We appreciate the useful comments by both reviewers. Below follows our responses to the comments by the reviewers, as well as descriptions of how the manuscript has been modified. The original reviewer's comments are in italics.

Referee # 1

In this paper the authors presented the time evolution of radiative forcing from 1750 to 2010 of all the main forcing agents in the atmosphere, with a special focus on short lived chemical species, ozone and aerosols. They used a chemical transport model to calculate the atmospheric composition for time slice simulations and a radiative transfer model to calculate the forcings due to changes in the single components of the atmosphere. The authors supported the model results with a good number of measurements, ozone, aerosol components, and surface radiation.

The presentation and the analysis of the results is clear and insightful. The text is well written and the figures and tables included in the paper are clear and well chosen. I have few minor comments which are listed below.

Page 22550, line 8: The authors do not specify how long are the time slice simulations.

Response:

We have replaced the sentence: "Time slice simulations are done for 1850, 1900 and thereafter every 10th year until 2010, using meteorological data for the year 2006 with a three year spin up for each time slice simulation."

with:

"Time slice simulations are done for 1850, 1900 and thereafter every 10th year until 2010. Each time slice simulation is one year long and meteorological data for the year 2006 are used. Each simulation is initialized by the same pre-industrial model climatology and a three year spin up is used."

Page 22550, line 10: The authors used the meteorological data of year 2006 for all time slice simulations. I think they should include a sentence motivating this choice and explain if using different meteorological data may partly modify their results. Three years of spin-up seems adequate to reach equilibrium of short lived species, nevertheless I would mention which initial concentrations were assumed for the simulations.

Response:

For the initial concentrations used, see the new text in previous response.

In this study we used year 2006 meteorology for the time slice simulations. Due to computational limitation we have not performed the simulations using several year of meteorology. Since we look at global values and perturbation since pre-industrial times, the role of inter-annual variability in meteorology is small. A previous test using OsloCTM2 showed that the interannual variability in tropospheric ozone burden for 1998-2010 (using equal annual emission for every year) was within 3%, but this is not published (Bjørg Rognerud, private communication). In Skeie et al. (2011) global burden of BC were calculated using meteorology for the period 2001-2008, however the emissions

were slightly changed over the period. In that paper, we found that the regional contribution to global burden were similar using 2001-2002 meteorology and 2005-2006 meteorology. Clouds are the major factor in the interannual variability in the radiative forcing of the direct aerosol effect for a given distribution of BC. However, Samset and Myhre (accepted GRL 2011) find that this variability is quite modest (at least within 5%).

We have included a discussion regarding the use of meteorological data on page 22585 line 4 and included a reference to Hess and Mahowald (2009) as you suggested in one of your last comments.

"Hess and Mahowald (2009) investigated the role of meteorology (including lightning induced NOx emissions) on chemical components in model simulations for the period 1979 to 1999. They found that variability for global values were generally between 0.5 to 1 %, while locally the variability was larger. Since we in this study focus on global radiative forcing values and look at differences relative to pre-industrial times, using meteorological data for one year does not increase the total uncertainty significantly."

Page 22550, line 16: Which is the assumption behind the 20% reduction of 1850 emissions to obtain the pre-industrial emissions? Check the sentence ".. except agriculture, agriculture- and waste burning and : : :. ", all sectors are switch off except agriculture, waste burning and domestic?

Response:

All emissions in 1850 for sectors related with fossil fuel use (e.g. transportation, power plants, solvents, industry) are switched off. The emissions from the remaining sectors are reduced. The fraction used is larger than the global population growth (Goldewijk, 2005). This was based on assumption that improvements in technology (including agricultural practise) led to higher emission per capita in 1850 than in 1750. Exactly how large this effect is is difficult to estimate. Combining historical data on total area under agriculture (Betts et al., 2006) with global population data (http://themasites.pbl.nl/en/themasites/hyde/basicdrivingfactors/population/) shows that the cultivated area per capita increased from 0.0152 km^2/person in 1750 to 0.169 km^2/person in 1850. At this stage it will be a large effort to do a new pre-industrial simulation. The overall increase in the emissions between 1750 and 1850 is anyway quite small and an adjustment of the scaling factor would not alter the RF estimates relative to 1750 to a large extent, and a discussion regarding the uncertainty in pre-industrial condition and RF is already included on page 22583 line 15.

We will add a sentence regarding the pre-industrial emissions on page 22550, line 16:

"This is a larger reduction in emission than scaling with ~60% based on global population growth estimated by Goldewijk (2005). However, a simple scaling factor related to global population does not capture growth in emissions per capita between 1750 and 1850 due to technology improvements, migration and land-use change and we have therefore used a lower factor."

The sentence: *".. except agriculture, agriculture- and waste burning and…"* is ok since "agriculture- and waste burning" and "agriculture" is two separate sectors.

Page 22551, line 19-22: The authors do not say anything about stratospheric chemistry in the model. Is it explicitly calculated or climatological values are prescribed for O3 and other species?

