The authors use air craft observations and statistical methods to derive new estimates for mercury emission factors from wildfires. The presented work is of importance as natural mercury (re-)emissions become ever more relevant with the ongoing reduction of anthropogenic emission as internationally agreed under the Minamata Convention on mercury. Forests and vegetation are a major storage for mercury. With increasing temperatures wild fires will become ever more prevalent (Veira et al., 2016 ). The year 2020 with its massive fires in the Arctic were a recent reminder of this.

The presented manuscript is generally well written. However it is a bit long and the abstract needs some language editing. The employed methodologies are sound and the authors show an extensive knowledge of the field. They compare their results with previous estimates and what I find the most important clearly state and quantify the uncertainties of their method.

I support publication of the manuscript after a few questions have been answered.
Finally, I need to apologize for taking so long with this review.

**Remarks**

p.6
Section 2.2: How sure are you that you measure GEM only and not GEM + a fraction x of GOM? Given that you heat the sample line from ambient temperature to 50°C I think x could be larger than zero. What is your oppinion on this?

p.7
Section 2.3/2.4: I wonder what the detection limit (dl), especially for the CO measurements is. As there are large differences in the detection limits of background CO instruments and regular ones. The Picarro G2401-m manual I could find online states a dl for CO of 0.2 ppm which seems low compared to the observed CO average of 0.134ppm. Please correct me if I am wrong.
Anyhow, I would appreciate if you added this information.

p.9
ll.283-286: Do you include small scale fires from GFEDv4? Do you think small scale or smoldering fires could be of relevance to Hg emission from wildfires?

p.13
ll.392-402: You compare your results to GEM:CO emission ratios from the literature. To what extend do you expect that the mercury content of the fuel burned during each of these campaigns is comaparable? Or shorter: Could differences in Hg content explain the variability of the different ERs?
*I see you discuss this later in the manuscript but maybe it makes sense to mention it here briefly.*

ll.396: ‘This places the GEM:CO ER determined in our study near the lower end of this range, ...‘ Please add a reference to Table 1 here. Otherwise one has to search for it, readers will surely thank you.

p.14
ll.430-434: What effects the CO:$CO_2$ ER the most? I would expect temperature und thus fire size besides the fuel type could be important?

p.20
ll.628-640: I suggest that you give your best guess

**Language**

p.1
l.12: please correct: ‘the contaminant to the atmosphere’

l.13: I suggest ‘part’ in stead of ‘compartment’

l.17: ‘emissions plume’ sounds odd, I suggest just ‘plume’

l.19: ‘…,which ist 2.4 times background concentration’

p.14
ll.424-430: Text formatting seems off here.

**References**

Veira, A., Lasslop, G., Kloster, S. 2016. Wildfires in a warmer climate: Emission fluxes, emission heights, and black carbon concnetrations in 2090-2099.