Response to comment from Reviewer #2

General comments

1. "The paper largely focuses on interpreting the multi-model responses. While this is of course useful, it stops short of relating the new understanding to any real-world changes in SWV... How much does this work help in understanding past and possible future SWV changes?"

"In particular, note there has been some discussion of how the PDRMIP BC perturbations compare to observations (Allan et al, https://doi.org/10.1038/s41612-019-0073-9)..."

We will add a new figure and associated discussion to the paper. Figure R1 below (it will be assigned a different number in the revised manuscript) shows an analysis similar to that shown in Fig. 6 of Hodnebrog et al. (2019). We estimate historical ΔSWV in the TLS between 1980-2010 by multiplying the multi-model multi-experiment mean sensitivity of the TLS ΔSWV_{slow} to surface temperature change by the historical surface temperature change to estimate the slow component of the historical ΔSWV. We multiply the multi-model mean TLS ΔSWV_{fast}/ERF by the historical radiative forcing for each selected PDRMIP experiment and sum it up to estimate the fast component of the historical ΔSWV. The fast component of the historical ΔSWV contributed by each historical forcing agent is shown in Fig. R1b below. The historical surface temperature change and radiative forcing data used in this analysis are listed in Table R1 below.

This calculation suggests that forcing since 1980 has increased TLS SWV by 0.51±0.16 ppmv (Fig. 1a). 64% of this is due to the fast response, mainly from black carbon. 36% is due to the slow response, although this is probably an overestimate because our sensitivity values are for long-term.

We have also evaluated the SWV sensitivity and SWV fast response over 35°N-45°N between 100-80 hPa to re-compute the 1980-2010 SWV change using the same method, which is 0.65±0.20 ppmv. This value shows reasonable agreement with the SWV increase measured by Hurst et al. (2011) of 0.71±0.26 ppmv over Boulder between 16-18 km over 1980-2010, although one must not read too much into the comparison to mid-latitude measurements.

Dessler et al. (2014) and Hegglin et al. (2014) argue that there is not a detectible trend over this period. Such a conclusion is not inconsistent with ours because any actual trend estimate has to contend with short-term interannual variability (i.e, like that from the QBO and Brewer-Dobson Circulation variability), which can mask a small trend. Our estimate of the trend is for long-term (100 year-run) and therefore internal variability has a small impact.

For the fast component of the estimated historical ΔSWV, radiative forcing by BC plays the dominant role (Fig. R1b below). As pointed out by the reviewer, the radiative effect by BC in the PDRMIP is different from that shown in models using observationally constrained aerosol forcing, which may overestimate the heating in the UTLS region (Allen et al., 2019). However, Allen et al. (2019) also noted that uncertainties exist in their observationally constrained aerosol forcing. There are also uncertainties in the historical BC forcing listed in IPCC AR5 (Myhre et al. 2013). These bring in uncertainties in the fast component of the estimated historical ΔSWV by PDRMIP. These clearly merit more discussions in the future. We'll add this discussion about limitations and uncertainties of our PDRMIP estimates in the Summary and Discussion section.

Table R1: Historical global average surface temperature change and radiative forcing (RF) by Greenhouse gases (GHGs) and halocarbons over 1980-2010.

GMST[a] (K)	0.506
Total ΔSWV (ppmv)	0.51
ΔSWV_{slow} (ppmv)	0.18
ΔSWV_{fast} (ppmv)	0.32

Forcing agents	RF (Wm^{-2})	ΔSWV_{fast} by each forcing agent (ppmv)
CO_2[b]	0.715	0.007
CH_4[c]	0.055	0.008
BC[d]	0.3	0.286
CFC-12[e]	0.068	0.015
CFC-11[f]	0.015	0.004
N_2O[g]	0.042	0.004

a: We used NOAA Merged Land Ocean Global Surface Temperature Analysis (NOAAGlobalTemp) V5 (Zhang et al. 2020) to compute the global surface temperature change. We use values averaged over 2005-2015 minus that averaged over 1975-1985.

