Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-170-RC1, 2022

Comment on acp-2022-170

Anonymous Referee #2

Referee comment on "Evaluation of correlated Pandora column NO_2 and in situ surface NO_2 measurements during GMAP campaign" by Lim-Seok Chang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-170-RC1, 2022

General comments:
This study was performed based on the intensive field measurement data during the GMAP campaign in South Korea. The vision of this campaign is clear, the measurement looks well performed, and the analysis result looks interesting. But the key message is not well transferred to authors; In other words, authors need to spend more efferts to make a main root of this manuscript. This study actually focused on the relationship between the column density and surface level of not associated with the validation of satellite data (GEMS). But authors would like to underline the 'validation' purpose of GMAP, again, which is not clearly related to the main finding of this study. This mismatching makes the whole manuscript very distracted, hurts the organization, and finally results in the poor delivery of key points of this study. Thus, the major revision is largely required.

Specific comments:
- Whole manuscript: It seems that all results in this manuscript are related the GMAP 2020 (focus on Seosan), not GMAP 2021 (related to the Seoul Metropolitan). I strongly recommend to remove the statement of GMAP 2021 here. Namely, the result of GMAP 2020 is OK enough for this manuscript. It actually makes the key point more obvious because the data analysis using GMAP 2021 was not performed here. This problem is coming from authors' emphasis on the 'validation purpose' of GMAP campaign. Again, the results in this manuscript are not much related to the validation of GEMS data. This work mainly discussed the similarity/difference between the column and surface NO2. This topic is solely interesting enough. If the 'validation'-related wording is frequently raised in the manuscript, however, the merit of this study (analysis for the relationship between column and surface NO2) becomes weaker and readers would like to see the 'validation result' that is not included in this manuscript.
- Title: in this context, I would recommend to change the title, only focusing on the relationship between column and in situ NO2 during the GMAP 2020 campaign.
- Line 6 and whole manuscript: SI implies the 'surface in situ', but here the in-situ NO2 obtained from the aircraft measurement is rather utilized for the analysis (e.g., Figs. 7 and 8), which was collected in the same GMAP 2020.
- Line 9-10: This is a repetition of previous statements in line 5-6.
- Line 16-18: High wind speed and PBL height suppress the fluctuation of NO2? Why? Usually mixing is enhanced by the high wind speed, then the fluctuation becomes larger.
- Line 25-27: This is not the good conclusion after the GMAP 2020 campaign. Everybody knows the difference between column and surface NO2 value. This study can have a merit

because the difference between column and surface NO2 was analysed and diagnosed in detail using dense measurement of data. Please make a better statement to underline this merit.

- Line 39 and whole manuscript: 'column density' sounds much better that 'column amount'
- Line 39-41: Is there no equation number? And what is the reference of this equation?
- Line 42-57: Based on this paragraph, readers expect to see the importance of 'a priori vertical profile' for the accuracy of column data. But that is not the key point of this study right? The main finding of this study is the examination for the relationship between column and surface NO2 in terms of meteorological pattern, acquired by the clustering. Again, the background work suggested in the introduction chapter is not well connected to the key point of this study.
- Line 58: The meaning 'weak vertical profile correlation' is not clear. what is the profile correlation?
- Line 63-64: The meaning of statement "found that they originated ... from the surface layer" is not clear. How this explains the weak correlation between PC and SI NO2 during the KORUS-AQ? What is different from Wang and Christophere (2003) showing the high correlation in Alabama?
- Line 85-86: 'Low-orbit' and 'geostrationary' cannot be used together.
- Line 110-123: I would recommend to focus on the result of GMAP2020 campaign only. The result in GMAP2021can be a PART 2 paper in the future.
- Line 133: What is LPS?
- Line 113-114: Reference?
- Line 158-159: Cloud cover 0.6 looks a loose criteria. Is PC NO2 quality OK under the cloud cover = 0.5 (50%)? If yes, how is it justified?
- Line 162-163: I think that this data to 30 October 2021 are not part of GMAP 2021. Again, the solely usage of GMAP 2020 data looks meaningful and better to derive the obvious key message from this work.
- Line 179: 'NIER-GP2021-002' this format is right as the reference of ACP?
- Line 205-222: The methodology of clustering is not clear. The minimum amount of basic theoretical description is necessary. Here, the usage of XLSTAT software is the only clear part related to the conduction of clustering analysis, which does not look enough.
- Line 213-214: How did authors hypothesize this? Actually I can accept this idea, but in the manuscript, the reliable logic / scientific reason is needed to have a hypothesis.
- Line 239: Why the correlation is estimated in a 'log' scale?
- Line 242-244 + Fig. 3: This different correlation looks very interesting but there is no explanation about this. Some ideas to descibe this difference should be added here.
- Line 245-247: This is associated with the colume NO2 or surface NO2? It is not clear.
- Line 266-269: How to find the PBLH using the HYSPLIT simulations? The method is unclear.
- Line 270: Which region relates to this PBLH information? Region is unclear.
- Line 278: What is SBI?
- Line 284-288: The statement is not well connected to the previous sentence. Please improve.
- Line 289-294: I do not the function of this paragraph. Why do readers think this information related to the result of this study? What can readers know better based on this paragraph?
- Line 298-310: Based on my understanding, authors addressed that the PC-NO2 and SI-NO2 shows good correlation if PBL inside is well-mixed, and this mixing condition is determined by the meterology pattern, therefore we need to consider the meteorological pattern more significantly for the analysis of relationship between column and surface NO2. Am I right? If right, authors need to put the weight more on the role of homogeneity in the PBL to the correlation between column and surface NO2. Was it found before? If yes, discussion with some previous reference is necessary. If not (i.e. this work is the first to show the importance of PBL homegeneity related to the correlation between column and surface NO2), this should be more underlined.

- Line 339-341: Reference?
- Line 368-370: How can be the NO_2 (short lifetime) transported across the Yellow Sea? It is very debatable. Please add some discussions if authors would state the possibility of NO_2 long-range transport with several citations.
- Fig. 1: Recommend to have the GMAP 2020 information only.
- Fig. 3b: This contrast is frequently found or one of correlation is a really irregular one? It requires more and deeper statements.
- Fig. 5: Left figure is for the surface NO_2, but right figure is for the surface 'delta' NO_2, which are different from the absolute value. Please improve the figure caption for better explanation of figures.
- Fig. 7 and 8: Figure caption should be corrected. Fig 7 is for flight 5 and 7, but Fig 8 is for flight 1 and 3, so the date and information in detail is different.