Author Response to Reviewer #3

Please note that we have provided a document containing revised figures and tables, both for the main article and for the supplemental material. In our responses to the reviewers, we will refer to these revised figures and tables, rather than the original.

We thank the reviewer for providing detailed comments on our manuscript, and we address the reviewer's criticisms one by one below.

The manuscript discusses model simulations of direct and semi-direct aerosol effects due to carbonaceous aerosols from biomass burning emissions over southern Africa applying an atmospheric model. The atmospheric GCM used is a state of the art model; the aerosol cycle is not simulated but aerosol properties are prescribed; aerosol effects on the cloud's microphysics are neglected. A set of simulations is performed keeping the sea surface temperatures fixed and varying the assumptions about carbonaceous particle's properties. The authors analyze just the dry season and conclude 1. that carbonaceous aerosol warm the atmosphere and enhance a thermally driven circulation spinning up the water cycle, and 2. that purely scattering aerosol would cool the atmosphere and damp the water cycle.

I think, these conclusions are meager and similar conclusions can also be found in other publications. However, I think, there is space for more thorough analyses of the results, although the investigation has also some conceptual weaknesses.

To counter the Reviewer's opinion that our conclusions are *"meager,"* we like to point out that it has just very recently come to our attention that there is a paper in press in the *Journal of Geophysical Research* (*JGR*) that also examines the effects of biomass burning aerosols on the climate of southern Africa during the austral winter (*JJAS* in their paper) [*Tummon et al., Simulation of the direct and semi-direct effects on the southern African region during the biomass burning season, in press, JGR*]. We provide more comparison to this paper in the discussion to the Reviewer's point regarding comparisons to the literature below. The fact that such a paper is currently in press, given it's similarities to our present study, strongly supports our position that our study is both very timely worthy of publication in ACP. We agree with the reviewer that the manuscript may very well benefit from additional analysis, as suggested below, which we will address in a revised manuscript.

The authors state: "Our equilibrium experiments are neither appropriate to gain in- sight into the actual time evolution of the 20th century climate response to bb aerosol radiative forcing nor can they predict real changes in the African climate." What the investigation was good for?

There is a misunderstanding about the thrust of this study. We are not considering how bb aerosol forcing has changed over time, either past or future. This is the case because the aerosol forcing used here is not transient but constant and representative of the present day. The aerosol evolution over the 20th century is a separate issue. This

investigation is an attempt to understand the present-day effects of bb aerosols on the climate of southern Africa by taking into account the presence of uncertainties in their optical properties. Such a study is important because it highlights the manner in which the aerosols play a role by interacting with the climate processes. The basic climate features need to be understood alongside the phenomena of change. In fact, the basic role of aerosols may need to be elucidated first before the effects due to their changes can be quantified. Admittedly, we do the basic part only here, but there are so many uncertainties in this regard that it may well be impossible to appreciate the influences due to preindustrial to present changes if we do not first set the basic in order. That is what we have attempted to explore here. Further, we cannot predict *"real"* changes in the African climate because we are isolating only the affect of aerosols on the climate, without considering other forcings such as those due to changes in long-lived greenhouse gasses. We extend the analysis of previous studies such as *Roeckner et al.* [2006] and *Tummon et al.* [in press, JGR] because we consider additional prescriptions of aerosol optical properties, both from our base case model (MOZEX) and from observations (HIGHEX, SSAEX, and WHITE for AOD, SSAEX for AAOD) whereas the aforementioned studies only evaluated their model response to their own simulations of aerosols, which have their own flaws relative to observations.

Model set-up:
The treatment of the aerosol effects is pretty simple and it does not include all effects.

We do not include the aerosol indirect effect, but the direct and semi-direct effects are included and occur through the mathematical equations describing the thermodynamics of the atmosphere-surface system in a self-consistent manner. Global-model representations of the highly uncertain Twomey and Albrecht effects, especially in the context of the African continent, remain primitive. Including this effect at the time of this work was not possible neither is it well-defined. Under such circumstances, it behooves us to explore the aerosol-hydroclimate processes under the limiting condition of direct and semi-direct effects only, and later examine the aerosol indirect effects. The exclusion of aerosol indirect effects over southern Africa is not an unrealistic assumption during the dry season, since Swap et al. [2003] suggested that relative humidity and precipitation are so low in the austral winter that the indirect effects are essentially of little consequence, and the subcontinent is influenced by aerosols largely through direct and semi-direct radiative forcing only. Like other studies - even those purporting to include all "indirect" effects – we do not wish to convey we have solved the entire problem here. Instead, we contend that this study advances one facet of the connection of aerosols to hydroclimate in the region by resorting to inclusion of those aerosol physics that are at present better represented in the model.

