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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Seasonal and diurnal variations of methane and carbon dioxide in the Kathmandu Valley in the foothills of the central Himalaya" *by* Khadak Singh Mahata et al.

Anonymous Referee #2

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Highly precise and long-term measurements of greenhouse gases are essential to understand the underlying processes in the context of global climate change. It is particularly valuable in regions like Asia where it is currently limited by dedicated long term observations. This study demonstrates the observational variations of CO2 and CH4 mixing ratios in the Kathmandu Valley (Nepal), and compares them to those of other rural sites. The scope of this study is hence highly relevant to the public and authors' efforts on this regard need to be appreciated. However, the manuscript in its present form does not meet the standard to merit the publication in ACP, and needs to be adequately revised. I therefore recommend the current manuscript to undergo major revision to be considered in ACP.

General comments

As mentioned above, I value authors' effort in this study as an important step towards generating observational wealth which can be tremendously used by scientific community to understand a wide range of mechanisms involved in this aspect. Given the complicated interplay of many processes involved, single approach cannot answer the unresolved scientific questions related to these processes and mechanisms affecting mixing ratio variations. This requires defining far more systematic approaches and other robust tools. However the finding from this study can be very valuable if presented with adequate measurement and analysis methods/techniques used, including a good summary of the methods and a much clear report on the observational variations of these analysed tracers. The results will be more convincing by focusing on the key aspects of the data rather than trying to relate them to flux categories based on assumption (many times) and without sufficient tools (inverse modeling). In some places, results are presented nicely, but then an explanation is suggested based on other literature that focussed on other study regions/methods, without even demonstrating its relevance to this dataset. Also in some places, the study gets into overly ambitious interpretations of the results and conclusions on the basis of single analysis. On this basis, I recommend authors to focus on presenting this study by clearly stating the measurement and analysis techniques used, defining their strategies, providing analysed results & possible uncertainties and giving more convincing interpretations of the results and conclusions. I highly recommend authors not to jump to interpreting emission/flux sources and patterns based on the single site measurements and the analysis done in this study. Sections in this manuscript dealing with these aspects needs to be (preferably) removed or highly restructured.

Specific comments and suggestions for revised analysis

Page 3, lines 63-66 "Between 1750 . . . .cover changes" Misleading sentence. The given estimation is overly high for the accumulated CO2 in the atmosphere. As per IPCC reports & other studies, it is in the range of 230-250 Pg C. The given number is more towards total (cumulative) CO_2 emissions between 1750 and 2011, which were partly compensated by the ocean and terrestrial ecosystems.

Page 4, line 86-89 "important sources" Give reference

Page 4, lines 90-92 "Ecosystem and ….. between July-October Please give appropriate reference for this statement. Prasad et al., 2014 investigated based on satellite GHG concentration observations, not based on inverse or ecosystem models. In my knowledge there's no such inverse flux estimations available over South Asia, accounting (and decoupling) seasonal variations of CO_2 uptake and release to the atmosphere. However, Patra et al., 2011 showed inverse estimations of monthly co2 fluxes over South Asia. Please correct it.

Page 12, lines 308-310 "2.193 ($\pm$ 0.224) ppm, 419.4 ($\pm$ 23.9) ppm, 0.50 ($\pm$ 0.35) ppm, and 310 1.71 ($\pm$ 0.71) %" Uncertainty seems to be quite high for co2, ch4 and co. Why? Please include the reason in the text.

Page 12, lines 318-320 "CH_4 was ….. observation period" Given the above uncertainty ranges of Bode values (nearly 10.2% for CH_4, 5.7% for CO_2 and 70% for CO),the estimated percentages of increment relative to other observatories are also biased. These uncertainties need to be take into account, or at least properly mentioned.

Page 12, lines 320-322 "The small ….. Asia region"

Although I tend to agree that we can expect (+ seeing satellite images), most of the cases, higher CH_4 mixing ratios in Asia relative to Mauna Loa observatory, authors should note that this conclusion, as given in the text, about the whole Asia cannot be drawn from analysing "just two Asian sites" in a given time period. This sentence is misleading, and needs to be reformulated.

Page 12, line 324 "Ahmedabad (1.880 ppm) (Sahu and Lal, 2006) and Shadnagar (1.92 $\pm$ 0.07)"

See my above comment on uncertainty.

Page 12, lines 326-332 "Likewise, the ….. in China" These estimated increments are meaningless based on the uncertainty range of Bode's tracer mixing ratios. That is, these 5.7% and 5.5 % increments are statistically insignificant when it is compared with CO_2 values with 5.7% uncertainty range. I strongly recommend authors to remove this.

