Reviewer(s)' Comments to Author:

Reviewer: 1

Comments:

This is a thorough, holistic work on the effect of wind farms on air quality along the northeastern coast of the US, and a valuable and timely study. My comments are minor.

We appreciate these comments and agree that the suggestions (and subsequent changes) result in a clearer description of the modeling assessment.

line 72: it would be helpful to give more specifics about models referenced here (names and/or scope)

Based on this comment the text was revised to be more specific about the type of sophisticated models being referenced.

Sophisticated models such as 3D photochemical grid models (Baker and Woody, 2017; Bergin et al., 2008) and reduced complexity (typically statistically based) tools (Baker et al., 2023) have been used extensively to quantify both PM2.5 and O3 air quality impacts due to emissions from specific sources or sectors.

line 77: "universe" connotes an expansive coverage that is probably larger than intended

Based on this comment, we have changed the text to remove the ambiguous term “universe” and be clearer and more direct. The revised text follows.

A photochemical grid model was applied for a year anticipated to have large construction phase emissions and a year where offshore wind projects were anticipated to be in both the operation and maintenance phase.

lines 104-106: is this how the emissions are currently treated? (maybe intro. could address how emissions are currently treated if at all)

Based on this comment a new sentence was added to clarify that commercial marine vessels were modeled as have been done traditionally in past model assessments not related to wind energy projects. New text follows.

Commercial marine vessels have traditionally been included in modeling assessments and were treated in this assessment similarly in terms of emissions characteristics.

line 111: reference for or link to the IPM?

Based on this and similar comments, we have added a reference for the IPM model. Revised text follows. Unfortunately, there are not peer reviewed references for the core model.

EGU emissions for 2026 and 2055 were projected from 2016 using economic conditions and anticipated emission controls with the Integrated Planning Model (IPM; https://www.epa.gov/power-sector-modeling).

line 112-113: 2016 was not mentioned previously--is this a typo? (it would make sense to use the 2021 NEI since that was used in generating emissions for the AVERT modeling) lines 148-156: ah, I see why 2016 was mentioned earlier. I think the authors could reorganize things to be more clear, or at least add something at the start of the methods that explains all scenarios are built using emissions projections and meteorology from 2016.

Based on this comment, the text was revised to be clearer how 2016 is related to the other years included in this model assessment. Revised text follows.

EGU emissions for 2026 and 2055 were projected from 2016 using economic conditions and anticipated emission controls with the Integrated Planning Model (IPM; https://www.epa.gov/power-sector-modeling). EGU emissions for 2016 were based on emissions reported to the Continuous Emissions Monitoring (CEM) database and National Emission Inventory (NEI) for 2016 (U.S. Environmental Protection Agency, 2023).

lines 215-219: these are general statements that would work well at the start of the section, to reorder text to go from more general to specific. Then the first sentence (current line 204) could be reworked to be "Both models show impacts from... "

Based on this suggestion, the paragraph at lines 215-219 was moved to be the first paragraph in the section and the leading sentence of the next paragraph (originally the first paragraph) was revised as follows.

CMAQ and PCAPS prediction of annual average PM2.5 (Figure S4) and seasonal average MDA8 O3 (Figure S5) for 2026 and 2055 emissions scenarios show impacts from offshore wind project emissions tend to be highest nearest the project and decrease as distance from the project increases.

line 276: it should be fair to add that using a single year of meteorology isolates the effects of the emissions.

Based on this suggestion the text was modified. Revised text follows.

A single year of meteorology was used to isolate the effects of changing emissions, but the meteorology used may not capture the entirety of important meteorological conditions that result in offshore to shore air flow which could impact population exposure.

The font sizes of the figure axes labels, axes titles, and colorbar labels should be increased to match the legends.

Based on this comment, a review was done of the Figures presented in the manuscript and changes were made to Figure 1 and Figure 4 to improve the readability of the axes labels/titles. The images imbedded in this submission are not necessarily the high-resolution images that will be used in the final typeset version so we anticipate the Figures in the manuscript should look good overall.

