The manuscript titled "Impacts of meteorology and emissions on surface ozone increases over Central Eastern China between 2003 and 2015" present a very interesting and useful model study showing an increase in surface ozone over Central Eastern China (CEC) between July 2003 and July 2015 which is in agreement with recent studies (Xu et al., 2016, 018; Lu et al., 2018; Gaudel et al., 2018).
According to this present study, emission changes have a higher impact on the Maximum Daily 8-h Average of ozone (MDA8 ozone) than the meteorological changes. However the meteorological changes would better explain larger ozone increases (delta MDA8 ozone >= 10 ppbv between July 2003 and July 2015) than the emission changes. By this latter result, the authors would like to point out that, the long-range transport of ozone and its precursors from neighboring areas should be taken into account, for air pollution control.

The manuscript is well written and the quality of the text and its structure is very much appreciated. However I suggest major revisions, especially regarding one main conclusion of the manuscript that would need more evidence. Indeed, the impact of the transboundary transport on the surface O_3 above CEC that would explain an increase of surface O_3 between July 2003 and July 2015 is not very much convincing as it is written now.
You can find below general comments followed by more specific comments.

<u>General comments</u>:

1) I would suggest the authors to cite the following recent studies to put even better in perspective the increase of surface ozone between July 2003 and July 2015 using long-term time series:
Xu et al. (2016, 2018), Lu et al. (2018) and Gaudel et al. (2018)

Xu, W., Lin, W., Xu, X., Tang, J., Huang, J., Wu, H. and Zhang, X., 2016. Long-term trends of surface ozone and its influencing factors at the Mt Waliguan GAW station, China–Part 1: Overall trends and characteristics. *Atmospheric Chemistry and Physics*, *16*(10), pp.6191-6205.

Xu, W., Xu, X., Lin, M., Lin, W., Tarasick, D., Tang, J., Ma, J. and Zheng, X., 2018. Long-term trends of surface ozone and its influencing factors at the Mt Waliguan GAW station, China-Part 2: The roles of anthropogenic emissions and climate variability. *Atmospheric Chemistry & Physics*, *18*(2).

Lu, X., Hong, J., Zhang, L., Cooper, O.R., Schultz, M., Xu, X., Wang, T., Gao, M., Zhao, Y. and Zhang, Y., 2018. Severe surface ozone pollution in China: a global perspective. *Environmental Science & Technology Letters*.

Gaudel, A., Cooper, O.R., Ancellet, G., Barret, B., Boynard, A., Burrows, J.P., Clerbaux, C., Coheur, P.F., Cuesta, J., Cuevas Agulló, E. and Doniki, S., 2018. Tropospheric Ozone Assessment Report: Present-day distribution and trends of tropospheric ozone relevant to climate and global atmospheric chemistry model evaluation.

2) I find hard to understand why the authors have chosen the two periods July 2003 and July 2015. Could the authors explain more this choice?
According to the Tropospheric Ozone Assessment Report database (https://join.fz-juelich.de) and seasonal cycles studies (e.g. Sun et al., 2016), the month with higher ozone for most of the sites above CEC would be June. Why would the authors choose July?
In addition, according to Figure 1 of the present manuscript, the observations of surface ozone are available mostly in 2004, why would the authors choose 2003 instead of 2004?
It should be clarified in the text.

3) I didn't find a strong argument supporting the impact of transboundary transport on the surface O_3 above CEC that would explain the increase of surface O_3 between July 2003 and July 2015. The authors should add more evidence or be clearer in their analysis.

Specific comments:

Title:

I would suggest to add "July" in the title.

Abstract:

Line 23 p.1: Change "The increase in regional averaged O_3 resulting from…" to "The increase in averaged O_3 in the CEC region resulting from…"

Sections 2 to 6:

Line 19 p.4: The authors are using the global chemical transport model GEOS-Chem v11-01 but the current version reported on the website cited in the manuscript is v11-02. Would the use of v11-02 instead of v11-01 change the results? Could the authors add a word about it in the text?

Line 28 p.4: Could the authors say how long they estimate the spin-up? How many month?

Line 17-20 p.6: I would suggest to change "The contribution of anthropogenic NOx (NMVOCs) emission changes can be calculated by the difference between the 2015 standard simulation and 03N15M (03V15M), defined as 15E15M-03N15M (15E15M -03V15M). " to "The contribution of anthropogenic NOx and NMVOCs emission changes separately can be calculated by the difference between 15E15M (the 2015 standard simulation) and 03N15M (the 2003 NOx emission simulation), and between 15E15M and the 2003 NMVOCs emission simulation (03V15M)."

Line 25 p.6: It is very useful to cite the website link of the Chinese Data Center but unfortunately, there is no English version. Do the authors know whether it is planned to implement the English version? If yes, could the authors say a word about it?

