The reviewers' insightful comments are highly appreciated. Below we have listed the referees' comments in black and our response in blue.

We have made the following major revisions in the revised manuscript:

1. More descriptions of aerosol properties simulated in the model are added in the revised manuscript.
2. Two aerosol precursors (NO_2 and SO_2) observed by EPA are included to diagnose model biases in NO_3 and SO_4, respectively.
3. Analyses of meteorological variables, including temperature, relative humidity, wind speed and precipitation, are included.
4. Analysis of Ångström exponent is included to diagnose the model simulated aerosol particle size.
5. More quantitative information, including correlation and bias, is included in the discussion.
6. We have performed some sensitivity experiments to provide more in-depth analyses on model results, including changing the anthropogenic emission source (20km_NEI11), the chemical boundary conditions (20km_BC1) and the PBL scheme (20km_P7).
7. A bug in calculating equivalent potential temperature is fixed in the revised manuscript. The unit of relative humidity was wrong in previous version. The updated profiles of equivalent potential temperature do not change the conclusions of this study.
8. The OC (organic carbon) from observations are converted to OM (organic matter), which is simulated in the model, by multiplying by 1.4 to account for hydrogen, oxygen, etc.

Anonymous Referee #2

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 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 our study, we use the original "C" in Ginoux et al. (2001) and Shaw et al. (2008). It is clarified in the revised manuscript. More analyses about the two dust emissions are also included in the revised manuscript. The low emission in GOCART is due to the source function for potential wind erosion. We agree that "C" in DUSTRAN needs to be tuned for better agreement with observations. As our simulations show high biases of dust at the surface, the "C" value in DUSTRAN are not likely the main reason for low aerosols in the boundary layer in the warm season.

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.

The dust masses are partitioned into four size bins (0.039-0.156 μm, 0.156-0.625 μm, 0.625-2.5 μm, and 2.5-10.0 μm dry diameter), respectively. Aerosols are considered to be spherical and internally mixed in each bin (Barnard et al., 2006; Zhao et al., 2013b). The bulk refractive index for each particle is calculated by volume averaging in each bin. Mie calculations as described by Ghan et al. (2001) are used to derive aerosol optical properties (such as extinction, single-scattering albedo, and the asymmetry parameter for scattering) as a function of wavelength. It is clarified in the revised manuscript. Discussion of the impacts of bin-size assumption is provided in the revised manuscript.

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

The simulated aerosol extinction in the free troposphere above the boundary layer is close to or larger than CALIOP, suggesting that aerosols transported from remote areas through chemical boundary conditions (e.g., the differences between the 20km_BC1 and 20km_D2 runs in Supplementary Fig. 3) may not be the major factor contributing to the underestimation of dust in

the boundary layer in the SJV. It is clarified in the revised manuscript.