The assumption of an externally mixed aerosol might result in a too high SSA.

It is true that externally mixed aerosol may result in a higher SSA. However, as seen in Table 2 (revised figures and tables), in the main biomass burning plume, our SSA is generally lower (slightly more absorbing) than the observations, as a result of our carbonaceous mass adjustment (please see response to Reviewer #2 for more details

on the mass adjustment). Since the model response only cares about the aggregate SSA (be it from an internal or an external mixture), which is shown in Figure 1, this point about external mixing is moot. All that should matter in terms of the response of the model is how close are the optical properties of the *total* aerosol (i.e. bb aerosol plus sulfate and natural aerosols) compared to observations. As we have stated in the response to Reviewer #2, the SSA in these experiments, which ranges 0.88-0.89 over the biomass burning region (Table 2) is close to the SSA of 0.89±0.01 reported in *Haywood et al.* [2003] and suggested to be appropriate of the regional haze from bb aerosol observed during SAFARI-2000 [*Abel et al.* 2005]. Also, as *Magi* [2009] points out, it may be more appropriate to model the bb aerosol in southern Africa with the optical properties of regional haze rather than those of fresh fires, given the scales involved in simulating aerosols in climate models.

Coupling of a mixed-layer ocean model, easy to do, would have increased the value of the simulations.

Please also see our detailed response to Reviewer #4, point #6.

At the time of this work, a mixed-layer ocean was not available, and was not easy to do as the Reviewer states. Please see response to Reviewer #4 for more detail regarding oceanic effects. Additionally, we would like to point out that our results are similar to *Roeckner et al.,* [2006], which did include a mixed layer ocean. It is important to note that an atmosphere coupled to a mixed-layer ocean may not yield a realistic distribution of the state of the ocean. Prescribing the SSTs is an accepted mode of study for investigating the fundamental processes in climate. There is a trade-off: prescribing SSTs from observations yields a realistic lower boundary condition for driving convection and atmospheric thermodynamics, but its drawback is that the atmospheric changes cannot lead to changes in oceans and thus shuts off feedbacks. Both types of studies (prescribed SST and coupled ocean-atmosphere) are needed, of course, for a fuller perspective. We offer that discerning an effect with prescribed SSTs is a realistic way of establishing a platform for what the aerosols do. But, why stop with a mixed-layer ocean? The goal should be to push all the way to using a full ocean coupled model. However, the examination has to proceed in a stepwise fashion for a proper understanding and scaling of the aerosol-climate interactions. We agree that a later study with not only mixed-layer ocean but also a full ocean is needed to get more realism.

The scenarios attempt to bound the real world but the AOD of the experiment MOZEX is definitely too low and the absorptivity of all experiments is lower than observed during the SAFARI campaign.

Please see response to Reviewer #2, General Comment #2 as well as Table 2 (revised figures and tables) where we show the aerosol optical properties averaged over both the full region and in the primary biomass burning region. We agree that MOZEX AOD is too low; it is for this reason that we undertook HIGHEX. The *Leahy et al.* [2007] SAFARI-2000 campaign average SSA of 0.85 ± 0.02 is based upon ad PDF of SSA

measurements that include aircraft measurements of scattering and absorption, airborne flux radiometry, and AERONET. The spatial and time period for this PDF was defined as 14-28ºS, 14-35ºE, 10 August - 16 September 2000, and included measurements both near active fires and in plumes of more aged aerosols. We would expect our area-average ASO values to not necessarily agree with the campaign average due in part to sampling differences and different scales. The SSAs in the biomass burning plume (Table 2, revised figures and tables) for MOZEX, HIGHEX, and SSAEX range 0.88-0.89, which are close to the SSA of 0.89±0.01 reported in *Haywood et al.* [2003] and suggested to be appropriate of the regional haze from bb aerosol observed during SAFARI-2000 [*Abel et al.* 2005]. We discuss in the Conclusions the implications of an even more absorptive biomass burning plume. Basically, since the model response is most sensitive to the aerosol absorption optical depth (AAOD), a more absorptive aerosol at a high AOD would exacerbate the magnitude of the model response relative to what we have presented.

