Response to anonymous referee # 1

This is a useful paper providing a detailed analysis of the effect of meteorological parameter changes, linked to climate change, on various processes affecting aerosol concentrations. While several studies have already been performed in this field, this paper specifically attempts as much as possible separating different effects. This allows making evident the many competing effects acting. In a concluding section, the impact of different parameter changes on PM2.5 levels are compared and relate to a climate change scenario. The paper should be published in ACP after having taken into account groups of remarks.

Major comments:

(1) *Section 3, Base case simulations and model evaluation:*
A discussion should be added on how well processes are represented in the model in order to be confident about the calculated sensitivities with respect to meteorological parameters. The comparison with observations can only give part of the answer. A dynamical model evaluation (i.e. evaluating the model capacity to simulate sensitivities) is difficult and out of the scope of the study. Instead, the confidence in the simulated model sensitivities should be simulated based on our physical understanding. It would be helpful to add a table regrouping the various figures given in this section.
We have re-written the model evaluation part focusing more on the links between model performance and meteorology. We have also added a paragraph in Section 4 discussing the various processes simulated in the model which are targeted by the specific sensitivity runs as suggested by the reviewer. In Table S1 (column 4) we summarize/regroup results from Figures 2 through 8. We think that adding an extra table with more or less the same information would be redundant.

(2) *Section 10, Relative importance of meteorological parameters. It is not clear to me, how figures 9 and 10 are constructed. It seems to me that figure 9 summarizes results with respect to the previously applied changes in parameters.*
That is correct. In Figure 9 we summarize results of all the sensitivity tests that have been presented in detail as maps in Figures 1-8. Fig. 9 shows the sensitivity distribution of average $PM_{2.5}$ to a change (increase) of each one of the meteorological parameters on a percent basis. This is no clarified in the text.

(3) *Figure 10 apparently extrapolates these changes to future climate projections. But how is this done?*
That is correct. In Figure 10 we show the expected concentration change (in μg m^{-3}) calculated based on the $PM_{2.5}$ sensitivity and the expected change of each of the parameters shown in Table S1. For example the predicted sensitivity (simulation-average) of $PM_{2.5}$ to the overall temperature change for the summer period is -23 ng m^{-3} K^{-1} (Table S1, column 4). According to the different IPCC (2007) scenarios, the average temperature in Europe is expected to increase over the next century from 1 to 5.5 K (Table S1, column 2). If we multiply this range with the average $PM_{2.5}$ sensitivity (-23 ng m^{-3} K^{-1}) we get an expected concentration change of -23 to -128 ng m^{-3} (Table S1, column 5). If we do the same for the numbers representing the 10^{th} and 90^{th} percentile (Table S1, column 4, numbers in parentheses) we get the range of expected concentration change (Table S1, column 5, numbers in parentheses). This

range of values (-0.8 μg m^{-3} to 0.37 μg m^{-3}) is depicted in Fig. 10 for the temperature change during summer (red bar in first column). This is now explained in the revised text.

(4) *I doubt a little bit that it is possible to conclude on a +-10% change in wind speed due to future climate scenario. It should be explained that these values are very crude choices.*
As explained now in the text, projections for wind speed in Europe vary significantly with space. Here we use a projected change from −10 to 10% which is based on the IPCC SRES A2 scenario.

(5) *Does the "error" bar in the figures 9 and 10 represent the spatial differences to a uniform parameter change or also include the response to variability of a parameter change itself?*
Each bar in Fig. 9 shows the range due to the spatial variability. The results in Figure 9 are normalized so if the response remains close to linear they should be useful for a range of parameters. The results in Fig. 10 though include both the variability in space and the variability in the parameter change itself. We have added text clarifying this important point.

(6) *Is there a need of linear extrapolation, in order to extrapolate from a 10 % to a 40% change in precipitation intensity? If so, the uncertainty in this procedure should be stated, for instance a saturation of wet deposition with respect to further increase in strong precipitation is expected.*
Yes these estimates are based on a linear extrapolation. This assumption is explained in the text. We have added some text to discuss the corresponding uncertainty in the revised manuscript.

(7) *Temperature induced PM2.5 concentration changes are strongest with respect to those due to other parameters in figure 9, but weakest in figure 10. Is this simply due to the reference (i.e. relative vs. absolute changes)?*
This is not only due to the different units of the y-axis of these figures but also due to the expected future change of each parameter in Figure 10. For example the expected change in precipitation (on a percent basis) is much larger than that of temperature.

(8) *Technical correction: Page 10364 line 19: due mainly to changes "in" sulfate and sodium chloride.*
Done.

Response to anonymous referee # 2.

Review on manuscript: "Linking climate and air quality over Europe: Effects of meteorology on PM2.5 concentrations" by A. G. Megaritis, C. Fountoukis, P. E. Charalampidis, H. A. C. Denier van der Gon, C. Pilinis, and S. N. Pandis The manuscript studies the effects of individual meteorological parameters on PM2.5 concentrations over Europe, derives the sensitivity of PM2.5 to changes in each of the considered parameters and finally estimates the impacts of those meteorological parameters on future PM2.5 levels due to projected climate changes. The work has definitely a relevance to understanding how/why climate change may impact air quality, though the work does not offer any substantial novelty.

