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Interactive comment on “Long-term aerosol-mediated changes in cloud radiative forcing of deep clouds at the top and bottom of the atmosphere over the Southern Great Plains” by Hongru Yan et al.

Anonymous Referee #1

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In this work the authors provide more evidence to the cloud invigoration effect. They are presenting a thorough analysis of more than 10 years using a combination of surface and satellite measurements supported by reanalysis data. They also estimate the radiative forcing of the effect. Although the variety of data sets is impressive, essential information on the analysis is missing.

First of all the authors should provide information on the type of clouds and the typical meteorological states over the SGP site. Examples of essential questions waiting to

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be answered are: Is it only convective clouds, or do stratiform clouds form there? Do they separate frontal from post-frontal systems? Do they combine clouds from many different years and seasons? Do they account for air masses that come from different directions?

The authors use GOES data in 4 km resolution. It is not clear how many pixels they require to define a cloud. Even if they take one (which is not a good practice in remote sensing data analysis), it means that clouds sized below 4by4 km will not be analyzed. A 4by4 km cloud is not small and in case of convective clouds such one-pixel clouds can be 3-5 km thick. In some conditions such clouds could reach the freezing level. Therefore I would have doubts regarding the quality of such an analysis for warm clouds. The authors should note that satellite analysis is always biased to large clouds.

It is not clear why the authors have not used MODIS (at least as supporting information), which have more reliable retrievals and higher resolution.

The authors should explain more about the paper's statistics. How many shallow clouds? What are their definitions to the cloud subsets? Having a GOES image every 30 minutes, they probably sampled many of the clouds more than one time during different stages of their development. Will it affect their results?

Acronym usage is very intensive and it makes the paper's points difficult to follow.

They summarize empirical observations without explaining their physics. Why invigoration is mostly shown in moist environment (competition with entrainment)? Why warm base and mix or cold tops?

Finally, the introduction is not exact. It uses most of the right keywords and many of the important references but not in their precise context. For example, Andrea 2004 did not deal with anvils at all. Anvils were discussed in Koren et al, 2010 (which they cite). They could find few physical insights in the review of Tao et al, 2012 (which they cite) and new ideas in <http://onlinelibrary.wiley.com/doi/10.1002/2013JD020272/abstract>

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