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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fast responses on pre-industrial climate from present-day aerosols in a CMIP6 multi-model study" *by* Prodromos Zanis et al.

Prodromos Zanis et al.

zanis@auth.gr

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We would like to thank Reviewer #2 for the constructive and helpful comments. Reviewer's contribution is recognized in the acknowledgments of the revised manuscript. It follows our response point by point.

1) The Reviewer notes: "Page 1, Line 31: I suggest rephrasing to ": : :to shift away from the cooled hemisphere"." It was revised accordingly as suggested by the reviewer. 2) The Reviewer notes: "Page 2, Lines 30-31: I suggest extending the text in the parenthesis to read like "(affecting climate variables that are mediated by a change in surface temperature and involve the response of the oceans to the forcing)"." It was revised ac-

cordingly as suggested by the reviewer. 3) The Reviewer notes: "Page 3, Line 15: I suggest adding ": : :and variable climate sensitivity per unit aerosol forcing in models" at the end of the sentence." It was revised accordingly as suggested by the reviewer. 4) The Reviewer notes: "Page 3, Lines 15-16: The paper by Kasoar et al. (2016) is also a key one when it comes to explaining model diversity in climate responses to aerosols." The reference of Kasoar et al. (2016) was added in this sentence. 5) The Reviewer notes: " Page 3, Lines 17-24: I would say that this paragraph is somewhat out of place here and interrupts the flow. I suggest moving it to a later part of the paper, e.g. at the beginning of the section presenting the ERF results (Sect. 3). In the place of this paragraph in the Introduction, it would be nice to see a small paragraph making it clear what is new in this study. The Introduction jumps a bit too abruptly from a nice summary of aerosol-climate interactions to a brief paragraph of what this paper will present. But a paragraph on e.g. whether some multi-model study like the current one was pursued for CMIP5 or in other single-model studies would be useful. Then followed by a paragraph outlining what the current study adds to what already exists in the literature (i.e. the final paragraph that already exists)." The paragraph was transferred from Section 1 to Section 2. It was also added a new paragraph as follows: Despite the fact that the slow climate responses of anthropogenic aerosols dominate over the fast responses in zonal means, the fast adjustments are important in regional scale and in global scale for the case BC aerosols as has been noted in several previous single model (e.g. Andrews et al. 2010; Ganguly et al., 2012; Kvalevåg et al. 2013; Li et al., 2018;) and multi-model studies (e.g. Samset et al., 2016; Stjern et al., 2017; Voigt et al., 2017; Liu et al., 2018). 6) The Reviewer notes: "Page 4, Lines 22-23: "from other 3 experiments" -> "from 3 additional experiments"." It was revised accordingly as suggested by the reviewer. 7) The Reviewer notes: "Page 4, Line 28: I suggest removing "Supporting" as that initially implies to the reader that this refers to the Supplement part of the paper." "Supporting" was substituted with "Relevant". 8) The Reviewer notes: "Fig. 1: It is never mentioned in Sect. 2 what types of emissions are actually varied in the sensitivity simulation with present-day aerosol emissions. Does that include only anthropogenic or e.g. also biomass burning emissions. Looking at Fig. 1, it seems that the former is true. But it needs to be clarified." The sensitivity simulations with present-day (2014) aerosol precursor emissions refer to anthropogenic emissions of SO2, BC and OC. This has been clarified now in the text by adding " ...with emissions for anthropogenic aerosol precursors of SO2, BC and OC set to present-day (2014) levels." 9) The Reviewer notes: "Page 5, Lines 1-2: Arguably the Middle East has higher emissions than Europe and N. America." Middle East was also added in the text among the regions with high SO2 emissions. 10) The Reviewer notes: "Page 5, Lines 21-23: Yes, that is the most likely (and classic) explanation, but it needs to be supported by a reference or two." Two references have been added (Shindell et al., 2013; Myhre et al.,2013). 11) The Reviewer notes: "Page 6, Line 6: The numbering/ordering of supplementary figures seems unusual, i.e. Fig. S9 appears in the text after Fig. S2." Following the reviewer's comment, we re-numbered the supplementary figures according to the order they are discussed in the manuscript. 12) The Reviewer notes: "Page 6, Lines 6-7: This statement is a bit rushed. The ERF of BC is comparable to (though indeed smaller than) the sulfate forcing locally over the main emission regions (e.g. East and South Asia)." In this statement we compare the individual SO2, BC and OC ERF patterns in piClim-SO2, piClim-BC and piClim-OC simulations, respectively, of CNRM-ESM2-1, MRI-ESM2-0 and NorESM2-LM (Figure S3) with the all-aerosol ERF patterns for these models shown in Figure 4. We agree that BC ERF is comparable with SO2 ERF over main emission regions such as East and South Asia as well as in other regions over Africa, Middle East and Indian Ocean. Overall, though, sulfate ERF patterns are similar (not identical) to the all-aerosol ERF patterns indicating the dominating role of sulfates in the all-aerosols ERF. This does not cancel out the role of BC in ERF which for some regions can be even higher than sulfates. We have revised the sentence as follows " ... indicating the overall dominating role of sulfates in the all-aerosols ERF (although there are regions where the role of BC outweighs the role of sulfates in the all-aerosols ERF). 13) The Reviewer notes: "Page 7, Line 1: That's mainly true for DJF, right? If so, please state." This statement refers to Figure 6 for the annual basis, but

