

Interactive comment on “Effect of biomass burning on marine stratocumulus clouds off the California coast” by J. Brioude et al.

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We would like to thank the reviewer for her/his time and effort and appreciate her/his help to improve our paper.

This paper blends satellite data with a transport model for several aerosols types in order to deduce the radiative impact of indirect effect specifically attributable to biomass burning aerosols. The novel aspect of this paper is the use of a continental tracer that is separate from the biomass burning aerosol tracer in the model in order to separate satellite cloud properties specifically impacted by biomass burning aerosol from cloud properties impacted by other continental influences. The authors further sort cases into low/high humidity, low/high static stability, and low/high surface temperature. This is a valuable contribu-

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tion and illustrates an approach that adds new information to the discussion of aerosol/cloud interaction in marine stratocumulus clouds. In general, however, I found it quite difficult to trace the quantitative results reported in the abstract to the methods described in the text. I also had difficulty following the explanation of how the meteorological contribution was isolated, which made it difficult for me to evaluate authors' conclusion that the meteorological bias is small.

The abstract states of the effect near the coast, “the combined effect of an indirect radiative forcing of -7.45% on average with a bias due to meteorology of +0.89%.” Is this per cent of the average value? Can this be reported in Wm^{-2} ? And I do not follow where the 0.89% meteorological bias comes from. Does the 0.89% follow from the multi-variate regression analysis? If so, I could not follow the connection of this quantity to the discussion of the regression analysis.

=> -7.45% is the change of the the outgoing radiative flux at the top of the atmosphere due to BB aerosol indirect effect. We cannot report the value in W/m^2 because we need to calculate T (the transmittance) to know exactly the outgoing radiative flux at the top of the atmosphere. We need a good representation of the aerosol background, and a radiative model for that. Furthermore, the values reported are valid at 14:00 local time only. In a future paper, we will calculate the value in Wm^{-2} and study the diurnal cycle of biomass burning aerosol indirect effect. We added 2 equations to explain more clearly how the bias is calculated from the multivariate regression.

In line 610 the authors argue that “the difference in specific humidity is small ... with an average difference of $+0.15gkg^{-1}$. The difference in LTS is small too”. Is $+0.15g km^{-1}$ for high biomass burning aerosol minus low? How big is this difference compared to the mean value, and is it really the case that high BB aerosol air masses are more humid than low BB aerosol air masses? This seems unlikely, at least near the coast. Is this true of continental air masses in general? Or just BB laden air masses? If the methodology employed in this study could demonstrate that continental air masses in general are not sufficiently different

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from maritime air masses in terms of humidity and LTS to meaningfully impact cloud properties, that would be a very useful result.

=> Yes, $+0.15\text{gkg}^{-1}$ means a slightly higher humidity associated with the presence of BB aerosols. As stated in the text, the mean value is shown on figure 4. The average value is about 7.5g/kg . So the difference is relatively small. However, the fact that the humidity is higher in presence of BB aerosols doesn't necessarily mean "cause and effect" between the two. The humidity comes from the NCEP GFS model, and the BB aerosol tracer comes from the FLEXPART model. It is unlikely that the data assimilation in the NCEP GFS model takes into account changes in humidity and temperature due to biomass burning plumes because of sparse in-situ measurements over the ocean. So meteorological differences found in presence or absence of BB aerosols is probably the consequence of slight differences in transport pattern when offshore BB aerosols are observed.

We focused our study on offshore continental tracers only, to avoid changes in aerosol signature between marine and continental air. So we can't demonstrate anything concerning differences between marine and continental airmasses.

On line 564 it states: "On average, the highest impacts of biomass burning on cloud are found at high humidity and low LTS. High humidity promotes greater cloud fraction and thus larger differences in cloud fraction can occur in the presence of BB aerosol." The logic of the second sentence is not clear to me. Why does enhancing the LTS with BB aerosol above the boundary necessary lead to a greater increase in cloud cover when the humidity and cloud cover are already high?

=> An increase of LTS reduces the vertical entrainment of dry air from above in the marine boundary layer (MBL). If the air in the MBL is dry, the cloud fraction is small on average. Changes in vertical entrainment of dry air will not change significantly the cloud properties (if a cloud exists) because clouds are already sparse. However, if

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the air is humid in the MBL, the cloud fraction is statistically larger, because the water vapor can saturate over larger regions. Changes in LTS will modify locally the vertical entrainment of dry air, and then can promote the formation of a cloud (because the humidity is already high enough) or suppress an existing cloud by reducing the water vapor content. So it makes sense that larger differences occur when the humidity is higher.

Finally, the authors use MODIS mass concentration product to convert the arbitrary units of the tracer concentration from the model to realistic values of biomass burning concentration. MODIS, of course, does not measure mass concentration, it retrieves aerosol optical depth and then makes a host of additional assumptions to report mass concentration. Are there published values for the estimated uncertainty of this product? If, indeed, there is a substantial random error in the MODIS mass concentration product (which I am assuming is the case) would that translate into random error in the biomass burning concentration used to separate clean from polluted clouds in the analysis? The authors should address the errors in the MODIS data and discuss any impacts it might have on their conclusions.

=> Yes, errors in the MODIS aerosol mass concentration product affect the conversion of FLEXPART passive tracers with arbitrary concentration unit into a realistic concentration unit (see Figure 2). The results in this paper are calculated in the presence or absence of BB aerosols. Errors on BB aerosol change the mass distribution of the tracer, but don't change the fact that BB aerosols exist or not. So errors on BB tracers don't change the results. However, errors on the continental tracer mass distribution can modify the way that the subdivisions of the dataset are made, and can potentially modify the results on changes in cloud properties due to BB aerosols. However, In table 1 we have shown that by using the continental tracer, high continental tracer load only or anthropogenic tracer to subdivide the dataset, the differences in cloud fraction and cloud albedo are not significant. So we expect an uncertainty of the results due to

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uncertainty in MODIS aerosol mass concentration within the uncertainties described in Table 1, which fall within the range of the standard deviation. We will add a comment in the text.

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