Author Response to Reviewer #2:

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.

This manuscript presents a GCM study of the biomass burning aerosol impacts on southern Africa's climate via direct and semi-direct aerosol effects (no aerosol-cloud interaction), with an emphasis on surface temperature and precipitation responses. While this study including some interesting aspects, the study is more like a routine modeling exercise without new insight, the approach is somehow questionable, and the findings do not match the claims of what this study offers. More specifics are given below. On the other hand, I think it is unrealistic to demand new findings for every publications and this study does seem to contain publishable results. I would suggest the authors consider a significant revision and state clearly what this study is about.

We thank the reviewer for taking the time to thoroughly address his or her concerns with the manuscript while also acknowledging that this work does indeed have merit. Below we address, in detail, both the reviewer's General and Specific comments, providing additional data and analysis where necessary (please refer to revised figures and tables as appropriate).

General Comments

1. Significance of this study:

It has claimed in the introduction and other sections in the manuscript that this study considers "a wide range in bb aerosol forcing", which gives "substantial additional perspectives" relative to previous GCM studies and attempts "to address uncertainties in bb aerosol radiative forcing". (here bb stands for biomass burning). However, reading through the text, I found none of these statements are substantiated. There is no "wide range" considered – among the four model experiments, MOZEX shows unrealistic features of bb in southern Africa and WHITE is just a hypothetical case assuming all bb aerosol are non-absorbing, which does not fit in the "range". Only two cases are somewhat close to the reality, HIGHEX and SSAEX, which are very close to each other. A realist range in terms of AOD and SSA can be defined from observed multi-year or seasonal variations of these values from bb aerosols, which the authors did not do.

I don't see this studying adding any substantial additional perspectives and addressing uncertainties in biomass burning aerosol radiative forcing, either.

The purpose of this study, which requires necessarily a model and experiments, is to test the sensitivity of the climate system ***response*** to variations of two key aerosol optical properties - the aerosol optical depth and aerosol absorption optical depth (and, by extension, the single scattering albedo) in a systematic way utilizing available observations that illustrate the range in the consequences for climate that perturbations in these two aerosol characteristics impart. In other words, it is a first-degree estimate of the range of aerosol radiative (both direct and semi-direct) impacts on the region during the main biomass burning season. This is similar to the approach taken by *Abel et al.* [2005], a study that estimated ranges for bb aerosol radiative forcing by a wide range of changes to the horizontal distribution of aerosol AOD (i.e. from their model and from their model scaled to MODIS AOD), the vertical distributions of aerosols (oceanic or

land profile), and to changes in single scattering albedo (0.89 or 0.84 everywhere), surface albedo and clouds. It is also similar to the approach adopted in *Menon et al.* [2002], *Randles and Ramaswamy* [2008], and a paper in press that has just recently come to our attention [*Tummon et al., in press, JGR;* see response to Reviewer #1 for more on this paper], though our study considers more aerosol cases than the paper in press. Since we are running full equilibrium AGCM experiments, we are necessarily limited in the amount of cases we can consider compared to *Abel et al.* [2005], which only calculated radiative forcing. It is instructive to point out that *both* aerosol parameters noted above (aerosol optical depth and aerosol absorption optical depth) need to be explored in order to gage the scope of the response. This study attempts to do that using some constraints offered by the observations.

