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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "The impact of MM5 and WRF meteorology over complex terrain on CHIMERE model calculations" *by* A. de Meij et al.

A. de Meij et al.

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Anonymous Referee #1 General Comments It is an interesting paper providing some answers on the effect of meteorology on air pollution. It includes results (model output and measurements) not shown before and some explanations are given on the agreement or disagreement between model output and observations. On the other hand, all the conclusions concerning model performance (both in terms of meteorology and air quality) are rather uncertain because the measurements were limited; there was only a small number of measuring stations and they only provided surface measurements. Therefore, one cannot get information on the variation of the model performance throughout the model domain and there can be no comparison of the vertical structure of the atmosphere (which plays a key role in air pollution applications) with observations. Some more specific comments on this paper follow in the next section.

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We selected those stations (non traffic, non urban) stations for which we had also meteorological data available. The combination of PM measurements and meteorology is for this work very useful as we investigate the differences of meteorological driver models on PM calculations. Due to this combination we had only 5 stations for our comparisons.

We added a new section (4.1.5) where we evaluate the vertical potential temperature gradient profile for the Linate airport location. In this section we evaluate the vertical profile of the potential temperature gradient calculated by WRF and MM5 by comparing the results with observations from the Linate airport location. In Fig. 3 we compare the potential temperature gradient (ptg) profile between 10m and 200m at the hours 0.00h, 06.00h, 12.00h and 18.00h for the whole year. Positive values indicate that the atmospheric layer between 10m and 200m is stable, negative values indicates that the layer is unstable, values around 0 indicates neutral conditions of the atmosphere (Stull 1988). We see that the ptg profile by MM5 and WRF is in good agreement with the observations. At 0.00h the ptg profile by MM5 is in general higher than by WRF. At 06.00h the ptg profile by WRF and MM5 are similar and correspond well with the observations. At 12.00h we see that from spring time (day 60) to autumn (day 280) the ptg profiles are negative, indicating unstable conditions in the first 200m. These instable conditions are well captured by both MM5 and WRF. During winter time both models calculate stable conditions, which corresponds to the observations. At 18.00h we have limited observational data available. However, the ptg profile by WRF agrees well with the observations. In general we can say that the potential temperature gradient by WRF is better than by MM5.

Figure 3. Vertical potential temperature gradient profiles between 10m-200m by WRF, MM5 for the Linate airport, together with the observations for 0.00h, 06.00h, 12.00h and 18.00h for the whole year.

Specific comments In the Abstract and in the Concluding Remarks sections (lines 6 and 22 respectively) it is mentioned that “ The analysis shows that the performance

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of both models is similar, however some small differences are still noticeable”. It should be clarified that this refers to surface meteorological data only. As mentioned afterwards, “The PBL height by WRF meteorology is a factor 2.8 higher at noon in January than calculated by MM5”: this is a substantial (and not just noticeable) difference in the simulated vertical structure of the atmosphere.

Following the referee’s suggestion we corrected this.

Another finding reported in the Abstract, line 20, is that “…changing the Noah Land Surface Model for the 5-layer soil temperature model, the calculated monthly mean PM10 concentrations increase by 30%...” It is not specified (in the Abstract) which model this statement refers to (MM5 or WRF).

Corrected.

In the first paragraph of the introduction (line 8) it is mentioned that “…chemistry-transport-dispersion models (CTMs) have the advantage that they can be used to complement monitoring data, assess the effects of future changes in aerosol and aerosol precursor emissions...” CTMs also treat gaseous pollutants (and so does the paper). Therefore this statement should refer to gaseous pollutants too.

Corrected.

Both the third and fourth paragraphs of the Introduction talk about the fact that pollutant concentrations and their uncertainties depend on meteorology, chemistry and emissions. In this sense, they overlap and should be merged into one concise paragraph.

Corrected.

In page 2322 line 5 there should be a reference supporting that Po Valley is one of the most polluted, industrialized and heavily populated areas in Europe. Corrected.