Why have the aerosol properties of the experiment HIGHHEX only been scaled at altitudes below 4 km?

Here we repeat our response from Reviewer #2 General comment #2. The vertical distribution of black carbon from MOZART has been validated against observations by *Koch et al.* [2009]. As noted by *Koch et al.* [2009], most global models do not sufficiently confine BC to lower model levels due to either weak upper-level removal processes or excessive vertical diffusion. As shown by Figure 9 in *Koch et al.* [2009], above about 400-600 hPa, regardless of the location of observation (four Western-hemisphere sites were considered), MOZART overestimates the amount of BC relative to the observations. An important semi-permanent feature in the southern African atmosphere during austral winter is the absolutely stable layer (~500 hPa), which tends to trap bb aerosols [*Tyson et al.,* 1996]. For these reasons and combined with the observation of *Haywood et al.* [2003] that biomass burning aerosol tended to be well-mixed in the African boundary layer, we only increased BC and OC below approximately 4 km (~600 hPa). Otherwise, we would have exacerbated the positive bias in BC aloft.

Industry and traffic emit large amounts of BC. Are these emissions included?

The original MOZART-2 aerosol distributions used in MOZEX include emissions from all major sources, including industry and traffic [*Horowitz et al.*, 2006]. Similarly, the observations of AOD from TOMS and AERONET necessarily include BC from all sources. In adjusting our OC and BC mass concentrations we only increase them compared to MOZEX, and we make the assumption in doing so that that increase is due to biomass burning aerosol OC and BC. A similar assumption was made by *Abel et al.* [2005] to scale their model AOD to MODIS.

On the basis of data collected during the SAFARI-2000 field campaign [e.g. *Eatough et al.*, 2003; *Formenti et al.*, 2003; *Gao et al.*, 2003; *Kirchstetter et al.,* 2003], *Magi* [2009]

determined that OM and BC account for 83% of $PM_{2.5}$ in the tropics of southern Africa and 54% in the extratropics. Organic matter (OM) was defined as 1.4 × OC. From this information and mass scattering and absorption cross sections determined during the campaign, *Magi* [2009] determined that in both the extratropics and tropics, OM plus BC accounted for 100% of the aerosol absorption (27% OM, 73% BC in extratropics; 26% OM, 74% BC in the tropics), and OM plus BC accounted for 80% and 90% of the scattering in the extratropics and tropics, respectively. Thus, it seems very reasonable to assume that the majority of the discrepancy between observed and modeled optical properties is due to deficiencies in OC and BC during the primary biomass burning season in southern Africa.

We do not have model diagnostics to determine the percent of total aerosols from biomass burning sources in this study. However, we do have the following information. $PM_{2.5}$ aerosol mass in the standard AM2 configuration (i.e. MOZEX) ranges from 1 to 12 μm m^{-3} (median 6 μm m^{-3}). OM mass ranges as high as 7.5 μm m^{-3} (median 2.5 μm m^{-3}) while BC mass is as high as 0.8 μm m^{-3} (median 0.3 μm m^{-3}) in southern Africa during the biomass burning season [*Magi et al.,* 2009]. The median contribution of OM and BC to $PM_{2.5}$ aerosol mass is 55% and sulfur is 26% during the biomass burning season [*Magi et al.,* 2009; here again organic matter is again OM = 1.4 × OC]. This clearly indicates that OC plus BC dominates the aerosol mass in southern Africa during the biomass burning season in the model base case (MOZEX). Even so, as *Magi et al.* [2009] report, the OM and BC here are likely underestimated; for example, *Eatough et al.* [2003] and *Formenti et al.* [2003] suggest submicron aerosol ranges from 15-75 μm m^{-3} in regional hazes near direct sources of biomass burning in southern Africa.

Further analyses:
No experiment shows any significant change in precipitation during the dry season. Roeckner et al have shown that aerosol induced changes of the soil water content affects the onset and the strength of the wet season. I suggest analyzing additionally the soil moisture and potential aerosol impacts throughout the whole year.

We thank the reviewer for the suggestion to analyze soil water content and the variability of it and other hydrologic parameters throughout the whole year. We can analyze to do this should a revised manuscript be warranted.