b, c, e, f, and g: We compute the RFs using the formulae listed in Table 3 of Myhre et al. (1998). These formulae were also used to compute RFs of CO_2, CH_4, and N_2O in IPCC reports (Myhre et al. 2013).

b, c, and g: Concentrations of GHGs were used to compute RFs. We use the CO_2, CH_4, and N_2O samples collected in glass flasks at Cold Bay, Alaska, United States (CBA) from the ERSL GML website (Dlugokencky et al. 2020). For CO_2, concentrations averaged over 2005-2015 and averaged over 1978-1985 are used. For CH_4, concentrations averaged over 2005-2015 and averaged over 1983-1985 are used. For N_2O, concentration averaged over 2005-2015 was used, and 310 ppb (The IPCC 1990 and 1992 assessments) was used as a substitute for 1980 concentrations due to lack of data.

e-f: Concentrations of CFC-12 and CFC-11 were used to compute RFs. We use CFC-12 and CFC-11 data from combined stations from the NOAA/ESRL Global Monitoring Division. Concentrations averaged over 2005-2015 and averaged over 1977-1985 are used.

d: We use 0.4 Wm^{-2}, the BC RF between 1750-2011 reported in IPCC AR5, minus 0.1 Wm^{-2}, the BC RF between 1750-1993 reported in 1995 IPCC report (See Table 3 of Myhre et al. 2013). Note uncertainties for the IPCC AR5 BC RF is 0.05 to 0.8 Wm^{-2}, and uncertainties for the 1995 IPCC BC RF is 0.03 to 0.3Wm^{-2}.

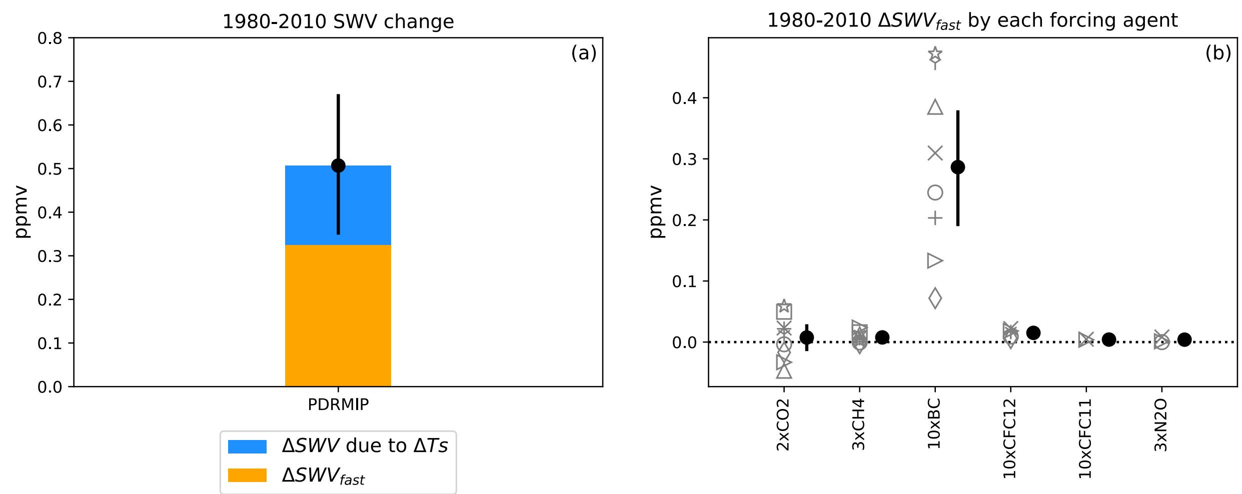

Figure R1: (a) 1980-2010 TLS (30°S-30°N, 70 hPa) SWV change estimated using PDRMIP results. See texts above for method description. The uncertainty for the PDRMIP estimated SWV change is the sum of the uncertainty in the slow component and the uncertainty in the fast component. (b) The fast component of the PDRMIP-estimated 1980-2010 SWV change contributed by each historical forcing agent.