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In page 2323, line 10 it is mentioned that PM10 and O3 “are prevailing&#8221 the most in Po Valley. The term “prevailing” here is unclear. Does it mean that they have larger concentrations/more adverse health effects than other pollutants for example?

Corrected, (more adverse health effects)

Four monthly simulations are mentioned in the last paragraph of the methodology section (page 2323). It should be specified if there is nudging of the simulations towards observations or not. Also, there are comparisons with yearly data. Where there yearly model runs as well?

Corrected: Four simulations are performed with CHIMERE, two simulations with MM5 meteorology (CHIMERE/MM5) for January 2005 and June 2005, and two simulations with WRF meteorology (CHIMERE/WRF) for January and June 2005. The meteorology has been created for the whole year 2005, with no nudging to the observations of the meteorological stations. For the four simulations, a spin-up time of 4 days is applied in order to initialize the model.

Citation should be added to the definition of the planetary boundary layer in line 15, page 2328. That is Stull, 1988.

Corrected, thank you.

In line 14, the term “ absolute temperature” can easily be confused with the absolute temperature scale in Kelvin (K) where zero is the absolute zero. Since the discussion of temperature is in degrees C, the term “ temperature” instead of “absolute temperature” is more suitable here. Moreover, in the paragraph under this title, in all subsequent discussions of temperature and in Table 3a, the statistic Relative Bias (RB) is used for model evaluation purposes. This statistic takes unreasonably large values when the average observed temperature is close to 0ÿC and becomes infinity if the average observed temperature is equal to 0ÿC, even

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if the model temperature estimates are very good and differ from observations only by a few tenths of a degree. Therefore such values do not necessarily represent a bad model performance. They are the result of the fact that in the Celsius temperature scale, zero is defined as the temperature where water freezes. It is suggested that RB is either not used for temperature or that its calculation is based on the absolute temperature scale, after the conversion of temperature in degrees Kelvin.

The term “absolute temperature” corrected to “temperature”. The part of analysis based on the “relative BIAS, RB” has been erased for all parameters.

In the first paragraph of page 2335 where wind direction is discussed, in all subsequent discussions on wind direction and in Table 3d a number of statistics such the mean, bias, coefficient of determination etc. have been calculated. One should note that circular data such as wind direction should be treated in a different manner. Common evaluation statistics such as the ones calculated here do not apply. To bring a simple example on this, the arithmetic mean of 0ÿ and 360ÿ is 180ÿ although 0ÿ is apparently a better choice as a mean value. Moreover, the terms “ underestimation” and “ overestimation” (e.g. p. 2335, line 5) are meaningless here. An “ underestimation” of a measurement of 10ÿ wind direction by 6ÿ yields a value of 4ÿ. An “ underestimation” of a value of 10ÿ by 12ÿ yields 358ÿ which can then be seen as a (large) overestimation. The whole statistical analysis of wind direction should be redone accounting for the particularities of circular data. Further explanations on this can be found in a number of publications such as the “ Statistical analysis of circular data” by N.I. Fisher which, in general, is a good starting point for the study of this field.

The statistical analysis of wind direction has been redone. The wind data for Ispra monitoring station have been discussed and checked. Finally the data have been taken from another, more representatively located anemometer. This has resulted in changing the dominant wind direction from western (as in previous data set) to north-

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southern. However, the data are available in this case only for the second half of the year (July – November 2005) and so the comparison analysis could be redone for this period only (also for the wind speeds). RMSE, BIAS, SD and R2 as well as mean values have not been used anymore for wind direction data. Instead the mean absolute error (MAE) has been calculated accounting for the particularities of circular data, to get some numeric values for comparison. Apart from this, wind roses have been used to visual check of the models performance. Thank you for the suggestion.

Concerning rain, certain statistics have been calculated in order to evaluate the prediction of the correct amount of rain by MM5 and WRF. However, it is probably even more important for the prediction of rain to know whether a model captured the event of precipitation during a certain day or not. For this purpose hit ratio statistics should be calculated and reported for rain.

