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Interactive comment on “Retrieval of aerosol single scattering albedo and polarized phase function from polarized sun-photometer measurements for Zanjan atmosphere” by A. Bayat et al.

Anonymous Referee #2

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General comments: The manuscript by A. Bayat et al. deals with observations performed using a sun photometer in a wonderful and not intensively explored geographical area using ground based remote sensing of the atmosphere. The main message of the manuscript is to show the potential of the polarized phase function (PPF) measured using sun photometer observations. The manuscript needs to be largely improved and I request major revisions. First of all, a general comment related to the relationship between the previously cited manuscript. Li et al. (2004) has many parts in common with your manuscript and also the approach followed by the authors looks mostly the

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same. A couple of plots presented by the authors have even the same shape though the measurements presented are obviously from two different places (Beijing and Zanjan). The main objective of Li et al. (2004) is to show the value of polarized phase function for the retrieval or aerosol microphysical properties. However, Li et al. (2004) is only mentioned for the cloud clearing algorithm. The structure of the paper is quite disordered. Both the topic and the results of this manuscript are presented like in a short oral communication, many details should be better addressed and quantified, some sentences that should be considered as conclusions of this paper come before the description of some plots. English needs also to be improved. In the following I report the major concerns I have with this paper. 1. The introduction of the manuscript is mainly focused around passive photometric observations. This is right but other relevant results come from studies performed with other techniques This should be reported as well. For example, aerosols have been largely studied so far using LIDAR, from ground based and satellites (e.g. CALIPSO). Aerosol does not only mean ground based or satellite photometry. 2. In section 3, the authors describe the correlation between the PFF and the Angstrom exponent as well as the anti-correlation between the PFF and the optical depth. In both cases, the reported plots do not allow the reader to go beyond a qualitative analysis of this correlation. Correlation coefficient should be provided for both the plots 3 and 5 along with the linear fitting parameters (also directly on the plot). 3. sun photometer observations are representative of the full atmospheric column: though I assume the distribution of the aerosol over your site might be clear to you, the authors should comment more about the real value of columnar aerosol measurements for aerosol type also considering the lacking of any characterization of the vertical distribution of aerosol over the observation site. 4. It is surprising to see that the authors, dealing with generic shape particles and observing their PFF as shown in section 2 and in the form of the scattering matrix considered, use the Mie theory for spherical particles to cluster/type the aerosols. It seems meaningless to me. T-Matrix should be used and several codes are easily accessible for free through the web portal: www.scattport.org. The only limit in the use of T-Matrix is related to the trade-off be-

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tween particle aspect ratios and effective radius, but prolate spheroids up to 7 microns with aspect ratios of 1.4 can be easily simulated (see Wiegner et al., 2009 Tellus-B). Moreover, T-Matrix allows the authors to take advantage of further aspect related to the particle polarization, like particle orientation distribution. 5. The huge sensitivity of the results obtained with a scattering code to the variation of the imaginary value of the refractive index, known and shown in several papers, gives less value to the discussion of the results presented in figure 7. The data cluster of figure 7 shows two things. First, given the small difference between the curves describing the relation between PFF and the Angstrom exponent (very small for the values of the refractive index of $m=1.45$ and $m=1.50$), the separation among different aerosol types the authors want to introduce is quite forced. Second, the fitted curves show the non-high linearity of the correlation between the PFF and the Angstrom exponent. This could also indicate a limited validity of the authors' hypothesis. Finally, I ask the author to think again about their manuscript and to underline the original aspects they are introducing and the real benefit the retrieval of aerosol microphysical properties can gain from their results. The value of the manuscript could also benefit from the assessment of the relationship between PFF and aerosol intensive properties over a larger dataset.

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