Reply to comments by reviewers of the ACPD manuscript 'OMI Observations of the 2008 biomass burning season'

The main comment by all reviewers of the manuscript stated that the paper did not include evidence to support the claim in the manuscript that the observed reduced fire activity was probably the result of the enforcement on existing regulations on biomass burning, and suggested additional data analysis to add confidence to the reported results. We have improved the manuscript by carrying out a more detailed analysis of the linkage between precipitation and fire activity, using fire-counts and satellite measurements of aerosol load as proxies of biomass burning. The additional analysis confirmed the assertion in the original version of the paper that the low level of fire activity in 2008 in South America cannot easily be explained in terms of precipitation effects alone. This result, therefore, suggests that non-meteorological factors may have also played a role. In the discussion of results we suggest possible economic or regulatory factors that may have been important but we can not conclusively point to any particular non-meteorological reason. We thank all the reviewers for their constructive criticism and suggestions most of which have been integrated in the revised version of the manuscript. Responses to reviewer comments are given below in bold typeface.

Reply to reviewer 1:

'In the discussion, the authors link the 2008 anomaly to the governmental regulations by the Brazilian government. Without any ground truth or additional information, this is quite a claim. This claim implies that space borne observations can be used to verify policy. I think the authors should tune down this claim quite a bit or should come with additional information (e.g. deforestation inventories, etc.). Today, I heard in the news that Brazilian deforestation indeed declines. The reason mentioned was not regulation, but the dropping food price.'

As suggested by the reviewer we have revisited the discussion section of the paper to qualify the main conclusion regarding the possible role of regulatory and/or economic factors in the reduction in 2008 fire activity. The role of governmental regulations is still mentioned as a possibility.

'Another argument that calls for a down-tuning of the government-driven connection is the fact that biomass burning was low in 2006. Indeed, it was claimed (Koren et al., 2007) that this indicated a trend-reversal of biomass burning in the Amazon. Unfortunately, 2007 turned out to be a year with quite extreme burning. This paper is quoted in the introduction, but not discussed further on in the paper.
I think the paper would improve with a more balanced discussion that mentions the possibility that regulations caused the lower amount of fires in 2008. But other explanation cannot be ruled out, see the 2006 case. It would be even better to include some ground truth, but I am not sure if these data are available. Also, a first indication of the 2009 situation (again low?) would support the claim.'

We have expanded the scope of the paper to include in the analysis the 2009 observations. As it turned out, fire activity in 2009 was even less intense than in 2008. Unlike in the 2008 season, however, the low incidence of fires in 2009 can be explained as a result of an unusually active rainy season that produced the highest rainfall amounts over the last ten years, even during the August-September period when most of the South American burning takes place. The precipitation anomaly plot and discussion included in the reply to the editor's comments addresses the issues of the 2006 and 2007 seasons.

2 Minor points
page 21510: line 10: I would replace 'understand' by 'analyse'

Done

page 21510, line 22: 'and other substance .....precursors'. This suggest that NOx and CO that were mentioned earlier are no ozone precursors. Rephrase.

Done

page 21511, line 3: spreads south. I would use: progresses southward.

Done

page 21511, line 6. The Labonne paper (Labonne et al. Injection height of biomass burning aerosols as seen from a spaceborne lidar. Geophysical Research Letters (2007) vol. 34 (11)) using CALIOP data shows that aerosols remain in the boundary layer over land. This issue is not resolved, but proper referencing is needed.

Done

page 21511, line 21: It is not made clear why MODIS AOT data are not used in this paper.

The revised version of the paper makes use of the MODIS aerosol optical depth data. The main satellite aerosol product used in the analysis is still the OMI aerosol absorption optical depth. As explained in the manuscript we rely on the OMI AAOD to uniquely identify the absorbing carbonaceous aerosols resulting from the biomass burning activity without the possible interference of aerosols from other sources.

page 21513, line 7. The authors make a direct connection between UVAI and biomass burning. This should be further substantiated. The connection is vital for the conclusions. There are more papers around that can help to make this connection (which is probably strong). E.g. Hoelzemann et al. Regional representativity of AERONET observation sites during the biomass burning season in South America determined by correlation studies with MODIS Aerosol Optical Depth. Journal of Geophysical

Research-Atmospheres (2009) vol. 114 pp. D13301.