Response:

We use climatological values in the stratosphere in OsloCTM2. The climatology is calculated from a multiyear OsloCTM2 simulation for the years 2000-2008, using full tropospheric and stratospheric chemistry, presented in Balis et al. (accepted Geophys. Res. Lett. 2011).

We will add the following sentence on page 22551 line 19 and rewrite:

"In the stratosphere monthly model climatological values of ozone and nitrogen species are used, except in the 3 lowermost layers in the stratosphere (approximately 2.5 km) where the tropospheric chemistry scheme is applied to account for photochemical O_3 production in the lower stratosphere due to emissions of NOx, CO and VOCs. "

Page 22551, line 23-28: Could be summarized in 1-2 sentences the main performances of the OsloCTM2 model compared to the other models, in particular for O3 and AOD?

Response:

Some changes in the model have been included since these multi model studies. Previously the OsloCTM2 overestimated O_3 in the upper tropical troposphere (Stevenson et al., 2006; Gauss et al., 2006), while the agreement with measurements in this region is better in the model version used in this study (Fig. 5 this paper). We rewrite the following paragraph:

"The aerosol module has been validated against in situ measurements and remote sensing data in Myhre et al. (2009) and the model has been involved in the AeroCom aerosol multi model comparison project (Kinne et al., 2006; Schulz et al., 2006; Textor et al., 2006). The chemistry module has been evaluated in Søvde et al. (2008), references above, as well as in multi model studies (Dentener et al., 2006a; Shindell et al., 2006b; Stevenson et al., 2006; Fiore et al., 2009)."

New paragraph:

"The chemistry module has been evaluated in Søvde et al. (2008, 2011), references above, as well as in multi model studies (Dentener et al., 2006a; Gauss et al., 2006; Shindell et al., 2006; Stevenson et al., 2006; Fiore et al., 2009). Remark that changes in the model have been included since these multi model studies were preformed where OsloCTM2 overestimated O_3 in the tropics. The model has been involved in the AeroCom aerosol multi model comparison project (Kinne et al., 2006; Schulz et al., 2006; Textor et al., 2006), and the aerosol module has been validated against in situ measurements and remote sensing data (Myhre et al., 2009). Myhre et al. (2009) found that the mean pattern of aerosol optical depth from the model is broadly similar to the AERONET network of ground-based sun photometers and satellite retrievals.

Page 22552, line 19:"an uncertainty range"

Response:

This is corrected in the text.

Page 22553, line 1-3: sentence long and hard to follow. Consider revising of this sentence. " for changes IN the stratosphere", you may also shorten saying ".. and for changes in tropospheric and stratospheric ozone".

Response:

We agree that the sentence is hard to follow. We have split the sentence, but kept “changes in the stratosphere” to emphasise the source region of the RF:

“The resulting RF estimates were separated for source and source regions. Estimates were separated for changes in ozone due to tropospheric O_3 precursors and changes in ozone due to chlorine and bromine components. The estimates were also separated for ozone changes occurring in the troposphere and for changes occurring in the stratosphere.”

Page 22554, line 17: "All RF values presented ARE relative to : : :."

Response:

We have rephrased the sentence:

“Unless otherwise stated all RF values presented are relative to year 1750.”

Page 22555, line 10: maybe better "observed" instead of "seen".

Response:

We agree and have replaced “seen” with “observed”.

Page 22557, line 18: ".. tropospheric O3 with.", is missing some text here?

Response:

No, but we will rephrase the sentence:

“There are limited observations in space and time that can be used to validate the modelled trend of tropospheric O_3.”

Page 22558, line 3: "Although : : : than in Shindell (2006b), we underestimate the CO in the NH WHERE CO concentrations : : : "

Response:

This is corrected in the text.

Page 22558, line 6-9: The authors compare the year 2000 simulation with observations averaged over the period 1980-2002. Nevertheless in their study they have simulations for years 1990 and 1980. So I wonder if they did a comparison between model and observations both for the same decades (separated comparisons for 1990 and for 2000).

Response:

In Fig. 6 we showed that the modelled ozone concentration at 500 hPa did not change much between 1990 and 2000, except in South Asia. However, sonde observations in this region are sparse prior to year 2000. In the southern tropics observations are from 1997 to 2002. The main reason for including the figure was to validate the model performance and not to look at the trends. We therefore chose to compare the model results for year 2000 to the observations and treat the observations as climatology.

Page 22558, line 22-23: the authors mention the stratospheric influx as cause of smaller seasonal cycle. This brings back to the methodology (Page 22551) where the authors do not explain how the stratospheric O3 is modeled in the simulations.

Response:

The same climatology for stratospheric ozone is used in all time slice simulations. This is now included in the methodology section (page 22551) as described above.

Page 22561, line 6-7: there are considerable uncertainties in the parameterization of NOx emissions (e.g. Grewe et al., 2001; Tost et al., 2007; Schumann and Huntrieser, 2007), and different parameterizations simulated opposite trends over the same period (Schultz et al., 2007, REanalysis of the TROpospheric chemical composition over the past 40 years (RETRO). A long-term global modeling study of tropospheric chemistry. Final Report, Tech. rep., Max Planck Institute for Meteorology, Hamburg, Germany, 2007).

