the Cerro Hudson volcano in 1991. This is an important piece of work since it presents an improved retrieval scheme to obtain SO_2 from TOVS measurements and, thus, opens the possibility to obtain climatological time series of this important trace species. After some modifications/extensions as detailed below, I strongly support its publication in AMT.*

General comments:

The optimal estimation scheme is used but not explained nor referenced. I would propose to add a paragraph with the main formulas (adapted to the actual retrieval problem) and add the main references. It would be very helpful to add a table or some graph summarizing the major error terms which have been investigated and how those are handled (some explicitly, some implicitly as part of the measurement error).

We thank the Reviewer in particular for pointing out the missing identification of the optimal estimation scheme. This is a clear oversight and some indication of inverse method used is important to be included in the paper, even if it is only a mathematical tool. We have now referred in the text to Rodgers (2000) generally and Miles et al., (2014) specifically, the latter of which used identical retrieval methodology in terms of the inverse method and cost minimisation part of the retrieval used with the forward model. In that text the methodology is explained quite exhaustively.

The issue of the handing of errors can be elucidated, indeed it could be made clearer that some sources of error can be handled by the retrieval (presence of cloud/ash, SO_2/H_2O covariance, measurement noise and an attempt at FM error), and others can only be explored to obtain a general indication of the sort of confidence that may be placed on the results (such as height and plume thickness uncertainty). Specific comments:

P1L20: 'detection method': The method presented here is more than pure 'detection' – it's quantification.

Text has been changed to "..detection and quantification method. . .".

P2L6: could you specify more precisely the channel boundaries for the different HIRS/2 instruments. How much does this affect the retrieval scheme?

The following text and references have been added:
“Retrievals are obtained using the Levernburg-Marquart minimisation method after Rodgers (2000), and the full optimal estimation scheme used here is described in detail in Miles et al., (2015).”

Rodgers, C., “Inverse methods for atmospheric sounding: theory and practice”, 1 edn, World Scientific, 2000.

Miles, G. M., Siddans, R., Kerridge, B. J., Latter, B. G., and Richards, N. A. D.: Tropospheric ozone and ozone profiles retrieved from GOME-2 and their validation, Atmos. Meas. Tech., 8, 385-398, doi:10.5194/amt-8-385-2015, 2015.

The following text has been added/amended in the discussion on error study, section 3:

“There are some sources of error that can be incorporated and dealt with by the retrieval. These include measurement noise, the presence of cloud or ash, SO_2/H_2O covariance and an estimate of forward model error discussed above. The main sources of error that cannot be adequately represented in the forward model are errors that impact ill-posed nadir SO_2 column retrievals in general. These are incorrect height assignment of the SO_2 plume, incorrect thickness in the plume represented in the forward model and particularly in the case of infrared measurements and sensitivity to the presence of cloud and/or water vapour. Their relative impacts vary and the sensitivity of the solution to them can be quantified using simulations. It should be noted that some of these errors (plume height and profile shape) cannot often be known at the time of retrieval, and as such the actual impact on the retrieval result also cannot be known. They are investigated here in order to give a general indication as to the potential error that can be associated with the results, to give a window of confidence. Others, such as the impact of cloud or ash on the retrieved SO_2 error can

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be investigated for use in quality control."

Specific comments:
P1L20: 'detection method': The method presented here is more than pure 'detection' – it's quantification.

Text has been changed to "..detection and quantification method. . .".

P2L6: could you specify more precisely the channel boundaries for the different HIRS/2 instruments. How much does this affect the retrieval scheme?

There are 15 instruments on just the TiROS/NOAA platforms (1978-2005 before MetOp and HIRS/4) each instrument had similar but slightly different channel configurations but where channels are considered to be in common the central wavenumber is similar. It would be a distraction and exhaustive to describe each of the channel boundaries for each of the instruments, but it is sufficient in the authors' view to mention that the channels can vary between platforms/instruments, but all broadly have the three channels used in the retrieval. It has not been fully investigated for the OE scheme but other HIRS/2 instruments have been used by the Prata fit method in the literature.

