Review of peat fire emissions estimate by Kiely et al.

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In this work new peat fire emission factor (EF) measurements from the literature and a new approach for model estimates of peat consumption developed by the authors are incorporated into the authors WRF-chem regional model for insular SE Asia and better agreement with ambient PM monitoring and AOD are observed. The impact of injection altitude is also explored. The new peat consumption approach may be developed further, but has already reduced bias and increased correlation. A few improvements could be made to the manuscript. Mainly, a little more could be added about the impact of secondary processes and efforts made to avoid recycling some confusion on literature EFs. My suggestions are further clarified in my detailed comments provided in order of appearance in the paper in "page number, line number:" format if not otherwise specified. If, after commenting at one "P, L", I saw the issue partially addressed later in the paper, I did not delete my first comment in most cases. This is because it may be worth addressing the issue earlier in the paper?

Title: very minor point "Revised" kind of implies they are changing a previous estimate they made?

1, 22: Two minor points. 1) There is a lot of peat in Malaysian Borneo. 2) The drainage canals may have already largely been built by the early 1980s? There's a debate now about blocking them to re-flood the peatlands.

1, 25: "fuel consumption" may be better/safer than "burn depth" since the area burned is hard to measure due to small size, smoldering, high regional cloud cover, etc. (Reid et al., 2013)

Reid, J. S., Hyer, E. J., Johnson, R., Holben, B. N., Yokelson, R. J., Zhang, J., Campbell, J. R., Christopher, S. A., Di Girolamo , L., Giglio, L., Holz, R. E., Kearney, C., Miettinen, J., Reid, E. A., Turk, F. J., Wang, J., Xian, P., Zhao, G., Balasubramanian, R., Chew, B. N., Janai, S., Lagrosas, N., Lestari, P., Lin, N.-H., Mahmud, M., Nguyen, A. X., Norris, B., Oahn, N. T. K., Oo, M., Salinas, S. V., Welton, E. J., Liew, S. C.: Observing and understanding the Southeast Asian aerosol system by remote sensing: An initial review and analysis for the Seven Southeast Asian Studies (7SEAS) program, Atmos. Res., 122, 403-468, doi:10.1016/j.atmosres.2012.06.005, 2013.

2. 1: GFED4s, the "s" indicates the product with small fires added?

2, 2-3: Technically, to "confirm" an EF with a model you'd have to compare the model to the measurements at the point of emission or somehow know that the model correctly treated all secondary processes, so it's probably better to claim "consistency" or "support" for the new EF. Also the paper should mention the relative change in PM due to post emission processes predicted by the model at some point.

2, 19: Is this statistic still current now that large peat deposits were recently discovered in the Congo?

2, 22: I think it may be more accurate to say “Prior to this conversion, drainage canals were installed” since I think the draining was largely completed as part of the “mega rice project” before the massive emissions of the 82-83 El Nino. Interesting side note, the drainage canals were built at the recommendation of a USFS expert despite the same type of drainage causing an environmental smoke disaster in the US in the 1930s as described in “Sand County Almanac.”

2, 27: surface fires are not typically extinguished in Indonesia, but just burn out.

3, 1-2: Here and throughout please be careful to use “peatland” to indicate all the vegetation and peat in areas underlain by peat or “peat” to indicate just “peat”. These statements should be phrased more quantitatively similar to what is presented later in the text

3, 10: “water content” of peat may be replaced by something about the “water table”, i.e. peat can burn down to the groundwater level

3, 17-19: The vast majority of field data on gases emitted by smoldering peat were measured by Stockwell et al 2016 not Hu et al. who review a small subset of the gases measured. The Roulston et al paper never actually recommended any EF, they just said the instantaneous EF change during the fire. The gravimetric measurement of EFPM by Jayarathne et al., 2018 is likely the best value for reasons described later in review. Stockwell et al 2014 gave the first detailed discussion of variation in peat fire EF (Table 3 and Fig. 7) and Stockwell et al 2015 provide the most comprehensive EF data for peat fires.

Stockwell, C. E., Yokelson, R. J., Kreidenweis, S. M., Robinson, A. L., DeMott, P. J., Sullivan, R. C., Reardon, J., Ryan, K. C., Griffith, D. W. T., and Stevens, L.: Trace gas emissions from combustion of peat, crop residue, domestic biofuels, grasses, and other fuels: configuration and Fourier transform infrared (FTIR) component of the fourth Fire Lab at Missoula Experiment (FLAME-4), Atmos. Chem. Phys., 14, 9727-9754, doi:10.5194/acp-14-9727-2014, 2014.

