nature portfolio

Peer Review File

Short-term exposure to fine particulate pollution and elderly mortality in Chile

Corresponding Author: Professor Xiao Hui Tai

This manuscript has been previously reviewed at another journal. This document only contains reviewer comments, rebuttal and decision letters for versions considered at Communications Earth & Environment.

A version of this paper was originally rejected for publication by Communications Earth & Environment, however that decision was reconsidered after appeal by the authors.

The author's responses to these comments can be found, in order, at the end of this file.

Version 0:

Decision Letter:

Dear Professor Tai,

Please accept our sincere apologies for the delay in obtaining reports on your manuscript titled "Acute exposure to fine particulate pollution and elderly mortality in Chile". Your manuscript has now been seen by 3 reviewers, whose comments are appended below. In the light of their advice we regret to inform you that we cannot publish your manuscript in Communications Earth & Environment.

You will see that the reviewers raise seemingly important concerns regarding the robustness of the applied methods, for example the validation approach, and the novelty of the insights gained from your analysis. Taking these points together with our editorial considerations, we are unable to conclude that your manuscript represents a sufficiently novel and compelling advance in our understanding of the health effects of exposure to air pollution in Chile. Unfortunately, these reservations are sufficiently important to preclude publication of this study in Communications Earth & Environment.

We are sorry that we cannot be more positive on this occasion and thank you for the opportunity to consider your work.

Best regards,

Kerstin Schepanski, PhD
Editorial Board Member
Communications Earth & Environment
orcid.org/0000-0002-1027-6786

Clare Davis, PhD
Senior Editor
Communications Earth & Environment

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The reviewed article focuses on a topic relevant to environmental health and to the current sociodemographic context of numerous countries that increasingly have an aging population. The conclusions contribute to the consistency of the current body of evidence in relation to mortality due to short-term exposure to particulate matter in older people.
Some comments from now on.
In the summary, the main result of the study is not mentioned, but rather it describes the result for a specific region where the greatest effect was found, however, it has a general result that is important and that is not mentioned.
The introduction section is adequate in terms of substantiating the problem and objective of the study, however, it contains

quite a few methodological elements that should be added to the methods section. In this sense, a main revision suggestion is the organization of the sections of the article, since after the introduction (which contains some methodological elements) comes the results section, which is difficult to follow as it does not have more information about the results. methods. Then the methods section comes after the discussion.
The results section is quite extensive and focuses on showing the great variability across regions according to different variables such as the proportion of older people and rurality, among others. A first result that draws attention is the association found between PM concentration and mortality in the same month, which should be taken with caution since the outcome could have actually occurred before the greatest exposure to PM in the same month; this must include a plausible explanation or taken as a limitation of this study. The fact that the greatest risks have been found in the communes/regions with the highest percentage of elderly people may be due to the fact that there are precisely more people susceptible to death, although not only due to contamination (e.g. circulation of respiratory viruses). . Rural communes also have special conditions, a smaller population and lower exposures to both environmental pollution from PM (less traffic and industries) and viral circulation. The evaluation of adaptation requires a greater explanation in relation to the biological plausibility that could operate in a phenomenon like this, in addition, adding some of this in the introduction or methods, in relation to other studies that have investigated adaptation and their conclusions. principals and the way in which adaptation can be measured.
Figure 5, which contains subgroup analysis, review the column number of observations and their values. Indicate the rate amplifier (Monthly MR column). In this figure, does mortality from all causes excluding cardiorespiratory causes include mortality from external causes? If so, substantiate this fact. Same comments for Fig. S10.
The discussion of the results is appropriate, as a suggestion you could counter your results with research that has studied exposure on a monthly basis (as in the study) as well as with studies that have considered daily exposure.
The methods section provides information that would allow the research to be reproduced.
You should check the English edition of the article.

Reviewer #2 (Remarks to the Author):

Review for Acute exposure to fine particulate pollution and elderly mortality in Chile (Manuscript#: COMMSENV-23-1866-T)

The manuscript investigates the impact of fine particulate matter (PM2.5) on mortality among individuals aged 75 and above in Chile, using novel satellite measurements and integrating public records. My concern lies primarily with its innovativeness, as it lacks novel methodologies and presents foreseeable outcomes.

I disagree with the statement that"However, most existing research has been conducted in developed countries due to the availability of reliable public health statistics, comprehensive air pollution monitoring, and data on other variables that affect both air pollution and mortality." Many studies have actually focused on the short-term associations between fine particulate air pollution and cardiovascular and respiratory mortality in Chile. For instance, the following study presents findings that diverge from the existing literature:
Short-term associations between fine particulate air pollution and cardiovascular and respiratory mortality in 337 cities in Latin America https://www.sciencedirect.com/science/article/pii/S0048969724012129#s0005

Line 58: For When referring to "Air pollution levels are far higher than in other wealthy nations," which specific wealthy nations are being compared?

Line 103 Why is the age threshold set at 75 years and not 65 years? As far as I know, the World Health Organization (WHO) defines individuals aged 65 and above as elderly. An explanation for this choice of age threshold is warranted.
Line 702-703: Line 702-703: It would be beneficial to provide further details about the study area, including climate, elderly population demographics, economic factors, etc.
Line 718-748: There seems to be inconsistency in resolution. Please clarify how the data were matched.
Line 779: Why was a generalized linear model chosen instead of a generalized additive model?
Line 884 "where" should be properly formatted.
Line 273 Could you clarify what diseases are included in "all-cause mortality"?

Reviewer #3 (Remarks to the Author):

The authors conducted an panel analysis aiming to evaluate the short-term effect of ambient PM2.5 on mortality among elderly in Chile. The scale of exposure and outcome are monthly (temporal) and commune (spatial). The source of PM2.5 was the product from Atmospheric Composition Analysis Group from the Washington University that uses satellite data. The authors observed a positive association with mortality, across different scenarios and sensitivity analysis. The authors should be commended by the conducting such analysis in a country in Latin America, however this reviewer has some major concerns.

Major

1 - Please, temper all statements about being the first using satellite exposure in LA. Others have done that, including local adaptation and better temporal resolution.

10.1016/S2542-5196(23)00237-1
10.1016/j.scitotenv.2024.171073
10.1016/j.envpol.2023.122797
10.1016/j.chemosphere.2023.140259

2 - Other point to be better discussed is the temporal resolution. Unfortunately, nowadays, the state-of-art analysis for short-term effects, or "acute", as written in the title, uses daily, maximum weekly, temporal resolution. Daily resolution allows for the model to account for lag effects, which is of paramount importance in short term effects of PM2.5 and temperature. All this was lost when analysing monthly resolution. This is even worth in the scenario of Chile and the regions with severe winter and other phenomena with high peaks on PM2.5 as described in the literature.

3 - Please, discuss the effect observed on the cities with

4 - The use of land temperature instead of 2 metres ambient temperature (e.g., from ERA-5-land reanalysis) is main limitation that should be discussed and justified, since temperature is a main confounder in this association.

5 - This reviewer suggests to exclude the analysis on adaptation. First, the time series might not be large enough to evaluate adaptation, usually requiring more than 30 years. Second, the authors evaluated it in even short periods of time, which is very short to observe any potential change in mitigation at the population level, unless a massive intervention was done.

6 - Please, justify why the authors did not explore non-linearity in the PM2.5 association and why non-linear (ideally accounting for LAG as in DLNM) for temperature as main analysis.

7 - Validation. Only linear correlation is not enough to validate an exposure. Please, report also other metrics, such as RMSE.

8 - Please, clarify that the model adjusted by q1-2010 and q1-2011, and not only q1,q2,q3,q4, without accounting for year. This reviewer is quite concerned about the best adjustment for seasonality and linear trend. In Fig S9 it is clear it has an impact on the point-estimate and precision. Please, justify why the quarter-year was chosen as main analysis. This figure could go to the main paper.

9 - The authors used a population-weighted estimation. What was the impact of this approach instead of using only commune area weights? Even in this scenario when the unit of analysis is the commune per se. Please, discuss.

Minor
1 - When citing pneumonia as an example of not too short effect, that is not true. The effect of temperature and air pollution on incidence of pneumonia is short, from days to two weeks maximum.

2 - Please, revise the term "obscure" the effect of PM2.5. This is not scientific. What happens is that it decreases the contrast range of PM2.5, decreasing its variability.

Many thanks for the opportunity to read your work.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Tai,

Your manuscript titled "Short-term exposure to fine particulate pollution and elderly mortality in Chile" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. Specifically, please clarify the reference period for 25,000 avoided death (line 677) and discuss the relevance of temporally matching of data sets with regard to the robustness of the results obtained.At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

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Please outline your response to each request in the right hand column. Please upload the completed table with your manuscript files as a Related Manuscript file.

