Response to Reviewers

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We greatly appreciate the insightful comments and suggestions of the reviewers. Below please find a list of the Reviewers' remarks in contrast to our responses to them:

Review #1

Major Concerns	Responses
1) The manuscript shows the analysis both for January and July. However, the impacts of three industrial regions on Taiwan in summer (July) is quite small, almost negligible even in the last few days when the impacts were relatively large. I don't think it is worthwhile spending much space for the July analysis, rather focusing on winter case would make the paper more concise and scientifically focused.	First, the authors really appreciate the reviewer spend much time and efforts reviewing this manuscript carefully and giving valuable opinions. They are truly grateful for the reviewer's comments which are very helpful to make this manuscript better. The authors admitted that they accidentally used non-precise and inappropriate words and so as to make misleading narratives. They promised that they will ask an English language editing company to revise the manuscript after all reviewers' comments are responded. Yes, the authors agree with the reviewer's suggestions and have cut down the contents of July analysis. They only keep the original Fig. 6 (Fig. 10 in the revised manuscript) in the main content and moved original Fig. 7 and Fig. 8 to Fig. S2.6 and Fig. S2.8 in the supplement of revised manuscript), and delete original Fig. 12, Fig. 13, Fig. 14, and Fig. 16.
2) The results of process analysis was described and discussed in 3.2, 3.4, 3.5, and 3.6, which formed a main part of this paper. However, the descriptions in these sections were not firmly reasoned. In these sections, the author argued "dominant" contribution of three industrial regions at some locations. For example, in	Yes, the authors have written several misleading narratives in the original manuscript. After careful checking, first, they have modified the arrangement of the manuscript such that they combined the section 3.3, 3.4 and 3.6 of the original manuscript to section 3.5 of the revised manuscript in order to cut down the content of July analysis, and separate section 3.5 of the original manuscript into section 3.4 and 3.5 in the revised manuscript. Therefore, Fig. 11 of the original manuscript was changed to Fig. 9 and Fig. 14 was deleted. Now the main part is section 3.2, 3.4, and 3.5 for the revised manuscript. Second, they revised misleading narratives in order to avoid the

3.2, the author pointed out that PM2.5 was influenced "mainly" by BRIR and YRDIR at the place #19. However, these arguments were not convincing. For the abovementioned example, Fig5 (c-2) and (c-3) which was regarded as representing the contributions by process of BRIR and PRDIR, respectively, showed similar variations to those of total contributions shown as Fig5 (c-1). However, the range of values largely differed each other, so I cannot understand why the author can conclude that the BRIR and YRDIR were "main" contributors to the variation of PM2.5 at #19. Similar arguments to this case can be found in these sections, and they considerably deteriorate the persuasiveness of the manuscript. I strongly recommend the author to revise such arguments in these sections and provide how to read and understand the main figures (Fig 5, 8, 11, and 14).	argued dominant contribution of three industrial regions. For example, One line 229-230 From Fig. 5(b-1)-(b-4), among the three industrial regions it is apparent that #18 was influenced by both the BRIR and YRDIR,……. On line 232-233 For #19, $PM_{2.5}$ was influenced mainly by YRDIR (Fig. 5(c-2)) and occasionally by BRIR (Fig. 5(c-3)) for those three industrial regions,……… On line 237-238 Although #20 is very near PRDIR, it was influenced more by YRDIR (Fig. 5(d-3)-5(d-4)) and other sources in the north other than three industrial regions since the prevailing wind was mainly northeast wind in January. On line 324-325 From Fig. S2.8(a-1) to Fig. S2.8(a-4), it was found that #17 was influenced more by YRDIR than BRIR or PRDIR on July 18th 2017. On line 337-338 According to the main content, among those three industrial regions BRIR and YRDIR were the major sources of #17 and #19 - #20, respectively.
Specific comments:	
L37: Seasonality of EAH is not "due to" rapid economic grows in Asian countries.	Yes, the authors thank the reviewer pointing out this error. In order to avoid the Chinese English writing, the authors promise that they will ask an English language editing company to help revise the revised manuscript after responding all reviewer's comments. In this temporary revised manuscript, that narrative was revised on line 37-38 as follows. The East Asian haze (EAH) has been disturbing in spring and winter around the East Asia due to the spread of anticyclones over