Response:

We agree that it is also considerable uncertainties in the NOx parameterization. On Page 22561 the main point is whether changes in O_3 due to changes in NOx emission from lightning is a forcing or a feedback, regardless the sign of the forcing/feedback. Therefore, and because we want to limit the length of the paper, we chose to keep the text as it is and not include any comments regarding parameterization of NOx emissions.

Page 22562, line 14-16: Similar results for OH were also found by Pozzoli et al. 2011(Re-analysis of tropospheric sulfate aerosol and ozone for the period 1980–2005 using the aerosol-chemistry-climate model ECHAM5-HAMMOZ, Atmos. Chem. Phys., 11, 9563-9594, 2011).

Response:

We will add the following comparison of our results and Pozzoli et al. (2011) on Page 22562 line 8:

"Pozzoli et al. (2011) calculated changes in global OH concentration from 1980 to 2005 using the aerosol-chemistry-climate model ECHAM5-HAMMOZ. They found an increasing trend due to anthropogenic emissions of 0.25% yr^{-1} which is greater than the relatively stable OH concentration in our study over this period (Fig. 8). Pozzoli et al. (2011) found that the anthropogenic trend was offset by a decreasing OH trend due to natural variability in meteorology (lightning, humidity and temperature) and natural emissions including biomass burning emissions emphasising the role of other factors than anthropogenic emissions on the OH concentration. "

and on Page 22562 line 14 we include the red text:

Recently, Montzka et al. (2011b), found that the OH concentration has been rather stable over the last decade, which is consistent with our model results (Fig. 8) and Pozzoli et al. (2011).

Page 22563, line 12: " water vapor increases throughout the period, but flattens : : : " or ".. is increasing : : : but flattening"

Response:

We have rewritten the sentence:

"The RF of stratospheric water vapour increases throughout the period, but flattens out over the last decades (Fig. 1c), as seen for the RF of CH_4 as well."

Page 22564, line 2: "SO2 emissions reached their maximum".

Response:

We have replaced: "The total anthropogenic SO_2 emissions reached its maximum of"

with: "The total anthropogenic SO_2 emission reached its maximum of"

Page 22564, line 9: "Of the total increase in sulphate burden, 36 % of the increase occurred prior to 1950, : : :", maybe better "36% of the total increase in sulphate burden occurred before 1950, : : : ".

Response:

We have replaced: "Of the total increase in sulphate burden, 36% of the increase occurred prior to 1950, mainly in early industrial areas in North America and Europe."

with “As much as 36% of the total increase in sulphate burden occurred before 1950, mainly in early industrial areas in North America and Europe.”

Page 22565, line 5-8: Similar results were also found by Pozzoli et al 2011. Figure 13. Increasing oxidation due to southward shift of the emissions. Despite total sulfur emissions decrease, the burden is almost unchanged between a simulation with varying and fixed anthropogenic emissions (but including inter annual variability due to meteorology and natural emissions) for the period 1980 and 2005.

Response:

We add the following on Page 22567 line 10:
“Similar results were also found by Pozzoli et al. (2011) for the period 1980 to 2005 with an almost unchanged global mean burden between a simulation with varying and fixed anthropogenic emissions.”

Page 22567, line8: I cannot find also in Section 2.2 a reference for the constant biogenic emissions. I suggest to add the references for emissions in Section 2.2 and how they compare with other studies.

Response:

On page 22567 line 8 we have now repeated that we use constant natural emissions in this study to separate the anthropogenic contribution to the changing concentration (new text in red):

“The emissions of VOCs contributing to SOA formation used in this study are also listed in Table 1. The emissions of biogenic VOCs are assumed to be constant for all time slice simulations to separate the anthropogenic signal, while anthropogenic VOCs have increased over the period with a maximum in 1990.”

The biogenic VOC emissions are identical to the one used in Hoyle et al. (2009) and discussed in that paper. Due to the long length of the paper and the focus on anthropogenic contribution we choose to not include a comparison with other studies in this paper. In the method section we will include a short paragraph regarding the natural emissions on Page 22550, line 19:

“The natural emissions (Table 1) are assumed to be constant throughout the simulation period to separate the anthropogenic contribution. The lightning emissions are based on Price et al. (1997) scaled to 5 Tg N $year^{-1}$ and distributed over the year by convection activity data. The natural emissions of CO, NOx, and hydrocarbons from vegetation, ocean and soil are from the POET emission inventories (Granier et al., 2005). The isoprene and biogenic volatile organic compounds (VOCs) important in SOA formation are as used in Hoyle et al. (2009). The natural emissions of sulphur species are as described in Berglen et al. (2004) except that we use the parameterizations of the DMS flux from Nightingale et al. (2000) and emission from continuous degassing volcanoes from Dentener et al. (2006b). “

and hence deleted the sentence in the method section: “The natural emissions from lightning, vegetation, soil and ocean are assumed to be constant throughout the simulation period to separate the anthropogenic contribution.”