If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Kerstin Schepanski, PhD
Editorial Board Member
Communications Earth & Environment
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Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

On X(Twitter): @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Thank you very much for reviewing the comments and sending the second version of the article, which has been more concise and clear regarding the rationale and methods. The reviewers shared several comments, so it was very important that they be clarified or reanalyzed to give better coherence to the text. In general, the new version of the manuscript, in my opinion, is publishable since the research results are novel and of great interest to the community of researchers who study air pollution and health. On the other hand, the methodological detail provided allows the work carried out to be reproduced, which will allow other research groups to carry out similar studies in their countries/cities that contribute to the consistency of the results of this study.
I only have two concerns left:
- Line 677, “For the entire study period (2002-2019), the latter represents over 25,000 avoided deaths due to air pollution in Chile annually,” it seems to me that the word “annually” is unnecessary since the 25 thousand are for the entire period

considering the 18 years of study.
- As I mentioned before, the plausibility of the results using the concentrations in the same month may include a more epidemiological-biological reflection on this matter in the discussion. I think it is an important aspect to discuss beyond its mention.

Reviewer #2 (Remarks to the Author):

The author has solved my confusion and I have no further comments.

Reviewer #3 (Remarks to the Author):

The authors did a great job and answered very clearly all my comments. The manuscript has improved.

Thank you for aiming to keep science at high bar, usually not so common nowadays.

R1's comments are in black/Arial; our point-by-point responses are in blue/Times

Thank you very much for reviewing the comments and sending the second version of the article, which has been more concise and clear regarding the rationale and methods. The reviewers shared several comments, so it was very important that they be clarified or reanalyzed to give better coherence to the text. In general, the new version of the manuscript, in my opinion, is publishable since the research results are novel and of great interest to the community of researchers who study air pollution and health. On the other hand, the methodological detail provided allows the work carried out to be reproduced, which will allow other research groups to carry out similar studies in their countries/cities that contribute to the consistency of the results of this study.

> Thank you for your kind remarks!

I only have two concerns left:
- Line 677, "For the entire study period (2002-2019), the latter represents over 25,000 avoided deaths due to air pollution in Chile annually," it seems to me that the word "annually" is unnecessary since the 25 thousand are for the entire period considering the 18 years of study.

> Thank you for catching this mistake. You are right and we have removed the word "annually" from this sentence.

- As I mentioned before, the plausibility of the results using the concentrations in the same month may include a more epidemiological-biological reflection on this matter in the discussion. I think it is an important aspect to discuss beyond its mention

> Thank you for pointing out that we did not address your previous point sufficiently. In the second paragraph of the Discussion, we now elaborate on the biological pathway as you suggest, and describe the implications of the potential temporal mismatch in monthly data on this explanation for the mortality effects of PM2.5 exposure. The relevant section (~line 476) now reads:
>
> > A limitation of monthly aggregated data is that a fraction of deaths occurs before the peak PM 2.5 exposure in the same month. This temporal mismatch could limit the causal interpretation of our estimates and attribution of the effects to known biological pathways. In particular, the main known acute effects of PM 2.5 pollution are respiratory and cardiovascular illnesses leading to increased mortality. These same factors would naturally explain the increased monthly mortality that we observe, but this relies on the assumption that mortality increases after PM 2.5 exposure.

R2's comments are in black/Arial; our point-by-point responses are in blue/Times

The author has solved my confusion and I have no further comments.

Thanks again for your feedback on our manuscript!

R3's comments are in black/Arial; our point-by-point responses are in blue/Times

The authors did a great job and answered very clearly all my comments. The manuscript has improved.

Thank you for aiming to keep science at high bar, usually not so common nowadays.

Thank you for your kind words and for your feedback on our manuscript!

R1's comments are in black/Arial; our point-by-point responses are in blue/Times

Thank you for your thoughtful and constructive review, and for the generally enthusiastic assessment of our manuscript. We have provided a point-by-point response to each of your Suggested Improvements below.

You may also notice other changes, which were made in response to comments from the other reviewers. We think the result is a much-improved manuscript. Thanks again for taking the time to review our work!

Comment 1:
The reviewed article focuses on a topic relevant to environmental health and to the current sociodemographic context of numerous countries that increasingly have an aging population. The conclusions contribute to the consistency of the current body of evidence in relation to mortality due to short-term exposure to particulate matter in older people.
Some comments from now on.

In the summary, the main result of the study is not mentioned, but rather it describes the result for a specific region where the greatest effect was found, however, it has a general result that is important and that is not mentioned.

Thank you for pointing this out. We agree that it is important to highlight this result. We now include it in the abstract and also in the first paragraph of our Discussion, to highlight our contribution. The relevant text in the abstract reads:

> We find that a 10ug/m^3 monthly increase in PM2.5 exposure is associated with a 1.7% increase (95% C.I.: 1.1-2.4%) in all-cause mortality for individuals aged 75+. The effect size is remarkably similar across many dimensions, including baseline levels of exposure, rural and urban areas, income, and over time, demonstrating the consistency of the current body of evidence in relation to mortality due to short-term exposure to particulate matter in older people. The most notable source of heterogeneity is geographical, where the effect is closer to 5% in the center-south of Chile and in the metropolitan area.

The first paragraph of the Discussion now reads (line ~574):

> Our study uses satellite-based data to estimate population-weighted PM2.5 exposures in Chile with complete geographical coverage, and uses these to estimate the effect of PM2.5 exposure on elderly mortality. The inclusion of rural areas and the use of granular commune-level data allow us to study dimensions of heterogeneity with a level of detail that has not been explored in previous work. Our work contributes more broadly to the understanding of the relationship between fine particulate pollution and mortality in countries outside the developed world, and to the consistency of the current body of evidence in relation to

> mortality due to short-term exposure to particulate matter in older people. The approach we develop is applicable to other countries and contexts.

Comment 2:
The introduction section is adequate in terms of substantiating the problem and objective of the study, however, it contains quite a few methodological elements that should be added to the methods section. In this sense, a main revision suggestion is the organization of the sections of the article, since after the introduction (which contains some methodological elements) comes the results section, which is difficult to follow as it does not have more information about the results. methods. Then the methods section comes after the discussion.

Thank you for this suggestion -- unfortunately we are unable to make this change due to the journal's formatting requirements.

Comment 3:
The results section is quite extensive and focuses on showing the great variability across regions according to different variables such as the proportion of older people and rurality, among others. A first result that draws attention is the association found between PM concentration and mortality in the same month, which should be taken with caution since the outcome could have actually occurred before the greatest exposure to PM in the same month; this must include a plausible explanation or taken as a limitation of this study.

Thank you for this suggestion. We have expanded the discussion on the differences between monthly and daily data in the Discussion, and included the point that you raised as a limitation. Please see our response to Comment 8 (below) for full details.

Comment 4:
The fact that the greatest risks have been found in the communes/regions with the highest percentage of elderly people may be due to the fact that there are precisely more people susceptible to death, although not only due to contamination (e.g. circulation of respiratory viruses).

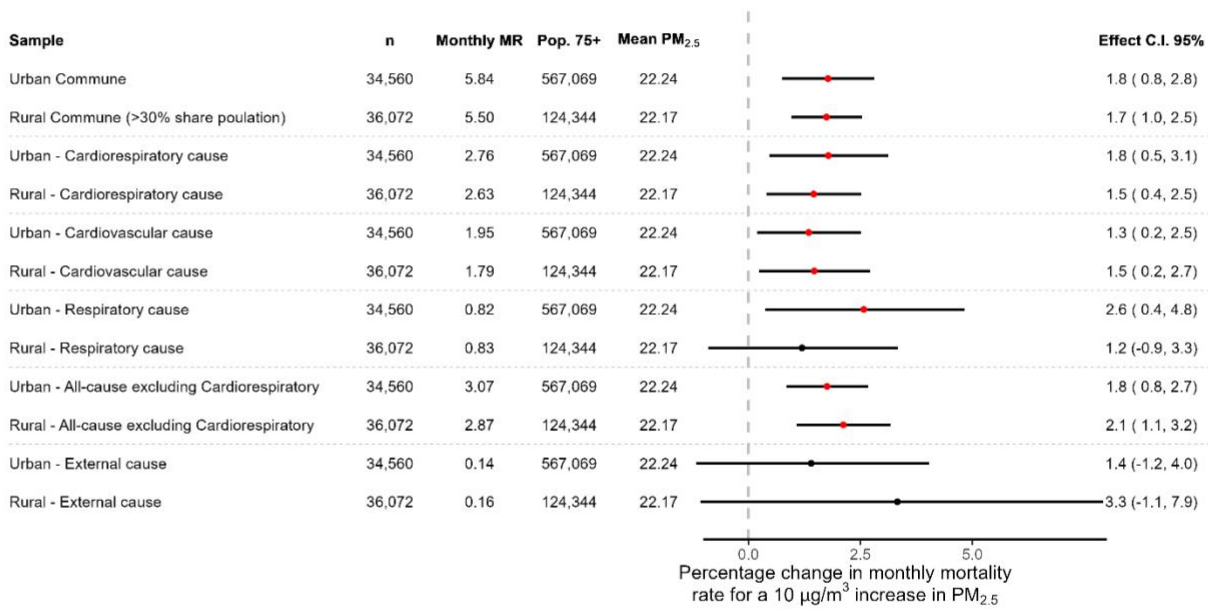
Thank you for this suggestion. We have expanded on the discussion of this result in our initial manuscript, to include the points you raised. The relevant text (in Results, line ~386) now reads:

> The larger effect in communes with higher elderly population shares may be due to limited capacity of healthcare systems, increased circulation of respiratory viruses, or different age structures within the 75+ group (Xu et al., 2023). Additionally, people in communes with lower share of elderly population might be less susceptible to death, due to a potential survivorship bias, where individuals that are most ``resilient" to air pollution constitutes the majority that have survived to age 75 (Dennekamp et al., 2010; Cogen et al., 2023).

