

Health and Economic Impacts of Improved Air Quality from Transitioning Global Shipping Energy to Hydrogen

Corresponding Author: Professor Steve H.L. Yim

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

This article presents a well-written and clearly communicated health co-benefits analysis of a transition to hydrogen in the shipping sector globally. It demonstrates that the health benefits of this transition are significant. The implication is that these additional and often underappreciated benefits can provide evidence that should motivate policymakers to adopt the reforms needed to accelerate the transition to hydrogen in the shipping sector. This is important because such a transition is currently moving too slow, threatening the pursuit of internationally agreed climate goals. It is also important because, as the article also implies, there has been relatively limited attention to how co-benefits can drive forward this transition in the shipping sector.

While I think that the article is publishable on its technical and methodical merits, I would suggest a few revisions to make it more relevant to those working on co-benefits research and policymakers.

1) I think either the intro or the discussion needs to tie the analysis more explicitly to some of the claims in the work on co-benefits. One of the central claims in this field of study is that quantifying near-term and local health benefits can provide a rationale for taking actions on a long-term global climate challenge. I think that this needs to be stressed.

Pearce, David. "Policy frameworks for the ancillary benefits of climate change policies." *Ancillary benefits and costs of greenhouse gas mitigation* (2000): 517-560.

Nemet, G.F., Holloway, T. and Meier, P., 2010. Implications of incorporating air-quality co-benefits into climate change policymaking. *Environmental Research Letters*, 5(1), p.014007.

Zusman, E., Chae, Y., Kim, H. and Farzaneh, H., 2021. An Introduction to co-benefits: core concepts and applications. In *Aligning Climate Change and Sustainable Development Policies in Asia* (pp. 1-15). Singapore: Springer Singapore.

Karlsson, Mikael, Eva Alfredsson, and Nils Westling. "Climate policy co-benefits: a review." *Climate Policy* 20, no. 3 (2020): 292-316.

2) At the same time as you make this point, I also think it is important to underline that the much of the work on co-benefits has focused on conventional energy solutions; I do not think that there has been much work done on shipping and hydrogen. You are therefore filling an important gap.

3) I felt that the policy recommendation and implications was rather weak. There is sentence that basically suggests that local governments should do more to regulate shipping and enable transitions to hydrogen. However, I think you could say more about what those regulations would like, what incentives might need to be in place to support their implementation, and what capacities governments at different levels would need to move them forward. There is also some attention to the not only the technical or economic feasibility but also the institutional feasibility of implementing co-benefits solutions that could be noted in this regard.

Mayrhofer, J.P. and Gupta, J., 2016. The science and politics of co-benefits in climate policy. *Environmental Science & Policy*, 57, pp.22-30.

Zusman, E., Akaoshi, K., Hanaoka, T., Malley, C.S., Wangwongwatana, S., Onmek, N., Paw-Armart, I., Thi, K.O.N., Huy, L.N., Kuylenstierna, J.C. and Hirayama, T., 2025. The truth about co-benefits: a multidimensional feasibility assessment for thailand and beyond. *Environmental Research Communications*, 7(2), p.025009.

4) Finally, in terms of strengthening the policy recommendations and institutional feasibility, I think you could suggest that there should be a greater emphasis in global/national/local policymaking processes in incorporating robust analysis of health and other co-benefits in decision making processes. This will help, again, to strengthen the evidence based for policy decisions and investments in the capacities to implement them.

Small additional point: Please provide the citation for this claim previous findings.

Moreover, consistent with previous findings, we find that the health co-benefits are most prominent in regions with high

population density and shipping activity, particularly in Eastern and Southern Asia.

Reviewer #2

(Remarks to the Author)

The authors applied integrated air quality modeling, health impact assessment and VSL evaluation to evaluate the potential benefits from transition from fossil fuels to hydrogen in shipping. The manuscript is well drafted. However, I think the study was designed poorly and missed several pieces:

1. When comparing the all transition to hydrogen scenario with half transition to hydrogen scenario, how the "half" was determined? The locations where the switch from fossil to hydrogen matter for both the air quality and human health effect.
2. I was surprised to see the benefits were larger from ozone than PM2.5. After switching to hydrogen fuels, won't the sulfur emissions greatly reduced, and thus affecting the PM2.5 concentration (such as Jung et al., 2023, Lv et al., 2018)? From supporting Table 1, I did not see the comparisons for the air pollutants changes before and after switching, which the authors should strength. A thorough comparison for major air pollutants changes should be included in the analysis, specifically for SO₂, NO_x, and BC.
3. Another major defect for the study design is that the authors fail to consider the hydrogen leakage. H₂, even though it is itself radiatively inert, undergoes similar chemical reactions to methane (CH₄) in the atmosphere, leading to detrimental climate and air quality effects. There has been several studies illustrating it. Without considering this, the results from this study would overestimate the air quality benefits from hydrogen transition, especially from ozone (Chua et al., 2024; Ocko et al., 2022)

References:

- Jang, E., Choi, S., Yoo, E. et al. Impact of shipping emissions regulation on urban aerosol composition changes revealed by receptor and numerical modelling. *npj Clim Atmos Sci* 6, 52 (2023). <https://doi.org/10.1038/s41612-023-00364-9>
- Lv, Z., Liu, H., Ying, Q., Fu, M., Meng, Z., Wang, Y., Wei, W., Gong, H., and He, K.: Impacts of shipping emissions on PM_{2.5} pollution in China, *Atmos. Chem. Phys.*, 18, 15811–15824, <https://doi.org/10.5194/acp-18-15811-2018>, 2018.
- G Chua, DL Mauzerall, LW Horowitz, V Naik, F Paulot: AGU Fall Meeting Abstracts 2024 (2527), GH21A-2527, Ozone-related Health Impacts of Hydrogen Leakage.
- Ocko, I. B. and Hamburg, S. P.: Climate consequences of hydrogen emissions, *Atmos. Chem. Phys.*, 22, 9349–9368, <https://doi.org/10.5194/acp-22-9349-2022>, 2022.

Reviewer #3

(Remarks to the Author)

Paper explores the health and cost co-benefits of transitioning shipping to hydrogen, preventing premature deaths and diminishing the costs associated to health by improving air quality. Timely and important matter to discuss. Although the results seem interesting, there a few nuances that need to be further explored or explained in the paper.

It would be interesting to add context to the averted deaths, how many total are attributable to shipping as is? Then the reader would have a better understanding if the 132k averted deaths are impactful or not.

English could be improved for better readability of some sentences, such as the one starting on line 70. So sentences throughout the paper are somewhat confusing to read.

Line 100 - The authors should add an explanation as to why the SO₂ land-based emissions increase. The supplementary data does provide some insight, but there are countries that have no SO₂ emissions for the energy sector, so have the authors considered that the energy production mix of said countries is limited for hydrogen production and would require new production from fossil-based sources to increase hydrogen production? That would be counter-intuitive and against current energy policy.

No contextualization is given when introducing the changes to PM_{2.5}. The reader needs to be aware of what the base level concentrations are to know if a 1 µg/m³ variation is of any significance, especially in cases where the difference is 0.1. This is further needed when the authors use words such as "significantly". A percentage would also help. The same applies to O₃.

It would be good if the resolution of Fig. 3 was increased.

The authors should also consider a significant cost in the co-benefits of shipping transition, the costly retrofit of older vessels

to use hydrogen and new vessels alike. The cost of a full fleet conversion could invert the economic benefits of the health impacts.

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Dear Authors,

I have reviewed the revised manuscript and am satisfied that the authors responded to my comments. I therefore recommend the article for publication.

Reviewer #2

(Remarks to the Author)

Thank the authors trying to answer my comments. However, until now, I did not believe the authors fully address them.

1. Related with my concerns about the locations of hydrogen switching, the authors frequently cited the Chua et al., (2024a, b), which were both conference abstracts. I am not sure how solid/robust conclusions I can draw about the justification of the methodology from these simplified abstracts. Warwick et al. (2023) discussed similar scenarios about green hydrogen switching and then focused on their discussions mainly based on the lower boundary conditions (LBC) of H₂ concentration caused by the clean energy switching, which not justify the methods the authors applied, which was based on H₂ emission driven.

Comparing between Fig.2 and Fig.4, I was very astonished to see that some countries, such as India, experienced such large air-pollution related mortality burden changes in the whole country, when the air quality changes appears to be only noticeable in coastal regions (Fig.2).

2. The explanations for the magnitude changes between ozone and PM_{2.5} seems okay. However, From the Table 1 for the anthropogenic emissions from half (all) transition, the authors' explanations for BC increases did not make any sense at all. "Notably, BC emissions from the shipping sector increase under both the All-transition and Half-transition scenarios. This occurs because baseline shipping BC emissions (16 kt/year) are negligible compared with other major air pollutants such as SO₂ (2079 kt/year) and NO_x (11501 kt/year), and BC emissions associated with hydrogen production lead to an overall increase in total shipping-related BC emissions." I'd like to see how the authors' response to the BC increases.

3. For the model evaluation in the supporting, I found the authors did not report any statistical evaluation for the air pollutants. Meanwhile, the bias correction were only applied for the dust and aerosol, which are two components of PM_{2.5}. I will suggest the authors perform a thorough model evaluation as well as bias-correction for both total PM_{2.5} and ozone, either using the satellite data, or multi-data fused surface data.

Reviewer #3

(Remarks to the Author)

Thank your for the revision, I am satisfied with how the paper turned out.

Version 2:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

Thanks the authors for addressing my comments. I have the following comments about the figures and model evaluation:

1. In Supporting Fig. 14, add the units for the baseline mortality rates.

2. correlation does not mean everything especially for the global model evaluation. It only demonstrate that the model in this study can simulate the general spatial patterns of both PM_{2.5} and ozone as other datasets. However, it does not say how best the model performs, without comparing with surface observations. I'd like to see the relative differences for both PM_{2.5} and O₃ related with Fig. 11 and 12 in the supporting, as well as the IOA metrics between this study and the other datasets.

Version 3:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

Thanks for addressing my questions on the model evaluations. I have no further comments.

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Response to Reviewers

The term “revised manuscript” in this context denotes the PDF file we have submitted, which clearly highlights the modifications made from the previous version. We have addressed each comment made by Reviewer #1, Reviewer #2 and Reviewer #3. For clarity, we have presented the reviewers’ comments in blue and our responses in black. It is noted that we improved the regridding approach used to harmonize the population data and CESM-simulated air pollution concentrations at a 0.25° resolution (see Methods), and consequently revised all mortality estimates. The improvement in the interpolation method does not affect the main conclusions of this study: a hydrogen-based energy transition in global shipping could deliver substantial air quality, health, and economic co-benefits.

Response to Reviewer #1

General Comments:

This article presents a well-written and clearly communicated health co-benefits analysis of a transition to hydrogen in the shipping sector globally. It demonstrates that the health benefits of this transition are significant. The implication is that these additional and often underappreciated benefits can provide evidence that should motivate policymakers to adopt the reforms needed to accelerate the transition to hydrogen in the shipping sector. This is important because such a transition is currently moving too slow, threatening the pursuit of internationally agreed climate goals. It is also important because, as the article also implies, there has been relatively limited attention to how co-benefits can drive forward this transition in the shipping sector.

While I think that the article is publishable on its technical and methodical merits, I would suggest a few revisions to make it more relevant to those working on co-benefits research and policymakers.

Response: Thank you for your accurate summary of our work and your recognition of the importance of this research. We have improved the manuscript based on your constructive comments. Below, we provide our replies under each part of the comment.

Comment 1. I think either the intro or the discussion needs to tie the analysis more explicitly to some of the claims in the work on co-benefits. One of the central claims in this field of study is that quantifying near-term and local health benefits can provide a rationale for taking actions on a long-term global climate challenge. I think that this needs to be stressed.

Pearce, David. "Policy frameworks for the ancillary benefits of climate change policies."

Ancillary benefits and costs of greenhouse gas mitigation (2000): 517-560.

Nemet, G.F., Holloway, T. and Meier, P., 2010. Implications of incorporating air-quality co-benefits into climate change policymaking. *Environmental Research Letters*, 5(1), p.014007.

Zusman, E., Chae, Y., Kim, H. and Farzaneh, H., 2021. An Introduction to co-benefits: core concepts and applications. In *Aligning Climate Change and Sustainable Development Policies in Asia* (pp. 1-15). Singapore: Springer Singapore.

Karlsson, Mikael, Eva Alfredsson, and Nils Westling. "Climate policy co-benefits: a review." *Climate Policy* 20, no. 3 (2020): 292-316.