	Asian continent. (Fu et al., 2014; Yang et al., 2016).
L43-45: Why did you specify these data and models for trajectory analysis?	The authors tried to make examples by mentioning the NOAA's data and models MM5 or WRF. They didn't mean to specify these data and models. In order to avoid misleading, the authors have revised the narratives on line 43-45 as The trajectories could be calculated from, for example the archived meteorological data of NOAA ARL (www.ready.noaa.gov/archives.php) or the model outputs of MM5 (Mesoscale Model version 5, Dudhia, 1993) or WRF (Weather Research and Forecasting, Skamarock and Klemp, 2008)..
L50-51: Could you state more clearly why TS method would contain substantial uncertainty?	In the original manuscript, the authors intended to express that TS methods estimated the contribution of some upstream place on a receptor is to get the product of weighting of frequency passing that upstream place and concentration at that receptor. The authors have removed that narrative "Using trajectory to express the moving of a polluted plume would contain substantial uncertainty." in the original manuscript but added the following narratives on line 51-54 in the revised manuscript. The plume transport from an upstream place to the receptor would exchange and react with air and pollutants along the path of transport. It suggests the plume arriving the receptor is no longer the plume emitted from the initial upstream place. The farther the upstream place is away from the receptor; the more uncertainty will be in the TS method. Therefore, the TS method would contain substantial uncertainty.
L54: The difference between those two runs does not directly mean the contribution of specific source but impact of the reduction of that specific source. To distinguish these two concepts is quite important.	The authors agreed with the reviewer's opinion regarding to the BFM methods and have modified narratives in the revised manuscript. On line 57-61: The difference of the base case and the zero-out case is the reduction of the zero-out source. The reduction is approximate the contribution of that zero-out source only under the assumption when the contributions of each sources are additive. However, there is indirect contribution not considered in BFM method, i.e., the chemical reactions between the specific zero-out source and surrounding sources is neglected. The indirect contribution could be large if the zero-out sources and surrounding sources are both huge and have enough time to react. The following description is not included in the revised manuscript but provide to the reviewer for communication. If pollutants from BRIR or YRDIR moved to the sea and

	transported southward or pollutants from PRDIR moved to the free atmosphere and transported eastward, it is expected the pollutants emitted from those aforementioned three industrial regions should not have enough time to react with pollutants other than the industrial regions including areas other than three industrial regions in mainland China, along the transport and arriving at Taiwan. In other words, the contribution from the chemical reactions between the pollutants from industrial regions and pollutants from surrounding area is insignificant. In that case, we can roughly consider the reduction of the BRIR/YRDIR/PRDIR sources as the contribution of these industrial sources. When the pollutants from those three industrial regions arrived at Taiwan, it may react with pollutants from the local when they meet in the first place. In Chen et al. (2014), they estimated the indirect reactions between pollutants from mainland China and pollutants in Taiwan accounted about 10% of $PM_{2.5}$ in Taiwan. It is expected that the chemical reactions between pollutants from areas other than three industrial regions and pollutants from three industrial regions is not important because those two masses of pollutants did not mixed well during the transport.
L56-58: What do you mean "under-represented chemical reaction" here? Could you explain more specific?	The authors have change the word "under-represented" to "ignoring" on line 64 in the revised manuscript.
L67: CTM? This should be AM method?	Yes, the authors have modified that sentence to "However, the CTM especially the AM method is able to give clearer contributions from a specific source compared to the TS method." on line 74 in the revised manuscript.
L87: These abbreviations (LRT, LP) have already been defined	Thanks the reviewer's reminder. The authors have removed the repeated words.
L90: Meaning of these terms (LRT-Event and so on) should be explained	Yes, the authors should explain these terms and have already done on line 97-98 in the revised manuscript. They rewrite the sentence as "…LRT-Event (high concentration events caused nearly by pure LRT), LRT-Ordinary (non-events caused nearly by pure LRT), and LRT/LP&Pure LP (other days influenced by mix of LRT and LP & pure LP),…."
L98-99: Are power and industrial sectors the largest for entire Asia or any specific region in Asia?	Unlike developed countries, power and industrial sectors are the largest for most countries in Asia. According to the MIX Asian emission inventory, China and India dominate the emission of Asia for most of the species (Li et al. 2017). In the statistics of emissions

