We thank both reviewers for their comments.
Replies and changes are listed below.

1 General presentation

Following both reviewers' comments, the manuscript has been revised extensively. Most significantly,

1. The usage of acronyms has been greatly reduced in the text, in order to improve readability. In figures, we have chosen to replace SDRECS by the more compact $\mathrm{DRE}_{\mathrm{clr}}^{\mathrm{sw}}$.
2. The conclusions have been revised to better emphasize, the results that may be model specific from those that are applicable to others models participating in CMIP6.

2 Reviewer 1

1. **the paper effectively does not consider the role of fires in their model / data comparisons. Consequently, the comparisons between data and model over India Asia (in particular) and anywhere in the tropics will be substantively compromised (e.g. Ramanathan and Carmichael, Nat Geo 2008 and references therein). In fact it seems likely that their poor model/data comparison in the tropics is likely because fires are not obviously considered in the comparison.**
   Our model includes monthly biomass burning emissions from Global Fire Emissions Database. We agree with the reviewer that uncertainties in biomass burning could contribute to model biases in tropical regions. However, decadal changes in biomass burning are small relative to those in anthropogenic emissions in the regions that we focus on (India, China, the Eastern US, and Europe), which suggests it is unlikely to contribute to errors in the simulated trends in the aerosol effect. We have have added a reference to the van der Werf et al. [2017], which describes the methodology used to derived GFED emissions and highlighted that it is based on satellite observations. See also reply to comment 5 and to comment 4 from reviewer 2.
   The text was revised as follow:

   Monthly biomass burning emissions are from the historical global biomass burning emissions inventory for CMIP6 (BB4CMIP6, van Marle et al. [2017]). Emissions for the 1997 to 2015 period in this inventory have been derived from satellite-based emissions from the Global Fire Emissions Database (GFED, van der Werf et al. [2017]).

2. **Line 165: What does this mean? this is apparently one of the many "p-tests" that gets used about the literature but essentially has no meaning if not explained.**
   We have revised the text as follow:

   We use the non-parametric Mann-Kendall test [Kendall, 1938] to identify significant changes in the aerosol effect. This test quantifies monotonic correlations between two variables. It is based on a rank procedure that makes it less susceptible to outliers than the Pearson correlation and thus especially well-suited for the analysis of environmental dataset. Here, we use a critical p value of 0.05 for trend significance. When a significant trend is detected, we estimate the linear trend using the Theil-Sen method [Theil, 1950, Sen, 1968].

3. **Line 216: (fix this statement) "Changes in AOD are dominated by spring and summer"**
   We have revised the text as follow:

   Observations show that the AOD decreases most in spring and summer

4. **Line 231: ??? Again too much acronyms / jargon. What is conclusion about these differences?**
   We have revised the text as follow:

   In both Europe and the US, we find that the change in the aerosol effect inferred from the SYN calculation is larger than that estimated from CERES-EBAF outgoing radiation corrected for surface albedo changes ($\mathrm{EBAF_C}$ *and* $\mathrm{EBAF_M}$*). The magnitude of the changes in the MATCH AOD, which is used to calculate the SYN estimate, is also greater than in more recent retrieval of AOD from MODIS (Table 1). This suggests that the rate of change in SYN aerosol effect may be biased high in Europe and Western Europe.*

5. **Line 353. . . no biomass burning as part of anthropogenic emissions? The bulk of fires is due to human activities so this is an odd statement. Perhaps you mean the subset that is not human driven? That said, you would see substantial differences between observed and actual outgoing radiation if you leave this term out (which you apparently do over the tropics).**
We have performed an additional simulation to estimate the forcing from biomass burning. The text was revised as follow:

We estimate the forcing from biomass burning and non-biomass burning sources separately, as the contribution of anthropogenic activities to changes in biomass burning emissions remains uncertain [Heald et al., 2014]. The average 2001–2015 simulated direct radiative forcing from fires is -0.011 $\mathrm{W\,m^{-2}}$*, which falls within the range of previous model assessments (*$0.0 \pm 0.05\mathrm{W\,m^{-2}}$*, [Myhre et al., 2013]).*

2.1 Reviewer 2

1. **Readability: A good illustration is the term SDRECS. While it is well defined in the paper, it is technical and not standard in similar literature. Why not simply write "change in outgoing radiation"? Most of the paper deals with shortwave under clear sky conditions, so this is implicit even from the title. The same goes for Rsutcsaf , Rsutcs and similar.**
The manuscript has been modified significantly to improve readability. Following both reviewers' recommendations most acronyms from the main text have been removed. There is no widely accepted acronym to designate the clear-sky shortwave direct aerosol effect. In figures, we have replaced SDRECS by $\mathrm{DRE^{sw}_{clr}}$, which we think is easier to understand. We have kept rsutcsaf and rsutcs notations as they are based on *CMIP6* naming convention (http://clipc-services.ceda.ac.uk/dreq/index/CMORvar.html). This is now clearly stated.