2. "As noted in the specific comments, I feel that there is inadequate recognition that some of the results presented here are also presented, either explicitly or implicitly, in some earlier papers from the PDRMIP group – this is particularly so for the ERFs where no reference to, or comparison with, those earlier results, is given."

Thanks for pointing this out. We'll add references to related results from earlier PDRMIP studies in the revised paper. Please see the responses to specific comments below related to previous PDRMIP studies:

"47-48: There is a slight overlap between this submitted paper and the paper published in ACP by the core PDRMIP team – Hodnebrog et al: https://doi.org/10.5194/acp-19-12887-2019, which is not cited here…"

We will reference results from Hodnebrog et al. (2019) in the revised paper.

"129: I think it is necessary that a comparison of ERFs (and the associated feedback parameter) with Richardson et al. (including for the CFCs and N2O in their supplement) is presented both to confirm they are in reasonable agreement and also to make clear that the ERFs derived here are not original work with the PDRMIP output."

We will add a statement making it clear that we are not the first to calculate ERFs with PDRMIP output. Table R2 (below) compares multi-model mean ERF with those listed in Richardson et al.

(2019); clearly, this comparison shows good agreement. In the revised manuscript, we will add this table comparing our values to Richardson's to the supplementary material.

The ERF in the submitted manuscript is computed using the same method as the "ERF_{sst}" in Richardson et al. (2019), so we directly compare multi-model mean values with their ERF_{sst}.

Table R2: Comparison of multi-model mean values with Richardson et al. (2019). In the parentheses, we list 95% confidence intervals obtained from Monte Carlo samples as described in Section 2.2 of the submitted manuscript for experiments that are performed by 6 or more models.

	$2xCO_2$	$3xCH_4$	2%Solar	10xBC	$5xSO_4$	10xCFC-12	10xCFC-11	$3xN_2O$	$5xO_3$	10xBCSLT
ERF_{sst} (Wm^{-2}) from Richardson et al. (2019)	3.71±0.30	1.15±0.25	4.17±0.13	1.18 ±0.75	-3.71±1.94	1.39 (1.21 to 1.54)	1.19 (1.17 to 1.21)	1.60 (1.23 to 2.14)	3.47 (2.45 to 4.49)	1.10
ERF (Wm^{-2})	3.75 (3.58 to 3.92)	1.20 (1.02 to 1.38)	4.23 (4.15 to 4.33)	1.21 (0.68 to 1.93)	-3.68 (-5.28 to -2.63)	1.35	1.17	1.80	3.45	1.21

Specific comments

"14: This conclusion is specific to the TLS"

Yes, we agree with this, although the cold point temperature does have *some* influence in the lowermost SWV(Dessler et al., 1995). But the control is not as strong as that in the TLS (see Fig. 6b-c in submitted manuscript) and the lowermost SWV is controlled by multiple factors. We'll edit the text in the revised manuscript to make sure there is no confusion.

"16: "becomes weaker at higher altitudes and at higher latitudes below 150 hPa." This is a bit ambiguous. Does this means heights at pressures below 150 hPa or heights below the height of the 150 hPa surface. These would have opposite meanings."

It means altitudes below the 150 hPa surface. We'll edit the text in the revised manuscript to avoid confusion.

"57: Presumably the 3xCH4 experiments have no resulting change in SWV due to the oxidation of additional methane?"

Yes, indirect chemical effects are not included in the $3xCH_4$ experiment. We will add a sentence saying this to the revised manuscript.

"90 and many other places: There are repeated statements that there is no surface temperature response in the fixed SST runs, but this is not correct, with implications for the definition of ERF."

Yes, the reviewer is correct that land surface temperatures can respond to the forcing. We'll edit the text in the revised manuscript and make sure there is no confusion.

"139: tend to be larger" Isn't it clearly larger?

Yes, the reviewer is correct. We'll edit this text in the revised manuscript.