Rather than stating that the study considers a "*wide range of biomass burning aerosol forcing*" it perhaps would have been more appropriate to state that this study considers a "*range of absorbing aerosol optical depths ... to address differences in the model climate response to differences in biomass burning aerosol optical properties.*" In the revised Table 2 (see revised figures and tables), we now also show the aerosol absorption optical depth (AAOD), where AAOD = AOD - SSA × AOD. The AAOD ranges from a low approaching zero (WHITE) to a high of approximately 0.04 in HIGHEX. Though the differences in the area-average SSA between HIGHEX and SSAEX are small (as is the case for the area-averaged forcing as well, Figure 4), we would argue that spatially they are different (as can be seen in from the revised Figure 1). Despite the spatial differences in SSA between HIGHEX and SSAEX, there is a considerable similarity in the response of the climate. This may indicate that as models incorporate more realistic representations of aerosol distributions, the climate ***response*** of those models may not vary substantially, provided that the magnitude and distribution of the modeled AAOD is reasonably represented. This similarity in response may be strongly linked to the cloud response, which can eclipse the impact of aerosol radiative forcing alone, which was also the case in *Randles and Ramaswamy* [2008] and described in *Koch and del Genio* [ACPD, 10, 7323-7346, 2010, www.atmos-chem-phys-discuss.net/10/7323/1020/]. This is an important point to be underscored as several earlier studies of the semi-direct effect of absorbing aerosols fail to highlight this. Furthermore, this study reaffirms, as posited by *Randles and Ramaswamy* [2008] that the response of the model hydrologic cycle (i.e. precipitation, clouds, atmospheric water vapor) to aerosol forcing is most sensitive to the magnitude the AAOD, as evidenced by the contrary climate response seen when AAOD approaches zero in WHITE.

