

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 #2

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This manuscript shows an interesting study on investigating the characteristics of surface ozone variations over south Korea in the past decade. The Kolmogorov-Zurbenko filter (KZ-filter) is used in the time series analysis based on air quality data and meteorology records. The comprehensive analyses present the diversity of spatio-temporal variability over different regions and the impact of meteorological factors. This study explains the variability in detail, which makes it a very valuable study for surface ozone change in South Korea. The text is concisely written. I recommend its final publication after addressing several concerns that have been identified as noted in the suggestions below.

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Suggestions for addressing certain details necessary for the revised manuscript are also listed below:

1. Before doing meteorological-ozone correlation study, it is necessary to exclude possible effects of emission change on the surface ozone variability. Maybe a few sentence to discuss whether the local emission change is significant during study period.

In addition, you can include more associated study in discussing Asian ozone change due to industrial development. For example, Lei et al., 2012 shows that the future surface ozone change in Asia is more affected by changes in local emissions than that in remote emissions.(Lei, H., Wuebbles,D., Liang, X.-L., Olsen,S., 2012. Domestic versus international contributions on 2050 ozone air quality: how much is convertible by regional control?, Atmospheric Environment, V68, Pages 315-325, doi: 10.1016/j.atmosenv.2012.12.002)

2. The study is based on observational air quality and meteorological data. The raw data may contains large uncertainty due to various reasons including inconsistency, diversity in measuring method, geographic affects etc. which limits their representativeness in the analysis for a long term trend. I noticed that a quality control strategy may be used. As presented in the manuscript, it seems that only 124 sites out of 290 sites are used in the analysis. What is the quality control criteria? That needs to be clearly introduced.

3.The K-Z filter method is used. It is necessary to add text to clearly explain why the coefficients are selected (e.g. 29, 3, 365, 3), and how their value or uncertainty may affect your conclusion.

4.It is also necessary to discuss the limitations on the K-Z filter method and resulted uncertainty in the result. For example, if considering the precipitation change as a driver for ozone variation, it is noted that precipitation has large seasonal and annual variability. Therefore, the statistic method filter short-term component may contain contributions that were originally caused by seasonal precipitation system. It is better

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to discuss the quality of the analysis in the text.

5. I suggest plotting fig 2(d) with a broader value range and stating that the long term surface ozone change is not distinct in the text. In fact, previous studies on observed surface ozone change on other parts of world show that there is no clear increasing trend for surface ozone. Considering the biases in measurements, the significance of result need to be discussed.

6. In the section 3.2, daily minimum ozone in the coastal cities may not be good as an indicator for background ozone effect. Usually, the minimum surface ozone concentration occurs during nighttime. The change of minimum ozone is more affected by multiple factors, mainly including nighttime NO_y chemistry, temperature, nighttime surface mixing etc. High temperature accelerates the removal of ozone by NO_y chemistry, and thus associated with lower ozone concentration. There would have other environment factors and chemicals in affecting NO_y chemistry (e.g. Lei, H. and Wuebbles, D., Chemical Competition in Nitrate and Sulfate Formations and its effect on Air Quality, *Atmos. Environ.*, 80, 472-477, doi:10.1016/j.atmosenv.2013.08.036.2013.), which indirectly affect the change of minimum ozone.

In addition, using wind speed at the time of minimum ozone occurs in the analysis can be meaningful. From figure 7, it seems that inland sites have higher wind speeds, while coastal sites have lower speeds. Low and High ozone concentrations occur on each region. This is more of a local characteristic. In addition, the minimum ozone usually occurs in nighttime. Therefore, there is no photochemistry as described in the manuscript. The description in this part may not stand. This part should be reanalyzed.

7. In the section 3.4, the site of Gangneung locates on the back of mountain. The ozone change there would be more associated with the topography. I would suggest you to discuss these factors that have more sensitive influence to local surface ozone change.

8. It is very interesting to have the wind direction analysis in section 3.5. However, I have a general question on this part. Does some of the wind directions (e.g. easterly wind,

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northeasterly wind.) are much less occurring than westerly wind? If so, the discussion in the text should include this aspect.

9. In the SVD analysis on 3.6, the long term ozone concentration has been filtered out a lot of information. It is clear that the short term variability over Korea takes more information for surface ozone change in Korea. Therefore, it is necessary to discuss this in the text. In particular, I am very interested in the results by using the short term or seasonal ozone information in SVD.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 1191, 2014.

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