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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Extensive spatio-temporal analyses of surface ozone and related meteorological variables in South Korea for 1999–2010" *by* J. Seo et al.

Anonymous Referee #1

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This manuscript presents a thorough analysis of the spatial characteristics and temporal changes of surface ozone (O3) over South Korea during the period 1999-2010. The KZ filter (used to decompose time series into short-term, seasonal and long-term components) is combined with linear regressions to examine the relationship between meteorological variables and ozone over different areas. The authors conclude that baseline temperature and insolation are relevant for the baseline ozone levels inland and in the Seoul Metropolitan Area (SMA), while the transport of regional background air masses impact on ozone concentrations on the coast. Their analyses of the probability of O3 exceedances as a function of temperature or of the relationship between

the exponential of the short-term component of O3 with wind speed are very interesting. They also use singular value decomposition (SVD) to assess the possible impact of changes in NOx on ozone concentrations. The authors are fair, they give credit to previous work and recognise most of the limitations of their analyses. This valuable study will contribute to improving our understanding of recent surface ozone changes and will also be useful for future projections.

I do not have any major objections to the scientific content, methods used or conclusions. I only have some comments and suggestions to clarify a few points (see section "Specific comments"). However the use of English and style should be improved. I have provided an annotated version of the paper to assist the authors with this, but I expect that somebody with good written English skills revises the language very carefully before the final submission. I will fully support the publication in Atmos. Chem. Phys. once this latter issue has been addressed by the authors.

SPECIFIC COMMENTS

* As Indicated above, the authors give credit to previous work. However, further discussion about recent changes in tropospheric ozone over the Northern hemisphere is needed. Both in the introduction and conclusions the authors mention the recent increase in O3 levels over the Northern Hemisphere and East Asia. This may be the general case for most of East Asia, but the picture for Europe or North America might not be so clear, in particular over the last decade. The authors cite papers such as Vingarzan et al. (2004). This paper already indicates that trends were not uniform in the years preceding 2004, although both they and more recent publications suggest that background ozone levels over the Northern mid-latitudes have continued to rise. Some authors (e.g. see below some papers by Samuel Oltmans) point to significant regional differences and to the flattening of O3 levels in the Northern mid-latitudes over the last years. It is also known that ozone trends derived from different platforms are not always consistent with each other (see some relevant literature e.g. in the introduction of Saunouis et al., 2012), which might also be responsible for some of the regional

differences. To conclude, I think the authors should include a short sentence (and cite one or two relevant publications) to indicate that there is no clear consensus on the increase of ozone in the North hemisphere over the last decade.

* Section 2.1. The authors mention that observations of O3 and NO2 are available at 290 sites while they use 124 of them. Is that selection based on data availability? Please provide details on the selection criteria.

* The authors use KZ-29,3 to filter the short-term component (period smaller than 50 days). Then they do a meteorological adjustment of the baseline ozone concentrations and finally apply KZ-365,3 to extract the information for periods larger than around 1.7 yr. This looks reasonable, but I wonder myself how sensitive results can be to the choice of the window length (m) and iterating times (p) used in the KZ filter. Could the authors explain how/why they have chosen those specific values of m and p? Were they looking for the mentioned periodicities (around 50 days and 1.7 yr)? Is this based on previous work? Or has this been done following trial and error?

* I understand that the residual "delta(t)" in equations (6) and (7) is not part of the long-term component and that it is the part of the seasonal component which cannot be explained by the meteorological regression model. Is this right? If so, shouldn't the authors test that the statistical characteristics of that residual are similar to that of white noise (e.g. normality, no autocorrelation, homoscedasticity)?

* Section 3.2: The authors indicate that "The nationwide average of R-squared is 0.50 for surface insolation (SI), 0.29 for PS, 0.22 for Tmax, 0.14 for TD, 0.05 for RH, and 0.03 for WS, respectively". R-squared is basically the variance explained by each variable. I think it would be very relevant to also know how much of the variance they are able to explain with all meterological variables combined. Considering that they have used a multiple linear regression model of baseline ozone on the baseline of those meteorological variables (see eq. 5 or 7), why don't they also indicate the value of R-squared for that model?