Line 27 p.6: The authors used the word "background sites". If the authors refer to observed ozone at sites which are not influenced by recent, locally emitted or produced anthropogenic pollution, they should use the word "baseline" instead for consistency purposes (Cooper et al., 2014; Dentener et al., 2011)

Cooper, OR, Parrish, DD, Ziemke, J, Balashov, NV, Cupeiro, M, Galbally, IE, Gilge, S, Horowitz, L, Jensen, NR, Lamarque, J-F, Naik, V, Oltmans, SJ, Schwab, J, Shindell, DT, Thompson, AM, Thouret, V, Wang, Y and Zbinden, RM. 2014. Global distribution and trends of tropospheric ozone: An observation-based review. *Elementa: Science of the Anthropocene* **2**. DOI: https://doi.org/10.12952/journal.elementa.000029

Dentener F, Keating T, H Akimoto H, eds. 2011. Hemispheric Transport of Air Pollution 2010: Part A: Ozone and Particulate Matter. *New York: UN*. (Air Pollut. Stud, vol. 17).

Line 4 p.7: Why the authors didn't choose the year 2004 for all the sites? Is July 2003 comparable with July 2004?

Line 12-14 p.7: Is the simulation for 2004 the same as for 2003? Does it start in January 2004? Could the authors make it clearer?

Line 25-30 p.7: Could the authors explain how they chose the “nine representative sites”? Would it be possible to show the diurnal cycle from observations and the simulations for the July month for each site with the standard deviation? The comparison observations/model, day/night would be more straight forward.

Line 3-4 p.8: Does the trend of observed O_3 at Mt Tai come from Sun et al. (2016)? The paper should be explicitly cited. Does the simulated increase of about 1.3 ppbv yr^{-1} refer to the same model: nested version of Geos-Chem or GFDL-AM3?
I would suggest showing the time series observations/model in a figure in the supplement material.
Could the authors report the 95% confidence intervals with the trends?

Line 15 p.8: Is the increasing rate calculated from a delta between 2 years looking at one month comparable to the increasing rate calculated from a full time series?
Are the authors sure of the rate of 0.74 ppbv yr^{-1}: 74.4 ppbv compare to 65.5 ppbv, 11 years apart would give around 0.8 ppbv yr^{-1}, wouldn’t it?

Line 29-30 p.8: I am not sure to understand the reason of the choice of focusing on MDA8 O_3 and not the daily mean. Could the authors clarify this point?

Line 9 p.9: Please clarify “domain-averaged”?

Line 13 p.10: I would suggest to explain Table 2 earlier in the text when the authors first refer to the Table in section 3.2.

Line 15 p.11: “even if” would suggest that the authors would expect another result for ΔMDA8 O_3 greater than 10 ppbv. If it is the case, could the authors say a word about what result they would expect?

Line 4 p.12: Do the authors still mean MDA8 O_3 using the words “O_3 concentrations”? Please be consistent.

Line 22 p.12: Remove “in”.

Line 7-9 p.13: According to Table 4, the larger O_3 flux in 15E03M would be 1906 Gg mon^{-1} and not -1232 Gg mon^{-1}, is it correct? Could the author explain where -1232 Gg mon^{-1} come from?

Line 14 p.13: Change “excessive O_3 production” to “excessive net O_3 production”

Line 25-26 p.13: Could the author rephrase the sentence? Half of photochemically formed O_3 in the CEC region in July 2003 can not be removed by transport in July 2015 as it is not the same air masses 11 years later.

Line 27-28 p.13: The sentence “[…] the absolute value of O_3 transport flux increased by 395 Gg mon^{-1}” is confusing. According to Table 4 all fluxes for both horizontal and vertical transport are actually decreasing when comparing simulation 03E03M with 15E15M. This is in agreement with stronger winds in July 2003 than in July 2015 shown in Figure S6.
The sentence needs to be rephrased.

Line 30-31 p.13: "As a result, the increase in O_3 concentrations from July 2003 to July 2015 is mainly due to transboundary horizontal transport, vertical transport and photochemical reactions."
Regarding the transboundary horizontal transport, what would be the source regions of O_3 that would affect O_3 above CEC? Regarding the vertical transport, do the results imply there is an increase in stratospheric intrusion?
According to Figure S6 that shows weaker winds in July 2015 than in July 2003, the transboundary horizontal transport doesn't seem to be a major process that would explain an increase in surface O_3 above CEC. Indeed, from Table 4, the local photochemical processes rather than the transport processes seem to be leading the increase in O_3. The slow changes in time of the dry deposition process could also explain the increase in surface O_3 because it cannot compensate the increase in net photochemical production of O_3.

Conclusions:
Line 4 p.14: Would it be possible to put the numbers in perspective with long-term time series study above individual sites such as Mount Waliguan, Mount Tai, Shangdiazi?

Line 14-15 p.14: The terms "transport" can refer to the winds, which are part of the "meteorological" condition. Could the authors explicitly use the words "winds", "humidity" and "temperature"? That would help the reader.

Line 24-25 p.14: To my opinion, the manuscript does not show a strong argument to support the theory of the impact of transboundary transport on surface O_3 above CEC. Could the authors be clearer and bring more evidence?