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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "WRF-Chem simulation of aerosol seasonal variability in the San Joaquin Valley" *by* Longtao Wu et al.

**Anonymous Referee #2**

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In this study, the authors use the WRF-Chem model to simulate the seasonal variability of aerosol properties in the San Joaquin Valley. The authors investigate the roles of 1) horizontal resolution of model; 2) dust emission schemes; and 3) meteorology in modeling aerosol properties and compared the model results against ground-based (e.g. IMPROVE) and satellite (e.g. MISR and CALIPSO) observations. This paper has scientific merit to be published on ACP; however, some major revisions are needed.

General comments:

1. Uncertainties in dust schemes

First of all, the authors did not thoroughly describe the dust schemes in the paper, but only cited a paper by Zhao et al. (2010), in which the two dust schemes are used to simulate the dust emissions over Africa. The parameters "C", the empirical



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proportionality constants, in both schemes are tuned for the African dust emissions. Whether the authors use updated or original values for "C" is never discussed in the paper. Since the dust emission schemes are associated with such large uncertainties (in terms of values of C), the discussions in section 4.2 (sensitivity to dust scheme) makes not much sense to the reviewer, because both schemes need to be tuned before any new case studies with different domains, simulation periods, and re-analysis inputs.

In addition, in Zhao et al. (2010), the dust emission schemes are coupled with 8-bin version of MOSIAC, while in Zhao et al. (2013) with MADE/SORGAM. In this paper, the dust emission schemes are coupled with 4-bin version of MOSAIC. Please mention how the dust masses are partitioned in these four bins.

Please also discuss the relative importance of local dust vs. transported dust over SJV.

2. Lack of in-depth analyses

In the paper, the authors demonstrate differences in modeled and observed aerosol properties without giving in-depth analyses. The quality of the paper can be significantly improved if the authors can provide more in-depth analyses other than just quoting conclusions from other papers. Here are three examples:

Lines 239-242: To explain the underestimations of OC in 4km and 20km simulation, the authors quote the explanation from Fast et al. (2014): "low bias in WRF-Chem simulation is primarily due to incomplete understanding of SOA processes." To my knowledge, a simple version of VBS SOA scheme is used in Fast et al. (2014) but not in this Wu et al. paper. If this is the case, then the authors' explanation is definitely wrong. If the VBS SOA scheme is also adopted in this Wu et al. paper, then "incomplete understanding of SOA processes" does not explain the differences between the OC loadings in two cases with different horizontal resolutions because SOA processes are treated the same way in two cases.

Lines 245-248: To explain the low bias in modeled sulfate, the author mention that low

bias in sulfate is also shown at one site Bakersfield in Fast et al. (2014). However, in Fast et al. (2014), the sulfate concentrations over some other sites are reasonable compared to observations. The authors are trying to explain their model results (domain integrated; one-year simulation) by comparing against model results over one site and two-month period from Fast et al. (2014). The authors claim, "it [Fast et al. (2014)] suggests that improvement in understanding the photochemical processes involving sulfate is needed to reproduce seasonal variability of sulfate in the SJV. "; However, Fast et al. (2014) never studies the seasonal variability of aerosol properties.

Section 4.3 The Role of Meteorology: In this section, the authors focus on the role of instability only other than "meteorology". The other meteorological fields also strongly control the aerosol properties, but are never discussed or mentioned in the study. For example, between 4km and 20km, the surface wind fields, which are important for dust emissions, are definitely very different. The precipitation fields, which are important for wet removal processes, are definitely very different between two cases too. The reviewer strongly suggests the authors add these results, because they can also partially explain the differences among three cases (4km, 4km_D2, 20km).

Specific comments:

Figure 1: Add domain-integrated values of daily anthropogenic emissions (miug/day) in each sub figures. Similar to anthropogenic emissions, please add dust emissions for three cases too (not necessarily in figure 1).

Table 2 and Figure 6: it seems that table 2 and Figure 6 provide some same information. It may be better to merge table 2 and Figure 6.

Line 337: Please explain the reason to use climatological fire emissions from GFED instead of using daily fire emission from GFED. The fire emissions from GFED are available for 2013 as mentioned on the website (http://www.globalfiredata.org/).

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