General comments and responses:

General Comment#1: It is stated that WRF has been evaluated extensively with respect to meteorology, but no references are given. In this context an evaluation of the model against meteorological observations within the domain of interest is needed. If no references can be found, this evaluation should be included in this manuscript.

Response: We agree with the referee that necessary references should be provided regarding the performance of WRF over China. We have added the following references at line#147 in the revised manuscript. These references were selected as representative because they applied the similar versions or configurations of WRF during their simulations. These recent publications provided detailed evaluations and demonstrations of the meteorology simulation performance of WRF in China.

Reference:

Gao, Y. Q., Lee, X. H., Liu, S. D., Hu, N., Hu, C., Liu, C., Zhang, Z., and Yang, Y. C.: Spatiotemporal variability of the near-surface CO2 concentration across an industrial-urban-rural transect, Nanjing, China, Sci Total Environ, 631-632, 1192-1200, 2018.

Tang, J. P., Niu, X. R., Wang, S. Y., Gao, H. X., Wang, X. Y., and Wu, J.: Statistical downscaling and dynamical downscaling of regional climate in China: Present climate evaluations and future climate projections, J Geophys Res-Atmos, 121, 2110-2129, 2016.

Wang, W. G., Shen, X. Y., & Huang, W. Y. (2016). A Comparison of Boundary-Layer Characteristics Simulated Using Different Parametrization Schemes. Boundary-Layer Meteorology, 161(2), 375-403. 10.1007/s10546-016-0175-4

Yang, Y., Hu, X.-M., Gao, S., & Wang, Y. (2019). Sensitivity of WRF simulations with the YSU PBL scheme to the lowest model level height for a sea fog event over the Yellow Sea. Atmospheric Research, 215, 253-267. https://doi.org/10.1016/j.atmosres.2018.09.004

General Comment#2: The authors claim that the WRF-VPRM model can be used to assess carbon budgets related to biospheric fluxes and to anthropogenic emissions. However, it should be clear that VPRM is a highly simplified light use efficiency model that represents upscaling of flux observations from eddy covariance measurements made over Europe, which would need further optimization through inverse modelling (see e.g. Kountouris et al., 2018) even for the European domain. Applying the same VPRM parameters to a different domain will result in even larger errors in fluxes. Furthermore, anthropogenic fluxes are simply used as input to WRF-VPRM, thus regional carbon budgets can directly be derived using the emission inventory data themselves.

Response: We totally agree with the referee that applying the same VPRM parameters to different domains will result in uncertainties, we have added Kountouris et al. (2018) as a reference to support the discussion of model uncertainty. We mentioned the parameterization issue at several places in the original manuscript (for instance, line#111, 173, 176), and pointed it out in the "Conclusion" section (line#337) that VPRM parameterization need further improvement. To our knowledge, WRF-VPRM was first applied to Europe domain by Ahmadov et al. (2007), and the parameters followed Mahadevan et al. (2008) which were derived from eddy covariance measurements collected at 22 towers in North America (5 towers in Canada and 17 towers in United States, see Table1 in Mahadevan et al., 2008). Ahmadov et al. (2007) mentioned that the parameters were slightly modified but the values were not reported. For our study, we used the calibrate VPRM parameters for different vegetation types by using observed NEE from a group of 65 eddy covariance tower sites over North America, and using these parameters over US domain also has uncertainties (Hu et al., 2020). For WRF-VPRM application in China, Li et al. (2020) evaluated the parameterization with eddy covariance data collected at two sites in northeast China, and reported that the default parameterization can successfully reproduce the temporal variations and intensity of biospheric fluxes, but also pointed out that the bias over mixed forest site should be due to the VPRM parameterization. Unfortunately, Lin'an tower didn't have eddy covariance measurements to support the modification of VPRM parameters, so we tried to use the hourly CO2 concentration measurements to reveal the uncertainty of the model. It is true the parameters can be further calibrated using tower flux data over China, and we hope there will be more efforts devoted to collect such data over different land categories in the near future. Inverse modeling is certainly one of the options to help verify or indicate the uncertainty of biospheric model, but it also retains uncertainty such as being sensitive to the formulation of prior flux. Thus research efforts are needed to improve both of them as has been recognized by the community (Kondo et al., 2020). Justification/evaluation of an appropriate prior flux from WRF-VPRM over China is one of the objectives of this study. Following Li et al. (2020) which was the first study discussing VPRM uncertainties associated with parameterization in northeast China, this study intended to investigate the case in south China with Lin'an tower data, which is a necessary step for future inverse calibration or calibration using flux tower data. Despite the uncertainties associated with the VPRM parameters, Li et al. (2020) and this study demonstrated that WRF-VPRM captured many characteristics of CO2 fluxes/concentrations, including seasonal/episodic/diurnal variation of fluxes/concentrations. We acknowledge the uncertainties associated with WRF-VPRM, that is why we'd like to use tower data to evaluate and understand the uncertainties in this study, which could guide future calibration of VPRM in the region. In terms of anthropogenic flux, the anthropogenic emission int this study was from Open-Data Inventory for Anthropogenic Carbon dioxide (ODIAC) emission version 2018, which had its own uncertainties and cannot be treated as truth. All the WRF-VPRM uncertainties associated with fluxes must be evaluated/examined by more atmospheric observations, this study is just one of such attempts. We fully agree with the referee that it is necessary to improve VPRM parameterization based on local eddy covariance data, and pointing this out is one of the objectives of this study. In fact, East Asia is one of the regions having largest uncertainty in CO2 budget estimation (Kondo et al., 2020). Our study is one of the attempts to help improve the understanding in this area with biospheric modeling method, and certainly more observational and modeling efforts are necessary to reduce the uncertainty in the future. To address the reviewer's concerns, we emphasized these points in the revised manuscript.

