Replies to Reviewers

Again, we would like to thank the two referees for useful comments which have helped improve the manuscript. Our responses appear in black below.

Reviewer 1

The authors have done a credible job of re-explaining and/or revising the manuscript in response to the extensive reviews. There is one point for which our viewpoints remain at right angles and one new curiosity that I found when reading the revised manuscript.

The authors' insistence on using air-mass weighted OH as a key variable is still fundamentally wrong and they know better. They know that the k(OH+CH4) and OH are unusually correlated and thus the product of the means as used here is clearly not equal to - and may not behave like - the mean of the product, which is related to the CH4 lifetime. At this stage, they do not want to redo all they fitting exercises and as a reviewer, I have done what I can. It should be published, but unfortunately, some of the analysis will not be as helpful or useful for future model comparisons.

As appears in the manuscript, the methane production from H2, 'a' is calculated using the following equation:

a = CH4 * k(CH4+OH) * dOH/dH2

If we use OH weighted by CH4+OH rather than airmass to determine both k(CH4+OH) and dOH/dH2, we determine different values for k(CH4+OH) and dOH/dH2 individually, but the effects cancel and our value for 'a' remains unchanged. dOH/dH2 doubles and k(CH4+OH) halves. To aid future model comparisons, we now also quote values for k(CH4+OH) and dOH/dH2 where OH has weighted by CH4+OH, in addition to the values based on mass-weighted OH.

The odd curiosity that I just noticed has to do with Figure 1D and discussion (~L240-245). Ignoring the CH4 feedback cases (dashed lines), one would expect that the tropospheric H2 injected at the tropopause will fall off like CH4 in the stratosphere and be converted to H2O by 50 km as observed (the lower bump in H2 being from CH4 production). This looks fine for the 750 ppb H2 case where the water increases as expected by (0.750 – 0.500) = 0.25 ppm! (light blue line). For +1000 ppb H2, however, the increase is half that expected (0.38 vs 0.50, orange line); and for the 2000 ppb H2 we barely get +1.2 ppm H2O instead of +2.0 ppm. What is going on with the sudden shift in H2O yield from H2? The authors might want to add a small comment on this.

In our model simulations we do not get exactly 1 ppm of water vapour for every 1 ppm of H2 added to the model (we get less). However, we are not as far off this value as the reviewer suggests above as in the 2000 ppb H2 simulation, H2 has been increased by 1.5 ppm rather than 2.0 ppm. For this simulation, we get ~+1.2 ppm H2O for +1.5 ppm surface H2. We have added the following sentence to the manuscript 'Note that in our simulations the maximum increase in stratospheric water vapour (occurring at ~55 km) is slightly less than 1 ppm per 1000 ppb increase in surface H2.'

Reviewer 2

The revised paper is improved but I am still unhappy with some aspects:

l211-217 It is a bit odd to compare a methane lifetime with respect to OH with a total lifetime (i.e. with respect to OH, Cl, soil, and stratospheric sinks), given that it is straightforward to inter-convert between these two lifetimes, with simple assumptions about the Cl, soil and stratospheric lifetimes (e.g., see Prather et al., 2012; Stevenson et al., 2013). Please just straightforwardly compare like with like, and when referring to "the methane lifetime" (l217) be explicit about whether this is the total lifetime or with respect to OH. This is important as it is a major source of confusion in the literature, so all publications should be crystal clear about this to reduce this confusion.

l274 Close brackets

Done.

l300 CO2 emissions (I don't think we are anticipating reductions in CO2 concentrations anytime soon).

Done.

l343 The earliest publication with a GWP for H2 to my knowledge is Derwent et al (2001), so it would seem appropriate to reference that here.

We have added a reference to Derwent et al. (2001) here.

l470-477 The methane feedback factor is a strictly defined thing, and it uses the total methane lifetime with respect to all sinks. You can't just optionally define it differently! This is closely related to the first point above. Both reviewers queried the strangely high value for the methane feedback factor used - the authors have clarified why it was high (they calculated it using the incorrect methane lifetime) - they should just do this properly, and not seed confusion in the literature. It has only a minor impact on the final result.

We have removed reference to more than one methane feedback factor for UKESM and only quote the value with respect to all sinks.

Fix these points and I will be happy.