This manuscript by Kim and co-authors, discusses the impact of high-resolution *a priori* profiles on satellite formaldehyde retrievals. They use high resolution simulations from the Weather Research and Forecasting-Chemistry model (WRF-Chem) over California (4 km x 4km) during April and July 2010 to obtain the atmospheric information necessary to compute formaldehyde air mass factors (AMF). The paper includes a basic validation of the model using airborne PTR-MS (Proton-Transfer-Reaction Mass-Spectrometry) and long-path DOAS (LP-DOAS) measurements obtained during the California Nexus of Air Quality and Climate Change (CalNex) campaign. The paper continues discussing the AMF dependency with respect time of the day and the vertical distribution of formaldehyde as well as their spatial distribution. The paper finishes showing the dominant chemical regimes in the L.A. basin at different times of the day derived from WRF-Chem simulations as an example of the capabilities that future geostationary sensors will enable.

The paper is well written and tries to address an important question relevant for the future generation of air-quality satellite sensors such as the impact of high resolution a-priori information in the retrieval accuracy. However, some aspects of the paper should be improved before publication in ACP. These are summarized below with further discussion following for each section.

1. A better description of the radiative transfer calculations is needed. It is not clear how some of the most basic parameters needed for a radiative transfer calculation are treated, i.e. geometry and surface reflectance. Clarifications about how the wide spectral range is used is needed.
2. The discussion of WRF-Chem validation with CalNex data could be expanded with detailed description of the methodology used to match PTR-MS and LP-DOAS measurements with WRF-Chem simulations.
3. AMF calculations at 4 km x 4km pixels are shown but these are not compared with calculations at coarser resolution. There is no analysis included about the error in AMFs due to the spatial resolution of a priori vertical profile information. It will good to include such analysis. Furthermore, AMF calculations are affected by other sources of error such as surface reflectance or topography. This should be at least discussed in the text. Some conclusions and suggestions are qualitative and vague and should be backed up by further quantitative analysis.
4. Section 3.3 doesn't seams to belong to this paper. While it is important to highlight the capabilities of future satellite sensors it is not clear how that example provides any further information about the impact of high-resolution a priori profiles in satellite retrievals.

Abstract: With the evidence provided in the text the following sentence is not fully supported "Our analyses suggest that an air mass factor (AMF, a factor converting observed slant columns to vertical columns) based on fine spatial and temporal resolution *a priori* profiles can better capture the spatial distributions of the enhanced HCHO plumes in an urban area than the nearly constant AMFs used for current operational products". High resolution AMFs are not compared with low resolution AMFs.

Section 2.3:

- At the wavelengths of interest for UV retrievals the surface and atmospheric thermal emission is not relevant. Why are they included in the simulations?
- "We adopt the spectral resolution of 0.2 nm and a spectral range of 300.5 – 365.5 nm". Typical formaldehyde satellite retrievals perform AMF calculations at one wavelength ~340nm. How are the calculations between 300.5 and 365.5 nm used? What is the impact of the 0.2 nm resolution? With typical fitting windows between ~328 nm to ~360 nm why is the ~300 nm to ~328 nm spectral range included?
- For each pixel what is the viewing geometry used? Is it assumed the longitude of a geostationary orbit to work out solar, viewing and azimuth angles? This is important information that needs to be included in the description. The similar scattering weights in figure 7 indicate small variations in the viewing geometries (solar angle).
- How is the surface reflectance modelled in the radiative transfer calculations? Is it assumed to be a Lambertian surface with wavelength dependency and time of the day dependency, is it assumed to be a BRDF?

Section 3.1:

- The description about how WRF-Chem and LP-DOAS measurements are collocated and compared should be expanded. There are at least three dimensions that should be considered: horizontal, vertical and temporal. Is the horizontal and vertical sampling of the LP-DOAS measurements accounted for? If so, how? Is there any filtering of LP-DOAS? How is the averaging in the time-domain done?
- Likewise for the comparison between WRF-Chem and aircraft data. There is no description about how WRF-Chem simulations and aircraft profiles are matched. It needs to be included to understand the significance of figure 2.

