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Interactive comment on "Complexities in the First Aerosol Indirect Effect over the Southern Great Plains" *by* Sam Pennypacker and Allison L. Steiner

Anonymous Referee #4

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Review of Complexities in the First Aerosol Indirect Effect over the Southern Great Plains

This manuscript studies the effect of the first indirect aerosol effect on cloud properties over the Southern Great Plains using a combination of land-based and space-based measurements. There are a number of major and minor comments that need to be addressed prior to consideration for publication.

Major comments: 1. The introduction is missing an important discussion of radiative and semi-direct effects on aerosol-cloud interactions. The manuscript references these as possible explanations for some of the results on line 301, but there is no discussion of this in the introduction. I would suggest referencing some papers on the radiative effect, as well as some recent papers that suggest there may be a smooth transi-

tion between microphysical (IE) and radiative effects such as Ten Hoeve et al., 2012 in JGR Atmospheres and Koren et al., 2008 in Science. In these maximum events, could the aerosol effect already be saturated or even be reversed depending on the chemical characteristics? 2. One of the most controversial assumptions is that surface concentrations of aerosol measured by the IMPROVE sites represent the relative concentration of aerosol at the cloud layer. At a minimum, the authors need to justify this assumption by comparing the IMPROVE data to column-integrated aerosol retrievals from MODIS, AERONET, etc. and/or other sensors such as Lidar which can provide vertically-resolved measurements. 3. The authors make several hypotheses throughout the manuscript with little support behind the statements. For example, lines 219, lines 297-302, lines 307-317. I would suggest the authors perform additional analyses to support these hypotheses. 4. The authors state that one of the reasons why their results are not consistent with the results of Rosenfeld et al. 2014 is the selection of the maximum and minimum thresholds, yet no support is provided to test this hypothesis. The authors should perform a sensitivity analysis of their max and min thresholds to test if their results are robust. 5. The authors attempt to assess the Twomey effect on the cloud effective radius, as well as the impact to COT and CWP. However, these are all inter-related. For instance, the reduction in cloud droplet size will be better observed between low and high aerosol loading conditions if similar cloud sizes are used in the comparison. This has been shown in a variety of studies.

Minor comments: 1. Please further explain the statement on Line 85, "Large-scale dynamics set the stage for mesoscale activity by controlling factors critical to aerosol and cloud microphysics in varying degrees, including tropospheric stability" 2. Please justify why the authors only used the Terra satellite and not the Aqua satellite. 3. The Methods section states that the authors examine profiles of temperatures, specific humidity, and winds to constrain the aerosol-cloud relationships. However, only results for wind speeds are shown in Figure 6. How did the authors decide that other meteorological factors were not important to analyze? 4. Line 313: should be "five" 5. Line 197" "a" should be "at" 6. Line 714: The description of "top" and "bottom" in the parentheses

is confusing.

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