This paper presents an evaluation of the CO and O3 budgets in the Asian Summer Monsoon region, based on simulations using the GEOS-Chem model. About half of the paper is devoted to evaluating the model chemical climatology using IASI satellite and MOZAIC/IAGOS aircraft measurements, and the comparisons show reasonable seasonal and spatial chemical behavior for the model. It is good to see IASI constituent data being utilized, and the comparisons with the model are made using the appropriate averaging kernels. The model is then used to perform sensitivity tests to quantify source regions and chemical budgets for the monsoon UTLS region, with results focusing on CO, NOx and ozone production rates. The overall chemical budget calculations and results seem reasonable. Chemical behavior in the monsoon region is a topic of substantial current interest, and the results here contribute to understanding the details of the GEOS-Chem simulation and chemistry and transport in the real atmosphere. Overall the paper is reasonably well written and the authors have done a good job of including numerous references to previous work. I recommend this paper for publication in ACP, but have several comments for the authors to consider in revision.

1) One overarching comment is that I personally disliked papers that include numerous figures with many small panels ('postage stamps'), for which the reader is expected to scrutinize details in each of the panels. Figures 1,2,3,5,6,7,13 and 14 are such figures in this paper, showing detailed evolution of various diagnostics during May-October. I would recommend an alternative methodology of showing one or two key months in each of these figures, with enlarged scale to allow focus on the important details. The seasonal evolution can be described in words, and the entire sequence could be included in Supplementary material if necessary.

2) One detail that I don't understand regards the appearance of the 'S-shaped' ozone profile in the GCxAvK calculations, which don't appear in the GC model itself (Fig. 6). I don't understand this because the averaging kernels are broad in the vertical (6-8 km), and so how can they introduce narrow vertical structure into the weighted model results? Is this possibly due to the a priori profiles that are also used in the calculations?

3) Correlation coefficients are often quoted in comparing IASI vs. model results. Do these refer to spatial or temporal correlations?

4) There are numerous English errors in the text that should be corrected. Also, Fig. 10 is called out before Fig. 9.