Response: Thank you for this insightful suggestion. We have linked the analysis both in the Introduction and Discussion sections more explicitly to some claims in the work on co-benefits:

Introduction section: As climate mitigation policies can simultaneously yield significant co-benefits for air quality and public health (Karlsson, Alfredsson, & Westling, 2020; Pearce, 2000; Zusan, Chae, Kim, & Farzaneh, 2021), embedding decarbonization within the long-term strategy of the shipping sector presents a critical opportunity to maximize these co-benefits (L45-47 in the revised manuscript).

While extensive research has examined the climate benefits of hydrogen adoption across various economic sectors (Hauglustaine et al., 2022; Sun, Shrestha, Hamburg, Kupers, & Ocko, 2024; World Energy Council, 2022), far less attention has been given to its near-term air quality and public health implications. Assessing these health co-benefits can provide additional motivation for accelerating actions on the long-term global climate challenge (Karlsson et al., 2020; Nemet, Holloway, & Meier, 2010) (L66-69).

Discussion section: These findings address an important a critical knowledge gap in assessing the health and economic co-benefits of clean energy transition in the shipping sector, strengthening the evidence base for explicitly integrating health and economic co-benefits into maritime decarbonization strategies (L339-341).

Comment 2. At the same time as you make this point, I also think it is important to underline that the much of the work on co-benefits has focused on conventional energy solutions; I do not think that there has been much work done on shipping and hydrogen. You are therefore filling an important gap.

Response: To highlight this research gap, we have added the following content in the Introduction section (L69-72): To date, assessments of health impacts from emissions reduction in global shipping have focused primarily on conventional fossil-based fuels (Bilsback et al., 2020; Y. Zhang, Eastham, Lau, Fung, & Selin, 2021), leaving the potential health and economic co-benefits of hydrogen-driven maritime decarbonization largely unexplored.

Comment 3. I felt that the policy recommendation and implications was rather weak. There is sentence that basically suggests that local governments should do more to regulate shipping and enable transitions to hydrogen. However, I think you could say more about what those regulations would like, what incentives might need to be in place to support their implementation, and what capacities governments at different levels would need to move them forward. There is also some attention to the not only the technical or economic feasibility but also the institutional feasibility of implementing co-benefits solutions that could be noted in this regard.

Mayrhofer, J.P. and Gupta, J., 2016. The science and politics of co-benefits in climate policy. *Environmental Science & Policy*, 57, pp.22-30.

Zusman, E., Akahoshi, K., Hanaoka, T., Malley, C.S., Wangwongwatana, S., Onmek, N., Paw-Armart, I., Thi, K.O.N., Huy, L.N., Kuylensstierna, J.C. and Hirayama, T., 2025. The truth about co-benefits: a multidimensional feasibility assessment for thailand and beyond. *Environmental Research Communications*, 7(2), p.025009.

Response: Thank you for this insightful comment. We have reinforced the policy implications of the findings and clarify the multi-dimensional feasibility considerations associated with maritime decarbonization. We now elaborate on what specific regulatory measures local and national governments could adopt to reduce shipping-related emissions and support the transition to hydrogen: regions with increased emissions from hydrogen production will require greater financial support, including subsidies from hydrogen-importing countries, differentiated port dues favoring low-emission vessels, expanded shore-power mandates, and robust certification and safety standards for maritime hydrogen use. In NO_x-saturated regimes, efforts should prioritize reducing highly reactive VOC emissions (L394-398).

A successful transition to hydrogen-based shipping fuels also requires multi-level governance with clearly defined roles and strengthened institutional capacity (Mayrhofer & Gupta, 2016; Zusman et al., 2025). Globally, the IMO should provide credible long-term signals through setting binding lifecycle-based GHG limits and a global carbon pricing mechanism. Regional and national governments can act as policy frontrunners through phased standards, market-based instruments, and co-funding the first demonstration hydrogen-powered vessels and supply infrastructure, thereby reducing early-stage investment risks and fostering sustained stakeholder engagement (Zusman et al., 2025). Ports play a critical role in policy implementation by investing in hydrogen and ammonia bunkering, applying differentiated fees to incentivize low-carbon vessels, and developing green shipping corridors to coordinate fuel supply and demand. Finally, integrating robust analyses of health and economic co-benefits into policymaking across governance levels is essential to strengthen evidence-based decisions and guide targeted investment in implementation capacity (L400-411).

Comment 4. Finally, in terms of strengthening the policy recommendations and institutional feasibility, I think you could suggest that there should be a greater emphasis in global/national/local policymaking processes in incorporating robust analysis of health and other co-benefits in decision making processes. This will help, again, to strengthen the evidence based for policy decisions and investments in the capacities to implement them.

Response: We have highlighted the importance of incorporating systematic co-benefits analyses into energy transition and maritime policy processes in the Discussion section (L409-411): Finally, integrating robust analyses of health and economic co-benefits into policymaking across governance levels is essential to strengthen evidence-based decisions and guide targeted investment in implementation capacity.

Small additional point: Please provide the citation for this claim◊previous findings. Moreover, consistent with previous findings, we find that the health co-benefits are most prominent in regions with high population density and shipping activity, particularly in Eastern and Southern Asia.

Response: We have provided citations for this claim (L356-357).

Reviewer #2 (Remarks to the Author):

The authors applied integrated air quality modeling, health impact assessment and VSL evaluation to evaluate the potential benefits from transition from fossil fuels to hydrogen in shipping. The manuscript is well drafted. However, I think the study was designed poorly and missed several pieces:

Response: We thank the reviewer for the careful evaluation of our manuscript. We have provided additional details to clarify the study design, added a comparison of major air pollutant emissions before and after the hydrogen transition, and conducted additional simulations to explicitly account for potential hydrogen leakage. Below, we provide our replies under each part of the comment.

1. When comparing the all transition to hydrogen scenario with half transition to hydrogen scenario, how the "half" was determined? The locations where the switch from fossil to hydrogen matter for both the air quality and human health effect.

Response: The Half-transition scenario is defined based on shipping energy demand, with 50% of global shipping energy transitioning from conventional fossil fuels to hydrogen, which is consistent with Warwick et al. (2023) and Glen Chua, Denise L Mauzerall, Larry Wayne Horowitz, Vaishali Naik, and Fabien Paulot (2024a). In the Half-transition scenario, emission reductions are applied proportionally at each model grid, preserving the spatial distribution of shipping activity and emission hotspots as in the baseline. This design allows us to isolate the effect of transition magnitude (full versus partial adoption) without introducing additional uncertainties associated with changes in shipping activity patterns (L493-497 in the Methods). The Half-transition scenario is designed to represent a plausible near- to medium-term pathway constrained by hydrogen supply, infrastructure deployment, or fleet transition, which is consistent with Warwick et al. (2023) and Glen Chua, Denise L. Mauzerall, Larry Wayne Horowitz, Vaishali Naik, and Fabien Paulot (2024b). The All-transition scenario provides an upper-bound estimate of the potential health and economic benefits of hydrogen-based shipping decarbonization (L481-484 in the Methods). We have revised the Methods to clarify both the quantitative definition and spatial implementation of the Half-transition scenario.

2. I was surprised to see the benefits were larger from ozone than PM2.5. After switching to hydrogen fuels, won't the sulfur emissions greatly reduced, and thus affecting the PM2.5 concentration (such as Jung et al., 2023, Lv et al., 2018)? From supporting Table 1, I did not see the comparisons for the air pollutants changes before and after switching, which the authors

should strength. A thorough comparison for major air pollutants changes should be included in the analysis, specifically for SO₂, NO_x, and BC.

Response: The larger health benefits from ozone compared with PM_{2.5} arise primarily because the hydrogen transition is evaluated against a post-IMO regulatory baseline, in which SO₂ emissions from shipping have already been substantially reduced. In the 2022 baseline scenario, global annual emissions of SO₂ and NO_x from the shipping sector are 2,080 and 11,501 kt, respectively. When all (or half) of shipping energy use is replaced by hydrogen, SO₂ and NO_x shipping emissions decrease by 1,056 (528) kt and 11,140 (5,570) kt, respectively (Table 1 below). The much larger reductions in O₃ precursors compared with PM_{2.5} precursors explain why the ozone-related health benefits dominate the total avoided mortality in our results.

The Supplementary Fig. 2 in the revised manuscript shows spatial distribution of changes in air pollutant emissions resulting from a full hydrogen transition in the shipping sector. We have added Table 1 below (Supplementary Table 2) to provide a thorough comparison for global annual emissions of major air pollutants before and after energy transition. We also added an analysis in the Results section (L122-125): Global annual emissions of major air pollutants from the shipping sector—SO₂, NO, CO, NMVOCs, and POM—decrease by 51% (25%), 97% (47%), 81% (41%), and 84% (42%), respectively, when all (half) of global shipping energy use is replaced by hydrogen (Supplementary Table 2). Notably, BC emissions from the shipping sector increase under both the All-transition and Half-transition scenarios. This occurs because baseline shipping BC emissions (16 kt/year) are negligible compared with other major air pollutants such as SO₂ (2079 kt/year) and NO_x (11501 kt/year), and BC emissions associated with hydrogen production lead to an overall increase in total shipping-related BC emissions.

Table 1. Global annual emissions (Kt/year) of major air pollutants from the shipping sector, and their changes due to energy transition towards hydrogen.

Experiments	SO ₂	NO	CO	NMVOCs	BC	POM	NH ₃
Baseline	2079.0	11501.0	1389.0	570.0	16.0	303.0	1.9
All_transition	1023.0	361.0	82.0	107.0	50.0	50.0	1.8
Changes in All_transition	-1056.0	-11140.0	-1307.0	-463.0	34.0	-253.0	-0.1
Change ratio	-50.8	-96.9	-94.1	-81.2	212.5	-83.5	-5.3
Half_transition	1551.0	5931.0	736.0	339.0	33.0	176.0	1.9

Changes in All_transition	-528.0	-5570.0	-653.0	-231.0	17.0	-127.0	0.0
Change ratio	-25.4	-48.4	-47.0	-40.5	106.3	-41.9	-2.1

3. Another major defect for the study design is that the authors fail to consider the hydrogen leakage. H₂, even though it is itself radiatively inert, undergoes similar chemical reactions to methane (CH₄) in the atmosphere, leading to detrimental climate and air quality effects. There has been several studies illustrating it. Without considering this, the results from this study would overestimate the air quality benefits from hydrogen transition, especially from ozone (Chua et al., 2024; Ocko et al., 2022)

References:

Jang, E., Choi, S., Yoo, E. et al. Impact of shipping emissions regulation on urban aerosol composition changes revealed by receptor and numerical modelling. *npj Clim Atmos Sci* 6, 52 (2023). <https://doi.org/10.1038/s41612-023-00364-9>

Lv, Z., Liu, H., Ying, Q., Fu, M., Meng, Z., Wang, Y., Wei, W., Gong, H., and He, K.: Impacts of shipping emissions on PM_{2.5} pollution in China, *Atmos. Chem. Phys.*, 18, 15811–15824, <https://doi.org/10.5194/acp-18-15811-2018>, 2018.

G Chua, DL Mauzerall, LW Horowitz, V Naik, F Paulot: AGU Fall Meeting Abstracts 2024 (2527), GH21A-2527, Ozone-related Health Impacts of Hydrogen Leakage.

Ocko, I. B. and Hamburg, S. P.: Climate consequences of hydrogen emissions, *Atmos. Chem. Phys.*, 22, 9349–9368, <https://doi.org/10.5194/acp-22-9349-2022>, 2022.

Responses: Thank you for pointing out the potential impact of hydrogen leakage on air quality and health benefit assessment. Hydrogen can leak throughout the supply chains and influence the tropospheric ozone burden (Chua et al., 2024b; Ocko & Hamburg, 2022). To assess the potential impact of hydrogen leakage on the estimated health benefits, two additional simulations are conducted following the same experimental setting as the All-transition and Half-transition scenarios respectively, but are driven by surface H₂ emissions to incorporate a high hydrogen leakage rate of 10% (Chua et al., 2024b). A hydrogen-emission-driven 2022 baseline experiment is also conducted to estimate the health benefits under energy transition scenarios accounting for hydrogen leakage. Anthropogenic H₂ emissions in 2022 are estimated from CO emissions in CAMS-GLOB-ANT_v6.2 using source-specific factors (Paulot et al., 2021), with global biomass burning emissions from the input data sets for Model Intercomparison Projects (input4MIPs). Emission inventories accounting for hydrogen leakage are constructed following Chua et al. (2024b), applying 130× and 65× scaling factors to baseline shipping H₂ emissions when all or half of shipping energy is shifted to hydrogen, respectively. This results in total anthropogenic H₂ increases of +49% and +25% (L508-520 in the Methods section).

