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Interactive comment on "Determination of the atmospheric lifetime and global warming potential of sulphur hexafluoride using a three-dimensional model" *by* Tamás Kovács et al.

Anonymous Referee #2

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This paper is a model assessment of the atmospheric lifetime and global warming potential of sulphur hexafluoride (SF6). It relies on the use of a rather unique state-of-the-art 3-D chemistry-climate that is able to represent the key processes controlling the SF6 atmospheric cycle up to 140 km. The present work is a very substantial improvement on previous SF6 assessments. The main outputs are new updated values for SF6 atmospheric lifetime and GWP that are much more reliable than previous estimates. Using sensitivity simulations, the authors are also able to explain most the differences between previous values. Finally, they demonstrate how the SF6 loss by electron attachment in the mesosphere affect the SF6-derived mean stratospheric age-of-air (AoA) in polar regions. This should be taken into account when deriving AoA from

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Discussion paper

SF6 measurements. It is clear and well written. The methodology is solid. The title and abstract adequately represent the content of the paper. I do not have any very significant comments/corrections and therefore I recommend publication. I just provide below some minor suggestions that the authors may wish to consider.

L66: “denote the reference latitude and altitude which are chosen to be the upper tropical troposphere (latitude = 1oN, altitude = 13.9 km)”. could the authors justify the choice of the latitude and altitude of the reference location, even if it is irrelevant?

L146: “Note that the SF6- anion is not modelled directly. Instead the SF6 attachment loss rate is calculated by multiplying kat by the probability of permanent destruction of the resulting SF6- (reactions of SF6- with H and HCl) to the sum of these reactions and processes which recycle SF6- to SF6 (reactions with O and O3, and photodetachment) (Morris et al., 1995).” Not very clear. And the reference give (Morris et al., 1995) cannot provide much explanations as Morris et al. assumed that the associative attachment forming SF6- does not regenerate SF6 (see L278). This could be developed in an annexe or rephrased. My understanding is that: Because of its very short lifetime, SF6- can be calculated from is photochemical steady-state expression and not transported. Therefore, replacing SF6- by its steady expression in the SF6 continuity equation, it can be shown that the net effect of electron attachment on SF6 (net because some of the SF6 lost by electron attachment is recycled back via SF6- reactions with O and O3, and photodetachment) can be calculated as the SF6 attachment loss rate multiplied by the probability of permanent destruction of the resulting SF6-; this probability is taken as the ratio of SF6- reactions rates with H and HCl over the total SF6- loss rates (i.e. SF6- reactions with H, HCl, O and O3, and photodetachment).

L251: “This model tracer can be compared to the MIPAS observations in Figure 5f, which shows that WACCM agrees reasonably well with the measurements in the lower stratosphere”. “reasonably well” seems to be a bit strong and not fully consistent with the following sentence. Perhaps, something like WACCM appears to reproduce the general features of the MIPAS distribution. However, it also clear that. . .

L269: “being anti-correlated with the solar radio flux at 10.7 cm”. Even though most solar activity indices are strongly correlated, the more meaningful indices for electron density are solar fluxes at much shorter wavelengths (e.g. X-rays, Lyman alpha). Make the statement more general: being anti-correlated with solar activity, for example as measured by the radio flux at 10.7 cm. . .

L228: “As is clear from Figure 3, the model simulation and satellite observations agree within the atmospheric variability, which becomes relatively large above 30 km especially at high latitudes, although the model is systematically larger than the observations above 20 km.”. The two parts of this statement seem to be partly contradictory or, at least, confusing. Usually, when observations and models are compared, it can be done either on the mean (climatology) or the variability (standard deviation around the mean). For example, a model can reproduce the observed variability but have a bias on the mean. And the reverse is also possible. I guess the statement “the model simulation and satellite observations agree within the atmospheric variability” is meant to be applied to the mean. In that case, as stated in the second part of the statement, there is a clear systematic bias in the mean here and the fact that there is a large variability (standard deviation) with bars covering model-calculated is not relevant here. If the targeted quantity for the comparison is the mean, the relevant uncertainty is the error on the mean (which depends on the sigma but not only, it depends also on the number of profiles) and the same thing should be done on the model-calculated profiles. If observed and model profiles with errors on the means are partly covering each other, one can then claim that obs and model agree within the errors. But I guess that is not the case here. I would suggest to state simply that the variability is high and that there is a systematic bias. If the authors want to get into the variability, they can calculate in their model. I don’t think it would bring anything valuable here.

L287: “Our estimated partial lifetime of SF6 due to photolysis for the SF6 tracer which includes all loss processes is 48,000 yr”. SF6 due to photolysis but SF6 tracer includes all loss processes? Not clear. Rephrase.

L411: “They established the range of published radiative efficiencies for SF_6 to be 0.59 – 0.68 $Wm^{-2}ppbv^{-1}$, with a mean value of 0.56 Wm^{-2} $ppbv^{-1}$ “ The mean value does not correspond to the range. For instance, just above is cited the value of Myhre et al., (2013): 0.57 Wm^{-2} $ppbv^{-1}$. The range needs to be corrected.

The quality of Figure 8 can be improved.

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