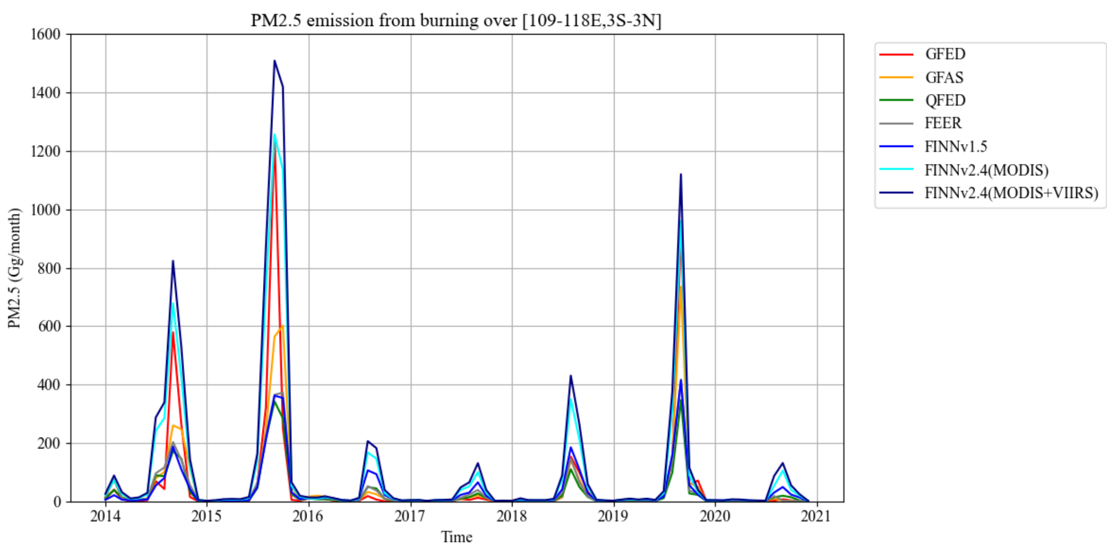
Comments on the manuscript entitled "Radiative and microphysical responses of clouds to an anomalous increase in fire particles over the Maritime Continent in 2015" by Takeishi and Wang.

General comments:

The authors have investigated the impact of fire emissions on the radiation and microphysics field around Kalimantan Island. The scope of this article is quite essential, and the scientific significance of this article might be considerable, as the interaction between aerosols and clouds is not investigated over the Maritime Continent. However, I regret to say that the quality of the scientific approach and presentation seem insufficient to be published in this journal.

1) FINNv1.5 is unsuitable for significant fire events in the equatorial region (cf. Liu et al. 2020, https://doi.org/10.1016/j.rse.2019.111557).

   The below figure denotes monthly PM2.5 emission accumulated over the southern part of Kalimantan Island [109-118, 3S-3N, same as the rectangle of "Kalimantan" in Figure 2]. FINNv1.5 uses MODIS active fires and has the advantage of detecting small fires, but it does not include peat fires. That means the interannual variation of FINNv1.5 is relatively small compared to that of other fire emission inventories.



2) A detailed analysis has not been conducted to reveal the cause of "*The simulated response of clouds to fire particles in our simulations clearly differs from what was presented by two previous studies that modeled aerosol-cloud interaction in years with different phases of El Niño–Southern Oscillation (ENSO)*" (lines 13-15).

   In general, the authors have conducted "*as is*" the model was released. Then, compared with the previous research, and concluded as "*it differs*." HD18 uses Thompson and Eidhammer (2014) as the microphysics parameterization. It means they do not consider the indirect effect of aerosols because "*the user needs to select a double microphysics scheme; either Lin et al. or the Morrison microphysics schemes are the current possible choices*" (Section 4.3.3 of WRF/Chem Users' Guide; https://ruc.noaa.gov/wrf/wrf-chem/Users_guide.pdf). Also, LW20 implemented special treatment for peatland fires as they wrote as "*The plume rise algorithm in WRF-Chem, specifically modified to improve the representation of tropical peat fire, was described in Lee et al. (2017)*" (Section 2.1 of LW20). The authors should show how these differences affect the results. I also note here that it might be helpful to conduct an additional investigation for the reliability of the result of 4-km grid spacing, as "*The characteristics of the simulated diurnal precipitation cycle changed at a grid spacing of around 2−3 km*" (Yashiro et al., 2016, SOLA, doi:10.2151/sola.2016-053).