it also holds for DJF and JJA. The sentence was revised as follows: However, there are regional differences among the models in the pattern of induced fast temperature responses especially over Europe, North America and Africa on annual basis as well as for DJF and JJA. 14) The Reviewer notes: "Page 7, Lines 10-11: And what about the other two seasons not shown? Worth mentioning as they may also play a role." Of course, the transition seasons SON and MAM may also play a role. However, in order to keep a balance between the discussion and the length of the manuscript we decided to limit the whole discussion throughout the paper in the annual basis and the warm and cold seasons. We hope that this decision is understandable by the reviewer. 15) The Reviewer notes: "Page 7, Line 9: It is certainly not 'slight' – at its peak it's actually larger than the zonal mean effect in mid-latitudes." We agree with the reviewer. The word "slight" was deleted. 16) The Reviewer notes: "Page 7, Lines 18-20: Is it not relatively easy to look at land snow/ice cover changes in models, or at least at surface albedo changes? This Arctic warming is a quite pronounced feature of this analysis, therefore a more complete explanation would be desirable." It should be noted that sea ice does not change in these simulations. So, it is only snow over land or on the sea ice that can lead to a positive snow/ice albedo feedback in these simulations. We have looked the respective changes in snow cover fraction over land between piClim-aer and piClim-control simulations (see Figure IV below). The Figure does not show significant changes over the northern polar latitudes in DJF to justify a positive albedo feedback contribution to the warming signal. This was somehow expected as we have already noted that ERF changes are not consistent with the DJF Arctic Warming and thus Arctic radiation changes does not seem to be a plausible explanation. Furthermore, in these simulations there is no ocean circulation changes which implies as plausible cause for the warming atmospheric circulation changes. This is verified with geopotential height and wind vector changes at 850 hPa. A full quantification of the poleward heat advection is beyond the aims of this study. We have revised the sentence as follows "However, the respective changes in snow cover fraction over land between piClim-aer and piClim-control simulations (not shown) do not support such an albedo feedback.

This is consistent with the fact that ERF changes and thus Arctic radiation changes do not seem to be a plausible explanation for the DJF Arctic Warming. Furthermore, in these simulations there is no ocean circulation changes and it remains as plausible cause for the warming, the atmospheric circulation changes which are verified from the geopotential height and wind vector changes at 850 hPa."

Figure IV: Differences between piClim-aer and piClim-control in snow cover fraction over land for the ensemble of 7 models on an annual basis (a). for DJF (b) and for JJA (c). The dot shading indicates areas in which the differences are statistically significant at the 95% confidence level.