Comment 5:
Rural communes also have special conditions, a smaller population and lower exposures to both environmental pollution from PM (less traffic and industries) and viral circulation.

Thank you for this comment. While we did not find major differences between the effect in rural vs urban communes in our initial analysis, we extended the analysis to include detailed mortality causes in our revised manuscript. We found differences in the effect of PM2.5 pollution on respiratory mortality causes, with urban communes seeing larger effects (see figure below, included as SI Figure S5 in the revised manuscript). These results are consistent with the explanations that you suggested, and we have included an explanation in the Discussion (line ~636).

> Differences between urban and rural areas has been understudied in the literature, with one study finding a similar result as we do in China, where PM2.5 exposure had larger effects on respiratory causes of death in urban areas (Liu et al., 2021).
>
> This result may be due to differences in the source and composition of PM2.5; in the case of Chile, in rural areas, pollution comes mainly from natural sources and wood burning (Jorquera et al., 2021), while in cities, it is due to traffic and industrial activity (Kavouras et al., 2001). Another potential explanation might be more circulation of respiratory viruses in areas with higher population density.



> **SI Figure S5:** Effect of PM2.5 for the urban and rural sample for various mortality causes for the 75+ age group. Each pair of rows shows the comparison of models fitted for urban and rural communes, for different mortality causes, with information on the number of commune-month observations (n), average monthly mortality rate (monthly MR, deaths per 1,000 inhabitants), monthly 75+ population, mean PM2.5 (ug/m^3, population-weighted), and the overall effect (black and red dots; red means statistically significant) with bars representing

> 95% CIs. External cause refers to all deaths not attributed to diseases, such as accidents or suicides. A commune is classified as rural if 30% or more inhabitants living in rural areas. Effects for urban and rural communes and very similar, with largest differences for respiratory causes.

Comment 6:
The evaluation of adaptation requires a greater explanation in relation to the biological plausibility that could operate in a phenomenon like this, in addition, adding some of this in the introduction or methods, in relation to other studies that have investigated adaptation and their conclusions. principals and the way in which adaptation can be measured.

Thank you for this suggestion. We agree with you that our explanation on adaptation in our initial manuscript could be improved. Our analysis spans an 18-year period and might not be sufficient to make definitive conclusions about adaptations, which require a longer timescale, such as 30 years or longer. Previous studies on air pollution in Chile have not studied adaptation, since the longest time period studied has been 10 years (Pino-Cortes et al., 2020). In our revised manuscript, we instead frame our findings as an investigation of changes over time, that could hint at potential adaptations, and state clearly that a full analysis would require more years of data than we have available.

We include an explanation in the Introduction (line ~198), that reads:

> Additionally, by using a longer time period (18 years, compared to up to 10 years in studies covering the same geographical region (Pino-Cortes et al., 2020; Busch et al., 2023; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007; Gouveia et al., 2024)), we investigate changes in effects over time, hinting at potential adaptations to air pollution.

We changed the title of the subsection in Results from "Adaptation" to "Changes over time and income", to avoid an explicit reference to adaptation. In this section (line ~400), the relevant text reads:

> With documented harms to both population health and the economy due to climate change, a critical question is to what extent populations have adapted to these changes (Carleton & Hsiang, 2016). Fully investigating this question in the context of fine particulate pollution would require data on a decadal scale over multiple decades. Our data span close to two decades, and we estimate effects over 4-5 year periods in our panel (2002-2019). While this does not cover a sufficiently long time period to provide definitive evidence on adaptation, it is significantly longer than other recent work in Chile that cover up to a 10-year period (Pino-Cortes et al., 2020), and provides a first approximation to potential adaptations over time. We do not find convincing evidence of changes over time; coefficient estimates do not display an obvious trend (Fig. 4A; SI Fig. S7 displays estimated effects for single years). This could be due to two opposing effects: an expected adaptation over time which should decrease the effect size over time,

> and an aging population (see SI Fig. S8) which might increase the effect size (Fig. 3E).

Comment 7:
Figure 5, which contains subgroup analysis, review the column number of observations and their values. Indicate the rate amplifier (Monthly MR column). In this figure, does mortality from all causes excluding cardiorespiratory causes include mortality from external causes? If so, substantiate this fact. Same comments for Fig. S10.

Thank you for this comment. For the rate amplifier, we believe you are referring to the 75+ population for each subsample (denominator of the mortality rate). We have added this as a separate column in the figure. We reviewed the column for the number of observations – note that this is the number of commune-month observations in our panel data, and is distinct from the 75+ population. We apologize for the confusion. External cause in this figure refers to all deaths not attributed to diseases, such as accidents or suicides. All of this information is now stated clearly in the figure caption. We applied the same changes to Fig. S10 (Fig. S11 in the revised version), and the new Fig. S5 (shown in our response to Comment 5 above).

Regarding external causes, we explain more clearly in both the manuscript and figure that all mortality causes studies include external causes. We also added a sentence discussing this finding. The relevant text in the Results (line ~563) reads:

> In addition, there is a positive effect for mortality excluding cardio-respiratory causes, indicating other pathways in which PM2.5 affects mortality, such as other diseases or indirect effects like reduced healthcare capacity.

Comment 8:
The discussion of the results is appropriate, as a suggestion you could counter your results with research that has studied exposure on a monthly basis (as in the study) as well as with studies that have considered daily exposure.

This is a good point. We have expanded the discussion on monthly effects vs. daily effects. The relevant text in the Discussion reads (line ~594):

> The estimated effects should be interpreted as the short-term mortality induced by monthly deviations in PM2.5 concentrations. We do not capture the chronic effect of exposure to PM2.5 pollution, which has been established in multiple other studies (Lepeule et al., 2012; Krewski et al., 2009; Pope et al., 2020; Chen & Hoek, 2020), even for Chile (Busch et al., 2023), with generally much larger effect sizes (in the range of 2-25% (Pope et al., 2020)). Daily, acute effects have also been documented in other studies, with similar effect sizes (0.8-4.2%), despite important differences in methodologies and contexts (Dominici et al., 2006; Liu et al., 2021; Pino-Cortes et al., 2020; Sanhueza et al., 2009; Cakmak et al., 2007; Cifuentes et al., 2000; Kloog et al., 2013; Orellano et al., 2020).We

provide a slightly different perspective that is focused on monthly exposures, which do not capture immediate daily responses to increases in PM2.5. Monthly effects capture the effect of sustained weeks-long increases in PM2.5, such as those produced by wildfires, as well as the effect of illnesses with a longer window between onset and death. Such monthly effects are less well-studied and are not subject to regulatory standards, but could become increasingly important due to more frequent moderate exposure events such as wildfires and droughts (Rau et al., 2022). Consistent with findings in the US (Rau et al., 2022). and in Latin American cities (Gouveia et al., 2024), we find that the magnitude of monthly effects is similar to or exceeds that for daily acute effects, but is smaller than chronic effects. A limitation of monthly data is that mortality could potentially have occurred before the peak PM2.5 exposure in the same month.

Comment 9:
The methods section provides information that would allow the research to be reproduced.

You should check the English edition of the article.

I enjoyed reading this paper. There are strong motivations for this work, citing gaps in existing literature. The methodology and descriptions of the work are strong.

We thank the reviewer for the kind remarks. We have checked and edited the writing, and are not aware of any further issues. We would be happy to address any specific concerns you may have.

R2's comments are in black/Arial; our point-by-point responses are in blue/Times

Thank you very much for your constructive feedback. We have provided a point-by-point response to each of your Suggested Improvements below.

You may also notice other changes, which were made in response to comments from the other reviewers. We think the result is a much-improved manuscript. Thanks again for taking the time to review our work!

Comment 1:

The manuscript investigates the impact of fine particulate matter (PM2.5) on mortality among individuals aged 75 and above in Chile, using novel satellite measurements and integrating public records. My concern lies primarily with its innovativeness, as it lacks novel methodologies and presents foreseeable outcomes.

I disagree with the statement that "However, most existing research has been conducted in developed countries due to the availability of reliable public health statistics, comprehensive air pollution monitoring, and data on other variables that affect both air pollution and mortality." Many studies have actually focused on the short-term associations between fine particulate air pollution and cardiovascular and respiratory mortality in Chile. For instance, the following study presents findings that diverge from the existing literature:
Short-term associations between fine particulate air pollution and cardiovascular and respiratory mortality in 337 cities in Latin America
https://www.sciencedirect.com/science/article/pii/S0048969724012129#s0005

Thank you for pointing out this important related paper and raising the important issue about highlighting our contributions. While we agree that our lack of mention of the Gouveia et al. that you pointed out appears to be a major omission, this article was in fact published in February 2024, after our initial submission in November 2023. As a result, we could not highlight our contributions with respect to this article in our initial manuscript. We also agree with you that there are other studies that have focused on short-term associations between fine particulate pollution and mortality in Chile. While we cited those studies in our initial manuscript, we apologize for the lack of clarity in our explanation of the differences between these prior works and our paper.