	from five anthropogenic sectors in Asia, the point source like power/Industry has the largest emission for SO_2, NMHC, TSP/PM_{10}/$PM_{2.5}$, OC, and CO_2, and is comparable to transportation for NO_X. The transportation is the largest emission for CO and BC. According to Zheng et al. (2018), the emissions from power and industrial sectors are the largest among all anthropogenic emissions except NH_3 that are mainly from agriculture in China in recent years. For NMHC, the emission from industry, residential, transportation, and solvent use are comparable to each other. Another famous Asian emission inventory REAS (latest version 3.1, Kurokawa and Ohara, 2020) also show similar results. However, there are occasional exception, for example, the domestic sector in South Asia other than India in 2015 has the largest emission for SO_2, NOx, CO_2, and PM_{10}/$PM_{2.5}$ than other sectors. and BC. While in Taiwan, SO_2 and CO are mainly from point source like power and industry; however, TSP/PM_{10}/$PM_{2.5}$/VOCs are mainly from area sources. NO_X are mainly from point and mobile sources (TEPA, 2017). As for Zheng et al. (2018) mainly discussed the anthropogenic emission in China, the authors understand the reviewer's comments and changed the citation to Li et al. (2017) and Kurokawa and Ohara (2020) on line 106 in the revised manuscript. Kurokawa, J., and Ohara, T.: Long-term historical trends in air pollutant emissions in Asia: Regional Emission inventory in Asia (REAS) version 3.1, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1122, in review, 2020. Li, M., Zhang, Q., Kurokawa, J.-I., Woo, J.-H., He, K., Lu, Z., Ohara, T., Song, Y., Streets, D. G., Carmichael, G. R., Cheng, Y., Hong, C., Huo, H., Jiang, X., Kang, S., Liu, F., Su, H., and Zheng, B.: MIX: a mosaic Asian anthropogenic emission inventory under the international collaboration framework of the MICS-Asia and HTAP, Atmos. Chem. Phys., 17, 935–963, https://doi.org/10.5194/acp-17-935-2017, 2017. TEPA: Building of the Taiwan emission data system. Taiwan EPA report, EPA-106-FA18-03-A263, in Chinese, 2017. Zheng, B., Tong, D., Li, M., Liu, Fei, Hong, C., Geng, G., Li, H., Li, X., Peng, L., Qi, J., Yan, L., Zhang, Y., Zhao, H., Zheng, Y., He, K., and Zhang, Q.: Trends in China's anthropogenic emission since 2010 as the consequence of clear air actions. Atmos. Chem. Phys., 18, 14095–14111, https://doi.org/10.5194/acp-18-14095-2018,