Our results indicate that when considering a high leakage rate of 10% in the transition scenarios, the changes in spatial pattern and magnitude of MDA8 ozone remain largely unchanged relative

to the correspond All-transition or Half-transition scenarios, with differences generally within 0.5 ppb (Fig. 2 in the revised paper vs. Figure 1 below) (L170-173 in the Results). Under the high hydrogen leakage scenarios, global ozone-attributable premature mortality is estimated at 100,078 (95% CI: 47,194–164,048) and 35,858 (95% CI: 17,130–58,314) when all or half of shipping energy demand transitions to hydrogen, respectively, representing changes of 1.7% and 3.4% compared with the corresponding no-leakage cases. These modest increases are likely within the range of internal model uncertainty (see Methods). Moreover, the spatial distribution of ozone-attributable health benefits remains virtually unchanged, indicating that the main conclusions of this study are robust even under an extreme hydrogen leakage assumption (Fig. 4 in the revised manuscript vs. Fig. 2 below) (L295-302 in the Results). Nevertheless, hydrogen leakage should be considered in future assessments, particularly as hydrogen deployment expands across sectors and production scales increase (Chua et al., 2024a) (L364-365 in the Discussion). We have added the above analysis in corresponding sections in the revised manuscript.

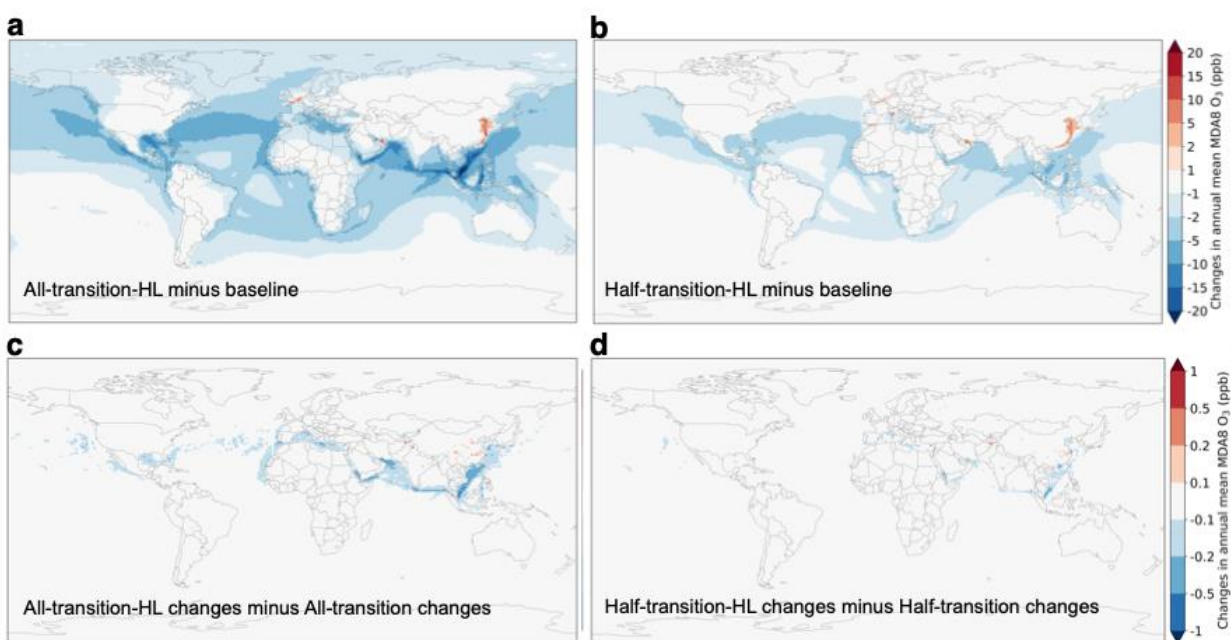


Figure 1. Changes in MDA8 O₃ concentration resulting from the energy transition to hydrogen in the shipping sector when accounting for hydrogen leakage. a, b, Changes in global ground-level Maximum Daily Average 8 h (MDA8) ozone concentration (ppb) when all (a) or half (b) of the shipping sector's energy use shifts to hydrogen and a hydrogen leakage rate of 10% is considered. **c,d,** Differences in MDA8 ozone changes between scenarios with hydrogen leakage and their corresponding transition scenarios without leakage (All-transition-HL vs. All-transition (c); Half-transition-HL vs. Half-transition (d)).

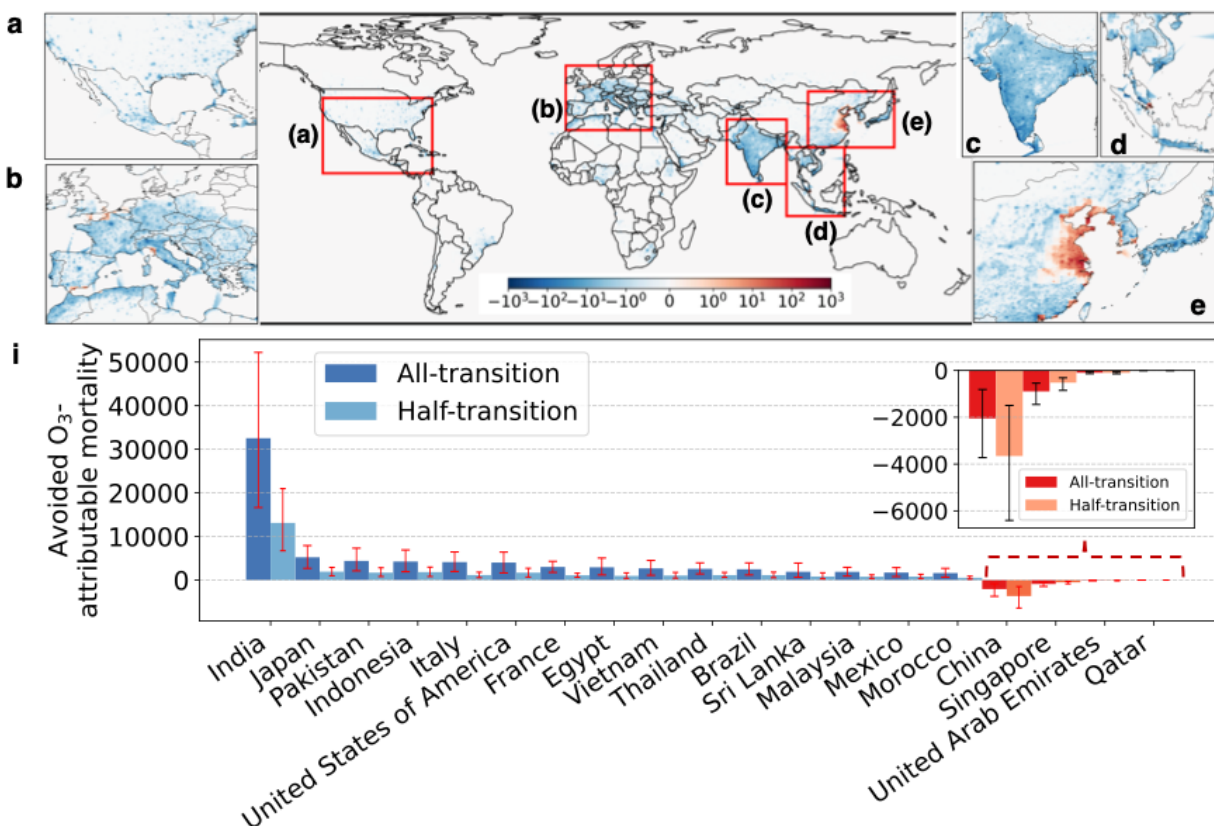


Figure 2. Health benefits from avoided O₃-attributable premature mortality due to the energy transition in global shipping when accounting for H₂ leakage. Global distribution of ozone-attributable mortality changes and zoomed-in maps (outlined by red rectangles) over **a** the United States and Mexico, **b** the Mediterranean basin, **c** South Asia, **d** South-Eastern Asia, and **e** East Asia. The bar chart in panel **i** shows the country-level avoided mortality in All-transition and Half-transition scenarios. The dark and light blue bars indicate that O₃-attributable mortality in a given country decreases when all or half of the shipping sector's energy use shifts to hydrogen, respectively. The dark and light red bars indicate that O₃-attributable mortality increases when all or half of the shipping sector's energy use shifts to hydrogen, respectively.

Reviewer #3 (Remarks to the Author):

Paper explores the health and cost co-benefits of transitioning shipping to hydrogen, preventing premature deaths and diminishing the costs associated to health by improving air quality. Timely and important matter to discuss. Although the results seem interesting, there a few nuances that need to be further explored or explained in the paper.

Response: We appreciate your acknowledgment of this work's timeliness and significance. In response to your helpful suggestions, we have revised the manuscript to provide further exploration and explanation of several nuances.

Comment 1. It would be interesting to add context to the averted deaths, how many total are attributable to shipping as is? Then the reader would have a better understanding if the 132k averted deaths are impactful or not.

Response: Thank you for this helpful suggestion. To provide context for the estimated averted mortality under the shipping transition scenarios, we have conducted a counterfactual simulation in which shipping emissions are removed from the 2022 baseline, allowing the estimation of shipping-attributable mortality in the present-day climate (L484-487 in the Methods).

Our results show that maritime emissions are estimated to account for 43,689 (95% CI: 30,857–57,508) PM_{2.5}-attributable and 100,263 (95% CI: 47,319–164,273) ozone-attributable deaths. These estimations are consistent with the findings from other studies (Bilsback et al., 2020; Winebrake, Corbett, Green, Lauer, & Eyring, 2009). The resulting improvements in ambient air quality are estimated to prevent 130,475 (95% CI: 64,749–207,941) and 53,107 (95% CI: 23,257–87,000) premature mortalities annually from PM_{2.5} and ozone exposures in the All-transition and Half-transition scenarios, representing 91% and 37% of total air pollution-attributable mortalities due to shipping emissions in 2022, respectively (L190-199).

We have also added the corresponding context for the avoided PM_{2.5}- and ozone-attributable mortality, respectively: the global avoided annual PM_{2.5}-attributable premature mortalities in the All-transition and Half-transition are estimated at 32,041 (95% CI: 22,370–42,552) and 18,416 (95% CI: 12,921–24,379), respectively, accounting for 73% and 42% of total PM_{2.5}-attributable mortality due to shipping emissions (L208-210); the health benefits from reductions in ozone concentration exceed those associated from PM_{2.5}, with 98,434 (95% CI: 42,413–165,354) and 34,692 (95% CI: 10,354–62,602) ozone-attributable premature mortalities avoided under the All-transition and the Half-transition scenarios, respectively, representing 98% and 35% of total O₃-attributable mortality due to maritime emissions (L252-255).

Comment 2. English could be improved for better readability of some sentences, such as the one starting on line 70. So sentences throughout the paper are somewhat confusing to read.

Response: We have thoroughly revised the manuscript to improve the English language and readability throughout. As an example, the sentence beginning on line 70 in the original manuscript has been revised to: We design feasible transition scenarios based on the production capacity of globally announced hydrogen projects by 2050 (IEA, 2024), accounting for the life-cycle emissions of hydrogen production (L76-80 in the revised manuscript).

Comment 3. Line 100 - The authors should add an explanation as to why the SO₂ land-based emissions increase. The supplementary data does provide some insight, but there are countries that have no SO₂ emissions for the energy sector, so have the authors considered that the energy production mix of said countries is limited for hydrogen production and would require new

production from fossil-based sources to increase hydrogen production? That would be counter-intuitive and against current energy policy.

Response: In our simulations, the hydrogen required for the shipping transition is predominantly green hydrogen produced from renewable or nuclear energy (IEA, 2024), with a small share of blue hydrogen and no grey hydrogen. Therefore, the observed increase in land-based SO₂ emission is not due to additional fossil electricity generation. Instead, it primarily originates from process-related and upstream emissions associated with green and blue hydrogen production, particular the manufacturing of wind turbines and solar panels, electrolysis production and operation, and hydrogen compression and storage (Mann, 2001, 2004; Simons & Bauer, 2011). We have provided more information on the IEA hydrogen projects and added the explanation for the SO₂ land-based emissions increase in the revised manuscript: These hydrogen projects are expected to produce approximately 200,800 kt of hydrogen annually—88% green, 11.8% blue, and 0.2% biomass-based hydrogen with CCUS—of which 30% and 15% are allocated to the shipping sector under the All-transition and Half-transition scenarios, respectively (L106-108); Land-based SO₂ emissions increase primarily due to additional releases from green and blue hydrogen production, with the highest emissions in South America, Australia and New Zealand, and North America, at 130, 121, and 116 kt per year, respectively (L117-120).

Comment 4. No contextualization is given when introducing the changes to PM_{2.5}. The reader needs to be aware of what the base level concentrations are to know if a 1 µg/m³ variation is of any significance, especially in cases where the difference is 0.1. This is further needed when the authors use words such as "significantly". A percentage would also help. The same applies to O₃.