17) The Reviewer notes: "Page 7, Lines 30-31: Suggest rephrasing to "So, the heating due to present-day BC emissions cannot justify this warming in NorESM2"." It was revised accordingly as suggested by the reviewer. 18) The Reviewer notes: "Page 8, Line 13: Again, why "jump" from Fig. 7 to Fig. 9?." Following the reviewer's comment we re-numbered the figures according to the order they are discussed in the manuscript. 19) The Reviewer notes: "Page 8, Lines 18-19: Please discuss further what the mechanism of this general land drying is thought to be. Is this mostly a thermodynamic effect (due to cooling) or a dynamical effect?" In fact, there is not general land drying but rather the reduction of precipitation is seen over parts of continental regions (e.g. East Asia, Central and South Africa, Central and South America). As has been shown in previous studies (Samset et al., 2016; Liu et al., 2018) there is strong correlation between global precipitation "fast" response and atmospheric absorption revealing the thermodynamic influence (due to cooling) on precipitation reduction in the global scale. However, at the regional scales there are dynamical contributions due to circulation changes. Liu et al. (2018) showed that, in sulfate perturbation experiments in ocean coupled simulations (fast+slow responses) or in SST fixed simulations (fast responses), the diabatic radiative term has only a small contribution to the changes in precipitation over almost all regions, whereas regional precipitation is mostly controlled by the atmospheric dynamics (see their Figure S5).This is clear for the case of East

Asia precipitation reduction in monsoon season. The dipole pattern of JJA precipitation responses over East Asia (Figure 8c) is similar to the pattern of fast precipitation responses and of the changes in column-integrated dry static energy flux divergence over this region in perturbation experiments with a five-fold increase in SO4 over Asia (see Figure1 and Figure 7 from Liu et al., 2018). The paragraph was modified as follows: "These fast precipitation responses are characterized generally by a reduction over parts of the continental regions (e.g. East Asia, Central and South Africa, Central and South America) with a global annual change of -0.02$\pm$0.01 mm/day. As has been shown in previous studies (Samset et al., 2016; Liu et al., 2018) there is strong correlation between global precipitation "fast" response and atmospheric absorption revealing the thermodynamic influence (due to cooling) on precipitation reduction in the global scale, but regional energy budget analysis clearly indicates the importance of dynamical contributions for heat transport at regional level (Muller and O'Gorman, 2011; Richardson et al., 2016; Liu et al., 2018). Liu et al. (2018) showed that, in sulfate perturbation experiments in ocean coupled simulations (fast+slow responses) or in SST fixed simulations (fast responses), the diabatic radiative term has only a small contribution to the changes in precipitation over almost all regions, whereas regional precipitation is mostly controlled by the atmospheric dynamics either (see their Figure S5)." 20) The Reviewer notes: "Page 8, Line 23: I do not really see much of an increase anywhere around the tropics in Fig. 4a. Only in JJA." Figure 5a shows a decrease of up to -0.05 mm/day peaking at about 7 deg S. In JJA (Figure 5c) this decrease is larger (up to -0.08 mm/day) peaking at around 0 deg. 21) The Reviewer notes: "Page 9, Line 9: I suggest rephrasing to "On an annual basis there is a characteristic dipole pattern of precipitation decreases over East Asia and increases over southern India..."." It was revised accordingly as suggested by the reviewer. 22) The Reviewer notes: "Page 9, Lines 19-20: How do we see a weakening of the monsoon circulation? Please explain a bit more clearly/extensively in the text, as this may not be clear to the reader." Over East Asia there is an anticyclonic anomaly (Figure 9c) which deteriorates the climatological southerly and southwesterly winds, thus weakening the East Asian monsoon and leading to lower precipitation (Figure 8c). Over India, there is a cyclonic flow anomaly extending from the Arabian Sea towards the Bay of Bengal (Figure 9c) associated with a positive anomaly in precipitation constrained to a latitude lower than 22 deg N (Figure 8c). This cyclonic anomaly reinforces the climatological westerly -southwesterly winds over south India, thus strengthening the Indian monsoon and leading to more precipitation. However, the cyclonic anomaly weakens the climatological westerly flow at about 22 deg N, thus constraining the positive precipitation anomaly up to this latitude. This is presumably linked with a southward shift of the ITCZ as can be implied by the pattern of positive geopotential height anomaly north of 22 deg N and negative geopotential height anomaly south of 22 deg N (Figure 9c). The circulation changes due to fast responses in Ganguly et al. (2012) (see their Figure 2a) shows similarities with our Figure 9c. There is a cyclonic flow anomaly in the Arabian Sea associated with a positive anomaly in precipitation. Also, there is a positive precipitation anomaly over Bay of Bengal in both studies. In our case this precipitation anomaly is more extended (from the Arabian Sea towards Bay of Bengal) because the cyclonic anomaly is also more extended to the east. The following sentence was added: “Over East Asia there is an anticyclonic anomaly (Figure 9c) which deteriorates the climatological southerly and southwesterly winds, thus weakening the East Asian monsoon and leading to lower precipitation (Figure 8c). Over India, there is a cyclonic flow anomaly extending from the Arabian Sea towards the Bay of Bengal (Figure 9c) associated with a positive anomaly in precipitation constrained to a latitude lower than 22 deg N (Figure 8c). This cyclonic anomaly reinforces the climatological westerly - southwesterly winds over south India, thus strengthening the Indian monsoon and leading to more precipitation. However, the cyclonic anomaly weakens the climatological westerly flow at about 22 deg N, thus constraining the positive precipitation anomaly up to this latitude. This is presumably linked with a southward shift of the ITCZ as can be implied by the pattern of positive geopotential height anomaly north of 22 deg N and negative geopotential height anomaly south of 22 deg N (Figure 9c). The circulation changes due to fast responses in Figure 9c shows similarities

