

## ***Interactive comment on “The role of carbonyl sulphide as a source of stratospheric sulphate aerosol and its impact on climate” by C. Brühl et al.***

### **Anonymous Referee #2**

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This paper is hard to assess, because it is an easy read. It describes interesting research into COS and its climate impact, presenting some comprehensive model results, some general thoughts and some back of the envelope calculations regarding COS abundance in the atmosphere and its radiative forcing. It never provides a clear main motivation and it is sometimes hard to understand what is derived “back of the envelope” and which statements use part of the comprehensive model results. In addition the paper is missing some coherence. Facts discussed are not supported by figures (strong seasonality of COS, p20831), and figures shown (figure 9) are actually not discussed in the text (apart from the global mean). The model description is vague and requires some further explanation. The impression is given that the model

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produces its own QBO, yet real dates are referred to. Was the model nudged or initialised from a nudged run and are phase lags accounted for? Assertions are made about superimposed SO<sub>2</sub> injections, but no results are shown or seem to exist in the literature yet. With respect to COS it is hard to find out what the boundary and initial conditions are. I assume the mentioned COS measurements have been interpolated and assembled into a geographical map (with seasonal dependence) of mixing ratios that are prescribed at the surface/throughout the boundary layer? And what are the initial conditions (or the spin-up procedure)? The discussion starts with figure 1 showing COS in different phases of the QBO. What are the phases and why is September 2000 shown? Phases of the QBO are referred to as different or “another QBO phase”(p20832) – why not start the discussion with a timeseries and let the readers know what the QBO phases are and what would be expected in terms of interannual variability (or seasonality respectively)? Why not use March 2001 throughout when there is no observational data for September 2000? I think many interesting aspects are touched on and I believe the paper will be suitable for publication, but some important revisions are required to improve coherence and comprehensibility.

P20825, l15: A good place to let the reader now about the ECHAM Pinatubo studies; please provide a citation.

P20826: Please provide a rational/explanation for/of the link between the different sections here. Is section 2 providing the open questions? What question is the comprehensive model study addressing? How does the comprehensive model help the back of the envelope GWP calculations? How is the model RF defined (which is as far as I can tell the only model quantity used in the GWP estimates)? A clear roadmap would be helpful for the reader!

P20830, l6-19: What does this paragraph add to the following discussion? How can a superimposed injection correspond to a distribution? The model will surely establish its own distribution (from the injection), which may or may not agree well with observations.

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P20830, I25: I guess this confirms the obvious ... Please see my general comment above about describing the model setup better.

P20831, I9: How is the global air mass flux defined (I guess it is actually tropical and not global)? Is it based on the residual circulation in low latitudes or on a control tracer relative to the tropopause or ...

P20831, I9: A long discussion about regional differences follows; why not illustrate those with a figure?

P20831, I24: Figure 1 is introduced (again) but seasonality (not shown) is highlighted.

P20831, I11: Some more information? Is this important?

P20832, I23: A clear distinction should be made between QBO and seasonality (see above). Section 4: What information (if any) is taken from the model simulations (apart from the mean radiative change, see below)? The section seems to rely on the formula by Roehl et al. and some assumptions about lifetimes (not from the model). So the end result seems to be a mixture of a short-time ECHAM integration result (what precisely is shown in Figure 9?) complemented with back of the envelope lifetime assumptions.

P20836, I20-23: Very confusing! Which numbers refer to what?

Table 1: I agree that it is useful to incorporate the table, but the authors should try to add value to it by editing their discussion points in (highlighting, another column with alternative values, etc.).

Figure 1: The difference is just visible; maybe absolute values and differences would be more useful. To set the scene for QBO and seasonality this is not the most useful figure to go first.

Figure 6: Better side-by-side on the same altitude scale.

Figure 9: I am not convinced this figure is needed, given the discussion provided.

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