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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The net climate impact of coal-fired power plant emissions" *by* D. T. Shindell and G. Faluvegi

Anonymous Referee #1

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Review of "The net climate impact of coal-fired power plant emissions" by D. T. Shindell and G. Faluvegi

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General comments The paper presents an interesting and relevant analysis of the climate effects of coal fired power plants in India and China, and it focuses on the balance between the long term warming effect of CO2 vs the strong short term climate effects of pollutants. The effects of air pollution control strategies on net RF(t) and net dT(t) are calculated for various scenarios. It is an important study that illustrates the climate impact of air quality strategies and the magnitude of the potential "unmasking effects".

The paper is rather compact and the presentation should be improved, as it is often

difficult to see what the authors have done. I have several questions and suggestions for clarifications, which I think will improve the paper and clarify what the authors have done. (The quality of figures (colours, labels) should also be improved.) I recommend publication given that these improvements are implemented. See also my specific comments below.

Specific comments

Page 21259, line 14-15: Mention that NOx not only affects O3, but also CH4 and nitrate.

Page 21259, line 18-20: The statement "the interplay has not been examined closely" is ambiguous and vague, and potentially incorrect. Thus, rewording is needed. There are many studies and references that should be added here, e.g. the paper by Shindell et al. in Science 2009; Arneth et al., Science 2009; and Aunan et al., 2009. Radiative forcing from household fuel burning in Asia. Atmos Environ.

Page 21259, line 27: "is not well known" is an exaggeration since there are many studies discussing this balance (e.g. Berntsen et al., 2006. Abatement of greenhouse gases: Does location matter? Climatic Change). Please reword.

Page 21260, line 8: I suggest labels on the scenarios (A, B,. . .. or 1, 2, . . .) to help the readers.

Is it assumed that the changes in NOx and SO2 are parallel in the air pollution control scenarios? Please clarify.

It could be mentioned that these sources only have small emissions of BC.

This paper is potentially relevant as an additional reference:

Substitution of Natural Gas for Coal: Climatic Effects of Utility Sector Emissions by Hayhoe et al Climatic Change 54: 107–139, 2002.

Page 21261, line 7: It is not easy to keep overview of the various scenarios and assumptions. The sentence “so there are zero emissions” could be related directly to the scenarios with “none” in table 1.

Page 21261, line 12: Is this the only case in which historical emissions are included? For which period? Pre year 2000? Please clarify. It would also be useful to have a short discussion of implication of choice of time frames for the analysis.

As far as I understand the calculated RFs are superimposed on an assumed background. This needs a few words of explanation. And what about this background; is it constant or is a specific scenario assumed?

Page 21262, line 21-22: The sentence “were not simulated in the model as these are highly uncertain” sounds strange; please explain better and discuss how such uncertain effects can be treated in assessments and analyses.

Page 21263, line 4-7: Explain briefly, here or later in the paper, the limitations of this approach relative to a more sophisticated CO2 model and the potential implications for the results-

Page 21263, line 14: I suggest using the expression “primary mode ozone” and refer to:

Prather, M.J., 2002. Lifetimes of atmospheric species: integrating environmental impacts. Geophysical Research Letters 29, 2063.

and

Wild, O., Prather, M.J., Akimoto, H., 2001. Indirect long-term global radiative cooling from NOx emissions. Geophysical Research Letters 28, 1719–1722.

Page 21263, line 19: The authors should explain why the calculated RF for nitrate is positive; and how the interactions between sulphate, ammonia and nitrate work.

Page 21263, line 20: Stress that figure 1 shows a constant emission case and that it is, as far as I can see, used as a reference.

Page 21263, line 20: Instead of just showing the net RF in fig. 1, I strongly suggest showing the individual components and the total net RF, all as function of time. This will give the reader valuable information of the behaviour (magnitude and temporal development) of the various components and will help to understand the results throughout the paper. This could be done by having two parts of fig. 1; e.g. a) showing the individual effects and b) showing the total net (with and without AIE).

Fig 1: It should also be made clearer what the RF numbers are given relative to, what the background is and how history is treated.

Page 21264, line 8-9: Regarding the sentence “However, photochemical regimes. . ...” Please, explain better and discuss briefly the implications for the results.

Page 21264, line 18: Since the pollutants are short lived and since there is no lag in the climate system involved. Add that this is obviously different if we consider dT(t) instead.

Figure 2: Are the values given as differences from the control run (shown in figure 1)? This needs explanation.

Page 21265, line 7-27: This is very important and interesting, and I think more attention should be given to these issues; either in this section or later in the paper.

Figure 3: How are the spatial RFs for CO2 and CH4 calculated? Please explain briefly.