We will also include the following text on Page 22550 line 11 regarding the emissions, since the reference was missing:

“For year 2010 anthropogenic emissions from the Representative Concentration Pathways scenario RCP4.5 (Thomson et al., 2011) are used, which are consistent with the historical emission data (Lamarque et al., 2010). For biomass burning we have used the year 2000 emissions for 2010.”

Page 22568, line 11: can be summarized in 1-2 sentences the main conclusions from Hoyle 2009 about these differences?

Response:

A throughout discussion is presented in Hoyle et al. (2009). We have replaced the sentence: “The differences among the models are discussed in Hoyle et al. (2009).” with:

“The differences among the models, mainly related to the amount of mass available for partitioning, meteorological data, removal schemes, and biogenic VOC, are discussed in detail in Hoyle et al. (2009).”

Page 22574, line 18: "the model overestimates : : :"

Response:

This is corrected in the text.

Page 22579, line 9: typo ".. in the in the downward: : :."

Response:

This is corrected in the text.

Page 22584, line 28-29, Page 22585, line 1-4: On this regard, effect of meteorology on chemical composition of the troposphere, the authors may write 1-2 sentences on what found by previous studies on inter annual variability due to natural emissions and meteorology, for example the recent study of Hess and Mahowald (2009, ACP).

Response:

See the response to one of your first comments.

Page 22585, live 23: ".. of what are currently believed to be the main anthropogenic components. "

Response:

This is corrected in the text.

Figures:

Figures 2 and 9, maybe is better to say ": : : between 2000 and 1950 : : :" as the shown fields are 2000-1950.

Response:

We have rewritten the figure captions for Fig. 2, Fig. 6 and Fig. 9:

Fig. 2. The 2000–1950 difference in annual mean zonal concentration for O_3 absolute (a), O_3 relative (%) (b), NOx (c), CO (d), OH (e) and HNO_3 (f).

Fig. 6. Relative change from 1970 to 2000 (% decade^{-1}) in surface O_3 (a) and at approximately 500 hPa (b) and time evolution of regional mean O_3 at the surface (c) and approximately 500 hPa (d).

Fig. 9. The 2000–1950 difference in annual mean column load (left column) and annual zonal mean concentration (middle column). The development in regional burden relative to 1850 is shown in the right column. The aerosols included are sulphate, FFBF OC, BB OC, SOA and fine mode nitrate.

Referee # 2

The paper seeks to investigate how the size of anthropogenic forcings have varied through time. Although a wide range of different climate forcers are followed, the paper's main emphasis and strength lies in its calculation of short lived climate forcers (ozone and aerosol) via detailed chemical transport and radiative transfer modelling, while the calculation of the other forcers mainly relies on, and extends, pre-existing work and techniques. The paper clearly states where the new and original contribution lies while acknowledging the work of others. The breadth and rigour of this paper is impressive and the presentation of the results and comparison to other works and observations is clear and thorough. I have two questions as well as several minor comments which are listed below:

Linearity Assumptions

The paper does not carry out detailed calculations for several of the forcers, and instead has to rely on some assumptions to calculate time-series for the forcings. In places these assumptions have not been given sufficient justification and a reason or reference should be added if possible. Specifically: For the stratospheric components the chemistry was not calculated. Instead the RF time-series due to ozone depletion was linearly scaled with effective stratospheric chlorine and water vapour forcing is scaled linearly with the time-series of methane forcing. Can the authors provide any justification for this assumption? Equally is the assumption that the cloud lifetime effect varies linearly with the cloud albedo effect a reasonable one?

Response:

Since we have not done detailed calculations for some of the mechanisms we had to make some assumptions. We have made the following changes to the text to justify and to acknowledge the limitation of the linear assumptions:

1. RF time-series due to ozone depletion was linearly scaled with effective stratospheric chlorine.

We have assumed that RF due to stratospheric ozone depletions (RF stratospheric O_3) scale linearly with equivalent effective stratospheric chlorine (EESC). Using EESC is reasonable since it provides an estimate of the total effective amount of ozone depleting components in the stratosphere. Forster and Shine (1997) investigated the linearity of stratospheric O_3 forcing, and found the RF was quite linear, within 7%, with the magnitude of the ozone depletion.

In the method section on Page 22553 line 5 we include:

"The scaling of EESC with a present day RF is a simple approach to construct the RF time series assuming linearity of stratospheric O_3 RF. Forster and Shine (1997) investigated the linearity of stratospheric O_3 forcing, and found the RF was quite linear, within 7%, with the magnitude of the ozone depletion"

2. Water vapour forcing is scaled linearly with the time-series of methane forcing:

Myhre et al. (2007) calculated radiative forcing due to stratospheric water vapour from CH_4 oxidation based on vertical CH_4 profiles from satellite data. They explicitly calculated RF for stratospheric H_2O for the periods 1750-2000, 1950-2000 and 1979-2000 and found that the RF was relatively linear with the CH_4 RF (15-20%).

