

Interactive comment on “Insights into a historic severe haze weather in Shanghai: synoptic situation, boundary layer and pollutants” by C. Leng et al.

Anonymous Referee #2

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1 General Comments

Authors report 10 days of aerosol optical, physical and chemical properties in December 2013 along with meteorological fields. Though authors don't explicitly state they imply during this period (1-10 Dec 2013) historically record high concentrations of PM10 and PM2.5 particles were observed.

The strong or the best aspect of this manuscript is large number of complimentary set of observations, however, authors have not utilised them to draw any strong scientific conclusions or bring about clarity in hitherto less understood aspects of haze forma-

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tion. In particular, part of the manuscript where statistical analysis is reported to bring out casuative relationship between visibility and aerosol properties needs expansion. Overall, the topic is of high public interest and the scientific community will be interested in knowing properties of particles reported in this manuscript as to understand what constituted the high haze events in Yangtze River Delta region.

The manuscript contains too many grammatical mistakes and often sentence structures are difficult to get meaning out of it. I highly recommend editing of the manuscript by native English speaker.

2 Specific Comments

1. Page 32563 Line 19 about correlation between visibility and water soluble ion. Looking at the Fig. 13 it is difficult to judge whether the low correlation is outcome to selection of wrong function to fit? If that is not the case authors may explain in manuscript.
2. Page 32564 Line 5 about indirect effect of aerosol. Aerosols do not modify precipitation itself but modify precipitation efficiency of the cloud. Authors may consider re-framing their statement.
3. Page 32564 Line 17: None of the three citations in bracket refers to haze phenomena over Africa!
4. Page 32564 Line 22: I can understand economic development may result in more air pollution but don't understand why social development will also result in more air pollution?
5. Page 32567 Line 21: The attenuation can be calculated ...". Proper citation for this statement are Weingartner et al., (2003) and Hansen et al., (1984).

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Weingartner, E.; Saathoff, H.; Schnaiter, M.; Streit, N.; Bitnar, B. & Baltensperger, U. Absorption of light by soot particles: determination of the absorption coefficient by means of aethalometers *Journal of Aerosol Science*, 2003, 34, 1445-1463, doi: [http://dx.doi.org/10.1016/S0021-8502\(03\)00359-8](http://dx.doi.org/10.1016/S0021-8502(03)00359-8)

Hansen, A. D. A.; Rosen, H. & Novakov, T. Aethalometer: An instrument for the real-time measurement of optical absorption by aerosol particles NASA STI/Recon Technical Report N, 1983, 84, 13538+

6. Page 32567 Line 23: How was it determined that data were affected by dust? Is there any statistics available on how many such data points removed?
7. Page 32567 Line 28: Authors claim that MPL is used world wide however they cite only its use in Europe and America. Authors may consider citing use of lidar for aerosol study from Africa, Australia, Asia, if they know so.
8. Page 32568 Line 5: Authors have cited He et al. (2006) for overlap correction, however, He et al. (2006) themselves have relied on Campbell et al. (2002).
9. Page 32568 Line 11-12: As in previous comment, He et al. (2006) have not discussed errors of overlap correction instead they have cited Welton et al. (2002). Moreover the overlap error is instrument specific and the value 10% reported by Welton et al. (2002) may or may not applicable to system used in this study. Authors should provide their own analysis of error though they may use approach taken by Welton et al. (2002) for determining the error.
10. Page 32568 Line 16: What method was used to control relative humidity in nephelometer. If it was using heated inlet it will reduce volatile and semi-volatile aerosols.
11. Page 32569 Line 5: Since the visibility data are discussed in more details later on it will be appropriate if authors provide more details on visibility measurements
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like what type of sensor was used, what was accuracy and if there were specific data filtering, analysis etc. applied to visibility measurements?

12. Page 32570 Line 5-6: It is authors assumption that severe haze event might have caused health problem. Either authors should state it as assumption or cite study that has assessed impact of haze on health.
13. Page 32571: On this page authors have compared their values with concentrations reported in various literature. Observations in the current study are limited to only first 10 days of December. It is not clear whether values reported in literature belongs to same period, month, or season. Authors may consider revising text such that literature based values are mentioned along with what time of a year they represent.
14. Page 32572: Were BC concentration or absorption coefficient estimates corrected for shadowing effect? Weingartner et al. (2003) have shown that depending on mixing state of soot particles, age and source type, shadowing effect can cause upto 47% error. Gadhavi and Jayaraman (2010) have reported upto 8% of error due shadowing effect in rural regions of India.