*"*148 and throughout: Rather little is said about intermodel differences. For example, on HADGEM3, more discussion of its apparent outlier status on some plots seems necessary. The text says it is "likely connected" to the larger surface warming, but it seems the climate sensitivity is about double the multi-model average but the slow SW response is around a factor of 4 larger. Is that because the TTL temperature change is 4 times higher (per unit ERF)?"*

To answer the question about the HadGEM3 model, Figure R2 below shows the equilibrium slow response of TTL temperature (ΔT_{slow}) per unit ERF. The HadGEM3 ΔT_{slow}/ERF is between 2.64-3.97 times the multi-model mean ΔT_{slow}/ERF for experiments 2xCO_2, 3xCH_4, 2%Solar, 10xBC, and 10xCFC-12. Figure R3 below shows the vertical profile of tropical atmospheric temperature slow response (ΔT_{slow}) for 2xCO_2. All models show a maximum warming in the upper troposphere. Since the surface warming in HadGEM3 is larger than all other models, its upper tropospheric warming is also largest. Longwave radiation emitted from the upper troposphere warms the TTL level (Lin et al., 2017), so the larger upper tropospheric warming in HadGEM3 also results in larger TTL heating than other models. The relationship between surface warming and TTL warming is not linear.

That said, we cannot conclusively identify a cause given the information archived. So we will remove the claim that the difference is likely connected to surface warming and add a sentence saying more work on the causes of these differences is warranted.

"Another example is that apparently half the models have a slow SW response to BC of the opposite sign (Fig 1a) to the multi-model mean. Is there any obvious reason why? As far as I can see BC causes a warming in all models."

Figure R4 below shows the vertical profile of tropical temperature slow (a) and fast (b) responses per unit ERF for the 10xBC experiment. The 10xBC does cause a warming at the surface and in the troposphere due to a positive TOA ERF in all models. In the TTL and lower stratosphere (LS), however, the heating is mainly caused by the fast adjustment (Fig. R4b below). The slow temperature response in the TTL is the residual of the total response minus the fast adjustment, which is negative or close to zero (Fig. R4a below).

It is therefore our contention that some of these negative values are artifacts of the method we use to estimate equilibrium response. Support for this comes from Fig. 3 of the paper. The values in this figure come from regressions of ΔSWV_{slow} vs. ΔTs in the BC runs. This method does not require differencing two large numbers, so we feel it is more robust. It shows that most models have a positive response of SWV due to BC-induced warming. For those models that produce negative slopes for ΔSWV_{slow} vs. ΔTs in the BC runs, there is large uncertainty in the regression, because the surface temperature change in those models are small. We will note this in the revised manuscript.

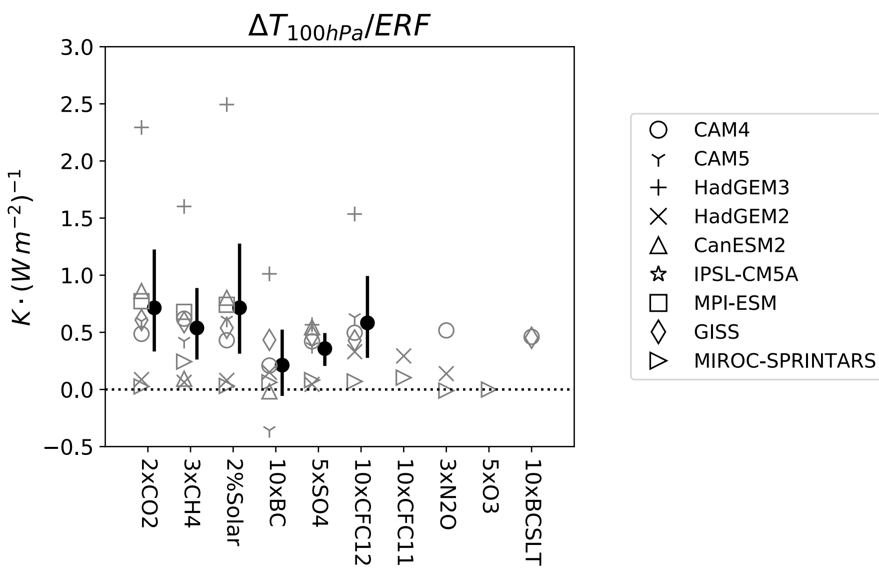

Figure R2: Equilibrium slow response of TTL temperature (100 hPa, averaged between 30°N-30°S) per ERF for all models and perturbations.