This instrument is a broadband radiometer rather than a spectrometer, so small differences in the central wavenumber of channels between instruments is not considered sufficient to appreciably alter or limit this approach between instruments for the OE scheme. They were designed to respond to specific, principal absorbing species relevant to the purpose of the instrument, which was to characterise temperature profiles, water vapour, total ozone, cloud top pressure and surface reflectance, and as such do not change much between instrument as they were designed to be similar. The width of the channels is pertinent if there are multiple absorbing species within the envelope of the instrument response function. All such species are required to be taken into account in the forward modelling of the atmosphere if they impact the measurement such that their absence would contribute to model error.

P4L24: It would help the reader if a higher resolved spectrum of the single major contributors to the radiance could be provided, overlaid by the channel-boundaries. Are there any other gases contributing in each channel?

Other gases do indeed contribute to the channels, but the predominant species (the absence of which would contribute appreciable error in modelled channel radiance) are included and modelled in RTTOV at a climatological value if they are not retrieved. The error of including at climatological value was investigated in great detail using the RFM as a way of estimating forward model error (in addition to the impact of modelling the atmosphere at the lower spectral resolution of RTTOV compared to the RFM). Other potential contributions to forward model error that can be estimated this way include spectral resolution of the forward model, including in the FM non-retrieved gases at their climatological value, excluding other gases which are known to exist in the real atmosphere and representing the vertical atmosphere at limited height resolution.

Error contributions deemed significant in the discussion of the RFM used to estimate forward model error (FME) are those that for a channel stand out from the others in terms of magnitude, and that exceed the noise equivalent brightness temperature difference for a given scene temperature. The absolute difference between a test case and the reference case is taken as the channel contribution of FME. The simulations were performed for all 19 of the HIRS/2 channels, irrespective of the fact that only three are used in the column retrieval. Extensive simulations with the RFM were performed that tested the sensitivity of the channel brightness temperatures to variation of the

elements listed above. The RFM was run with spectral resolution ranging from 0.001 cm−1 to 0.1 cm−1 to quantify the effects of a reduction in spectral resolution. This only appreciably impacted channels not used here. The change in simulated channel brightness temperature for gaseous species at their climatological level and 1 standard deviation from it were used to quantify the individual impacts of non-retrieved trace gas variability. This only really impacted the channel used to detect column ozone and not used in the retrieval. The RFM was also used to simulate the impact of including all of the minor species such as SF6 and F12-14 (anthropogenic halides). They showed that provided one accepted that their variability was low in the real atmosphere, there was very little sensitivity to them and they did not require inclusion in the background profile used in the forward model, but by this method their exclusion contributed quantitatively to the FME. All elements of forward model error were combined in quadrature. The forward model error as defined by calculations with the RFM is not definitively appropriate to a forward model based on RTTOV. It will contribute to the estimate of the total FME for the purposes of this method development in the absence of an equivalent term being evaluated for RTTOV (which is considerably more challenging to obtain), and broadly constitute a minimum envelope of FME for the HIRS/2 channels. This exercise has only been performed for HIRS/2 NOAA11, but since the channels in the retrieval are very similar, the FME contribution is not expected to change appreciably, but could be the subject of future work.
The above is mentioned but very succinctly in section 2.3 for the sake of brevity and concentrating on the channels used in the retrieval.

The following text has been added to section 2.1 to be more clear:
“Other atmospheric gases not retrieved but contribute appreciably to channel brightness temperature are represented in the forward model by a climatological value. The potential error that this can introduce is incorporated into the estimate of forward model error.”

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1 has been added to the text as the new Figure 1, in addition to the following text:

"Channel 11 from HIRS/2 aboard NOAA11, centred on 7.2 μm, is shown in Fig. 1. Also shown are simulated transmission spectra for water vapour (which this channel was designed to detect) and SO_2, for two column amounts. It demonstrates both that the channel and spectral feature coincide well, and for large column amounts of SO_2 the channel would be strongly affected."

