

## ***Interactive comment on “Do biomass burning aerosols intensify drought in equatorial Asia during El Niño?” by M. G. Tosca et al.***

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This is a very well written paper and gets straight to the point. They ran CAM3 with high and low emissions cases and look at differences in the model. Model components are briefly yet adequately explained. Introduction lays out the problem very nicely. They are moderately careful with regard to some of their key conclusions. Not much to argue with in this context. In short, I wish more papers were written in this general format. I do have a number of issues on the observation end, but in the context of this simple simulation it is not worth getting into.

However, I do have some issues with the works broad interpretation. These I think are more for the official record in ACPD and less for the model-given the circumstances not much more than they can do. Since the 1997 El Nino event people have speculated on  
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the possible feedback between smoke and dynamics. I still remember as a graduate student at the University of Washington a conversation I had with Conway Leovy he immediately fingered possible emissions feedback through suppressed precipitation. But, it is a very tough problem to get at. As Tosca et al pointed out in the paper, regional emissions are partly phase locked with precipitation patterns and a clear tipping point exists with regard to surface water levels in the peat lands and fire prevalence. Our own work shows similar results of Tosca et al with the neutral and la nina years have similar fire characteristics whereas el nino has massive smoke production. But there in lies the problem. Global climate models have variable success reproducing el nino events. So if there is uncertainty in the model, how can one determine real feedback? If the authors want to go this route, then they have to isolate el nino cases versus normal and la nina cases from their 30 year runs. El Nino periods are already dry and they have to prove it in the model. El Nino impact then ends with the flip of the itcz. Did the model run suggest a delay or advance in this flip?

What they are really showing in this paper is not the enhancement el nino, but rather to local convection. Here lies another problem. The orographics of the maritime continent are brutal, and subject to many mesoscale flows. I am sure the Randerson's group is aware of this as half the staff was standing in front of my students paper last agu where she explained ti to them. Mesoscale models cannot simulate controlling orographic and MJO precipitation. So who much can we trust CAM3 at 2.5x2.5 degrees? Is a 10% decrease in modeled precipitation really something we should get excited about? It would be helpful if the authors spent an extra page demonstrating that they understand the nature of the meteorology of the region. This should then be linked back to in their discussions. -J.S. Reid

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