2. **One challenge, especially in the latter part of the paper (the regional trends and RF discussions), is to follow where the conclusions depend on the specific aerosol parametrizations of the GFDL model, and where they can be assumed to be more general. I would encourage the authors to add some further discussion of how model dependent the conclusions are. E.g. in the Conclusions, how general are the remarks about possible issues with the CEDS inventory? This is an important discussion for a dataset that will form the basis for much of CMIP6. A specific example: The authors conclude that "we find significant uncertainties in the CMIP6 emissions, including in the seasonality of NH3". In the paper, as far as I can understand, this is documented through the following: "We conducted a sensitivity simulation using the seasonality of NH3 column from AIRS (Warner et al., 2017) 265 to modulate NH3 emissions. We find that this revised seasonality significantly reduces the simulated winter trend in SDRECS (0.08 Wm − 2 dec − 1), improving the agreement with observations." I would expect some more discussion and documentation on this point, to make such a broad conclusion.**

   (a) We have added a figure in the supplementary materials comparing the seasonality of ammonia emissions modulated using CMIP6 seasonality and AIRS seasonality (see Fig. 1 below).

   (b) We have revised the conclusion to emphasize that some of the biases may be model specific while others are associated with CMIP6 emissions and will thus affect all models. The text was revised as follow:

   Some of these biases may be model-specific, including the treatment of the mixing between sulfate and black carbon or the representation of the photochemistry of sulfate and nitrate. Others are attributed to the CMIP6 emissions and will likely affect other models. In particular, we find that the model bias in winter over India can be largely accounted for by uncertainties in the seasonality of ammonia and black carbon emissions. Similarly, comparisons between the CMIP6 and MEIC emission inventories over China suggest that the model bias in this region can be largely attributed to an underestimate of the reduction of SO_2 *emissions after 2007.*

3. **For the DRF discussion, it would be good to put the results in a broader context. AeroCom is mentioned; where is AM3 relative to the model mean in terms of forcing strengths? E.g. a comparison to the similar (but much less detailed) results in Myhre et al 2017, ACP (https://www.atmos-chem-phys.net/17/2709/2017/) would be useful.**
We have added a comparison with the results from Myhre et al. [2017] in the RF section:

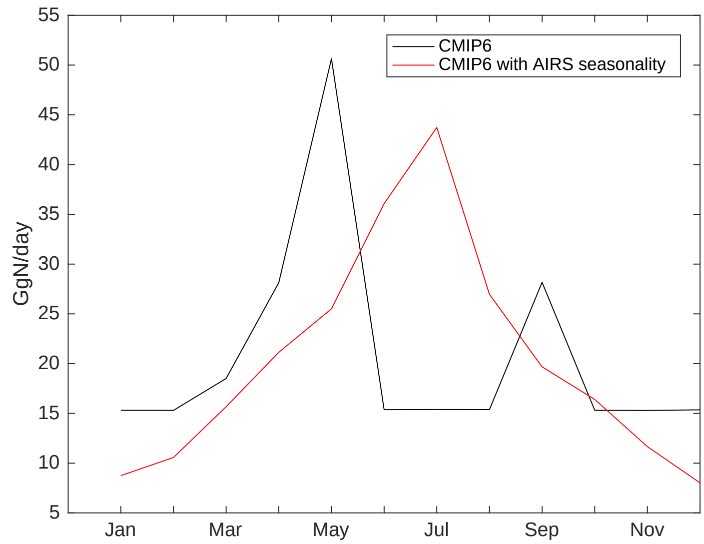

Figure 1: CMIP6 ammonia emissions for India with seasonality from CMIP6 (black) and AIRS (red)

From 2001 to 2015, the direct aerosol forcing is simulated to be +0.03 $\mathrm{W\,m^{-2}}$*, including +0.12, -0.03, and -0.03* $\mathrm{W\,m^{-2}}$ *from black carbon, sulfate, and nitrate, respectively. Myhre et al. [2017] recently reported a similar change in the overall direct radiative forcing (+0.01* $\mathrm{W\,m^{-2}}$*) but different contributions from sulfate (+0.03* $\mathrm{W\,m^{-2}}$*) and black carbon (+0.03* $\mathrm{W\,m^{-2}}$*). Many factors could contribute to these differences including the radiative properties of aerosols (e.g., the mixing of sulfate with black carbon [Bond et al., 2013]) and the emission inventories. Further studies are needed to examine whether changes in the sensitivity of radiative forcing to anthropogenic emissions are robust across models. Such assessment would be especially important in the northern midlatitudes, where the direct radiative forcing from aerosols and greenhouse gases from 2001 to 2015 are simulated to be of similar magnitude (+0.25* $\mathrm{W\,m^{-2}}$*).*

4. **A more technical example: In Figure 1, introduced on line 177, the authors show both CEDS and MEIC emissions. However, "MEIC" isn't defined or discussed until line 300, making it difficult to understand even the first figure without already having read the entire manuscript. Please review for clarity, with a community reader in mind**
We have added the following text in the method section
Anthropogenic emissions in India and China are expecte d to be more uncertain than in the US and Europe [Saikawa et al., 2017a,b]. Fig. 1 shows that the regional Modular Emission Inventory for China (MEIC) [Zhang et al., 2009], shows a decline of SO_2 *emissions starting in 2006 and accelerating in 2012, a decrease of NO after 2012, and near-stable BC emissions after 2007. In 2014, MEIC NO,* SO_2*, and BC emissions are 24%, 48%, and 32% lower than CMIP6 emissions, respectively.* NH_3 *emissions are similar in magnitude but exhibit different seasonality: CMIP6* NH_3 *emissions peak in spring, while MEIC exhibits a broad peak in summer, consistent with top-down constraints [Paulot et al., 2014, Zhang et al., 2017].*

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