The relationship between AOD and aerosol index has been amply documented in the literature. The comprehensive analysis by Hsu et al [JGR, 1999] has been added as reference.

page 21513, line 10. The OMI biomass burning record only covers 4 years!

The AI analysis has been presented in terms of the combined 24-year AI record. Year 2009 has also been included in the analysis

page 21514, line 12. 'dramatic'. Again, do not exaggerate. From figure 3 it is clear that a drop in biomass burning activities is not uncommon in South America.

The statement has been rephrased.

page 21514, line 19: 'twice as long'. I do not see this. Longer OK, but twice as long?

It is about twice as long. We stand by that statement.

page 21515, line 5-13. Here it is suggested that the reduction in the fires follows political boundaries. But what happened in 2006? The argument can also be reversed by saying that the 2007 fires were anomalously high in Brazil.

The analysis of the precipitation record in terms of rainfall anomaly actually supports the idea that the low 2006 activity may have been linked to a precipitation increase where as the 2007 anomalously high fire activity was the result of an extended drought. This analysis is included in the revised version of the manuscript.

To substantiate this claim some additional information should be given. What did the Brazilian government do to prevent fires? And are there data available that monitor deforestation.

Unfortunately there is no information on what might have been done by the Brazilian government to prevent fires. Data on annual rate of deforestation is available from Brazilian authorities. No correlation between deforestation rate and biomass burning activity is observed because land clearing by fire is just one of several deforestation mechanisms that include logging and public works for economic development.

From space, we have products like burned area, and other land-use change products. These should show substantial changes in deforestation between 2007 and 2008 if the claim is correct.

As mentioned in the paper deforestation actually increased by 11% in 2008 interrupting a decreasing trend since 2005. This fact underscores the fact that deforestation is not necessarily associated with fires.

And what about the 2009 biomass burning season. It would help very much to get a first impression of the 2009 fires (again low?).

The analysis of the 2009 data shows indeed a larger decline than in 2008 in burning activity. Increased levels of precipitation, however, can explain the decline.

page 21516, line 3: Indeed the wind speed increases from 2005-2008, but it seems more logical to explain this as a variability in the system.

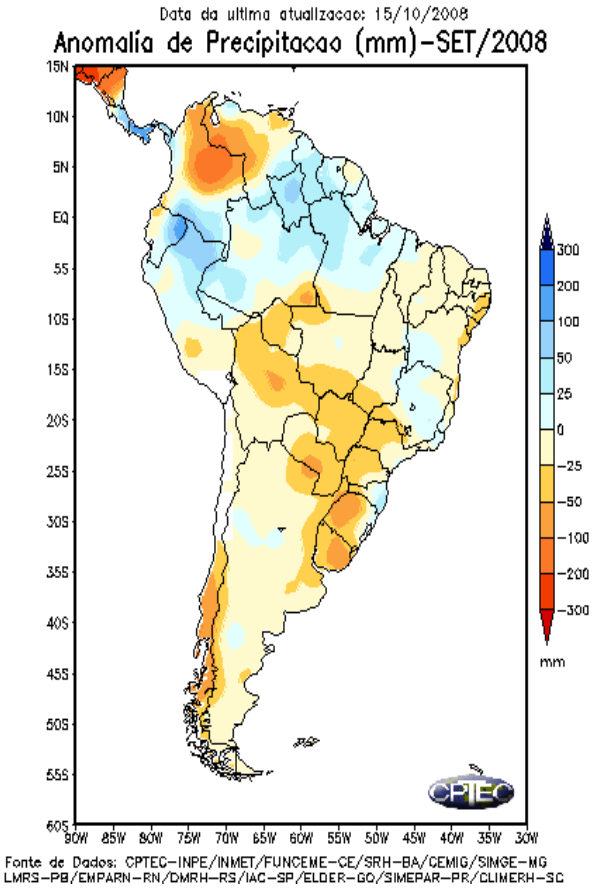
We agree.

page 21516, line 10 and further. I do not think all the possibilities have been explored. And what about the role of clouds in masking fires and smoke?