It is not thought necessary to plot the other two channels, since they are a further water vapour channel and window channel and are not particularly illuminating.

The following has been added to the text:

"Further information about the principle absorbers of the other channels not used in the retrievals can be found in NOAA (1981)."

NOAA (1981), NOAA Technical Report NESS 83: Atmospheric Sounding User's Guide, Technical report, National Oceanic and Atmospheric Administration. NESS 83.

it P5L19, 'up to 300 DU': Could you give examples from literature how this number covers the upper limit of volcanic eruptions. In addition, the spectral plots (see comment P4L24) should include lines of SO_2 for different column amounts.

It is not stated that this number (300 DU) covers the upper limit of volcanic eruptions. The limit was chosen in this case to be appropriate for the case study eruption (and those smaller), where a priori knowledge existed (e.g. from TOMS, references given in text) to suggest that in nearly all instances this would be sufficient. This would be the case for the 2008 eruption of Kasatochi which was of a similar magnitude. The training limit is very important due to the way in which RTTOV calculates layer transmittances for gases because some species require higher order terms in their predictor coefficients that are challenging to characterise. To train the model for higher

New_fig_1.jpg

Fig. 1. Transmission spectra of H_2O and CO_2 simulated for

SO_2 column amounts is no doubt possible, but would be the subject of further, future work as non-linearities in the behaviour of the model at very high SO_2 loadings, sensitivity to profile shape and saturation effects would all have to be adequately examined.

In order to put column amounts discussed into context, the following text has been added:
“100 DU represents an SO_2 column from a large, explosive volcanic eruption. Pinatubo, for example, yielded column amounts of 350-500 DU (depending upon instrument) after 24 hours which reduced to 100 DU after 7 days (Carn et al., 2005). The OMI instrument (see Table 1) captured column amounts of around 200 DU after the 2008 eruption of Kasatochi (Prata et al., 2010).”
Spectral transmittances for two column amounts are now given in a new Figure 1.

P5L24 ‘see Fig. 1’: Fig. 1 has not been described up to this point, but only later in the text. Further, it does not show the cost ‘up to the training limit’ of 300 DU, but only to 200DU.

This has been amended to reflect the actual scale used.

P5L30 ‘calculated numerically’: Could you explain this more in detail. Are the analytic Jacobians used at all?

They are evaluated numerically by successive calls to the forward model for fractional perturbations of the state vector. The analytical Jacobians aren’t used directly in the retrieval.
The text has been changed as follows: “As a result, these are evaluated numerically in the forward model by successive FM calls where each element of the state vector is fractionally perturbed in turn”

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P5L31, 'manually': What does this mean? How large are the limits for H_2O, SO_2? How does it work when it is mentioned 'The weighting functions are allowed to make linear extrapolations. . .'? Is this only valid for the last iteration step?

The word manually has been removed to avoid confusion. It was used in an attempt to convey how the limits were imposed/hard-coded. The limits listed below necessarily apply to all retrieval steps to enable the forward model to function, because the forward model is used to evaluate the weighting functions. The limits, particularly in the case of water vapour, are generally extreme, and on such pixels where it is necessary (a handful out of a week's worth of orbits) there are typically other issues with the measurement that the forward model has a problem replicating. Such pixels are removed at point of quality control as they undoubtedly lead to either non-convergence or very large errors.

The text has been modified as follows:
"…constrained in the FM by the physical limits that RTTOV will accept, or that are appropriate for the forward model. These are 0.01 to 800 DU for SO_2, 1e-6 to 16 times the column water amount predicted by ECMWF and a maximum cloud top height of 16 km (a conservative upper limit for tropopause height."

