The manuscript titled "**Aggravated surface O_3 pollution primarily driven by meteorological variation in China during the early COVID-19 pandemic lockdown period**" assesses the individual effects of meteorology and emission reductions on O_3 changes during COVID-19 lockdown by using GEOS-Chem model. By updating the emission inventories using satellite measurements and further validation of model performance, the study provides a more solid analysis for this issue. The article is well organized. It can be accepted after considering the following suggestions.

This study has validated the model performance in simulating NO_2 by comparing with observed NO_2 column. However, the VOCs simulation is bot validated. At least, the model simulated HCHO column can be validated by comparing with observed HCHO column.

The study finds that the large effect of meteorological changes on O_3 changes during lockdown period. However, they only discuss the effect of temperature and solar radiation. The influence of other meteorological factors such as humidity and wind speed should be also discussed.

In the abstract, I suggest to mention the time period that you focus on is from 2019 to 2020, rather than from before lockdown to the lockdown period in 2020.

The emission inventory used here is MIX 2010. Why not using MEIC inventory which have updated emissions in 2019 or 2017?

Lines 216-218: Why does the change of anthropogenic VOC emissions from 2010 to 2019 can be ignored?

Lines 222-225: It is unclear about how you derived the VOCs emission during lockdown 2020. Do you mean that you derive VOCs emission according to the ratio of HCHO column between lockdown period and before lockdown period and the VOCs emissions in 2020 before lockdown period? If so, the VOCs emissions in 2020 before lockdown period should be introduced here. In addition, why not using the ratio of HCHO column in 2019 to that in 2020 to update the VOCs emission?

You have mentioned that the large uncertainty in HCHO prevent accurately quantifying the emission decline. I didn't see that the scaling method that you adopted to update VOC emissions can solve this problem.

Figure 8. The observation shows O_3 concentrations in the Northern China is higher than in the southern China, while the model simulation shows an opposite spatial distribution especially for 2020. The author should clarify the reason for the inconsistency and how this affects your major conclusion about the relative importance of meteorology and emission reductions in O_3 changes.

Figure 8. In Sichuan Basin, the relative difference from model simulation is negative while the observed relative difference is positive. The opposite trend between model simulation and observation is also displayed in Shandong province. The underlying causes should be clarified.

Lines 497-499: The results of changes in the cloud fraction and the downward visible direct flux should be provided.

Lines 505-506: Why does the intercept is negative in North China?

Figure 1. It should be specified that which is minuend and subtrahend for the calculation of relative difference.