Stockwell, C. E., Veres, P. R., Williams, J., and Yokelson, R. J.: Characterization of biomass burning emissions from cooking fires, peat, crop residue, and other fuels with high-resolution proton-transfer-reaction time-of-flight mass spectrometry, Atmos. Chem. Phys., 15, 845-865, doi:10.5194/acp-15-845-2015, 2015.

The statement “Roulston et al. (2018) and Wooster et al. (2018) found that assumed EFs for tropical peat fires could be underestimated by a factor of three” should be more quantitative. One can assume peat fire EFs are too big, or too small, by as much as they want. Roulston only said that the EF can change during the fire, but did not suggest any procedure that was more valid than random sampling. Wooster et al had experimental issues and their “peat EF” had variable contributions from non-peat fuels in all but one location as clarified more later.

3, 20: “EFs were”

3, 21-22: Kuwata et al would likely be “peatland” fires not “peat” fires since they monitored from a great distance. The idea of being able to measure a precise annual average EF implied here is probably not realistic. It’s hard to measure CO2 from fires at a distance due to mixing effects and CO2 is the largest contributor to the EF. It’s interesting that 13 and 19 average to 16 which is close to the 17 that Jayarathne et al essentially got. The year to year differences could be due to a number of factors besides the year. The uncertainties seem too low at e.g. 19 +/- 2.

3, 2-27: I don’t think the observations were scaled so rewrite: “Previous studies have scaled particulate fire emissions from global fire emission datasets, or simulated fire-derived aerosol optical depth (AOD) or PM, by a factor 1.36 – 3.00 in order to match observations” as “Previous studies have scaled particulate fire emissions from global fire emissions inventories by a factor 1.36 – 3.00 in order to match observations of PM or simulated fire-derived aerosol optical depth (AOD)”

3, 29: “datasets” could be many things (including the papers with high EF) so change “datasets” to “inventories” throughout.

3, 33: lowering the water table exposes more peat to the possibility of burning.

3, 34: The two biggest El-Nino peat fire events were 82-83 and 97-98. The biggest El-Ninos create fire episodes that span two years.

4, 2-4: rewrite to clarify that Jayarathne is peat only and the others are for peatland.

4, 4-6: It’s very important to be precise about mortality before the atmospheric community loses credibility. Smoke is a contributing factor to premature mortality, which is not like a perfectly healthy person “getting shot.”

“responsible for” should be “contributed to” and “excess deaths” should be “premature deaths” so phrase reads: “contributed to between 6,513 and 17,270 premature …” and fix line 5 same way.

4, 10-14: change dataset to inventory in 4 places?

4, 30: Does dynamics and processes include evaporation and secondary formation, deposition, etc?

5, 6: POA volatility? SOA scheme? Relative amount of modeled secondary to primary emissions?

5, 16: burned per pixel

5, 25: EFs come from the peat itself? They are based on some measurement, probably the one sample of Indonesian peat burned in a lab at the time Akagi 2011 was compiled. We started working towards peat fire field measurements in 1994 against large odds. In 2000 we were able to legally import and burn one sample in our lab. In 2015 we finally completed field measurements.

6, 8: The surface fuels likely burn on the day of detection, but, if the peat ignites, having it release emissions over the next few days at least would be more realistic, but probably too late to change. Maybe it should be noted that this could shift the emissions forward in time. Also if a fire creeps along and is visible for many days hopefully it is counted as a fire all those days and not tossed as a duplicate on some days. (Later noticed this was partially addressed perhaps belatedly?)

6, 10: delete “a”

6, 11: I think hotspot detection is widely believed to be more sensitive than burned area detection so I would instead expect many hotspots did not have an associated burned area.

Can any attempt be made to use LandSat or SAR burned area data, which may be more sensitive burned area products?

6, 14: Ok, our crew in Indonesia noted surface areas that burned where the peat did not ignite by the end of the dry season so peat area burned could be less than surface area burned.

6, 15-17: Stockwell et al., (2016) measured burn depth on numerous sites (see their supplement) and report a close average value of 34 +/- 12 cm. Might be good to add to Table 1 and reference string for completeness. Similarly, Konecny et al., (2016) reported a peat bulk density of 0:120+/- 0:005 g/cm that could also go in Table 1?

6, 22-30 and Table 2: I have a number of comments on the EF in Table 2. I’ll go in order by study.

Christian et al., (2003): This was just one sample of Sumatran peat burned in lab in 2000. One might be tempted to weight this study less than the more extensive studies and especially the extensive 2015 field studies of Stockwell and Smith.