While previous papers focus their analyses on cities (in particular the capital, Santiago), we analyze the whole country, covering both urban and rural areas. Previous papers cover only a fraction of the country (Pino-Cortes et al., 2020; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007). More recent work in Latin America that uses satellite-based estimates similarly only focuses on cities (Gouveia et al., 2024). In general, evidence from rural areas is scarce worldwide (Liu et al., 2021; Kim et al., 2020; Arku et al., 2020; Di et al., 2017) and, to the best of our knowledge, nonexistent in Latin America.

Using granular data with complete spatial coverage allows us to investigate heterogeneity in the estimated effect over multiple dimensions, including region, urbanicity, baseline

PM2.5 levels, population shares of 75+ individuals, and income. These dimensions of heterogeneity have not been fully addressed in previous work, which most commonly focuses on differences due to age and sex. While other studies consider cities in their entirety (e.g., Santiago, with 52 different communes), our commune-level data allow us to characterize heterogeneities within cities where they exist.

Our main substantive contribution in this analysis, which we did not highlight prominently enough (as brought to our attention by another reviewer), is that we find that the effect sizes across many of these dimensions are remarkably similar, demonstrating the consistency of the current body of evidence in relation to mortality due to short-term exposure to particulate matter in older people.

Since the coverage of rural areas is a key distinction between our work and previous work, in our revised manuscript, we added an extended analysis of the differences between rural and urban communes for additional mortality causes. In our previously reported results, we do not find a difference in effect sizes on all-cause mortality (all registered deaths) for rural and urban areas. In our extended analysis, we do not find differences for cardiorespiratory or cardiovascular causes either. The largest differences are for respiratory causes, with larger effects in urban areas (see figure below). We include these results in the revised manuscript (Appendix Fig. S5).

We have incorporated all the above changes to our revised manuscript. In the abstract, we highlight our contribution to the literature on heterogeneous effects:

> We find that a 10ug/m^3 monthly increase in PM2.5 exposure is associated with a 1.7% increase (95% C.I.: 1.1-2.4%) in all-cause mortality for individuals aged 75+. The effect size is remarkably similar across many dimensions, including baseline levels of exposure, rural and urban areas, income, and over time, demonstrating the consistency of the current body of evidence in relation to mortality due to short-term exposure to particulate matter in older people. The most notable source of heterogeneity is geographical, where the effect is closer to 5% in the center-south of Chile and in the metropolitan area.

In the Introduction, we changed the statement you had concerns with, to: "Historically, most research has been conducted in developed countries due to the availability of reliable public health statistics, comprehensive air pollution monitoring, and data on other variables that affect both air pollution and mortality."

We also include a more comprehensive discussion of the literature (line ~82), including Gouveia et al. 2024 and another paper published earlier this year using satellite-based estimates of pollutants in Brazil.

> The recent emergence of new data measurement techniques, in particular remote sensing (Van Donkelaar et al., 2015), opens the possibility of measuring air pollution exposure with complete spatial coverage and over a much longer time scale. Such measurements have been used to monitor air pollution globally and

> over time (Rentschler & Leonova, 2023; Li et al., 2023), and to study its impact on mortality outside the developed world. Recent work has studied the empirical relationship between satellite-based PM2.5 exposures and mortality in Africa (Heft-Neal et al., 2018; Heft-Neal et al., 2020), Indonesia (Siregar et al., 2022), Brazil (Requia et al. 2024) and in cities in Latin America (Gouveia et al., 2024). For Chile, earlier studies on the short-term association between PM2.5 and health-related outcomes have used data from ground monitoring stations and focused on the capital, Santiago, and other cities, covering only a fraction of the country and population (Pino-Cortes et al., 2020; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007). More recent work that uses satellite-based estimates similarly only focuses on cities (Gouveia et al., 2024). In general, evidence from rural areas is scarce (Liu et al., 2021; Kim et al., 2020; Arku et al., 2020; Di et al., 2017), and in particular in Latin America.

In the final paragraph of the Introduction (line ~185), we also make our contributions clearer:

> Next, we demonstrate the additional insight that can be derived from data with complete and granular spatial coverage by investigating heterogeneity in the estimated effect over multiple dimensions, including region, urbanicity, baseline PM2.5 levels, population shares of 75+ individuals, and income. These dimensions of heterogeneity have not been fully addressed in previous work, which most commonly focuses on differences due to age and sex. Studying the entire country provides more variation in these characteristics compared to that in limited geographical areas such as cities; in addition, our commune-level data allow us to characterize heterogeneities within cities where they exist. Additionally, by using a longer time period (19 years, compared to up to 10 years in studies covering the same geographical region (Pino-Cortes et al., 2020; Busch et al., 2023; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007; Gouveia et al., 2024)) we investigate changes in effects over time, hinting at potential adaptations to air pollution.

The extended rural vs. urban analysis is mentioned in the Results section, included as Appendix Fig. S5, and discussed in the Discussion (line ~636):

> Differences between urban and rural areas has been understudied in the literature, with one study finding a similar result as we do in China, where PM2.5 exposure had larger effects on respiratory causes of death in urban areas (Liu et al., 2021). This result may be due to differences in the source and composition of PM2.5; in the case of Chile, in rural areas, pollution comes mainly from natural sources and wood burning (Jorquera et al., 2021), while in cities, it is due to traffic and industrial activity (Kavouras et al., 2001). Another potential explanation might be more circulation of respiratory viruses in areas with higher population density.

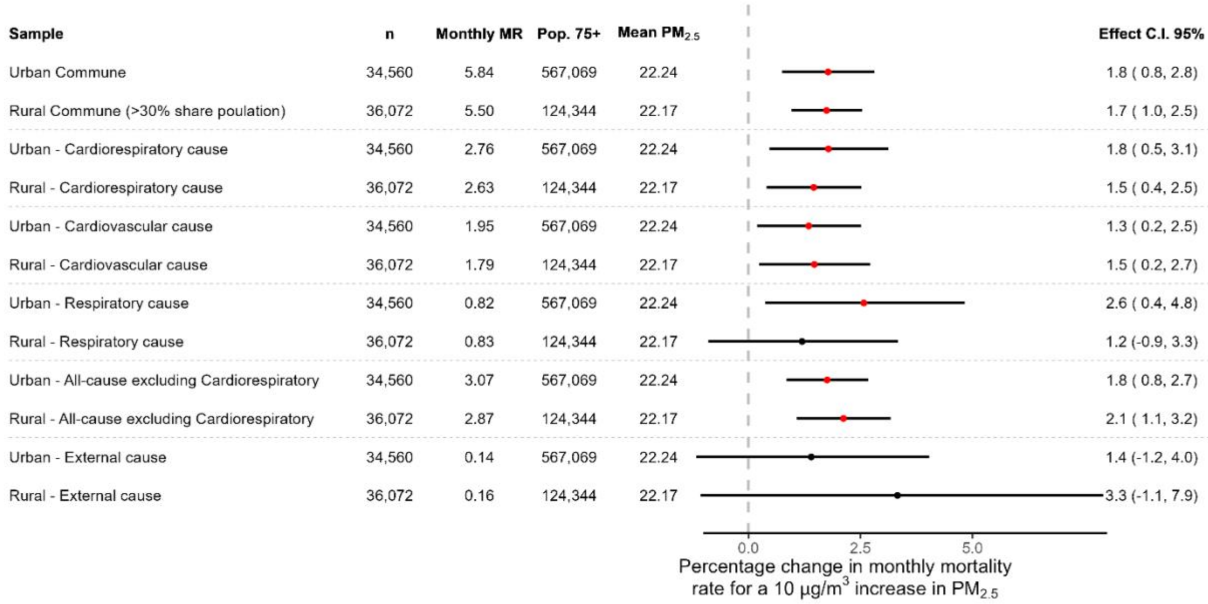


Fig. S5: Effect of PM2.5 for urban and rural communes for different mortality causes for the 75+ age group. Each pair of rows shows models fitted for urban and rural communes, for different mortality causes, with information on the number of commune-month observations (n), average monthly mortality rate (monthly MR, deaths per 1,000 inhabitants), monthly 75+ population, mean PM2.5 (ug/m^3, population-weighted), and the overall effect (black and red dots; red means statistically significant) with bars representing 95% CIs. External cause refers to all deaths not attributed to diseases, such as accidents or suicides. A commune is classified as rural if 30% or more inhabitants live in rural areas. Effects for urban and rural communes and very similar, with largest differences for respiratory causes.

Comment 2:
Line 58: For When referring to "Air pollution levels are far higher than in other wealthy nations," which specific wealthy nations are being compared?

