Response to Reviewer 1

I thank the reviewer for his/her constructive comments. My responses are detailed below. (Reviewer comments in italics).

1-21: Fires are obviously a source of CO2, but it would be good to add a statement on whether fires are a net source of CO2 to avoid confusion

This is an extremely complex subject that requires comprehensive earth system models to address and goes far beyond the scope of this paper, which is focusing on gross emissions. To give the reader some indication of the magnitude of fire contributions to atmospheric CO_2, I have included the following sentences: "While a significant fraction of the emitted CO_2 is taken up again by vegetation regrowth, much of it remains in the atmosphere for years and potentially even up to centuries, e.g., in the case of tropical deforestation fires or peat soil burning (van der Werf et al., 2017). Model simulations suggest that in the absence of fires, atmospheric CO_2 concentrations would be about 40 ppm lower, indicating the importance of fires for the atmospheric carbon budget (Ward et al., 2012)."

2-10: The Johnston et al paper estimated 339,000 annual premature deaths, the number mentioned here is an interquartile range.

The original text says "…accounting for <u>as much as</u> 600,000 premature deaths per year globally", indicating that this number is an upper limit. It is not the interquartile range, which would be the difference between upper and lower quartile. In response to the reviewer, and to make things fully transparent, I have changed the text to "…accounting for up to 600,000 premature deaths per year globally (75th percentile of model estimates; Johnston et al., 2012)".

2-22: Please specify the units, C or DM? Also, a range of estimates is not necessarily the same as uncertainty, please see final point below

The text here does not discuss biomass or carbon burned, which could be expressed as C or DM. Rather, it discusses carbon released, which could be expressed as C or CO_2. However, the sentence here is completely unambiguous: "…the estimates in these studies of the annual amounts of <u>**carbon**</u> released still range over a factor of three from 1.5 to 4.7 Pg a^{-1}." The rules of the SI system explicitly forbid including the use of constructs such as "4.7 Pg C a^{-1}", and suggest including this information in the text, as I have done here.

I did not state that the estimate range is the same as uncertainty. Rather, in the introductory sentence to this paragraph I state "… large uncertainties persist regarding the amounts emitted…". This is illustrated in the following text by wide range of current estimates, which I feel clearly supports the existence of large uncertainties, without equating the range of estimates with the range of uncertainty.

2-34: To some degree this differentiation was also done by Akagi et al. (2011), would be good to credit them

Sentence on 2-34 modified to "…the other by Akagi et al. (2011), which included more recent data and additional species and burning types..."

3-19: I applaud using top-down constraint, but it also makes for blurring the distinction between bottom-up and top-down measurements. For example, it is widely accepted that there is about a factor 3 difference in AOD calculations based on bottom-up and top-down methods (e.g., Kaiser et al., 2012), merging both approaches may hide this issue and thus requires a bit more information on whether and when merging these estimates is appropriate. Also, the author talks about 'appropriate correction methods' but this is not further specified as far as I can see.

It was not an easy decision for me whether to include these data from remote sensing or not. In the end, I felt that most users of my compilation would be better served if this information was included, since it provided large-scale coverage and data for some species poorly sampled by other techniques, e.g., COS. Users who do not want these data included for particular reasons can easily eliminate them by using the spreadsheet in the supplement and removing the corresponding lines. I did not include any aerosol data in these estimates, as I do not feel that there is any way to address post-emission changes to reconstruct aerosol emission data from large-scale AOD measurements. The data included are only for species that are either long-lived on the time/space scales in question (e.g., COS and HCN), or for which emissions can be reconstructed with some confidence by the use of chemical modeling, e.g., ethane. The "appropriate methods" are actually already discussed in the preceding sentence "…included a correction for atmospheric transformations, using model calculations involving transport times and reaction rates of the species concerned." I have made this more explicit by changing the sentence to: "…appropriate correction methods (i.e., chemistry-transport model calculations to correct for atmospheric transformations)". Readers interested in the details for each paper would have to go back to the original papers, as going into specifics here would not be of interest to most readers.