Not likely, OMI observations are sensitive to smoke aerosol cloud mixtures and smoke above clouds.

At least, a regional analysis of the rainfall pattern should be made. Now only a bulk rainfall analysis of south America is presented. Winds were stronger over the atlantic ocean, and could have brought more rain over the East of Brazil.

We looked into this. The attached map shows the regional September 2008 rainfall anomaly. The northeast region were most of the decrease in fire activity was not affected by significant precipitation. On the contrary, most of the fire-prone region registered a precipitation deficit relative to climatology, which should have resulted in increased fire activity. Regional anomaly is discussed in the manuscript but the map is not included due to space considerations.



It is claimed that political boundaries are followed in figure 5, but this is not very clear to me. The largest reduction in fires is seen in the North-East part of Brazil. Surprisingly enough, figure 2 shows a maximum in the OMI aerosol layer in that area, and not over the area where the fires increased in 2008.

Unfortunately the graphic representation is not as clear. For that reason we have quantified numerically the observed changes.

So, the link between fires and aerosols seems less robust than is suggested. To claim (l 25) that the reduction in fires was the result of governmental regulations is in my opinion a step too far. Ground truth, documentation of governmental actions, and additional regional analysis are all needed to further substantiate this claim.

We are actually downplaying that conclusion. The new analysis supports the fact that the observed decrease can't be due to increased precipitation. Therefore, we argued that other factors may have been important but we do not have any ways to offer proof.

page 21517, line 6: I think variability and trend are mixed up here. To call the 4-year time series (5.5., 5.5, 6.7, 7.0) a trend seems a bit odd. It is part of the normal variability of the circulation patterns.

We agree. The statement has been rephrased.

Reply to reviewer 2

Indeed the question of the impact of policy and enforcement on biomass burning trends in the Amazon is very important. The paper shows convincing evidences to a significant reduction in the Amazonian biomass burning during 2008. They show that while the trends in Africa and over the Atlantic are positive with increasing AI, there is a clear reduction in number of fires and in the aerosol loading over the Amazon. They suggest that since meteorology can not explain this trend it should be due to anthropogenic. It would be very important to know if indeed this is due to policy and enforcement but to really convince that this is the main reason for the trend more evidences are needed.

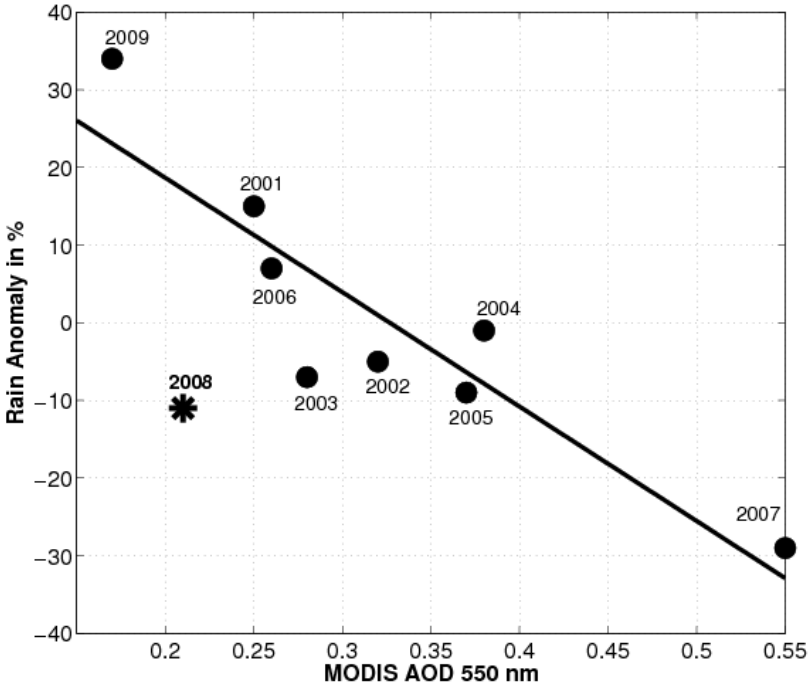
In the revised version of the paper we are downplaying the assertion that the observed anomaly in 2008 is associated with policy and enforcement. We are actually making the case that year 2008 was essentially different in the context of the historical record that shows a well defined link between precipitation and fire activity. As for the possible causes of that departure, we are just suggesting the possible role of economic and/or government regulations. We can not offer any quantifiable proof of these possible effects.