Reference:

Ahmadov, R., Gerbig, C., Kretschmer, R., Koerner, S., Neininger, B., Dolman, A. J., and Sarrat, C.: Mesoscale covariance of transport and CO2 fluxes: Evidence from observations and simulations using the WRF-VPRM coupled atmosphere-biosphere model, J Geophys Res-Atmos, 112, 2007.

Hu, X. M., Crowell, S., Wang, Q. Y., Zhang, Y., Davis, K. J., Xue, M., Xiao, X. M., Moore, B., Wu, X. C., Choi, Y., and DiGangi, J. P.: Dynamical Downscaling of CO2 in 2016 Over the Contiguous United States Using WRF-VPRM, a Weather-Biosphere-Online-Coupled Model, Jounal of Advances in Modeling Earth Systems, 12, 10.1029/2019MS001875, 2020.

Kondo, M., Patra, P. K., Sitch, S., Friedlingstein, P., Poulter, B., Chevallier, F., Ciais, P., Canadell, J. G., Bastos, A., Lauerwald, R., Calle, L., Ichii, K., Anthoni, P., Arneth, A., Haverd, V., Jain, A. K., Kato, E., Kautz, M., Law, R. M., Lienert, S., Lombardozzi, D., Maki, T., Nakamura, T., Peylin, P., Rodenbeck, C., Zhuravlev, R., Saeki, T., Tian, H. Q., Zhu, D., and Ziehn, T.: State of the science in reconciling top-down and bottom-up approaches for terrestrial CO2 budget, Global Change Biol, 26, 1068-1084, 2020.

Li, X. L., Hu, X. M., Cai, C. J., Jia, Q. Y., Zhang, Y., Liu, J. M., Xue, M., Xu, J. M., Wen, R. H., and Crowell, S. M. R.: Terrestrial CO2 Fluxes, Concentrations, Sources and Budget in Northeast China: Observational and Modeling Studies, J Geophys Res-Atmos, 125, 2020.

Mahadevan, P., Wofsy, S. C., Matross, D. M., Xiao, X. M., Dunn, A. L., Lin, J. C., Gerbig, C., Munger, J. W., Chow, V. Y., and Gottlieb, E. W.: A satellite-based biosphere parameterization for net ecosystem CO2 exchange: Vegetation Photosynthesis and Respiration Model (VPRM), Global Biogeochem Cy, 22, 2008

General Comment#3: I fully agree with Reviewer #1 in that more details are needed with respect to the description of the model setup, but also the observation sites. For example, only at the end of the discussion it is mentioned that the city of Hangzhou is located 60 km away from the Lin'an tower. This clearly belongs to the description of the data used, ideally in a specific section within the methods section, entitled for example "atmospheric observations".

Response: We agree with the two referees that more details are necessary to provide a clear description of the observational sites and modeling method. Regarding the details of Lin'an site, the following figure had been added to show the locations of Lin'an tower, downtown Hangzhou, and Shanghai, along with wind rose figures to demonstrate the prevailing winds at 10m and 55m at Lin'an tower. We have also added a few more sentences in the revised manuscript (line#131-137) describing the location and prevailing winds at Lin'an tower.