Response: Thank you for this helpful suggestion. To provide contextualization when analyzing the impact of the shipping transition on air pollution concentration, we have added Figure 3 below (Supplementary Fig. 3 in the revised manuscript) showing the relative change in PM_{2.5} and O₃ concentration, defined as the difference between the transition and baseline scenarios normalized by baseline levels. We have also added corresponding analysis in the main text: Some East and Southeast Asian countries show reductions exceeding 1 µg/m³, corresponding to approximately 2–10% of baseline PM_{2.5} concentration (Supplementary Fig. 3), and reductions greater than 0.5 µg/m³ (approximately 4–10%) occur in several Mediterranean coastal countries (L142-145); Additionally, noticeable increases in PM_{2.5} concentration (0.1–1.0 µg/m³; 2–10% of baseline concentration) are observed in several regions, particularly the United States Gulf Coast, Northern Chile, Western European countries and Northwest India (L146-149); In East Asia, Southeast Asia, and South Asia, PM_{2.5} decreases by approximately 0.1–0.5 µg/m³ (1–4% of baseline concentration) (L152-153); Ozone levels decrease by more than 10 ppb along major international shipping routes, equivalent to approximately 20% of baseline ozone concentration (L159-161); Coastal land areas experience reductions of 1–5 ppb, corresponding to approximately 2–15% of baseline levels (L162-163).

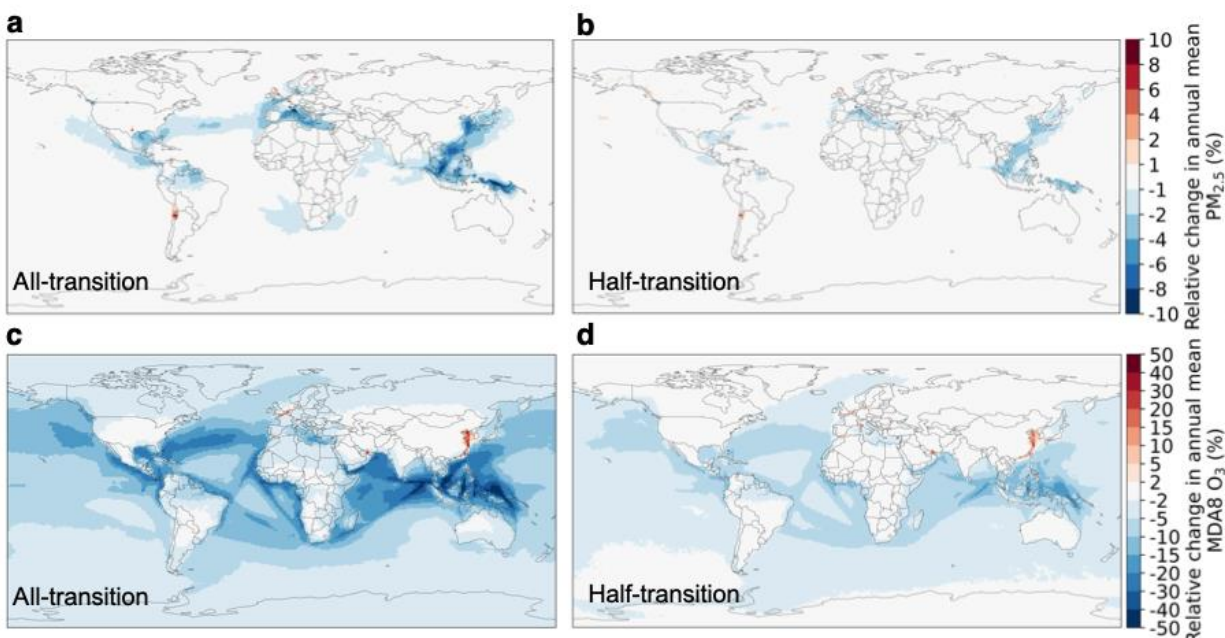


Figure 3. Relative changes in air pollution concentration resulting from the energy transition to hydrogen in global shipping. a, b, Relative changes (%) in global ground-level PM_{2.5} concentration when all (a) or half (b) of the shipping sector's energy use shifts to hydrogen, defined as the difference between the transition and baseline scenarios normalized by baseline PM_{2.5} levels. **c, d,** Relative changes (%) in global ground-level Maximum Daily Average 8 h (MDA8) ozone concentration when all (c) or half (d) of the shipping sector's energy use shifts to hydrogen, defined as the difference between the transition and baseline scenarios normalized by baseline O₃ levels.

Comment 5. It would be good if the resolution of Fig. 3 was increased.

Response: We have improved the resolution of Fig. 3 and Fig. 4 in the revised manuscript.

Comment 6. The authors should also consider a significant cost in the co-benefits of shipping transition, the costly retrofit of older vessels to use hydrogen and new vessels alike. The cost of a full fleet conversion could invert the economic benefits of the health impacts.

Response: Thank you for this insightful suggestion. Based on publicly available data and literature, we have conducted a preliminary estimate of the cost of a full fleet transition and compared it with the economic benefits associated with avoided mortalities (L382-387 in the Discussion section): High costs may arise from fleet renewal and new hydrogen-powered vessels construction; assuming that 112,501 hydrogen-powered merchant vessels above 100 gross tons (UNCTAD, 2025) are newly built worldwide before 2050, with an estimated cost of 47.5 million US\$ per vessel (W. Zhang et al., 2025), the total cost would be approximately 214 billion US\$ per year. Nevertheless, the annual economic benefits associated with avoided mortalities due to shipping transition could offset these expenses (309 billion vs. 214 billion US\$).

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Response to Reviewers

The term “revised manuscript” in this context denotes the PDF file we have submitted, which clearly highlights the modifications made from the previous version. We have addressed each comment raised by Reviewer #2. For clarity, we have presented the reviewers’ comments in blue and our responses in black. The numbering of figures and tables here is consistent with that in the main manuscript and the Supplementary Information.

General Comments:

Thank the authors trying to answer my comments. However, until now, I did not believe the authors fully address them.

Response: Thank you for reviewing our revisions. We have further revised and improved the manuscript in response to your latest comments. Below, we provide detailed responses to each part of your comment.

Comment 1. Related with my concerns about the locations of hydrogen switching, the authors frequently cited the Chua et al., (2024a, b), which were both conference abstracts. I am not sure how solid/robust conclusions I can draw about the justification of the methodology from these simplified abstracts. Warwick et al. (2023) discussed similar scenarios about green hydrogen switching and then focused on their discussions mainly based on the lower boundary conditions (LBC) of H₂ concentration caused by the clean energy switching, which not justify the methods the authors applied, which was based on H₂ emission driven.

Comparing between Fig.2 and Fig.4, I was very astonished to see that some countries, such as India, experienced such large air-pollution related mortality burden changes in the whole country, when the air quality changes appears to be only noticeable in coastal regions (Fig.2).

Response: We apologize for the misunderstanding. We cited Warwick et al. (2023) and Chua et al. (2024) to justify the plausibility of transitioning 50% of global shipping energy to hydrogen, instead of explaining the locations of hydrogen switching. The Half-transition scenario represents a plausible near- to medium-term pathway and follows the assumption of a 50% fossil fuel-to-hydrogen transition in the transport sector adopted by Warwick et al. (2023) and Chua, Naik, Paulot, Feng, and Horowitz (2025) (L404-407 in the revised manuscript). Besides, we have replaced the citation of Chua et al., (2024) with a peer-reviewed publication (Chua et al., 2025), which provides a detailed description of the methodology.

Regarding the locations of hydrogen transition, to preserve the spatial distribution of shipping activity and emission hotspots in the baseline, emission reductions are applied proportionally at each model grid in the Half-transition scenario. This design allows us to isolate the effect of transition magnitude (full versus partial adoption) without introducing additional uncertainties associated with changes in shipping activity patterns (L418-423 in the revised manuscript). In addition, we account for emissions associated with hydrogen production. The spatial locations of

these emissions are determined based on IEA Hydrogen Production Projects Database (IEA, 2024).

To better illustrate the changes in MDA8 O₃ and ozone-attributable premature mortality, we have improved the colorbars in Figures 2 and 4 in the revised manuscript. The spatial patterns of mortality changes closely correspond to those of MDA8 O₃ changes, with more pronounced reductions along the west coast of India compared with its inland regions. Ozone-attributable premature mortality is determined by MDA8 O₃ concentration, population, and baseline mortality rates for cardiovascular and respiratory-related diseases (see Methods in the revised manuscript). In the west coast of India, ozone concentrations decrease by approximately 2–5 ppb, whereas inland regions experience smaller reductions of 0.5–2 ppb in the All-transition scenario (Fig. 2c). These changes lead to corresponding variations in relative risk for excessive ozone exposure (Supplementary Fig. 15b). Combined with India's large population and relatively high baseline mortality rates (Supplementary Fig. 13 and Supplementary Fig. 14c, d), this explains the spatial pattern of ozone-related mortality changes shown in Figure 4.

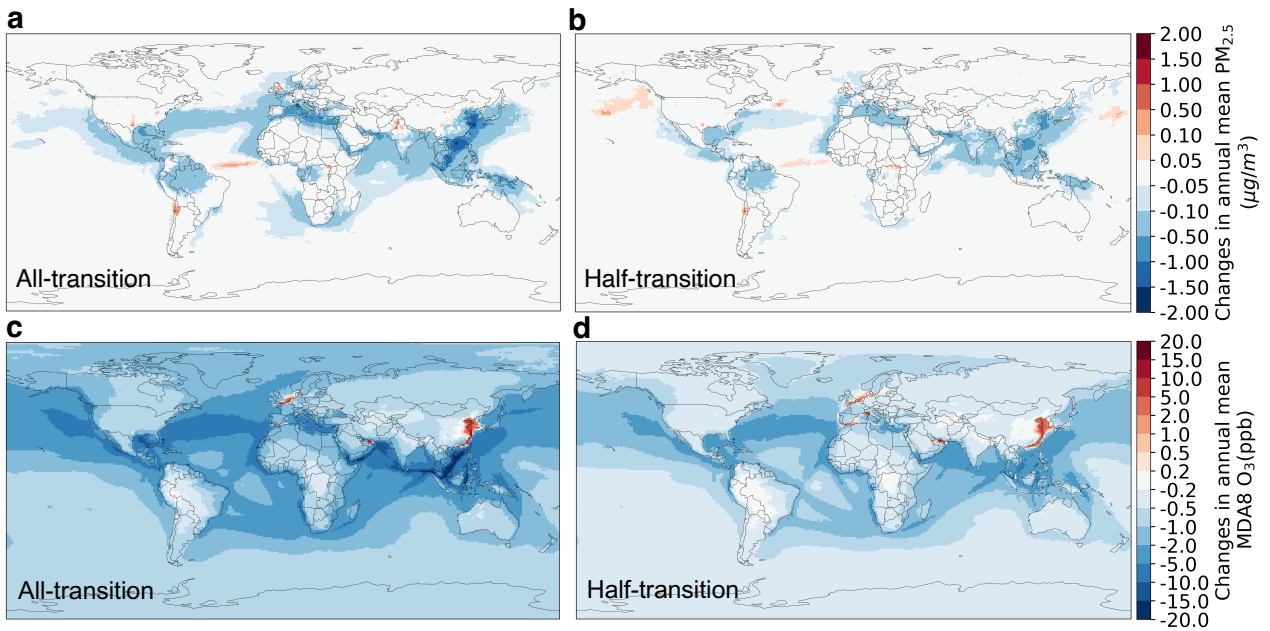


Figure 2. Changes in air pollution concentration resulting from the energy transition to hydrogen in global shipping. a, b, Changes in global ground-level PM_{2.5} concentration (µg/m³) when all (a) or half (b) of the shipping sector's energy use shifts to hydrogen. **c, d,** Changes in global ground-level Maximum Daily Average 8 h (MDA8) ozone concentration (ppb) when all (c) or half (d) of the shipping sector's energy use shifts to hydrogen.