The authors make the point that they can not see any other reason for this trend. I think that this is good and interesting but adding more information to this would make it better. I would encourage the authors to look for more evidences for this explanation either from the Brazilian INPE reports or form other studies on the topic. During the last year there were many news items on this topic (like: http://news.bbc.co.uk/2/hi/8358094.stm) Although these are not scientific evidences it can, at least, show that the trends are viewed by others and to suggest possible explanations (enforcement vs. economics).

In the revised version we refer to possible economic/regulatory connections

Other comments:
1) The main source for data here is the OMI AI. While AI records can go back to 1979 to produce longer time series (Fig 3) it is not clear why the authors does not use the MODIS aerosol information to show the last years trends. Aerosol index depends on the aerosol height, the authors should mention this. Therefore not seeing it over the Amazon can be also due to smoke concentrating in the lower atmosphere. Showing that these trends are similar with the MODIS AOD ones (see the attached figure) would make the point stronger.

We have integrated the MODIS data in the analysis. The plot below illustrates the observed relationship between the 6-month (May-October) rainfall anomaly (in percent) and the spatially-averaged MODIS aerosol optical depth over the South American region used in the analysis for 9 years. The result illustrates that year 2008 departs significantly from the observed AOD–precipitation anomaly over 9 years. This plot has been included in the revised version of the manuscript.



2) To convince that the fire reduction has political borders it would be interesting to see also the fire count maps of 2007 and 2008 not only the differences between them.

Maps have been added as suggested.

Reply to reviewer 3

The manuscript provides an interesting analysis of the southern hemisphere distribution of biomass burning aerosols over South America and Africa. The distribution is anomalous for 2008 over South America and the South Atlantic ocean compared to the three years before, but not over Africa. The anomaly over South America is attributed to changes in soil management practices and/or enforcement of regulation.
The change over the South Atlantic is attributed to a more efficient transport due to stronger free tropospheric winds in 2008.

Although the presentation of the anomalies is convincing, the explanation for the change in biomaas burning in South America is not. The change in UVAI and fire counts are clearly coupled, and it is shown that they are not directly related to changes in precipitation over SA. This is presented as proof for the absence of a meteorological explanation for the anomaly. A map with the reduction of the 2008 fire counts compared to those in 2007 shows political boundaries of the Brazilian state Pará, which is very intriguing. However, I'm not convinced that this is proof for governmental enforcement of fire regulations.

In the revised version of the paper we are downplaying the assertion that governmental enforcement of fire regulations played a role. We are actually making the case that year 2008 was essentially different in the context of the historical record that shows a well defined link between precipitation and fire activity. As for the possible causes of that departure, we are just suggesting the possible role of economic and/or government regulations. We can not offer any quantifiable proof of these possible effects.

First, meteorological connections are dismissed by a rather simple comparison with local precipitation. Biomass burning will be influenced by precipitation, clearly, but also by the availability of biomass and maybe other factors. The availability of biomass can be affected by precipitation, temperature and other climatological factors during the growth seasons, months before the actual burning. Therefore, a climatological or meteorological connection for the anomalous 2008 SA biomass burning is not ruled out by the comparison with precipitation alone.

Precipitation is the meteorological parameter most clearly linked to fire activity. In the revised version of the manuscript we showed that the inter-annual rainfall variability seems to adequately explain most of the observed variability in fire activity. Year 2008, however, remains at odds in relation to the precipitation connection.

