

This study designed a field measurement system and found a new method to better calculate the real part of refractive index where information aerosol density is available. The topic of the study is undoubtedly of high scientific and practical importance. On the whole, the experimental methodology and data analyzing procedures look to be correct and the findings will significantly improve the estimation of aerosol radiative forcing. Therefore, this manuscript is helpful for the audience of atmospheric chemistry and physics, but not without a major revision. Some comments and suggestions are listed below.

### **Main Points**

(1) I am a little concerned about the title “A new parameterization scheme of ...”. For a parameterization scheme, parameters should be changeable at different situation. In this study, a coefficient of 0.18 is obtained from two field measurements and should be applicable to the polluted regions. But as shown in equation 2, the definition of this coefficient suggests that it would vary with the molecular polarizability and molecular weight of the aerosols composition. As a result, I am not sure whether the coefficient 0.18 would be applicable for sea salt aerosols or organic-dominated aerosols. An alternative way is to limit the scope to a certain kind of atmospheric condition. By the way, is the parameter derived from the two measurements exactly the same?

(2) The usage of “RRI” or its related form in the paper is always confusing. Whether it is measured or calculated, whether it is size-resolved or not. It is suggested to double-check the usage of “RRI” or its related form all through the paper. A clear parameterization table with definition would be helpful for readership to better understand the paper. Also, why the authors use different size-resolved RRI at different places? I think there are size-resolved RRI at 200nm, 250nm and 300nm in different discussion.

(3) The structure of the current manuscript is not well organized. For example, in the Data and Methods part, the readers will have an expression that this paper is based on one campaign in Taizhou. However, in the discussion part (line 298), the measurement data at PKU site is also used, but without any description of the measurement. Another example is that there is too much background discussion and methodology in the conclusion part.

(4) The authors may need to be more careful on some statements made in the manuscript. For example, line 344, “Our proposed parameterizations scheme is a perfect substitute” is not appropriate for a scientific paper. Also, line 258 “the RRI tend to increase with the OM mass fraction ratio”. I don't recognize clear trend in fig 7. A simple hypothesis testing may be needed here.

(5) The mode 1, 2, 3 derived from DMA-CPMA-CPC measurement are considered as light absorbing aerosols, scattering aerosols and double charged aerosols. Though the aerosols with lower density are very likely the fresh emitted light absorbing aerosols, those with higher density could also be fully aged light absorbing aerosols. In my opinion, mode 1 is more like “fractal aerosols” and mode 2 is “compact aerosols”. This definition may not influence the final conclusion, but still need to be carefully discussed. One suggestion is to compare the aerosol number in Mode 1 and BC number

concentration measured by SP2 at different size to make sure they are comparable.

(6) Line 20, the authors stated “For the first time, the size-resolved ambient aerosol RRI and peff are measured simultaneously by our designed measurement system”. Since the particle size (also chemical compositions) is linked to distinct formation processes and stages of haze development, such as nucleation and growth from clean, transition, to polluted periods (Guo et al., Elucidating severe urban haze formation in China, *Proc. Natl. Acad. Sci. USA* **111**, 17373, 2014; Wang et al., Persistent sulfate formation from London Fog to Chinese Haze, *Proc. Natl. Acad. Sci. USA* **113**, 13630, 2016), it would be necessary that a connection between the RRI and haze development is identified.

(6) I also believe that some references in this paper were outdated, and a significant effort is needed to address such. Below are some examples.

Line 26, The author stated that “Atmospheric aerosols can significantly influence the regional air quality and climate system by scattering and absorbing the solar radiation (Seinfeld et al., 1998)”. Several other most recent papers on this topic need to be discussed (i.e., An et al., Severe haze in Northern China: A synergy of anthropogenic emissions and atmospheric processes, *Proc. Natl. Acad. Sci. USA* **116**, 8657, 2019; Zhang et al., Formation of urban fine particulate matter, *Chem. Rev.* **115**, 3803, 2015; Wang et al., Light absorbing aerosols and their atmospheric impacts, *Atmos. Environ.* **81**, 713, 2013).

### **Technical comments**

Line 13, delete “Mainly”

Line 15, change “Results” to “The results”

Line 16, the sentence “vary by 40% corresponding to the variation of the measured aerosol RRI” is confusing.

Line 19, delete “schemes”

Line 173, “relations ship” should be “relationship”

Line 301, “equation 7” should be the equation 9?