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# ***Interactive comment on “TEM analysis of the internal structures and mineralogy of Asian dust particles and the implications for optical modeling” by G. Y. Jeong and T. Nousiainen***

**Anonymous Referee #1**

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General:

This manuscript present a detailed analysis of 9 slices of large (>10 micron) Asian dust particles collected from a receptor site in Korea. The authors use the high resolution imaging obtained with transmission electron microscopy to develop generalized models to be used in optical modeling. I commend the authors for a detailed and innovative approach to particle imaging. However, I raise a few questions about the validity of this approach. First, how can the authors ensure that they are imaging an individual particle as it was present in the atmosphere and not an agglomeration that formed on the collection filter? Second, I have doubts that such generalized models can be of much

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use for radiative calculations and remote sensing considering they were developed on the basis of a few particles. A good portion of the manuscript is devoted to implications; I did not feel like this section added much to the paper as it was speculative and qualitative in nature. Based on these comments I suggest the authors perform major revisions to the manuscript to focus on the detailed analysis of particles. I think more information can be added to the experimental to be clearer about the statistical nature of their measurements. Perhaps more focus can be placed on the chemical composition as it relates to the source and atmospheric processing (or lack thereof). These general comments are based on specific points listed below.

Specific:

Line 36: “particles” is misspelled

Lines 39-40: The authors state that “There have been many reports on the microphysical characterizations of mineral dust, but no investigations of the internal structures or mineral composition of individual dust particles” Microscopic measurement of individual aerosol particles has been around for some time now. This is stated in the introduction so it is contradicting.

Lines 58-59: The authors should state how the inclusion of this detail will improve radiative transfer modeling. The authors state it is important to include this detail, but is never proven that the detail is needed.

Line 91: The authors need to be more specific about the sort of mass spectrometry: I suggest changing “time of flight mass spectrometry” to “single particle mass spectrometry”.

Paragraph starting on line 85: The word “microphysical” is used without any precise definition. Later on in the conclusions it is stated that microphysical properties are different than “single scattering” properties. Microphysical properties include single scattering properties. The authors need to be more clear about what they are trying to

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say here.

Line 116: The authors should quote values of the refractive indices here.

Experimental: How can the authors be sure that these particles were present as individual particles in the atmosphere? Isn't it possible that these particles agglomerated on the filter? In the process of the sample preparation (Pt coating, carbon "welding" of "loose agglomerates" (line 152)) it seems possible to more permanently "stick" these particles together. Along these lines of thought: did the authors ever obtain closure of their SEM derived size distributions with size distribution measurements obtained in real time (e.g. with an aerodynamic particle sizer or the like)? It is clear that 35 total slices were taken and 9 of those slices were utilized for a high resolution analysis. Later in the paper these high resolution analyses are used to develop generalized models. I do not believe that enough sampling was undertaken to make such generalizations. How many total particles were used to derive the 35 slices?

Line 152: How was the carbon deposited? What form is the carbon in? Amorphous, organic, elemental, graphitic?

Line 165: What does it mean for identification to be "delicate"? I suggest that the authors mean "difficult".

Line 169: Is it possible for there to be other minerals that have the same chemical composition and lattice spacing? Line 212: Is there a reference describing the dehydration behavior of minerals in vacuum? What effect might the FIB have had on the sample? Is it possible the beam disrupted the sample?

Line 242: For the submicron goethite grains: I suggest the authors show the EDX spectra for these inclusions as evidence.

Line 331-332: Types I, II, and III need to be indicated on figs 14 and 15

Line 334: How is abundance quantified? In order to undertake the sort of modeling the authors call for, these results need to be quantified.

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Line 342-343: Provide a reference for the formation of the clay coatings. Section 3: This section is too speculative. The authors have performed not calculations to prove that the detail provided by the measurements will have implications for radiation models. I suggest that this section be removed or bolstered with calculations. The application of the structural models presented in section 3.2 to detailed models of light scattering would be one way to accomplish this, however, it is not clear how abundant those types are.

Conclusions: It is stated: “All microphysical properties, including size distributions, particle morphology, and composition should be known and accounted for to allow for realistic optical single-scattering treatment”. I think this is not feasible for current models, which is why parameterizations and process models are developed. To try and model everything perfectly is beyond the scope of many modeling studies. Also it is stated: “when computing bulk properties, averaging should in principle be performed for single-scattering properties rather than for microphysical properties; what is averaged matters, because the microphysical properties and the resulting single scattering properties are not linearly proportional” Technically, single scattering properties are microphysical properties. The distinction between microphysical properties and single scattering properties needs to be distinguished. But more generally, I think the authors need to be specific about what they mean here: what single scattering properties? Cross sections, phase functions? What microphysical properties are averaged?

Line 499: It is stated that Goethite was the dominant iron oxide. How was this quantified? How statistically relevant is this? Was this determined from the few slices of the few particles that they analyzed? Or, was this measured elsewhere?

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6619, 2014.

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