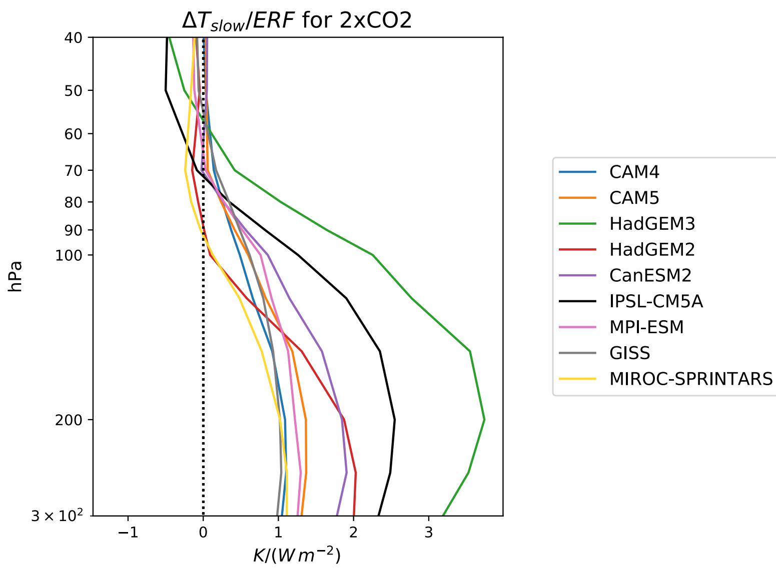

Figure R3: Vertical profile of atmospheric temperature equilibrium slow response normalized by ERF (K·(Wm^{-2})$^{-1}$) for 2xCO_2. Temperature is averaged over 30°N-30°S.

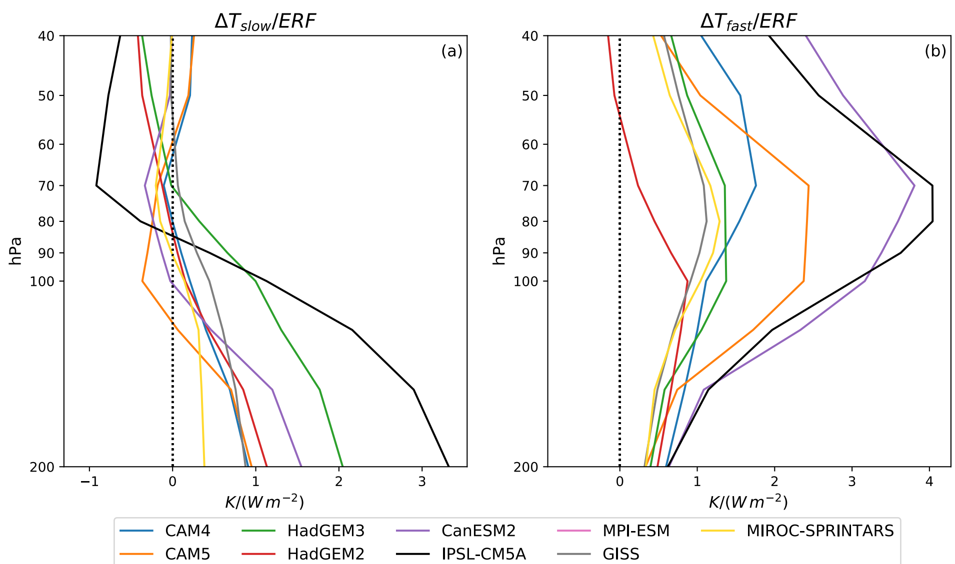

Figure R4: Profiles of equilibrium slow (a) and fast (b) temperature response for the 10xBC experiment, normalized by ERF (K·(Wm^{-2})$^{-1}$), and averaged over 30°N-30°S. The color coding indicates results from different models.