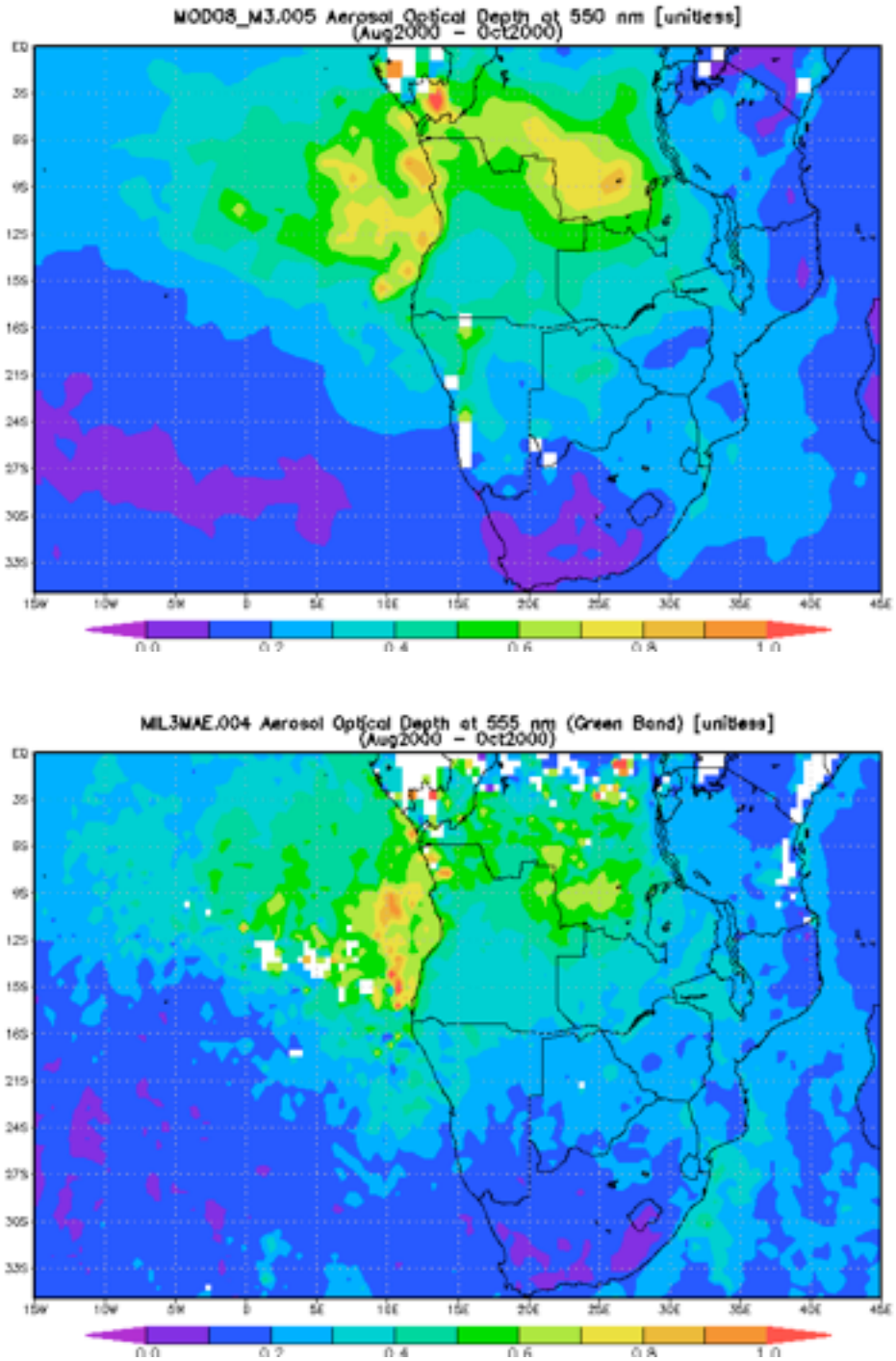

Figure R.1: Here we show the Y2000 ASO AOD from MODIS (top) and MISR (bottom) from the NASA Giovanni website (http://daac.gsfc.nasa.gov/giovanni/). We show these plots to make two points. First, retrievals of aerosol optical properties will differ depending on the sensor chosen (and the choices made in creating a gridded area- and time- averaged data product). Secondly, despite the fact that HIGHEX, SSAEX, and WHITEX may overestimate AOD compared to TOMS due to the effects of aerosol humidification as described in the text and detailed below, their AODs are not that different from MODIS. For example, the AOD hot-spot at roughly 9S, 27E is ~0.8 for both MODIS and our experiments.

2. Method

The approach of using TOMS aerosol data is problematic. First, the authors used the TOMS AOD and SSA (plus AERONET when needed) to generate the column AOD and SSA map, then they used this map to adjust the MOZART BC and OC in order to match the TOMS-based AOD (in HIGHEX and SSAEX) and SSA (in SSAEX) for input to the GCM simulations. However, the adjusted AOD and SSA are still substantially different from TOMS-based values, as the AOD at 500 nm is significantly higher and the SSA significantly lower than TOMS (Fig. 1a and 1b). It is also puzzling that over some area the HIGHEX AOD is lower than both TOMS and MOZEX (Fig 1a), and the TOMS SSA values outside of the thick plume were not even considered in the model adjustment (Fig 1b). The use of the TOMS in model adjustment thus needs to be better explained.

We disagree that our method is "*problematic*." Our simulated optical properties are not identical to the observations, but neither are the simulated aerosol optical properties applied in other models that have examined the impact of bb aerosols on the climate of southern Africa [e.g. *Roeckner et al.* 2006; *Tummon et. al.,* in press in the *Journal of Geophysical Research*. For example, in *Tummon et al.* [in press, *JGR*] 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 spatially the peak in their AOD is roughly 5 degrees too far to the south. One other point to make is that the present state of observations is incomplete and does not permit quantitative details on *all* the variables that are needed for evaluating climate response. This is not a criticism of observations but a reality. Hence, assumptions are necessary (within some bounds) to do modeling experiments. It is critical to evaluate the sensitivity to variations in parameters as part of the assumptions, and that is what we do here. These are basic necessities in unraveling the aerosol processes in climate.