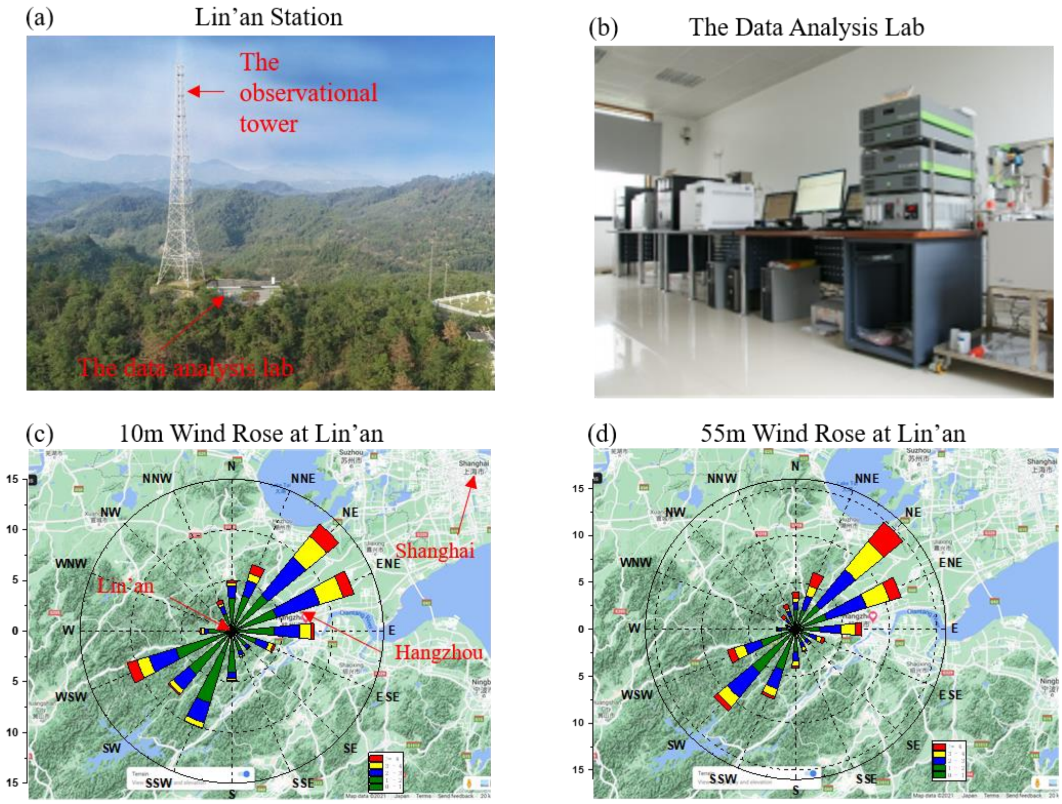

Figure : Photos of the (a) Lin'an regional atmospheric background station and (b) the data analysis lab; and wind rose map at Lin'an derived from wind speed and wind direction observations for 2016-2018 at (c) 10m and (d) 50m.

Regarding the modelling method, we have added the following table in the revised manuscript to provide a detailed description of the model configuration. Necessary descriptions of the WRF configuration were also added in the main text (line#90-93). Considering that the WRF configuration was popular in China and most WRF users may be quite familiar with it, the table was added into the supplementary material. The configuration of VPRM parameterization was also added into the supplementary material.

Table S1. WRF-VPRM Model Configuration

Attribute	Configuration	Reference
Short wave radiation	Duhia algorithm	Dudhia (1989)
Long wave radiation	Rapid radiative transfer model (RRTM)	Mlawer et al. (1997)
Boundary layer	Yonsei University (YSU) scheme	Hong et al. (2006)
Microphysics	Morrison scheme	Morrison et al. (2009)
Cumulus	Grell-3 scheme	Grell and Devenyi (2002)
Land surface model	Noah land-surface scheme	Chen and Dudhia (2001)
Vertical levels	47	-
Model top	10hPa	
Horizontal resolution	20 km × 20 km with 234 (south-north) × 285 (west-east) grid points; 4km × 4km with 215	-

	(south-north) × 280 (west-east) grid points	
Time step	60s	-
Meteorological initial and lateral boundary conditions	NCEP/DOE Reanalysis 2 (R2)	-
Interior nudging	Spectral nudging	-
Nudging variables	horizontal wind components, temperature, and geopotential height	-
Nudging coefficient	3×10^{-5} s^{-1}	-
Nudging height	above PBL	-
Wave number	5 and 3 in the zonal and meridional directions, respectively	-

Table S2. VPRM Parameter Values Used in This Study

	Evergreen forest	Deciduous forest	Mixed forest	Shrub	Savanna	Crop	Grass
PAR_0 (μmol PAR·m^{-2}·s^{-1})	745.306	514.13	419.5	590.7	600	1074.9	717.1
λ (μmol CO_2·$m^{-2}s^{-1}$/μmol PAR·m^{-2}·s^{-1})	0.13	0.1	0.1	0.18	0.18	0.085	0.115
α (μmol CO_2·m^{-2}·s^{-1}·°C^{-1})	0.1247	0.092	0.2	0.0634	0.2	0.13	0.0515
β (μmol CO_2·$m^{-2}s^{-1}$)	0.2496	0.843	0.27248	0.2684	0.3376	0.542	-0.0986

Reference:

Chen, F., and Dudhia, J.: Coupling an advanced land surface-hydrology model with the Penn State-NCAR MM5 modeling system. Part I: Model implementation and sensitivity, Mon Weather Rev, 129, 569-585, 2001.

Dudhia, J.: Numerical Study of Convection Observed during the Winter Monsoon Experiment Using a Mesoscale Two-Dimensional Model, J Atmos Sci, 46, 3077-3107, 1989.

Grell, G. A., and Devenyi, D.: A generalized approach to parameterizing convection combining ensemble and data assimilation techniques, Geophys Res Lett, 29, 2002.