We are comparing to the United States and European countries. We have clarified this in the text, and included a reference, Van Donkelaar et al., 2021, which has a figure (Figure 1 of their paper) which clearly illustrates the difference. The relevant text in the manuscript now reads (line ~60):

> Air pollution levels are far higher than in other wealthy nations, such as the United States or European countries (Van Donkelaar et al., 2021)…

Comment 3:
Line 103 Why is the age threshold set at 75 years and not 65 years? As far as I know, the World Health Organization (WHO) defines individuals aged 65 and above as elderly. An explanation for this choice of age threshold is warranted.

Thank you for this suggestion. We decided to focus on the 75+ age group as this age group is the most susceptible to PM2.5 pollution, according to various references (Fan et al., 2020; WHO, 2023; Yang et al., 2020) that we now cite clearly in the text (line ~106). Due to global populations aging rapidly, this group will likely continue to be disproportionally affected by air pollution (Rentschler & Leonova, 2023; Li et al., 2023; Longev, 2021).

In line with your suggestion, we recognize that the 65+ age group is also a susceptible group. Our manuscript contains a version of our main analysis for the 65+ group, in Figure S11 in SI. We find that the results and conclusions of our study apply for the 65+ group as well, with almost identical results to the 75+ group.

Comment 4:
Line 702-703: Line 702-703: It would be beneficial to provide further details about the study area, including climate, elderly population demographics, economic factors, etc.

Thank you for this suggestion. We have added details on the climate, population, elderly population (75+) and GDP per capita of Chile. The relevant text now reads (line ~772):

> The study was conducted in the continental area of Chile, a Latin American country in the southern hemisphere, located between the -17° and -56° latitude and -65° and -76° longitude. Chile has multiple climatic regions due to latitude differences and oceanic and mountains boundaries: tropical in the north, Mediterranean in the center and Antarctic in the south. Chile has a population of 17.5 million inhabitants in 2017, with 0.8 million of them in the 75+ age group, and a GDP per capita of 15 thousand USD in 2022. The geographical aggregation used for the analysis was the commune level, the third order (smallest) administrative unit in Chile. There are a total of 346 communes (third level administrative division) in 56 provinces (second level administrative division) in 16 regions (first level administrative region) in Chile.

Comment 5:
Line 718-748: There seems to be inconsistency in resolution. Please clarify how the data were matched.

Thank you for this observation. While the grids from the PM2.5 air pollution and land temperature data are slightly different in terms of resolution (1.11 km vs 1km), we did not match the individual grid cells as we are using them as inputs to calculate exposure at a commune level. In the section "Estimating population-weighted exposures", we explain how to match the grid level PM2.5 or land temperature estimates with detailed district census polygons containing population data, which allows us to estimate the population-weighted exposure for air pollution and land temperature at each commune.

Comment 6:

Line 779: Why was a generalized linear model chosen instead of a generalized additive model?

In our initial manuscript, we presented results for a linear term for PM2.5 and quadratic and spline terms for temperature (Figure S9; Figure S10 in the revised manuscript). These models produced very similar effect sizes for the PM2.5 exposure variable and so we chose the latter for simplicity. For the PM2.5 term, the use of a linear form facilitates comparison with other work (e.g., Liu et al., 2021; Kloog et al., 2013; Orellano et al., 2020; Rau et al. 2023). This is a similar strategy as Gouveia et al. 2024, which uses a linear term for PM2.5, and spline terms for temperature.

We have extended this analysis to explore non-linearity in PM2.5 exposures. In the figure below, we consider quadratic terms and cubic splines with 1, 3 and 4 knots. We find that these more flexible models result in roughly the same shaped response function as simple linear models, particularly in regions where majority of the data lie (16% of our data have PM2.5 exposures below 10 ug/m^3). This is a similar finding to Heft-Neal et al. 2018, which explores polynomial and spline terms for PM2.5 (with a linear term for temperature), finding that the shape of the response function is similar. They similarly opt for the linear version.

We now include this figure in the Appendix (Figure S12). In the main text we mention this robustness check in Results, under “Other sensitivity analyses.” The relevant text reads (line ~553):

> Finally, we considered more flexible functional forms for the PM2.5 variable (SI Fig. S12), finding roughly the same shaped response function as simple linear models, particularly in regions where majority of the data lie.

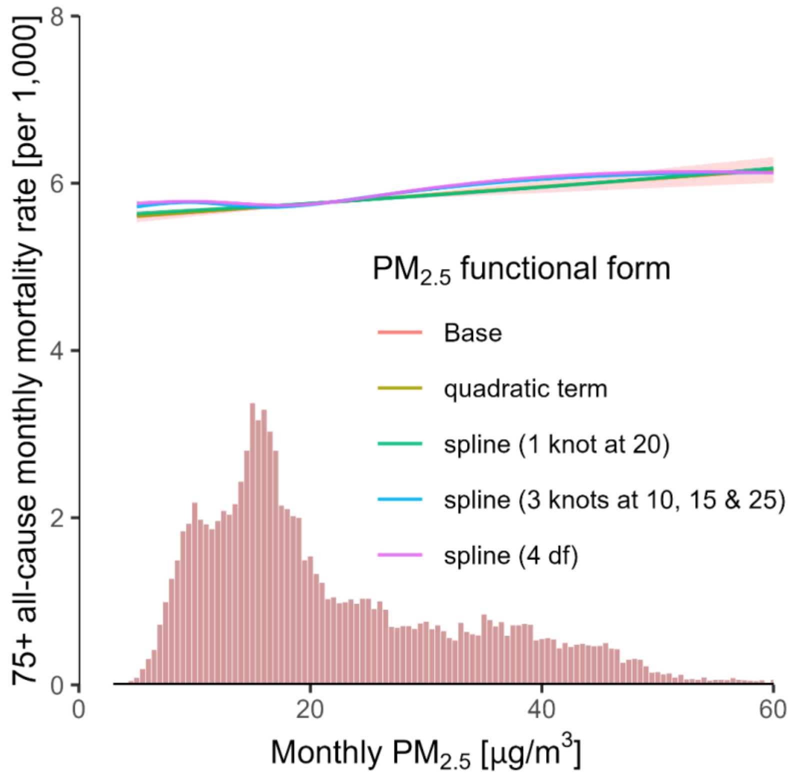


Fig. S12: Response function using more flexible functional forms for PM2.5. We consider quadratic and spline terms for PM2.5 exposure, instead of the linear (Base) model that was presented in the main results. These more flexible models result in roughly the same shaped response function as simple linear models, particularly in regions where majority of the data lie (84% of the data have PM2.5 exposures exceeding 10 ug/m^3).

Comment 7:
Line 884 "where" should be properly formatted.

Thanks, we have fixed this.

Comment 8:
Line 273 Could you clarify what diseases are included in "all-cause mortality"?

All-cause encompasses all registered deaths, including deaths from any disease, as well as external causes, which are deaths not attributed to diseases, such as accidents or suicides. In old Line 273 when talking about mortality rates, we included a footnote: "Mortality here includes all causes, which are deaths from any disease as well as external causes; see *Materials and Methods*." In the caption for Figure 2, we have also added "All-cause encompasses all deaths" to clarify this point.

Response to R3 (Communications Earth & Environment # COMMSENV-23-1866A-Z)

R3's comments are in black/Arial; our point-by-point responses are in blue/Times

Thank you for your extremely thoughtful and constructive review. We have provided a point-by-point response to each of your Suggested Improvements below.

You may also notice other changes, which were made in response to comments from the other reviewers. We think the result is a much-improved manuscript. Thanks again for taking the time to review our work!

The authors conducted an panel analysis aiming to evaluate the short-term effect of ambient PM2.5 on mortality among elderly in Chile. The scale of exposure and outcome are monthly (temporal) and commune (spatial). The source of PM2.5 was the product from Atmospheric Composition Analysis Group from the Washington University that uses satellite data. The authors observed a positive association with mortality, across different scenarios and sensitivity analysis. The authors should be commended by the conducting such analysis in a country in Latin America, however this reviewer has some major concerns.

Major

Comment 1:
1 - Please, temper all statements about being the first using satellite exposure in LA. Others have done that, including local adaptation and better temporal resolution.

10.1016/S2542-5196(23)00237-1
10.1016/j.scitotenv.2024.171073
10.1016/j.envpol.2023.122797
10.1016/j.chemosphere.2023.140259

Thank you for this helpful suggestion and for bringing these papers to our attention. We apologize for our lack of discussion of these papers in our initial manuscript --- two of them were published within a month or two before our initial submission, and the remaining two papers after our initial submission. We agree that these are important papers. We have made substantial changes to the Introduction and Discussion section of our manuscript, removing statements about being the first to use satellite-based exposure estimates in Latin America, as well as highlighting our contributions with respect to these articles. To briefly summarize each of the four papers you raise:

- Gouveia et al. 2024. Short-term associations between fine particulate air pollution and cardiovascular and respiratory mortality in 337 cities in Latin America.