For now, we show in a revised Table 2 (revised figures and tables), the ASO change in soil moisture. The positive bias (relative to CTRL) for MOZEX and HIGEX are expected given their positive biases in P-E. Interestingly, SSAEX has a negative bias in soil moisture in the area-average.

Of course, area-averages sometimes cancel out opposite changes within the same domain. In Figure 7 (revised figures and tables), we show the spatial change of soil moisture relative to CTRL for each experiment as well as the spatial change in P-E. There is a clear difference in the storage of soil moisture between HIGHEX and SSAEX. This seems reasonable considering the changes in P-E (Figure 7), which are smaller in SSAEX compared to HIGHEX. These differences in P-E and soil moisture may warrant

an investigation of the monthly-mean evolution of the model response to bb aerosol forcing as the Reviewer suggests for a revised manuscript. We also note the strong positive anomaly in P-E over the Atlantic for both HIGHEX and SSAEX, as evaporation decreases strongly in this region (as expected from the anomalous surface wind patterns, not shown).

The discussion section lacks any comparison to other publications.

In the manuscript, we make numerous comparisons to other studies such as *Roeckner et al.* [2006], *Paeth et al.,* [2006], *Abel et al.* [2005], *Lau et al.* [2006], *Randles and Ramaswamy* [2008], and *Johnson et al.* [2004], amongst others. In our responses to the reviewers, we also include references to *Miller et al.* [2004; response to Reviewer #4], *Koren et al.* [2004; response to Reviewer #4], *Lau et al.* [2009; response to Reviewer #4], *Koch and del Genio* [ACPD, 2010], and the paper we very recently discovered in press in the *Journal of Geophysical Research*, *Tummon et. al.* [in press, *JGR*]. Throughout both the manuscript, and now in the responses, we also provide numerous references to observational papers from SAFARI-2000 such as *Haywood et al.* [2003], *Leahy et al.* [2007], and *Magi* [2009] amongst others. If the Reviewer is aware of other studies to which we should refer, we would ask him to kindly provide references that we may have missed.

It is difficult to make quantitative comparisons to other modeling studies because our experimental design may be very different. We have tried to relate our aerosol optical properties to the observations in a broad sense.

We would, however, like to draw comparisons with the paper in press in the *Journal of Geophysical Research* (*JGR*) that also examines the effects of biomass burning aerosols on the climate of southern Africa during the austral winter (*JJAS* in their paper) [*Tummon et al., Simulation of the direct and semi-direct effects on the southern African region during the biomass burning season, in press, JGR*]. The *JGR* paper uses a high resolution regional model (RegCM) with prescribed SSTs and on-line (interactive) aerosols and no aerosol indirect effect. They only consider the forcing of aerosols simulated by their model and a case similar to MOZEX in which biomass burning emissions are from GFEDv2 [*van der Werf et al.,* 2006]. In both of their cases, their simulated aerosol optical depth peaks one to two months early and dies off one month too early compared to both satellite and AERONET measurements (and most of our simulations), and spatially the peak in their AOD is roughly 5 degrees too far to the south. In contrast, we consider multiple aerosol scenarios with differing prescriptions of aerosol optical depth and aerosol absorption optical depth to yield an even wider perspective into the model response to direct and semi-direct effects of bb aerosols. Even so, the *JGR* paper reports extremely similar findings to ours (i.e. surface air temperature decrease, increased vertical velocity, increased low-level convergence, and increased clouds particularly in the main bb aerosol region with inclusion of aerosol forcing relative to a case without aerosol radiative interactions). In fact, their temperature decrease is practically the same as our experiments MOZEX and HIGHEX (they obtain -0.21°C for their AERO experiment while HIGHEX has a decrease of

-0.27°C; for their GFED experiment they obtain -0.07°C while we obtain -0.12°C for MOZEX). In both papers, the aerosols act to reduce the positive bias in surface temperature compared to observations (supplemental Figure S.3). They generally get stronger increases in precipitation, but their precipitation increases are also confined to the tropical belt where atmospheric conditions are favorable to instability. We reiterate that the fact that such a paper is currently in press, given it's similarities to our present study, strongly supports our position that our study is both very timely worthy of publication in ACP.

Typo Table2: area average 19E - 50W; isn't it 19W – 50E?
This was a typo and has been fixed. Thank you for pointing this out to us.