P6L25, 'The estimate accounts for inaccuracies that arise due to modelling the atmosphere at reduced spectral resolution, limited vertical resolution, inclusion of nonretrieved trace gases at a climatological level or their preclusion entirely, relative to a reference case.': What is meant with 'limited vertical resolution' and 'modelling .. at reduced spectral resolution'? How has this error been derived (line-by-line compared to band model)? How strong does this error depend on the atmospheric situation?

Please also see response to P4L24 comment above.

Reduced spectral resolution refers to the fact that the RFM is a line by line model, but may be run at poorer resolution to represent something close to the way in which RTTOV represents spectral transmittances.

It is acknowledged that FME contributions may change depending upon the state, but even though the changes are expected to be small, characterising FME is a non-exhaustive process that can only estimate contributing sources of error. In this case, there are larger sources of error from elsewhere (such as incorrect height assignment, errors in representation of SO_2 profile in forward model or the presence of multi-layer optically thin cloud or ash) that are expected to be considerably more dominant.
Section 2.3 now states: "…limited vertical resolution (100 m versus 1 km as used in the forward model outside the region of the SO_2 perturbation),"

P7L9, '100 DU': Could you put this number in perspective of the typical maximum column amounts e.g. after Pinatubo?

The following has been added to the text:
"100 DU represents an SO_2 column from a large, explosive volcanic eruption. Pinatubo, for example, yielded column amounts of 350-500 DU (depending on instrument) after 24 hours which reduced to 100 DU after 7 days (Carn et al., 2005). The OMI instrument (see Table 1) captured column amounts of around 200 DU after the 2008 eruption of Kasatochi (Prata et al., 2010)."

P7L16: How are the a-priori errors of the state vector element for water vapour set? Is this error only considered in the measurement space? Further, could you explain how the off-diagonal elements of the a-priori covariance matrix are set.
The following text has been added to the relevant section: "The a priori error for water vapour is based on the variance of water vapour the ECMWF atmospheric training

profiles discussed above relative to the mean."
The error covariance of water with SO_2 is only considered in the measurement space, since it is applied to a channel that is sensitive to both water vapour and SO_2 it effectively absorbs the error covariance – it is mapped onto measurement space. As such there are no off-diagonal elements specified in the a priori covariance matrix. Provided the QC described is applied, they are not applicable.

P8L1, Chapter 3.1, and Fig.1: Regarding the error bars shown in Fig. 1: Could you summarize which errors they contain? Have these errors been incorporated in the synthetic observations? (I assume no or only partially, otherwise the retrieval results should somehow scatter around these errors). Further, do the error bars represent 1 or 2-sigma values? The maximum value tested here seems to be 150 DU. Could you extend this range? You should also show, at which values the method fails and at which column amount of SO_2 *the channel signal becomes saturated.*

This figure is used to demonstrate deficiencies in the Prata method and the linear behaviour of the OE column retrieval, as appropriate for the case study in particular.
The error bars show the retrieved error. The simulations were performed with simulated measurement noise (which is small) and FME. That is why an OE retrieval is so much more useful than a band model, such as the Prata model shown, which has no possibility of estimating error or quantifying uncertainty.

It is not the case that the maximum value tested is 150 DU, as can be seen from inspecting the far edge of the axis at 200 DU – a limit appropriate for the case study presented. The model fails above the training limit and in such a non-linear way that the authors feel it would be a distraction to show or dwell on this matter, as it is behaviour related to the complex inner mechanics of RTTOV which is itself a comprehensive and complicated system. The channel becomes truly saturated above 1000 DU, far beyond the training limit, and as discussed elsewhere it would be the topic of future and non-trivial work to qualify the model behaviour for significantly larger eruptions.

This work is intended as a proof of concept, which the authors feel it demonstrates, rather than the definitive or comprehensive examination of the use of this instrument for all eruptions and for all HIRS/2 instruments. It is a worked demonstration applied to an eruptive event of significance, and as such the simulations and testing are all suitable for supporting both concept and case study.

