PLEASE NOTE: Following an explicit request from the editorial board, in hope to avoid misuderstandings, the authors would like to to retract some of the phrases present in the original reply. Please consider this as the response to the Short Comment.

Response to Short Comment by Guido Visconti

We thank the commenter for having taken the time to read the manuscript and give us such a long and thorough feedback. We address below all the points he raised.

Comments are in blue. Author responses are in black.

This paper as most of modeling papers neglects the experimental data. I would require the authors to make a comparison of their baseline results (without the SO2 injection) with the available data as done for example in the Vet et al (2014) paper (see Figure 1 bottom).

We don't know about other modeling papers, but in our work the comparison with available data for the baseline deposition is already made [see Fig. 13, in particular; but also: Table 2; Section 1, page 3 lines 17-18; Section 4.3, page 28 lines 1-12; see also Section 4.3, page 30 lines 1-13]. By the way, we made such a comparison exactly with values from Vet et al. (2014), who give estimates (as stated in lines 4-5, page 28) that rely on both a multi-model intercomparison and available observations.

They should also produce a figure for the baseline deposition results so that the effects of the injection could be compared with absolute values.

Fig. 13 does already show the baseline deposition results integrated over the different regions of the globe (both land and oceans) and we believe it should not be a major problem for the commenter to take the integrated values expressed in Tg-S/yr, divide them by the surface area of the specific region and obtain the deposition flux in the same units as in the figure presenting the geoengineering changes (Fig. 11). Should it? Anyway, for the sake of "graphical completeness" in the supplementary material of the revised manuscript we will also provide the lat/lon map in units of mg-S m^{-2} yr^{-1}.

As a matter of fact Kravitz et al. (2010) paper shows for a 2.5 Mt S injection a deposition which is comparable to the present observed acid deposition in regions of Europe, Asia and North America (see attached Figure 1). The suspicion is that this paper has similar results (increase in areas deposition rate up to 15%).

This comment is contradictory. First of all, the commenter's attached Figure 1 shows indeed that the sulfur deposition in regions of Europe, Asia and North America increases up to 15%. To us this is not "comparable to the present observed acid deposition", but one order of magnitude less! Said that, why should he write "suspicion", when our paper clearly shows the relative deposition changes in Fig. 11, Fig. 14 and also in Table 5? (with no mystery at all…).

If this is so it means that injecting sulfur in the stratosphere would produce an acid deposition similar or greater to the present one especially for the envisioned large injection rates at the end of the century (Kravitz et al., 2017).

As replied above, an injection of 2.5 or 4 Tg-S/yr in the tropical lower stratosphere does not produce an acid deposition similar or greater to the present one, anywhere in the globe (but typically lower than 15%, or even much lower). Different is the case for larger envisaged injections. Sadly, the Kravitz et al. (2017) paper came out after we submitted this, so we could not discuss it in our conclusions. However, we already planned to discuss their results in the revised manuscript (along with those from the other companion papers, such as Tilmes et al., 2017 and Richter et al., 2017).

By the way they refer always to Kravitz et al. (2009) paper ignoring the correction to the same paper (Kravitz et al., 2010).

We are, of course, aware of the correction. However, as the authors state in the correction, the mistake they made "does not change the conclusion that all but the most sensitive, pristine areas of the world have significant buffering capacity against additional sulfuric acid that would result from geoengineering.". Indeed, our comparison with the results from Kravitz et al. (2009) is mainly done with their Figure 3 as presented in their Correction. We agree with the commenter that we should mention in the paper the presence of the Correction itself and we shall do so in the revised manuscript.

If they really want to show the effects of QBO they could make this comparison with the QBO on and off in their model and again make a comparison with experimental data.

We disagree with the commenter on this. Our point is not to discuss the presence of the QBO in a baseline scenario, regarding base sulfate deposition. As we point out in page 14, line 6, this has been studied in Hommel et al. (2015) and is not the point of our paper. As they point out, the amount of baseline stratospheric sulfur is so low and the particles so small that the effect is difficult to constraint. The point we try to make in our paper is that the added sulfur, which produces much larger sulfate particles than the ones already present, is rather sensible to the QBO wind shear. Furthermore, the different confinement produces different dynamical effects that we showed in Fig. 5, Fig 7 and lastly in Fig. 12, where the deposition of the added sulfate is analyzed during the two different QBO regimes. This also means that for greater injections (like the ones discussed in Kravitz et al., 2017) which are capable of significantly impact the QBO, the deposition would not follow the pattern shown in our paper and in Kravitz et al. (2009-2010), but would be more localized in the tropical regions (see Fig. 12). However, as shown in Richter et al. (2017), this QBO modification could be reduced if the sulfur injection is not made at the equator, but closer to the subtropics.

Beside this question of QBO is quite peculiar. QBO (like AO or PDO) should be an intrinsic feature of any general circulation model and not introduced with a specific routine in the model. The authors should explain such characteristic.

For a discussion of the methods used in the CCM family to treat the QBO, including an external nudging procedure, we suggest the commenter to read Morgenstern et al. (2010) and Morgenstern et al. (2017), both cited in our paper. We agree that having an internally generated QBO would be ideal (for inclusion of the feedbacks), however our externally nudged QBO produces results that, when compared to available observations performs rather well: see for instance Visioni et al. (2017b), where a validation of ULAQ-CCM with available CH_4 and N_2O data (from HALOE and TES) is presented, or Pitari et al. (2016a) and Pitari et al. (2016b), where the

ULAQ-CCM results are compared against available observations for past volcanic eruptions of aerosol optical thickness (against SAGE-II and AVHRR measurement), w* (against MERRA reanalyses) and age-of-air (against measurements available in Strahan et al., 2011, Andrews et al., 2011 and Engel et al., 2009). We take the liberty to cite here an intelligent comment of the anonymous reviewer 2: "I particularly appreciate the fact that the paper is trying to isolate the effect of the QBO on sulfate geoengineering in the absence of feedbacks, which provides insight which might be lost or obscured in a model with a fully interactive QBO".

In light of this (and considering that, as opposite to what the commenter states in the first line of the comment, we always provide in our studies any form of evaluation based on available observations), we disagree with the commenter when he states that a certain feature SHOULD be an intrinsic feature of a GCM. Our model, as many CCMs, doesn't have such an intrinsic feature (i.e. internally generated QBO), but uses a nudged QBO from the observed time series of equatorial winds, and still performs in a reasonable way when compared to observations. Furthermore, in this particular case, having an externally nudged QBO allows us to separate the different effects of the stratospheric sulfur injection in a way that makes it possible to provide useful information regarding the stratospheric distribution of the aerosols and the strat-trop exchange under geoengineering conditions, which then relates to the zonal deposition of sulfate.