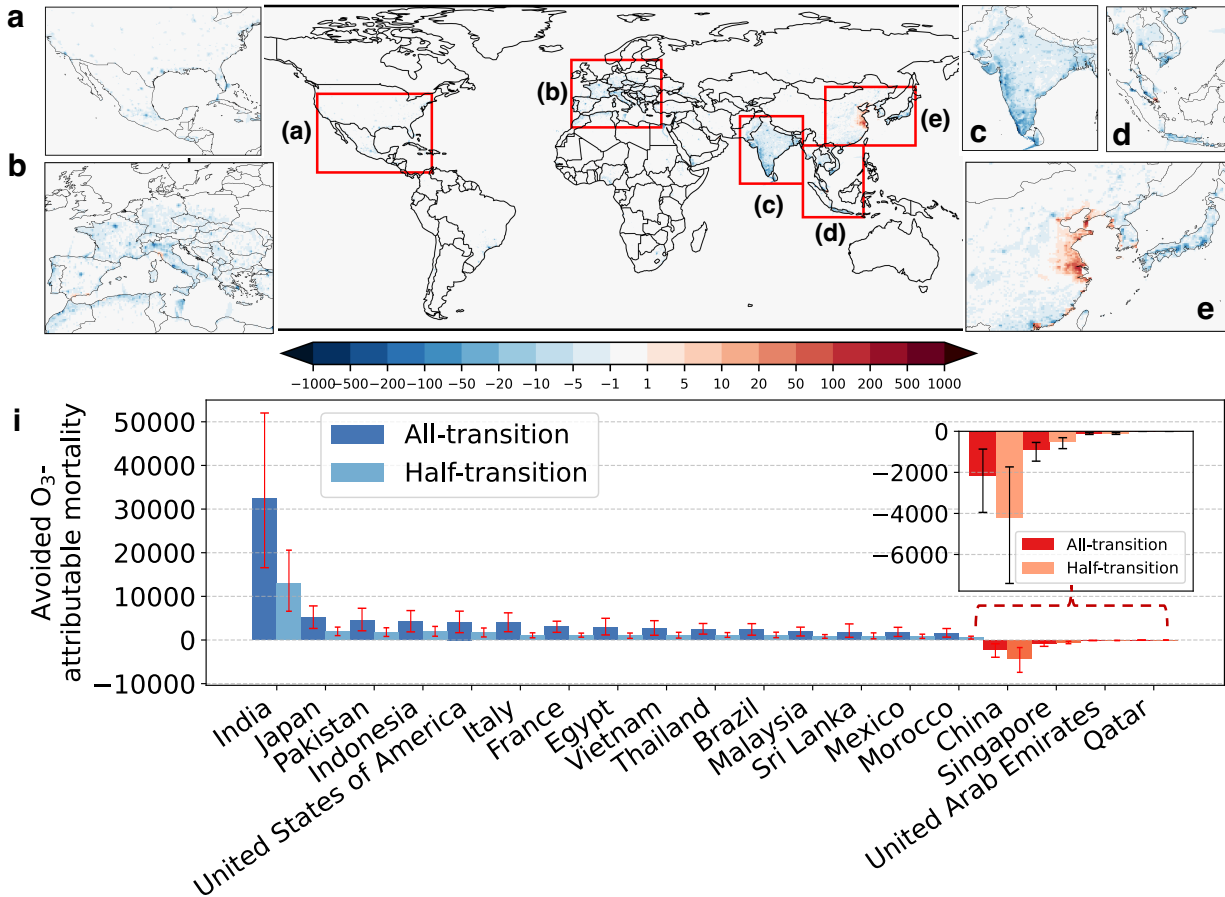
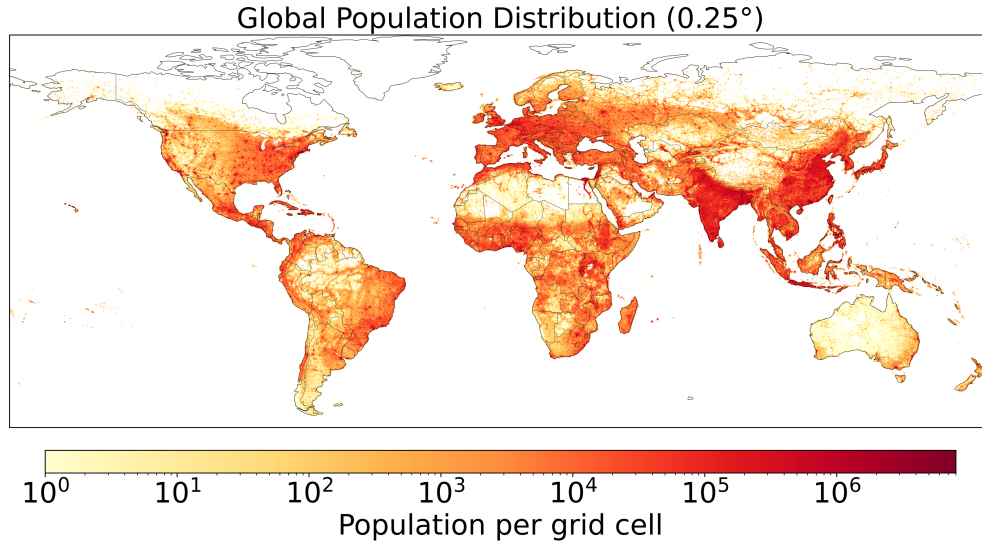
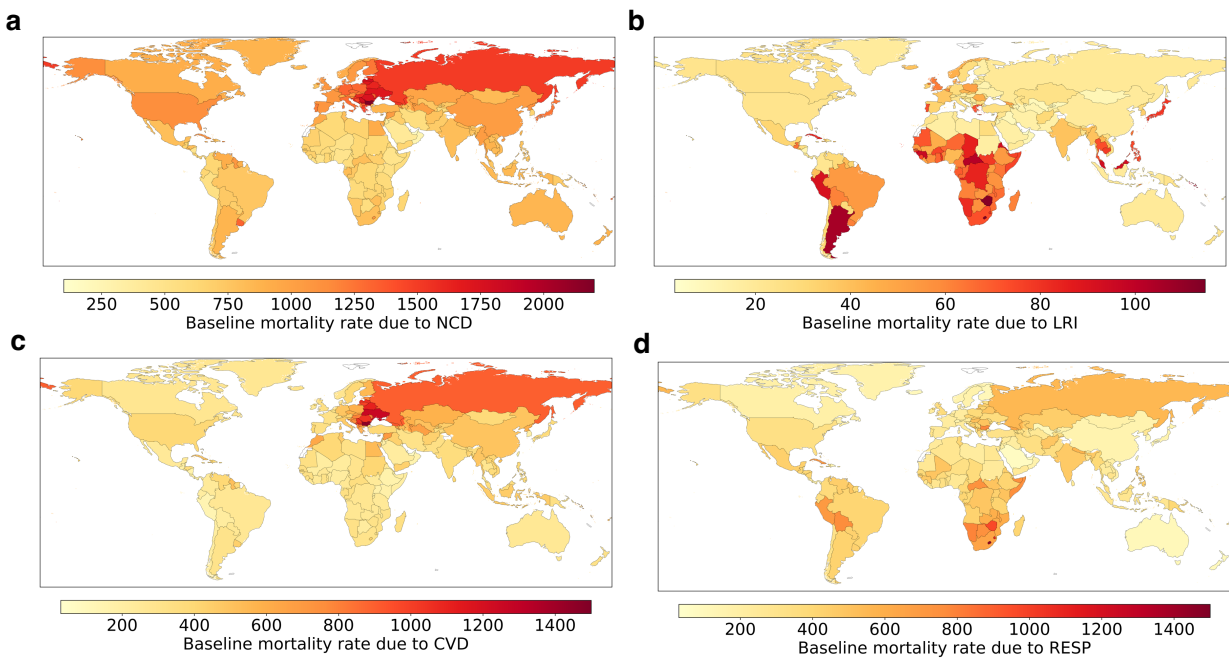


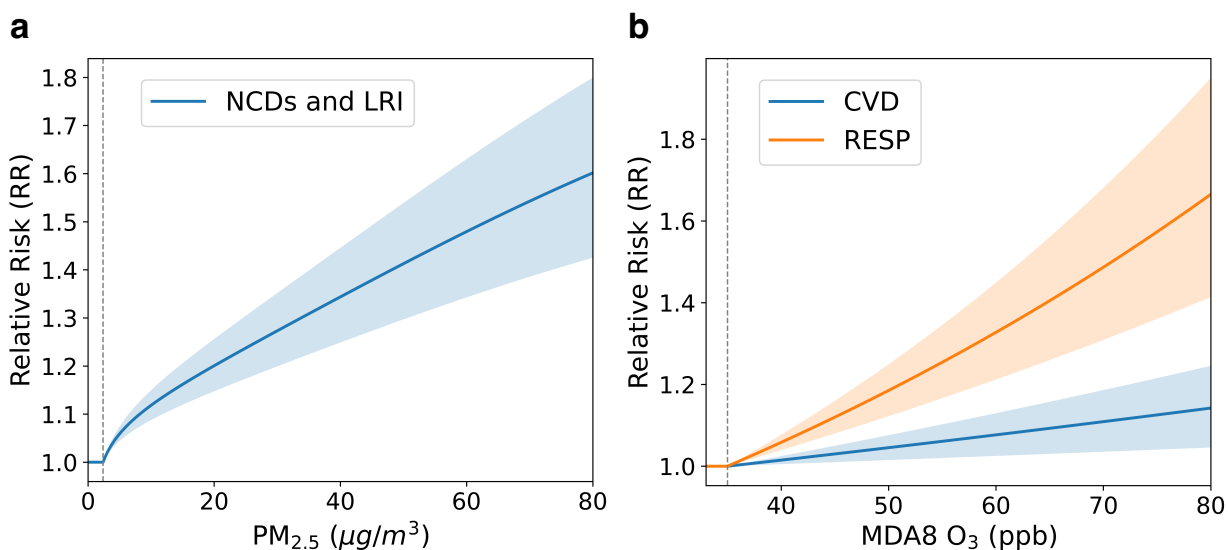
Figure 4. Health benefits from avoided O₃-attributable premature mortality due to the energy transition in global shipping. Global distribution of ozone-attributable mortality changes (transition scenarios minus baseline) and zoomed-in maps (outlined by red rectangles) over **a** the United States and Mexico, **b** the Mediterranean basin, **c** South Asia, **d** Southeast Asia, and **e** East Asia. The bar chart in panel **i** shows the country-level avoided mortality in All-transition and Half-transition scenarios. The dark and light blue bars indicate that O₃-attributable mortality in a given country decreases when all or half of the shipping sector's energy use shifts to hydrogen, respectively. The dark and light red bars indicate that O₃-attributable mortality increases when all or half of the shipping sector's energy use shifts to hydrogen, respectively.



Supplementary Fig. 13. Distribution of global population aged 25 years and above for 2019 at 0.25 degree resolution.



Supplementary Fig. 14. Baseline mortality rate due to **a**, non-communicable diseases (NCDs); **b**, lower respiratory infections (LRI); **c**, cardiovascular diseases (CVD) and **d**, respiratory diseases (RESP) for ages above 25, respectively.



Supplementary Fig. 15. Relative risk associated with excessive air pollution exposure. **a**, PM_{2.5} exposure for non-communicable diseases (NCDs) and lower respiratory infections (LRI). **b**, O₃ exposure for cardiovascular diseases (CVD) and respiratory diseases (RESP).

Comment 2. The explanations for the magnitude changes between ozone and PM2.5 seems okay. However, From the Table 1 for the anthropogenic emissions from half (all) transition, the authors' explanations for BC increases did not make any sense at all. "Notably, BC emissions from the shipping sector increase under both the All-transition and Half-transition scenarios. This occurs because baseline shipping BC emissions (16 kt/year) are negligible compared with other major air pollutants such as SO₂ (2079 kt/year) and NO_x (11501 kt/year), and BC emissions associated with hydrogen production lead to an overall increase in total shipping-related BC emissions." I'd like to see how the authors' response to the BC increases.

Response: Thank you for pointing this out. Below we provide a clearer explanation for the increase in global BC emissions from the shipping sector. In All-transition and Half-transition scenarios, BC emissions of shipping sector decrease by 13.5 kt and 6.8 kt relative to baseline, respectively. However, hydrogen production introduces additional BC emissions of 53.8 kt and 26.9 kt, resulting in net increases of 40.3 kt and 20.2 kt, respectively. We have added global emissions of major air pollutants associated with H₂ production (Cho, Strezov, & Evans, 2022; Simons & Bauer, 2011) in the Supplementary Table 2.

Supplementary Table 2. Air pollutant emissions from shipping and hydrogen production under baseline and transition scenarios (Kt/year). Global annual emissions of SO₂, NO, NMVOCs, BC, POM and NH₃ from the shipping sector in the baseline, All-transition and Half-transition scenarios. Emissions associated with hydrogen production in the All-transition and Half-transition scenarios are also included. "Total emissions" of the two transition scenarios represent the sum of shipping emissions and emissions from hydrogen production. "Changes relative to baseline" indicate the net emission changes resulting from the shipping transition.

