

## Response to reviewer#1

A few technical correlations:

line 227: "7 km×7 km before August 2019" is better. Operational TROPOMI measurements began in May 2018.

Response: Thank you, we revised accordingly.

Line 347: Either remove or provide further clarification for this sentence: "Moreover, there is a very small fraction of negative values for enhancement calculated based on SDZ as background, indicating a not real background for all climatic conditions, and this message is important in atmospheric inversions." To me, occasional negative enhancements for a small temporal grid are not very surprising, as even the background sites can have random analytical errors and be affected by emissions.

Response: Thank you, we removed this sentence.

Line 410: "TPOMI"=> "TROPOMI"

Response: Thank you, we revised accordingly.

Line 453: "introduced wrong message in both" => "introduced errors in both"

Response: Thank you, we revised accordingly.

Line 493: "system errors": Do the authors mean "systematic errors"?

Response: Yes, we mean "systematic errors" and revised.

Supplementary Information: The figures and their captions are often on different pages. Please fix this formatting issue.

Response: Thank you, we revised accordingly.

## Response to reviewer#2

Dear Editor and Authors:

Overall, the authors have improved their work greatly as compared with before. Their analysis is deeper. They have referenced more papers. They have started to work with higher frequency data in space and time. They have talked more about strengths and

weaknesses of existing platforms. They have provided access to their dataset, which will allow the community to continue to grow and improve.

In addition to all of this, the authors mention an important caveat of their paper in their response, and I hope that they place it clearly in the paper's final version. The meaning is that the surface data provides a more accurate representation of higher CH<sub>4</sub> loadings than observed by TCOON, and even more so than from TROPOMI. While they have not deeply performed this analysis of how much an impact of a low bias solely relying on low-resolution (space and time) models and observations may have, they clearly demonstrate this. Placing some comments about this weakness or in the direction of future research will add to the value of the paper, since it is via high frequency surface measurements like those provided, that such future improvements by the community can be made.

“ Indeed, the surface CH<sub>4</sub> concentrations at XH site observed by Picarro showed differences when retrievals were successful and not for TCOON (Fig.S14). And when retrievals were not successful, surface CH<sub>4</sub> usually reached peaks.”

Response: Thank you for this good point, and we added this important caveat in lines 381-383.

One final issue I wanted to raise is my question about elevation was not regarding the model, but instead how the observations were calibrated with respect to the reduced air pressure found in Shanxi and other locations in China's center and west, which are at a higher elevation than those from the TCCON stations. I wanted to check if such a concentration calibration correction was applied, and if so, how well it performed.

Response: Thank you for this question, and we added the discussions and short comings for this question in lines 384-389. The reduced air pressure were not considered in the comparisons across different sites (Beijing, Xianghe, Shangdianzi, and Hefei), and the elevations of these four sites were less than 500m, which is within the boundary layer and mixed well for CH<sub>4</sub> concentrations. But we acknowledge that it is better to consider this factor when comparing with the higher elevation observations such as in Shanxi sites (e.g. higher than 2 km).

I strongly support the current version's publication, once a few minor spelling, grammar, and organizational issues are clarified, and this caveat that the authors point out their data can offer an improvement to from the current community approaches, is more clearly written into the conclusions/findings. I am certain that it will make an important and positive contribution to the understanding of methane concentrations and their changes in China.

Response: We check the MS thoroughly and revised small errors (e.g. lines 416, 457, 496). We thank you for the understanding of this paper and for the careful review, which substantially improved the quality of this paper.