

***Interactive comment on* “Ground-based MAX-DOAS observations of tropospheric formaldehyde and comparisons with CAMS model at a rural site near Beijing” by Xin Tian et al.**

Anonymous Referee #2

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Content-Summary

The paper entitled “Ground-based MAX-DOAS observations of tropospheric formaldehyde and comparisons with [the] CAMS model at a rural site near Beijing” focuses on the analysis of MAX-DOAS measurements of HCHO from the University of Chinese Academy of Science (UCAS) approximately 50km away from the Beijing city centre during the period of October 1 – December 31, 2014. The study includes the period 3-12 November 2014, during which the Asia-Pacific Economic Cooperation (APEC) summit was held. During APEC the Chinese government implemented strict control measures on emissions allowing this to be a rare “clean” period.

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Not surprisingly the authors found decreased HCHO vertical column densities (VCDs) during the APEC period when compared to pre-APEC (-38%) and post-APEC periods (-31%). These reductions were thought to be from the control measures plus meteorological effects (predominant northwest wind fields bringing “cleaner” air to the site).

The authors also compared their HCHO VCDs with those determined using the Copernicus Atmosphere Monitoring Service (CAMS) model and found a good agreement ($R=0.83$). The trends of both data sets were similar, with the exception of the CAMS model’s HCHO VCDs being significantly lower during periods of minima HCHO VCDs. The author’s attribute this difference due to the CAMs model underestimating primary HCHO emissions.

Referee’s Recommendation

This paper has good scientific significance and quality; however the presentation of the material and analysis of the results needs to be improved. In particular, although the level of English is passable several repeatable errors in grammar interrupt the general flow of the paper. I have no provided no comments on these errors. The paper’s conclusions are fairly clear, but some of the detail of how the analysis was done is vague and needs to be more thoroughly explained. In particular, there is no mention of error analysis, while there are associated error bars on the figures, for example. At this time, I believe the paper is not ready for publication. However, if the comments outlined below are addressed I would be happy to consider a revised version for publication.

Scientific Comments

I have separated my comments into 2 categories (major and minor). Major comments require the author’s attention. See page and line as required. {Q} = questions, {REFs} = reference, {C} = comments.

Major

(1) Use of the Geometric Approximation Only

P5, I8: Your comment, "...it has lower systematic errors because of the geometrical approximation" needs to be backed up. Do you have a reference, maybe a short explanation of your reasoning? You need to somehow prove to me that it is better to use a geometric approximation of the VCD vs. one of several RTM/inversions approaches. You did not do this here, but rather allude to some other studies. I am not convinced that the geometric approximation is the best? Prove me wrong?

Alternatively, you could provide a comparison of your VCDs with RTM-inversion derived HCHO VCDs. I would also suggest that you provide a comparison of your HCHO VCDs to those measured via satellite. This would give me more confidence in your conclusions.

(2) Emission Totals from 2008 for CAMS model P7, I12: You use emission totals from 2008. Your year of study is 2014, that is a difference of 6 years and a lot can change. Why didn't you use a more recent emission inventory? Is there one? If so, why didn't you use it?

(3) HCHO VCD error

P9, I11-120: It is likely that when the wind comes from the south it is more polluted than when the wind comes from the north. However, an average HCHO VCD of $7.57 \cdot 10^{15}$ vs. $6.64 \cdot 10^{15}$ is hardly conclusive. This is a 14% difference. WHAT IS THE ERROR OF YOUR VCD? I would estimate that is it a least 10%, likely over 20%. As such, your statistics here are weak. Please define the error of your VCDs and then re-word this section. For example, in Figure 9, you have error bars on your VCDs, but no mention of how you calculate them [they also look very low to me].

P10, I6: This comparison shows a distinct difference between pre-, APEC, and post-periods. However, again, there is no mention of errors? Please revise including using appropriate significant figures. Did each period have the same number of data points?

(4) Figure 12 – Correlation Analysis

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I understand what you are trying to achieve here. However, I am not sure why you choose the period Oct 26 – Nov 20, 2014? This seems random? Why not use all your data?

The NO₂ VCD is not described. Is it VCDgeo? Is it data from the same instrument and time? Did you also compare your O₃ with the 7-9 & 16-18 periods? You don't have to show the plot but I would like to know the R of that? Hopefully it is very low to prove your point. Similarly, did you compare the NO₂ VCDs with the HCHO VCDs from 11-14 period. You need a more complete assessment here to really prove your point.

What happens if the R value for O₃ and 7-9&16-18 periods is also high? I believe you have something here but be careful about how you present it. I also need to know exactly where your O₃ monitor is, is it at ground-level?

