In this manuscript the authors use a coupled biosphere-atmosphere model SPA-DEHM to simulate the CO2 surface exchanges and 3-D concentration fields over Denmark at a high horizontal resolution of 5.6 km. The simulated CO2 surface fluxes and concentrations in the atmosphere are evaluated against observations from 5 EC flux tower and 1 tall tower atmospheric station in Denmark. Based on results from the simulation, annual CO2 budget over this country is estimated and compared to others of similar latitudes and country size. A synoptic storm event is also investigated to examine contribution from land surface fluxes, ocean fluxes and fossil fuel fluxes, particularly impacts from the Roskilde Fjord system. The authors discuss sources of uncertainties regarding the simulation of CO2 surface fluxes and propose future directions for model improvement. The manuscript is well structured and addresses variations of CO2 surface fluxes and concentrations at different time scales. However, the authors seem to more focus on the biosphere model and overlook errors/uncertainties from atmosphere model that would influence simulation of CO2 surface fluxes and concentrations; further, the scientific message of the manuscript is rather vague in its current form. I would like the authors to consider my questions and revise the manuscript before I recommend the publication of this paper. Detailed of my comments will be found in the following.

General comments:

1. This study uses a coupled biosphere-atmosphere model to simulate CO2 surface fluxes and concentrations at a horizontal resolution of 5.6 km. Yet it's unclear whether and to what extent model performance will be improved with this fine resolution. Have you done any sensitivity test with coarser resolutions to show model improvement? Or have you compared results from your simulation to those from global models or regional models? How much are they different in terms of flux estimates and annual budgets?

2. The simulated CO2 concentrations are evaluated against only one site in Denmark. It is relevant to include other European sites around the study area if there is any (e.g. MHD) to see: 1) whether the boundary conditions and regional transport are good enough for the nested coupled simulation; 2) whether the high resolution coupled model over Denmark improves representation of CO2 variabilities at those sites.

3. The authors attribute uncertainties in simulating CO2 surface fluxes using SPA to PFT-specific parameters regulating carbon allocation and turnover, as well as accuracy of PFT maps (especially agricultural-related landcover types). Have you examined whether the climate drivers and wind fields simulated by DEHM are in good quality? How much uncertainty in these variables?

4. In Section 4.3, the authors discuss the reasons why signals from the Roskilde Fjord system is not detected. While there are certainly representation errors in terms of grid size and uncertainties related to surface water pCO2, another important source of uncertainties comes from transport errors. For example, the vertical resolution of DEHM is only 29 layers, which is rather coarse compared to its horizontal resolution. And the physical schemes related to boundary layer mixing are probably not capable to capture the land-sea breeze and diurnal variations of boundary layer height. This should be addressed and discussed in the manuscript.

5. For the "Abstract" section, it's too long and the description of the model setup is too detailed, which dilute the scientific message and significance that you would like to convey. An abstract should be concise, well-structured and focus on the most important findings and implication from the study, rather than simply listing the main results.

Specific comments:

Page 2 Lines 23–26 The statement is not accurate. There are numbers of studies on regional inversions over regions less covered by observational networks compared to US and Europe, like East Asia, South Asia, Amazonia, Siberia, etc., although with larger uncertainties.

Page 3 Lines 15–25: Please rephrase this paragraph. The description of the study area should be an independent section (see the next comment). And you should summarize here each section in the following manuscript.

Page 3 Line 26: There should be a section before model setup to describe the study area, including the landcover classification, coastal lines, major cities, important geographic characteristics (e.g., Roskilde Fjord system), etc.

Page 4 Line 5: How many vertical layers are there in the planetary boundary layers?

Page 5 Line 26: It would be better to mark the locations of EC flux sites and the tall tower for CO2 measurements on a map.

Page 6 Line 4: How about the model performance on diurnal and daily variations of NEE? As you focused on a storm event during Oct. 19–29, 2013 in section 3.2, it would be better to have an idea of the capability of SPA to capture short-term variabilities.

Page 6 Line 10: Why rapid leaf growth in response to environmental drivers would cause a delay in spring photosynthesis?

Page 6 Line 25: What's included in "agricultural other"? It seems that it has substantial contribution to monthly GPP and respiration.

Page 6 Line 30: What is the altitude of this site? Can you further describe the dominant wind directions for each season (from observations), and potential influences from local pollution and vegetation activity? Again it would be better to have location of this station on the map.

Page 7 Line 3: How much does landcover classification vary over years? Do areas for certain landcover classifications vary a lot? If not, I would suggest to include the period 2012–2014 as well to calculate GPP, respiration, annual carbon budget, etc.

Page 7 Lines 14–24: It would be better to demonstrate the seasonal variations of GPP/respiration and contribution from landcover classifications with plots compared to tables.

Page 7 Line 16: The monthly contribution should also depend on the productivity of each land classification.

Page 8 Lines 14–17 Better to show the seasonal variations of CO2 fluxes in coastal areas in a figure in the supplementary material.

Page 8 Line 31 Do you have observations of wind direction and speed corresponding to each

CO_2 measurements? It would be nice to plot concentration roses also based on observed CO2 and wind datasets, and see if model captures them well.

Page 11 Line 31: I think it's not precise to say that Roskilde fjord is not in the footprint of the tower. It could be in the footprint of the tower. As you mentioned, the marine signals cannot be seen because they are rather weak compared to land signals, or the current model is not capable to represent the complex topography, surface water pCO2 or transport. And as mentioned in the general comments, there are also uncertainties related to transport errors.

Technical corrections:

Throughout the manuscript, the authors use "land-use classifications" to indicate different vegetation types. In my opinion, it would be more appropriate to use "landcover classifications" as "land-use" emphasizes more human-induced influences (see https://oceanservice.noaa.gov/facts/lclu.html).

Page 2 Line 13: Data for which period?

Page 4 Line 11: "molefractions"

Page 6 Line 13: "by spring barley" -> "for spring barley"

Page 7 Line 3: "however,"-> "and"

Page 7 Line 10: "evident" -> "distributed" or "found"

Page 8 Line 6: "continuously" -> "persistent"

Page 8 Line 14: "hides" -> "masks"

Page 10 Line 30: "calibration of validation" -> "calibration and validation"

Page 10 Line 34: "appraises"?

Page 10 Line 34–35 For which period?

Figure 2,3,5,6 It would be better to: 1) mark locations of the flux towers and atmospheric station on the maps, as well as locations of major cities and Roskilde Fjord; 2) add lat/lon on all the maps for reference; 3) if possible, keep color bars and scales as same for comparisons between different panels and figures.