with the ones presented by Ganguly et al. (2012) (see their Figure 2a) where it is also noted a cyclonic flow anomaly in the Arabian Sea associated with a positive anomaly in precipitation as well as a positive precipitation anomaly over Bay of Bengal.”

23) The Reviewer notes: "Page 9, Line 32: Please add “fast” between “The” and “response”, since Dong et al. (2016) also focused on fast responses." It was revised accordingly as suggested by the reviewer. 24) The Reviewer notes: "Page 9, Lines 30-32: Yes, but since the ocean temperatures are kept fixed, the effect of aerosols on the monsoon is only partly realised. Which is fine, given the focus of the paper on fast responses, but it is worth stressing this again here. The studies by Ganguly et al. (2012) and Shawki et al. (2018) provide nice insight into the differing fast and slow effects of aerosols on the South Asian monsoon, as well as the complementary global and regional mechanisms that are at play.." This fact has been pointed in the text as follows: “However, the effect of aerosols on the monsoon is only partly realized because the ocean temperatures are kept fixed. For anthropogenic aerosols, despite the fact that the slow response due to SST change may dominate the total monsoon rainfall and circulation changes over India (Ganguly et al., 2012) and East Asia (Kim et al., 2016), the fast adjustments are important as has been noted in several previous studies. Decomposition of the total response into fast and slow components indicate that almost all of the precipitation reductions over India (south of 25 oN), Arabian Sea, and Bay of Bengal are a result of the slow response to aerosol forcing, whereas increases in precipitation over the north-western part of the subcontinent as well as decreases over north-east India and Nepal region are due to the fast response to aerosol forcing (Ganguly et al., 2012).” It was also added a sentence about the Shawki et al. (2018) results. " Shawki et al. (2018) showed also similar results in the fast precipitation responses, with a precipitation decrease over India and increase over East Asia in JJA (see their Figure S3), due to SO_2 reductions (opposite perturbation experiment in relation to our study) in different emission regions. It was shown, however, that the location of the emission region plays an important for shaping the detailed features and magnitude of the response.

