The authors have done much to make the paper more traceable in terms of the numbers. It was essential to have the Bellouin et al. (2016) paper out, if at least in Discussion.

The comments by the Reviewer #2 are in black, our responses are given in red. We thank Reviewer #2, and we have followed his suggestions.

I remain confused by the ship plume revisions. The ref to the Paoli 2011 QUANTIFY paper is great for describing the problem of ship plumes, but that paper does not show the scale of the difference (error) in global ERF from shipping with and w/o the plumes. It only shows regional EU models for shipping. That is why I asked the authors to look up the later work to at least estimate the magnitude and bias of the ECLIPSE model. It simply cannot be taken at face value here.

To address this potential confusion, we have reformulated to:

"The models may overestimate the ozone production of NOx emissions from shipping, as they do not represent ship plumes, but assume instantaneous dilution of emissions in the grid boxes (Paoli et al., 2011). Holmes et al. (2014) obtained a 40% decrease in ship NOx RF when they improved their representation of ship plume chemistry. It is therefore possible that the RF for NOx is overestimated by 50% or more."

Holmes, C. D., Prather, M. J., and Vinken, G. C. M.: The climate impact of ship NOx emissions: an improved estimate accounting for plume chemistry, Atmos. Chem. Phys., 14, 6801-6812, 10.5194/acp-14-6801-2014, 2014.

On the final points (#3) - I agree that the "the ozone itself is not long-lived" but the ozone perturbation is long-lived, as long as that is clear.

We agree that a short clarification can be in place and add this sentence in the caption to Figure 2:

"For CH4 emissions, the ozone effect is formally classified as short-lived ozone, but the perturbation timescale is the same as for the methane-induced ozone effect."

Regarding the apparent long-lived perturbation from SO2: I am glad the authors checked and could find none, but the figure 8 (bar chart) is then easily misread (by me). If we look at Shipping NH Winter, and compare the SO2 (light blue) and NOx (yellowish) bars, I see that NOx picks up after 20 years and dominates the SO2, this holds for 50 yrs although it looks like they are decaying at the same rate. But at 100 years, the SO2 bar is bigger than the NOx bar - that would imply a longer-lived impact from SO2 than from CH4 (the NOx primary forcing). This holds also for Global NH Summer as far as I can read. Can you please just put in a note explaining why the SO2 bars dominate at 100 years and that this is NOT due to long-lived SO2 impacts (or else spell out what they are).

We add this clarification in Section 3.2 that describes the results in Figure 8:

"The response of the SO2 emissions appears to decay more slowly than for the NOx emissions between 50 and 100 years, which might not be expected given that the SO2 response has only a short-lived component. This apparently peculiar behavior occurs since the net response of NOx emissions is a sum

of partially cancelling warming and cooling processes, and the degree of cancellation varies with time. The processes related to CH4 have a longer response tail than the aerosol-related processes."