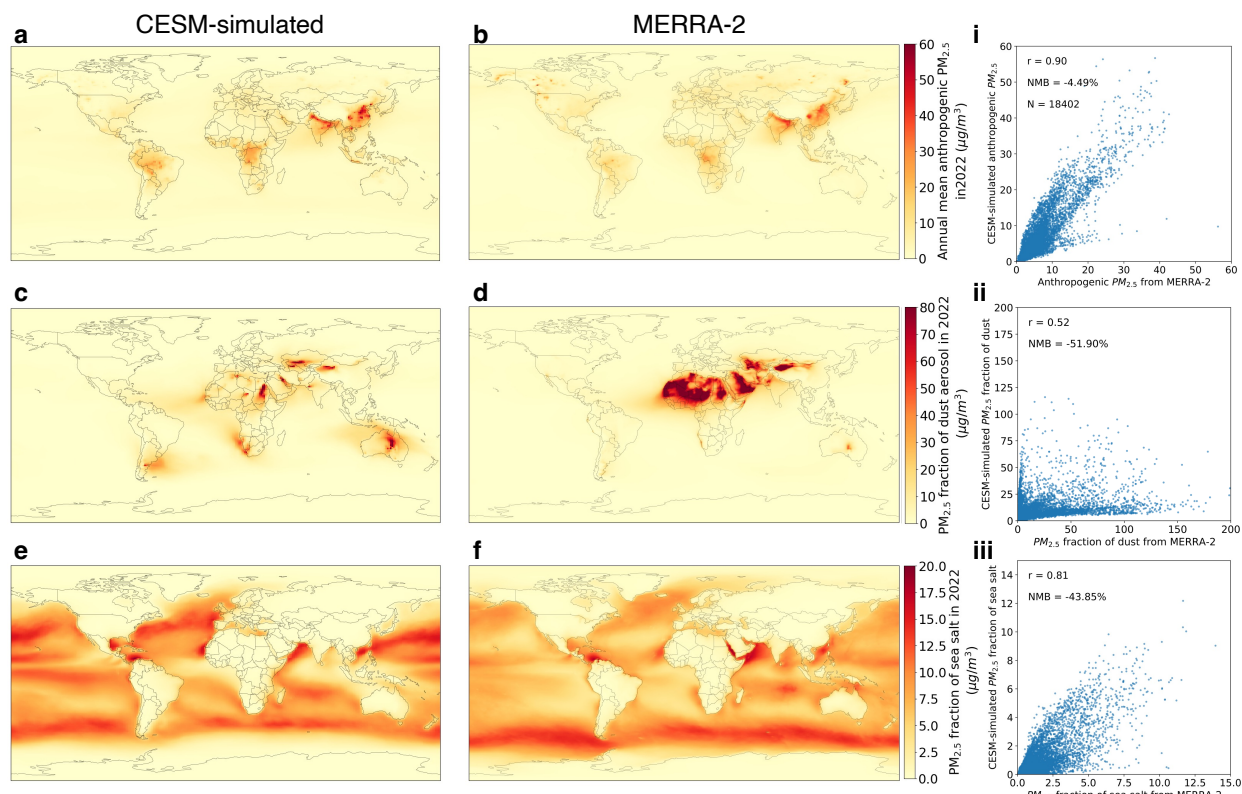
Experiments	SO ₂	NO	CO	NMVOCs	BC	POM	NH ₃
Baseline shipping	2076.3	11485.6	1387.8	569.6	13.5	259.4	1.9
All-transition scenario							
All-transition shipping	0	0	0	0	0	0	0
H ₂ production emissions	1023.4	361.0	82.4	106.3	53.8	63.3	1.8
Total emissions	1023.4	361.0	82.4	106.3	53.8	63.3	1.8
Changes relative to baseline	-1052.9	-11124.6	-1305.4	-463.3	40.3	-196.1	-0.1
Change ratio (%)	-50.7	-96.9	-94.1	-81.3	299.5	75.6	-5.3
Half-transition scenario							
Half-transition shipping	1038.2	5742.8	693.9	285.0	6.8	129.7	1.0
H ₂ production emissions	511.7	180.5	41.2	53.4	26.9	31.7	0.9
Total emissions	1549.9	5923.3	735.1	338.0	33.7	161.4	1.86
Changes relative to baseline	-526.4	-5562.3	-652.7	-231.6	20.2	-98.0	0.04
Change ratio	-25.4	-48.4	-47.0	-40.7	149.6%	37.8%	-2.1

Comment 3. For the model evaluation in the supporting, I found the authors did not report any statistical evaluation for the air pollutants. Meanwhile, the bias correction were only applied for the dust and aerosol, which are two components of PM_{2.5}. I will suggest the authors perform a thorough model evaluation as well as bias-correction for both total PM_{2.5} and ozone, either using the satellite data, or multi-data fused surface data.

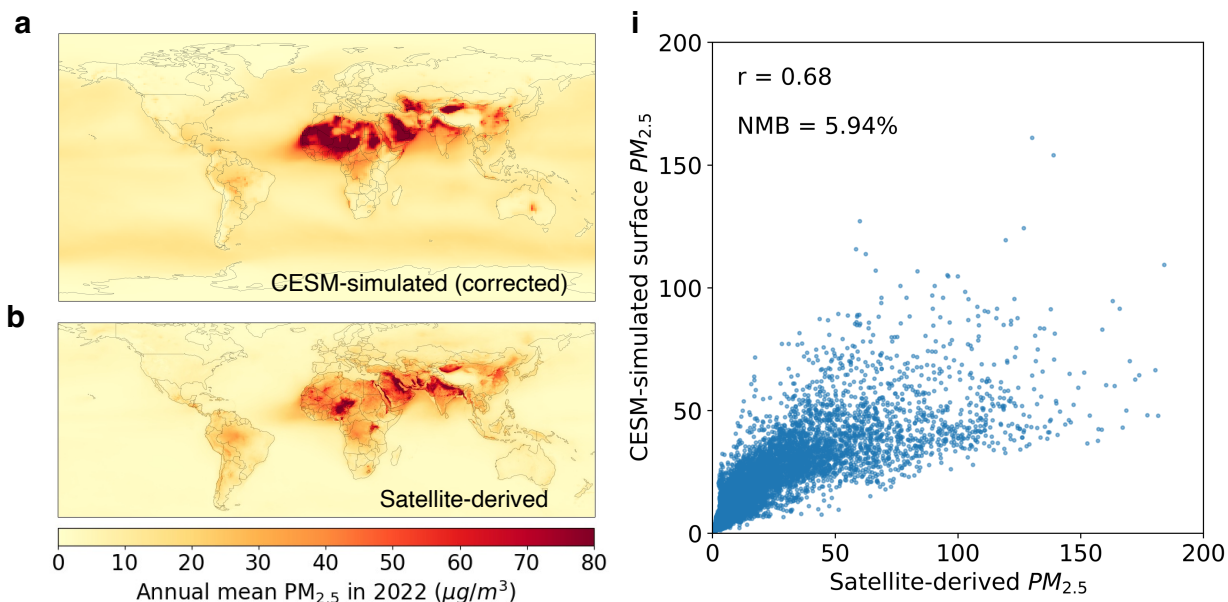
Response: Thank you for the helpful suggestion. We have conducted a thorough model evaluation for surface PM_{2.5} and MDA8 ozone concentration (Model evaluation section in the revised manuscript). Based on the statistical evaluation, we applied bias correction only to the

dust and sea salt components of PM_{2.5} concentration. The simulated PM_{2.5} excluding dust and sea salt, as well as surface MDA8 O₃, show good agreement with reanalysis data, satellite data or multi-source fused data.

CESM-simulated surface PM_{2.5} and MDA8 ozone concentrations are evaluated against multiple global datasets. Compared with MERRA-2 reanalysis data (Global Modeling and Assimilation Office (GMAO), 2015), CESM 2.2 performs well in simulating the spatial distribution of global surface PM_{2.5} (excluding dust and sea salt components) in 2022 with a Pearson's correlation coefficient (r) of 0.90 and a normalized mean bias (NMB) of -4.5% (18,402 data pairs), meeting recommended performance benchmarks ($r \geq 0.5$ and $|NMB| \leq 15\%$) (Emery et al., 2017) (Supplementary Fig. 10). Larger discrepancies are observed for the dust and sea salt components, with $r = 0.52$ and $NMB = -4.5\%$ for dust, and $r = 0.81$ and $NMB = -43.9\%$ for sea salt. Because meteorological conditions are nudged toward MERRA-2, changes in PM_{2.5} concentration under shipping transition scenarios are assumed to be primarily driven by anthropogenic emission changes, whereas dust and sea salt aerosol concentrations are expected to remain largely unaffected by the energy transition. Nevertheless, given the nonlinear relationship between PM_{2.5} exposure and health outcomes (Burnett et al., 2018), we correct CESM-simulated surface PM_{2.5} in the baseline and transition scenarios by replacing the dust and sea salt components with those from MERRA-2 reanalysis for 2022 (Global Modeling and Assimilation Office (GMAO), 2015). The bias-corrected PM_{2.5} concentration agrees reasonably well with satellite-derived PM_{2.5} (Shen et al., 2024), with $r = 0.68$ and $NMB = 5.9\%$ ($N=18,402$), meeting the evaluation criteria (Supplementary Fig. 11) (L448-463 in the revised manuscript).

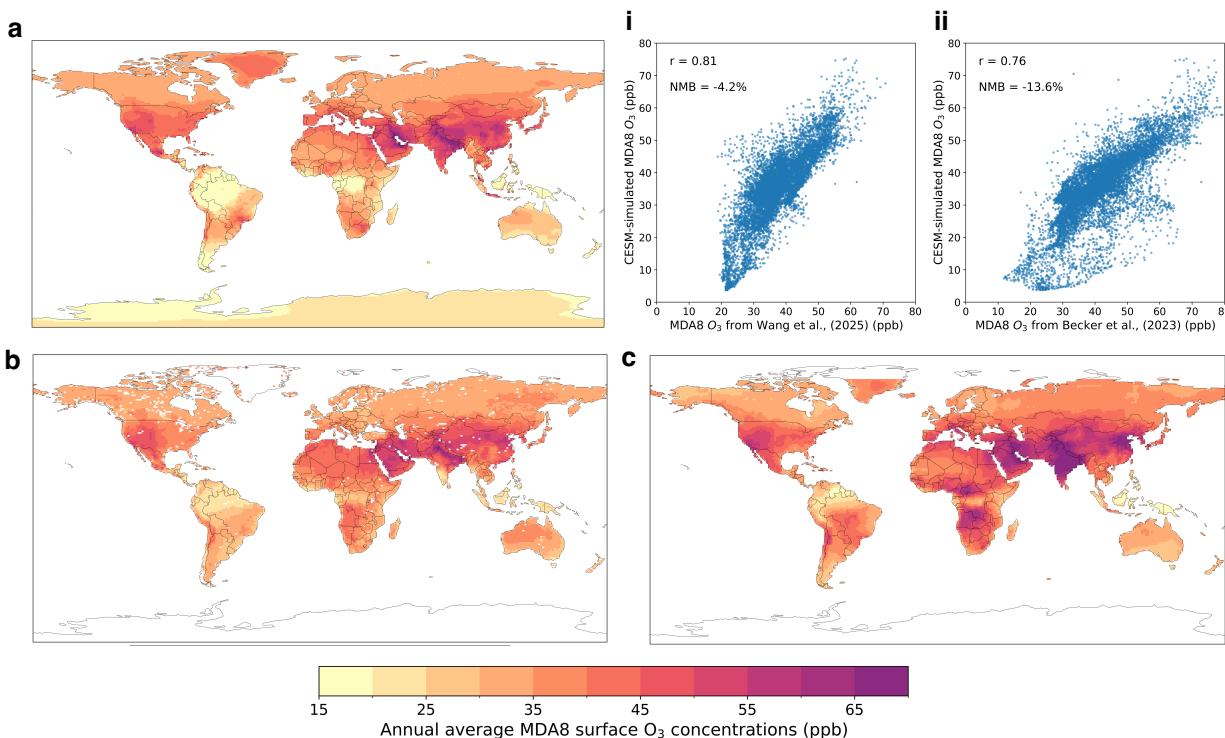


Supplementary Fig. 10. Comparison between CESM-simulated surface PM_{2.5} with MERRA-2 reanalysis dataset. **a, b**, Annual mean surface PM_{2.5} concentration (excluding dust and sea salt fractions) in 2022 ($\mu\text{g}/\text{m}^3$) from the CESM baseline experiment and MERRA-2 dataset (Global Modeling and Assimilation Office (GMAO), 2015), respectively. **c, d**, Annual mean surface PM_{2.5} fraction of dust aerosol in 2022 ($\mu\text{g}/\text{m}^3$). **e, f**, Annual mean surface PM_{2.5} fraction of sea salt aerosol in 2022 ($\mu\text{g}/\text{m}^3$). **i-iii**, Scatter plots comparing CESM simulation results and MERRA-2 for PM_{2.5} excluding dust and sea salt (i), dust (ii) and sea salt (iii). Each point represents a paired grid cell value. The Correlation coefficient (r) and Normalized mean bias (NMB) are shown in the upper left corner of panels **i-iii**.