The figure caption has been amended as follows:
“Retrievals based on simulations by a line-by-line model (RFM), with synthetic measurement noise. The error bars for the column retrieval are the retrieved errors.”

P8L25, ‘Measurements were simulated for a plume at a range of altitudes from 8-18 km’: However the caption in Fig. 2 says vice-versa: ‘A 2 km thick triangular profile centred at 12 km is used to simulate measurements. The profile is then used in a retrieval with the retrieved height assigned to a range of altitudes.’ Could you tell in which way the test retrievals have really been performed?

This section (referring to what is now figure 3) now reads:
“Measurements were simulated for a plume at a range of altitudes from 8-18 km. Figure 3 shows the impact on the retrieved SO_2 column at a specified, fixed altitude of 12 km as a fraction of the true column at these altitudes. Errors range from typically $\pm$0-30
The caption for what is now figure 3 has been amended to read:
“A measurement was simulated for a volcanic plume of triangular profile centred at a range of altitudes, for a range of total column amounts. A retrieval is then performed

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where the plume is assumed to be at 12 km. The fractional difference, or error, is plotted."

P8L28: In this paragraph it is only referred to the Figure, however the results are not described. Please give also in the text at least some quantification of the resulting errors.
Please see above.

P9L1-3: It is not clear what is different here compared from the paragraph before.

The text has been modified to make its purpose more clear. Specifically, it refers to a test of precision/accuracy of RTTOV vs RFM forward models, to show that it behaves in a similar way compared to the line-by-line model irrespective of SO_2 plume altitude: "The performance of the column fit was also directly assessed against a line-by-line model (RFM) for plume altitudes from 8 to 18 km (where the plume height assignment used in the retrieval was the same as that used in the measurement simulated by the RFM) and it was found that…".

P9L6-13: Also here in the text some numbers (percentage error) should be mentioned. Further, could you explain, why there is such a large difference between the errors when the plume thickness is over- versus underestimated. Would this result not speak for application of a rather sharp profile in the retrieval to minimize the errors

It is true that this suggests an underestimate of plume thickness would imply smaller errors than an overestimate. Further work would be required to establish an optimum thickness if there is one, particularly in relation to the vertical grid of the forward model, which may be a limiting factor. It is sufficient here to state that a profile that most resembles the true profile should be the best. In this case there is plenty of ancillary information to give some indication of both plume thickness and plume altitude, namely lidar data. The actual error plume thickness cannot be known, but as with error in height assignment these simulations are a useful indicator to give confidence windows to whatever values might be retrieved.

Indicative numbers have been added to the text:
“The retrieval simulations suggest that errors are larger when the plume thickness is overestimated (typically 13
P9L29: ‘water vapour clouds’ should perhaps read ‘liquid water clouds’ ?
Text has been amended accordingly.

P9L29, ‘above 5 km’: However in Fig. 4 the retrieval seems to be OK up to 8-9 km. Can you give an explanation why the retrieval has problems to fit cloud heights above a certain altitude. How much does it depend on the atmospheric situation (tropics vs mid/high latitudes)?

The pertinent panel in figure 4 is the bottom right, which shows that deviations (errors) in retrieved water vapour column begin when a cloud is at 6 km. Poor fitting of water vapour leads to errors in the retrieval of SO_2, because the 7.3 micron channel is sensitive to both water vapour and SO_2. A cloud at 5 km shows no perceptible deviation, and it is likely that the threshold is somewhere in between in this case, which is the origin of the 5-6 km warning. The H_2O weighting function of this channel peaks at 700hPa (but as the reviewer mentions this may vary slightly depending on the state, which is why a mid-latitude profile was used). The 6.8 micron channel weighting function peaks at 500 hPa. In the abstract 6 km was stated as this will definitely contribute an error to the retrieval of SO_2.
For clarity this has been corrected to 5 km since it is stated as “…above..” the given level.

P10, chapter 3.4: What is the upper limit of the retrieved SO_2 (e.g. due to saturation effects)? This could also affect the total mass calculation in very dense plumes. Also some information about the convergence criteria of the retrieval and how many iterations are necessary are missing.