Hong, S. Y., Noh, Y., and Dudhia, J.: A new vertical diffusion package with an explicit treatment of entrainment processes, Mon Weather Rev, 134, 2318-2341, 2006

Mlawer, E. J., Taubman, S. J., Brown, P. D., Iacono, M. J., and Clough, S. A.: Radiative transfer for inhomogeneous atmospheres: RRTM, a validated correlated-k model for the longwave, J Geophys Res-Atmos, 102, 16663-16682, 1997.

Morrison, H., Thompson, G., and Tatarskii, V.: Impact of Cloud Microphysics on the Development of Trailing Stratiform Precipitation in a Simulated Squall Line: Comparison of One- and Two-Moment Schemes, Mon Weather Rev, 137, 991-1007, 2009.

Specific comments and responses:

Comment#1: In addition to the mean bias (MB), the normalized mean bias (NMB) does not really provide additional information, as the mean of atmospheric CO2 for specific locations/periods is always within 10% of 400 ppm. I therefore suggest not reporting the normalized mean bias.
Response: We agree with the referee that normalized mean bias doesn't provide additional information as the atmospheric CO2 concentration is close to 400ppm, thus the relative bias can always be generally estimated with the mean bias. We have removed the values of normalized mean bias in the revised manuscript.

Comment#2: Abstract: Line 18: "characterize CO2 dynamics" I suggest rephrasing "characterize the dynamics of CO2 in the atmosphere"
Response: We appreciate the referee for the detailed writing suggestions, we have rephrased it as suggested in the revised manuscript.

Comment#3: Line 23: "determined" rephrase, e.g. "dominated"
Response: It has been rephrased in the revised manuscript.

Comment#4: Line 44: "calibrated" -> "adjusted"
Response: It has been rephrased in the revised manuscript.

Comment#5: Line 45: "determine posterior flux"
Response: "terrestrial" has been removed in the revised manuscript as suggested.

Comment#6: Line 76: "suffer from" -> "due to"
Response: It has been rephrased in the revised manuscript.

Comment#7: Line 94: A reference for CT2019 is needed. From where and when were the data downloaded? See also NOAA's usage policy under https://www.esrl.noaa.gov/gmd/ccgg/carbontracker/citation.php
Response: We greatly appreciate the referee for this comment. We have added the following text in the acknowledge and Jacobson et al. (2020) by following the NOAA's usage policy.
Text added in the acknowledgement: "CT2019B results were provided by NOAA ESRL, Boulder, Colorado, USA from the website at http://carbontracker.noaa.gov . CarbonTracker data was downloaded from https://www.esrl.noaa.gov/gmd/ccgg/carbontracker/download.php. "
Reference:
Jacobson, A. R., Schuldt, K. N., Miller, J. B., Oda, T., Tans, P., Andrews, A., Mund, J., Ott, L., Collatz,G. J., Aalto, T., Afshar, S., Aikin, K., Aoki, S., Apadula, F., Baier, B., Bergamaschi, P., Beyersdorf, A., Biraud, S. C., Bollenbacher, A., Bowling, D., Brailsford, G., Abshire, J. B., Chen, G., Chen, H., Chmura, L., Colomb, A., Conil, S., Cox, A., Cristofanelli, P., Cuevas, E., Curcoll, R., Sloop, C. D., Davis, K., Wekker, S. D., Delmotte, M., DiGangi, J. P., Dlugokencky, E., Ehleringer, J., Elkins, J. W., Emmenegger, L., Fischer, M. L., Forster, G., Frumau, A., Galkowski, M., Gatti, L. V., Gloor, E., Griffis, T., Hammer, S., Haszpra, L., Hatakka, J., Heliasz, M., Hensen, A., Hermanssen, O., Hintsa, E., Holst, J., Jaffe, D., Karion, A., Kawa, S. R., Keeling, R., Keronen, P., Kolari, P., Kominkova, K.,

Kort, E., Krummel, P., Kubistin, D., Labuschagne, C., Langenfelds, R., Laurent, O., Laurila, T., Lauvaux, T., Law, B., Lee, J., Lehner, I., Leuenberger, M., Levin, I., Levula, J., Lin, J., Lindauer, M., Loh, Z., Lopez, M., Luijkx, I. T., Lund Myhre, C., Machida, T., Mammarella, I., Manca, G., Manning, A., Marek, M. V., Marklund, P., Martin, M. Y., Matsueda, H., McKain, K., Meijer, H., Meinhardt, F., Miles, N., Miller, C. E., Molder, M., Montzka, S., Moore, F., Morgui, J.-A., Morimoto, S., Munger, B., Necki, J., Newman, S., Nichol, S., Niwa, Y., ODoherty, S., Ottosson-Lofvenius, M., Paplawsky, B., Peischl, J., Peltola, O., Pichon, J.-M., Piper, S., Plass-Dolmer, C., Ramonet, M., Reyes-Sanchez, E., Richardson, S., Riris, H., Ryerson, T., Saito, K., Sargent, M., Sasakawa, M., Sawa, Y., Say, D., Scheeren, B., Schmidt, M., Schmidt, A., Schumacher, M., Shepson, P., Shook, M., Stanley, K., Steinbacher, M., Stephens, B., Sweeney, C., Thoning, K., Torn, M., Turnbull, J., Tørseth, K., Bulk, P. V. D., Dinther, D. V., Vermeulen, A., Viner, B., Vitkova, G., Walker, S., Weyrauch, D., Wofsy, S., Worthy, D., Young, D., and Zimnoch, M.. : CarbonTracker CT2019B, DOI: 10.25925/20201008, 2020.