Reviewer: 2

Comments:

This paper presents analyses of model results related to future emissions changes from the construction of offshore wind energy facilities and the associated reduction of onshore EGU emissions. The authors use a chemical transport model and a reduced form model with emissions inputs for different scenarios. The reduced form model results from 2024-2031 are used in a health and economic benefits model to estimate the mortality and costs associated with the changes in the ambient PM2.5 and ozone concentrations from offshore wind energy in the North Atlantic. The paper is timely and provides interesting results about the resulting pollutant concentration changes from energy sector changes (i.e., fossil fuel to wind energy). Overall, the paper is well written and interesting.

We appreciate the thoughtful and useful comments provided by the reviewer. We feel that the changes made based on these comments has made the manuscript stronger.

General Comments:

The current description of the emissions and air quality modeling workflow is a bit difficult to follow. For example, it was not totally clear until line 274 that WRF was only run for 2016 and not for any other year in the modeling period. It was vaguely mentioned in methods on line 148 but does not explicitly say that 2016 was used for all model years in the paper.

Based on this comment text was changed to be clearer that WRF 2016 was used for all CMAQ simulations. Revised text follows.

All CMAQ simulations used the same 2016 meteorology.

Additionally, there might be a few typos related to the years in the methods description that contribute to this confusion (e.g., 2025 on line 90, maybe 2016 on line 112 - although this could be for the 2016 run so not clear because it follows a sentence related to 2026 and 2055 modeling).

The reviewer is correct that 2025 on line 90 was a typo and that was corrected. Text was revised to be more clear that 2016 was the baseline for projected future years. Revised text follows.

EGU emissions for 2026 and 2055 were projected from 2016 using economic conditions and anticipated emission controls with the Integrated Planning Model (IPM; https://www.epa.gov/power-sector-modeling). EGU emissions for 2016 were based on emissions reported to the Continuous Emissions Monitoring (CEM) database and National Emission Inventory (NEI) for 2016 (U.S. Environmental Protection Agency, 2023).

In general, adding a table that describes which model is used and for which year might be helpful (model name, emissions, meteorology, boundary conditions).

Based on this comment, a new table was added to the manuscript that presents key information for each model simulation. Revised text follows from the beginning of the methods section and the new table itself.

Table S1 provides a list of emissions scenarios applied with each model.

Table S1. Model simulations done using CMAQ (simulations 1 to 6) and PCAPS (simulations 7 to 17). Rows colored in blue indicate where emissions were the same for each model to support a direct quantitative comparison of results. The PCAPS model is applied only with emissions changes so some components (such as anthropogenic emissions) were not included in the model application. All CMAQ simulations used 2016 meteorology and the same lateral boundary inflow conditions. PCAPS does not have meteorology or lateral boundary inflow as input options.



Based on the text it seems 2016 was used for a baseline WRF, hemispheric CMAQ, NEI platform for emissions, and CMAQ. Then once the 2016 CMAQ modeling framework was evaluated, the 2016 WRF was used for all future model year, but it's not clear which boundary conditions CMAQ is using for 2026 and 2055, possibly also from 2016.

The reviewer is correct in the interpretation of how CMAQ was applied. Text was revised to specifically note that lateral boundary conditions were the same in all CMAQ simulations based on this comment. Revised text follows.

Biogenic, wildfire, sea spray emissions, and lateral boundary inflow conditions were kept the same for projected 2026 and 2055 simulations.

Additionally, does PCAPS also use meteorology and boundary conditions, where do those inputs come from?

PCAPS does not use year specific meteorology or boundary conditions. It only uses emissions perturbations as input.

PCAPS model performance and the impact on the health outcomes modeling (Lines 215-229, Figures S4 and S4, and Figures S10 and S11). In general, it visually looks like PCAPS is underestimating both the peak concentrations and spatial extent of the PM2.5 and ozone concentrations (Figures S4 and S5). It might be interesting to see a difference plot for the two models for both the PM2.5 and ozone in Figures S4 and S5, along with a spatial plot of the population. While the reduced form model is extremely useful for being able to simulate long time periods, it would be helpful to better understand the uncertainties in the health outcomes associated with using the reduced form model. Table 2 is informative for understanding these uncertainties, however, because the results are spatially averaged for the domain it does not provide insight on the uncertainties that might have a greater impact on the health outcomes (e.g., a location with a large population area where CMAQ simulates concentration changes, but PCAPS does not). Is it possible to use 2026 and 2055, wdith 2026 as the baseline, as a test case to compare using the CMAQ outputs with BenMAP versus the PCAPS outputs with BenMAP?