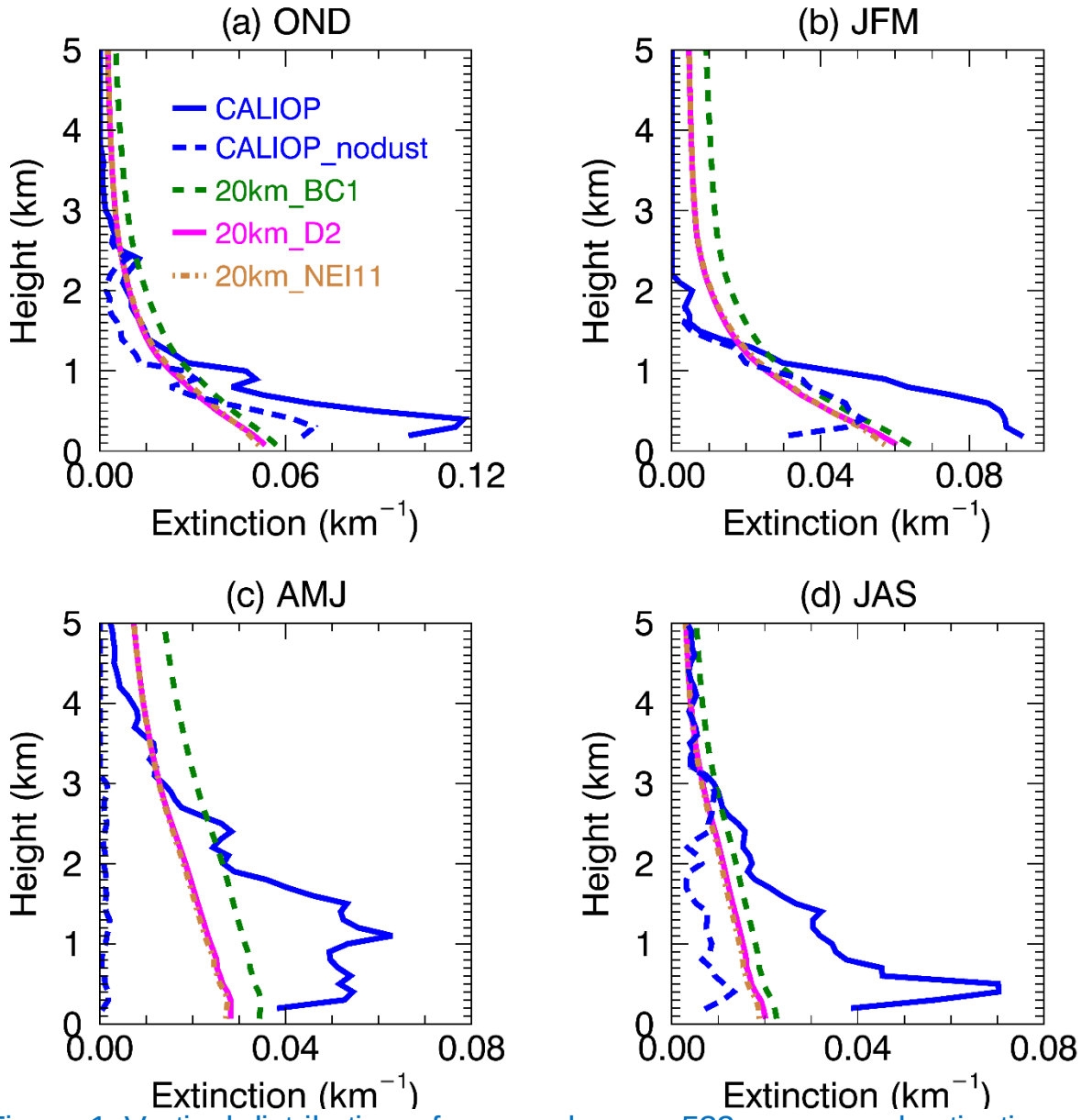

Figure 1. Vertical distribution of seasonal mean 532 nm aerosol extinction coefficient (km^{-1}) from CALIOP, CALIOP_nodust, and the WRF-Chem (20km_D2, 20km_BC1 and 20km_NEI11) simulations over the red box region in Fig. 1a in WY2013.

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:

Following three reviewers' comments, more analyses on differences in modeled and observed aerosol properties are given in section 4 of the revised manuscript.

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.

Thanks for the insightful comment. We have checked our setting and confirmed that SOA processes are not included in our current setting. We tried to run the WRF-Chem model at 20 km resolution (20km_VBS2) following the settings in Fast et al. (2014). However, that simulation produces reasonably AOD in AMJ while AOD in other seasons are underestimated. We keep our current settings and discuss the impacts of SOA processes in the revised manuscript. The statement of "incomplete understanding of SOA processes" is removed in the revised manuscript.