Comment#8: Line 114: “pure” I suggest using “process based”

Response: It has been rephrased in the revised manuscript.

Comment#9: Line 130 “samplings of CO2 surface concentrations with monthly intervals are collected through” -> “atmospheric samples near the surface are collected at monthly intervals and analysed for CO2 through”

Response: It has been rephrased in the revised manuscript.

Comment#10: Line 134: please provide a clear reference for the OCO-2 data. From where and when were the data downloaded?

Response: The reference for the OCO-2 data is: Kiel et al., (2019). The download link of the OCO-2 data was provided in the “Acknowledgement” as suggested by the journal guidance. The link was: https://co2.jpl.nasa.gov/#mission=OCO-2 (the download options for NETCDF4 or HDF5 format of the OCO-2 data were provided at the bottom of the webpage)

Reference:

Kiel, M., O'Dell, C. W., Fisher, B., Eldering, A., Nassar, R., MacDonald, C. G., and Wennberg, P. O.: How bias correction goes wrong: measurement of X-CO2 affected by erroneous surface pressure estimates, Atmos Meas Tech, 12, 2241-2259, 2019.

Comment#11: Line 137: please provide a clear reference for the TCCON data from the Hefei site. From where and when were the data downloaded? Please ensure also that the TCCON data use policy is followed (see https://tccon-wiki.caltech.edu/Main/DataUsePolicy).

Response: We greatly appreciate the referee for this reminder. Description of the TCCON-Hefei site and data was provided in Wang et al. (2017), and this publication was included in our reference. We also briefly describe it in the revised manuscript as:

“Daily ground-based Fourier transform spectrometer (FTS) Measured XCO2 at Hefei site (31.90˚N, 117.17˚E) was also collected through the Total Carbon Column Observing Network (TCCON) for year 2016 (Wang et al., 2017). The TCCON-Hefei site was located in the northwestern rural area of Hefei city and measurements were conducted from September 2015 to December 2016."

We also add the DOI of TCCON-Hefei data (Liu et a., 2018) as required by the usage policy.

Reference:

Liu, C., Wang, W., Sun, Y.: TCCON data from Hefei, China, Release GGG2014R0. TCCON data archive, hosted by CaltechDATA, California Institute of Technology, Pasadena, CA, U.S.A., http://dx.doi.org/10.14291/tccon.ggg2014.hefei01.R0, 2018.

Wang, W., Tian, Y., Liu, C., Sun, Y. W., Liu, W. Q., Xie, P. H., Liu, J. G., Xu, J., Morino, I., Velazco, V. A., Griffith, D. T., Notholt, J., and Warneke, T.: Investigating the performance of a greenhouse gas observatory in Hefei, China, Atmos Meas Tech, 10, 2627-2643, 2017.

Comment#12: Line 166: "forest which" -> "forest, which"

Response: The whole sentence was rephrased as: "Regarding vegetation type, the model showed the largest MB over deciduous forest of -1.01 and 1.27 ppmv in summer and winter, respectively, which only covered a very small portion in northeast China."

Comment#13: Line 187: The attribution of model-observation discrepancy to the vertical allocation of emissions is not plausible. It would be required to at least state the distance of upstream (strong) emission sources that could have an impact on the CO2 profile over the site.

Response: We agree with the referee that it is not plausible to saying the vertical allocation of emission is responsible for the model-observation discrepancy without detailed discussion. Other factors such as the parameterization of VPRM and the anthropogenic emission intensity may also contribute to the discrepancy. We have rephrased the statement as "The discrepancy is likely due to the combined effect of vertical allocation of anthropogenic emission and parameterization of VPRM". WRF-VPRM showed prominent better agreement with observations at the ESRL sites in remote areas than Lin'an tower. The major differences between ESRL sites and Lin'an are the vegetation types and geolocations. Validation against the OCO-2 data suggested that WRF-VPRM didn't show significantly different performance over different vegetation types, thus we have rephrased the discussion as anthropogenic emission allocation may play an important role because Lin'an was close to downtown centers while the ESRL sites were located in real remote regions far from anthropogenic emissions as shown in the following figure (Figure 1(a) in the revised manuscript).