The main point of presenting monetized health damages was to use that as an opportunity to show PM2.5 and O3 impacts in a way that they could be more directly compared quantitatively. There are many aspects of the tools used and the way they were applied that could impact the results and as the reviewer notes the reduced complexity tool’s underprediction tendency might imply that the monetized benefits clearly did not represent a conservatively high estimate. We have modified the text to acknowledge this and also to point out that the monetized health impacts estimated as part of this assessment are small compared to other emission control plans/scenarios. New text in the results/discussion section follows.

Given the tendency of PCAPS to predict lower O3 and PM2.5 compared to CMAQ (Table 2) this estimate could be different depending on whether these differences were spatially coincident with large population centers. Annual total monetized damages (Figure 5 and Table S4) were much less than estimated for national rulemakings for the mobile and EGU sector and also several hypothetical scenarios applying emission reductions to specific industrial sectors (Simon et al., 2023).

Minor Comments:

Line 90: "2026 and multiple 2055 scenarios" based on the text that follows it seems like there are two 2025 and three 2055 CMAQ scenarios modeled. Are the same scenarios for PCAPS also run for the CMAQ-PCAPS comparison? Consider replacing "multiple scenarios" with the specific number here to be less vague.

Yes, the reviewer is correct the emissions used for CMAQ and PCAPS for 2026 and 2055 scenarios were the same to provide for a direct comparison between models. However, PCAPS only uses emission perturbations, so it does not need a baseline set of conditions for comparison the way CMAQ does. The total number of simulations is not exactly the same between models.

Lines 100-101: Is there a reference for the PSD program that could be added here?

Based on this comment, we have modified the text and made it more general since there are multiple permit related programs that are relevant, and those specifics seem beyond the scope needed for this assessment. We ended up providing a broad reference to the New Source Review program which includes PSD as part of that.

Offshore wind project emissions were based on information provided in air quality permit programs (https://www.epa.gov/nsr).

Line 111: Is there a reference that can be added here for IPM?

Based on this and similar comments, we have added a reference for the IPM model. Revised text follows. Unfortunately, there are not peer reviewed references for the core model.

EGU emissions for 2026 and 2055 were projected from 2016 using economic conditions and anticipated emission controls with the Integrated Planning Model (IPM; https://www.epa.gov/power-sector-modeling).

Lines 111-114: The description for modeling the EGU emissions after the 2016 NEI is unclear. This reads as the 2016 CMAQ run uses the CEMs data from the 2016 NEI for the EGU emissions. But how is the 2016 NEI EGU emissions then used in IPM to project the 2026 and 2055 emissions? Add some brief details here for a reader that does not use IPM.

Based on this comment and a similar comment from another reviewer the text was changed to hopefully be clearer about the relationship between these years.

EGU emissions for 2026 and 2055 were projected from 2016 using economic conditions and anticipated emission controls with the Integrated Planning Model (IPM; https://www.epa.gov/power-sector-modeling). EGU emissions for 2016 were based on emissions reported to the Continuous Emissions Monitoring (CEM) database and National Emission Inventory (NEI) for 2016 (U.S. Environmental Protection Agency, 2023).

Line 126: "capacity for 2031 were applied to the 2055 IPM projected EGU emissions." Consider adding a justification for this statement. The year 2031 comes a bit out of the blue here and why is it okay to use that year to adjust the 2055 EGU emissions?

Based on this comment text was revised to more clearly acknowledge that we do not have any information about 2055 that is different than 2031, which is where all wind projects are fully operational and beyond the construction phase. Revised text follows.

The predicted emissions changes from AVERT based on offshore energy capacity for 2026 were applied to the 2026 IPM projected EGU emissions and those based on offshore energy capacity where all offshore projects were in the operation phase with no construction were applied to the 2055 IPM projected EGU emissions.

Line 141: "hemispheric scale simulation" was this hemispheric CMAQ?

The lateral boundary conditions were from a hemispheric GEOS-CHEM simulation. This detail was not included to avoid reader confusion as there are already quite a few different models being used and discussed and because the source of lateral boundary inflow would not seem to be an important factor for model predictions of changes in emissions in the U.S. and offshore.