We will add new text and a reference to this paper in the text on Page 22553 line 17:

"To construct a RF time series of H_2O in the stratosphere we make a simple assumption assuming a linear relationship between the CH_4 RF and stratospheric H_2O RF. For stratospheric H_2O RF due to CH_4 oxidation Myhre et al. (2007) made explicit calculations for 1950, 1979 and 2000 and the results were relatively linear with CH_4 forcing. Remark that the time series presented here do not include dynamical causes for stratospheric H_2O, only forcing due to CH_4 oxidation. "

3. Cloud lifetime effect varies linearly with the cloud albedo effect:

As far as we know, there is no specific information in the literature regarding the relationship between cloud lifetime effect and cloud albedo effect for changes in the aerosol burden (total burden and shift in the geographical distribution). However, Rotstayn and Penner (2001) find, in a GCM simulation that estimated both the RF for the cloud albedo effect and the "quasi-forcing" (as they call it) for the cloud lifetime effect, "…. by showing that the magnitude and spatial distribution of the two effects (cloud albedo and cloud lifetime effect), as well as the surface temperature response of our model, were similar". Although this does not prove that a simple scaling is 100% correct, the similarity in spatial distribution indicates that both processes are governed by the change

in aerosol concentrations without any strong non-linear effects and thus that a simple linear scaling is a reasonable approach. We therefore assume that the cloud albedo and cloud lifetime effects are linearly related. For the cloud lifetime effect the uncertainty regarding the linear assumptions is probably less than the uncertainty in the magnitude of the RF. To emphasize the simple linear assumptions we insert the red text in the method section:

"For the cloud lifetime effect, the RF time series is created in a simple way by scaling the best estimate of the current RF given in the review by Isaksen et al. (2009) who established, based on published estimates using models and satellite data, a best estimate and an uncertainty range for the cloud lifetime effect in 2007 relative to pre-industrial times. The cloud lifetime effect is scaled back in time using the RF time series for the cloud albedo effect. Rotstayn and Penner (2001) find, in a GCM study, that the magnitude and spatial distribution of the cloud albedo effect and cloud lifetime effect were similar. This indicates that both processes are governed by the change in aerosol concentrations without any strong non-linear effects and thus that a simple linear scaling is a reasonable approach."

In the result section we now make a comment that this is a simple assumption. On Page 22576 we insert the red text:

"Therefore we have included the cloud lifetime effect as a RF mechanism in this study. In this study detailed modelling of the cloud lifetime effect is not performed and we make a simple assumption of a linear relationship between the cloud albedo effect and the cloud lifetime effect. The time series for the cloud lifetime effect (Fig. 1c) are estimated by scaling the best estimate in Isaksen et al. (2009) of −0.35 W m^{-2} with the cloud albedo effect time series, and has therefore a similar historical development as the cloud albedo effect."

Uncertainty estimates:

Several of the uncertainty estimates in figure 1d are based on the range calculated by other authors for a multi-model ensemble scaled to your calculated value. Eg the uncertainty estimate for: tropospheric aerosols taken from Forster et al 2007, stratospheric water vapour taken from Forster et al 2007, direct aerosols from Myhre 2009 and indirect aerosols from Isaksen 2009. This does not give the error from the modelling techniques incorporated in this paper but merely reflects the amount of error spread found in previous studies. If a more complete error analysis was not possible then perhaps its worth explaining the reason for this.

Response:

To perform a complete error analysis using e.g. a Monte Carlo approach for our modelling is not possible due to limited computational capacity. Also as stated in the AeroCom study by Schulz et al. (2006) a combination of several model estimates is necessary to explore the uncertainties in the RF estimates. Providing an uncertainty range is therefore challenging in a study using only one model and we therefore utilize the error spread found in previous studies.

We will include in the discussion section regarding RF uncertainty on Page 22584 line 19 the following:

"The error estimates for all mechanisms (Fig. 1d) are therefore based on spread found in previous studies."

Minor comments:

Abstract: For completeness include calculated size of indirect aerosol effect.

Response:

We have estimated RF time-series of many components and we chose to not include the calculated RF values in 2010 for other components than LLGHGs and the direct aerosol effect in the abstract. If including the indirect aerosol effects, both the cloud albedo and the cloud lifetime effect should be included for completeness. The estimate in 2010 of the cloud lifetime effect is taken from Isaksen et al. (2009) and not a main results of this work. In the abstract we will replace the sentence:

"Since preindustrial times the RF of LLGHGs has been offset by the direct and indirect aerosol effects"

with:

"Since pre-industrial times the positive RF (LLGHGs and tropospheric O_3) has been offset mainly by the direct and indirect aerosol effects"

to emphasize that it is not just LLGHGs and direct and indirect aerosol effect that contribute to the total anthropogenic RF.