Supplementary Fig. 11. Comparison between bias-corrected CESM-simulated surface $PM_{2.5}$ with satellite-derived data. **a, b, Spatial distribution of** annual mean bias-corrected CESM-simulated surface $PM_{2.5}$ concentration and satellite-derived surface $PM_{2.5}$ (Shen et al., 2024) in 2022 ($\mu g/m^3$). **i, Scatter plot comparing** bias-corrected $PM_{2.5}$ with satellite-derived $PM_{2.5}$. Each point represents a paired grid cell value over global land ($N = 18,402$). The Correlation coefficient (r) and Normalized mean bias (NMB) are shown in the upper left corner of panel **i**.

Surface MDA8 ozone under the baseline is evaluated against two independent global ozone datasets: a geospatial ensemble learning–based multi-source data fusion product (Wang et al., 2025) and a dataset derived from regionalized air quality model performance combined with Bayesian maximum entropy data fusion (Becker et al., 2023). Across global land, CESM 2.2 reproduces the spatial distribution of surface MDA8 O_3 with $r = 0.82$ (NMB = -4.2%) and 0.76 (-13.6%) relative to the two datasets (11,798 and 12,621 data pairs, respectively; Supplementary Fig. 12), meeting the evaluation criteria (Emery et al., 2017) (L465-470 in the revised manuscript). Therefore, we believe the simulated MDA8 O_3 concentration in this study appear sufficiently reasonable and additional bias correction is not applied in this study.



Supplementary Fig. 12. Comparison between CESM-simulated MDA8 surface ozone with other studies. Global annual average of Maximum Daily Average 8 h (MDA8) surface ozone concentration **a** in the baseline experiment for 2022, from **b** Wang et al. (2025), and from **c** Becker et al. (2023). Scatter plots of **i** CESM-simulated MDA8 O₃ versus MDA8 O₃ from Wang et al. (2025), **ii** CESM-simulated MDA8 O₃ versus MDA8 O₃ from Becker et al. (2023). Each point represents a paired grid cell value. The Correlation coefficient (r) and Normalized mean bias (NMB) are shown in the upper left corner of panels **i** and **ii**.

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Reviewer #2 (Remarks to the Author):

Thanks the authors for addressing my comments. I have the following comments about the figures and model evaluation:

1. In Supporting Fig. 14, add the units for the baseline mortality rates.

Responses: We have added the units for the baseline mortality rates, which are “deaths per 100,000 population in 2021”.

2. correlation does not mean everything especially for the global model evaluation. It only demonstrate that the model in this study can simulate the general spatial patterns of both PM_{2.5} and ozone as other datasets. However, it does not say how best the model performs, without comparing with surface observations. I'd like to see the relative differences for both PM_{2.5} and O₃ related with Fig. 11 and 12 in the supporting, as well as the IOA metrics between this study and the other datasets.

Responses: We thank the reviewer for this helpful comment regarding model evaluation.

In the revised manuscript and Supplementary Information, we have strengthened the evaluation in several aspects. First, we now include the spatial distribution of absolute and relative differences for both PM_{2.5} and MDA8 O₃ (Supplementary Figs. 11.2 and 12.2), and additionally report the Index of Agreement (*d*) to provide a more comprehensive statistical assessment (Supplementary Figs. 11.1 and 12.1). The formula for the Index of Agreement is based on Willmott (1981). Additionally, we briefly discuss previous evaluation literature of aerosols, PM_{2.5} and O₃ simulated by CESM2 (CAM6-chem with MOSAIC and MAM4) used in this study, thereby further supporting the reliability of our simulations.

A new section (Supplementary Note 1) has been added to present the full details of model evaluation. The evaluation methods and key results are summarized in the Methods section of the main text (L445–478 in the revised manuscript).

Supplementary Note 1: Evaluation of simulated surface PM_{2.5} and ozone concentrations

We evaluate the performance of CESM2.2 (CAM6-chem, MOSAIC coupled with MAM4) in simulating surface PM_{2.5} and MDA8 ozone concentrations using multiple observation-constrained global datasets and statistical metrics. Additionally, we briefly discuss previous evaluation literature of aerosols, PM_{2.5} and O₃ simulated by CESM2 (CAM6-chem with MOSAIC and MAM4) used in this study, thereby further supporting the reliability of our simulations.

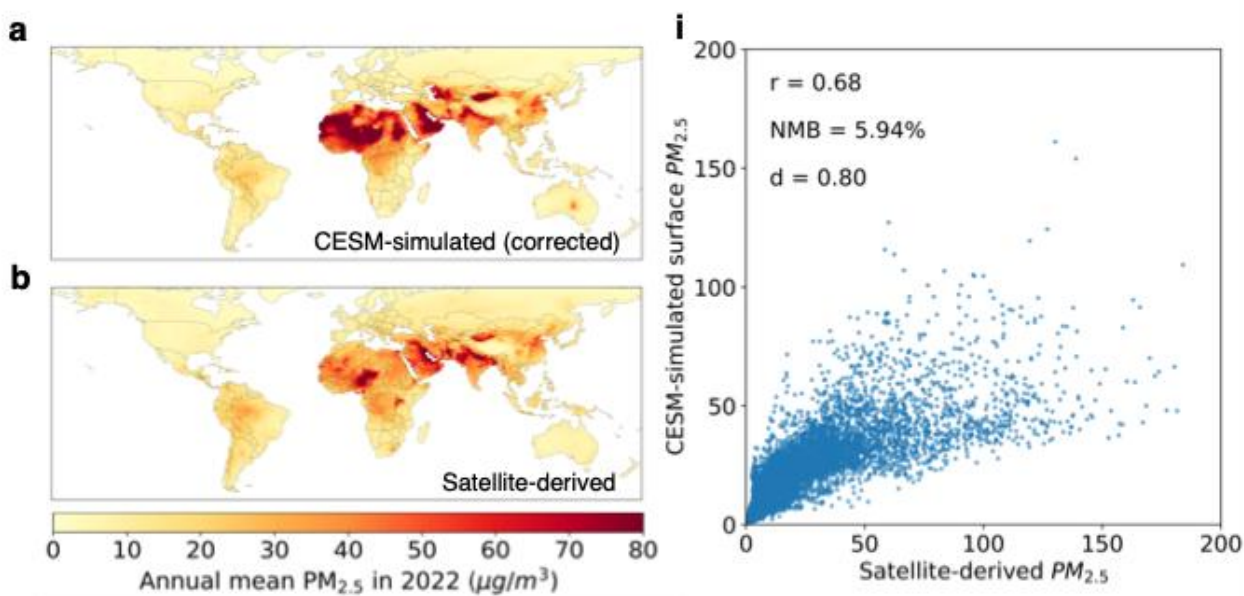
1.1 PM_{2.5} evaluation and bias correction

We first evaluate CESM-simulated PM_{2.5} against MERRA-2 reanalysis data^[1]. CESM2.2 performs well in simulating the spatial distribution and magnitude of global surface PM_{2.5}

(excluding dust and sea salt components) in 2022, with a Pearson correlation coefficient (r) of 0.90 and a normalized mean bias (NMB) of -4.5% (18,402 data pairs), meeting commonly used evaluation benchmarks ($r \geq 0.5$ and $|\text{NMB}| \leq 15\%$)^[2] (Supplementary Fig. 10a vs. 10b, and 10i). In contrast, larger discrepancies are observed for the dust and sea salt components (Supplementary Fig. 10c-f). The model exhibits relatively low skill for dust ($r = 0.52$ and $\text{NMB} = -4.5\%$) and substantial underestimation of sea salt ($r = 0.81$ and $\text{NMB} = -43.9\%$). These biases in natural aerosols are likely associated with uncertainties in emission parameterizations, biases in meteorological and land surface drivers, and the inability of coarse model resolution to resolve key small-scale processes^[3, 4].

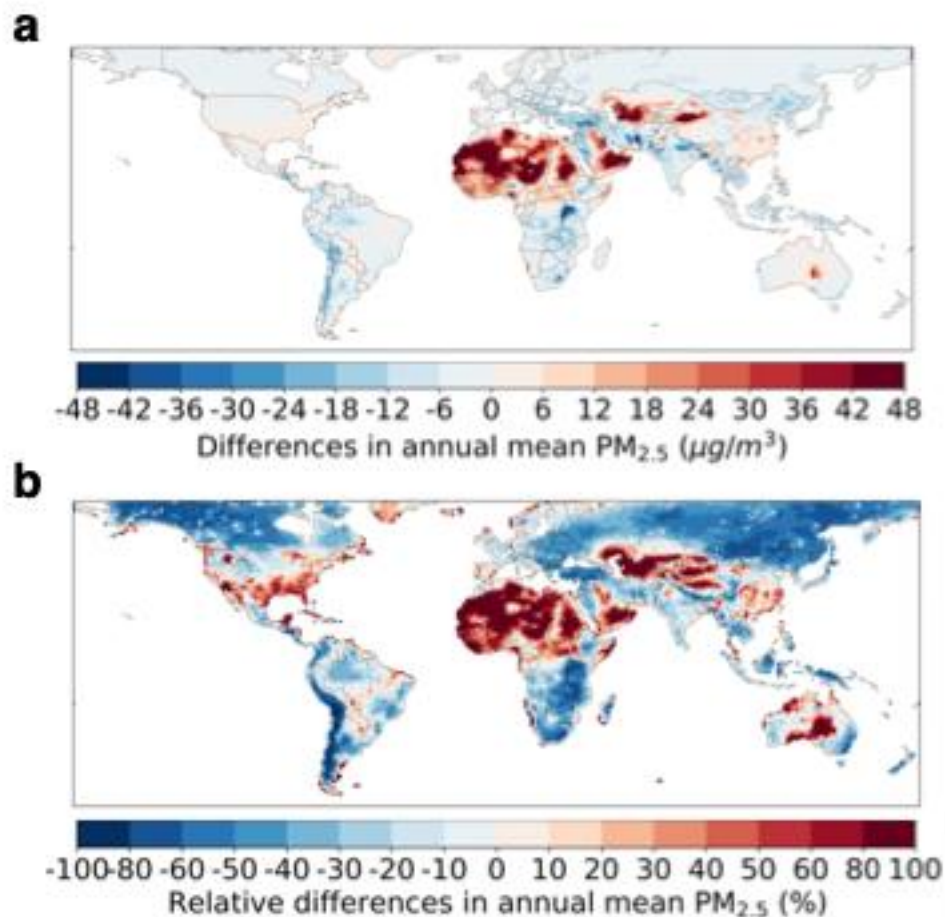
Because meteorological conditions in our simulations are nudged toward MERRA-2, changes in $\text{PM}_{2.5}$ concentration under shipping transition scenarios are primarily driven by anthropogenic emission changes, whereas dust and sea salt are expected to remain largely insensitive to the energy transition. While biases in natural aerosols have limited influence on $\text{PM}_{2.5}$ changes in this study, they can affect absolute levels that are important for health impact estimates given the nonlinear exposure–response relationship between $\text{PM}_{2.5}$ exposure and health outcomes^[5]. Therefore, we correct the CESM-simulated $\text{PM}_{2.5}$ concentrations in both baseline and transition scenarios by replacing the dust and sea salt components with those from MERRA-2 fields^[1] to better constrain absolute $\text{PM}_{2.5}$ levels.

The bias-corrected $\text{PM}_{2.5}$ concentration is compared against a widely used global ground-level $\text{PM}_{2.5}$ dataset derived through combining Aerosol Optical Depth retrievals, chemical transport model, and ground-based observations^[6]. The benefit of using such a gridded dataset is that it reduces representativeness mismatches between point measurements and coarse model grid cells, especially in regions with sparse monitoring networks. The bias-corrected $\text{PM}_{2.5}$ agrees reasonably well with satellite-derived $\text{PM}_{2.5}$ ^[6] over land, **with $r = 0.68$, $\text{NMB} = 5.9\%$ and d (Index of Agreement^[7]) = 0.8** (N=18,402), meeting recommended evaluation benchmarks ($r \geq 0.5$, $|\text{NMB}| \leq 15\%$ and $d > 0.7$)^[2, 8] (Supplementary Fig. 11).



Supplementary Fig. 11.1 Comparison between bias-corrected CESM-simulated surface PM_{2.5} with satellite-derived data. **a, b**, Spatial distribution of annual mean bias-corrected CESM-simulated surface PM_{2.5} concentration and satellite-derived surface PM_{2.5} (Shen et al., 2024) in 2022 ($\mu\text{g}/\text{m}^3$). **i**, Scatter plot comparing bias-corrected PM_{2.5} with satellite-derived PM_{2.5}. Each point represents a paired grid cell value over global land (N= 18,402). The Correlation coefficient (r), Normalized mean bias (NMB) and index of agreement (d) (Willmott & Wicks, 1980) are shown in the upper left corner of panel **i**.

