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Interactive Comment

# *Interactive comment on* "The imprint of stratospheric transport on column-averaged methane" *by* A. Ostler et al.

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While the main aim of this study – to improve the quantification of the methane budget through better understanding of stratospheric transport – is highly interesting and valuable, I do have major concerns about the way this has been done here. I agree that it is generally a very good idea to constrain models with observations, but not in this particular case. The observational data used here is not only outdated but also partly unsuitable. As outlined in section 3.1 the authors rely on very little data from only a few balloon launches (e.g. on a single flight in mid latitudes):

"The observed mean age dataset used in the current study consists of 7 vertical profiles of SF6 obtained at three different locations in the Northern Hemisphere (NH) at

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altitudes between 17 and 37 km (see Table 1). One profile was presented in Patra et al. (1997), the remaining profiles are part of the study from Harnisch et al. (1996)."

As shown in Table 1 these flights were all carried out between 1987 and 1995, so at least 20 years ago. Mean age derived from SF6 measurements was much less well quantified then, e.g. because the tropospheric mixing ratio trend of SF6 was much less well known at the time. This is particularly the case for the earliest flights and leads to large uncertainties in mean age that are not considered here or in the original papers.

In addition it is a well-known fact that SF6 produces biased mean age in high latitudes during winter time as it has a significant mesospheric sink (e.g. Strunk et al., GRL, 27, 3, 341-344, 2000; Waugh et al., Rev. Geophys., 40, 4, 2002; Engel et al., Atmos. Chem. Phys., 6, 267–282, 2006). It should therefore not be used to quantify mean age in high latitudes in particular during winter and early spring, but the four flights considered here were all carried out between January and March. Unsurprisingly the authors find the largest age differences between the earlier and the later version of the model (and therefore the largest methane differences) in high latitudes. Looking up Harnisch et al., 1996 they report mean ages of up to 10 years in the high latitude stratosphere which is known to be completely unrealistic. And looking at Figure 1 of this work the biggest changes in mean age appear to be in high latitudes close to the 100 hPa level. This is very close to the tropopause which has a mean age of zero (or at least close to zero) by definition. Are the authors suggesting that the air close to the arctic tropopause has previously been estimated to be three 3 years too young?

Finally, what I find very puzzling is that the authors have not used any other available mean age data (e.g. Diallo et al., Atmos. Chem. Phys., 12, 12133–12154, 2012; Engel et al., Nat. Geosc. 2, 2009), especially not the most extensive data set in existence which has been published by their fellow KIT scientists in Stiller et al., Atmos. Chem. Phys., 12, 3311–3331, 2012. I hope that the authors will consider these points and that this will lead to a much improved estimate of the impact of stratospheric transport on column-averaged methane.



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Best regards,

Johannes Laube

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