Furthermore and more importantly, the biomass burning season for 2008 over SA is anomalous when compared to 2005-2007, but not when compared to the period 1978-

2007, as shown convincingly by the long term record of the AI. Years with a low maximum of AI during the biomass burning season are quite common, which is rather intriguing. It is not likely that fire regulations are enforced irregularly during those years. A climatological connection would be more probable.

We agree, but year 2008 is especially intriguing.

The political boundaries that show up in the fire count maps are interesting, but some proof of the increased enforcement of fire regulation in the states bordering pará should be presented to convince me of the reason for the reduced fire activity as presented in the manuscript.

See reply to first comment.

These issues should be addressed to improve the scientific quality of the paper.

Minor comments:
p 21510, l 26: Norwest Paraguay -> Northwest Paraguay

Done

p 21514, l 1: Middle -> middle

Done

p 21514, l 1: Fig. 2 -> Fig. 4

Done

p 21515, l 8: Matto -> Mato ?

Done

p 21515, l 8: It seems to me that the reduced fire activity is in all states bordering the south-east boundary of Pará. An indication of the state boundaries may help, also to show more convincingly the relationship between the observed reduction in fire activity and political boundaries.

State borders have been added.

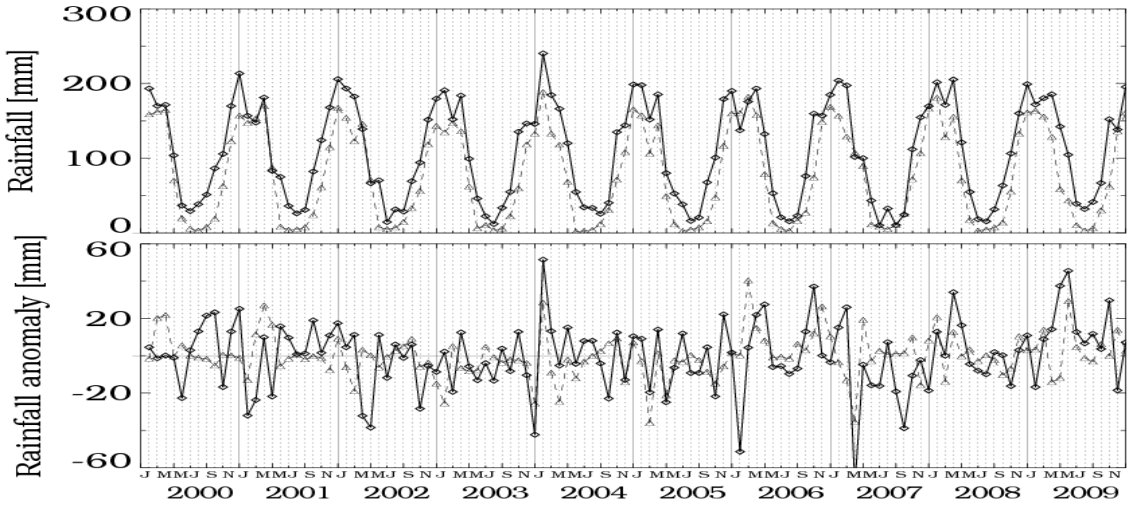
Reply to Editor:

This manuscript deals with a very interesting and important topic. Also the manuscript is clearly written. Nonetheless I have the following major concerns which need to be addressed :

Main concerns:

- I find the proof (figure 4) to totally disregard meteorological conditions in SA as an explanation (in part) of the observed 2008 anomaly is rather weak. One should possibly better plot the anomalies in precipitation (and not just the precipitation). For example one would see that for the month with highest fire counts and AAOD (Sept. SA), so the peak in BB, the precipitation is clearly higher in 2006 and 2008 compared to 2007. Also the month before the peak is rather wet in 2008. This may suggest that meteorological factors are important (or at least not negligible) wrt interannual variations in BB in SA in these years. So how important is changes in precipitation in specific months ? This is not really highlighted with Fig.4.