"One thing I miss from this study, and encourage the authors to look at if they have the resource, is the degree to which the model's background climatology of stratospheric water vapor or TTL temperature could explain some of the intermodel differences."

We have investigated the SWV in the fixed SST baseline simulations. Based on our analyses, the baseline climatology SWV does not explain the inter-model differences in the responses to forcing agents. As an example, Fig. R5 below shows the TLS SWV slow response (first row) and TTL temperature slow response (second row) vs. the baseline TLS SWV climatology and baseline TTL temperature climatology. We omitted 3xN_2O, 5xO_3, and 10xBCSLT, because fewer than three models performed these experiments. There is no correlation between the SWV and temperature slow responses and the baseline climatology. In particular, HadGEM3 produces extremely large slow responses for most experiments, however, in Fig. R5 below, its baseline SWV and temperature climatology is not the largest among the models.

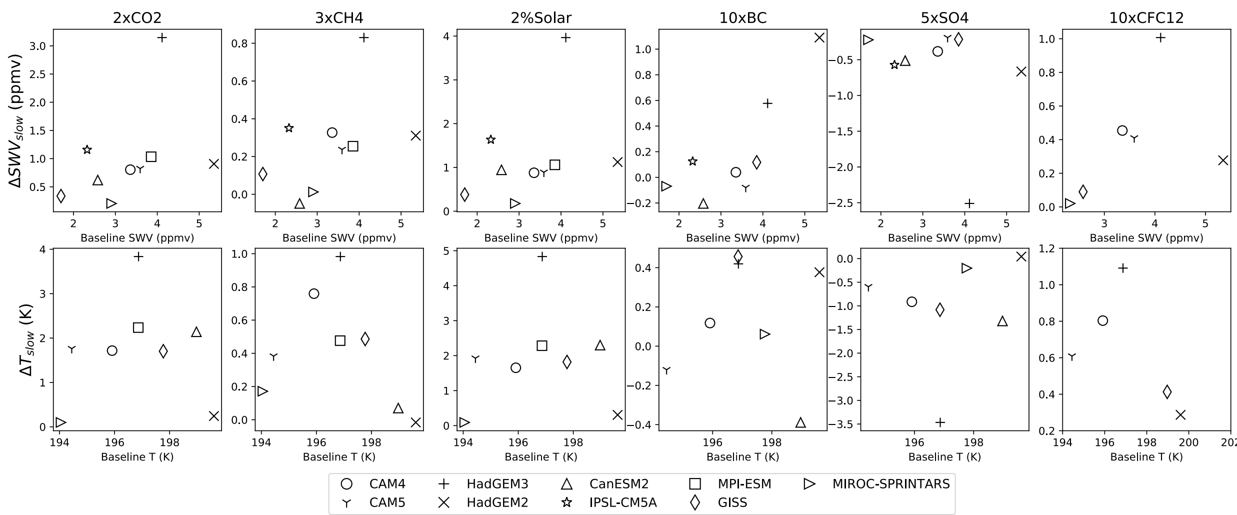

Figure R5: Top row: The TLS SWV slow response (ppmv) vs. the baseline TLS SWV climatology (ppmv). Bottom row: The TTL temperature slow response (K) (100 hPa, averaged over 30°N-30°S) vs. the baseline TTL temperature climatology (K) (100 hPa, averaged over 30°N-30°S). The baseline climatology is obtained from the fixed SST simulations averaged over the last 10 years.

"156: Is this linear regression done once across all simulations and all perturbations. If not, I am unclear which perturbations have been used for the regression."

We regressed the annual mean ΔSWV_{slow} time series vs the annual mean ΔTs for each perturbation and model separately. We showed the scatter plot for each perturbation and model in Figs. S1-3 of the submitted manuscript and showed the slopes of the regression done for each perturbation and model in Fig. 3 of the submitted manuscript.

"159: This is a relatively short paper and I wondered whether the supplementary figures could be brought into the main text?"

