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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sulfur cycle and sulfate radiative forcing simulated from a coupled global climate-chemistry model" *by* I.-C. Tsai et al.

Anonymous Referee #2

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Review of Tsai et al. 2009 ACPD, Sulfur cycle and sulfate radiative forcing simulated from a coupled global climate-chemistry model

Overall, this article presents a straight forward study of the impact on the global sulfur cycle when a simple sulfur module is added to an online global chemistry-climate model. This new model is one of several that have been developed in the recent past that uses online coupling of aerosols and meteorology. The article is worthy of publication with some modifications, as it documents the behavior of the model in relation to similar models and contributes to the larger discussion related to the magnitude of the aerosol impacts on climate, specifically the direct and first indirect effects in this case.

TECHNICAL ISSUES:

p. 22373, l. 5: Could the relatively low sulfate loading in GCCM be responsible for the smaller direct effect versus many of the other models?

p. 22377, l. 9ff: This paragraph opens by stating that the direct effect can be estimated by comparing either simulations A1-N1 or A2-N2. However, nothing is mentioned after this about A2-N2 in this paragraph. Can the direct effect really be estimated from A2-N2? A2-N2 will include differences in meteorology due to the 1st indirect effect, which will feed back to the overall sulfate field. So, the difference in this case would include both the direct effect and some aspects of the first indirect effect. Since the comparison A2-N2 is not discussed in this paragraph, this reviewer suggests just removing the reference to A2-N2 in the opening sentence.

Also, while it is clear to most readers from the context, this paragraph would be a good place to mention that the "direct effect" referred to in this paper is the anthropogenic direct effect, i.e. it is the anthropogenic component of the total aerosol direct effect due to sulfate. This point should also be made here, or elsewhere as appropriate, for the indirect effect.

p. 22377, l. 24: The general statement is made that the indirect effect is substantially larger than the direct effect. This is based on the global average values for these effects. Are the authors sure that this is true regionally as well for all portions of the globe?

p. 22379, l. 6: "apparently most of it is from the indirect effect"... Is this true? From Table 5, going from the differences for the 0 to 1 to 2 simulations, dT from the ozone effect is -0.04 K, dT from the (direct effect + ozone effect) is -0.02, and dT from (direct+indirect+ozone effects) is -0.09 K. So, if we assume the effects interact linearly and back out the ozone effect and direct effect from the combined value, we get (-0.09)-(-0.04-0.02)=-0.03 K for the indirect effect. This puts the value of for the indirect effect between that for the ozone and direct effects. Am I understanding this correctly?

p. 22380, l. 15: "A better comparison can be performed between either the A-series or N-series of simulations..." This statement is made in reference to understanding feedbacks onto the sulfur cycle from coupled chemistry. It is noted that the 0 and 1 scenarios are based on assuming a specified effective radius. It should be emphasized more in the text that the results of this section are going to be very dependent on the choice of effective radius and thus are just a representative example of the impact. If a different choice were made for the effective radius value, then the results could be different because the cloud fields would change. For example, the authors state rightly on p. 22380, l. 28 that the wet deposition has an impact. There will also be changes to local wind and temperature fields that will affect the sulfur as the clouds change.

An alternative would be to find a way to make the runs more comparable. This could be done by finding a way make the average effective radius the same between two simulations, which would then then be used for comparison. For example, to decipher the indirect effect, Gustafson et al. (2007) used an initial simulation with the indirect effects turned on to get an average aerosol number and hygroscopicity that was then fed into a 2nd run for use in the comparison. This helps to minimize differences between cloud properties due to differences between cloud model assumptions.

p. 22381, l. 27ff: The use of Figure 7 in combination with the traditional statistics is a good way to present the internal variability issue. The visual comparison helps the reader comprehend the statistics. However, the authors should caution the reader that even though the statistics indicate some of the metrics reach the level of statistical significance, this is only based on a five year period. This is not long enough to encompass the full variability of the atmospheric system in terms of climate and year-to-year variability. Thus, the results should be understood as tentative and used with caution.

p. 22386, l. 16: "we demonstrated that climate signals from the direct forcing of sulfate are indistinguishable from the internal climate variability of the model..." The authors should reword this to state the this is shown in the context of the variability of the 5-year simulation, not the full model variability.

p. 22386, l. 25ff: The reviewer does not understand the leap to needing the fully couple ocean-atmosphere model to understand the sulfur cycle on a regional scale. Is this because the internal variability of the model would change, possibly influencing the statistical significance of the signal? I do agree that changes to the sulfur cycle on a regional scale are going to be much more significant for some regions than for a global average. That point is worth making within this paragraph.