Wooster et al., (2018): In general, it’s important to clarify peat or peatland throughout the paper as noted above. It seems like one should not use peatland EFs for peat because FINN has EF and fuel consumption for surface vegetation already so applying peatland EF to peat will double-count? I have enough concerns about the EF in the Wooster study that it’s a bit of a challenge to determine the best explanation.

1. When using light-scattering to measure PM, the response depends on particle density as they noted, but also the size distribution. They took a good step to calibrate the light-scattering with

filters also collected during the measurements. However, the graph shows many calibration points being collected at extremely high concentrations (e.g. 17-25 mg/m3 when .3-1 mg/m3 is already stretching normal measurement level). That means some PM data could be affected by unrepresentative gas to particle partitioning and greatly inflate some EFPM. This doesn't seem to have impacted their "pure peat results" from "location 5" (vide infra), but may be why they got EFPM for smoke that reflected some burning surface vegetation that were in their words "among the highest ever recorded" and well above the literature average for various surface vegetation types.

2. They use a significantly higher %C for peat than the other studies, which increases all EF across the board without saying how many peat samples were measured or how. It would have been nice to know if there was one or many peat samples (see comment on Stockwell 2015).

3. They left the carbon in the PM out of the carbon mass balance in their EF calculations. Along with ignoring NMOG this would inflate all EF by ~5%. In addition, in a unique, unexplained step; they report two different EFPM for every site depending on using CO or CO2 as a reference.

4. They report EF for (1) "peatland" or "mean of all locations"; (2) "peat only" which their text says actually is "mostly peat" and includes some burning surface vegetation; (3) "really pure peat only" (location 5). For PM there are two sets of EFPM for each of these categories. In their table the EFPM range from 34.4 to 11.4. After reducing the EF for location 5 (actual pure peat) by 5% (to better reflect true total carbon emissions) the EFs are CO2 (1627), CO (308), CH4 (4.96), PM-avg (12.6). If any peat fire EF data were to be used from this study, then these seem appropriate and not the values in the table. In particular, I'm not sure where the 28 for EFPM the authors quote came from.

Stockwell et al 2016: Stockwell and Jayarathne co-deployed in Borneo. Stockwell reported a subset of the filter-based EF data to interpret simultaneously-collected PAX data. Thus the Stockwell OC and PM should not be included, but the BC can be since it is an independent measurement by PAX rather than thermal-optical analysis. This Stockwell study may be most extensive high quality field measurements of pure peat emissions for BC and trace gases.

Stockwell et al., 2015: This was a lab study, not a field study. It's probably not included in the paper, but nine samples of Indonesian peat measured in this study ranged from 50 to 61% C and from 50 to 58 %C when nominally at the same site; illustrating the heterogeneity and the need for intensive sampling of this parameter. The value 0.57 +/- 0.025, based on the 7 most representative samples, was used in this study, Stockwell 2016, and Jayarathne 2018.

Jayarathne et al., 2018: Gravimetric filter sampling has challenges, but is often considered the most accurate approach for PM, OC, EC, etc EFs. OC/PM ratios are best measured on the same sample.

Table 2 general comment. I think it would be best not to recycle any confusion regarding Jayarathne/Stockwell overlap and peatland versus peat. If that meant that the real best EF were different from what is shown as the Table 2 average and used in the model; and the model could not be run again, that would be understandable, but then the discussion could speculate that secondary PM or biomass burned was actually higher or add other topics.

7, 5: Stockwell et al., (2016) showed brown carbon absorption is very important for peat smoke.

7, 11 – 9, 3: This is an excellent idea to vary burn depth with guidance based on soil moisture – though water table may be even better (Putra et al., 2018). Did the soil moisture in high fire regions get significantly lower than the near constant regional average reported?

Perhaps in a future version it can be added that burning that deep can take time and that there is a lag between rainfall and water table level.

Putra, E.I., M.A. Cochrane, Y. Vetrita, L. Graham and B.H. Saharjo. 2018. Determining critical groundwater level to prevent degraded peatland from severe peat fire. IOP Conference Series: Earth and Environmental Science 149 012027 doi: 10.1088/1755-1315/149/1/012027

Also, in early November at precisely-located sites in Kalimantan, Stockwell et al 2016 (supplement) report actual burn depths for site specific comparison; or one might compare to the average of 34 cm. However, their data may be biased to dry spots that were still burning? As reported in Stockwell reaching the maximum depth takes time and termination of the fires likely requires the water table to rise since peat is difficult to "rewet".

9, 5: Excellent feature to experiment with injection altitude.

9, 8: Smoldering peat can release smoke very close to the ground (pics in Stockwell 2016 supplement).

