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Comment on egusphere-2022-21

Anonymous Referee #1

Referee comment on "Aerosol Optical Properties of Extreme Global Wildfires and Estimated Radiative Forcing with GCOM-C SGLI" by Kazuhisa Tanada et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-21-RC1, 2022

Review of "Aerosol Optical Properties of Extreme Global Wildfires and Estimated Radiative Forcing with GCOM-C SGLI" by Tanada et al.

The authors presented an analysis of "wildfire-emitted" aerosol optical properties retrieved from the GCOM-C SGLI satellite instrument and representative of six major biomass burning regions of the Earth. The manuscript further attempts to estimate the changes in TOA radiative forcing due to the extreme wildfire events during the 2018-21 period, separately for the six regions. There is no denying the significance of the motivation behind this work, along with the excitement of having a newer dataset available for constraining the aerosol properties in our global models.

In my opinion, however, the manuscript is unsuitable for publication in its current form. Major additional work and analysis are needed before resubmitting. Moreover, the findings are not revealing, and the discussions do not add a lot to our existing knowledge about aerosol optical properties. My most significant concern is that often too much is interpreted from the retrievals that are not thoroughly vetted. The flaws in the analysis are not helped by challenging readability (and some errors/inconsistencies). Please consider the points made below as grounds for this assessment.

Major comments:

- It is important to understand that satellite observations of AOD (and more importantly of derived properties such as SSA) are after all retrievals and require a thorough validation/evaluation, at least using the aerosol products from other existing passive sensors (such as MODIS/VIIRS) and ground-based observations from AERONET sun photometers wherever available. Unless there is an existing publication demonstrating the robustness and uncertainty limits of the data set (other than the ATBD document

that the authors point to, which is not peer-reviewed), authors should be cautious before using the retrievals for making detailed interpretations about aerosol properties.

- The title and manuscript suggest that the analysis focuses on the aerosol properties emitted from extreme wildfire events and the radiative impacts due to those. However, the claim is kind of misleading for the following reasons: (1) it is not very clear how are extreme wildfires detected? The authors do mention vaguely in section 2.1 that SGLI's WFRP research product is being used to detect the hotspot locations of wildfires, but what criterion is being used to distinguish between extreme wildfires or other fires is not explained. Also, it is well-known that satellite imagers have issues detecting the accurate magnitudes of fire radiative power when thick plumes of smoke emitted from the wildfire itself cover the fire locations, therefore the uncertainty in detecting extreme wildfires using this technique is not discussed. (2) Even though the focus is on the wildfire-emitted aerosols, throughout the manuscript (barring section 3.4) the aerosol properties are discussed in the context of monthly means (including the calculations of direct radiative forcing). So, considering Angola for example (and it might be true for other regions also), where August month sees significant biomass burning from savannas and grasslands as well, how are the authors secluding the impact of aerosols emitted from wildfires only?
- Changes in SSA and AE with respect to relative humidity and vegetation: the entire section 3.3. discusses how SSA increases and AE decreases with increasing relative humidity. Firstly, the analysis does not add anything new to our current understanding because it is well understood in the literature that swelling of particles leads to enhanced scattering and hence higher SSA. Similarly, AE would obviously be lower for swollen/humidity-grown particles. Another conclusion from this analysis is that SSA varies depending on vegetation types, which again I think is well known and which is why models consider different emission factors for emission constituents depending on different biomes (e.g., boreal forests versus tropical forests, see Akagi et al. (2011)). In fact, apart from vegetation, the differences in the composition (or SSA) of the freshly emitted aerosol mixture could also arise from the type of burning i.e., flaming versus smoldering, which is not even discussed here.
- Finally, the discussion on aerosol aging effects is again limited to the humification effects only. The section lacks a discussion on changes in SSA due to mixing and chemical processes, such as lensing (where BC is coated with organic material) or photobleaching (or photo-oxidation) of brown carbon. When considered together, the impacts on aerosol absorption might not be straightforward to infer based on mere relative humidity justifications.

Following are some examples of specific errors and inconsistencies:

Line 34-35: "The remaining 30 % are water, CO, and various particles (Council, 2004)". I don't think this statement is correct because apart from the things listed here, there are other GHG emitted such as methane, nitrous oxide, as well as volatile organic compounds (VOCs), that can act as precursors for secondary organic aerosols.

Line 39: "Organic carbon reflects solar radiation and causes a negative forcing." Again, not completely true because brown carbon (a subset of the organic carbon) has significant absorption in the near-UV wavelengths.

Line 44: what is the reference for the estimate of indirect forcing listed here, please clarify and if possible demonstrate the variability of these estimates based on more recent studies in the literature. Also, what about aerosol semi-direct effects?

Line 47: Not just particle size distribution and chemical composition, optical properties are also largely dependent on aerosol hygroscopicity and mixing state**.**

Line 48: This sentence is wrong, and the in-text citation is not even included in the bibliography. Most of the smoke particles are in fact dominated by fine or accumulation mode (Eck et al. 1999), while dust and sea salt would be coarse mode dominated.

Line 54: What is direct radiative effect efficiency? Please be consistent with the use of radiative forcing, direct radiative effect, indirect forcing, indirect effect, etc. in the text throughout. There are subtle differences between each of these definitions.

Line 71: It mentions that errors correspond to a 1 sigma confidence level throughout the manuscript, yet error depiction is missing for several critical figures such as Fig. 1 and 7.

Other discrepancies: It is not mentioned how the cloud presence/contamination is handled in the dataset? Unless we are only looking at clear-sky data, but if so, it's nowhere mentioned until we reach the discussions specific to radiative forcing calculations.

Fig. 9 shows Angola base-period/no fire period as July 2020 and Table 3. Suggests base period is 11-20 Sep 2020. Which one is it? Moreover, a quick look at MODIS/VIIRS observed fire pixels will tell you that there are enough fire pixels during both the base periods mentioned here.

References:

Eck, T. F., Holben, B. N., Reid, J. S., Dubovik, O., Smirnov, A., O'Neill, N. T., Slutsker, I., and Kinne, S.: Wavelength dependence of the optical depth of biomass burning, urban, and desert dust aerosols, J. Geophys. Res., 104, 31333–31349, 1999.

Akagi, S. K., Yokelson, R. J., Wiedinmyer, C., Alvarado, M. J., Reid, J. S., Karl, T., Crounse, J. D., and Wennberg, P. O.: Emission factors for open and domestic biomass burning for use in atmospheric models, Atmos. Chem. Phys., 11, 4039–4072, https://doi.org/10.5194/acp-11-4039-2011, 2011.