	2018.
L103-104: This should be "the impact of reduction in source emission in each industrial region", because BFM can estimate "impact" not "contribution". Or you can define the wording that you will use the word "contribution" for the deference between control runs and sensitivity run.	Thanks the reviewer's suggestion. The authors have revised the narrative to "As mentioned above, the difference of Base and sensitivity scenarios is the reduction of the specific source. Only when the chemical reactions are not important then the reduction can be approximate the contribution of that specific source. In this study, the pollutants from those three industrial regions transport directly to Taiwan instead of meandering movement. Therefore, we can roughly estimate the contribution of BRIR, YRDIR, and PRDIR to $PM_{2.5}$ as the difference between the *Base* case and the *Brir*, *Yrdir*, and *Prdir* cases." on line 112-115 in the revised manuscript.
L123-127: For Figure1, the formal, not abbreviated, names for each monitoring station should be appeared here.	Thanks the reviewer's reminder. The authors have rewritten the names for each monitoring stations on line 133-139 in the revised manuscript. For meteorology evaluation, we chose eight representative stations: Peng Jiayu (PJY, #1 in Fig. 1), Taipei (TPE, #2 in Fig. 1), Chupei (CP, #3 in Fig. 1), Taichung (TC, #4 in Fig. 1), Chiayi (CY, #5 in Fig. 1), Tainan (TN, #6 in Fig. 1), Kaohsiung (KH, #7 in Fig. 1), and Hengchun (HC, #8 in Fig. 1) stations to evaluate the modeling performance of temperature, wind speed, and wind direction. Since most residents lived at the relatively flat western Taiwan, the observations at the Banqiao (BQ, #9 in Fig. 1), Pingzhen (PZ, #10 in Fig. 1), Miaoli (ML, #11 in Fig. 1), Zhongming (ZM, #12 in Fig. 1), Chiayi (CY, #13in Fig. 1), Tainan (TN, #14 in Fig. 1), Zuoying (ZY, #15 in Fig. 1), and Hengchun (HC, #16 in Fig. 1) stations were chosen for $PM_{2.5}$ evaluation.
L130-131: Why you don't show the model domains in Figure 1 but just describe horizontal resolution?	Yes, the authors have redrawn the Figure 1 which shows the model domains in the revised manuscript.
L146: "MB" has already been defined in the previous sentences	Thanks the reviewer for carefully pointing out this extra. The authors have already removed the repeat one.
For the evaluation of WRF and CMAQ shown in Table 1 and 2, the results from which domain were used? And in addition to the summary of statistical indices in Table 1,	The authors have explained the simulated results from the fourth domain was evaluated for Table 1 and 2 one line 158 in the revised manuscript. The authors have added figures of comparisons of observed and simulated temperature (Fig. S2.1), wind speed (Fig. S2.2), and wind direction (Fig. S2.3) in the supplement of the revised manuscript.

figures of comparisons of temperature and wind between observation and simulation are quite informative. Could you put them together at least as supplement?	In addition, the authors also added Fig. S2.4 which show the comparisons of observed and simulated $PM_{2.5}$ in the supplement of the revised manuscript.
You should explain how you draw Fig3. Are the values in Fig3 difference between Base case and sensibility case? If so, it's better to note it in the manuscript or in figure caption. Fig3 is a bit busy, so it seems better to select fewer locations out of seven to avoid redundancy.	Yes, the authors have already explained how to get the values in Fig. 3 both on line 223 in the revised manuscript and the caption of figure 3. In addition, the authors have removed few locations but only remained BQ, ZM, and CY to representative northern, central, and southern Taiwan. On line 182-183 in the revised manuscript: As mentioned, the impact was considered as the reduction of specific source removed or roughly the contribution of that specific source, i.e. the difference between the base and sensitivity scenarios.
L176: Remove unnecessary "the".	Thanks the reviewer for pointing out this typo. The authors have already removed the extra "the".
Could you check the wording "China East Sea"? "East China Sea" has been also used for the same area in many literatures.	Thanks the reviewer's careful checking for this manuscript. The authors have already unified the word to "East China Sea" in the revised manuscript.
For Figure5, you should explain how to deduce the values shown in the figure, in particular the values in Fig5(*-2,3,4). Are they the difference between Base case and sensitivity case? If so, you should instruct briefly how to interpret these Figures. Is the title of y-axis correct? This should be "_concentration" or "daily concentration change"?	Yes, the authors have followed the reviewer's suggestion to explained Fig. 5 are deduced by the difference between Base case and sensitivity cases on line 220-222 in the revised manuscript as follows. Similar to Fig. 2, we deduced the difference of base and sensitivity scenarios for IPR analysis. This study considered the reduction as the approximate contribution for each industrial region. Therefore, the reader should keep in mind that the following discussion is satisfied on when the chemical reaction between each industrial region and surrounding was ignored. Thanks the reviewer's reminder that the title of y-axis should be "daily concentration change" or "change in daily concentration". The authors have already corrected this error in Fig 5 And Fig S2.8 in the revised manuscript.
L204: Fig5(a-1) and (a-2) do not seem quite similar to each other. Could you specify more about which features of both figures	Yes, the authors agree that they did not use precise vocabulary and have removed the word "similar" to avoid misleading. The revised narratives on line 223-224 in the revised manuscript is "The positive and negative contribution terms in Fig 5 (a-1) and Fig. (a-