The statistics for rain have been redone. RMSE, BIAS, SD and R2 as well as mean values have not been used anymore for rain data. Instead the sums of precipitation amount have been shown for each of analyzed time periods and the hit rate statistics have been calculated.

It is mentioned in page 2340, line 12 that relative humidity is in general overestimated by MM5. By looking at the yearly bias values in Table 3b one cannot conclude this. Yearly averages of relative humidity are slightly overestimated by WRF indeed.

Corrected. The yearly RH by MM5 is underestimated, while yearly RH by WRF is slightly overestimated.

In line 18 of the same page, “For the winter period WRF gives higher temperatures&# 8221;; higher than what, MM5 or observations? Corrected, higher than MM5.

Since section 4.2.1 refers to winter 2005, the statement in line 7 page 2343 that the underestimation of PM10 is because of overestimation of the relative humidity by the

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two meteorological models, is not true. Examination of Table 3b shows that in winter 2005 relative humidity was underestimated. Besides, this is also mentioned in the second paragraph of section 4.1.4 (line 17 page 2340).

Corrected.

In line 15, page 2344 a comparison between the “cloud cover” and the “cloud attenuation” between MM5 and WRF is done. In order to get an idea of the sensitivity of PBL height to cloud cover or cloud attenuation, it would be useful to quantify the differences in those quantities between the two models. Moreover, MM5 does not predict cloud cover and cloud attenuation. Has there been some diagnostic procedure to estimate those quantities? Which one? An alternative solution is to make a quantitative comparison of cloud liquid water which is a prognostic variable of MM5.

In our model CHIMERE, cloud characteristics are diagnosed in the same way by the same pre-processor for both the meteorological models. The presence of a cloud is determined on the cloud liquid water content at a certain height in the model. The cloud liquid water content comes from the meteorological models MM5 and WRF. We have analyzed the amount of cloud liquid water between the two models, and we found differences in the amount and frequency of the presence of cloud liquid water. We added a line to explain that the cloud cover is diagnosed with the pre-processor in CHIMERE, which allows us to compare the presence of clouds and the amount of cloud liquid water between the two meteorological models.

In lines 16-21, page 2344, the differences in cloud attenuation between the two models is attributed to the differences in cloud liquid water. In line (22) of the same page, it is stated that more cloud liquid water in WRF is the result of more cloud attenuation. The cause-and-effect relationship between cloud liquid water and cloud attenuation is reversed in those statements. This is a contradiction which should be clarified.

Corrected. More cloud liquid water content by WRF, result in more cloud attenuation

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by WRF (and more rain by WRF as described in section 4.1.2).

In page 2353, lines 21-23 some findings concerning the model performance in predicting ammonium and nitrate are reported. Given the uncertainties in the measurements of ammonium and nitrate, one cannot draw reliable conclusions on the agreement of the model with measurements. Therefore this part should be excluded from the conclusions section.

The reviewer correctly remarks that the measurements of NH4+ and NO3- aerosol with the quartz filter during summertime are indeed questionable, because of the evaporation of these aerosols from the filter. However, the conclusion is based on the comparison of the aerosols with the observations during wintertime. The loss of aerosol mass from the filter due to evaporation does not occur during that time of the year. Therefore we believe that we can say in the conclusion part that NH4+ is in good agreement with the observations and that NO3- aerosol is underestimated when MM5 is used.

In page 2355, line 28; low inversion heights are presented as a cause of stagnant conditions. If by “ stagnant conditions” the author means low wind and weak vertical mixing, low inversion heights are rather a result, not a cause.

Corrected.

In page 2356, lines 6 and 7, it is mentioned among the findings of this study that gas and aerosol concentrations have a non-linear dependence on the meteorological conditions. In this paper, although some monotonic relationships between meteorological variables and concentrations of air pollutants were highlighted, there was no investigation on a possible (non-)linearity of those relationships. Therefore there is not enough evidence in the present paper to conclude a non-linear dependence. However, this statement on the non-linearity can remain if references are added (e.g. the references in page 2321, line 23).