This has not been tested with RTTOV. It would require the regression coefficients to be trained for much larger column amounts. Exploratory work with the RFM found that channel brightness temperature differences for a given change in SO_2 column amount become increasingly small over 600 DU for this channel with HIRS/2 on NOAA11. Since this is above column amounts observed even for Pinatubo, it has not been investigated exhaustively and is not referred to in the text. This may change slightly depending on the altitude of the SO_2, but would require further simulations and further work to investigate if this method were to be applied to a an eruption with very high SO_2 column amounts (Pinatubo or larger).

The text in section 2.1 has been modified as follows:

“The 7.3 μm channel is sensitive to both water vapour and SO_2. This channel may be said to saturate for SO_2 columns above 600 DU where significant increases in SO_2 result in small changes in channel BT below the envelope of the channel noise and other error terms.”

P12L28: Could you state which SO_2 altitude profile has been used for the case study and how it has been derived. Is the resulting SO_2-altitude error included in the column errors in Fig. 5?

More work was done than has been stated to identify the plume altitude from Hudson, and some of it bears repeating to explain the origin of the SO_2 profile used in the model, before it is stated.

Lidar information has been used to identify the probable altitude of the plume of between 10-13km, peaking at around 12 km. This is considered to be a very reasonable estimate of the true shape of the SO_2 profile, given the considerable ancillary information available.

As stated, Figure 5 shows only the retrieval error. Given the amount of information about the plume height, it is unlikely that the plume height used in the retrieval is more than 1 km out. Figure 5 suggests that this would result in errors of not more than 10

The following has been added to the text:
“In addition, contemporary lidar measurements of the Hudson plume were made at the CSIRO (Commonwealth Scientificand Industrial Research Organisation) Division of Atmospheric Research, at Melbourne, Australia (38 S, 145 E) (Young et al., 1992, Barton et al. 1992). These measurements are sensitive to ash, sulphate aerosol and meteorological (water) cloud. The backscatter profiles tend to indicate peaks at around and above 20 km, and frequently at 10-13 km. The higher peak is attributed to aerosol from the Pinatubo eruption. Young et al. (1992) interpret the majority of observations that are thought to include Hudson material as the feature at 12 km in October, with variable cirrus at 10 km. It is reported by the authors that the plume was observed consistently from 28th August until December 1991 between 10 and 13 km, with a decreasing scattering ratio. The relative proportions that contribute to the backscatter measured are expected to be dominated by ash in the first few weeks after the eruption. Little ash is expected to be present after a month beyond the eruption, but by this time the vast majority of the SO_2 will have oxidised into aerosol. Whilst lidar is not sensitive to the presence of gaseous SO_2 inferences can be drawn from the height of the aerosol it eventually becomes. In this case the lidar information is considered to be a valuable starting point as a guide for estimating the cloud height of the SO_2, in the context of other information.”

“Using all of this information, the Hudson plume is modelled as a triangular peaked profile with a baseline of 2 km between 11 and 13 km, peaking at 12 km.”

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P14L5: To make this calculations more clear and give the reader a better feeling for the derived e-folding times and its possible uncertainties it would be necessary to plot derived daily masses after 17th August and show the fitted exponential decay line.

This would be the case, but as mentioned in the text, due to the narrowness of the swath and the rate of motion of the plume, in addition to the presence ash in the first day after eruption, of total mass estimates on successive days do not follow a smooth curve. There are also several ways of estimating plume total mass, each have inherent issues associated with them that introduce error. Adding up total mass of the area represented by the footprint adds no information as other approaches like Kriging might, but in the case of HIRS/2 the calibration scanlines are missing, sometimes the plume is only partially sampled by a given orbit and there is movement between orbits. TOMS had the benefit of a wider swath, but HIRS/2 was able to sample both day and night so had more opportunity to monitor the plume. We found that gridding, which in theory might get around the problem of incomplete sampling resulted in total masses that were heavily dependent upon grid size and in most cases under-estimated the maximum plume mass compared to summing the areas represented by the satellite footprint. Just summing footprints results in a fairly noisy representation of the decay. It is a concern that to venture too far into this discussion is a distraction from the main point of the paper, which is to introduce the technique and demonstrate its effectiveness using a case study. We feel that adding a figure would require significantly more discussion about this issue than is warranted here. Furthermore, while the majority of the SO_2 was released by Hudson on 12th August, about 30