Line 148: "CMAQ was applied for multiple annual scenarios using 2016 meteorology." Multiple annual scenarios might imply everything shown in the paper, but it is not explicit. Consider making it clear that all five of the future CMAQ simulations use the 2016 WRF.

Based on this comment the text was revised to be clear that the 2016 WRF was used for all simulations. Revised text follows.

All CMAQ simulations used the same 2016 meteorology.

Lines 159-163: If the year specific WRF was not run (i.e., all runs use 2016 WRF), how were the day and hour specific BEIS and sea spray emissions run for the future years? Also, the fire emissions description says "day specific satellite detections" but those are unavailable for 2026 and 2055, were they projected?

Based on this comment, text was added to note that biogenic, geogenic, and fire emissions were the same for 2016 and all future years to allow for isolating the impacts of changes in EGU and offshore wind energy project emissions. New text follows.

Biogenic, wildfire, and sea spray emissions were kept the same for projected 2026 and 2055 simulations.

Lines 164-172 (Reduced complexity tool application) Is meteorology used as an input to PCAPS? Earlier in the paper (line 95) it says PCAPS was run for 2024-2031, here it says 2026-2031.

The typo pointed out by the reviewer was fixed. “2026-2031” was changed to “2024-2031”.

Also, for the 2026-2031 emissions, are these emissions similar to the 2055 scenario 3 emissions (offshore emissions + onshore EGU AVERT reduced emissions)? Meaning, are the reductions in EGU emissions accounted for in the PCAPS modeling during this time period? Then, for the 2026 and 2055 PCAPS modeling, are the exact same scenarios as the CMAQ run (two for 2026 and three for 2055)?

Yes, the reviewer is correct, the emissions used in PCAPS reflect changes to the offshore wind projects and also to EGUs in each of the years being modeled. The reviewer is also correct that the 2026 and 2055 PCAPS emissions were the same as the CMAQ emissions to provide a direct comparison between models. However, since PCAPS uses emissions perturbations it did not need a baseline scenario for comparison as CMAQ does so the total number of emissions scenarios varied between models.

Lines 179-180: "a set of empirically-derived health impact functions" which functions were used, can you add those with references here?

Based on this comment we made a change to the text to be clear that the reference provided for BenMAP includes more information about the empirically-derived health impact functions. The revised text follows.

A comprehensive description of BenMAP health impact functions and valuation estimation methods applied is provided elsewhere (U.S. Environmental Protection Agency, 2024c).

Line 196: Does AVERT use meteorology? If not, how is the wind energy generation determined, are there baseline average estimates that are used?

AVERT does not use year specific meteorology. However, the wind energy algorithm in AVERT is based on multiple pieces of information, one of which is climatological wind speeds which vary by season in the Atlantic Ocean. AVERT knows that winds are higher in the colder months and therefore more emissions capacity is available for offshore wind and that translates into AVERT predicted changes in emissions onshore.

Line 197: What causes the increase (outliers) in the EGU emissions shown in Figure S2?

Based on this comment an additional sentence was added to that section to explain the occasional increase in emissions predicted at some EGUs by AVERT. The new text follows.

Occasional increases in emissions reflect situations where the Monte Carlo scenarios used to relate power and emissions in AVERT result in modest increased load at some facilities to generate a balanced solution for the region.

Line 200: What causes the increase in the NOx, SO2, and PM2.5 emissions in West Virginia?

The increase in EGU emissions predicted by AVERT at some onshore locations is covered by the response to the immediately preceding reviewer comment.

Limitations: Great discussion in here. Just one thing to maybe consider. In terms of the life cycle assessment of the wind energy, what about the transportation emissions associated with getting the wind turbines to the offshore locations? I guess the fossil fuel extraction and transport is not included for the EGUs, so maybe this is similar? I'm wondering if it makes sense to add a brief mention in the limitations about the life cycle assumptions on the LCA modeling and that other considerations could also be included, depending on the starting point of the LCA.

Based on this suggestion we have added some more text to the limitations section to acknowledge that there are more impacts on upstream and downstream systems as noted by the reviewer. New text for that section follows.