We explain very briefly in lines 19-26 on page 9737 why there are differences between the TOMS/AERONET AOD, AAOD, and SSA (Figure 1 a-c) and the model experiments (Figure 1 d-l). Here, as the reviewer suggests, we explain the model adjustment in much greater detail. Such a discussion could be contained in a revised manuscript. First, the experiment MOZEX was run for 35 years and aerosol mass distributions, optical properties, and relative humidity averaged over the last 30 years. These 30-year averaged quantities, were then used to solve the following two equations, which is identical to the method applied in *Menon et al.* [2002] and *Randles and Ramaswamy* [2008]:

(1) $$AOD = \sum_z (M_{BC}\sigma_{BC} + M_{OC}\sigma_{OC}f_{OC}(RH) + M_{SO4}\sigma_{SO4}f_{SO4}(RH) + M_{du}\sigma_{du} + M_{ss}\sigma_{ss}f_{ss}(RH))$$

(2) $$AAOD = \sum_z (M_{BC}\sigma_{BC}(1-\omega_{BC}) + M_{OC}\sigma_{oc}f_{OC}(RH)(1-\omega_{OC}(RH)) + M_{du}\sigma_{du}(1-\omega_{du}) + M_{ss}\sigma_{ss}f_{ss}(RH)(1-\omega_{ss}(RH)))$$

where *M* refers to aerosol mass, σ to the mass extinction efficiency [$m^2\ g^{-1}$], *f(RH)* to the variation of σ with relative humidity, and ω to the single scattering albedo of an aerosol component, which may also vary with RH depending on the aerosol component. DU

and SS refers to dust and sea salt, respectively. Equations 1 and 2 were solved for $\boldsymbol{M_{BC}}$ and $\boldsymbol{M_{OC}}$ under the following assumptions:

(a) We assumed that, for each aerosol component (i.e. BC, OC, SO_4, dust, and sea salt), the optical properties (which also vary depending on wavelength) were those in the AM2-LM2 Mie tables described on page 9734 (Lines 22-27). The equations were solved at 500 nm, and the spectral variation of aerosol optical properties was determined by the AM2-LM2 Mie tables.
(b) RH was assumed from the 30-year mean from MOZEX; therefore, *f(RH)* and *ω(RH)* were fixed to a single value for each aerosol constituent. This assumption has consequences for the optical properties of HIGHEX, SSAEX, and WHITEX if their RH differs from the 30-year MOZEX average.
(c) Dust, sea-salt, and sulfate mass were the same as MOZEX.
(d) AOD and AAOD (right of equal sign in Equations 1 and 2) were from either MOZEX or the "observationally-based maps" in Figures 1 (a-c) (where AAOD = AOD - SSA × AOD), depending on the experiment.
(e) Masses were adjusted below ~4km (~600 hPa) only (see below for explanation).

These assumptions allow us to solve Equations (1) and (2). However, in a mass-based model, it is difficult to constrain aerosol optical properties exactly because of non-linearities associated with aerosol hygroscopic growth. As noted in lines 18-23, page 9737, because of assumption (b) the resulting $\boldsymbol{M_{BC}}$ and $\boldsymbol{M_{OC}}$ may yield different AOD, AAOD, and SSA from either MOZART or the "observationally-based" maps if the experimental relative humidity is different from that assumed in solving Equations (1) and (2) (i.e. 30-year mean from MOZEX). As we show in Figure 5 (revised tables and figures), there are indeed changes in atmospheric water vapor in HIGHEX, SSAEX, and WHITE compared to MOZEX, and these changes necessarily would yield optical properties that are different from those used to solve Equations (1) and (2). Our method is similar to that applied in *Menon et al.* [2002] and is similarly imperfect.

It is interesting that the authors recognize the differences after the BC/OC adjustment and went on to say "However, since our purpose in this sensitivity study is to try and bound the real world, it is not necessary that we precisely mimic observations, which are also very uncertain". Do you consider TOMS observations as "real world" or not? If they are, why did it not matter? If they are not and very uncertain, why did you bother to use them?

