Review of “Effect of anthropogenic aerosol emissions on precipitation in warm conveyor belts in the western North Pacific in winter - a model study with ECHAM6-HAM”, by Joos et. al.

This paper examines the anthropogenic aerosol effects on the precipitation from warm conveyor belts (WCB) in the wintertime over North Pacific Ocean. The authors use the ECHAM6-HAM global climate model coupled to the aerosol module HAM, and the aerosol effects are represented by difference of the most polluted and cleanest trajectories of PD simulations. The authors find that the change in overall amount of precipitation due to anthropogenic aerosols is small and insignificant, but the precipitation is suppressed in most polluted warm conveyor belt trajectories.

However, the aerosol effects on precipitation only could be detected in the most polluted cases, and reliability of the statistical methods is also suspicious. Thus, the paper should be revised in response to the major criticisms and resubmitted.

Major criticisms.

The aerosol effects on warm conveyor belt is evaluated by the difference between the most polluted and the clean cases of PD simulations. The authors state that only trajectories has a very similar initial amount of moisture (8-10 g/kg) are compared, which ensures that differences in precipitation can be attributed to differences of the aerosol loading. Similar moisture provides similar initial condition for the precipitation, but it cannot ensure the precipitation to be the same. The precipitation is influenced by many other factors (e.g. atmospheric circulation, stability) besides humidity. One evidence is that the initial precipitation of PI simulation (none aerosol effects) exhibits large diversities (Figure 4f) with very similar initial humidity (Figure 4a). Thus, even with similar initial humidity, the difference of the polluted and the clean cases could not be simply attributed to the aerosol effects.

Moreover, even assuming that there is no aerosol effects in the model (a chemical transport model), such difference still could probably be detected in the simulation based on present statistical methods. It is because the most polluted cases could be correlated to the weak scavenging (less precipitation). The average precipitation of starting region of WCB is up to 8 mmd^{-1}, which may efficiently scavenge the aerosols from the atmosphere and reduce aerosol concentration. In such situation, the polluted (clean) cases could always accompany with less (more) precipitation. If comparing the most polluted cases with the cleanest cases, it could be possibly found that more aerosols tend to “reduce” the precipitation, although there are even no aerosol effects.

It is noticed the total condensate (LWC+IWC) difference is quite small between the polluted and clean cases (Figure 4e). It provides an evidence for that the difference of clean and polluted cases in precipitation could be not due to the aerosols. The aerosols tend to reduce the precipitation, through slowing down the autoconversion of cloud water to rain water with the increase of LWC. However, the differences of the LWC between the polluted and clean cases are very small and insignificant. It implies that the difference in the precipitation may not be a result of the aerosol indirect effects.

The author states that no signal can actually be seen from comparing all WCBs in the PI simulation to all WCBs in the PD simulation, which is due to the very high variability of pollution inside one WCB. Aerosol effects can only be seen when comparing the cleanest with the most polluted trajectories. I agree with the author in this point. However, the polluted cases could be related to the less initial precipitation. Therefore, based on present methods, it is hard to tell whether the precipitation difference is a result or a cause for different cases. The only possible way to evaluate aerosol effects on the most polluted WCB is to compare the most polluted with the cleanest cases with similar initial precipitation, which is strongly recommended to be included in the revised manuscript. The manuscript could only be accepted in the situation that there is significant precipitation difference based on the recommended method.

Other criticisms.

Line 8: “supressed” should be “suppressed”.

Line 66-74: Previous studies show that the anthropogenic aerosols tend to invigorate midlatitude cyclones and the related precipitation, which is contradictory to the results of this study. Corresponding explanations and discussions should be included in the manuscript.

Line 174: The concentration of sulfate aerosol should be compared. It because the AOD and CCN change are mostly from sulfate (Yan et al. 2015).

Line 195: Why are the cleanest WCBs on average moister than the most polluted ones? Do the authors check the relationship between aerosol concentration and initial precipitation?

Line 224: The precipitation from reanalysis data could still have some bias. The authors should better make a comparison with the observed precipitation (e.g. GPCP precipitation).

