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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

Received and published: 24 May 2016

This paper presents a wealth of information on emissions of a wide range of gas phase species and particulate from most of the important sources in the Indo-Gangetic plain region of Nepal. These are important measurements and greatly extend the information on many of the emissions from a number of sources. I therefore recommend publication of the paper in ACP.

The paper is very long. This is, to a large extent necessary as the authors present a large amount of information and provide a significant amount of important and detailed discussion. In addition, the supplementary material contains all the basic data used to produce the average data presented in the main paper and a methodology of how the averages have been developed. The authors should be applauded for this.

However, the data available from the NAMASTE sampling varies in extent and detail quite considerably from one source to another. Clearly the earthquake also had a disruptive effect on the sampling. This, in itself is not a problem, some of the sources are sampled in great detail and make significant advances on what is currently available (such as the field sampling of cook stoves) and may be used to develop emissions inventories across the region. In other cases only a couple of measurements are made (e.g. brick kilns), whilst these may be important in that they identify whether or not the source may be important they are more preliminary in nature. Neither of these is a problem and I welcome seeing all the results, however, as presented the long discussion of data from different sources has the effect of burying some of the major implications in a forest of less important results. I suggest that to gain maximum impact from the paper the authors consider a more informative summary in the conclusions section that compares their data to present literature, highlights where the authors recommend changes to current thinking about emissions inventories and the estimated size of any effect of such consideration (eg crop residue burning), and points out where further work is needed and how their data shows what this entails (eg brick kilns and garbage burns). The authors may also wish to amend the abstract in a similar fashion. I would like to see the importance of any recommendations quantified in the abstract and conclusion and not only in the main body of text.

The paper reports NOx emission ratios for a wide range of sources. However, despite the FTIR measurements making independent determinations of NO and NO2, there is no real discussion of their relevant contribution to primary NOx at all. Given the toxicity of NO2 and the widespread regulatory framework for NO2 that exists this is somewhat

surprising since the data could be used to inform air quality management decisions in this area. I would very much recommend that the NO2/NOx is discussed for each of the sources wherever possible.

The paper reports high relative emissions of NOx and absorbing aerosol from diesel generators and diesel agricultural pumps (page 18 line 1) and these figures should be included in revised emission inventories for the region. However, there are very few measurements of these sources in this study, only 2 diesel generators were studied (and only one of these sampled for NOx by the FTIR) and two irrigation diesel pumps were studied. How confident are the authors that these samples are representative enough of these emission classes across the region to argue that revised emission inventories should include these sources?

Page 8 line 33: It is incorrect to say that SSA and the absorption Angstrom exponent were measured. The measured properties from the PAX are the absorption and scattering coefficients. The SSA and absorption Angstrom exponent are derived.

Page 10, lines 33-36: Were the ERs derived from the WAS and the FTIR for the same species within uncertainty and if not why was the FTIR data chosen, was this simply on S/N, could there have been biases?

Page 11, line 6: You need to say that n is the total number of measured species.

Page 11, lines 11-14: How are FTIR, WAS and filter data combined into the denominator of equation 1? I can imagine that different averaging times could lead to sampling very different source strengths and hence biasing the denominator relative to the numerator for a given species.

Page 11, line 20: are all the CO and CO2 data sufficiently above the background in all the samples? If not taking the average of the ratios will introduce uncertainty (and possibly bias) and it may be better to take the average of deltaCO and delta CO2 data and then calculate a ratio.

Page 12 line 4: The MAC used to calculate BC is an average value over a wide range of source types. There is some variation of MAC between different sources of BC, particularly some of those in this paper. The authors should comment on this uncertainty and variability.

Page 12 section 2.4: There is considerable uncertainty in the emission factors for the mixed fuels. The authors provide a detailed summary of their assumption but don't provide an estimate of the uncertainty in their estimate. This would be good to see.

Page 13 line 16: As before, SSA is derived, not measured.

Page 13, lines 19-20: "the shape of the absorption cross section". Better to write "the wavelength dependence of the absorption cross section"

Page 13 lines 21-22: The bars in figure 1 seem to represent the variations in AAE seen for different burns of the same type. There will also be some burn to burn variation in AAE as well, it would be good to show this also.

Page 13, line 23: should read "upper right hand corner"

Page 13 line 26: I am not sure I understand what is said here correctly. If there were no PAX measurements at 870 nm for the agricultural pumps and all but one of the garbage burns, how were the AAEs in figure calculated? I can understand that based on the SSA they can be assumed to be almost pure BC and hence an appropriate MAC is chosen to derive a BC mass but unless an AAE measurement derived value is possible they should not be included in figure 1.

Page 14 section 3.2: I am somewhat surprised that NO2 is below detection limit. However, the detection limits don't appear to be available anywhere in the paper to know whether the NO2/NOx ratios are simply too low to be measured (<2%) or are abnormally low for the vehicle type.

Page 16 line23-24: it isn't clear why this isn't provided in the earlier section

Page 20 line 8-10: It is stated that the scaling of lab emission factors is carried out according to Stockwell et al (2014) but this uses emission ratios to CO for smouldering and not methane. Furthermore, the full discussion of the method is not really presented in detailed in Stockwell et al (2014) but is in Yokelson et al (2008). It might be good to cite this paper directly.

Page 26 line 18: define the subscripts CK and ZZ and use elsewhere, otherwise write out what is meant.

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