Although regional biases remain, with overestimations over North Africa and the Arabian Peninsula and underestimations along the western coast of South America and the southeastern coast of Africa, the relative differences are generally within 30% across key regions of interest, including Europe, South Asia, East Asia, and Southeast Asia (Supplementary Fig. 11.2), where substantial changes in PM_{2.5} and attributable mortality are observed (Figs. 2 and 3). PM_{2.5} over North Africa is dominated by dust aerosols, for which bias correction has been applied based on MERRA-2 reanalysis data. Ren, Lin ^[9] showed the absolute differences between modeled multi-year mean PM_{2.5} over China (from multiple CMIP6 models) and satellite-derived PM_{2.5}. Compared with their evaluation results (Figure S2 in Ren, Lin ^[9]), CESM-simulated PM_{2.5} in our study demonstrates improved performance over China.



Supplementary Fig. 11.2 Differences between bias-corrected CESM-simulated surface PM_{2.5} and satellite-derived data. a, Absolute differences ($CESM - satellite$). d, Relative differences ($(CESM - satellite)/satellite \times 100$).

We note that many of previous global studies have relied on satellite-derived data, reanalysis datasets, or other modeling studies to evaluate simulated PM_{2.5} concentration^[10-13]. For instance, Banks, Kooperman and Xu^[10] used satellite-derived PM_{2.5} to evaluate surface PM_{2.5} simulations by CESM2 (CAM6 with MAM4), showing that both the spatial distribution and magnitude were well captured over land. In this study, by employing a comprehensive aerosol chemistry module (MOSAIC) coupled with MAM4, where the nitrate aerosol is included and ammonium aerosol is explicitly treated^[14], the simulation of surface PM_{2.5} is substantially improved compared with that in Banks, Kooperman and Xu^[10], particularly over East Asia and South Asia.

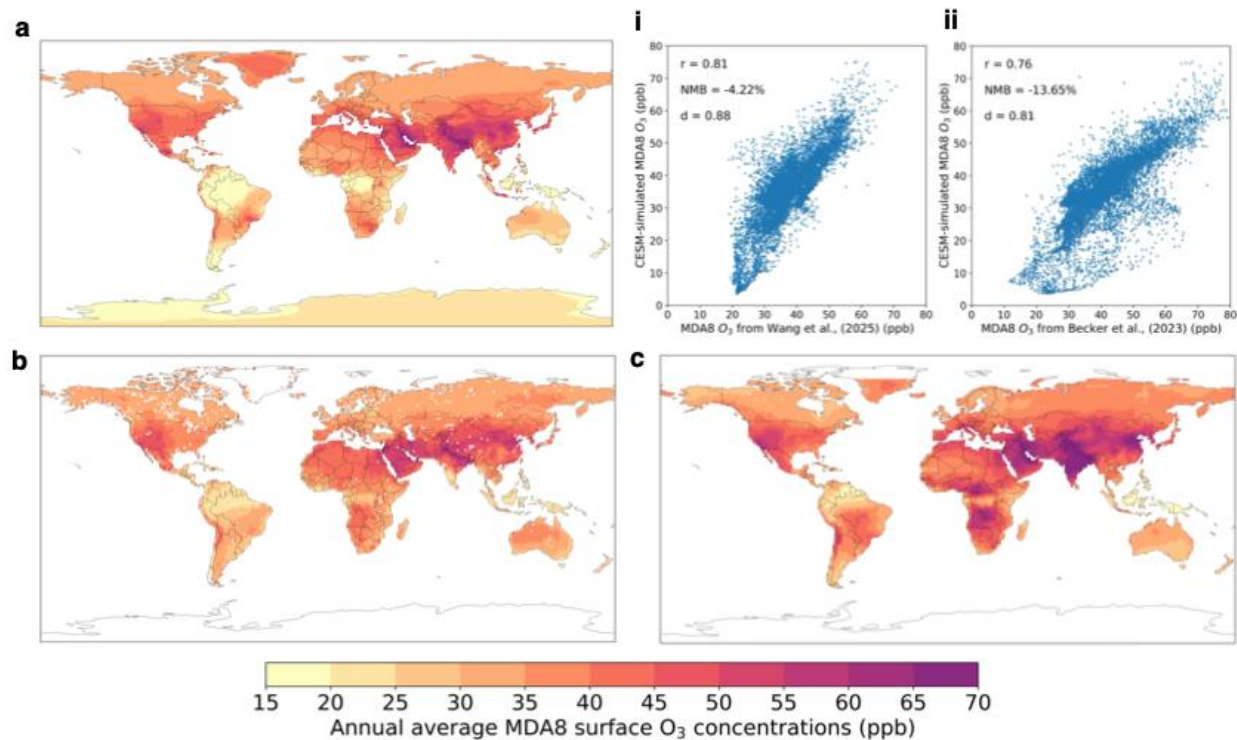
The simulation of major aerosol species (sulfate, BC, POM, SOA, nitrate and ammonium) in the CESM2 configuration used here (CAM6-chem with MOSAIC and MAM4) has been extensively evaluated in previous studies^[14-16], against ground-based observations, satellite observations and other chemistry model output from the Aerosol Model Intercomparison Initiative (AeroCom). Lu, Liu^[14] showed that this configuration performs reasonably well in simulating spatiotemporal

distributions of sulfate, nitrate and ammonium aerosols. Sulfate shows the best performance, with high correlations ($r = 0.91$ over the United States and 0.85 over East Asia) and relatively small biases (NMB = -10.2% and -5.7% , respectively), although it is substantially underestimated in China ($>50\%$), likely due to underestimated SO_2 emissions and incomplete representation of sulfate formation pathways. Nitrate is reasonably captured in China (NMB = 10.2%) with generally well-reproduced seasonality, but is overestimated in parts of East Asia and exhibits low correlations over the United States. Ammonium is also well reproduced, particularly over Europe and East Asia. In addition, Liu, Ma ^[16] evaluated black carbon simulated by the MAM4 scheme against in situ surface observations and showed that MAM4 improves the representation of near-surface BC concentrations, especially in polar regions.

1.2 Ozone evaluation

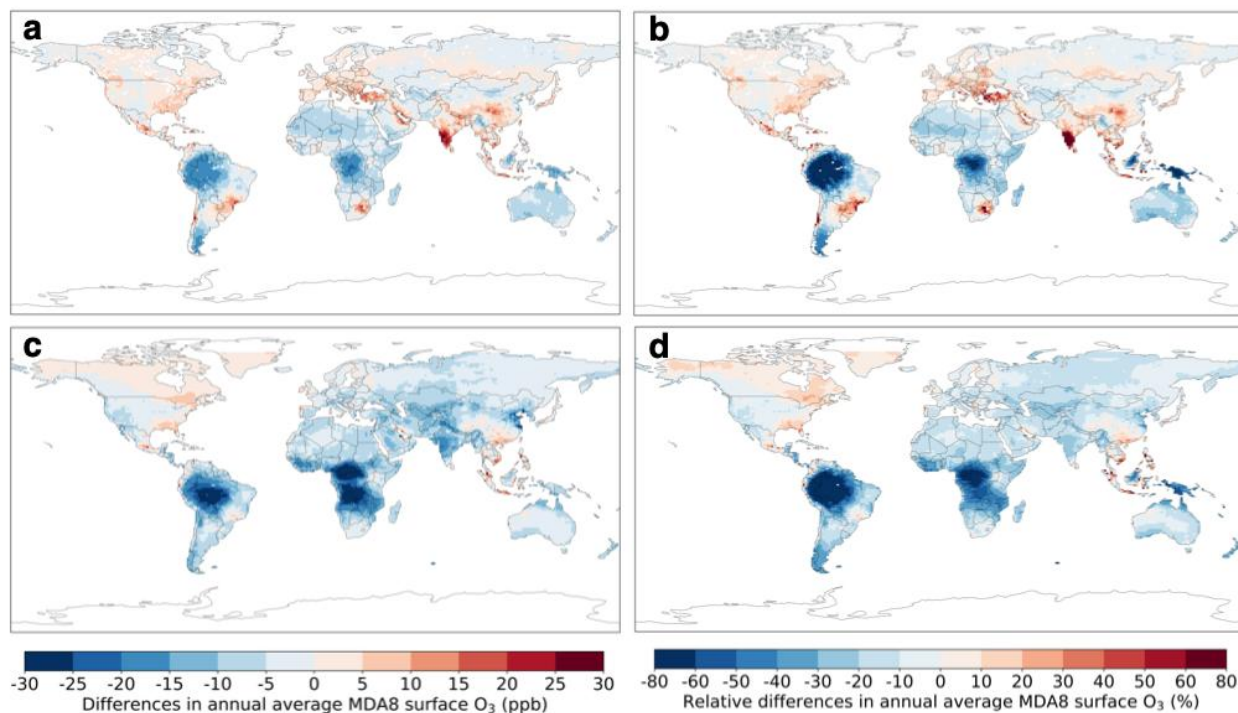
Surface MDA8 ozone under the baseline is evaluated using two independent global datasets. The first is a high-resolution (0.05°) global ozone product developed by Wang, Yang ^[17] using a geo-ensemble learning framework that integrates multi-source observations, including satellite retrievals of ozone precursors. This dataset has been extensively validated against in situ measurements, showing strong performance with a mean bias of less than 1.5 ppb. The dataset has also been widely used to assess population exposure and ozone-attributable mortality, making it particularly suitable as an observationally constrained benchmark for ozone concentration in this study. The second dataset is a global ozone product developed through data fusion of observations and atmospheric chemistry models^[18], which improves performance in regions far from monitoring stations.

Across global land, CESM2.2 reproduces the spatial distribution of surface MDA8 ozone with $r = 0.81$ (NMB = -4.2% , $d = 0.88$; $N = 11,798$) and $r = 0.76$ (NMB = -13.6% , $d = 0.81$; $N = 12,621$) relative to the two datasets, respectively (Supplementary Fig. 12.1), meeting the evaluation criteria.



Supplementary Fig. 12.1 Comparison between CESM-simulated MDA8 surface ozone with other studies. Global annual average of Maximum Daily Average 8 h (MDA8) surface ozone concentration **a** in the baseline experiment for 2022, from **b** Wang et al. (2025), and from **c** Becker et al. (2023). Scatter plots of **i** CESM-simulated MDA8 O₃ versus MDA8 O₃ from Wang et al. (2025), **ii** CESM-simulated MDA8 O₃ versus MDA8 O₃ from Becker et al. (2023). Each point represents a paired grid cell value over global land (N= 18,402). The correlation coefficient (r), the index of agreement (d) and normalized mean bias (NMB) are shown in the upper left corner of panels **i** and **ii**.

The spatial distribution of differences (Supplementary Fig. 12.2) shows that simulated ozone generally lies between the two datasets across major regions, including North America, Europe, South Asia, and East Asia, with relative differences mostly within 20%. However, systematic regional biases persist, including underestimation over northern South America and central Africa and overestimation in parts of Southeast Asia.



Supplementary Fig. 12.2 Differences between CESM-simulated MDA8 surface ozone and two independent global ozone datasets. a, c Absolute differences between CESM-simulated MDA8 surface ozone and that from Wang et al. (2025) and Becker et al. (2023), respectively. **b, d** The same with panels a and c, but for relative differences $((CESM - dataset)/dataset) \times 100$.

The chemical mechanism used in this study (MOZART-TS1) has been widely evaluated and used in previous studies^[19-23]. Emmons, Schwantes^[20] comprehensively evaluated surface ozone simulated by CESM2 (CAM6-chem) using the gridded surface ozone developed for the Tropospheric Ozone Assessment Report during 1990–2015, focusing on four regions: southeast United States, Europe, Japan, and Australia–New Zealand. Their results show that CESM2 reproduces the spatiotemporal variability of surface ozone well, with correlations ranging from 0.76 to 0.93 across these regions and relatively low biases in winter (less than 5 ppb). Nevertheless, biases are larger in summer, particularly in the southeastern United States and Japan (~13 ppb). Our evaluation results are consistent with these findings, with overestimation of ~5–10 ppb in the southeastern United States and Japan (Supplementary Fig. 12.2a). Additionally, Lin, Emmons^[19] compared April 2016 monthly mean surface ozone simulated by GEOS-Chem and CAM-chem within CESM2 to surface observations, showing that both models agree well with the observations well and differ from each other by only 5–10 ppb.

Overall, the model in this study reproduces the global spatial patterns and magnitudes of both surface PM_{2.5} and MDA8 ozone reasonably well, with acceptable biases and strong agreement with observation-constrained datasets, supporting its use in health impact assessment.