

Interactive comment on “Development and implementation of a new biomass burning emissions injection height scheme for the GEOSChem model” by Liye Zhu et al.

Anonymous Referee #2

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This paper describes a MISR-based scheme for estimating the vertical distribution of biomass burning emissions in the GEOS-Chem model. Monthly gridded MISR injection heights from 2008 are used to develop the distribution for each month and each grid cell of a 2x2.5 degree GEOS-Chem grid. The paper demonstrates the impact that this new scheme has on GEOS-Chem predictions of CO and PAN over biomass burning regions, specifically Canada in July of 2008 (during the ARCTAS-B campaign) and over the Amazon in October of 2010 and 2011. They find that the new injection height scheme better matches observations of the vertical profile of PAN during ARCTAS-B and the vertical profiles of CO during ARCTAS-B and over the Amazon, as well as showing an improved match with NOAA ESRL surface observations of CO during 2008.

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The need for better approaches to simulating the injection height of biomass burning emissions is clear, and this work to develop an empirically-based approach based on satellite observations is an important advance in the field. The data used to demonstrate the impacts of the new scheme seem reasonable and allow comparisons with published results from other versions of the GEOS-Chem model. However, the manuscript is occasionally unclear and lacks key details in describing the new scheme, and the order of the discussion is sometimes repetitive and confusing. Thus, I recommend minor revisions to address the minor concerns and typos below.

Minor Concerns

L116-117, L148-150: I am not clear on how the MISR injection height data are converted into emitted percentages of biomass burning emissions on the GMAO grid at 2x2.5 degree resolution. Are all MISR heights in a given month and 2x2.5 degree grid box averaged together, weighted by their relative emissions? Is the land cover used to define that weighting? Since the final product is monthly on a 2x2.5 grid, what do the words “region” and “season” in L116-117 refer to? A little more description and possibly some equations would help to make the data processing clear.

L125-127: You mention the morning observation time of MISR as a limitation several times. What kind of errors do you expect this limitation would have on the model results? Have you considered any potential correction for this limitation, such as applying a normalized diurnal cycle to the MISR observations?

L130-131: My understanding is that even if the fires are smaller than a pixel, they can be detected if they have a sufficient impact on the brightness temperature of the pixel, so while some fires are too small to detect, not all fires smaller than a MODIS pixel are missed. Is that correct? If so, this sentence needs to be revised.

L136-137: Can you make a case that the Randerson et al. (2012) approach for small fires is likely to be accurate? Why did you pick this approach?

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L139: There is a lot of material left for the Val Martin and Kahn (in prep) paper – I'd suggest adding a little more detail from that reference here as it will be hard for future readers to track down the other paper without a reference.

L153: You might want to mention here why you chose this version – I think it's because it is what was used in Fischer et al. (2014), as stated on L175, but it should be made clear here at the first mention of the version.

L168-170: The phrase “As discussed in the preceding and following sections” is not very helpful in finding where the discussion is. Perhaps replace it with “As discussed in Section 2.2, since MODIS and MISR may not detect many small fires, . . .”

L181-182: You might want to clarify what you mean by “best” here – most chemically detailed, most accurate, most studied – and provide some brief evidence why this version of GEOS-Chem is the best for PAN in that sense.

L184-185: I don't understand how this sentence on standard input file settings and benchmark runs links with the previous sentence on the problems with evaluating monthly-average emissions with specific case studies. Do you mean that since most users will run using monthly-average biomass burning emissions, that's why you used that approach in this paper?

L197-199: Rather than refer back to a paper we can't read yet, I'd suggest referring back to Section 2.2, where you discuss this issue in a little more detail.

L200-220: The ordering of the sentences in this paragraph was very confusing to me. I think the point is to say that your approach in this paper follows Fischer et al. (2014) with exceptions, and then to list those exceptions. However, you start by saying what you removed from Fischer et al. (2014), then explain why Fischer et al. (2014) is your “standard” model, then you introduce a new CO emission factor approach, then state the horizontal resolution of the model. I think this could be made much clearer by rearranging the content.

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L239-241: Is there some reason it is important to mention that everything was synchronized to UTC? Wouldn't any other time zone work as well so long as you were consistent?

L252: How were the CO measurements over the Amazon performed?

L254-256: I'm not sure what this sentence means – how else would I get a vertical profile except by measuring at specific altitudes? Are you contrasting this with remote sensing approaches?

L257-266: Since you don't use the BARCA data later in the paper, I'd cut this part of the paragraph. However, you do use the NOAA ESRL data later (Section 3.3), so a discussion of those data should be included here.

L318: Why are you not showing the CO results for the 1 July flight as well?

L336: I think you need to explain why you did not pursue the daily or 3-hour emission approaches, and/or why you think the approach using data from Petrenko et al. (2017) is better.

L341-342: The match between 900 and 800 hPa looks like a coincidence to me, as the model profile is highest there but it is a local minimum in the observed profile. Can you argue that I should have more confidence in the match in that region?

L343-350: This "limitations and future work" paragraph would probably fit better in Section 4.

L358-359: For all of these "in prep" references, it would be good to either add more detail to this paper or provide a better reference, such as to a conference presentation if one exists.

L409: But there is a large negative change in Northwest Canadian Figure 6a, which does not seem consistent with you OH-based explanation. What is the cause of that feature?

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L425-430: Rather than referring to “most” stations with no change and “some” with decreases, can you be quantitative? How many sites had no change (and does this mean a change of less than 1 ppb? 10 ppb?), and how many had decreases? Did any have increases?

L467-469: You don’t present any comparisons to satellite observations in this paper, so this doesn’t really belong in your summary.

Typos and Style Suggestions

L98: Replace “this trace species” with “PAN” to be clear.

L134: I’d suggest that “account for” is closer to what you mean than “acknowledge” here.

L137: Extra “s” after “biome”

L330: Extra comma after “specific”

L352: I don’t think you need the word “above” as you mention the section number.

L371-372: You discuss the results in Figure 8c before introducing the figure. I’d cut the previous sentence to “. . .emissions pushed higher in the atmosphere.”

L394: I’m not sure “understand” is the right word here. Maybe “simulate”?

L398-399: Instead of referring to the “example in Figure 6”, I’d suggest describing it as “the 4 July smoke plume from ARCTAS (Fig. 6)”.

L412: Word “typically” is redundant with “Typical” at the beginning of the sentence.

L424: Extra space after “Network”

L463: I’d suggest changing this to “provided access to CO profiles that could be used for model-measurement comparison”

L464: “do not appear”, instead of “to not appear”

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