Gadhavi, H. & Jayaraman, A. Absorbing aerosols: contribution of biomass burning and implications for radiative forcing *Annales Geophysicae, Copernicus Publications*, 2010, 28, 103-111, doi: [10.5194/angeo-28-103-2010](https://doi.org/10.5194/angeo-28-103-2010)
15. Page 32572 Line 9 & Line 17-19: There is some-thing wrong about value and units of absorption efficiency. Usually mass absorption efficiency is reported in unit of m^2/g . Assuming it was typographic error. Value of alpha (mass absorption efficiency) turns out to be $8.28m^2/g$ for 6th Dec but $7m^2/g$ for clean period based on the values provided in the brackets. Authors may throw light on to this discrepancy.

16. Page 32572: Absorption coefficient, scattering coefficient and extinction coefficients are function of wavelengths. At what wavelength extinction coefficient was calculated?
17. Page 32573: Authors attribute two peaks in diurnal pattern of number concentration to rush hour traffic. 7th December was Sunday. (Assuming Sunday is holiday in Shanghai) Why is 7th December peaks are not any different from previous days (in fact they are higher than later days) if these peaks are due to rush hour? In fact the statement is contradictory statement to your discussion about effect of boundary layer dynamics on concentration. To have well defined peaks such as shown in Fig. 5 due to peak hour traffic, life-time of particles should be less than an hour. To have similar peaks due to action of boundary layer dynamics, life-time of particles should be more than a day or so. Both the effect can be simultaneously only if the observations were made close to traffic junction.
18. Page 32573: Authors state that number size distribution is wide during hazy episodes. Authors may consider including analysis on whether this observation consistent with hygroscopic growth of the particles.
19. Page 32573: In discussion of CCN, authors may consider discussing what fraction of N is CCN and whether that fraction changes between hazy and clear days?
20. Page 32575: conclusion drawn in this section about vehicular vs stationary sources based on ratio of nitrate to sulphate ion is not included in conclusion section as well as in abstract! Why?
21. Page 32579 Line 12: Authors should note that MODIS AOD values are not daily averages. They are one value per day at satellite overpass time.
22. Page 32582 Line 10-11: Authors state that during Haze (which is also a period of high pollution) kappa values are high but during clear day kappa values are

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low. This is in contrast to Gunthe et al. (2011) who reported decrease in kappa value with increase in pollution. Also the kappa value reported in this study are substantially lower than values reported for Beijing by Gunthe et al. (2011).

Gunthe et al., Cloud condensation nuclei (CCN) from fresh and aged air pollution in the megacity region of Beijing, Atmospheric Chemistry and Physics, vol. 11, 11023-11039, 2011

23. Page 32582: For a correlation analysis to be useful, authors should report more details on how did they arrived on choice of functions for fitting, what were significance test done?
 24. Page 32592 Fig. 1 In spite of low PM_{2.5} and PM₁₀ on 8th December (and 4th Dec. morning) visibility is low. Authors may elaborate on it in the manuscript.
 25. Page 32598 Fig. 7: CO concentration in range of 1 to 3 $\mu\text{g}/\text{m}^3$ are extremely low values. For example Gao et al. (2005) have reported CO values of the order of several hundred $\mu\text{g}/\text{m}^3$ at Mt. Tai. I expect CO concentration in Shanghai higher than Mt. Tai.
- Gao et al., Observational study of ozone and carbon monoxide at the summit of mount Tai (1534 m a.s.l.) in central-eastern China, vol. 39, issue 26, 4779-4791, Atmospheric Environment, 2005.

3 Technical Comments

1. Page 32564 Line 5: Use of word "lights" for terrestrial radiation is not appropriate as it usually implies electromagnetic radiation in visible spectrum. I suggest to use word "electromagnetic radiation" instead.

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2. Page 32564 Line 7: I think "low atmospheric layer" should be "lower atmospheric layer"
3. Page 32565 Line 15: The statement "Studies of haze have ..." is a trivial statement, I suggest to avoid such statements.
4. Page 32565 Line 18: "... Shanghai based on online water ..." What is meaning of online here?
5. Page 32566 Line 12: "annual man" ; I suppose you meant "annual mean"
6. Page 32570 Line 25: Do you mean "atmospheric boundary layer"?
7. Page 32573 Line 2: What is meaning of word "integrating" here. Appears to me confusing along with word "size-resolved" used in the same sentence.
8. Page 32576 Line 1: "clod" -> "cold"
9. Page 32576 Line 15: "filed" -> "field"
10. Page 32580 Line 6: "filed" -> "field"

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32561, 2015.

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