As suggested we have plotted the precipitation anomaly in relation to the 10-year climatological mean (2000-2009). The anomaly analysis clearly highlights the role of precipitation on the overall inter-annual variability of fire activity in South America. For instance the clearly prevailing wet conditions in 2006 seem to explain the reduction in fire activity on that year.



The anomaly plot also shows the magnitude of the severe drought that in 2007 affected the SA region (a 30% precipitation deficit over the May-October period), very likely linked to the highest level of fire activity and aerosol load over the last 10 years. The opposite effect of precipitation is observed in 2009 when a ten-year record on rainfall amount (a 6-month 34% precipitation excess) seems to be related to the lowest level of fires and aerosol optical depth over a decade. The rainfall anomaly, however, does not explain the 2008 low levels of fire-activity and aerosol production. In terms of precipitation, year 2008 with an 11% precipitation deficit is

not significantly different than the 9% deficit registered in 2005. September (month of peak fire activity in SA) is actually slightly drier in 2008 than in 2005. Yet, 2008 was significantly less active in fires and aerosol production than 2005. The plot above has been included in the revised manuscript.

In this respect it would also be useful to see the spatial distribution of the precipitation anomalies in fig.5.

A map showing the spatial distribution of the anomaly in Sept 2008 is included in this report in reply to a similar comment by reviewer 1.

- Proof for the claimed causes for the 2008 anomaly is missing. Some evidence is needed to back-up the statement that "is very likely the result of the implementation of national and regional governmental regulations" (Discussion). Which regulations are these ? When and where have they been imposed, and what prove is there to show they actually were implemented on such a scale that could cause this large impact ?

In the revised version of the paper we are downplaying that assertion. We are actually making the case that year 2008 was essentially different in the context of the historical record that showed a well defined link between precipitation and fire activity. As for the possible causes of that departure, we are just suggesting the possible role of economic and/or government regulations. We can not offer any quantifiable proof of these possible effects.

In fact in Koren et al., 2007 they claimed the 2006 low BB was a result of regulations implemented in 2006. So are the authors referring to different regulations, and what happened then in 2007 wrt these regulations ?

See above.

Please also note the discussion/debate in literature regarding the 2006 'anomaly', Koren et al., 2007, Schroeder et al., 2009, Koren et al, 2009. From this discussion it is clear that one must be careful in attributing causes to changes in BB. Clear evidence is needed - possibly coming from different information/data sources- to back-up any claim wrt possible causes for changes in BB.

In the revised version we have made reference to the suggested literature on South American biomass burning.

- the manuscript focuses on 2008 as an anomalous year regarding Biomass Burning, whereas it is obvious from figure 4 that 2006 can be considered anomalous as well in this respect. However, nothing is said concerning 2006 and how the explanations

brought forward for the anomalous 2008 can (or can not) also explain 2006. Given figure 4 (and figure 3) and the topic of this paper 2006 can not be ignored completely.

The anomaly analysis briefly discussed above addresses these issues. The revised version of the manuscript discusses the 2006, 2007, 2008 and 2009 biomass-burning seasons.

Only one reference is provided addressing causes for deforestation in Amazone. References to important publications on this topic are missing (for example : Morton et al., 2007; vd Werf et al., 2008; Malhi et al., 2008; Laurance, 2007; Schroeder et al, 2009).

We have included additional references in the literature review.

Also in respect to the previous point references to earlier work addressing 2006 BB in Amazone is missing (Schroeder et al., 2009; Koren et al, 2009; Gloudemans et al., 2009). I expect to find this type of information and references in the Introduction and Discussion section.

Added.

Technical corrections : - page 21514 : 1st line Figure 2 should be Figure 4.
- Page 21514 : line 8 with peaks in September and October for SA and CA –> CA and SA

Corrected.

- References to relevant earlier work on interannual variability of BB emissions using the TOMS-AI is missing (Duncan et al., 2003). Duncan et al., also found similar interannual variation in AAI for Central Africa and Brasil as shown here. Little variation in Central Africa, large interannual variation in Brasil.

Added.