While potential relationships between offshore energy capacity with onshore EGU emissions in this assessment is it likely that this increased offshore energy capacity could change emissions for other sectors such as transportation. Relationships between offshore energy capacity and changes to non-fossil fuel sources of energy were not included as part of this assessment.

Future Directions: Also, great discussion in here. In addition to incorporating year specific meteorology (line 315), it might also become important to have models with feedback. So coupled meteorology-energy-emissions-air quality models, if we are talking, in general, about how models should move forward in the future.

Based on this comment and the immediately preceding comment we have noted that models like GCAM (Global Change Analysis Model) could be used to get more information about how emissions in other sectors might be influenced and changed due to offshore energy capacity. New text for that section follows.

Other types of modeling systems that consider broader relationships between sector emissions and energy (such as the Global Change Analysis Model) could be applied to provide insights on emissions related to changes in the transportation sector and competition with other types of non-fossil fuel based energy supply.

Reviewer: 3

Comments:

This is a review for "Characterizing air quality impacts related to North Atlantic offshore wind energy projects." They use models to understand the air quality impacts related to construction of offshore wind farms, operation and maintenance phases, changes in onshore electricity demand as a result of the added offshore wind energy demand load, and resulting human health impacts due to the changes in air quality. It is an interesting study that uses complex models to look at the impacts on longer time scales and reduced complexity models to compare with the air quality changes at shorter time scales. The authors also include a section on limitations and implications, which are both robust discussions. Finally, the figures are clear and the tables are easy to understand and support the narrative.

We very much appreciate the useful comments provided by the reviewer and feel the manuscript has improved as a result of the changes made.

For the emissions scenarios, some additional clarification is needed. Is there a control case for 2026 and 2055? This is not mentioned in the methods. Further, it's a little unclear if all of the emissions sectors are changing from 2016-2026-2055 or just the onshore EGU and onshore/offshore wind project related emissions are modified. Please revise and/or reorder the text in this section to provide additional detail as to how the emissions are used and modified for these scenarios.

The reviewer is correct that the different emissions years and scenarios is challenging to discern for readers.

The differences between Figure 3 and Figure S5 are a little perplexing. If by 2031 there are measurable decreases in MDA8 O3 due to offshore wind energy production, why are there increases in the 2055 CMAQ simulation?

The reviewer makes a great observation here and this deserves more explanation in the paper as it provides some important context for interpreting the results. The reason 2055 still has increase in offshore O3 and the 2031 simulation does not is because the 2055 simulation is based on an IPM projection of EGUs and 2031 is based on how EGUs look in a recent emission inventory. IPM predicted aggressive emission reductions and plant shutdowns which resulted in much less overall reduction in emissions from the increase in offshore capacity. This is discussed in the limitations section with existing text. A new sentence was added to the abstract to acknowledge that the future nature of EGUs (whether it is the IPM projection or something more like present day) impacts these findings and could result in less impacts if the future was more like what IPM predicted. We also slightly modified the TOC art to reflect this issue of onshore EGU emission reductions perhaps being more modest depending on the representation of future EGUs. The new text for the abstract follows.

Reductions in onshore emissions are predicted on assumptions about the nature of fossil-fuel plants operating when offshore wind energy projects are complete and also that the increased energy capacity would not simply meet increased demand over present-day levels or be balanced by non-fossil fuel-based energy sources such as nuclear, solar, or onshore wind farms.

Minor suggestions:

Line 90: 2025 scenario? Is this supposed to be 2026?

The reviewer is correct line 90 contains a typo. Text for 2025 was changed to 2026.

Paragraph at line 129: can the authors include that these emissions years are used for the reduced complexity model?

Based on this comment, text was revised as follows.

Additional year specific EGU emissions for 2024 through 2031 were developed for application in the reduced complexity tool using anticipated year specific offshore wind energy capacity applied with the AVERT model.

Paragraph at line 164: can the authors include a sentence describing how PCAPS has been applied to air quality analysis before?

Based on this comment, text was revised as follows.

The Pattern Constructed Air Pollution Surfaces (PCAPS) model has been applied for complex sector-specific emissions scenarios for stationary and mobile sources and predicted air quality results consistent with more sophisticated models (Baker et al., 2023) PCAPS version 1.1 was applied for each year between 2026 and 2031 with year-specific offshore wind project and EGU emissions.