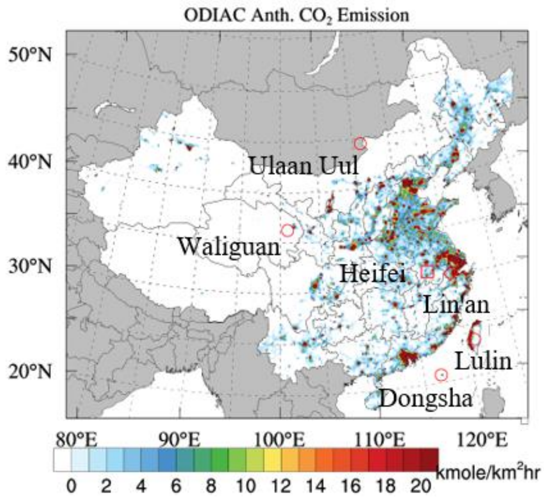

Figure: Anthropogenic emissions of CO2 and the locations of ground measurement sites

The referee pointed out a very interesting question to state the distance of upstream (strong) emission sources that could have an impact on the CO2 profile. Apparently the ESRL sites were all far from anthropogenic emission sources at local scale so we haven't probe into this issue. For instance, Lulin site (LLN) was located in the Lulin Mountain in central Taiwan with 2826 sea level height, while the anthropogenic emission sources in urban areas were mostly along the west coast, thus the regional anthropogenic emission can hardly affect CO2 profile at Lulin site. Based on the observations available, we checked the footprints (as recommended by the other referee) at Lin'an tower to identify the contributions from different distances as shown in the following figure. Footprints were calculated following the method proposed by Hsieh et al. (2000).

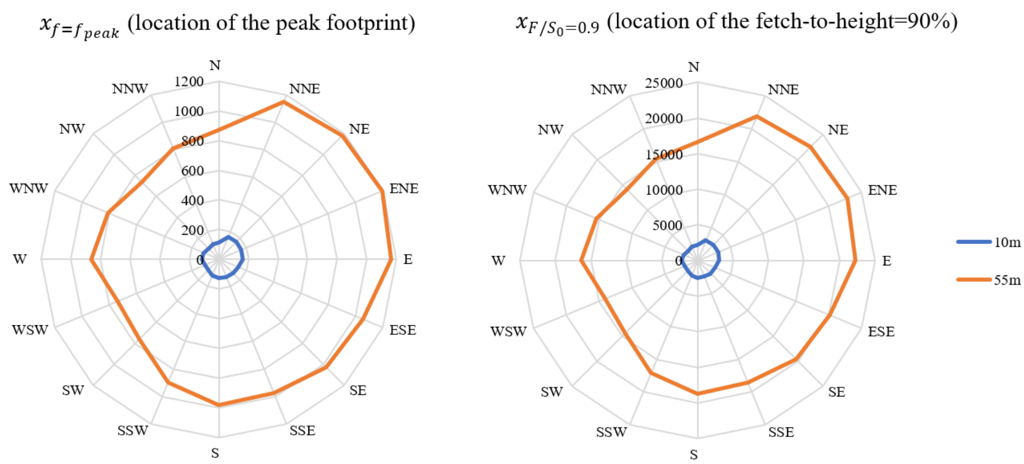

Figure: Locations where footprint reaches peak value (left); Locations where the fetch-to-height ratio equals 90%. Both units are meters.

The above figure shows the peak locations (left) of footprint and the location where the fetch-to-height ratio equals 90% (right) at 10m (blue lines) and 55m (orange lines) of Lin'an tower respectively. At 55m height, the peak location of footprints were about 1.2km from NNE, NE, ENE, and E directions. The location of fetch-to-height ratio equals to 90% were about 22km, suggesting that the upwind areas within this distance contribute 90% to the 55m height at Lin'an tower. This footprint can serve as one example to indicate the distance that upwind sources may affect the CO2 profile. More details of the footprint calculation and discussion were presented in the response for comment#10 for the other referee (https://editor.copernicus.org/index.php/acp-2020-1128-AC1.pdf?_mdl=msover_md&_jrl=10&_lcm=oc108lcm109w&_acm=get_comm_file&_ms=90658&c=196331&salt=840354016545253938). As we were comparing CO2 at 21m and 55m but there was no wind data at 21m, we decided not to include the discussion of footprint in the manuscript but only provide it here.

Comment#14: Line 188: "Biosphere models" please rephrase, e.g. "tracer transport models"; also: CASA is a biospheric process model providing biosphere-atmosphere exchange fluxes, to which level within a tracer transport model those are added is not prescribed by CASA. Also note that the injection height is relevant only for anthropogenic emissions of CO2 due to the associated smoke stack height or plume rise (see Brunner et al., 2019), not for biospheric fluxes.
Response: The sentence has been rephrased as suggested in the manuscript. We appreciate the referee's detailed comment and discussion of CASA. We also agree that emission injection height is relevant only for anthropogenic emission, and we thank the referee for reminding us to rephrase the writing to avoid misunderstanding.

