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Interactive comment on "Aerosol Optical Properties at SORPES in Nanjing, East China" *by* Yicheng Shen et al.

Anonymous Referee #1

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The article entitled, "Aerosol optical properties at SORPES in Nanjing, East China" focuses on the analysis of a long-term (2 year) dataset of aerosol measurements at the SORPES site. The article does present a new dataset that assists in characterizing the pollution at this site; in addition, the authors have compared some of the findings to other sites. The methods are well outlined. I do believe there are some assumptions that do need more detail or explanation. This includes the correction done for the malfunctioning heater in the nephelometer, as well as the assumption of the large role of new particle formation. I have expanded on both of these in the specific comments below. In addition, I would recommend that the conclusions are expanded a little to discuss the relevance and importance of the dataset from this site now that the pollution is characterized in this pollution. For example, in relation to other long-term sites in the

area and in China in general, what role can this site play?

I would recommend that the article is accepted after the comments below are considered.

Specific comments

Section 2.1. I believe a map of Nanjing and surrounding area, including its location relative to other cities in China would be helpful in understanding the site, and also in the analysis of the back trajectories and wind roses.

Page 5. How were the data points that were impacted by the faulty heater in the nephelometer identified? Also, what tests were done to ensure that this correction is not creating an artefact? For example, were those points across the monitoring period or in specific time period? How sensitive are your results to this correction (e.g. both the fact that it had to be done and the constants c and g that were used)? I understand that this correction was done in order to use the data, and that makes sense. However, I would recommend adding some more information on the impact of this assumption on the results.

Page 5 line 34, how was it decided to use the wavelength insensitive Cref? How was it determined that this was the most appropriate?

Page 7 line 26, the Wang et al reference for using a density of 1.7 g/cm-3 is not local to this monitoring station. How was it determined that it is applicable to this dataset?

Page 8 line 6, the first sentence in the first paragraph in 2.4 is not complete.

Page 8, line 10, I would recommend defining what is considered a "small" and "high" value.

Page 10, paragraph starting on line 25 details the AAE range. It is stated here that because the wavelength independent Cref was used, these values are smaller than if a wavelength dependent Cref was used. In the conclusions, it is acknowledged that

using a different Cref would lead to the conclusion that there is brown carbon. As this is an uncertainty that has an impact on the conclusion, I would recommend discussing this on page 10 as well.

Page 11, section 3.2. In the reasons for the seasonal variation there is no mention of the possibility of a seasonal dependence of emissions impacting on the seasonal cycle. Why is that?

Page 12, line 8-10. The increase in SSA is attributed to new particle formation (NPF). However, transport of scattering particles (which is seen in the later analysis) could also lead to a peak in the afternoon when the aerosols are well-mixed. It is not clear why this was only attributed to new particle formation. This assumption that NPF events are responsible for this then also plays a role in the assessment that the aerosols responsible for higher scattering originated outside of Nanjing (page 15). On page 15, it is stated that this is only true for NPF events, however this caveat is not stated in conclusions when this point is made again (page 22, line 21-22). On page 18, line 7 it is stated that there was a previous study looking at the dominance of NPF at the site. This is a key point that should be moved further up in the text to provide support to the discussion of NRF starting on page 12.

Page 14, line 22, the dilution with higher wind speeds is attributed to vertical mixing specifically, but I would imagine that horizontal transport and increased dispersion with high wind speeds would also play a role.

Page 15, line 20-21, what sources of emissions are in direction where the fresh traffic emissions were measured? If there are not obvious roads there, then the assessment of these as fresh traffic emissions would be incorrect.

Page 15, line 23-25, the WNW sector was said to have very few points (page 14, line 25-26), thus how robust is the assessment of the age that comes up with controversial results? It does look from Figure 5 that the winds come from this direction mostly in winter, can that help to understand the potential source of pollution (e.g. NOx not

emitted with BC)?

Section 3.6.1 states that AOP are not as closely related to GMD as SMD and VMD. Then why was GMD used in Figure 8 and Figure 4, where the general relationships and trends are discussed?

Technical corrections

Page 1 line 26, “in” is duplicated

Page 1 line 32 “of” is duplicated

Page 1 line 34, “b” is referred to but is not yet defined.

Page 2 line 26, as stated, there are many studies in these areas, and thus the ones listed are only some of the studies. I would recommend using “e.g.” before the references to indicate this.

Page 2 line 35, what year was SORPES station started?

Page 4 line 2, “hygroscopic growth is usually significant when RH increase above 50%RH” is this statement specific to this site or a general statement?

Page 4, line 27, For readers who are not familiar with SORPES, I would recommend moving this reference for the overview of the site (Ding et al 2013 and 2016) at the beginning of this section.

Section 2.3.4, the terms for equations 9-11 are not all defined in the text.

Page 8, line 34, PM2.5 are particles with an aerodynamic diameter less than or equal to 2.5 um.

Page 12, line 5, “wee hours” is colloquial; I would recommend using another term.

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