General comments:

The manuscript of Feng et al, "The influence of spatiality on shipping emissions, air quality and potential human exposure in Yangtze River Delta/Shanghai, China", is well written and provides some additional information on the spatial distribution of ship emissions of the inland waterway traffic. This manuscript feels like an attempt to achieve something greater in the future, because it introduces the methodology necessary for ship emission inventory work, atmospheric modeling and health effect evaluation without getting there in the end. The title wisely stops at human exposure, because this is what the paper delivers, but I wonder why the authors stopped there and did not take the final step from exposure to health effects.

The novelty aspect of this work could be improved; emission inventory work cites existing work and this paper does not bring much new to this topic. The atmospheric modeling was done with an existing code and no advances were made to improve the existing tools. From methodological point of view, this paper applies existing tools to a known environmental problem which means that the novelty must come from that contribution. There are two contributions which are brought to light in this paper. First is the contribution of inland waterway traffic to ship emissions and the second is the geographical reach of ship emissions when ship to shore distance is varied. The latter contribution hints to a design of new potential regulation which would not necessarily cover all of the 200 nautical mile distance from shore, but this motivation is currently only indirectly stated, if at all.

In some parts of the manuscript, authors state that they have used data from specific months whereas on other parts data for a full year seems to be used. It was challenging to understand which parts of the work were done with a full year's dataset and which with less data.

Detailed comments:

Page 1, Introduction, lines11-15. Authors discuss the health effect evaluation of ship emissions and quote Sofiev et al (2018). I wonder, what is the motivation of not citing the numbers of Sofiev et al, which reports the latest global health effect numbers, but authors choose to refer to 50 000 to 90 000 premature mortality cases instead? The values given in Sofiev et al (2018) are much higher than this.

Page 4, lines 24-29. Authors have chosen to report the case before the DECA implementation. I was wondering about the motivation of this decision, because it seems that the modeling work could have been easily applied also the DECA case and would have allowed the identification of the impacts of this policy change thus significantly improving the novelty aspect of this work.

Page 5, lines 15-22: Authors report the specifics of chemical transport model domains, but say very little of the emissions. There is a separate section for ship emissions, but I cannot see whether daily, monthly or annual emissions with or without the dynamic features of ship emissions were used or not. The activity data allows this, but have the authors considered these variations in to consecutive steps, too?

Page 5, lines 23-27. "Highest shipping impacts were expected in June because shipping activity and emissions are higher in summer than at other times of the year". There are references to Fan et al

(2016) and Jalkanen (2009) in this sentence. Actually Fan et al state “No significant differences in the total emissions quantities were observed among summer, autumn and winter”, which seems to contradict what the authors say.

Page 6, lines 1-2. I would like to see some discussion on the limitations of AIS in this. It is not used by all ships listed by the authors. The way the text is written now implies that all the ship classes listed here is covered by AIS, which is not necessarily the case since inland traffic may be incompletely represented.

Page 6, lines 4-6. Does the material obtained from MSA include boats? I would imagine that boats outnumber ships by at least an order of magnitude. Was any consideration given to boat contributions to emissions? Boats may not be the biggest source of CO2, NOx or SOx, but they are a significant source of VOCs and CO.

Page 6, lines 7-12. Use of speed entries of AIS. How did you count for the water flow? You have concentrated the study on an area which is along a large river, which means that there is a significant water flow. When a case like this occurs, speed over water is not the same as the speed over ground indicated by the AIS. If power predictions are based on speed over ground, then power prediction will fail. Have the authors considered this aspect?

Page 6, lines 13-14. This is a rather drastic assumption. Have you thought about linking the fuel type or S content to engine specifications? There are technical reasons why some engines cannot use certain types of fuels, but have authors chosen to neglect these limitations completely?

Page 7, lines 3-6. “Shopping” of emission data piece by piece from various data providers may lead to unexpected side effects, which can arise from the fundamental assumptions used in emission inventory construction work. The CO and VOC emissions both result from incomplete combustion of fuel and there is a high probability that these two are linked. Did the authors check what the CO/VOC share in IIASA inventory was and how different the CO/VOC share was in the combined inventory?