Corrected and included an additional reference, Easter and Peters 1994.

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In page 2356, lines 8-12, apart from the improvements in meteorology and the emissions, updated chemical mechanisms is also a key requirement for better air quality models.

Corrected.

Results in Table 3a have two decimal digits. Temperature measurements typically have an accuracy of one decimal digit (which means three significant digits for temperatures T such that |T|>=10ÿC and two significant digits for temperatures T such that |T|<10ÿ). If this is the case with measurements from the ARPA networks, the calculated statistics should be rounded accordingly. The same process should be applied to tabulated model data because measurements are involved in the process of modeling by data assimilation in the examined model runs or in the models which produced the initial/boundary conditions. Similar considerations about the number of decimal or significant digits apply in all of the presented results.

Corrected.

In tables 4, 5, 6 and 7 only the mean values of model and measured pollutant concentrations are reported. It is suggested that more statistical measures are calculated in order to get a more complete picture of the CHIMERE performance and its dependence on the meteorological input. For example, the statistics used in the meteorological evolution could also be used in the air quality evaluation. In addition, the “ factor of two” statistic could be calculated. This is defined as the fraction of model values which lie between 50% and 200% of the corresponding measurements and it is particularly useful for the evaluation of CTMs.

We inserted in the tables, 4, 5, 6, and 7 the standard deviation and the temporal correlation coefficients. In table 8 we added the standard deviation of the modelled and observed values. We analyzed the standard deviation and the temporal correlation coefficients and we described the outcome of that. In section 4.2.2 we added: The temporal correlation coefficients by CHIMERE/WRF are higher than by CHIMERE/MM5,

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indicating that the spatial gradients of the daily mean concentrations are better reproduced by the model using the WRF meteorology. In general, the standard deviations by CHIMERE/MM5 are larger than by CHIMERE/WRF. The reason for this is that for CHIMERE/MM5 higher PM10 peak values are calculated than by CHIMERE/WRF.

In the wind rose on the left-hand side of Figure 2, there are some wind directions (such as the eastern) where not a single observation was recorded throughout a year. This result is rather suspicious and the wind rose should be reexamined. The frequency that the wind in Ispra is coming from the Eastern part is less then 0.5 promile. Therefore this is not visible in the wind rose plot.

The wind rose has been re-examined and new data has been taken from another anemometer, see above where the statistical analysis of the wind direction has been re-done.

Technical corrections In line 27, page 2322 the use of “such as” implies that there were more meteorological parameters which were evaluated in the study. This is not true so “such as” should be removed. Corrected.

Many web references are included in the paper. Given that web pages change over time, the date of last access should be added to all of them. For example, instead of (http://aqm.jrc.it/citydelta), (http://aqm.jrc.it/citydelta, last accessed 06.06.2006). Corrected.

Page 2344, line 3: “ surface” should probably be replaced by “sensible” Corrected.

Page 2344, line 11: “observed” should be replaced by “estimated&# 8221;. Corrected.

Page 2344, line 18: “hydrometer” should be replaced by “hydrometeor&# 8221;. We refer to NCAR/TN-468+STR (2007) for the use of ‘hydrometer’.

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Page 2344, line 22: “is” should probably be replaced by “in” Corrected.

The text from line 24 page 2348 until line 3 page 2349 overlaps with the text in lines 16-22 page 2331. Instead of repeating the reader can be directed to the chapter where this content appeared first (3.1). Corrected.

The same applies to lines 1-7 in page 2350; their content has already been presented in section 4.2.4. Corrected. However, we think that keeping the following sentence the reader remains interested. ‘In section 4.2.4 we have seen that changing the LSM in WRF from Noah to the 5-layer soil temperature model and the PBL scheme from YSU into MRF, increase the calculated PM10 concentrations on average to 41% for the 5 stations.’ We deleted the sentence afterwards, because the result is already explained in section 4.2.4

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2319, 2009.

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