

Interactive comment on “Nepal Ambient Monitoring and Source Testing Experiment (NAMaSTE): Emissions of trace gases and light-absorbing carbon from wood and dung cooking fires, garbage and crop residue burning, brick kilns, and other sources” by Chelsea E. Stockwell et al.

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We thank Referee #3 for their overall positive assessment and suggestions, which will improve the paper. Next we reproduce the exact comments followed by a detailed response:

Anonymous Referee #3

Received and published: 29 July 2016

Summary: This paper describes the measurements of trace gases and carbonaceous (BC, and BrC) aerosols (mass and optical properties) from a variety of bio and fossil fuel burning sources in an around Kathmandu valley of Nepal in the eastern Himalayas. These measurements formed part of a field campaign using a mobile laboratory and a set of state-of-the-art instruments. These measurements provide important and valuable data on light absorbing aerosols and trace gases over this part of the Himalayas, and is done so extensively perhaps for the first time and thus are, useful for environmental and climate impact assessment studies. Having said the above, the data should also be taken with the pinch of salt that the measurements have been for a very short duration (about a fortnight), the number of samples used to evolve the means are statistically not very high. How well these sample represent the regional population of these sources remains to be seen. In other words, the data emanates from a highly under-sampled source. Of course, the authors also seem to be aware of this. Moreover, the reported values correspond only to a particular period of time of the year (not even a full month). They also do not represent the strong seasonal variations in some of these sources and also changes caused by changes in synoptic meteorology and long-range transport; which are very important over this region. Their spatial representativeness also is highly limited to the urban region of Nepal and its southern slopes, which would be impact by transport of emission from the adjoining north Indian plains. Nonetheless, it remains that these are the first comprehensive set of measurements from several sources that are specific to this region (on which field measurements are not possible in other parts of the world), and provide information that could be used as inputs for regional climate and environment impact assessment and probably would lead to more such measurements. In other words, they provide some ball-park figure on these parameters, which were otherwise not available. In view of the above, the paper may be accepted for publication in ACP, provided the authors take care of the following specific concerns in their revised version:

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R1. Make a clear statement of the limitation of the data (under sampling in time, space and sources) explicitly in the abstract and conclusions

Authors: Two Referees suggested clarifying the need for more sampling of these sources and we have added that to the abstract, introduction, and conclusions as described in the response to Referee #1.

R2. Provide the fractional figures (to the extent accurate) of the sources sampled, against the total number of such sources present in Kathmandu (for e.g. % of brick kilns sampled vs the total no. available in Kathmandu, the no. of two-wheelers sampled vs the total two-wheelers plying in the city, the no. of gensets samples vs the total etc in a table or in the respective sub-sections.

Authors: We hesitate to try to add an estimate of the fraction of each source sampled. We certainly understand the gist of the comment in that not nearly enough sampling has been done! Our work just begins a daunting task. We already made this point in several ways: 1) we clearly stated that we began to address the data shortage (and added emphasis as above), 2) we said fleet-average values for traffic requires a bigger study, 3) we estimated that there are 1000 kilns in Nepal and noted that we measured only two, 4) we gave the total fuel use as opposed to number of devices in most cases, but that makes it clear our sampling is just a beginning. According to statistical theory, it's the size of a random sample and not the fraction of the population sampled that determines representativeness and uncertainty. The more common target types are more likely to be selected. Our sampling was not purely random, but was targeted towards common local practice by in-country experts. The variability of these sources is not well-known and there are seasonal trends we could not measure, but our measurements were in the dry season, which is when pollution problems are the highest. Note transport and meteorology issues are not relevant to source measurements. As is typical for emerging issues, we use what data we have in the beginning and the available data, as a whole, are generally improved as more measurements are made. We do however agree with both Referees #1 and #3 that a reminder that more sampling

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is needed (especially for gensets, ag-pumps, brick kilns, and transportation) would be useful and we have added that to the conclusions and abstract as described above.

R3. For FTIR – please provide the actual averaging time used (for grab sampling), the best and worst S/N for the species being reported in this study and the resulting uncertainty in the derived concentrations.

Authors: We are not completely sure what the comment refers to, but there are several important issues raised and we try to address all of them. For both FTIR and WAS it takes about 10 s to acquire a grab sample so they are spot measurements rather than time-averaged. If the question is about the storage time in the FTIR cell for signal averaging that is 2-3 minutes and on P8, L21 we changed “several minutes” to “two to three minutes” to be more specific.

The FTIR S/N and the uncertainty, which is impacted by multiple factors, is different for every mixing ratio we retrieve. The uncertainty in mixing ratios ranges from very high near the detection limit to very low in most cases because of the extremely high concentrations in source plumes. The ER and EF for each source are based on the regression using all the samples of the source in an ER plot as described in the text. For less concentrated samples the uncertainty is higher but their weight is reduced in the regression with the intercept forced as explained in text. For the extremely high concentrations that dominate the ER plots, the uncertainty is mainly the uncertainty in the reference spectra given in Sect. 2.2.1. The natural variability in the ERs or EFs from source to source of the same nominal type (~40%, seen in the tables) tends to much larger than the uncertainties in the slopes for each source (~10%). Thus, as a reasonable estimate, the uncertainty for the mean EF for each source type is given as one standard deviation of the mean. We’ve clarified the relatively minor role of the uncertainty in the individual mixing ratios with the following change in the text:

P10, L31-32: Now reads: “Forcing the intercept effectively weights the points obtained at higher concentrations that reflect more emissions and have greater signal to noise

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so that error is dominated by calibration uncertainty.”

R4. In the ER calculation, state explicitly how the average ER estimated from the FTIR around a given source, compared with the values obtained from WAS data, for completeness of information and also in view that WAS value represents the ‘bulk’.

Authors: The samples by WAS and FTIR were at different times so the differences that may occur between them could be due both to uncertainty and natural variability. The number of FTIR samples is much larger as the cell can be refilled after the spectra are collected for two-three minutes, but the WAS samples add unique species and add sampling of the plume at more times overall. Despite the lack of exact overlap in timing, the single WAS samples for each source are in, or close to, the range of FTIR values. We don’t understand the comment about WAS representing the “bulk,” but think both approaches give a good overview of the emissions.

R5. It is highly appreciable the way authors have described all the assumptions used in their estimates. Yet, it would be better if they can give an upper and lower bound for the estimates due to the specific assumption they have made (like 50% wood and dunk in mixed fuels – suppose it is 80-20 or 30-70)

Authors: EFs are proportional to assumed %C and we discussed how to adjust the EF to reflect different %C that would arise from different mixes in the response to Referee #1. We had no way of measuring the real mix of fuels that was burning at the instant of our grab samples, nor do we know the range of mixtures commonly used so we took an illustrative guess. We have added the adjustment procedure in the mixed fuel EF section.

P12, L39: Add: “For mixtures differing from those we used, the EF scale with the assumed carbon fraction.”

We also added the assumed carbon fraction of the pure fuels to the text so the impact of arbitrary mixtures can be estimated directly from the information in the text.

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P12, L31-32: “Thus for the mixed-fuel cooking fires, we simply assumed an equal amount of wood (0.45 C) and dung (0.35 C) burned and used the average carbon fraction for the two fuels (0.40)”

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