We do consider the TOMS observations to be more representative of the actual aerosol optical properties than those calculated by AM2-LM2 based on MOZART aerosol distributions; however, we do recognize that these observations themselves are also uncertain. This uncertainty has to be factored into the interpretations. For example, as noted by *Torres et al.* [2002], the largest errors in *EP*-TOMS retrievals of optical properties occur when AOD is less than 0.2. This could explain why TOMS AODs are higher compared to the model simulations over the ocean to the south of the biomass burning plume (south of 20°S). Alternatively, the model could have too much sea salt mass in this region. As another example, the model simulations predict higher AOD than observed in the industrial Highveld region of South Africa (~26°S, 28°E) where there is a

large sulfate source (recall, all simulations have the same sulfate mass distributions). This could indicate that the model sulfate is too high here. In Figure R.1 above we show the AOD from MODIS and MISR (data products from NASA Giovanni) to illustrate that there is variability in satellite retrievals of aerosol optical properties (and in available data products, which are gridded and time-averaged). Given this, our experiments with higher AOD (HIGHEX, SSAEX, and WHITE) seem to fall within the variability of retrieved AOD and certainly do a better job of capturing the bb aerosol plume AOD compared to MOZEX (as shown by Figures 1 and 3 in revised figures and tables).

A priori, we did not know the ramifications of implementing TOMS-like aerosol optical properties in the model; we did, however, based on previous studies (e.g. *Randles and Ramaswamy* [2008]) expect the model response to differ from the model forced by MOZART aerosol distributions, since the AOD from MOZART in southern Africa is known to be low (e.g. *Magi et al.* [2009]). Our statement that *"...it is not necessary that we precisely mimic observations, which are also very uncertain"* is really a conclusion of this study. From the results of this study, first it appears that the model response is sensitive to the magnitude of the aerosol absorption optical depth (AAOD) (as evidenced by the differences between MOZEX, HIGHEX and WHITE). Secondly, the response of the model is less sensitive to differences in the spatial distribution of AAOD (or SSA) given that the magnitude of AAOD is similar over this given region, as evidenced by the similarities in the response of HIGHEX and SSAEX. Had we been able to implement the TOMS AOD (Figures 1a) into the model perfectly for HIGHEX we might expect that the model climate response would be slightly damped compared to our current result because our AOD (and AAOD) would be slightly lower than at present (e.g. as the sign of the response in MOZEX is the same sign as that of HIGEX, but lower in magnitude; Table 3). Then, given a perfect implementation of TOMS AOD in HIGHEX, we would then not necessarily expect much difference in the model response to a perfect implementation of TOMS SSA in SSAEX. In summary, if the GFDL AM2-LM2 AGCM were forced with a more realistic distribution of AAOD, the response of the model in southern Africa would not differ substantially if effort were then taken to also force the model with a more realistic distribution of SSA. Therefore, it is the absorbing aerosol component, and not the scattering aerosol component, that is the driving force for the model climate response in this region.

Second, I don't understand why adjust the MOZART BC and OC profiles just below 4 km to match TOMS. Unless in-situ or some other vertical measurements have suggested that the MOZART simulated aerosol only underestimates the BC and OC within the lowest 4 km but not aloft, the entire column mass should be adjusted accordingly.

The vertical distribution of black carbon from MOZART-2 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 the observation (four Western-hemisphere sites were considered), MOZART-2 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.

Also, why the BC vertical shape in SSAEX is so different from other shapes (Fig S2), i.e. the BC shows a maximum at 900 mb in SSAEX but almost a minimum in other experiments, if the only adjustment is overall BC/OC fractions below 4km?

We thank the reviewer for noticing this. There was a mistake while averaging the vertical profile for SSAEX (we averaged over the wrong region for this experiment). We have included a revised plot in the revised figures and tables document.

Minor specific comments about the text and figures.

1. Bottom of p. 9332 and top of p. 9333: uncertainties in bb aerosol radiative forcing are also in the amount of bb emitted to the atmosphere and its relative height to clouds.
   This important point can been added to the text as follows:
   "Much of the uncertainty in bb aerosol radiative forcing results from uncertainties in (1) the aerosol optical properties (2) the spatial distribution of the aerosol in both the horizontal and vertical (e.g. Haywood and Ramaswamy, 1998), (3) the amount of biomass burning aerosol, and (4) the vertical distribution of the absorbing biomass burning aerosol relative to cloud (e.g. Chýlek and Coakley (1974))."