One of my main questions is to what degree the approach of this paper ("Ideally, these measurements had been made within minutes after the smoke was released from the fires") differs from that of Akagi ("smoke that has cooled to ambient temperature, but not yet undergone significant photochemical processing"). What does that mean for the number of studies included, what does it mean for the average values, to what degree do ground-based studies (which in general include the smoldering phase) differ from airborne studies which may be biased towards flaming combustion with more pyroconvection, etc? The latter is mentioned in the text (e.g. 6-22) but in the end all measurements are averaged. In general, the modellers which will ultimately use this dataset need to know whether and why these numbers are different from the numbers being used so far. This is a key question but not addressed at all and a table that addresses these differences and potential causes for the most frequently used species would be welcomed

In the end, what both Akagi et al. and I want to represent is “fresh smoke”, i.e., the material that is emitted by the fire and that can serve as starting point for model calculations at scales typically used by a variety of models. In that sense, there is no difference between my approach and Akagi’s. Ideally, one would have an objective criterion, such as Akagi’s “cooled to ambient temperature”, but in reality I had to look at the data in some 400 papers, and had to decide for each species and each set of measurements whether in my judgement they represented “fresh smoke”. This led to the exclusion of numerous studies, of some shorter-lived species from some studies from which somewhat longer-lived species could be included, and the exclusion of some flights or samplings from studies where both fresh and somewhat aged plumes had been investigated. I don’t think there is any reasonable way to discuss these issues for each of 370 papers (now included in the revised version) and 121 species. In the end, I can only ask the user of these data to trust my judgement, gained over working on this subject for almost 40 years, about which data to include. Averaging over as many data (that are judged to be valid) as possible should reduce biases that result from the inclusion of any atypical data.

The issue of airborne vs. ground-based studies has been discussed extensively in the literature, including A&M2001, A2011, Burling et al. (2011), and briefly in this paper. I did not feel that there is anything new that I could contribute here other than highlighting the problems resulting from this issue. In response to the reviewer, I added the reference to Burling et al. (2011) on p. 5 and a sentence to the last paragraph of section 3.2: “A representative measurement of fire-average $\Delta CO/\Delta CO_2$ emission ratios from large forest fires is very difficult if not impossible, as ground-based measurements in such violent fires are not possible and aircraft measurements are prone to undersampling the smoldering emissions, especially the contributions from RSC.”

8-26: This is a somewhat surprising statement to me. Differences in bottom-up and top-down results can originate from uncertainty in many parameters, emission factors being one of them. The standard deviation of CO in boreal and temperate forests is relatively speaking not that much larger than in savannas which to me is not surprising given the large variability in moisture regimes and tree species and density in those forests. I feel it would be more useful to analyze whether there is a difference in ground and airborne studies to say something about RSC.

I’m not quite sure what the reviewer is getting at here. In response, I have changed “…which may be responsible for…” to “… which may significantly contribute to…”. The role of RSC is highlighted in the new sentence added: “A representative measurement of fire-average $\Delta CO/\Delta CO_2$ emission ratios from large forest fires is very difficult if not impossible, as ground-based measurements in such violent fires are not possible and aircraft measurements are prone to undersampling the smoldering emissions, especially the contributions from RSC.” Regrettably, I am not able to propose a simple solution to this problem.

My other main point of criticism relates to Table 2 and the statement in the conclusions that the uncertainty in biomass burning emissions nowadays is as large as in the 2000s. Table 2 shows various estimates and the large range stems for a substantial part from outliers such as

FLAMBE which predict 10 times higher emissions in tropical forests compared to savannas, totally different from for example GFED4 and GFAS1.2 (derived from GFED3). I understand that it is beyond the scope of this paper to assess which one is right but this deserves some explanation, for example using previously mentioned top-down estimates based on CO. Simply combining the 8750 Tg DM in tropical forests from FLAMBE and the CO emission factor (105 g CO per kg DM) indicates CO emissions from this biome alone of 900 Tg CO per year, something not corroborated by inverse estimates and also at odds with the best estimates of deforestation (e.g., Houghton and Nassikas, 2017, https://doi.org/10.1002/2016GB005546).

My point here is to highlight a problem, not to analyze the validity of the various studies that are listed in Table 2, for which I am not qualified and which would also go far beyond the scope of this paper. Therefore, I had to accept each of these peer-reviewed (by peers much more qualified than me) and published studies at face value. I did not feel that it was my place to call any of these studies an "outlier". What I am hoping for is that the community sees these large discrepancies by having them juxtaposed in one Table, and makes efforts to address them. Thus also the deliberatively provocative statement that "the uncertainty in biomass burning emissions nowadays is as large as in the 2000s". I would very much welcome a paper by the remote sensing community, maybe in the form of an intercomparison project, that will prove me wrong! But to address the reviewer's specific concern about FLAMBE, I have added a statement paraphrasing his/her comment and a reference to the review itself: "The inverse analyses may also be useful to indicate unlikely estimates based on remote-sensing techniques. For example, the burning of 8750 Tg dm in tropical forests estimated by FLAMBE, combined with the corresponding EF_{CO} (105 g kg^{-1}) would produce CO emissions of 900 Tg a^{-1} from this biome alone, well above the range of inverse CO emission estimates for all open burning (see also the comments by Reviewer 1, https://doi.org/10.5194/acp-2019-303-RC1)."