We thank the reviewer for a thorough review and constructive comments to improve this manuscript. Item-by-item replies are provided below; text in italics shows reviewer's comments.

Comment. *Some more discussion of the initialization parameters would be useful with reference to previous literature and observations. For instance, Kristiansen et al.(JGR, 2010) estimate SO2 emissions at 7 and 12 km, and some up to 20 km. The AVO also noted three distinct eruptions (http://www.avo.alaska.edu/volcanoes/activity.php?volcname=Kasatochi&eruptionid=605), although a one-day duration is used here. How might the assumptions used in the model initialization affect the model results, if at all? A brief mention of how uncertainties in OMI retrieval might affect final results would also be useful: i.e., from retrieval uncertainties due to volcanic ash etc.*

Reply. The following paragraphs are now added into the text to provide more details and discussions on the model initialization.

"In addition, because OMI only provides a snapshot of the distribution of SO_2 during the eruption and also likely missed the western-most part of SO_2 clouds in the study domain of Figure 1, the estimate of 1.5 Tg of eruptive SO_2 from OMI retrievals may have a low bias [Yang et al., 2010]. Consequently, a total of 2.0 Tg SO_2 emission is specified with the effective injection height of 10 km at the model grid box for Kasatochi. Based upon Waythomas et al. (2010), the eruption duration is assumed to be 24 hours (with a starting time of 2200 UTC on 7 August 2008) in the model.

It is worthy noting that the assimilation of OMI SO_2 into the model took place in the hour of OMI overpass time on 8 August 2008, and the assimilation here essentially is the replacement of SO2 field simulated by GEOS-Chem with the OMI SO_2 field (e.g., similar as model initialization). To avoid the discontinuity of SO_2 field due to this replacement in the model, a Barnes smoothing technique [Barnes, 1964] is used, in which the influence of the innovation (e.g., difference between OMI and molded SO_2) at the model grid box (having OMI SO2 data) on the change of SO_2 in another gridbox (not having OMI SO_2) is inversely proportional to the distance between these two grid boxes. The purpose of this assimilation is to maximum the use of what OMI observed to correct the model simulation that otherwise would be fully dependent on the specification of the volcanic SO_2 point source function in the model.

Through chemistry inverse modeling constrained by the SO_2 column amount retrieved from AIRS, OMI, and GOME-2, Kristiansen et al. [2010] estimated that the Kasatochi SO_2 emissions may have two peaks at 7 and 12 km above the sea level, and some up to 20 km. This estimate, for the bulk, is consistent with Figure 1 that shows the peak of SO_2 mixing ratio is in the range of 6-10 km. However, it also is noted that in model simulation by Kristiansen et al. [2010], the SO_2 emission is specified for two days at the model grid box where Kasatochi is located, and hence their scheme for the initialization of emission is similar to this study, although no OMI SO_2 retrieval are directly assimilated in their model. Nevertheless, both the retrieval of SO2 amount (such as those from standard OMI product) and retrieval of SO_2 height (such as from research algorithm developed by Yang et al., 2009) have uncertainties with best estimate of 20% and 1-2 km respectively; low bias in height retrieval often corresponds to high bias in SO2 amount retrieval, and vice versa. To investigate the impact of the SO_2 injection height (used after the OMI satellite overpass) on the simulation results, sensitivity simulations are conducted with different injection heights of 2, 4, 6, and 8 km (Section 4.2)."

Comment. *It is mentioned several times that the low SO2 in the model is likely due to more clouds in GEOS-5 than are seen by MODIS. Is it possible to test this hypothesis in GEOS- Chem by using MODIS observations for met fields, or simply perturbing the GEOS-5 cloud amounts? How does this fit in with fact that Yang et al. (2010) mention OMI may be biased low in the first couple of days after eruption?*

Reply. Using MODIS clouds fields into the GEOS-5 will have to consider that MODIS only provides twice/day observation (from Terra and Aqua), and hence a data assimilation framework will be required to integrate the MODIS observation into the GEOS-5 filed; this type of work itself warrants a separate study.

Perturbing the GEOS-5 cloud field is a good idea. We did extra analysis by reducing the cloud liquid water content by 15%, and we found the SO_2 amount is increased by 5% in the first two days (or 7% and 3% in 1^{st} and 2^{nd} day respectively). This suggests that more cloud in GEOS-5 field contributes in part to the low bias in simulated SO_2. We add the following in the main text: "Indeed, our sensitivity experiment shows that a reduction of liquid water path by 15% in the first two days in GEOS-5 field results in a 5% increase in SO_2 total amount (7% and 3% in 1^{st} and 2^{nd} day respectively)." .

