

Interactive comment on “Effects of various meteorological conditions and spatial emission resolutions on the ozone concentration ROG/NO_x limitation in the Milan area (I)” by N. Bärtsch-Ritter et al.

Anonymous Referee #3

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The paper deals with the well known, and by now, extensively investigated case of photochemical ozone episode in the Po Valley during the PIPAPO campaign. The study presents a sensitivity analysis of a series of meteorological parameters and emission inventory resolutions and specifications that control photochemical pollution episode. Most of my comments and criticisms will refer to the meteorological parameters sensitivity analysis being the latter a vital element of the photochemical simulation.

Although the case study selected is well known and well documented and the research intentions are well posed, in my view the paper fails in giving robust evidence on the validity of the results obtained, their originality, in presenting an original and clear ap-

proach and in supporting the conclusions.

The authors start by identifying a set of meteorological variables whose variability is considered to be important for ozone prediction. The variables selected (mixing height, temperature, humidity and wind speed) are indeed important for a correct ozone (in general) peak (in particular) determination, and it does not come as a surprise that by performing a sensitivity analysis on these variables the ozone concentration does change. What is missing in the paper is clear evidence that the variation takes place in the right direction and that the sensitivity analysis improves the model performance consistently when compared to measured data. Further to that there is no indication on why the sensitivity analysis was performed on the specified ranges and how these ranges compare to actual measurements of the meteorological variables during the episode. In other words what is the relationship between the ranges of values selected and the actual variability or uncertainty of those parameters obtained from the measurements.

Abstract: Line 10, page 734 - "The model results show that changes in meteorological input files have the largest effect on peak ozone." Apart from the lack of specification on what are the "changes" and apart from the fact that from the sentence they appear to relate to the "file" rather than the values, if this is the main conclusion of the study it does not come as a surprise and I do not think you need a model simulation to conclude that. The abstract is therefore vague and lacks detailed information on the study and its conclusions. I would not call 10.1 ppb/C a "slope" but simple a rate of variation.

Introduction: Line 26, page 734 - " Ozone variation is heavily sensitive to meteorological fluctuations " What is a meteorological fluctuation according to the authors? Do you intend the one caused by natural external forcing of the meteorological system, or the uncertainty on the knowledge of a meteorological variable? Line 12, page 735 - " Unsatisfactory input of the meteorological" What is satisfactory according to the authors? Prior to any analysis I assume the authors have to define what is satisfactory for them. This is a crucial issue that has to be tackled before any range of variability of meteo-

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rological parameters is defined. An analysis of the measured meteorological variables during the period selected and a comparison of the meteorological model results with measured data is in this case essential. Line 14, page 735 - "Adjustments in input files should be considered after a careful evaluation of the model results with regard to a wide range of measurements." Although I do not really like that input file content are adjusted, as mentioned, but rather that meteorological fields are consistent with measurements, this statement includes an essential element (measurements) that the authors seem to neglect later in their analysis. Line 16, page 735 - "Often only ozone concentrations are compared to measurements and may simply agree accidentally." Well this is kind of strong and unfair with respect to a series of studies conducted in that past. I do not see in this paper any argument that would make the ozone prediction more robust than others.

Area of investigation: The study seems to take into consideration only the station of Bresso. What about the large number of stations that make the PIPAPO campaign dataset?

Model description: A hydrostatic model is used for a region that is topographically complex. In spite of the 4DDA technique used by the model no evidence is given on how the meteorological fields match the measured data for the simulation period. What is the level of uncertainty in the meteorological simulation when compared to data? Why performing a sensitivity analysis if there is no evidence on the level of accuracy of the model simulation? There is no indication of the vertical extension of the 19 model layers. The domain is rectangular but there is no way of distinguishing what is the N-S and the E-W extension. Furthermore no indication is given on where the numerical domain starts. To what extent is this domain containing the Alps. The authors also state (Line 4, page 738) that such model "should provide more accurate meteorological fields" is this the case? With no evidence on that there is no reason for running a sensitivity analysis. I am sure the authors have this evidence but I would suggest them to present it to the reader and to use it to justify their choices. In spite of

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the use of "nudging" and measured data to run the model it is necessary to know how the simulation of the meteorology looks like. Vertical soundings were performed by aircraft measurements during the campaign that can be used for the purpose (Dosio et al., J. Geophys. Res., 107(D22), 8189, 2002)

Model domain: There is a bit of confusion on the modelling domains and Figure 1. It is not clear what are the meteorological domain, the photochemical model domain and the domains named "Milan" and "Po basin". Basically what is used for the simulation and what for the results analysis? A clear distinction should be made. A better version of Figure 1 with clear indication of what is what should be provided.

Design of meteorological conditions: The first sentence is unclear. What is also totally unclear is how the variation of the values of the meteorological variables was performed. Namely:

Line 25, page 740: The daily maximum of the original mixing height, 1000 m a.g.l., was increased " What does "original" mean in this context? Measured, calculated? And where was the variation performed, all over the domain? Why was the mixing height only increased? Why the authors did not choose values around the "original" one, whatever that is? In the "Model description" section the authors mentioned that the model overestimates the mixing height. Why is the sensitivity analysis increasing further its values?

Were temperature and humidity varied systematically all over the domain? If yes why? There is no indication whatsoever on the variation in the vertical? Why this range of values? What is the relation between the range of values selected and the actual situation measured? How does the wind field change as a response to the variation of the temperature and moisture fields? Is the mixing height consistent with the temperature variations, if performed also in the vertical direction? How do the varied temperature and moisture fields compare with actual measurements?

Wind fields. Where and when were wind speeds changed? It is said that the change

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was performed in order to preserve mass conservation. This implies that increased wind speeds in certain areas correspond to decreased wind speeds in others and vice versa, how this is actually changing the overall wind field in the domain at surface and in the vertical with respect to measured meteorology? How does the variation compare with actual measurements at stations not used for the nudging? Why was the wind only increased and not reduced thus enhancing stagnation? In view of suggesting an emission reduction strategy also this scenario of weather conditions should be considered. If your ozone prediction improves but the wind field does not match the measurements the study itself will fall under one of the earlier alleged frequent casual matching! What about wind direction? Which component was enhanced? What about the vertical velocity? What is the consistency with the variation of the mixing height?

Results and discussion: The station of Verzago should be displayed in Figure 1. It is not clear if the results presented are compared with measurement. Figure 3 is unclear. There is no way of understanding what is measured and what is calculated, what is a point measurement or calculation and what is an area averaged value. What is the meaning of the rectangle in Figure 3? Furthermore what about the tens of stations deployed in the region during the campaign?

Case A: Is the fact that ozone peak drops by increasing the mixing height a surprising result? What is the relevance of this if we do not get to know to what extent the three mixing height values used are close to the measured values?

For the other Cases similar arguments hold. The authors change parameters and obtain changes in the results, but how do the results compare with actual measurements? If the matching with measurements for ozone is acceptable, what is the overall consistency of the parameters that produced it with corresponding measurements?

Conclusions: The conclusions are somehow contradictory in the sense that the authors claim, not unexpectedly, a strong impact of meteorological conditions and reduced spacing in emission inventory on the modelled species concentrations. Suggest that

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attention should be paid in this respect (at least this is what I assume they want to say since sentence at line 23 of page 754 is not that clear), but at the end suggest a strong and clear emission strategy for the Milan region. I just wonder what in this study is guaranteeing that the strategy suggested will be the right one.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 733, 2003.

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