look similar?	2) appealed synchronously although their magnitudes were not in equal proportions.”
L204: You concluded that main contributor to #17 PM2.5 is BRIR, but I cannot understand why you can conclude like this. The values in Fig5(a-1) and (a-2) are quite different. You should give an instruction how to read and understand the Fig5	The authors have modified the narratives on line 223-225 in the revised manuscript as follows. The positive and negative contribution terms in Fig 5 (a-1) and Fig. (a-2) appealed synchronously although their magnitudes were not in equal proportions. It implies #17 was influenced by both BRIR and other nearby sources.
L205: Can HADV process "produce" PM2.5? The term "production" here is not appropriate.	The authors understand what the reviewer meant and have already modified that narrative on line 224-225 to “The increase of $PM_{2.5}$ was caused mainly by the process HADV, followed by ZADV and VDIF….”. In addition, the authors have examined the whole manuscript and modified all such narratives. On line 244 The build-up of $PM_{2.5}$ at BQ was mainly HADV with minor CLDS…… On line 259-261 For CY located in southwestern Taiwan, VDIF and HADV mainly contributed to the gains of $PM_{2.5}$, and the removal processes were mainly ZADV and AERO; however, occasionally when the positive contribution to $PM_{2.5}$ were ZADV and VDIF, the removal processes were HADV and AERO (Fig. 5(f-1)). On line 261-262 Compared Fig. 5(f-2)-(f-4) and Fig. 5(g-2)-(g-4), it is obvious the positive and negative contribution to $PM_{2.5}$ for CY were very similar to for ZM. On line 286-287 The major processes below layer 9 (~310 m) contributing to the increase of $PM_{2.5}$ were HADV, VDIF, and ZADV and removal processes were DDEP and AERO (Fig. 8(b-3)). On line 292-293 Although #18 and BQ were most affected by YRDIR, the major contribution process at BQ below 200 m (layer 7) was HADV….. On line 302-303 Second, for the haze from BRIR and YRDIR, the positive and negative contribution processes on BQ were mainly HADV/AERO…..

	On line 304-305 "While on Jan 9th, the major processes leading to the increase of $PM_{2.5}$ at BQ ……" On line 325 The positive and negative contribution processes were non-uniform below 80 m (layer 4). On 325-327 But from 120 m to 460 m (layer 5 to layer 11), the major processes to build-up of $PM_{2.5}$ were AERO and ZADV and the removal process was mainly HADV. On line 368-369 When the EAH moved to northern Taiwan, HADV and AERO were the major contribution processes of $PM_{2.5}$ at BQ. On line 371-372 The stronger the intensity of EAH, the impact on central and southern Taiwan was more obvious, the proportion of HADV contributed to $PM_{2.5}$ budget was more obvious near surface.
L211: What process considered in AERO can reduce PM2.5?	Since the ambient environment was cold in high latitude regions and warm in low latitude regions, the evaporation process of PM2.5 occurred in the haze during transporting moved southward. In the simulation study of Chuang et al. (2008), the evaporation of NH_3NO_3 occurred for the PM2.5 plume transported from Shanghai to Taipei and formed ammonia and nitric acid. The ammonia reacted with sulfur dioxides and form ammonium sulfate and the nitric acid remained in the plume and reacted with ammonia emitted in Taipei and formed ammonium nitrate again. It is expected the evaporation of organic carbon also occurred if ambient temperature increased. Another very minor process which could be ignored compared with abovementioned evaporation process is the coagulation of $PM_{2.5}$ particles converted to coarse particles. Chuang, M. T., Fu, J. S., Jang, C. J., Chan, C. C., Ni, P. C., and Lee, C. T.: Simulation of long-range transport aerosols from the Asian Continent to Taiwan by a Southward Asian high-pressure system. Sci. total. Enviro., 406, 168–179.
L213: If the intrusion of PM2.5 from PRDIR is like that depicted in Fig4, why the contribution of ZADV is not so large in Fig5(c-4)? Since #19 is located between PRDIR and Taiwan island and	Fig. 4 is the cross section of red line in domain 2 and domain3. The ZADV is not so large in Fig. 5(c-4) is probably # 19 is not on the red line (the cross section) in Fig. 1. In addition, the less influence of $PM_{2.5}$ from PRDIR was mainly on the mountains, as shown in Fig. 2(e) and Fig. 2(f), i.e. at high altitude about 1-3 km. The downward motion is not obvious unless the plume was blocked