K. Yang is co-author of this manuscript. The low bias mentioned in Yang et al (2010) is estimated after considering that there were more eruptions after the OMI's overpass, and also OMI might also miss the part of volcanic SO2 clouds. This low bias is counted here as we now use the point emission of 2.0 Tg instead of 1.5 Tg that was originally estimated in Yang et al. (2010).

Comment. *Table 1: I don't think this table is necessary; it seems to add unnecessary detail to paper. Could be summarized with a couple of examples in introduction to show spread in estimates of altitude.*

Reply. Thanks to your comments. I felt I would need to use this table if I had chance to present this work in a workshop or conference.

Comment. *Figure 1: Looks like western-most part of cloud isn't observed by OMI. Is this the case? If so, are you making assumptions about the remainder of the cloud, or can you use another instrument to infer information about that part of the cloud? Also, model shown on high-resolution grid here. Mention it is interpolated/smoothed data, or show on*

2x2.5 grid.

Reply. The text we added in the revision now provides discussions regarding the low bias of OMI SO2 in the first day and the possible miss of SO2 clouds in OMI retrieval and converge. See our above reply for comment 1. The purpose of assimilation here is to use what OMI observed to correct the model simulation that otherwise would be fully dependent on the specification of the volcanic SO2 point source function in the model (as most of past studies did). If OMI missed part of SO2 clouds, that part in the model will still have be influenced by the assimilation based upon the innovation we had in places where OMI SO2 and modeled SO2 data can be compared.

In the figure caption, we added: "For illustration purpose, the SO_2 data in (c) is interpolated at 1°×1° resolution, but is gridded into 2°×2.5° resolution in the model simulation."

Comment. *Figure 2: There is a labeling error here. (a,c) instead of (a,b) for example. Also, what is meant by GCno_omi? Is this GEOS-Chem using AEROCOM emissions? Clarify in text.*

Reply. Thanks, the labeling error is corrected now. In the original figure caption, we have "(e): same as (a) but from GEOS-Chem simulations without model initiation of EISF retrieved SO2 (GCno_omi)". We now make it more clear at the end of the figure caption: "Subscripts omi_init and no_omi respectively denote the simulation with and without initialization of OMI SO2 data"

Comment. *Figure 3: I am confused about why OMI and GEOS-Chem don't have same mass on day one. Also, Yang et al (2010) showed mass of 1.5 Tg on first day but you use 2 Tg to initialize, and then OMI shows up higher than GEOS-Chem in this figure. How does this factor into graph, initialization and analysis? Explain in text. There is no black x on graph as discussed in caption.*

Reply: Sorry for the confusion. OMI only gives a snapshot of SO2 distribution, and during the first two days where emission occurs in between, there should have more SO2 than what OMI captures. Hence, the first two data points shown in Figure 3a is based upon the extrapolation, as done in Krotkov et al. (2009). We now provided these details in the caption of Figure 3. "Note, the time series of OMI SO2 data is obtained from Krotkov et al. (2010), in which the data points in the first two days are estimated based upon the extrapolation to account for OMI's sampling bias due to limited spatial and temporal coverage.". In the figure caption, we also removed "The black x indicates the simulation with a point emission of 2.0 Tg at 10 km.".

Comment. *Figure 5: Color of orbit is confusing as it is the same as background and hard to see. "and" is spelled wrong. "6a, d" should be "6a, c".*

Reply: We have correct the error, and change use the black color to indicate the orbit track.

Comment. *Figure 6: The pink letters and lines used here are confusing as they seem to reference the same areas as in Figure 5, since both are discussed in text at the same time (but in Fig 5 "A" means something totally different from "A" in Fig 6). Consider using a different color and labeling scheme in Fig 6.*

Reply. We now change all letters into L1, L2, L3, …, etc. in Figure 6. We keep the pink color as it is the only color that are not used in the plot of the actual data in Figure 6.

Comment. *Figure 7: Capitalize "temporal". Figure 8: Is black line best fit or another parameterization?*

Reply: "Temporal" is now capitalized. Also, all best-fit line is shown in red; all modeled based data are shown as black star in the scatter plot.