Atmos. Chem. Phys. Discuss., 13, C5155–C5157, 2013
www.atmos-chem-phys-discuss.net/13/C5155/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Response of secondary inorganic aerosol concentrations and deposition fluxes of S and N across Germany to emission changes during high PM_{10} episodes in spring 2009" *by* S. Banzhaf et al.

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Received and published: 25 July 2013

This manuscript details a study of model based, brute-force emissions reductions of inorganic PM precursors and subsequent impacts on PM concentrations over the "German zoom domain . . . [that] inevitably . . . comprises parts of neighboring countries and seas" (this phrasing made me smile for some reason). The most interesting part of this manuscript to me was the analysis of the dependency of PM on cloud droplet pH, and I was disappointed that more discussion and model simulations was not provided

on this topic. The manuscript is well written and generally clear, but could use some minor editing for punctuation, some awkward phrasing, and casual language. I believe it warrants publication in ACP, but I have a few suggestions for the authors that may be used to improve it.

1) The period chosen for the study was reportedly very dry with "precipitation amounts . . . far below average," and very high sunshine duration. These conditions seem very conducive to PM formation, but not for analysis of aqueous chemistry, since, presumably, there were not very much cloud cover. Again, I believe the pH sensitivity is an important topic for research, so how robust is your analysis from this dry period? On the same note, what fraction of sulfate was oxidized in cloud water versus dry gas chemistry during this period?

2) The emissions reductions scenarios were examined entirely in the context of the model, which was validated using reasonable methodology for the base case. Is it possible to use the speciated observational data without regard to their spatial and temporal aspects to check if the responses simulated by the model are even possible, particularly for the extreme reduction cases? In other words, if you are modeling specific amounts of precursors at some site (including low ammonia for instance) that lead to specific speciated inorganic PM concentrations, is there some other place in Germany that has the same ratios of precursors and PM that was observed to compare? Seems like there needs to be additional grounding of the results in reality if at all possible.

3) What was the average droplet pH in the modeled pH case, or how far away was it from the constant of 5.5 and was there significant variability?

4) I disagree that this work is more significant because it was performed over a shorter period (spring) compared to previous studies (year-long), because, logically, the year-long studies also analyzed the spring. Instead, it is probably significant because the shorter episode was studied in more detailed with several emissions scenarios.

5) I would like to see a couple of sentences on how this particular model (RCG) is used in regulatory settings if at all. This would be helpful to connect your findings to actual policy process. Similarly, what are the current strategies for controlling this type of PM in Germany and are they on the right track according to what you found?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 15783, 2013.