Page 7, lines 16-21. What was the temporal resolution of the ship emission inventories used in this work?

Page 8, lines 15-17, the last sentence. There is no uncertainty involved in atmospheric measurements? Really? These can be tens of percent, easily. Cross comparisons of AQ measurement results between instruments can deviate significantly, depending on the equipment used.

Page 8, lines 25-29. I agree that population weighted approach has some merit, but that still is an incomplete representation of human activity. The approach used here assumes that people spend all their time at home and do not consider realistic behavior of people. There are some studies that take this into account (see for example Soares et al, GMD, 2014).

Page 9, lines 1-2. It seems that the annual estimate is based on two months of actual data. Why not using data for the whole year? This would remove one source of uncertainty from the final results. The lines 9-10 seem to suggest that data for the whole year 2015 was available for the authors.

Page 9, lines 18-20. The largest contribution to emissions comes from sources close to the shore. This underlines the importance of including all waterborne traffic sources and consideration of water flow/speed issue. Some discussion of these topics should be included in the manuscript.

Page 10, lines 8-10. Authors identify ships as a significant source of VOCs. Have you considered the role of small boats in VOC emissions? The VOC emission levels allowed for boat engines are significantly higher than those of marine diesel engines and there are a lot of small engines in boats.

Page 12, lines 19-21. If the atmospheric conversion of gaseous SO2 to particulate SO4 takes about a week (reacts with OH), why is the 12 nm distance relevant in this aspect? Surely during one week the gaseous SO2 travels further that 12 nm during that time and it cannot be used as an only explanation why ships further out than 12 nm do not contribute to SO2.

Figure 1
The legend text font size should be increased, it is very small reading as it is now. It is especially tough to read the text of the right hand side zoomed images.

Figure 2. This figure is confusing. If the symbols represent measurement values, I cannot see any numerical values linked to the symbols. If the colors correspond to gridded model concentrations, that is fine, but the measured values cannot be determined from these images. Perhaps another form of graphic could be used to provide the comparisons?

Figure 3. The legend texts are very small in this figure, too.

Figure 5, I would welcome some discussion why the distance to the shore is relevant in this context. Are the authors trying to see whether it is useful to limit the distance of regulated emissions to a specific value or what is the reasoning of choosing these distance bins?

Figure 9. Texts are too small, especially in the two top images.

Table 1. Are these daily, monthly or annual values? There is no indication of the timeline here? This data does not tell me very much of how well the model is able to capture the temporal variability of pollution peaks. Could a line graph be used here instead? This would help to see how well the model is able to capture the air concentrations.

Table 2. No units are given?

Table 3. No units given?

Supplementary material, S1

The authors seem to apply the Starcrest methodology in their emission modeling.

Page 2, text under Eq (1). Maximum speed and design speed of ships are two different things and IHS data often mentions economic speed. Which was one was actually used in the analysis?

Page 2, near Eq (4), aux boiler use. Did the authors consider the exhaust boilers at all in this regard? Also, the installed boiler capacity is difficult to determine from ship databases, because this field is not provided. I would like to know where the installed boiler data comes from.

Page 2, last paragraph. Authors make reference to Lloyds, 2009 which is not listed in the bibliography provided for S1. Also, why refer to data from 2009 if the AIS data is for 2015. How were the ships built during 2009-2015 treated?

Page 2, last paragraph. Authors assume all inland waterway vessels to have 7000 kW engine? No effort was made to identify these vessels and use proper description of installed power?

Page 3, S2, first paragraph. “Table S1 lists emission factors used in the present study”. This is not true and the emission factor table is missing.

Page 3, second paragraph. Add reference ICF, 2009

Page 3, second paragraph. OC and EC low load adjustment factors were treated the same way as PM. This is contrary to the behavior of EC and EC as a function of engine load. Authors might want to check the ICCT report “Black Carbon Emissions and Fuel Use in Global Shipping, 2015”, Oct 2017 for low load behavior of carbon fraction.