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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The impact of weather and atmospheric circulation on O_3 and PM_{10} levels at a mid-latitude site" *by* M. Demuzere et al.

M. Demuzere et al.

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Editorial Comments

Editor: R. Ebinghaus [20/02/2009]

This manuscript has been evaluated by one anonymous referee and J. Dawson. Measurements have been used to develop model-based predictive capabilities for ozone and PM10 levels with both weather type and multiple-regression approaches. Although both reviewers admit that this manuscript contains interesting aspects and methodologies both doubt the significance and relevance because of the limitation to just one site, the meteorological research station at Cabauw, the Netherlands and possibly the rural community around. The authors must clarify the relevance and representativeness of their study. Since this is a principal problem that cannot simply be solved by editorial

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revisions, further scientific elaboration and additional information, I suggest that the authors should give a statement on this particular aspect first. Or as a simple question: what is the (scientific) added value of this exercise for people not living in Cabauw?

In this reply we would like to clarify the relevance and representativeness of our study as asked for by the editor. This research is relevant for a broader scientific community since it discusses the potential of a novel weather type approach in forecasting O3 and PM10. This aspect is also recognised by reviewer 1 (J. Dawson) who states that -the major contribution of this work is the approach that the authors take in creating their models-. The measurement station Cabauw is suitable for developing and testing this new method for several reasons: The station is classified as a rural station with a limited effect of local emission sources, and has a comprehensive set of both meteorological and air quality variables measured at high temporal resolution. By using this appropriate data set, we were able to demonstrate the predictive capability of a new approach in air quality studies.

We are equally confident that this study is also relevant for other research groups because we have calibrated and validated objectively all models, using appropriate skill scores that compute the potential improvement against standard benchmark models (climatology and persistence). While proper validation of statistical models is often used for MLR and non-linear techniques in statistical modelling of air quality it is rarely used in Weather Type based models. In particular, we show that while Weather Type based modelling can be useful to gain some insight on the optimal atmospheric conditions leading to high (and low) levels of O3 and PM10 they usually fail to attain meaningful skill scores against persistence. We stress here that this result (lack of significant improvement against persistence) appears to contradict previous published material where compositing and weather typing was used. In fact these studies described synoptic situations associated with characteristic levels of an air quality variable without objectively quantifying this result, an approach that can lead to misleading results.

In the revised manuscript, we have put emphasis in showing the representativeness of

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our study. Consequently, we have selected three additional rural sites in the Netherlands (new Figure 1), where both meteorological and air quality variables are measured during the period 2001-2006. It was found that the correlations and the linear regression coefficients between the meteorological and air quality variables are similar for those sites compared to Cabauw (new Figures 5 and 6). In addition all sites have a similar annual cycle in both O3 and PM10 (new Figure 3). This analysis shows that the Cabauw station is representative for rural areas at least in the Netherlands. A comparison with the results from literature (e.g. Perez et al. (2001); Delcloo and De Backer (2005); Barrero et al. (2006); Lu et al. (2006); Andersson and Langer (2007); Papanastasiou et al. (2007) and Sousa et al. (2007)) shows that in other (rural) areas the relations between meteorology and air quality are similar and therefore do not strongly depend on the location of the (rural) site. From this analysis we conclude that Cabauw is representative as a rural site with only a limited effect of local emissions. This decision is further supported by the fact that Cabauw holds the most comprehensive set (among the four considered stations) of both meteorological and air quality variables.

We have modified our manuscript accordingly in order to stress on the relevance and representativeness of our study: the text that is modified in the revised manuscript is indicated in blue (in the manuscript itself). Note that references to the manuscript (pages, section, lines) refer to the revised version. The most important modifications are:

1. We have explained better the methods applied, especially the parts related to the circulation pattern approach. We have shortened the description of the more commonly multiple-regression models of air quality (pp 7, section 3.1; pp 11, section 4.2).

2. We have added information from three additional rural sites in the Netherlands to show that Cabauw is representative for rural areas in the Netherlands (pp 1: line 2; pp 5, lines 171-202; pp 6, Lines 203-251; p 11: lines 328-398; pp 13, Line 461; pp 18 lines 678-685; pp 19, line 730) and figures 1, 3, 5 and 6.

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Other specific remarks from the reviewers are addressed below.