the transport of PM2.5 between them occurs about 1-2 km high above the surface as in Fig4, any kind of vertical (downward) motion should transport PM2.5 from that layer to the location of #19 which must be at the surface	by the mountains in Taiwan (Fig. 4).
L227: What does "minor PM2.5" means here?	The authors have replaced the word "minor" with "certain" on line 247 in the revised manuscript.
L228: Why can you describe "The PM2.5 at BQ then transport up- and then southwards"? Which figure show this transport of PM2.5?	Thanks the reviewer for pointing the error. The removal process of $PM_{2.5}$ at BG was mainly ZADV. In order to explain clearly, the authors have modified the narrative as "The removal process of $PM_{2.5}$ at BQ was mainly ZADV, which implies PM2.5 at BQ then transport up and reflects BQ is located in a basin." on line 248-249 in the revised manuscript.
L228-229: Fig.(f-1) -> Fig5 (f-1)	Thanks the reviewer for pointing the error. The authors have already corrected the type.
L234-235: If this is true, why ZADV in Fig5 (f-4) is largely negative from Jan 8 to 10?	Because of the reviewer's comment, the authors found the ZADV has to be treated in an opposite way since the concentration gradient is positive for $PM_{2.5}$ from PRDIR, which is different from the usual cases that $PM_{2.5}$ concentration was usually higher near surface. The authors have modified some narratives in the revised manuscript. On line 206-207 The boundary layer mixing was enhanced by the pass of cold surge and increased $PM_{2.5}$ on the ground. On line 258-259 On Jan 8th to 10th, the negative ZADV indicated the concentration was decreasing at the lower 20 averaged layers but the concentration gradient was positive ($\frac{\partial PM_{2.5}}{\partial z} > 0$, the concentration of $PM_{2.5}$ from PRDIR was higher at high altitude than that at low altitude over Taiwan) implies the vertical velocity had to be negative, i.e. downward motion. Therefore, the boundary layer mixing of the aloft $PM_{2.5}$ plume was enhanced by the pass of the cold surge. (Yen et al., 2013; Chuang et al., 2016). The following is a brief review that was not in the revised manuscript but provide to the reviewer for communication. Yen et al. (2013) suggested the downward motion could bring Southeast Asian biomass burning pollutants aloft to surface through the subsidence of cold surge through the wind field derived from NCEP Global Forecast System analyzed data. Chuang et al. (2016) applied