(5) Assumption that the HCHO VCD is the correct result

On P12, I3 you state that the CAMS model UNDERESTIMATES

How do you know this? How do you know the MAX-DOAS result is the correct result and better than the CAMS model? What other VALIDATION do you have? Did you compare it to the satellite data; ground-data extrapolated to a column {see comment 1}? You may be right, but you may also be wrong. I am not convinced, especially without any error analysis of your HCHO VCDs or CAMS model. I would say that your CAMS model could be really off since it uses emission totals from 2008. Maybe the emission estimates in the model for 2008 are simply much lower than the 2014 values? You allude to this on P12, I14-15, right?

P12, I3: What do these ranges mean? Is it due to different grid-sizes?

(6) RMAX-DOAS vs Rmodel

P13, I10: Your R concept is interesting. Based on this I would think that R(DOAS) should be higher than R(model) for cases when the temp is cold (and secondary is HCHO is lower than predicted via the model), do you see this? Alternatively, if primary

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HCHO emissions are under predicted in the model R(DOAS) again would be higher than R(model) right? So what does this R concept really tell us? A graph like Figure 15, does not tell me much? However, if you separate out case studies maybe you get some more information.

P14, 121-23: If the CAMS model underestimates primary sources of HCHO then $R(\text{DOAS}) > R(\text{model})$ but “under a situation with a low temperature when the production rate of secondary HCHO is relatively low” won’t the CAMS model also underestimate the secondary HCHO production also causing $R(\text{DOAS}) > R(\text{model})$ as well? What is the assumed temp in the model, or does it use real-time met-data? How do we know what is the problem, is it a problem with the assumed temp, if so can you adjust that to check? OR is it a problem with the emissions inventory (perhaps a bigger issue).

Again, the above concept seems to have merit, but you need to develop this and explain it further, because I am somewhat confused. Also, despite your analysis I have no feeling as to how much HCHO is secondary and how much is primary (and isn’t that what the R calculations are for?).

Minor

P2, I3: {Q} Is the correlation coefficient ($R=0.83$)? If so, say ($R=0.83$, not ~ 0.83)

P2, I14: {Q} How is “APEC blue” defined? Perhaps a brief statement of how the actual reduction strategies were defined and the defined APEC levels would be useful? Is there an APEC-red for example?

P2, I16-17: {Q} Do you or the authors of the Wang et al. make any conclusion as to why the O3 rose to 189%? Does this have to do with being in a NO_x-limited or VOC-limited regime?

P2, I25-P3, I1: {Q} What were Zhang’s conclusions (briefly)?

P3, I1-2: ADD {REFS} for the published studies here.

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P3, I5: {REF} is not in your final reference list.

P3, I14-15: HO₂ and OH are radicals not ions, please correct this.

P3, I22: fix {REF}, you mean Honninger et al., 2004 right?

P4, I16: I would call it the Beer-Lambert Law

P4, I19: {Q} Were clouds a factor? How often was it cloudy? Was the data pre-screened in any way?

Figure 1: Change the colour red on your figure, it is hard to read.

P5, I21: ...some point sources (e.g. XX and YY). Add some key examples, factories or power plants?

Figure 2: fix the text on your figure (e.g. spectrograph as one word)

P5, I24: {C} change stepping motor to stepper motor?

P6, I2: {Q} Why was the temp set to 20C?

P6, I10: replace scanning times with SCANS

Figure 3: replace a₁, a₂, etc. with a₃ a₃₀ a₉₀ etc. {you don't need to number each one, simply add elevation angles to the alpha directly}

Table 1: fix text .. Longitude – one word, {Q} What is the MAYA? Is that Ocean Optics? If so, add that.

P6, I19: replace Doasis with DOASIS

P6, I22: replace [derived] with [measured] {as you did in your Figure4}

Figure 6 shows the period of 3-8 November. Why didn't you use the period of 3-12 November (the whole APEC period)?

P8, I11: You describe 2 peaks on Nov 4 and Nov 7, but what about Nov 3, as seen on

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Figure 6 that actually has the HIGHEST HCHO VCDs?

Figure 7: perhaps replace UTC time with LT for consistency.

Figure 9: Error bars equal retrieval error. {Q} How is this calculated?

P10, I19-20: Is there any way to determine which is more important, the control measures of the meteorology? Perhaps a longer term study? Please comment.

P11, I7-8: Could this have to do with a change in NO_x-limiting vs. VOC-limiting cases? Please advise.

P11, I14: Where was the surface O₃ measurement location exactly? What type of NO₂ VCD was it, geo-approximated, same instrument and location? Please describe.

P11, I18: Too many significant figures!

P12, I124: What is the assumed temp in the model for Dec 1, 2014 then?

P13, I16-20: Briefly state what associated errors clouds could pose. In I19 you say a slight variety (variation), give an error estimate please.

P14, I16: Where does this number come from and what dates? It is not the same as Figure 12 and it is not mentioned anywhere else in your paper. Is it a typo? Please advise.

P14, I20: Why the range? Grid sizes, I assume?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-440>, 2018.

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