2. p. 9734, line 6: related to what I stated earlier: "substantial additional perspectives" – in terms of what? Be specific if there is such information from this study.
   We can reword this as follows:
   "In the present study, using an atmospheric general circulation model (AGCM), we investigate the sensitivity of the climate impacts by considering a range in bb aerosol optical properties, namely the aerosol absorption optical depth (AAOD). We expect that in doing so, we will also obtain a wider range in model climate response that will offer additional perspectives on the direct and semi-direct impacts of absorbing bb burning aerosols on the hydrologic cycle over southern Africa relative to the aforementioned studies."

3. p. 9735, line 11-16: The problem here is that bb also produce sulfate aerosol, and BC and OC also have anthropogenic sources other than bb. The authors should at least demonstrate/ explain that over the study regions most (xx%) of bb aerosols are BC and OC and yy% of total aerosols are from bb in the studied area and season.

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 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.

4. p. 9736, line 15-16: the overestimate of AOD by EP-TOMS is mostly because of the cloud contamination in such a coarse resolution pixel size.

   We can reword as follows to reflect this important point:

   "Over-estimates by EP-TOMS occur when extinction optical depths are below 0.2 because the coarse resolution of the EP-TOMS product makes it difficult to resolve small-scale variability. Other sources of uncertainty in the EP-TOMS retrieval algorithm include aerosol vertical distributions, sub-pixel cloud contamination effects, and the surface reflectivity (Torres et al., 2005)"

5. p. 9736, last paragraph: Is there a discontinuity between TOMS and AERONET which may be a problem for your gap-filling method? For example, in a gridbox where both TOMS and AERONET are available, how do they agree with each other? I am not asking you to do thorough comparisons between TOMS and AERONET, but at least you should acknowledge such a problem and justify your approach.

Torres et al. [2002; 2005] did a very thorough comparisons of *EP*-TOMS and AERONET retrieved AOD and SSA in southern Africa. As stated on page 9736 lines 19-21, they found TOMS-retrieved AOD and SSA generally within the observational uncertainty of AERONET (±30% for AOD and ±0.03 for SSA). Thus, though there are differences between *EP*-TOMS and AERONET, we believe our method of gap-filling is reasonable. We did do our own comparison of TOMS and AERONET (not shown), but we did not wish to publish these results as they are not done in such a thorough manner as in *Torres et al.* [2002; 2005], which co-located TOMS and AERONET observations at high time and spatial resolution. We only had daily-averaged TOMS and AERONET data available to us, and TOMS data were at a gridded horizontal resolution of 1°×1°, which we first (before gap filling) re-gridded to 2°×2.5°. Even with such a coarse comparison (comparison done at 1°×1° resolution), we got a correlation coefficient (R^2) of 0.59 for TOMS and AERONET AOD considering the 14 sites in Table S.1 (revised figures and tables). Torres et al. [2002] got $R^2 = 0.97$ at Mongu. Of our SSA comparisons, 53% were within ±0.03 of the reported AERONET SSA (Torres et al. [2005] got 63% of TOMS SSA within ±0.03 of the spatially and temporally co-located AERONET retrieval).

6. p. 9737, line 11: Forcing efficiency – is this the shortwave direct forcing per unit of AOD at 500 nm? Clarify.

Yes, this is all-sky (i.e. cloudy) instantaneous direct radiative forcing per unit AOD for the model shortwave band. We have clarified this in the text, and it is also in the caption of Figure 4b (revised figures and tables).

7. p. 9737, method of adjustment: see my general comments.