This paper studies the effect of $PM_{2.5}$ on mortality for 337 cities in Latin America, for a 7-year period (2009-2015). They use the same satellite-based $PM_{2.5}$ data as we did, derived from the Washington university group (with 1.11km resolution). Gouveia et al. includes 20 cities in Chile and this overlap in scope understandably raises concerns about novelty. However, an important distinction is that this study focuses exclusively on cities, while a key contribution of our work is the complete geographical coverage of 327 communes (from 2002-2019), including rural communes in Chile. The cities in Gouveia et al. correspond to only 63 of the 327 communes we study. In general, evidence from rural areas is scarce (e.g., Liu et al. 2021), and it is not obvious a priori if larger effects should be observed in rural or urban areas.

Another feature of our analysis is that it investigates the impact of socio-economic status and other sources of heterogeneity, while Gouveia et al. focuses only on age and sex-specific effects. Studying the entire country provides far more variation in these characteristics compared to that in limited geographical areas such as cities; in addition, our commune-level data allow us to characterize heterogeneities within cities where they exist. For instance, Santiago, a city that Gouveia et al. considers as one unit in its entirety, contains 52 different communes. Among these communes there is substantial heterogeneity, many of them have higher income levels than the rest of the country, and some of them are in fact rural (more than 30% of people living in rural areas).

- Kephart et al. 2023. Ambient nitrogen dioxide in 47 187 neighbourhoods across 326 cities in eight Latin American countries: population exposures and associations with urban features. Lancet Planetary Health.

  This paper investigates satellite-based NO_2 concentrations in different cities across Latin America in 2019. While the study provides a robust assessment of NO_2 concentration levels, it focuses on a different pollutant and does not study the association with mortality, as our article does.

- Requia et al. 2024. Short-term air pollution exposure and mortality in Brazil: Investigating the susceptible population groups. Environmental Pollution.

  This paper studies the effect on mortality of $PM_{2.5}$, O_3 and NO_2 in Brazil for a 15-year period (2003-2017). They use satellite-based air pollution data at a coarser resolution of 12.5km. This paper has a similar scope to ours, focusing on another large Latin American country. The association was studied for sex and age, with no further analysis on other sources of heterogeneity, as our paper does.

- Requia et al. 2023. Short-term exposure to sulfur dioxide and daily mortality in Brazil: A nationwide time-series study between 2003–2017. Chemosphere.

  This paper studies the effect of SO_2 on mortality in Brazil, over a 15-year period (2003-2017), using satellite-based SO_2 concentrations at a resolution of 12.5km. This paper is similar to the above but focuses on SO_2, a different pollutant than we focus on, and in a different Latin American country.

  In our revised manuscript, we have incorporated a more comprehensive discussion of the literature in the Introduction (line ~82):

  > The recent emergence of new data measurement techniques, in particular remote sensing (Van Donkelaar et al., 2015), opens the possibility of measuring air pollution exposure with complete spatial coverage and over a much longer time scale. Such measurements have been used to monitor air pollution globally and

> over time (Rentschler & Leonova, 2023; Li et al., 2023), and to study its impact on mortality outside the developed world. Recent work has studied the empirical relationship between satellite-based PM2.5 exposures and mortality in Africa (Heft-Neal et al., 2018; Heft-Neal et al., 2020), Indonesia (Siregar et al., 2022), Brazil (Requia et al. 2024) and in cities in Latin America (Gouveia et al., 2024). For Chile, earlier studies on the short-term association between PM2.5 and health-related outcomes have used data from ground monitoring stations and focused on the capital, Santiago, and other cities, covering only a fraction of the country and population (Pino-Cortes et al., 2020; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007). More recent work that uses satellite-based estimates similarly only focuses on cities (Gouveia et al., 2024). In general, evidence from rural areas is scarce (Liu et al., 2021; Kim et al., 2020; Arku et al., 2020; Di et al., 2017), and in particular in Latin America.

In the final paragraph of the Introduction, we also make our contributions clearer (line ~185):

> Next, we demonstrate the additional insight that can be derived from data with complete and granular spatial coverage by investigating heterogeneity in the estimated effect over multiple dimensions, including region, urbanicity, baseline PM2.5 levels, population shares of 75+ individuals, and income. These dimensions of heterogeneity have not been fully addressed in previous work, which most commonly focuses on differences due to age and sex. Studying the entire country provides more variation in these characteristics compared to that in limited geographical areas such as cities; in addition, our commune-level data allow us to characterize heterogeneities within cities where they exist. Additionally, by using a longer time period (19 years, compared to up to 10 years in studies covering the same geographical region (Pino-Cortes et al., 2020; Busch et al., 2023; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007; Gouveia et al., 2024)) we investigate changes in effects over time, hinting at potential adaptations to air pollution.

Since the coverage of rural areas is a key distinction between our work and previous work, in our revised manuscript, we also include an extended analysis of the differences in effects in rural and urban communes for detailed mortality causes, which we have now included in Appendix Fig. S5.

We thank the reviewer for bringing up these recent publications, which underscore the growing interest in the field. We do not believe they detract from the contributions of our work, and we have highlighted our contributions more prominently in our revised manuscript.

Comment 2:

2 - Other point to be better discussed is the temporal resolution. Unfortunately, nowadays, the state-of-art analysis for short-term effects, or "acute", as written in the title, uses daily, maximum weekly, temporal resolution. Daily resolution allows for the model to account for lag effects, which is of paramount importance in short term effects of PM2.5 and temperature. All this was lost when analysing monthly resolution. This is even worth in the scenario of Chile and the regions with severe winter and other phenomena with high peaks on PM2.5 as described in the literature.

> Many thanks for this helpful suggestion. There have been several recent papers using monthly data, notably Gouveia et al. 2024, and Rau et al. 2023 in the US. Gouveia et al. use "short-term" or "subacute" to describe the effects, while Rau et al. 2023 use "mid-duration," and "moderate exposures," framing this as a contribution, since such effects are less well-understood but could become more important in the future since they capture effects due to wildfires and droughts.
>
> For use of the terms "short-term" and "acute," due to the Gouveia et al. paper being more closely related (and using the same satellite-based data as we do), we follow their lead and use "short-term," removing all references to "acute effects." This includes the title of our paper, which we revised to "Short-term exposure to fine particulate pollution and elderly mortality in Chile". We have also expanded the discussion on monthly effects, discussing their magnitudes in relation to daily and chronic effects, as well as their importance and limitations. The relevant text in the Discussion section reads (line ~593):
>
>> The estimated effects should be interpreted as the short-term mortality induced by monthly deviations in PM2.5 concentrations. We do not capture the chronic effect of exposure to PM2.5 pollution, which has been established in multiple other studies (Lepeule et al., 2012; Krewski et al., 2009; Pope et al., 2020; Chen & Hoek, 2020), even for Chile (Busch et al., 2023), with generally much larger effect sizes (in the range of 2-25% (Pope et al., 2020)). Daily, acute effects have also been documented in other studies, with similar effect sizes (0.8-4.2%), despite important differences in methodologies and contexts (Dominici et al., 2006; Liu et al., 2021; Pino-Cortes et al., 2020; Sanhueza et al., 2009; Cakmak et al., 2007; Cifuentes et al., 2000; Kloog et al., 2013; Orellano et al., 2020). We provide a slightly different perspective that is focused on monthly exposures, which do not capture immediate daily responses to increases in PM2.5. Monthly effects capture the effect of sustained weeks-long increases in PM2.5, such as those produced by wildfires, as well as the effect of illnesses with a longer window between onset and death. Such monthly effects are less well-studied and are not subject to regulatory standards, but could become increasingly important due to more frequent moderate exposure events such as wildfires and droughts (Rau et al., 2022). Consistent with findings in the US (Rau et al., 2022). and in Latin American cities (Gouveia et al., 2024), we find that the magnitude of monthly effects is similar to or exceeds that for daily acute effects, but is smaller than chronic effects. A limitation of monthly data is that mortality could potentially have occurred before the peak PM2.5 exposure in the same month.

Comment 3:
3 - Please, discuss the effect observed on the cities with

We did not receive a complete version of this comment and were unable to provide a meaningful response. We have expanded the discussion on several observed effects (e.g., for rural vs urban communes and 75+ population shares) based on comments from other reviewers. If there are further concerns, we would be happy to address them.

Comment 4:
4 - The use of land temperature instead of 2 metres ambient temperature (e.g., from ERA-5-land reanalysis) is main limitation that should be discussed and justified, since temperature is a main confounder in this association.

Thank you for this observation and for the suggestion to justify the selection of our temperature data. We chose the land temperature dataset over 2 meter ambient temperature for the following main reasons:

1. The land temperature dataset has a much finer resolution (1km vs 9km) than the ambient temperature dataset from the ERA-5-land reanalysis. The pixel resolution is especially relevant for characterizing population temperature exposures in cities, as we have granular population data at a census tract level. The temperature pixel resolution is also relevant for coastal countries, like Chile, where a large proportion of the pixel area may overlap with the ocean, which is expected to have different ambient temperatures than air above land. This issue is much more serious for pixels of size 9 km vs 1 km.

2. Land temperature and ambient temperature at 2 meters have a high correlation (0.9) (Good et al., 2017), with several studies explicitly recommending the use of satellite land temperature datasets due to its higher resolution and detail (Good et al., 2017; Li et al., 2023). Temperature is a control variable in our empirical model, and since the two measures are highly correlated, the choice of either should not affect estimates of our coefficients of interest.