Comment#15: Line 198: "Pu et al. (Pu et al., 2014)" -> "Pu et al. (2014)"
Response: The citation has been reformatted in the manuscript.

Comment#16: Line 204: Please reformulate, this sentence is not clear. What do you mean by "as a process-based model"?
Response: In the original manuscript, we intended to emphasize that WRF-VPRM can simulate atmospheric CO2 without a prior flux input. We realize that the sentence is redundant, and have removed it from the manuscript.

Comment#17: Line 209, Fig 4c: I suggest using daytime values at the Lin'an tower. Note that the air samples at the NOAA stations are also taken during daytime, usually in a well-mixed boundary layer. Otherwise nocturnal peaks in (modeled or observed) CO2 will simply dominate.
Response: We greatly appreciate the referee for this helpful comment. We have updated the figure and related discussion with daytime data from observation and model. The correlation was increased from 0.77 to 0.82. Fig 4c in the original manuscript was Fig.5(c) in the revised manuscript, as shown below.

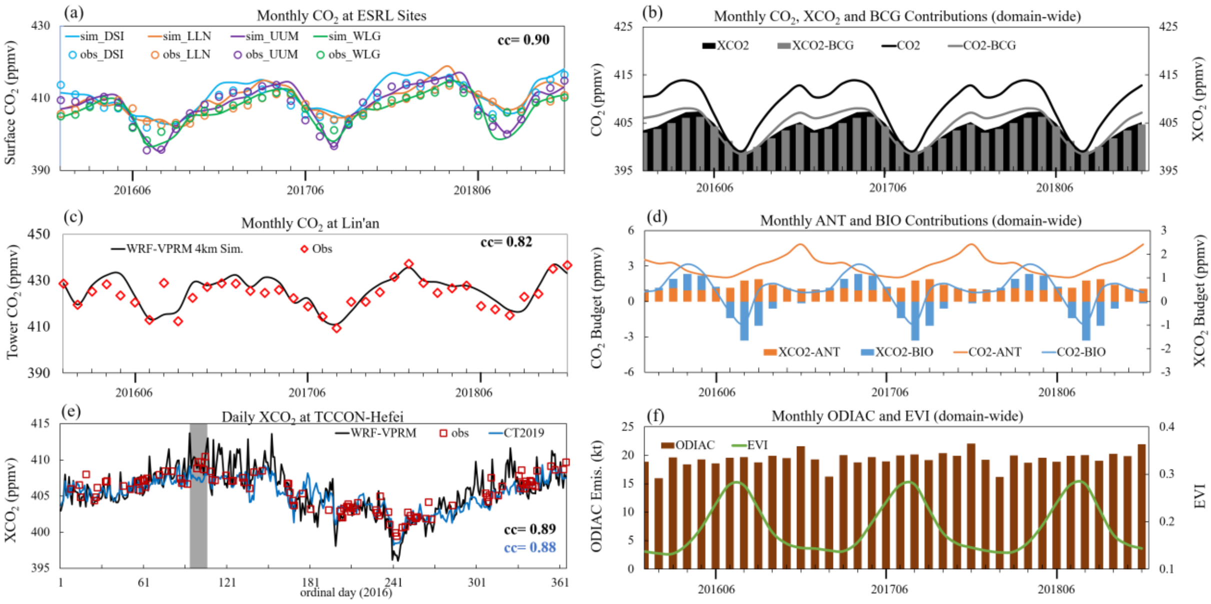

Figure : Monthly variations of (a) CO2 at ESRL sites, (b) total (black) and background (BCG, grey) CO2 (line) and XCO2 (area and bar), (c) CO2 at Lin'an station (averaged for daytime 21m and 55m data); (d) contributions from anthropogenic (ANT, orange) and biogenic (BIO, blue) for CO2 (lines) and XCO2 (bars); (f) ODIAC emission and MODIS EVI; and (e) Daily variation of XCO2 at TCCON-Hefei site.

Comment#18: Line 210:"we will probe into bias" -> "we will discuss details on the bias"

Response: We greatly appreciate the referee for the detailed writing suggestions. Discussion of the simulation bias at Lin'an has been revised based on a new set of 4km grid resolution simulation over a smaller domain covering Lin'an. And the original sentence at line#210 has been removed.

Comment#19: Line 224: "minimums" -> "minima", "maximums" -> "maxima"

Response: These words have been replaced as suggested in the full manuscript.

Comment#20: Lines 241 – 245: I strongly recommend using ppm/yr as unit for the different trends.

Response: We have used ppmv/yr as the main unit for the different rends in the revised manuscript when it is possible. We kept the percentage unit for some of the descriptions about model simulated XCO2 budgets trends (original line#243-245) because the ppmv values were too small for anthropogenic and biogenic contributions. For instance, the annual average contribution of XCO2-ANT to the budget was 0.59ppmv, thus the trend of XCO2-ANT was 0.0047 ppmv/yr (0.81%/yr), and it may not appropriate to use ppbv for describing CO2, so we kept the usage of percentage for this description.