Remark that the error estimate in the abstract for the direct aerosol effect was wrong. This is now corrected to -0.48 ± 0.32 W m^{-2}.

Introduction:

p22547 line 14 - need to add reference for land use effects (could move Forster citation to end?)

Response:

We have moved the Forster et al. reference to the end:

"Also, changes in stratospheric water vapour and ozone in the stratosphere as well as anthropogenic changes to the properties of the land surface give a radiative forcing on the climate system (Forster et al., 2011)."

P22548 line 2 - ice cores drilling ! ice core drilling

Response:

This is corrected in the text.

p22548 line 16 – is on the short lived components

Response:

This is corrected in the text.

p22548 line 29 ! simulations for the IPCC ..

Response:

This is corrected in the text.

Section 2.2

First paragraph – Why is a factor of 20% used to reduce agriculture and waste burning by in pre-industrial times. A reason or reference should be added if possible.

Response:

See response given to Referee #1.

p22551 line 22 – acronym VOCs not explained.

Response:

The acronym is now explained in new text added to the method section regarding natural emissions.

Section 2.4

Could add a short passage describing the other effects that land-use change has e.g. hydrological cycle and why this is not followed (e.g. feedback not climate forcer).

Response:

We have added on page 22553 line 27: "Land use change also influences the hydrological cycle by altering i.e. runoff, evapotranspiration and precipitation (Kvalevag et al., 2010). These climate effects are considered as climate feedback mechanisms and are not included in this study. "

Section 3.

p22554 line 17 ! Unless otherwise stated all RF values presented are relative to year 1750..

Response:

We have replaced the sentence “All RF values presented is relative to year 1750 otherwise something else stated in the text.” with: “Unless otherwise stated all RF values presented are relative to year 1750.”

Section 3.2.1

p22558 line 3 ! we also underestimate the CO in the NH where CO : : :.

Response:

This is corrected in the text.

section 3.2.2

p22561 Paragraph 1 last line– meaning not clear. Re-word.

Response:

We have deleted the following sentence: “The associated change in ozone related to changes in natural emissions might therefore not be counted as a forcing in a strict sense” since the previous sentence and the next paragraph is sufficient related to if the changes in ozone due to changes in natural emissions is treated as a feedback or as forcing.

Section 3.3

Last paragraph. - a comparison with other models results for stratospheric water vapour eg the range from Forster et al could be useful

Response:

The time series for stratospheric water vapour RF are estimated by scaling the RF in 2005 from Forster et al. (2007). Therefore we do not need a comparison with Forster et al. (2007), but we have rewritten the paragraph to make this clearer:

“The RF of stratospheric water vapour increases throughout the period, but flattens out over the last decades (Fig. 1c), as seen for CH_4 RF as well. The RF is estimated by scaling the RF estimate for year 2005 in Forster et al. (2007) and the same relative uncertainties as in Forster et al. (2007) (±71%) is adopted. This gives a RF in 2010 of 0.073 ± 0.052 W m^{-2} (Fig 1d).”

section 3..4.1

p22564 line 23 - should also be given in mg(SO4)m-2 for completeness.

Response:

We have included the burden as mg(SO4)m^{-2} as well:

"increase in sulphate burden in 2000 since 1850 was 0.44 Tg S (2.6 mg (SO4) m^{-2})"

Section 3.4.3

Last paragraph – you explain discrepancy with Forster et al due to other models not including SOA but your OC contribution alone is still higher than the range presented in this paper, so this explanation is still insufficient, this could be made clearer.

Response:

We have added a short explanation:

"It is substantially stronger compared to the RF of organic aerosols presented in Forster et al. (2007) of −0.05 ± 0.05 W m^{-2}, where most of the estimates considered did not include SOA chemistry. The RF due to primary OC is also strengthened due to a combination of larger burden and an increased ratio of OM/OC more in line with observations as discussed in Myhre et al. (2009)."

P22579 line 9 – in the repeated twice.

Response:

This is corrected in the text.

figure 3 – why are legends only on some figures?

Response:

We have improved the quality of figure 3. A legend is now included in all the sub-figures.

References:

Berglen, T. F., Berntsen, T. K., Isaksen, I. S. A., and Sundet, J. K.: A global model of the coupled sulfur/oxidant chemistry in the troposphere: The sulfur cycle, J. Geophys. Res., 109, D19310, 10.1029/2003jd003948, 2004.

Dentener, F., Drevet, J., Lamarque, J. F., Bey, I., Eickhout, B., Fiore, A. M., Hauglustaine, D., Horowitz, L. W., Krol, M., Kulshrestha, U. C., Lawrence, M., Galy-Lacaux, C., Rast, S., Shindell, D., Stevenson, D., Van Noije, T., Atherton, C., Bell, N., Bergman, D., Butler, T., Cofala, J., Collins, B., Doherty, R.,

Ellingsen, K., Galloway, J., Gauss, M., Montanaro, V., Muller, J. F., Pitari, G., Rodriguez, J., Sanderson, M., Solmon, F., Strahan, S., Schultz, M., Sudo, K., Szopa, S., and Wild, O.: Nitrogen and sulfur deposition on regional and global scales: A multimodel evaluation, Global Biogeochem. Cy, 20, GB4003, 10.1029/2005GB002672 2006a.