We have expanded upon our discussion of the method of adjustments, making explicit the equations we have solved and the assumptions made in order to solve them. Please see the discussion above. We would also like to point out that the ratio of retrieved AAOD from TOMS to modeled AAOD is 0.6 for MOZEX for the entire region in Table 2 (revised figures and tables), which is the same as what was found by *Koch et al.* [2009]. For the other experiments with absorbing aerosols, this ratio ranges 1.8-1.9 in the main biomass burning region (Table 2). Our method of adjustment is consistent with the method used in both *Menon et al.* [2002] and *Randles and Ramaswamy* [2008].

8. p. 9738, line 7-8 and Fig 2, comparison with AERONET: Have any of these stations been used in constructing the maps in 1a and 1b to adjust the modeled BC/OC? If so, then this comparison is not independent and not valid. Also, why not comparing SSA with AERONET retrievals?

Yes, these stations were used in the adjustment of modeled BC and OC. However, we fail to see the Reviewer's point that these comparisons are not valid. The point of this figure is to show that the MOZEX AOD was too low in comparison to the AERONET observations during the primary biomass burning season. For reference and comparison, we also show the AOD for the other experiments, which we *expect* to be very near the AERONET observations as they are indeed not independent. We reiterate, however, that AERONET data were only used if TOMS data for the co-located grid-box was not available on a given day, after the TOMS data were aggregated to the model resolution. There tended to be few AERONET retrievals of SSA (or we had *EP*-TOMS data available instead), so our observational-based maps are biased towards the *EP*-TOMS retrievals of SSA which tend to be less absorbing than AERONET. For example, at Mongu in the main biomass burning region, the SSA averaged for ASO is 0.8 while MOZEX, HIGHEX, and SSAEX have SSAs of 0.88, 0.87, and 0.89, respectively. These experimental SSAs are closer to the value reported for southern African bb haze (0.89) by *Haywood et al.* [2003] and are more appropriate for a 2°×2.5° gridbox [e.g. *Magi,* 2009]. A figure such as Figure 3 but for SSA is less meaningful because there are typically only a handful of AERONET SSA retrievals in a given month, making it difficult to determine a monthly mean value.

9. p. 9738, line 11-13: Is SSA=0.9 in Fig 1d and 1f the area average? You should, to be more appropriate, compare the SSA with SAFARI-2000 over the measurement area to see how much overestimate it is. In addition, the TOMS map (Fig 1b) shows SSA value of 0.96! Are they wrong? Why didn't you consider the TOMS value in your bounding experiments? At a minimum, you should comment on it!

Yes. All quantities reported in the paper are area-averaged over the region (3ºN-37ºS, 19ºE-50ºW) as denoted in the caption of Figure 1. As you suggest, we have added an additional data to Table 2 (revised figures and tables), in which we denote the aerosol optical properties both over the whole region over land (3ºN-37ºS, 19ºW-50ºW) and over land in the main biomass burning region (7ºS-17ºS, 11ºW-29ºW). In Table 2, it is clear that the experimental simulations have an SSA closer to observed in the main bb region than if we consider the larger region. The high (0.96) area-average SSA for the whole region from TOMS in Figure 1c (and Table 2) is a

consequence of area-averaging to the south of the main biomass burning plume, where AODs are often lower than 0.2 As noted in the text, TOMS tends to overestimate AOD (and therefore AAOD) below 0.2 (*Torres et al.*, 2005). It is possible that in this same region the model could have potentially overestimated AOD from sea salt and/or sulfate. In Table 2 it is clear that in the main biomass burning region, the SSA is lower for all experiments except WHITE. In fact, 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. Though we did not consider the higher TOMS SSA over the Atlantic (0.98-1.0) south of about 20ºS in SSAEX (where TOMS AOD is < 0.2 generally), we did consider very low SSA in WHITE for the entire region.

10. p. 9738, last sentence before section 3, regarding the BC and OC mass adjustment: This should be moved to earlier part when you talked about the adjustment. I was wondering how much mass you have to add when I was reading that part.

As the reviewer suggests, we can move the information regarding the amount of BC/OC mass adjustment into the previous two paragraphs so that this information follows the description of each experiment. We have also decided to include Figure S.1 (BC and OC column loads from each experiment) and the information from Table S.2 into the main text. This will be the new Figure 2 and part of Table 1, respectively.