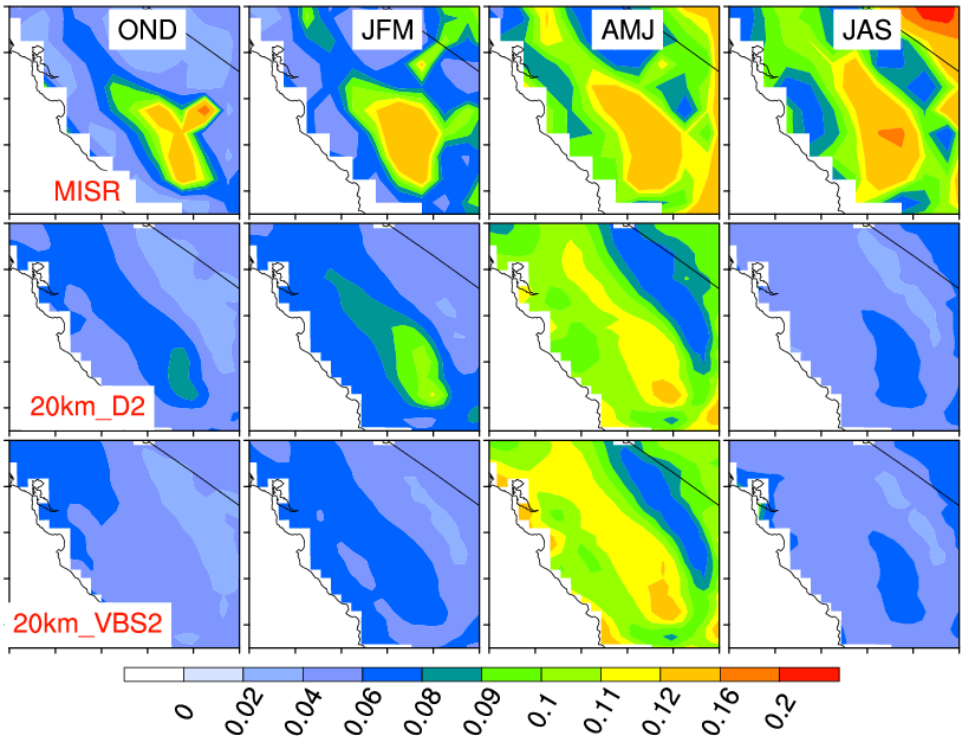

Figure 2. Spatial distribution of seasonal mean 550 nm AOD from MISR, the 20km_D2 and 20km_VBS2 simulations in WY2013.

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. We have removed this statement and include more discussions (precursor and marine intrusions) in the revised manuscript.

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).
Evaluation of temperature, RH, wind speed and precipitation are included in section 4.3 of the revised manuscript and the supplementary. More discussions of meteorological impacts on aerosol simulations are also included in the revised manuscript. Biases in surface wind speed and precipitation may not be the main reasons for the identified aerosol biases in the boundary layers during the warm season.

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).
We add the domain-averaged PM2.5 emission rate in each sub figure. Dust emissions are shown in Fig. 8 in the revised manuscript and the following figure.

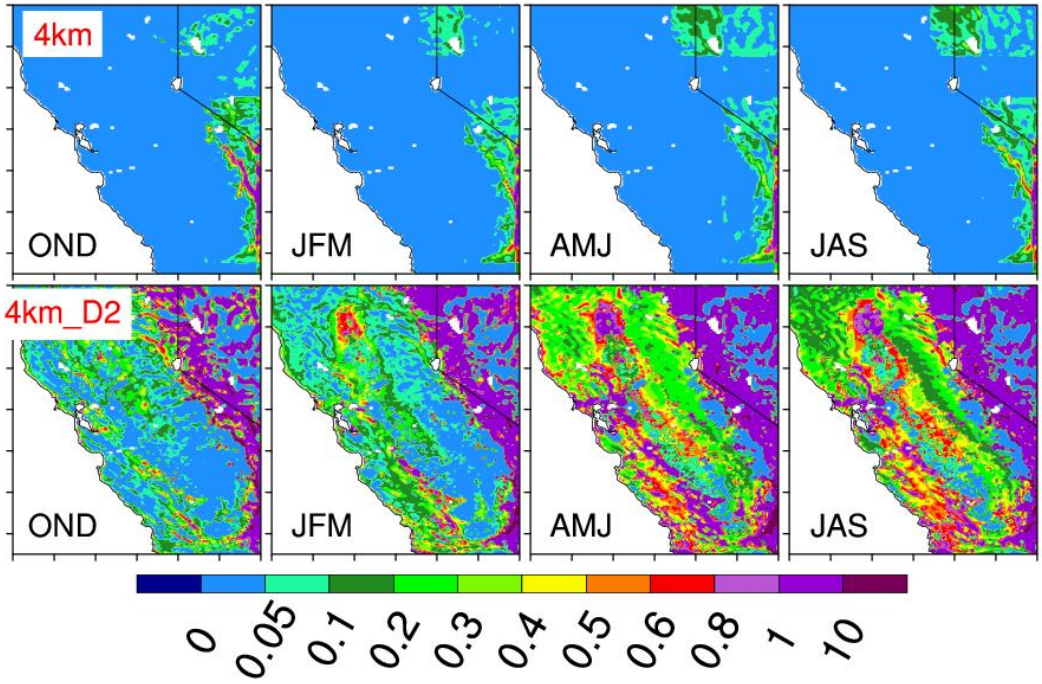

Figure 3. Mean dust emission rate (μg m^{-2} s^{-1}) from the 4km and 4km_D2 runs.

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.
Because some reader may be more interested in magnitude while other may be more interested in relative contribution, we prefer to keep both Table 2

(Table 3 in the revised manuscript) and Fig. 6 (Fig. 10 in the revised manuscript).

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/).

We use the standard emission preparation program (prep_chem_sources_v1.5) for the WRF-Chem model to generate our fire emissions. Currently, only GFEDV2.1 is available in this program. Since fire emissions are not the major issues in our current simulations, we keep current settings.