Page 13, lines 354-355 "burning activities due to rainfall in the region"

What about the burning activities in Bode area during this rainy period of time?

Page 14, lines 384-385 "The seasonal ….. residue burn" What about seasonality of atmospheric transport and other met. fields? I would think that it can also make a significant impact especially considering mountain valley effect.

Page 14, lines 388 "partially due to rain washout. " It can also very well due to relatively high advection and vertical mixing.

Page 15, lines 395-396 "related to less or no rainfall, which results in the absence of rain washout" if it is with transport or raining, it should also affect CH_4 and other tracers. Please clarify.

Page 15, lines 412-413 "which had > 2.5 ppm CH_4 and > 450 CO_2" It's likely that the increase in CO_2 & CH_4 is associated with advected signals from the North East and the East; however it is not clear how it can be interpreted as the advected airmass had the said values for CO_2 and CH_4 unless the study used any tracer transport and emission models. Wind direction and tracer concentration from the given site alone are not sufficient to conclude this. A clarification is needed here. Otherwise I recommend authors to remove this.

Page 15, lines 414 "(not shown in Figure 4)" I encourage authors to show CO as well for the completeness of the interpretation.

Page 15, lines 417 "high CH_4 emissions" What about CO_2?

âĂÍPage 17, line 455 “The westerly circulation (originated at longitude about 60E in 5 days back trajectories)” What does it mean? What’s originated? Talking about model? Based on modeled trajectories?

Page 17, lines 475-476 “CO2 mixing ratios whereas CO shows an âĂÍevening peak” It’s surprising. Why is it so? Please clarify.

Page 19, lines 519-521 “While the . . ... other seasons” I couldn’t follow how it’s related. A clarification is highly needed. What are these other most CO sources mentioned here?

Page 20, lines 551-559 “Highest day . . ...Mauna Loa and Waliguan” It’s lost. I see many assumptions here rather than convincing statements. What about biospheric activity and its seasonality? mesoscale transport mechanisms?

Page 21, lines 577-579 “Overall, the . . ... fire etc.” Importantly it shares the transport mechanisms.

Pages 21-22, Sec. 3.5 I strongly recommend authors to remove the whole section. It is not at all straight forward, as assumed here, to determine the impact of emission sources and transport, based on concentration measurements from single site. It does not make any sense unless a dedicated further study is involved to justify the stated assumptions here. The section, as in the present shape, does not meet scientific reasoning; hence need to be removed.

Page 23, lines 633-641 “Based on the . . ... post-monsoon seasons” This could be a likely scenario. Have these interpretations been supported by any emission inventories available? It is also important to point out the associated uncertainties involved in separating different emission sectors based on this approach. Note that this approach cannot separate near and far field sources, different lifetimes of tracers etc.âĂÍ

Page 25, lines 704-706 “but it is clear that” What makes it clear?

Page 26, lines 731-732 “Regional transport . . ... during pre-monsoon” âĂÍ I don’t see

any valid justification for this throughout.

Page 27, lines 743-744 “Low values . . ... mixing ratios.” Please provide supporting details.

Page 27, line 746 “useful for evaluation of satellite measurements âĂÍclimate” How?

Page 27, lines 747-749 “The analysis . . ... Kathmandu âĂÍValley. âĂÍ” Please remove this sentence. Note that this is not met here and the study only demonstrates the observational variations of GHGs in the study region.

âĂÍReference Please check. Formatting issues and sometimes journal details (or other important parts) are missing

Table 4 Columns 4 & 5 : What are these values? Looks like monthly values for Bode during Aug., and Sep. 2013; but then why are they different from corresponding 2nd column of the table 4?

Table 6 Column 2: This “*” meant for?

Figure 4 I didn’t follow the fig. well. What I understood is that the plot shows the frequency of hourly mixing ratios w.r.t the frequency of prevailing wind direction. Did it also take into account the wind speed? Then what about different percentages shown? For example, does it mean that 5% of sample time in August, the wind was from NE and “CH4_corrected” is above 2.5 ppm in which less than 1% time (in my eyes), CH4 is in 3-3.3 ppm range? In that case, it’s statistically difficult to say that the monthly enhancement is due to the polluted air masses from the NE and E. By the way, what are these “_corrected” values for CH4 and CO2? What about March-April scenario for CO2_corrected? Did two plots (Figs.2 and 3) use same set of master data, or any quality filtering had been done other than monthly averaging?

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