

## Review of Kang et al., 2024 (ACPD)

Title: *Effects of 2010-2045 climate change on ozone levels in China under carbon neutrality scenario: Key meteorological parameters and processes*

The overall manuscript is well-documented, but I have some major concerns and suggestions for improvement:

1. The title emphasizes the effects of climate change but does not highlight emissions, which have a much higher impact on ozone levels compared to climate change. Since the manuscript examines both, the title should reflect the role of emissions more explicitly.
2. GCAP2.0 is a one-way offline model, and the meteorology you used to drive GEOS-Chem is parameterized. In the "Results" section (e.g., Figure 1), meteorological variables are shown. Are these variables inputs or outputs of the model? Please clarify. Furthermore, it is crucial to clearly define what is considered a climate variable in this study and describe the differences in these variables between present-day and future scenarios, similar to the approach used for emissions (Section 2.2.2). Additionally, since GCAP2.0 is a one-way offline model, do changes in emissions have any feedback effect on meteorology? I assume not, but this should be explicitly addressed.
3. The manuscript frequently discusses regions like EC, NCP, or YRD. Instead of presenting results for all of China, zooming in on these regions while plotting would provide more clarity, particularly for localized changes.
4. The model's performance in capturing present-day results (e.g., Figure 3) is concerning. For instance, the MAM R-value is only 0.12, indicating a poor representation of trends. This raises questions about the reliability of future projections. Moreover, your results are at the lower end of CMIP6 model projections. Please provide a detailed explanation of why GCAP behaves differently, even for regional means.
5. For difference plots of the same variable, use a consistent color scale to facilitate comparison of magnitudes across different forcing factors. For example, in Figure 4, ensure the scales for "climate," "emissions," and "combined" effects are the same.
6. "Climate + Emissions" represents the combined effect of both forcings. Have you tried linearly summing the individual effects of climate and emissions and comparing this sum to the combined effect? If not, this analysis should be performed and discussed.
7. BVOC emissions are included in the "emissions" forcing. Since MEGAN is used, "climate" forcing also influences BVOCs. This raises the possibility of double-counting BVOC emissions in the combined effect. If double-counting is not an issue, please clarify this in the manuscript.
8. The manuscript states that meteorology explains 58–76% of the total change, yet net chemical production is described as the most important process. This appears contradictory. Please reconcile and clearly quantify the contributions of meteorological and chemical factors to the total change.
9. The manuscript omits some recent global studies on the climate effect on ozone, such as Bhattacharai et al. (2024) (STOTEN; <https://doi.org/10.1016/j.scitotenv.2023.167759>). Discussing your findings in the context of these studies would strengthen the manuscript.

10. Figure 1: Clearly indicate what the difference plots represent in the caption and text. For example, is the change shown as  $CfutEfut - CpdEpd$ , or is it only the effect of climate? The figure caption should be self-explanatory.
11. Line 409: There is no section called 5.1
12. Consider adding a discussion on the policy-relevant implications of the carbon neutrality scenario towards the end of the manuscript.
13. The carbon neutrality target is 2060, but you selected 2045 as the endpoint of your analysis. Is there any reason behind this?