We have now mentioned some of these further points in this section:
"...be overly-generous bounds by this method. This case is complicated by the fact

that about 30
The following text has been added:
“In reality the total mass observed does not decay smoothly, but has noise due to the fact that the plume is not always perfectly sampled, and the number of retrieved pixels excluded due to the presence of high or thick cloud or ash varies.”

Also:
“More recently, Carn et al. (2016) estimated the e-folding time of Cerro Hudson to be 7 days, based on mass estimates from TOMS (Constantine et al., 2000). They attribute this anomalously short e-folding time to the late southern hemisphere winter timing of the eruption. However, since Constantine et al., (2000) estimate nearly twice the total mass (4000kT) than that observed by HIRS/2 in this work (and the subsequent TOMS algorithm discussed here) it is possible that the inconsistency in e-folding times could be due to an over-estimate of initial erupted mass from the original TOMS algorithms. Total mass estimates (and therefore e-folding time estimate) would be improved greatly in accuracy if the HIRS/2 instruments aboard NOAA10 and NOAA12 that were also present were used to result in very comprehensive sampling of this eruption.”

P14K19, ‘2300 +- 600 kT’: How has the error of 600 kT been calculated?

This is the average retrieval error to appropriate significant figures. The text has been amended as follows:
“This OE column retrieval finds a new total erupted mass estimate for the 1991 eruption of Cerro Hudson of 2300 ± 600 kT from the HIRS/2 instrument aboard NOAA11, where the error is the retrieved error.”
Table 3 now also explicitly mentions this in its caption (see below).

P14L22: Please give also the total masses (including errors) of TOMS, Carn et al., 2016 and Prata et al., 2003.

The text has been modified to highlight the mass and origin of mass calculated, with further discussion added regarding the inconsistency in e-folding time that is thought to result from the Carn comaparison in particular. No errors are given in that text, but as their origin is the Constantine paper, it is mentioned elsewhere in the text that they estimated their retrieval error to be of the order of 30
The Prata et al., (2003) method estimates an error of 5
The following text has been added to the introduction of the Prata method: "Indeed, it is not possible to formally quantify error of mass estimates from this method as it currently stands."
The caption to what is now Figure 2 has had the following added: "No error estimates are possible for the Prata fit method."
Table 3 (showing comparative mass estimates) has had two comments regarding errors added:
"1Constantine et al. (2000), with errors estimated to be circa 30 %."
"4This work, with retrieved error."

Technical comments:
P1L31, 'The TOVS instrument': But in the sentence before it is explained as a suite of instruments.
This has been clarified in the text.

P1L32, 'TIROS': Is written 'TIrOS' in L28.
These have been unified.

P4L21, 'Table 1': Shouldn't this read 'Table 2'?
This has been corrected.

P11L7: '(Constantine et al. 2000)' to '(Constantine et al., 2000)'

This has been corrected.

P12L14: ‘verses’ to ‘versus’
This has been corrected.

P13L4: ‘317 channel’ to ‘317 nm channel’
Corrected.

P13L5: ‘340 to’ to ‘channel at 340 nm to’
Corrected.

P14L3: Should the formula not read: N(t) = N0 exp (- lambda t)
Corrected.

P15L15: ‘satellites’ to ‘satellite’
Corrected.

P23Fig3: ‘11.5, 12, 13 km’ should read ‘11.5, 12, 12.5 km’
Corrected.