It remains our opinion that the key figures are included in the paper. Thus, in order to keep the take-home message concise, we have left the content of the supplement unchanged.

"171-172: This repeats a point already made at 141-142."

We'll edit the text in the revised manuscript to make sure there is no such repetition.

"201: I am sorry if I miss it, but I see very little discussion of stratospheric temperature changes in the Jain et al paper. The role of CFCs on the vertical profile of temperature can be seen in many papers such as Forster et al. https://doi.org/10.1007/s003820050182 and Forster and Joshi 10.1007/s10584-005- 5955-7…"

In the submitted paper where we discussed the radiative heating in the UTLS by CFCs, we were referring to the text in Section 3.3 of Jain et al. (2013), where they stated that "Halocarbons absorb predominantly in the window region (750-1250 cm^{-1}), in the linear line limit; therefore in the stratosphere they absorb the upwelling radiation from the troposphere and increase the heating rate of the stratosphere". Forster and Joshi (2005) also pointed out that, compared to CO_2, halocarbons preferentially warm the TTL, rather than the troposphere.

We agree with the reviewer that it is useful to reference papers that explicitly investigated vertical temperature profiles forced by CFCs. We'll add these references in the revised manuscript.

*"*204: This statement on shortwave radiation is strange. There may be a small shortwave effect from the reduced reflected flux from the troposphere, but there is a long history of simulations that clearly attribute the stratospheric cooling due to increased tropospheric ozone to the decreased upwelling thermal infrared radiation. E.g. Ramaswamy and Bowen https://doi.org/10.1029/94JD01310, Berntsen et al https://doi.org/10.1029/97JD02226 and the Forster et al. paper referred to above."*

Thanks for pointing this out. We'll edit the text to say, "Increases of tropospheric O_3 (5xO_3) reduce the upwelling longwave radiation, which cools the stratosphere. The longwave radiation absorbed heat the TTL region…". We will also add references to these papers.

"206: "Tropospheric O3 is also transported". As I understand it, ozone is imposed in the models and not advected. I don't know what this sentence means."

This is correct: In the $5xO_3$ experiment, the PDRMIP group used 5 times the tropospheric ozone distribution (TROP) in the paper by MacIntosh et al. (2016). We will correct this statement in the paper.

"212: "larger than 50%". CAM5 and MPI-ESM look less than 50%?"

Yes, this was poorly worded. We have completely re-written the paragraph, so it now read: The $3xCH_4$ also includes some models that produce large TLS ΔSWV_{fast}/ERF magnitudes. This is likely due to TTL heating by $3xCH_4$ (Figure 5) due to the shortwave absorption by CH_4, which is explicitly treated in some models, including CAM5, CanESM2, MPI-ESM, and MIROC-SPRINTARS (Smith et al., 2018). These models are also the ones that produce the largest TLS ΔSWV_{fast} contributions.

*"*248: Returning to General Point#1, the Summary feels a very mechanical repetition of the results in the paper without any discussion of the wider implications, remaining uncertainties, or possible future avenues/priorities for improving understanding."*

We will add Fig. R1 and related discussions above in the Discussion and Summary section. We will discuss that estimated historical SWV change shows reasonable agreement with existing observed SWV record. We'll also discuss its uncertainties, which include the uncertainties in the radiative effect of BC forcing and the lack of long-term observation record of SWV as a reference.

"273: Strictly Fig 5 refers to TLS only"

We'll edit the text and make sure there will be no such confusion in the revised manuscript.

"519-520: I think the markers are only reported when there are more than 3 contributing models?"

Yes, the multi-model mean and error bars are shown for perturbations that are performed by more than three models. We will mention this in the revised caption.

"46L "responses" -> "responds""

We'll modify the text in the revised manuscript.

"Throughout: This may be common usage, but the paper refers throughout to the ensemble mean when other papers would refer to it as the multi-model mean (ensemble could refer to different runs from the same model with perturbed initial conditions or physics…"

Thanks for pointing this out. To avoid confusion, we'll use multi-mode mean in the revised manuscript.

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