9, 15: These are two excellent choices for plume rise experimentation in model.

9, 22 and 11, 3, Figure 1, Table 3: There is more than one PM monitoring site in Indonesia. The government agency BMKG has seven PM10 sites (http://www.bmkg.go.id/kualitas-udara/informasi-partikulat-pm10.bmkg) and 5 PM2.5 sites: (http://www.bmkg.go.id/kualitas-udara/informasi-partikulat-pm25.bmkg). Include a sentence to mention this and why not used (no access?)? I have 17 days of PM10 from 2015 for Palangkaraya that I happened to save on my hard-drive and it may be possible to get the rest. Indonesian sites could have high value for constraining emissions by virtue of being less sensitive to secondary processes, meteorology, and other sources. They would have high S:N. I also have Oct 2015, Palangkaraya visibility, which may be useful.

Any estimate of how burning in peatlands in "West Papua" might impact model/ambient-PM agreement?

9, 31: PM1/PM2.5 seems high, usually about 80%, and that's what they saw on next page.

11, 11-12: Why comparing to two months instead of whole season?

11, 14: Should it be explained somewhere that Wooster et al based estimates on an inversion using MOPPIT CO (and give MOPPIT version since last version supposedly has improved sensitivity in boundary layer, but still a lot of uncertainty)?

11, 15: "reasonable agreement"

11, 24: Jayarathne was estimating peat only. FINN + Jayarathne peat is 900 Tg right between FINNpeat and FINNpeatSM.

11, 31: "due mainly" - other things vary too, like burned area and EFPM for surface vegetation types? And should "peat" be "peatland"

12, 4: Does "smaller area" indicate that the burned area of Whitburn et al 2016 was smaller than the burned area in FINN and GFED? In general, maybe the peat consumption and other data in Whitburn et al could be compared to the products developed in this study.

Whitburn, S., Van Damme, M., Clarisse, L., Turquety, S., Clerbaux, C., and Coheur, P. F.: Doubling of Annual Ammonia Emissions from the Peat Fires in Indonesia During the 2015 El Niño, Geophys. Res. Lett., 43, 11007–11014, https://doi.org/10.1002/2016GL070620, 2016.

12, 5: "emission for peat only was" – Jayarathne et al estimated only the peat and their peat only value for PM is right between FINNpeat and FINNpeatSM peat only PM emissions. Their uncertainty is large, but properly propagated and useful since no other uncertainties are reported:)

12, 12 or later in paper: There are some more studies to compare to and it should be clear what geographic area the peat/PM ratios represent in each study. Per his short comment, Eck estimated 80-85% of aerosol was from peat combustion at Palangkaraya where the peat contribution is probably higher than in Singapore.

Eck cites Kaiser et al who got 83% of emissions from peat and Wiggins et al who got 85% of emissions from peat.

Kaiser, J. W., van der Werf, G. R., & Heil, A. (2016). Global climate biomass burning in "State of the climate in 2015". Bulletin of the American Meteorological Society, 97(8), S1–S275. https://doi.org/10.1175/2016BAMSStateoftheClimate.1

Engling et al., 2014 got 76% peat fire impact on TSP at Singapore for hazy days in 2006.

Engling, G., He, J., Betha, R., and Balasubramanian, R.: Assessing the regional impact of Indonesian biomass burning emissions based on organic molecular tracers and chemical mass balance modeling, Atmos. Chem. Phys., 14, 8043-8054, https://doi.org/10.5194/acp-14-8043-2014, 2014.

Possibly useful?

Hansen, A. B., Witham, C. S., Chong, W. M., Kendall, E., Chew, B. N., Gan, C., Hort, M. C., and Lee, S.-Y.: Haze in Singapore – source attribution of biomass burning PM10 from Southeast Asia, Atmos. Chem. Phys., 19, 5363-5385, https://doi.org/10.5194/acp-19-5363-2019, 2019.

13, 5-9: How much do emissions from the island of New Guinea impact the monitoring stations employed in this study (see Fig. 2 in Hansen et al above)? Also, line 9, "need for future"

13, 18: The word "matches" is used here and elsewhere when maybe "consistent with" is better or a % difference would be more useful.

13, 19-25: Would it help if the model released the peat emissions over the weeks following detection? Peak regional PM in Palangkaraya was in October rather than September.

Figure 4: FINNpeat and FINNpeatSM seem to overestimate PM and increasingly so with distance in the two examples shown. One could argue that the sites closer to the fires and with higher PM are best for constraining initial emissions or EFs for PM. Downwind sites are more subject to secondary formation, evaporation, deposition, or transport errors. However, Fig. 5 does show the lowest bias overall and the highest correlation overall for FINNpeatSM, which is useful. Is there value in comparing the integrated amount of PM modeled vs measured over the season (or by month) by site in a Table? Is there any CO monitoring to compare to?