	the WRF/CMAQ and found the Southeast biomass burning aerosols could be blocked by the mountains in Taiwan and then the boundary layer mixing assisted the subsidence of aloft aerosols to the surface. Huang et al. (2020) suggested the 700-hPa LLJ (Low Level Jet) may have carried the biomass burning plumes aloft located south of the frontal system (cold surge) and accompanied the upward/downward motion south/north of the frontal system. The downward motion occurred at the north of the front or subsidence of cold air region. While in the simulation of present study, the ZADV was negative which also implied the downward advection occurred when the cold surge passed. Yes, it is a pity that there is no observation for the pollutants profile during the pass of cold surge. Otherwise, it would be more persuasive. Chuang, M. T., Fu, J. S., Lee, C. T., Lin, N. H., Gao, Y., Wang, S. H., Sheu, G. R., Hsiao, T. C., Wang, J. L., Yen, M. C., Lin, T. H., and Thongboonchoo, N.: The Simulation of Long-Range Transport of Biomass Burning Plume and Short-Range Transport of Anthropogenic Pollutants to a Mountain Observatory in East Asia during the 7-SEAS/2010 Dongsha Experiment. Aerosol. Air. Qual. Res., 16, 2933–2949, https://doi.org/10.4209/aaqr.2015.07.0440, 2016. Huang, H.-Y., Wang, S.-H., Huang, W.-X., Lin, N.-H., Chuang, M.-T., da Silva, A. M., & Peng, C.-M. (2020). Influence of synoptic-dynamic meteorology on the long-range transport of Indochina biomass burning aerosols. Journal of Geophysical Research: Atmospheres, 125, e2019JD031260. https://doi.org/10.1029/2019JD031260. Yen, M. C., Peng, C. M., Chen, T. C., Chen, C. S., Lin, N. H., Tzeng, R. W., Lee, Y. A., and Lin, C. C.: Climate and weather characteristics in association with the active fires in northern Southeast Asia and spring air pollution in Taiwan during 2010 7-SEAS/Dongsha Experiment, Atmos. Envoron., 78, 35-50, http://dx.doi.org/10.1016/j.atmosenv.2012.11.015, 2013.
L256: Why did you exclude Fig.8(a)?	Fig. 8(a) is for #17. That sentence begins with "In addition to #17". It is obvious that # 17 was influenced by BRIR at the end of July 2017 and by YRDIR on most days in the same month. The Fig. 8 in the original manuscript has been moved to Fig. S2.8.
L267: Could you put the prevailing wind vector in Figures 2 and 6, otherwise I can	The authors have added monthly average wind field in Fig. 2 and Fig. 6 already. It is obviously the prevailing wind in winter was northeast wind (Fig. 2) but south wind in summer (Fig. 6).

not verify what you described here and similar descriptions in the manuscript explaining the impact of wind patterns.	
L280: Layer4? Is this Layer14?	Thanks the reviewer for pointing out this typo. The authors have corrected 4 to 14 in the revised manuscript.
L281: It is apparent that only vertical motion can not transport PM2.5 from BRIR to #17. What do you mean here?	The authors would like express the transport from BRIR to #17 was not just horizontal but also vertical even the distance is not long between them. The authors have modified the narratives on line 278-279 as "It implies the transport path from BRIR to #17 could be horizontal between BRIR and #17 and then vertical at the location of #17." in the revised manuscript.
L282-283: Why does ascent (descent) motion enhance (decrease) aerosol formation? What processes are involved ?	The authors have added above narratives on line 277-280 in the revised manuscript. It is possibly that the ascent motion of air parcel near the warm surface moved to a cold environment in higher altitude. This may cause condensation and triggered heterogeneous reactions of aerosols. On the contrary, the descent motion of air parcel may cause the evaporation of aerosols.
L291: Fig. (e-2)-(e-4) -> Fig11. (e-2)-(e-4).	Thanks the reviewer for pointing out this typo. The authors have corrected the typo in the revised manuscript. The Fig. 11 in the original manuscript have been changed to Fig. 8 in the revised manuscript.
L293: mixed -> mixture	Thanks the reviewer for pointing out the inappropriate word. The authors have corrected the word on line 294 in the revised manuscript.
L340: higher -> lower?	Thanks the reviewer for pointing out this typo. The authors have corrected the typo on line 347 in the revised manuscript.
L341: underestimated -> overestimated?	Thanks the reviewer for pointing out this typo. The authors have corrected the typo on line 348 in the revised manuscript.
L353: There is not Fig.S2.6 in the supplement	The Fig. S2.6 is on the last page of supplement, now as Fig. 2.11 in the revised supplement.
L380: There is no comparison for July 30th (no Fig. S2.6).	It is really a pity that there is no observation on July 30th due to bad weather (the influence of the thermal low). The authors have modified the caption which does not include observation.

Review #2

Review #3
