Author's response

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Title: Satellite inference of water vapor and aerosol-above-cloud combined effect on radiative budget and cloud top processes in the Southeast Atlantic Ocean
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Authors want to thank the Co-Editor, Paquita Zuidema, for her contribution and interactive comments. The answers to specific questions are addressed below, while the modifications made in the manuscript are in red.

1. There is a sentence in the abstract that is difficult to parse: "A detailed analysis of the heating rates shows that the absorbing aerosols are 90 % responsible for warming the ambient air where they reside, with approximately +5.7 K/day, while the accompanying water vapour above clouds has a longwave effect of +4.7 K/day (equivalent to 7% decrease) on the cloud-top cooling." There is no altitude provided, and I have trouble reconciling the positive longwave effect with the 7% decrease - in what? - followed by a reference to cloud-top cooling. Are the parentheses misplaced? Since this sentence is in the abstract, I would suggest revisiting it to clarify further for the reader what they should take away from it.

We changed the text with: ". A detailed analysis of the heating rate profiles shows that within the smoke layer at 4 km, the absorbing aerosols are 90% responsible for warming the ambient air with approximately 5.7 K/day. The accompanying water vapor however, has a longwave effect at distance on the cloud top, reducing its radiative cooling in the first 100 m by approximately 4.7 K/day (equivalent to 7%). We infer that this decreased cloud-top cooling in particular, in addition with the higher humidity above the clouds, might modify the cloud-top entrainment rate and its effect, leading to thicker clouds."

2. At the top of p.12, the last sentence in the top paragraph mentions that fuel moisture can make a significant contribution to the humidity within an aerosol plume. A back-of-the-envelope calculation based on Parmar et al., 2008, will contradict this assessment. I encourage the authors to explore this, as this has ramifications for the modelling of the moisture transport.

Potter (2005) suspected evidence of a contribution and proposed the need to determine how much moisture a fire adds to the air and whether this amount is or is not important. Clements et al. (2006) presented an experimental study, from which he deduced a confirmation of Potter's (2005) argument that water vapor from a wildland or grass fire can significantly modify the atmospheric dynamics. Parmar et al., 2008 conducted 16 combustion experiments of different types of biomass fires. His premise was that water vapor released from biomass burning may have different sources: 1) the production of H_2O by chemical reaction and 2) release of moisture that is not chemically bound to the organic molecules of the fuel. His results showed that non-bound biomass burning moisture ranges from 33% in dry African hardwood to 220 % in the fresh pine branches with needles. They suggest that fuel moisture can make a significant contribution to the water vapour content of fire plumes, but recognize that their study lacks measurements of water vapor release from biomass burning under field conditions, hence it is not possible to constrain their results and the modelling of moisture transport.

In our article we do not affirm that the water vapor can make "a significant contribution to the humidity within the aerosol plume". We merely state that previous studies show that fresh biomass can indeed release water vapor in the atmosphere. Our data show a clear covariance between the water vapor and the aerosol loading, that might be due to a specific regional pattern of circulation (that we show and discuss; also reference Adebiyi et al, 2015). We cannot disregard the possibility of water vapor and biomass burning aerosols released simultaneously from combustion processes, and the effect of these fire plumes on dynamics and moisture transport. Indeed, however, since we do not have the means to prove this statement, we are only suggesting a hypothesis from where the observed moisture comes from.

For clarification we modified the text as following:
Several studies (Potter, 2005, Clements et al., 2006; Parmar et al., 2008) suggest that depending on the moisture content of fresh biomass, the natural or anthropogenic biomass fires are releasing water vapour in the atmosphere (in addition to organic and black carbon, CO_2 and CO (Levine, 1990)), that can influence the atmospheric dynamics, thus moisture transport. Without measurements of water vapour release from biomass burning in field conditions, however, it is difficult to constrain the effect on water vapor transport. It might be important to account for the effect of this accompanying moisture, and to identify the different air circulation patterns that will lead the biomass-burning transportation off coast of South Africa.

3. Are there any systematic changes in some of your findings, Fig 10 and Fig. 11 in particular, as a function of month (JJA)? Fig. 6 shows that there is a seasonal change happening in the winds during these months, consistent with a strengthening of the temperature gradient between southern Africa and the Congo region (e.g., Adebiyi and Zuidema, 2016). One might or might not expect other relationships to also change and it could inform the LWP-qv discussion. I believe the authors have broken down their study by month already so I'm just looking for a short textual assessment here.

We have indeed broken down our study by month, but haven't looked at every month individually. Unfortunately, we haven't checked the relationship between LWP and absorption AOD for September and October. It was discussed at one point, but because of the limited number of 'low' situations in this period, we considered these data are not sufficiently statistically significant and we haven't pursued it further. It can still be done, but not in the time I have allocated now for this paper.
In order to clarify in the text, we added in the conclusions "Our results confirm previous satellite observations and studies that showed that clouds contain more water and are at slightly lower altitudes when large loads of absorbing aerosols are located above them. Indeed, we observed a significant increase of LWP between *low* and *high* cases, whatever the

meteorological conditions (**Error! Reference source not found.** and **Error! Reference source not found.**). These results are valid for June-August, since we have selected one meteorological regime with a similar number of *high* and *low* situations. We do not ignore the possibility that different results may be found in another period (such as September-October), characterised by different meteorological conditions and aerosol emissions.”

4. the abstract mentions "the region we focus on" in the 3rd paragraph. be explicit, also about the time period. It adds interest to your study, as no study to date has focused on that particular region.

Thank you. We modified the text with: During this analysis, we account for the variation in the meteorological conditions over our sample area, by selecting the months associated to one meteorological regime (June-August).