Line 230: A comparison of simulated (PD run) with observed (MODIS, MISR) AOD should be made here.

Line 240: The CCN, CDNC and LWP are significantly increased due to anthropogenic aerosols. Does it mean a strong second indirect aerosol effect? Why is the change of precipitation small and insignificant, while there is a dramatic cloud property change?

Line 245: How does aerosols affect ice water path in ECHAM6-HAM?

Line 246: Although different microphysical schemes lead to fairly large differences in liquid and ice water paths, a comparison of observed (MODIS) and simulated LWP is strongly recommended to be made here. The reason is that the simulated LWP affects aerosol indirect effect and the readers should know such important information.

Line 265: I might miss something here. How are these cases selected from the 2300 cases? Are those 69 cases shown here only for an example? The authors should make it clear here.

Line 272: The authors state that only a small portion of WCB trajectories is polluted. Why is the number of polluted cases larger than that of clean ones? Just by coincidence? It is better to show the polluted and clean cases with the same number in two different panels.

Line 300: The sulfate concentration and the CCN number should be compared here, for they determine the CDNC and LWP change. The internal mixed BC could serve as CCN, but its contribution could be much smaller than sulfate and OC. I don't quite understand why the authors only choose BC for analysis.

Line 313: According to my opinion, the reason could be that CDNC change is determined by the concentration of sulfate and POM, other than BC.

Line 318: Compared to the PI simulation (clean and polluted together), the LWP of PD simulation is larger, which implies significant aerosol indirect effect. Is the precipitation also significantly reduced accordingly in PD (clean and polluted together) run? In the response to the quick report, the authors state that the precipitation changes for all cases (clean and polluted together) are insignificant. It seems that the WCB precipitation does not change much, although there is significant aerosol indirect effect.

Line 335: The total precipitation of PD polluted cases (19.8 mmd^{-1}) is very close to that of PI cases (20 mmd^{-1}), which is contradictory to the conclusions of the manuscript. The authors state that "Precipitation formation is however suppressed in the most polluted warm conveyor belt trajectories." In the most polluted cases, the total precipitation is almost the same as the precipitation of pre-industry time. Explanations should be given here.

Line 336: Without the uncertainty ranges, it cannot be concluded that the average humidity of PI clean case is the smallest.

Figure 5: At initial point (900hPa), the precipitation of PD polluted case is much smaller (by 10 mmd^{-1}) than that of PD clean and PI cases, while the total condensate is almost the same. How to explain such dramatic precipitation difference?

Line 353: Effective radius change should be given.

Line 353-355: Again, the CCN concentration should be given. It should be a standard output of the model. BC's contribution to CCN is quite small.

Line 357 and Line 362: For most levels, the precipitation of PD clean run is larger than that of PI run (Figure 4f and Figure 5b), which is contradictory to the authors' statements.

Line 359: Why is the LWC almost the same for PD clean and PD polluted cases, but there is significant difference in precipitation?

Line 366: Difficult to understand. What does "timestep" mean? How to get "20%" from figure 6?

Tables and Figures.

Table 1: uncertainty ranges should be given.

Table 1: CDNC number should be given.

Table 1: With this table, it is impossible to know whether the difference between cases is statistically significant. An additional table including the difference between three type of cases should be shown with uncertainty ranges. Meanwhile, the significant changes are shown in boldface.

Figure 2: For model evaluation, only the comparison of PD simulation and ERA-Interim should be included.

Figure 2: The authors state that the overall amount of precipitation is comparable, but it is difficult to get such information from figure 2. Thus, the AOD, LWP, precipitation and CDNC change between PD and PI simulation should be plotted in a separated figure with significance information (based on student t-test) to make the change issue clearer.

Figure 5: More vertical levels should be marked to make it clear.

Reference:

Yan H, Qian Y, Zhao C, et al. A new approach to modeling aerosol effects on East Asian climate: Parametric uncertainties associated with emissions, cloud microphysics, and their interactions[J]. Journal of Geophysical Research: Atmospheres, 2015, 120(17): 8905-8924.