Response to Referee #2:

We appreciate the very helpful feedback from the referee. The referee's comments are listed in *italics*, followed by our response in blue. New/modified text in the manuscript is in **bold**.

This article estimates NOx emissions and lifetimes in the Po Valley of Italy using OMI and TROPOMI satellite retrievals before the current pandemic, and then uses the methods to estimate the reductions in emissions during the pandemic.

The authors are to be congratulated for a thorough and methodical analysis and for interesting results and a topical analysis. I am happy to recommend publication.

Really my only comment is the reference to DOFS. The acronym should be spelled out in the caption for Fig 10. The method should then be explained and referenced in the main text.

The DOFS is now spelled out in the caption. The sentences at lines 294-299 of the original manuscript are updated to explain the DOFS:

"**The degrees of freedom for signal (DOFS) of retrieved emission rates and chemical lifetimes, shown by Figure 10b and c, are the diagonal elements of the averaging kernel matrix as given in Appendix B. The DOFS quantifies the number of pieces of information retrieved from observation for a specific state vector element (Rodgers, 2000; Brasseur and Jacob, 2017). The observational information content of τ_c for each calendar month, as indicated by the DOFS, is only ~0.02 (Figure 10b). This implies that the chemical lifetimes for calendar months are dominated by prior influences from the climatological chemical lifetimes, which reflects our trade-off between emission rates and chemical lifetimes by applying relatively strong prior regularization to τ_c in each calendar month.**"

About the seasonal signal in NOx emissions, the authors do note that "future work is suggested to better understand its variability." This is indeed the case – it does seem more likely that the variability is related to the method and the estimates of the lifetimes than to actual variations in emissions. I would recommend some more discussion of this point – either what might cause changes in actual emissions, or how the method can have biases that vary by season.

The sentence "future work is suggested to better understand its variability" was about the variability of NO_x:NO_2 ratio, instead of NO_x emissions. It has been removed as we have updated the manuscript to include observation-based NO_x:NO_2 intra-annual variability according to another referee's comments. Sentences at lines 381-383 of the original manuscript are updated to discuss the observation-based NO_x:NO_2 ratio:

"**Monthly climatological NO_x:NO_2 ratio derived from ground-based observation network is used to convert NO_2 abundance to NO_x abundance, which improves upon the fixed value used in previous studies (Beirle et al., 2011, Valin et al., 2013, de Foy et al., 2015, Liu et al., 2016). However, uncertainty remains from contamination of NO_2 in-situ measurements (Visser et al., 2019) and the representativeness of surface-based NO_x:NO_2 ratio to the column-integrated one due to vicinity to emission sources and local ozone titration. Moreover, long-term trend in NO_x:NO_2 may exist as observed in The Netherlands by Zara et al. (2021),**

although biases in NO_x:NO_2 have limited impacts on chemical lifetime and relative emission change estimates."

One more sentence is added after these to discuss the emission variation by season:

"**The seasonal variability of estimated NO_x emission is determined by the seasonal variabilities of NO_2 TVCD, chemical lifetime, and NO_x:NO_2 ratio. We attempt to characterize these variabilites using as much observational data as possible, and yet future investigations are still needed.**"