In figure 3 the authors write “annual average instantaneous radiative forcing”. It should be discussed what it means for the results that instantaneous radiative forcing is used and not adjusted RF.

The RF numbers given in the figure text to fig. 3 are important and should be discussed more explicitly in the main text.

I suggest adding a table showing normalized RF values by burden changes (for current emissions). This would allow a comparison of these model results with other results in the literature.

Page 21265, line 24-26: It would be useful if the authors could discuss very briefly how their previous findings applied here relate to the results from Boer, G.J., Yu, B., 2003. Climate sensitivity and response. Climate Dynamics 21, 415–429.

Page 21266, line 1: I'm not sure if the order of the sections is optimal. Normally the discussion of uncertainties could come after all the results. But I guess the authors feel that the main results are the RF numbers and that the dT calculation is something extra and that the uncertainty is discussed simultaneously here.

Page 21266, line 4: add ". . ..and are based on many assumptions"

In section 5, resolution of model should be mentioned and potential impact on models results for the various components.

Page 21267, line 10-12: As far as I understand, the point is that to get an estimate of the magnitude on a global scale, the RF values in fig 2 should be increased by approx 10-30%. Could this be stated more explicitly?

(But I'm not sure if this is the right place for this issue since the addition of power plants outside China and India is not an uncertainty issue.)

Page 21267, line 21: Units correct? (8 g/kg NOx and 2278 g/kg CO2). Rather gram pollutant per kg fuel. And what is NOx given as, NO2?

There are some uncertainty issues that I think are missing in this section: âĂć Uncertainty in the relation emissions to concentration âĂć And uncertainty in the relation concentration to RF

This should be discussed and values should be given; either from their own model studies or typical values from the literature.

Page 21268, line 14-27: In section 6 the authors take the calculated RF responses one step further and estimate the temperature response. A better explanation of the

method is needed here. In my view, it is not sufficient with the brief information and references as given now. I suggest adding some equations so that it is, in principle, possible to reproduce the results. Also a better explanation of the lag in temperature due to the inertia of the ocean would be good to include.

With this limited information, the method for dT calculations may sounds complex and somewhat uncertain. One alternative could be to use a simple climate model (MAGIC type, or a one or two-box model). What are the pros and cons of the two methods (a simple climate model vs the adopted approach) ?

It is unclear how AIE is treated in the dT calculations.

The reasons for not doing GCM runs given at bottom of page 21268 and top of 21269 could be shortened.

It could be mentioned that global mean temperature is only one measure of climate change and that other parameters like circulations and precipitation also are important. References to studies of climate responses to heterogeneous RFs could be given (e.g. Jones et al. in JGR)

Figure 5: The latitude intervals for S and N are different. Should be the same to allow a comparison.

Page 21269, line 10: "its greater climate sensitivity": Add a few words on why it is so.

Page 21269, line 16: I agree that focus could also be given to rate of change. The authors could add a reference or two on why this is important.

Page 21269, line 19: How are the historical calculations done? To me it seems that they just move two emission scenarios back in time and use that as a proxies. I suggest less weight and attention to this.

Page 21270, bottom / Page 21270, top: The authors state that the dT(t) calculations are only meant as rough indications of possible dT responses, and I accept that. It is striking, however, how much attention that is given to the dT calculations. I feel that there is a slight unbalance here. This may be rectified with some more explanation of the method used for dT(t) calculations and perhaps less weight on the dT(t) results, although they are in principle more relevant.

Page 21270, line 1-5: What about AEI here?

Page 21270, line 1-25: One could say that the implementation of air quality measures gives a situation in which coal fired power plants cause a 'double warming effect' (as in the case of shipping due to SO2 reductions; see http://pubs.acs.org/doi/abs/10.1021/es901944r)

Page 21272, line 1-3: I suggest re-writing "...but should not be taken as a reason..." since this is beyond science and belongs more on the application and policy side.

Page 21272, line 8-14: These issues (the opposing effects of long term warming and strong short term cooling and the reductions in cooling) are important and could be given more focus. A reference to figure A2.4 in IPCC 1992, or to Charlson et al., 1991, Tellus 43A-B could be given.

The adequacy and limitations of using global mean values as indicator could be given some more attention.

Page 21273: The authors mention and briefly discuss some additional aspects related to coal fired power plants. I'm not convinced that this is the best place to do that. Could be better to mention this in the beginning of the final section and then end the section with the main findings related to climate – since this is the main focus of the paper.

Technical corrections

Make the units clear in table 1 (CO2 vs C, NO2 vs N, SO2 vs S)

Figure 2: Difficult to see the different colours.

Figure 4: The colours of the legends are red and blue, while the curves are both black

in the figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 21257, 2009.