11. Figure 1e and 1f: They provide very different content than 1a-1d and should be an independent figure being introduced in the "Results" section, especially currently Figure 1f was introduced after Fig 2.

We assume the reviewer actually means Figures 1g and 1h. As the reviewer suggests, we have made Figures 1g and 1h into a new Figure 4 (a and b) to be introduced in the "Results" section.

12. p. 9738, line 22: Is ASO over southern Africa an isolated "climate system"?

This is a global model and therefore ASO over southern Africa is clearly not an isolated "climate system" we did not mean to imply such. We can reword as follows:

"...which evaluated the representativeness of the aerosol optical properties against observations, here we evaluate the sensitivity of the climate response over the region to the bb aerosol specifications."

13. p. 9739, line 2: "compare and contrast the range of climate response possible given a realistic range" – what is the realistic range? As I said before, there is no range defined in this study, given the closeness of HIGHEX and SSAEX and unrealistic characteristics of MOZEX and WHITE. The authors should look the AOD and SSA ranges over the studied area from the long-term observations from TOMS or AERONET or other data to come up with a "realistic range" of bb aerosols over southern Africa.

As discussed in the response to the general comments on the significance of the study, <u>if we consider this study as testing the sensitivity of the regional climate response to AAOD, then we do indeed have a range of values spanning those similar to TOMS to close to zero</u>; we should remove the adjective "*realistic*" since we also consider an AAOD approaching zero. It is important that we consider the case WHITE, however, because it serves to highlight the opposing roles of absorbing and scattering aerosol direct and semi-direct impacts on climate in this region. Furthermore, we adjust our horizontal distribution (and, to a lesser extent, our

vertical distribution) of aerosol optical properties to be more consistent with available observations, as was done in *Abel et al.* [2005], though that study only examined aerosol radiative forcing and not model response. We could rephrase to state *"compare and contrast the range of climate response possible given a range of AAOD that widely bounds reality."*

The AERONET AOD (Figure 3, revised tables and figures) are the long-term climatological monthly mean and standard deviations. From Figure 3, the AOD from HIGHEX/SSAEX/WHITE falls well within the variability of the long-term AERONET observations. MOZEX AOD is clearly biased low compared to AERONET in the primary biomass burning region (e.g. Mongu and Senanga).

14. P. 9739, line 12-13: If the bb forcing estimated from this study and from Abel et al. (2005) are consistent despite the large difference in SSA (0.9-0.91 from HIGHEX and SSAEX and 0.84 in Abel et al.), does this mean that SSA doesn't matter at all? What is the implication?

There was a mistake in the original manuscript. In *Abel et al.* [2005], the SSA for highly absorbing bb haze was 0.89 (taken from *Haywood et. al.,* [2003]). In the main bb region (Table 2, our SSAs for MOZEX, HIGHEX, and SSAEX lie in the range 0.88-0.89.

We should point out that aerosol forcing depends on the aerosol optical properties (i.e. AOD, AAOD, SSA) and is highly sensitive to their vertical distribution and position relative to clouds. *Abel et al.* (2005) likely had a different vertical distribution of aerosols and clouds than in this model. We may have too much absorption above cloud compared to that study. Alternatively, our cloud distributions could be different horizontally, and this too could impact the area averaged forcing. The areas considered and the model resolutions are not exactly identical, either. Furthermore, we would like to stress that the response of the climate is more sensitive to the AAOD than the SSA.

15. p. 9741, line 25: Again, be clear about "range". There is no realistic range considered in this study.

We now refer to a range of AAOD that brackets the TOMS observations from zero to slightly higher than TOMS. We respectfully disagree that this is not a "*range*"; particularly if one considers differences in horizontal distributions of aerosols. We do, however, remove he adjective "*realistic*" because we consider the hypothetical WHITE case.