In this paper, different plume rise methods for biomass burning (BB) are tested according to their impact on the simulation of trans-regional transport of BB aerosols in CMAQ model. The results emphasize the importance of applying the in-line plume rise module which calculates the plume height based on real-time atmospheric stratification. In addition, the paper shows the empirically assigned daily variation of smoldering fraction and fire size also play a role. With the help of model simulations, the paper analyzes the ways in which BB smoke transports to Taiwan and interacts with the near-surface pollutants.
The results of this paper is valuable because it is important for future models to figure out how those in-line plume rise modules perform and then to improve them accordingly. Although several similar plume rise modules have been developed for many years, limited works were published to evaluate the effect of such modules by multiple observations like this one. However, the paper still needs further revision because of bad organization and presentation.

General comments:
1. The authors declare "three distinct transport mechanisms" could bring BB aerosol from nPSEA to Taiwan both in the Abstract and the Section 4. However, the three mechanisms all depends on the **same** mechanism (strong westerlies in the free Troposphere) to **transport** BB aerosol from nPSEA to Taiwan. The differences among the three mechanisms in fact lie on how the BB aerosol interacts with the local pollutants in Taiwan. Therefore, it might be inappropriate to say "three transport mechanisms" in the context of East Asian region. Please consider to change those expressions according to the descriptions in line 450 which in fact make more sense.

2. The authors decide to compare the simulation from 2013 with observations from 2014 in Section 3.2 because of "**incomplete** MPLNET dataset of 2013". However, the reason is not convincing enough. On one hand, figure 4a shows quite **complete** MPLNET data coverage in 2013 which seems enough to give an average of good quality. On the other hand, the authors could do a simulation of 2014 and then get similar results if fire conditions are similar between 2013 and 2014 (as stated in lines 248-250).

3. Section 3.3 is not well organized which makes the reader difficult to follow.
For example, the authors seem to indicate the high resolution of FINN inventory plays the key role in the good performance of the in-line calculation. However, without another experiment using a lower emission resolution and the same in-line calculation as a comparison, such indication is only a speculation and should not appear in the conclusion (lines 455-457) as a strong argument.
Also, "BB emission is mainly caused by small fires and dry conditions over the period in the region" is not enough for the readers to understand "why the inline module worked well to represent the BB condition". I guess the heights prescribed in the off-line module tend to overestimate the plume height under dry conditions (dryer atmospheric stratification damps the pyro-convection through entrainment). Therefore, the in-line module which considers the atmospheric condition performs better. Anyway, more information is needed.
At last, is Figure 8 represents the near surface level or some upper levels? In either case the

corresponding statement is needed in Figure 8. In addition, is it possible that the difference between Figure 8b and 8c results from the different smoldering fraction between FWrp and IWrp+EC. As shown in Figure 2c and 2e, at 17:00 LST (around 9:00 UTC), FWrp (IWrp+EC) happens to have small (big) smoldering fraction which means little (much) aerosol is emitted near the surface and fire hotspots are therefore unclear (clear) in figure 8b. If so, the authors should reconsider the validity of some statements in Section 3.3.

4. Finally not mandatory and only a suggestion, is it possible to add another simulation in which biomass burning pollutants are emitted directly into the first model layer (near surface layer)? Such setup is still used in many (even some of the most state-of-art) models and probably works better as a control or benchmark than the "Nofire" setup if the authors want to emphasize the importance of in-line plume rise module.

Comments by line:

22. It might be confusing to use "the calibrated model" because no specific **calibration** is mentioned in the abstract. Use "Such setup" or "This measures" might be better.

23. The authors might want to say "BB aerosol concentration prediction" instead of "BB emission prediction". Emission is the flux at which the pollution is emitted to the atmosphere and usually not predicted by the model. Please check the whole paper to avoid similar mistakes.

24. Please consider to remove the contents inside the brackets. It is unnecessary and even confusing to mention the observation type like MODIS AOD and CALIPSO in this way.

52. "the vertical distribution percentage of BB **emission** was".

65. "interaction" between what and what?

81. "supply" instead of "supplies". Please check the whole manuscript to avoid such grammatical mistakes.

Table 1. Please include information about the **emission inventory** for **D04**.

156. Only one "and" is needed.

170. "Burnt area size" is not the same as "Fire size". "Fire size" is more proper in this context.

Table 2. In line "IDef", "Smoldering fraction: **yes**" makes no sense. Please check if it is a mistake. Otherwise, more details are needed.

224. "The **systematic peaks** for these pollutants are believed to be the uncertainties involving the FINN BB emission". This sentence is confusing. Do the authors mean "the systematic error for"?

Table 3. "MNB" and "MNE" still exist without explanation. Please check the whole manuscript to replace them with "MFB" and "MFE", Also, it is strange to find R decreases when MNB and MNE both decrease from IWrp to IWrp+EC. More explanation might be needed.

Figure 3. The unit "ug/m-3" is wrong. Please use either ug/m3 or ug*m-3. Also, colors look different between lines in legend and figure. (For example, the line representing observation is black in the figure but appears to be gray in the legend).

240. Is the lidar "MPLNET v0 L1.5a" at the same position as DAK station? If so, please indicate it in section 2.1 and in Figure 1.

249. Please replace the letter "x" (**ex**) with symbol "×" (**multiply**). Also check the whole paper

to avoid similar mistakes.

362. The sentence “It is because the boundary layer height……” is out of context. Please delete it or reorganize the context.

408. The sentence “The detection of BB **intrusion into surface** sites ……” is not consistent with the context. The mechanism mainly describes the near surface pollutants get upward and mix with the BB smoke above, rather than BB smoke gets downward and intrude into surface.

416. “BB aerosols have the most direct influence on the surface site in western Taiwan” is not enough for readers to understand why it “is coherent to the reduction of surface O3, NOx, and SO42- aerosols in 2006”. More explanation is needed.

422. Please reorganize the sentence “The allocation fraction will need to improve looking……” which is difficult to understand.

Figure D2. The figure has never been mentioned in the main text. It could be removed if it is unnecessary for your conclusions.