Section 3.2: As mentioned above these section should include an estimate of the AMF calculations sensitivity with respect to vertical profiles spatial resolution by discussing "high" and "low" spatial resolution cases.

- Page 13 line 7: "General features of the AMF distribution in the area do not change significantly when a constant surface pressure is used in the RT simulations (see Supplementary Material Figure S1)." This statement is qualitative. Can it be quantified? How is the vertical distribution of HCHO and other trace gases treated when using a constant surface pressure? Are total columns kept constant? What is the value of that surface pressure? Figure S1 says "Low Spectral Resolution". Nowhere in the text it is introduced a "Low Spectral Resolution" or "High Spectral Resolution" calculation.

- Page 14 line 17: "The AMF over the ocean increases with time from 0.86 at 09 PDT to 1.03 at 15 PDT as the HCHO mixing ratio decreases with time, probably due to transport of the plume from the ocean to the inland area." Could be discussed the effect on AMF calculation of the development of the marine boundary layer? Would it be possible to quantify transport using WRF-Chem to support this statement?
- Figures 4 and 5: While mixing ratios are interesting, the actual quantity considered in the AMF calculations is the number density. Could that be shown instead?
- Page 15, line 18: "These findings highlight the importance of using time-varying, high spatial resolution a priori profile information for the accurate retrieval of geostationary HCHO measurements." While there is some quantitative analysis of the importance of using time-varying profiles by showing calculations at 3 different times, there is not such analysis for different spatial resolutions.
- Page 16, line 8: "The dependence of the AMF value on the profile shape is similar at each time of day." Would it be possible to provide a quantitative analysis backing it up?
- Page 16, line 13: "For UV-VIS retrievals, it is generally assumed that only the vertical profile shape, rather than the absolute magnitude of the absorber, affects the value of the AMF." UV-VIS retrievals, as shown in equations 1 and 2, consider the absolute magnitude of the absorber Ω_v. It is true that for similar shapes of the vertical distribution of number densities of HCHO columns the values of $S_Z(z)$ will remain constant since it is a normalized quantity. However, a consequence of the atmospheric chemistry, sources and sinks of HCHO is that high total columns and low total columns are generally linked to different shape factors.
- Page 20, line 20: "It is likely that the actual impact of aerosols on the AMF is relatively small when compared with other factors examined here." This is a qualitative statement that should be backed up with data. Otherwise it should be removed. Kwon et al., 2017 showed the impact of aerosols over East Asia not to be negligible changing columns up to 47%.

Section 3.3:

- Page 21, line 21: "Figure 9 shows 2000-2010 trends in surface O_3 from monitors in Pasadena and San Bernardino." A brief description of those monitors and their datasets should be added.

Technical comments:

Page 3, Line 2: remove the before sources.

Page 3, Line 5: Add reference for EPA HAP

Page 3, Line 8: Add reference with HCHO atmospheric chemistry.

Page 3, Line 12: Add reference to X. Jin et al., 2017 doi:10.1002/2017JD026720

Page 4, Line 10: Add reference to A. Lorente et al., 2017 doi:10.5194/amt-10-759-2017

Page 5, Line 10: Add reference for TROPOMI.

Page 21, Line 11: Add reference to X. Jin et al., as above

Page 24, Line 2: The “authors think” should be the “authors thank”.

Page 25, Line 25: The year of Borbon et al., should be 2013.

Page 37, Figure 3: It will be good to include the corresponding PDT values as well.

Page 41, Figure 7: Where it says slopre factor it should say shape factor.

Line 68, please include reference to Razavi et al., 2011 (first HCOOH retrievals from IASI).

Line 71, please include Gonzalez Abad et al., 2009 in ACE-FTS papers.

Line 98, please include citation about IASI CO_2 retrievals.

Line 118, correct typo (Pommier et al., 2016).

Line 141, actives to become active.

Line 206, should read “Both biases are however” instead of “Both biases is howeve”

Line 282, please specify which other studies.

Figure 2, include units in plots.

Figure 4, please include units in plots.