Thanks again for pointing out this issue. We have added this explanation to our Methods section. The relevant text reads (line ~821):

> Another commonly used source of temperature data is 2-meter ambient temperature. We chose land temperature for two main reasons. First, the satellite land temperature data has a finer resolution (1 km) than available ambient temperature data (9 km). This allows us to more accurately estimate population exposures within urban centers, and minimizes errors in coastal areas where pixels overlap the ocean. Second, land temperature and ambient temperature at 2-meter have a high correlation (0.9) (Good et al., 2017), and studies have recommended the use of land temperature datasets (Good et al., 2017; Li et al., 2023). Temperature is a control variable in our empirical model (see Empirical

> Strategy), and since the two measures are highly correlated, the choice of either should not affect estimates of our coefficients of interest.

Comment 5:
5 - This reviewer suggests to exclude the analysis on adaptation. First, the time series might not be large enough to evaluate adaptation, usually requiring more than 30 years. Second, the authors evaluated it in even short periods of time, which is very short to observe any potential change in mitigation at the population level, unless a massive intervention was done.

Thank you for this suggestion. We agree with you that our analysis might not be sufficient to make definitive conclusions about adaptations, which require a longer timescale. However, we believe there is still value in our analysis, which spans a much longer time period than other studies in the same geographical region. As such, in our revised manuscript, we instead frame our findings as an investigation of changes over time, that could hint at potential adaptations, and state clearly that a full analysis would require more years of data than we have available.

We include an explanation in the Introduction (line ~198), that reads:

> Additionally, by using a longer time period (18 years, compared to up to 10 years in studies covering the same geographical region (Pino-Cortes et al., 2020; Busch et al., 2023; Cifuentes et al., 2000; Sanhueza et al., 2009; Cakmak et al., 2007; Gouveia et al., 2024)), we investigate changes in effects over time, hinting at potential adaptations to air pollution.

We changed the title of the subsection in Results from "Adaptation" to "Changes over time and income", to avoid an explicit reference to adaptation. In this section (line ~400), the relevant text reads:

> With documented harms to both population health and the economy due to climate change, a critical question is to what extent populations have adapted to these changes (Carleton & Hsiang, 2016). Fully investigating this question in the context of fine particulate pollution would require data on a decadal scale over multiple decades. Our data span close to two decades, and we estimate effects over 4-5 year periods in our panel (2002-2019). While this does not cover a sufficiently long time period to provide definitive evidence on adaptation, it is significantly longer than other recent work in Chile that cover up to a 10-year period (Pino-Cortes et al., 2020), and provides a first approximation to potential adaptations over time. We do not find convincing evidence of changes over time; coefficient estimates do not display an obvious trend (Fig. 4A; SI Fig. S7 displays estimated effects for single years). This could be due to two opposing effects: an expected adaptation over time which should decrease the effect size over time, and an aging population (see SI Fig. S8) which might increase the effect size (Fig. 3E).

Comment 6:
6 - Please, justify why the authors did not explore non-linearity in the PM2.5 association and why non-linear (ideally accounting for LAG as in DLNM) for temperature as main analysis.

Thank you for this suggestion. We have now extended our analysis on non-linear and lag terms.

Non-linearity

In our initial manuscript, we presented results for a linear term for PM2.5 and quadratic and spline terms for temperature (Figure S9; Figure S10 in the revised manuscript). These models produced very similar effect sizes for the PM2.5 exposure variable and so we chose the latter for simplicity. For the PM2.5 term, the use of a linear form facilitates comparison with other work (e.g., Liu et al., 2021; Kloog et al., 2013; Orellano et al., 2020; Rau et al. 2023). This is a similar strategy as Gouveia et al. 2024, which uses a linear term for PM2.5, and spline terms for temperature.

We have extended this analysis to explore non-linearity in PM2.5 exposures. In the figure below, we consider quadratic terms and cubic splines with 1, 3 and 4 knots. We find that these more flexible models result in roughly the same shaped response function as simple linear models, particularly in regions where majority of the data lie (16% of our data have PM2.5 exposures below 10 ug/m^3). This is a similar finding to Heft-Neal et al. 2018, which explores polynomial and spline terms for PM2.5 (with a linear term for temperature), finding that the shape of the response function is similar. They similarly opt for the linear version.

We now include this figure in the Appendix (Figure S12). In the main text we mention this robustness check in Results, under "Other sensitivity analyses." The relevant text reads (line ~553):

> Finally, we considered more flexible functional forms for the PM2.5 variable (SI Fig. S12), finding roughly the same shaped response function as simple linear models, particularly in regions where majority of the data lie.

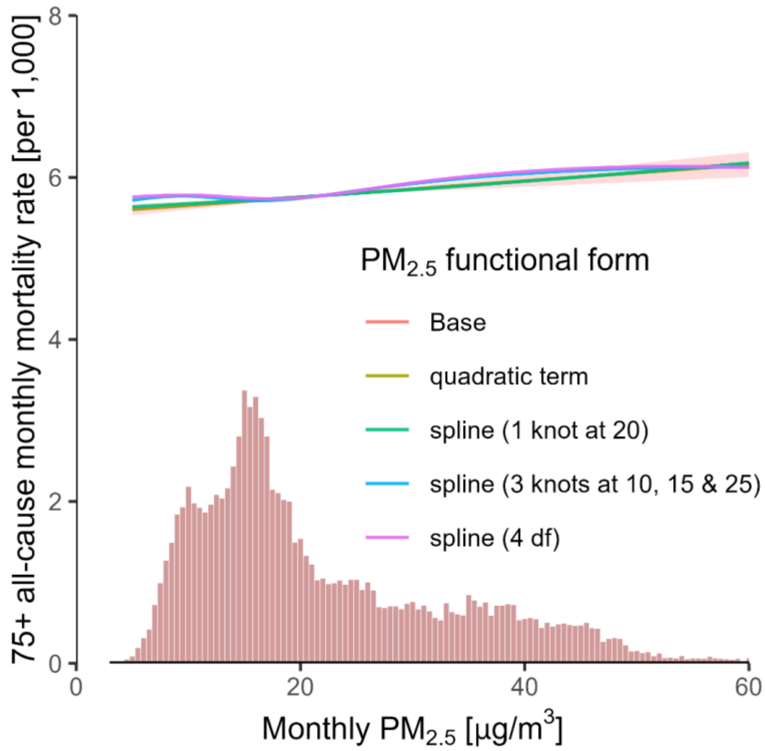
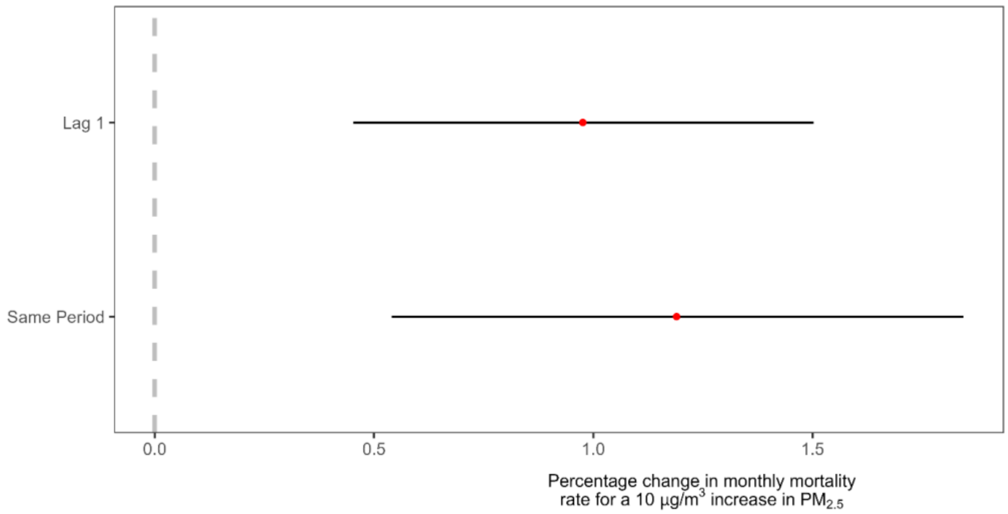


Fig. S12: Response function using more flexible functional forms for PM2.5. We consider quadratic and spline terms for PM2.5 exposure, instead of the linear (Base) model that was presented in the main results. These more flexible models result in roughly the same shaped response function as simple linear models, particularly in regions where majority of the data lie (84% of the data have PM2.5 exposures exceeding 10 ug/m^3).

