

Interactive comment on “Dynamic model evaluation for secondary inorganic aerosol and its precursors over Europe between 1990 and 2009”

by S. Banzhaf et al.

Anonymous Referee #1

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The paper by Banzhaf et al. presents a comprehensive evaluation of a meteorological (RACMO2) and chemical transport (LOTOS-EUROS) modeling system. Their work focuses on secondary inorganic aerosols and related pollutants over a 20-year period. Before it can be published in GMD, I have a number of comments that the authors should consider. One minor comment I have is the inconsistent use and spelling out of acronyms throughout. For example, LOTOS-EUROS is not spelled out until page 4651 and IIASA is not spelled out until page 4654; both acronyms are used earlier in the paper. Other acronyms, such as the RAINS and GAINS models (page 4654) are not spelled out. Please review the entire manuscript for consistency, and spell out all acronyms that might not be common for an international audience.

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I have two more important issues that need to be addressed as well. First, there are a number of instances where the authors discuss differences in trends or model performance. It is not always clear if such differences are statistically significant or not. In my specific comments below I will point out some discussion that needs to be expanded. Second, I am somewhat concerned about the comparisons across the three time ranges: 1990-2009, 1995-2009, and 2000-2009. These time ranges can have very different numbers of sites located in very different regions of Europe, and it is not clear to me how much of the differences in trends or model performance is due to changes in emissions and how much is due to the available data in a given time frame. The 2000-2009 has the additional complication of, perhaps, not being long enough to infer statistically significant trends. If the authors can address these general comments, as well as the specific ones below, I would be happy to recommend this paper be published in GMD.

Page 4654 line 12: The authors use the word “pops” here. Do they mean “persistent organic pollutants (POPs)?” Page 4654-4655 and Figure 1: Showing emissions normalized to 1990 is fine, but it might be helpful to include the actual emissions in tons, perhaps in the supplemental section. It is not clear to me how substantial emissions from shipping are compared to total emissions in this region. Total emissions are generally decreasing (Figure 1a) but shipping emissions behave somewhat differently (Figure 1b). Also, the section on the source apportionment module needs to be clarified. What does the 10 kton threshold apply to? A single source, such as a large power plant? An entire source sector, such as all power plants? Or something else? Page 4657: I want to make sure I understand the three selection criteria. I interpret criterion 1 to mean that a complete year means 75% valid data. I interpret criterion 2 to mean that 80% of the 10, 15, or 20 years need to be considered complete to be included in the analysis. Is this correct? Also, I don’t understand why the highest variability is expected at the beginning of each time period; why is this? Couldn’t an “unusual” year occur somewhere in the middle or end of the range? Finally, I am a little uncomfortable with the visual screening (criterion 3). It seems to me that “outliers” can occur, and I



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am not sure why sites showing “constant values” over time are necessarily wrong. This visual screening removes more than half of the available SO₂ sites. Page 4658: There are so few TNO3 and TNH4 sites across the model domain. This limitation needs to be stressed, and I don’t know how this impacts the results pertaining these two parameters. This comment also applies to the discussion on page 4665 (lines 11-21). Page 4661-4662: Does RACMO2 assimilate surface meteorological data for nudging? If so, were any of the surface meteorological stations used? If they were, it would not be surprising that there was good correlation for some of the meteorological fields, since this would not be an independent verification of the model. Page 4664 lines 15-23: The authors discuss different correlation coefficients in different time periods, but I am not sure if these differences are statistically significant. This is especially true for SO₄, which changed from 05 to 0.56. A test of significance might be needed here. Also, the reasons for this apparent change in model performance seems to border on opinion. I think the last sentence in this paragraph should be re-written. Bottom of page 4664 to top of page 4665: Could the underestimation of air concentrations of SO₄ be due to underestimating SO₂-to-SO₄ conversion, or possibly overestimating deposition of SO₄? This comment could also apply to the discussion in the second paragraph on page 4674. Bottom of page 4665 to top of page 4666: The authors state that the model results compare more favorably in the later years, and that the model predicts N compounds better than S compounds. However, some discussion on statistical significance may be warranted here. Page 4666 lines 4-11 and Figure 4: Does Figure 4 include trends that are not significant? I am not sure both Figure 4 and 5 are needed. I prefer what is shown in Figure 5, as there is an indication of trend significance, as well as magnitude. If the authors remove Figure 4, please modify any subsequent text that refers to this figure. Page 4667-4668: I am concerned about reading too much into trends based on only 10 years of data. It only takes a single “unusual” year to confound trend estimates for such a short period. This section could be re-written, it is somewhat confusing. At least there is some mention of significant versus non-significant trends. Page 4670-4671: I don’t think the first paragraph in section 3.3 is needed, it doesn’t add

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to the paper. Figure 6 is also not needed, since trends in these species are presented in earlier figures. Page 4671: The section on SIA formation needs some clarification. What does the 10 kton threshold apply to? See my earlier comment regarding page 4655.

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