Comment#21: Line 265: "may have also estimated" I assume that there is corresponding output from WRF-VPRM with hourly biosphere fluxes from respiration and photosynthesis, such that it can be confirmed that WRF-VPRM simulates non-zero respiration during nongrowing season.

Response: Yes, WRF-VPRM did provide hourly outputs of respiration and photosynthesis uptake. In this sentence (line#265) we intended to say "may overestimate the nighttime respiration". We apologize for the typo induced misunderstanding. We had no flux measurements at Lin'an thus unfortunately we cannot evaluate if the nighttime respiration was overestimated. Li et al. (2020) validated the hourly respiration with eddy covariance data at a mixed forest site Wuying (47.15˚N, 131.94˚E), so we compared the simulated respiration and photosynthesis uptake between Wuying and Lin'an to indicate that the model may overestimate respiration in warmer areas where VPRM did calculate respiration as non-zero during nongrowing season, as shown in the following figure (Figure S5 in the supplementary material).

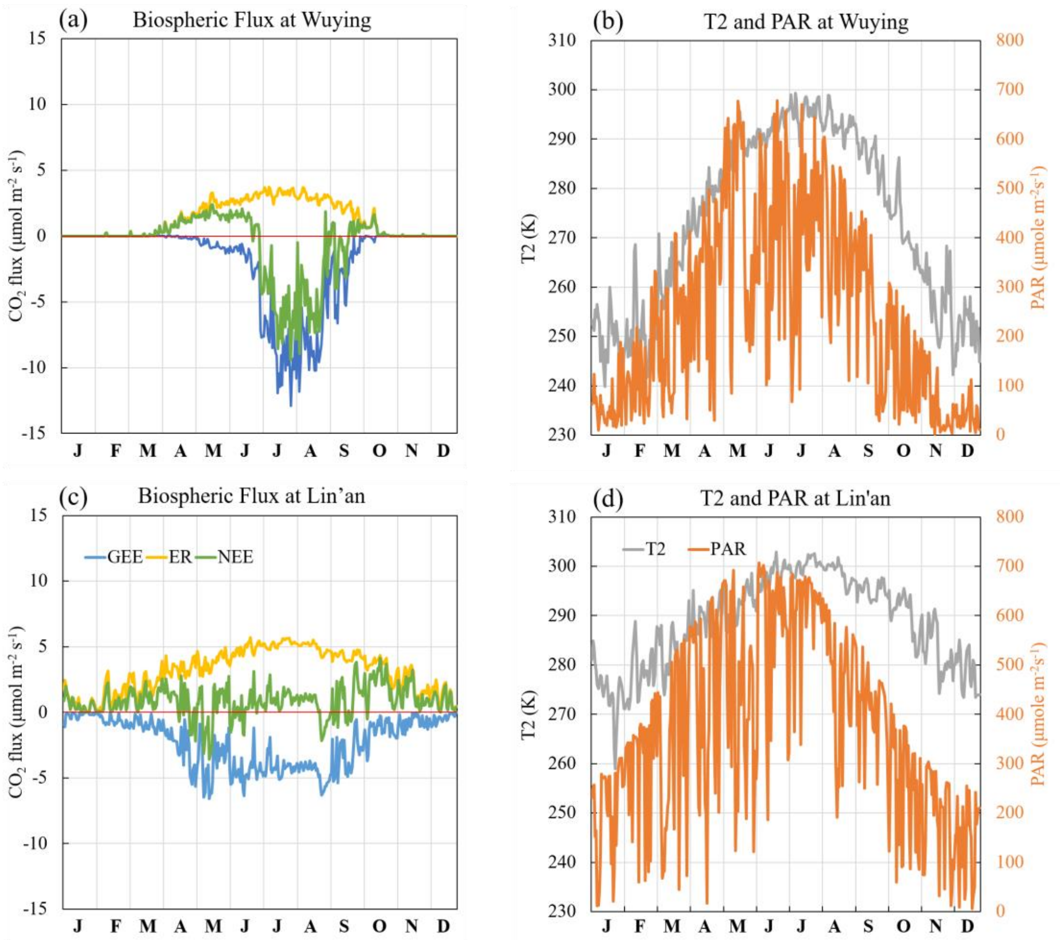

Figure: Comparison of WRF-VPRM simulated daily variations of biospheric fluxes (left column) and meteorology (right column) between Wuying (top row) and Lin'an (bottom row).

Comment#22: Line 280: "above or underestimation" -> "above, or due to underestimation"
Response: It has been rephrased in the revised manuscript, we greatly appreciate the referee for the detailed writing suggestions.

Comment#23: Line 300: it should be made clear that here the concentration footprint is meant, rather than the flux footprint. See e.g. Lin et al. (2003) for concentration footprint, and Schmid et al. (1994) for flux footprint.
Response: We appreciate the help from the referee to pointing out the difference between

concentration footprint and flux footprint. The discussion of footprint was removed from the manuscript mainly because there was no wind data at 21m height.