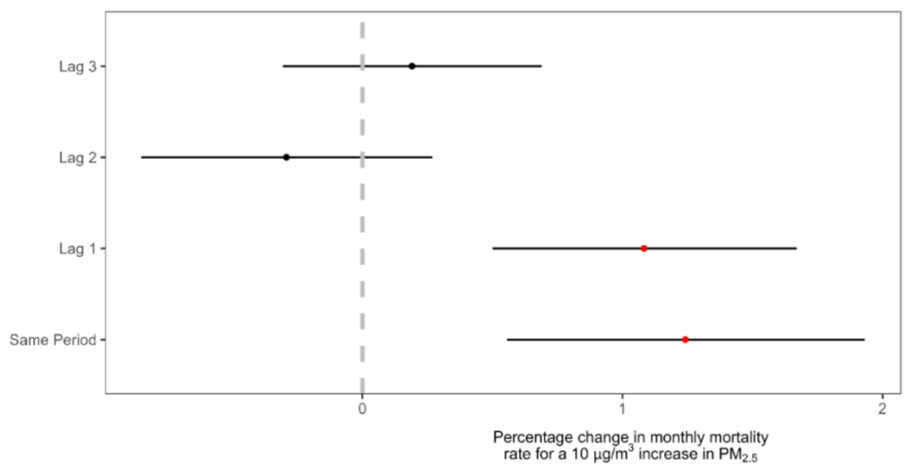
Lags

As for lags, similar studies using monthly data (Gouveia et al. 2024; Rau et al. 2023; see our response to Comment 2 above) did not incorporate any lags for PM2.5 exposure in their models, perhaps due to the difficulty in interpretation of the lagged variables, as you point out. Here, we present several suggestive results, including lags of up to 3 months.

The first model below includes 1 lag term and the current month's exposure. Both coefficients are positive and significant, on the same order of magnitude as our main result. If we consider both the effect of prior and current month exposure, the cumulative effect is potentially larger.



For a model with current month and up to three lag terms, the effects are positive and significant for the same period and lag 1, on the same order of magnitude as the previous model. The coefficients for lags 2 and 3 are close to zero and not significant.



The main goal of our analysis was to capture the short-term effect of monthly deviations in air pollution (in the same month). The results from these models support the main results that we present. After careful consideration, we decided not to include this new analysis into the manuscript, as the current set of results allows for a more straightforward comparison with other work. We also believe that to make conclusive statements about lagged responses, a fuller investigation needs to be done, for example one that takes into account highly correlated seasonal variables or that incorporates more complicated lagged structures such as distributed lag non-linear models, as you suggest.

Comment 7:
7 - Validation. Only linear correlation is not enough to validate an exposure. Please, report also other metrics, such as RMSE.

Thank you for this suggestion. Our initial focus on linear correlation was to our knowledge in line with related work (e.g., Heft-Neal et al. 2020, van Donkelaar et al. 2016). We now note that there is some work that does a more comprehensive evaluation using both R^2 and RMSE (e.g., Tong et al. 2023 for O_3, which describes R^2 as a measure of overall accuracy, and RMSE as a measure of deviance). We have added RMSE values in Table S1 in the SI, reproduced here:

Table S1 Comparison of monthly $PM_{2.5}$ monitor data against satellite data. Number of months of observations (n), coefficient of determination (R^2) and root-mean-square error (RMSE) for each year and region (from North to South).

	n	R^2	RMSE
2015	434	0.67	12.57
2016	485	0.63	17.61
2017	511	0.64	11.85
2018	570	0.65	14.23
2019	612	0.62	9.96
2020	615	0.58	10.70
2021	631	0.57	12.03
Region XV	80	0.002	6.93
Region I	65	0.38	4.79
Region II	83	0.44	5.52
Region III	165	0.02	6.21
Region IV	156	0.46	4.50
Region V	224	0.37	10.18
Region M	717	0.82	6.39
Region VI	288	0.85	7.32
Region VII	421	0.71	12.68
Region XVI	162	0.58	16.48
Region VIII	587	0.55	10.73
Region IX	243	0.75	14.78
Region XIV	165	0.63	14.81
Region X	216	0.81	14.72
Region XI	208	0.61	32.61
Region XII	78	0.05	9.28
All	3858	0.61	12.76

We find that the RMSE between satellite-based and monitor estimates is 12.76 ug/m^3, and is consistent across the years. In our previous analysis, we identified regions with poor correlation of satellite vs monitor data to be regions XV, III, and XII. We also identified region XI to have relatively high correlation, but with extreme values (SI Fig. S1) on the high end of the spectrum that are not being accurately captured by satellite estimates. The large RMSE reflects this observation. We have incorporated these findings into the results in the SI (Section on "Accuracy of satellite-based PM2.5 estimates").

In an additional check, we identified regions with larger RMSE (regions XI, IX, X, XIV and XVI), and estimated a model excluding these regions. We find an effect size of 3.1%

(CI: 2.3-4.0%), slightly higher than the main model but not statistically significantly different.

Comment 8:
8 - Please, clarify that the model adjusted by q1-2010 and q1-2011, and not only q1,q2,q3,q4, without accounting for year. This reviewer is quite concerned about the best adjustment for seasonality and linear trend. In Fig S9 it is clear it has an impact on the point-estimate and precision. Please, justify why the quarter-year was chosen as main analysis. This figure could go to the main paper.

Thank you for this question. Yes, the model adjusts for quarter-year, q1-2010 and q1-2011, etc., in your example. We have clarified this in the manuscript, under Empirical Strategy (~ line 967): "… time fixed effects at a quarter-year level (72 in total; 4 quarters each for 18 years."

We share your concerns about seasonality and linear trend adjustments, and for this reason included the previous Fig. S9 (Fig. S10 in the revised manuscript). The two models you raised, with separate year and quarter effects (2003 +... + 2019 + Q2 + Q3 + Q4), and the quarter-year effects (which are used in our main analysis) show almost identical results for the effect of PM2.5.

The reviewer is correct to point out that other models affect both the point estimate and precision of estimates. Our data are at a commune-month level, and we use fixed effects for commune and quarter-year. The use of more restrictive fixed effects, such as month, would improve the identification of the causal effect, while the use of less restrictive fixed effects, such region (instead of commune), would do the opposite. With region fixed effects, communes in the same region with different PM2.5 exposures would be compared to each other. The use of the more restrictive commune fixed effects essentially compares communes to themselves at different times. Hence, this reduces the variation in the PM2.5 and mortality variables that are used to estimate the effects. In our case, the inclusion of both commune and month fixed effects results in insufficient variation to estimate the effects of interest. We might compare our approach to Gouveia et al. 2024, which uses a similar monthly PM2.5 exposure measure (although they use a different two-step regression approach). While they use a fixed effect for year-month, their spatial granularity is much lower, at a city level: Santiago, for example, contains 52 different communes. Our commune-level analysis does not allow us to exploit this spatial variability. Hence, we believe our choice of quarter-year and commune fixed effects is a suitable balance between causal identification and the amount of variability in the data.

We include this explanation as part of the main results (line ~521):

> To check the robustness of our results to modeling assumptions, we consider a wide range of alternative model specifications. First we use Poisson and ordinary least squares models instead of the negative binomial model that was presented in the main results (SI Fig. S9). Instead of quarter-year fixed effects, we consider the

more restrictive month-year fixed effect, and other variations of month fixed effects (SI Fig. S10). While these more restrictive fixed effects improve the identification of the causal effect, they absorb most of the variation in PM2.5, thus diminishing its observable effect on mortality. While estimates remain positive, they are not statistically significant. We also consider fixed effects that vary by region, and more flexible functional forms for the temperature variable.

We appreciate the feedback to include the Fig S10 (old S9) in the main body, but after considering space constraints, we limited the Figures in the main text to those containing substantive results. In the less space-constrained SI we provide an explanation of this and all other robustness checks.

Comment 9:
9 - The authors used a population-weighted estimation. What was the impact of this approach instead of using only commune area weights? Even in this scenario when the unit of analysis is the commune per se. Please, discuss.

Thank you for this question. We believe this comment is referring to our method of using population numbers at a census tract-level to estimate exposure to air pollution and temperature, compared to an alternative which simply uses the intersection of grid cells with commune boundaries, without taking into account population.

We used the population-weighted method as it provides a much more accurate assessment of population exposed to air pollution and temperature. If instead we simply used the gridded data averaged over entire commune polygon areas, the estimates could be heavily biased. In Chile there are larger communes with small urban centers, where the majority of people live, and unpopulated areas in the rest of the commune. We might also expect temperature and air pollution less populated areas to be different than in densely populated areas, as air pollution originates from human activities, and cities create urban heat islands. Ultimately what matters for health effects is not the presence of the air pollution or temperature, but rather the exposure to people. In that regard, a population-weighted approach provides accurate estimates not just of air pollution and temperature, but of people exposed to it. For these reasons, population-weighted approaches have been favored in the literature (e.g., Gouveia et al. 2024; Requia et al. 2024).

Comment 10:
Minor
1 - When citing pneumonia as an example of not too short effect, that is not true. The effect of temperature and air pollution on incidence of pneumonia is short, from days to two weeks maximum.

Thank you for this comment. We have removed the example of pneumonia.

Comment 11:

2 - Please, revise the term "obscure" the effect of PM2.5. This is not scientific. What happens is that it decreases the contrast range of PM2.5, decreasing its variability.

> Thank you for this suggestion. We have changed the wording of the sentences that use the term “obscure”, changing it to “We use quarter-year instead of month fixed effects, as most of the variation in PM2.5 is absorbed by month fixed effects, thus diminishing its observable effect on mortality.”

Many thanks for the opportunity to read your work.