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Discussions
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Interactive comment on "Aerosol Vertical Mass Flux Measurements During Heavy Aerosol Pollution Episodes at a Rural Site and an Urban Site in the Beijing Area of the North China Plain" *by* Renmin Yuan et al.

Anonymous Referee #1

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East Asia, especially China, is facing heavy haze pollution in wintertime. Though many measurements on air pollutants have been extensively conducted across China, there is still a lack of flux data on pollutants, which may play a substantial role in haze formation. This study combines measurements on meteorological conditions, flux, as well as PM2.5 concentration in BTH region to derive aerosol vertical mass flux, and provide some observational insight on aerosol vertical flux under stable condition. Therefore, this manuscript adds to our current knowledge of aerosol vertical exchange and its impact on meteorology. However,I have some concerns about the methods/data analysis

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Discussion paper

used in the study and the interpretation of results , and more in-depth analysis and discussion ought to be provided.I think this manuscript can be considered for publication only if the authors could adequately address the comments below.

Major comment: 1.There are two observation sites, a rural site (GC site) and an urban site (CAMS site). The Monin-Obukhov similarity theory (MOST) is applied in rural site because the surface is homogenous. But in the urban site, the observation was within the urban roughness sublayer (3-5 mean building height), MOST is invalid due to the lack of constant-flux conditions, the local similarity theory should be used. In other words, the function or the parameters in the similarity relationship should be different for the rural and urban site.

2.The function and parameters of the similarity relationship are not universal, the authors should explain why they use these function and parameters in the paper. For example, in Eq. 4, the authors said that they take the parameters b1 and b2 follow DeBruin et al., 1995. But in DeBruin et al., 1995, it said that "For stable conditions there is no consensus on the universal function", b1=5,b2=0 were found by DeBruin et al., 1993, and "the scatter was very large". So DeBruin may not be the best choice. Especially, in Yuan et al., 2016, the parameter b1 and b2 follows Wyngaard et al., 1971., which is very different from DeBruin et al., 1993. When b1 and b2 follow DeBruin et al., 1995, it means that $\eta(\xi)$ stays constant with stability; but when b1 and b2 follow Wyngaard et al., 1971, it means that $\eta(\xi)$ changes constant with stability. The author should explain why they choose DeBruin et al., 1995.

3.In L359 The conventional meteorological parameters were measured at 20m above the ground surface. But in L275, the author said that the measurement heights of temperature and wind speed were 1.5 m and 10 m at CAMS site (Beijing). It should be clear which data were used to calculate the aerosol flux. Because the average height of the building was 24m in CAMS site, and LAS was located at 43meters. The temperature measured at 1.5m within the canopy layer is different from 43m above the canopy layer, and the calculation of aerosol fluxes from Eq. 12 was badly influenced.

4. Another issue that the authors need to address is the assumption between AERI(atmospheric equivalent refractive index) and aerosol mass concentration as well as aerosol adsorption. First, there do exist some light-absorbing trace gases in the atmosphere, which may influence AERI significantly. Second, aerosol absorption generally contribute a relatively small part of the extinction. By contrast, scattering components like sulfate and organic matters dominate aerosol extinction during haze pollution episode, especially under high humidity. Last but not at least, aerosol extinction is also closely related to the number concentration and size distribution, which need to be considered here. I do not think it is technically robust to simply get the relationship between the imaginary part of the AERI and the atmospheric aerosol mass concentration in Eq.6.

Minor issues: Some statements in this manuscript are very hard to follow. Language editing is needed for improving the accuracy of language as well as overall readability.

Line 43: Please rephrase 'heavy pollution weather'

Line 48: 'few studies' should be 'few study'

Line 72: what is the boundary layer box model? Usually box model is zero-dimensional.

Line 106: should be 'makes it possible'

Eq. 11: replace z with (z-d)

Line 304:More detail needed, not "personal experience".

Line 378: weakly unstable is not free convection. The free convection assumption was not satisfied at night.

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