25) The Reviewer notes: "Page 10, Lines 1-2: Does this paper focus on fast or slow responses. Please clarify this (and I recommend that this is done elsewhere in the text too when referencing findings of other papers, given how different fast and slow responses (and mechanisms) can be)." In Bartlett et al. (2018) the role of oceanic heat transport is deemed minor because of its slower response time than the time-scale of the analysis (2010-2023). Although the experiments are conducted with a fully coupled model, the analysis time period is relatively short in comparison to the time scale required for large-scale oceanic responses to arise due to the ocean's thermal inertia, especially considering that these are transient experiments with continuously evolving forcings. This implies that, while air-sea interactions are accounted for, atmospheric circulation anomalies induced directly by aerosols will likely play a dominant role in driving the changes discussed thus resembling rather fast responses. This has been clarified in the text. 26) The Reviewer notes: "Page 10, Lines 3-4: Most of the papers cited in this sentence cannot be found in the References list of the current manuscript." The references were added in the reference list. 27) The Reviewer notes: "Page 10, Line 10: I am not sure I understand: the west African monsoon involves the inflow of moist air from the central Atlantic Ocean into West Africa. What I see in Fig. 9c is more a strengthening than a weakening of the monsoon." The slight weakening of the easterlies in JJA takes place over west Sahara. Over the Sahel region there is a weak wind flow (in the ensemble of piclim-control) and indeed as noted the reviewer there is a westerly anomaly mainly over the west coast (Guinea, Sierra Leone, Liberia) which can enhance humidity inflow at this region. However, our comment was mainly referring to the monsoon circulation over the Gulf of Guinea where the flow anomaly is diffluent and this circulation anomaly weakens the monsoon flow. We revised the sentence accordingly as follows: "Specifically, the slight Sahel drying in JJA (Figure 8c) is associated with Sahel cooling (Figure 6c), and in terms of circulation changes with positive GH anomalies and an anticyclonic anomaly presumably weakening the West African monsoon over the Gulf of Guinea (Figure 9c). 28) The Reviewer notes: "Page 10, Lines 10-12: The study of Hodnebrog et al. (2018) is of relevance when discussing the

influence of aerosols on west African rainfall (in that case biomass burning aerosols, but still relevant)." The following sentence was added: Also, local black carbon and organic carbon aerosol emissions from biomass burning activities were suggested to be a main cause of local drying of the atmosphere and the observed decline in southern African dry season precipitation over the last century (Hodnebrog et al., 2016). 29) The Reviewer notes: "Page 10, Lines 13-15: Yes, but Westervelt et al. (2017) used a coupled ocean atmosphere model." This has been specified in the text s follows: In response to U.S. SO2 emission reductions (opposite to the perturbation in our study), in long-term perturbation experiments with three fully coupled chemistry-climate models, a northward shift of the tropical rain belt and the ITCZ was also noted delivering additional wet season rainfall to the Sahel (Westervelt et al., 2017). 30) The Reviewer notes: "Page 11, Lines 25-31: I think the second half of this paragraph needs some tightening/rephrasing." This part of the paragraph was rephrased as follows: NorESM2 is one of the models showing a strong warming in the Arctic in the piClim-aer simulation versus the piClim-control simulation. However, the perturbation experiment piClim-BC with present day BC emissions do not show this warming. Instead, the pattern of Arctic warming seen from the temperature differences between piClim-aer and piClim-control is resembled by the perturbation experiment piClim-SO2 with present day SO2 emissions. 31) The Reviewer notes: "Page 12, end of Conclusions section: I think here it would be good if the authors could add a little paragraph reminding the reader that all these results were obtained from short-term simulations (and therefore refer to the fast responses), and that the long-term responses will likely be quite different. Also, please mention if a subsequent AerChemMIP study intends to explore aerosol influences on climate on long timescales." A sentence was added as follows: Finally, it should be reminded that all the above results are based on 30-year perturbation CMIP6 experiments with fixed SST and sea ice, and hence they refer to fast climate responses through rapid atmospheric adjustments. The slow climate responses in long-term centennial CMIP6 simulations through feedbacks affecting climate variables that are mediated by changes in surface temperature and involve the response of the oceans and cryosphere to the forcing are in progress within the framework of the IPCC AR6.

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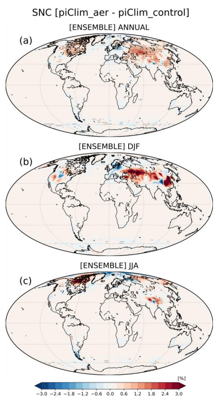

Figure IV: Differences between piClim-aer and piClim-control in snow cover fraction over land for the ensemble of 7 models on an annual basis (a). for DJF (b) and for JJA (c). The dot shading indicates areas in which the differences are statistically significant at the 95% confidence level.

Fig. 1. Figure IV: Differences between piClim-aer and piClim-control in snow cover fraction over land for the ensemble of 7 models on an annual basis (a). for DJF (b) and for JJA (c).