Dentener, F., Kinne, S., Bond, T., Boucher, O., Cofala, J., Generoso, S., Ginoux, P., Gong, S., Hoelzemann, J. J., Ito, A., Marelli, L., Penner, J. E., Putaud, J. P., Textor, C., Schulz, M., van der Werf, G. R., and Wilson, J.: Emissions of primary aerosol and precursor gases in the years 2000 and 1750 prescribed data-sets for AeroCom, Atmos. Chem. Phys., 6, 4321-4344, 2006b.

Fiore, A. M., Dentener, F. J., Wild, O., Cuvelier, C., Schultz, M. G., Hess, P., Textor, C., Schulz, M., Doherty, R. M., Horowitz, L. W., MacKenzie, I. A., Sanderson, M. G., Shindell, D. T., Stevenson, D. S., Szopa, S., Van Dingenen, R., Zeng, G., Atherton, C., Bergmann, D., Bey, I., Carmichael, G., Collins, W. J., Duncan, B. N., Faluvegi, G., Folberth, G., Gauss, M., Gong, S., Hauglustaine, D., Holloway, T., Isaksen, I. S. A., Jacob, D. J., Jonson, J. E., Kaminski, J. W., Keating, T. J., Lupu, A., Marmer, E., Montanaro, V., Park, R. J., Pitari, G., Pringle, K. J., Pyle, J. A., Schroeder, S., Vivanco, M. G., Wind, P., Wojcik, G., Wu, S., and Zuber, A.: Multimodel estimates of intercontinental source-receptor relationships for ozone pollution, J. Geophys. Res., 114, D04301, 10.1029/2008jd010816, 2009.

Forster, P. M. D., and Shine, K. P.: Radiative forcing and temperature trends from stratospheric ozone changes, J. Geophys. Res., 102, 10841-10855, 1997.

Gauss, M., Myhre, G., Isaksen, I. S. A., Grewe, V., Pitari, G., Wild, O., Collins, W. J., Dentener, F. J., Ellingsen, K., Gohar, L. K., Hauglustaine, D. A., Iachetti, D., Lamarque, J. F., Mancini, E., Mickley, L. J., Prather, M. J., Pyle, J. A., Sanderson, M. G., Shine, K. P., Stevenson, D. S., Sudo, K., Szopa, S., and Zeng, G.: Radiative forcing since preindustrial times due to ozone change in the troposphere and the lower stratosphere, Atmos. Chem. Phys., 6, 575-599, 2006.

Goldewijk, K. K.: Three Centuries of Global Population Growth: A Spatial Referenced Population (Density) Database for 1700–2000, Population & Environment, 26, 343-367, 10.1007/s11111-005-3346-7, 2005.

Granier, C., Lamarque J.F., Mieville A., Muller J.F., Olivier J. , Orlando J., Peters J. , Petron G., Tyndall G., and S., W.: POET, a database of surface emissions of ozone precursors, available on internet at http://www.aero.jussieu.fr/projet/ACCENT/POET.php 2005.

Hess, P., and Mahowald, N.: Interannual variability in hindcasts of atmospheric chemistry: the role of meteorology, Atmos. Chem. Phys., 9, 5261-5280, 10.5194/acp-9-5261-2009, 2009.

Hoyle, C. R., Myhre, G., Berntsen, T. K., and Isaksen, I. S. A.: Anthropogenic influence on SOA and the resulting radiative forcing, Atmos. Chem. Phys., 9, 2715-2728, 2009.

Kvalevag, M. M., Myhre, G., Bonan, G., and Levis, S.: Anthropogenic land cover changes in a GCM with surface albedo changes based on MODIS data, International Journal of Climatology, 30, 2105-2117, Doi 10.1002/Joc.2012, 2010.

Lamarque, J. F., Bond, T. C., Eyring, V., Granier, C., Heil, A., Klimont, Z., Lee, D., Liousse, C., Mieville, A., Owen, B., Schultz, M. G., Shindell, D., Smith, S. J., Stehfest, E., Van Aardenne, J., Cooper, O. R., Kainuma, M., Mahowald, N., McConnell, J. R., Naik, V., Riahi, K., and van Vuuren, D. P.: Historical (1850–2000) gridded anthropogenic and biomass burning emissions of reactive gases and aerosols: methodology and application, Atmos. Chem. Phys., 10, 7017-7039, 10.5194/acp-10-7017-2010, 2010.

Myhre, G., Nilsen, J. S., Gulstad, L., Shine, K. P., Rognerud, B., and Isaksen, I. S. A.: Radiative forcing due to stratospheric water vapour from CH4 oxidation, Geophys Res Lett, 34, L01807, 10.1029/2006gl027472, 2007.