MINOR ISSUES:

In the introduction, a paragraph is devoted to discussing the emergence of online coupled aerosol-meteorology/climate models. As it stands, the discussion focuses solely on global models, which by necessity use a simpler sulfate cycle than is possible in regional online coupled aerosol-meteorology/climate models. Consider adding a sentence or two noting that regional models are also tending towards online coupling. In the final discussion at the end of the paper, this can then be tied into the need for improved aerosol-cloud interactions and also understanding the regional impacts. The regional models are able to use more complicated and physically based algorithms, and can serve as a way to to develop more effective, simpler algorithms based on comparisons with the more complicated ones that are in the regional models. One example, though not the only one you could use, is WRF-Chem (Fast et al. 2006, Grell et al. 2005, Gustafson et al. 2007), which includes the direct and both indirect effects.

p. 22370, Eqn. 3: The c should be subscripted after the N.

p. 22371, l. 10: "spin-off" should be "of spin-up"

p. 22378, l. 5: "Similar to the approach applied earlier" is stated, but this reviewer does not see the earlier use of the alternative way to calculate the forcing using the difference of differences. Maybe I missed it?

p. 22378, l. 17: The first sentence of Section 3.3 is a little awkward.

p. 22380, l. 1: The last sentence in Section 3.3 could be stated a little better (the

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convoluted and tedious part). Would it be more accurate to state that the responses would be difficult to separate and cannot be deciphered from the given simulations alone?

p. 22381, l. 5: Refer to the "indirect effect" here as the "anthropogenic indirect effect" to give it context.

p. 22382, l. 16: "Note that the internal variability in shortwave radiation and surface temperature from simulations of the A-series are a bit smaller than from N-series, so the above discussion is based on a stricter standard."... Consider stating this point up front, instead of stating "it will be discussed later" (p. 22379, l. 13) earlier in the discussion. Having this knowledge before reading this section would help the reader to accept the statistical comparison more the first time they read it.

p. 22391, l. 30: The reference for Jockel et al. is out of order.

Figures: The labels on the filled contour plots are very small. At least on the review manuscript they cannot be read. They should be made larger.

Figure 6: The caption refers to gray shading but the figure is in color. This should be corrected.

There are a significant number of minor grammatical mistakes in this paper. This reviewer suggests that the authors run the final manuscript past an editor for proper English grammar. Given the number of places that should be addressed, they will not be listed here.

REFERENCES:

Fast, J. D, Gustafson Jr.,W. I., Easter, R. C., Zaveri, R. A., Barnard, J. C., Chapman, E. G., and. Grell, G. A.: Evolution of ozone, particulates, and aerosol direct forcing in an urban area using a new fully-coupled meteorology, chemistry, and aerosol model, J. Geophys. Res., 111, D21305, doi:10.1029/2005JD006721, 2006. (First WRF-Chem reference for direct effect)

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Interactive Comment

Grell, G. A., Peckham, S. E., Schmitz, R., and McKeen, S. A., Frost, G., Skamarock, W. C., and Eder, B.: Fully coupled "online" chemistry within the WRF model, Atmos. Environ., 39, 6957–6976, 2005. (General reference for WRF-Chem)

Gustafson Jr., W. I., Chapman, E. G., Ghan, S. J., and Fast, J. D.: Impact on modeled cloud characteristics due to simplified treatment of uniform cloud condensation nuclei during NEAQS 2004, Geophys. Res. Lett., 34, L19809, doi:10.1029/2007GL030021, 2007. (First WRF-Chem reference for cloud-aerosol interactions for 1st & 2nd indirect effects)

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22365, 2009.