Other comments:

Referee: J. Dawson [8/01/2009]

General comments: 1. Many similar papers have run multiple regressions of ozone as a function of various meteorological independent variables. The correlation and regression sections of this paper (4.1 and 4.2) do not appear to contribute much to the current body of knowledge. Given how common analyses like these are, these results should probably be de-emphasized / shortened. The weather-type approach is more novel, and I believe this is the major contribution of the paper. (Also, these kinds of analysis are rarer for PM than for ozone.) Keeping the focus on the more original contributions of the paper (weather-type), while de-emphasizing the commonly done parts (correlation and regression), would make the paper seem more original, rather than just an application of multiple regression to yet another location.

In my opinion, the major contribution of this work is the approach that the authors take in creating their models; the actual model predictions over one small town are of rather minor importance to a general audience. Multiple-regression models of air quality are rather common; the real contribution of this work appears to be the weather-type model and the comparison of the weather-type model to other kinds of models (including the multiple regression model).

We agree with these general comments. Therefore, we have made a substantial effort to change the overall framework of this research paper, putting a stronger emphasis on the methods themselves, especially on the circulation pattern approach. Therefore, we have included the following modifications:

p8, Lines 273 to 284: As this section was too lengthy, we formulated the theoretical description of the multiple regression in a more concise way.

P9, lines 328 to 398: As stated before (answer to the editor) the entire section 4.1

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has been re-written. The main differences with the previous version of the manuscript include: - Addition of three rural sites in determining meteorology and air quality relationships

- Analysis restricted to O3 and PM10, as these two pollutants are most important for this paper.

- More emphasis on the differences/similarities between the rural stations, in order to test the representativeness of the Cabauw measurement site.

p11, lines 400 to 441: section 4.2 is condensed into 3 relative short paragraphs; In this way, a stronger emphasis is given on the circulation pattern approach.

2. The number of grammatical errors is quite distracting to the reader. These are just a few examples: - The authors use both -Lamb- and -lamb- to describe the weather-type approach. - There are several places where adjectives and adverbs are confused. - Uncommon introductory words like -hereby- and -thereby- are not quite used correctly and are rather distracting.

In order to address these remarks, the revised version of the manuscript was read by all co-authors and a native English speaker. By doing so, we hope to have been able to remove all grammatical errors and language inconsistencies.

Abstract: Some quantitative results would be useful here. This may be a good place to compare R values to show how the models performed.

We fully agree with the reviewer that some relevant quantitative information must be provided in the abstract. The abstract has been changed as follows: p1., Line 20: provides insight in the meteorological processes that play a role in O3 and PM10 levels in rural mid-latitude sites in The Netherlands. p1., Line 22: based on observations from four rural sites and are determined by p1., Line 28-34: The regression models perform satisfactory, especially for O3, with an R^2 of 0.57 and 0.25 for PM10. Including previous day air quality information increases the performance with 15 and 18% for O3 and

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PM10 respectively. The Lamb weather types show a seasonal distinct pattern for high (low) episodes of average O3 and PM10 concentrations, and these are clear related with the meteorology-air quality correlation analysis. Although using a circulation type approach can bring some interesting physical relations forward, our analysis reveals the circulation method is limited in terms of short-term air quality forecast for both O3 and PM10 (R^2 between 0.12 and 0.23)

Data: 1. What is meant by 2 m (dew point) temperature? Does this mean both 2 m temperature and dew point temperature? This is confusing. This is also the case in a few other places where parentheses are used in a similar manner.

This is addressed in 2 ways: - As we altered our analysis slightly, replacing Td directly with RH, we do not refer anymore to the 2 m (dew point) temperature

- The data description section 2.1 is changed in order to describe the ECMWF data more appropriately:

p5., Lines 163-168: The data covers the 2001- 2004 period, identical to the period selected to construct the linear model from the measurements described in section 2.2. For the circulation pattern approach, 12h UTC mean sea level pressure (MSLP) is used, while mean temperature (K) and relative humidity (%) are daily averaged from the four provided time steps for the period 2001-2004. For the long-term mean, 12UTC MSLP and daily mean temperature and relative humidity are extracted for the period 1971-2000.

2. What is KNMI? This is never introduced.

This is added in the text -KNMI is the Royal Dutch Meteorological Institute-. This information was added to the new version of the manuscript (page 6, line 196)

3. The chemical reactions in 2.3 are probably not necessary.

We agree with the reviewer and deleted the chemical reactions. As stressed before (when answering general comment 1), we condensed the information in this section.

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4. Table 2: Is all this information necessary?

