Comment on acp-2021-604

Anonymous Referee #2

Referee comment on "COVID-19 lockdown emission reductions have the potential to explain over half of the coincident increase in global atmospheric methane" by David S. Stevenson et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-604-RC2, 2021

The manuscript discusses the impact of NOx emission reduction on the atmospheric CH4 growth rate associated with COVID-19 lockdowns around the world. They show that the changes in NOx are the key factor that drive the CH4 changes. The research topic is important for understanding the role of anthropogenic emissions on atmospheric oxidability and the CH4 budget. However, I cannot agree that the simple calculation presented in this study can support the conclusion.

1. My main concern is that the authors directly apply the sensitivity of CH4 to NOx emission changes estimated by previous studies for broad regions using different models for different periods. Since the OH chemistry is higher nonlinear, such estimation can lead to a large bias. For example, if we use the sensitivity of OH to NOx emission changes in the N. Hemisphere (-0.39) and the 16.72Tg NOx emission changes to estimate CH4 changes, we get 6.5Tg CH4 changes in the N. Hemisphere, much smaller than the 8.5Tg when considering sensitivity in 4 different regions. The sensitivity given by Wild et al. (2011) and Derwent et al. (2011) are estimated by perturbing the emission for the whole year, but the lock-down time-period are different in each country. For example, the emission reductions in China (East Asia) mainly occur during February, and gradually back to normal from April (Fig.6 in Miyazaki et al. (2021)). In winter the sensitivity of CH4 to OH may be much lower than in other seasons (low OH production and CH4+OH reaction rate). Thus apply the sensitivity estimated for the whole year may lead to overestimation of CH4 changes. In addition, most of the sensitivities in table 1 are estimated based on simulation for 2000 or earlier. Changes in global emissions from 2000 may influence the sensitivity of OH to precursor gases.

I agree that the reduction in NOx can contribute to the rising CH4 during 2020, but I don't think the 4.9ppb increase in the CH4 mixing ratio estimated in this study is reliable considering the nonlinearity in OH chemistry. Besides, emissions of other chemical species such as CO also changed during the lockdown period. So, the conclusion that "the NOx changes can account for all or most of the observed methane changes" cannot be supported by the simple calculation present in the manuscript. The changes in emissions

are already available (Lamboll et al. 2021). I recommend the authors quantify the sensitivity of OH to emission reductions by conducting model simulations for 2020.

2. Most of the sensitivity of CH4 to NOx emissions changes listed in Table1 is not the original data that we can find from the references. I think the authors should clarify how they convert the data from the references to which is listed in table 1 in the supplementary.