Some general comments:
(1) *As meteorological effects on PM2.5 are central in this work, the model's ability to reproduce observations in various meteorological conditions should be discussed in a more clear and transparent manner. I'd recommend to either make a summary about that in the end of Model evaluation part, or better to re-write the model evaluation, looking at each of the individual PM components and analyzing the model ability to accurately calculate it in different seasons. The bottom line is that Model Evaluation should be made shorter, more reader-friendly and (!) should make a clear statement how good/bad the model performance is at variable meteorological conditions.*
We have re-written the model evaluation section focusing on the model's performance under different meteorological conditions for each PM component following the reviewer's suggestion.

(2) *I'd recommend to shorten sections 5 through 9, especially with respect to the amount of numbers, as it is rather hard for a reader to consume all these quantitative information. It is shown in Figures anyway.*
We have reduced the amount of numbers trying to make these sections shorter and more reader-friendly as suggested by the reviewer.

(3) *Calculating the relative importance of meteorological parameters on PM2.5 (sec. 10), the authors assume the same meteorological changes all over in Europe. As climate predictions indicate, there will be regional differences in the change of different meteorological parameters (for example, larger increase of winter temperatures in Northern Europe and smaller in Central/Southern Europe, whereas the opposite in summer). Those differences will overlay the differences in chemical regimes around Europe (thus different predominating PM components possessing different properties). Could the authors discuss on if/in which way these inhomogeneities may have significant effect on the main conclusions.*
This is a valid concern. That is why we use a range of values for the expected changes of each parameter (Table S1) and focus on the sensitivities. These sensitivities are expected to be more robust and be applicable to a zeroth order at least over the range of expected changes. Given that, we do not expect that regional differences will significantly change our conclusions.

Other comments:

(4) *p. 10347 lines 9-10. Introduction: "Over past decades, increased levels of … PM" - do the authors imply that PM levels have been increasing? Everywhere? - Then references should be made. Anyhow, PM is affecting both human health and climate even at average (background) levels.*
We have re-phrased this to avoid any misunderstanding. This sentence is now changed to: "*Over the past decades, atmospheric particulate matter (PM) has received considerable attention due to its impact on human health, climate change, and visibility*."

(5) *p. 10348, 10367: Better to refer to the latest IPCC report (2013)*
Done.

(6) *p. 10349, 4-5: "increasing mixing height in S-E Europe – above 100 m " - probably means "increase by more then 100 m"?*
Corrected.

(7) *p. 10355, 16-21: The authors explain the model over-prediction of PM1 nitrate and ammonium in Mace Head by the assumption on bulk equilibrium and shift to coarse mode. But would not this cause in less fine ammonium nitrate?*
The shift to coarse mode is what is probably occurring in the atmosphere under the presence of high levels of sea salt. This shift, however, is not well captured by the model, which predicts most of the nitrate at the fine mode. We have added a recent reference (Trump et al., 2014) that explores this issue in detail for Mace Head.

(8) *p. 10368, 9-11: "During all seasons, the increased volatilization of ammonium nitrate dominates, causing large decreases in PM2.5 with increasing temperature". However, it seems from the model evaluation tables S1 and S3 that modeled nitrate tends to be too sensitive to temperature. Could the authors comment on this.*
The sensitivities of ammonium nitrate to temperature in Table S1 are in the expected range based on the corresponding thermodynamics. Similar decreases of nitrate levels over the continental Europe, as temperature rises, was also shown by Aksoyoglu et al. (2011).

References

Fountoukis, C., Megaritis, A. G., Skyllakou, K., Charalampidis, P. E., Pilinis, C., Denier van der Gon, H. A. C., Crippa, M., Canonaco, F., Mohr, C., Prévôt, A. S. H., Allan, J. D., Poulain, L., Petäjä, T., Tiitta, P., Carbone, S., Kiendler-Scharr, A., Nemitz, E., O'Dowd, C., Swietlicki, E., and Pandis, S. N.: Organic aerosol concentration and composition over Europe: insights from comparison of regional model predictions with aerosol mass spectrometer factor analysis, Atmos. Chem. Phys. Discuss., 14, 7597–7635, 2014.

Karydis, V. A., Tsimpidi, A. P., Fountoukis, C., Nenes A., Zavala, M., Lei, W., Molina, L. T., and Pandis, S. N.: Simulating the fine and coarse inorganic particulate matter concentrations in a polluted megacity, Atmos. Environ., 44, 608–620, 2010.

Trump, E. R., Fountoukis, C., Donahue, N. M., and Pandis, S. N.: Improvement of simulation of fine inorganic PM levels through better descriptions of coarse particle chemistry, under review, 2014.

Tsimpidi, A. P., Karydis, V. A., Zavala, M., Lei, W., Molina, L., Ulbrich, I. M., Jimenez, J. L., and Pandis, S. N.: Evaluation of the volatility basis-set approach for the simulation of organic aerosol formation in the Mexico City metropolitan area, Atmos. Chem. Phys., 10, 525–546, 2010.