We have deleted this table, and merged information from the original Tables 1 and 2 in the new Table 1.

Results and discussion: 1. Sections 4.1 and 4.2 do not seem to add much to what is already known about ozone and PM. These sections should probably be made much shorter.

We agree with the reviewer and therefore we have condensed the information, reducing the number of lines by almost 40 %. In the present version of the manuscript, these sections focus more on the similarities between meteorology and air quality for different rural stations in The Netherlands.

2. Table 4 is probably not necessary.

We have deleted Table 4 as well.

3. Figures 6 and 7: These could use either a legend or more explanation in the caption, explaining what the weather type abbreviations are. Also, something happened to Figure 7 to make it less clear.

In order to explain the circulation types, we added the following information in the caption of Figure 8 (previously Figure 5): Monthly mean frequencies of Lamb Weather Types over the period 2001-2004. The acronyms of circulation patterns are as follows: U = unclassified, NW = northwest, W = West, SW = southwest, S = south, SE = southeast, E = east, NE = Northeast, N = North, C = Cyclonic and A = Anticyclonic.

Validation: Table 6 has numbers with anywhere between 1 and 4 significant digits, such as 4 and 27.98 respectively. These should all have the same number of significant digits. (I would suggest 2 or 3.)

The numbers in Table 3 (previously Table 6) have been changed in order to present the same number of significant digits.

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Conclusion: It is a stretch to say that &both multiple linear regression modes provide suitable results for the forecasting O3 and PM10 for Cabauw, given the models R2 values in Tables 5 and 6. There are more accurate methods (albeit, more computationally expensive ones), such as chemical transport modeling.

We understand the reviewers considerations on this matter, thus we have changed the sentence on page 19, line 685 to the following: 'Secondly, both multiple linear regression modes provide promising results in forecasting especially O3 for rural sites in the Netherlands, outperforming both climatology and persistence models.'

Furthermore, in the following section, we clearly state that ‘the statistical performance of both MLRMET and MLRMETCHE is good in comparison to similar studies for both the calibration and the validation period. In order to emphasize this a bit more, we added the following: 'is good in comparison to similar statistical studies for both the'

In general, a much more broad discussion could be held focussing on the comparison of dynamical and statistical models in terms of air quality modelling, each of them characterised by their (dis)advantages, but because of this, we opt not to refer to any results of dynamical air quality models. In doing so, we hope to keep the focus on the statistical approaches, and to present the reader the advantages and disadvantages of the 2 methods (MLR and circulation patterns) discussed here.

Referee: Anonymous [28/01/2009]

The paper is interesting, although limited to just one station in the Netherlands. The major limitation of the approach is however that the impact of the primary driving force of air quality concentrations, the emissions, are neglected. In fig 4 in the observed concentrations of NO, NO2, and even O3, the impact of decreasing emissions over the weekend is clearly visible. It is recommended that the authors make some reference to the impact of emissions, and making clear in this way the limitations of their approach.

The reflection of the anonymous reviewer consists of 2 parts:

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1. the limitation of the original paper to just one station.

This has been discussed extensively when answering similar objections raised by the editor and J. Dawson.

2. The fact that the emissions are not taken into account as the primary driving force is a major limitation of this approach.

We agree with the reviewer that local emissions are an important source, and should be taken into account for short-term air quality modelling. This is also seen from the multiple regression analysis in the METCHE mode, where previous-day emissions were included as predictor values for the model.

However, the aim of this research is to provide insight in the meteorological and air quality interactions with only limited interference from local emission sources. Therefore, the rural stations are chosen with the expectation that non-local correlations would be more clearly revealed and that the confounding effects of local urban vehicular NOx emissions will be limited. We acknowledge that the development of these models using rural stations only limits the potential application of such an approach to sites where the variability of emissions are of minor importance. In order to clarify this, we have added this sentence:

p5, line 181-182: implying a limitation of this approach to sites where the variability of emissions is of minor importance.

Finally, another reason not to include the emissions in the analysis results from a different constrain, is stated in section 4.2:

'The aim of this research is to develop an approach that is also useful for downscaling operational low resolution or AOGCM output data in terms of air quality assessment on the longer time scales. In this context, there is no information on the future air quality data and emissions as a dependent predictor variable.'

We think it is important to assess the possible future effects of changing meteorology

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and changing emissions on air quality separately. Our final aim is to study the effect of the former and to develop a tool to derive changes in air quality due to possible changes in meteorological conditions only.

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