Myhre, G., Berglen, T. F., Johnsrud, M., Hoyle, C. R., Berntsen, T. K., Christopher, S. A., Fahey, D. W., Isaksen, I. S. A., Jones, T. A., Kahn, R. A., Loeb, N., Quinn, P., Remer, L., Schwarz, J. P., and Yttri, K. E.: Modelled radiative forcing of the direct aerosol effect with multi-observation evaluation, Atmos. Chem. Phys., 9, 1365-1392, 2009.

Nightingale, P. D., Malin, G., Law, C. S., Watson, A. J., Liss, P. S., Liddicoat, M. I., Boutin, J., and Upstill-Goddard, R. C.: In situ evaluation of air-sea gas exchange parameterizations using novel conservative and volatile tracers, Global Biogeochem. Cy, 14, 373-387, 2000.

Pozzoli, L., Janssens-Maenhout, G., Diehl, T., Bey, I., Schultz, M. G., Feichter, J., Vignati, E., and Dentener, F.: Re-analysis of tropospheric sulfate aerosol and ozone for the period 1980–2005 using the aerosol-chemistry-climate model ECHAM5-HAMMOZ, Atmos. Chem. Phys., 11, 9563-9594, 10.5194/acp-11-9563-2011, 2011.

Price, C., Penner, J., and Prather, M.: NOx from lightning 1. Global distribution based on lightning physics, J. Geophys. Res., 102, 5929-5941, 10.1029/96jd03504, 1997.

Rotstayn, L. D., and Penner, J. E.: Indirect aerosol forcing, quasi forcing, and climate response, J. Clim., 14, 2960-2975, 2001.

Schulz, M., Textor, C., Kinne, S., Balkanski, Y., Bauer, S., Berntsen, T., Berglen, T., Boucher, O., Dentener, F., Guibert, S., Isaksen, I. S. A., Iversen, T., Koch, D., Kirkevag, A., Liu, X., Montanaro, V., Myhre, G., Penner, J. E., Pitari, G., Reddy, S., Seland, Ø., Stier, P., and Takemura, T.: Radiative forcing by aerosols as derived from the AeroCom present-day and pre-industrial simulations, Atmos. Chem. Phys., 6, 5225-5246, 2006.

Shindell, D. T., Faluvegi, G., Stevenson, D. S., Krol, M. C., Emmons, L. K., Lamarque, J. F., Petron, G., Dentener, F. J., Ellingsen, K., Schultz, M. G., Wild, O., Amann, M., Atherton, C. S., Bergmann, D. J., Bey, I., Butler, T., Cofala, J., Collins, W. J., Derwent, R. G., Doherty, R. M., Drevet, J., Eskes, H. J., Fiore, A. M., Gauss, M., Hauglustaine, D. A., Horowitz, L. W., Isaksen, I. S. A., Lawrence, M. G., Montanaro, V., Muller, J. F., Pitari, G., Prather, M. J., Pyle, J. A., Rast, S., Rodriguez, J. M., Sanderson, M. G., Savage, N. H., Strahan, S. E., Sudo, K., Szopa, S., Unger, N., van Noije, T. P. C., and Zeng, G.: Multimodel simulations of carbon monoxide: Comparison with observations and projected near-future changes, J. Geophys. Res., 111, D19306, 10.1029/2006JD007100, 2006.

Skeie, R. B., Berntsen, T., Myhre, G., Pedersen, C. A., Ström, J., Gerland, S., and Ogren, J. A.: Black carbon in the atmosphere and snow, from pre-industrial times until present, Atmos. Chem. Phys., 11, 6809-6836, 10.5194/acp-11-6809-2011, 2011.

Stevenson, D. S., Dentener, F. J., Schultz, M. G., Ellingsen, K., van Noije, T. P. C., Wild, O., Zeng, G., Amann, M., Atherton, C. S., Bell, N., Bergmann, D. J., Bey, I., Butler, T., Cofala, J., Collins, W. J., Derwent, R. G., Doherty, R. M., Drevet, J., Eskes, H. J., Fiore, A. M., Gauss, M., Hauglustaine, D. A., Horowitz, L. W., Isaksen, I. S. A., Krol, M. C., Lamarque, J. F., Lawrence, M. G., Montanaro, V., Muller, J. F., Pitari, G., Prather, M. J., Pyle, J. A., Rast, S., Rodriguez, J. M., Sanderson, M. G., Savage, N. H., Shindell, D. T., Strahan, S. E., Sudo, K., and Szopa, S.: Multimodel ensemble simulations of present-day and near-future tropospheric ozone, J. Geophys. Res., 111, D08301, 10.1029/2005JD006338, 2006.

Thomson, A., Calvin, K., Smith, S., Kyle, G., Volke, A., Patel, P., Delgado-Arias, S., Bond-Lamberty, B., Wise, M., Clarke, L., and Edmonds, J.: RCP4.5: a pathway for stabilization of radiative forcing by 2100, Clim. Change, 109, 77-94, 10.1007/s10584-011-0151-4, 2011.