* A question on the choice of variables used. In section 3.2. the authors show that R-squared values are higher for insolation than for Tmax, and mention the more indirect effect of T on O3 production (Dawson et al., 2007). But have they tried to use daily T averaged at daytime instead of Tmax? Please note that this is just a suggestion, not a major concern. I do not expect the authors to modify their analysis at this stage. Only if this is not too onerous it would be interesting to know if with a different choice the explained variance may improve. It might also be good to briefly introduce why some of the meteorological variables indicated in the paragraph above (e.g. TD, RH) are used.

* Similar question (about the impact of T and surface insolation SI on O3) but from a different perspective. I understand that the correlations in Fig. 5 are done for all baseline data, considering the warm and cold seasons. I expect the surface insolation to have a stronger impact than T in winter, since it will favour the vertical mixing of pollutants and reduce the O3 loss by titration while the effect of temperature might be not so clear. Might it be that T becomes much more relevant during the high ozone season (May – October) and that for that period the values of R-squared for [O3,BL] with T,BL and SI,BL become much closer than shown in Fig. 5?

* Another comment following the one before: In the conclusions the authors say "The high meteorological influences in the SMA and inland regions are related to effective photochemical activity, which results from large local precursor emissions and stagnant condition with low wind speeds". This would be true during the high O3 season, but most of the time they show results for the whole year.

* Throughout the paper the authors mention that meteorological effects (temperature and surface insolation) on ozone levels are high at the inland and SMA cities and low at the coastal cities, where the wind speed and long-range transport are more relevant. For instance, they finish section 3.2 with the sentence "Therefore, the meteorological effects on the O3 productions become more important in the inland region where the wind speeds are lower". It is very clear to the reader what they mean by this. However, I would also consider the wind speed to be a meteorological effect and therefore I am

not sure the terminology they use is the most apropriate one. Is there another possible way of expressing this?

* Section 3.4 (Relative contributions of O3 variations in different time-scales). As indicated by the authors, Figs. 9a and 9b illustrate the negative relationship between the relative contributions of [O3,ST] and [O3,season]. They say “the large relative contributions of [O3,ST] at the coastal cities indicate the stronger effects of the synoptic-scale transport of background O3 there”. The highest contribution of [O3,ST] is in the North East, where I believe there is only one ozone monitoring site. Figure 1c shows the location of that site, close to the coast (on the East) but also to the mountains (on the West). I have a couple of considerations: (a) Is there any particularity about the location and topography of that site that might cause the very high relative contribution of the short-term component there? (b) Taking into account the lack of other O3 monitoring sites in the proximities, I assume the spatial interpolation performed with the AIDW method might not work so well for the elevated area in the North East; this could affect any of the contour plots shown in the paper. This will not affect the validity of the main conclusions of the manuscript, but it might be worthwhile to acknowledge it.

TECHNICAL CORRECTIONS

The list of technical corrections would be too long to list here. As indicated above I have provided a pdf version of the paper with annotated changes (see supplement). Please do not pay attention to the formatting (the text was simply copied from the ACPD printer-friendly version to a text editor), but to the changes annotated in that document.

REFERENCES (From the papers mentioned above, only those which do not appear in the ACPD manuscript are listed here)

Oltmans, S. J., et al: Recent tropospheric ozone changes – A pattern dominated by slow or no growth, Atmospheric Environment, 67, Pages 331–351, 2013.

Oltmans, S. J., et al: Long-term changes in tropospheric ozone, Atmospheric Environ-

ment, Volume 40, Issue 17, 3156-3173 , 2006.

Oltmans, S. J., et al: Trends of ozone in the troposphere, Geophys. Res. Lett., Volume 25, Issue 2,139–142, 1998.

Saunois, M., et al. Impact of sampling frequency in the analysis of tropospheric ozone observations, Atmos. Chem. Phys., 12, 6757-6773, 2012.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C280/2014/acpd-14-C280-2014-supplement.pdf

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