14, 5-8: Surface fuels burn quickly after detection and then the peat smolders till the end of the dry season. At the end of the dry season we saw an average burn depth of 34 cm, but burning down to that depth and or laterally across much of the burn scar takes time. The model choice to shift all peat emissions forward to the day of detection could cause Sept/Oct too high? On lines 6 and 7, be specific using "model" or "models", all of them, or which ones?

14, 13-16: The effect of a non-linear relationship might also be mimicked by a longer time-frame for peat consumption as speculated above.

14, 16: Has a relationship between soil moisture and burn depth in fact been measured? The Putra et al reference cited above discusses a large data base relating rainfall, water table, and burn depth.

14, 18-20: This seems to finally acknowledge the point about the emissions taking time.

15, 1-2: "Only hotspots that last 3 days are a fire" makes no sense so it's good the authors ignored this. There could be cases where a 3-day fire is needed to ignite wetter peat.

15, 3: The timing of the peaks in Pekanbaru is excellent.

16, 5: Without knowing anything about secondary processes the best way to validate EF is with EF measurements and the authors are using updated EF, but undetected burned area can't really be written off? There could be undetected fires that are offset because the model could e.g. underestimate evaporation?

17, 1-2: Injection throughout the boundary layer, as opposed to all at the surface always reduces the modeled PM. Per above, there could be missing fires that are compensated for in the injection choice. Is there any model-assumed evaporation associated with faster dilution into the whole boundary layer?

17, 12: Is POA inert in model? A recent study suggests about 50% of PM can be lost by evaporation (or deposition) for some fires.

Selimovic, V., Yokelson, R. J., McMeeking, G. R., and Coefield, S.: In situ measurements of trace gases, PM, and aerosol optical properties during the 2017 NW US wildfire smoke event, Atmos. Chem. Phys., 19, 3905-3926, https://doi.org/10.5194/acp-19-3905-2019, 2019.

Other BB studies (e.g. Vakkari et al., 2014; 2018) saw SOA dominate.

17, 23-24: The author's model gets a better r value with AOD, not sure if this sentence is needed?

17, 29: "mean" should be "peak"? Otherwise a mean of 1800 seems to conflict with the much lower means given just below?

18, 1-6: Are these peat/PM values for whole model domain or some subset? The values are higher than for the study of Lee et al discussed on next page

18, 6: So does this discussion then indicate that PM/CO increases slightly with aging. E.g. a factor of 76/71 still fairly close to the fires? Also, is this really known to be all from SOA when there are also inorganic precursors like NH3, NOx, SO2, etc?

18, 11-12: don't understand this.

19, 3: Does current study agree that half or more of the pollution during haze episodes in Singapore is not from fires?

19, 4-6: not sure what this adds?

19, 8-20, 2: More sites close to the fires (like Palangkaraya) could help determine the proper choice of injection altitude? Maybe it makes sense to inject non-peat emissions throughout the BL and peat fire emissions at the surface?

20, 7: In the AOD calculation, is the air-mass factor impacted at some PM level, is BrC ignored at 550, is it possible to compare to inferred AOD at Palangkaraya in Eck et al?

20, 11: add “surface” before “PM2.5” to make this conclusion even more obvious?

20, 16: Here and earlier, a more inclusive geographic term than “Indonesia” might be “insular Southeast Asia.”

21,1: “datasets” to “inventories”? If not specifying inventories, should the qualifier “some” be included since there is agreement with some previous work?

21, 3: This study simulated more than just Sept-Oct so why limit to those months?

21, 8: fix “a new emissions datasets”.

The new EF were likely applied to peat fires in Malaysia too? There can be many peat fires in Sarawak (one part of Malaysian Borneo). The Smith et al study measured peat fire EF on mainland Malaysia. Also lines 26, 28, 32 may overly single out Indonesia, which dominates but doesn’t account for all the peat combustion in the model domain.

21,12: mention agreement with some other estimates?

21, 13: improved estimates of peat consumption should improve CO2 as well?

21, 19: Need work on secondary processes also.

21, 24: Work is needed on how rainfall, soil moisture, water table depth, burn depth, and burn rate or timing are related. In Putra et al TRMM rainfall had a minimum in August, but measured ground water level in an extensive series of dip wells hit a minimum one or two months later.

21, 31: “impact” better as “contribution” since the ambient PM itself was not revised.