Detailed comments:

1) Section 2., lines 80-81: The authors should show the version of WRF/Chem applied for this study. It seems the authors have used a 5:1 ratio for the nesting; there might be a problem until version 4.2 as described as "*All of the odd nest ratios (excluding 3:1) had an incorrect feedback. The indexing was off by (ratio/2 -1). For example, a 5:1 ratio would be off by a single fine grid point. Even ratio feedback was not impacted*" (cf. 1st para. of "Others"

section of version 4.2 in https://github.com/wrf-model/WRF/releases).

2) Section 2, last para. (Lines 109-112): The authors should show the version of the FINN emission inventory. Liu et al. (2020) had investigated PM2.5 exposure in Singapore using five emission inventories for 2015. They found that "*FINNv1.5 consistently underestimates smoke PM2.5 in high fire intensity years and most poorly captures the temporal variability of observed smoke PM2.5*" (Section 3.3.1 of Liu et al., 2020). Therefore, I doubt using FINNv1.5 for this kind of research as peat fires are not included in FINNv1.5 (cf. Table 1 of Liu et al. 2020), although peatland is distributed in the southern part of Kalimantan Island. If there is a reasonable reason for using FINNv1.5, the authors should explain it in the revised manuscript. As fire emission is the key to this research, I would like to ask the authors to give additional information in the revised manuscript for 1) the treatment of plume rise calculation (biomass_burn_opt), and 2) diurnal variation. For example, in HD18, "*Biomass burning emissions are based on the Fire Inventory from NCAR (FINN, Wiedinmyer, et al., 2011) and were distributed using the online plume rise model (Grell et al., 2011). These emissions have a diurnal cycle, with maximum values at 15 local standard time (LST), which is important to consider to obtain a realistic diurnal variation of the aerosol optical thickness (Hodzic et al., 2007; Wang et al., 2006)*" (Section 2.1 of HD18). I also note here that if you have used 'grid_finn_fire_emis_v2020' (https://www.acom.ucar.edu/Data/fire/) for the conversion of FINN emission, the maximum value can be seen at 13LST as the default setting (given by "lt_fac" in fire_file.f90). I am not sure there is a diurnal variation at significant fire burning. Still, the authors should explain the diurnal variation used in this study, and I think the diurnal variation should be validated by observed diurnal cycles of AOD (or AOT of HIMAWARI-8).

3) Section 3.1, lines 129-131: I cannot understand why the authors insist "*AOD measurement data from the nearby AERONET stations is unavailable.*" It

seems several AERONET stations had AOD data in September 2015 (https://aeronet.gsfc.nasa.gov, I have accessed on 19 October 2021). For the conversion of 500nm AERONET AOD with 550nm AOD, equation 1 of Jiang et al. (2019; https://doi.org/10.3390/rs11091011) could be useful.

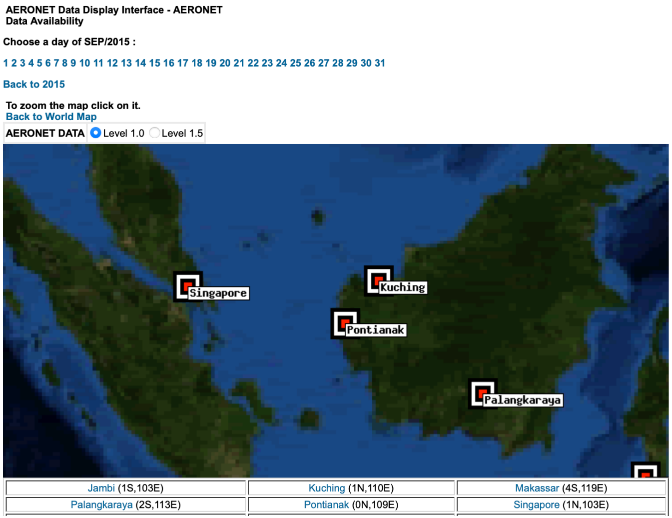


Figure for the location of AERONET stations near the target region, which has AOD data in September 2015.

Technical corrections:

1) Page 8, Figure 6: It seems the text "*AOD from MODIS Aqua (MYD08_D3) shows a similar result*" is not suitable for the figure caption. If there is no significant difference between Aqua and Terra, the authors should mention it in the main text.