

The comments of both referees are on black font below, and the authors' response in on blue.

Referee # 1 (anonymous)

Referee's comment

The paper aims at estimating monetized health impacts caused by emissions of 1 t of PM_{2.5} or 1 t of PM_{2.5} precursors. The monetized health impacts are given distinguishing between emissions of main source categories and emissions in urban vs. non-urban areas. The calculation is made according to the state of science; the science used is sound, the methodology is well described, calculations have been thoroughly made.

Authors' response

Thank you for these positive comments.

Author's changes in manuscript

No changes made.

Referee's comment

However, progress beyond the current state is not made, new developments are not addressed.

Authors' response

The manuscript includes also progress beyond the current state-of-the-art. However, we have not highlighted these sufficiently well in the original manuscript.

First, the emission and partly also dispersion modelling has been performed using a very fine spatial resolution, up to 250 x 250 m²; these have not previously been used for such a geographically extensive area. In comparison, e.g., Heo et al. (2016) used as the basic material the predicted concentrations on a spatial resolution of 36 x 36 km², i.e., two orders of magnitude coarser than in the present study. This resolution scale is characteristic of all the previous studies on public health costs.

We have previously shown (Karvosenoja et al., 2010, Korhonen et al., 2019) that the exposure values evaluated using integrated assessment models can be very sensitive to the methodology. In particular, exposure values can substantially increase with an increasing spatial resolution. Karvosenoja et (2010) showed that using a finer spatial resolution, from 10 km to 1 km, resulted in an increase of the population weighed concentration caused by traffic emissions by an order of magnitude. It is therefore essential to use a sufficiently fine model resolution in view of the health impacts. This is especially important for primary particles from emission sources at fairly low emission heights.

Second, we have also considered the health costs related to the PM_{2.5} precursor emissions on a much finer spatial resolution (5 x 5 km²) than any previous corresponding study. A couple of previous studies have considered precursors (Muller and Mendehlson, 2009, Heo et al. 2016). However, both of the above mentioned studies addressed the U.S. as their modelling domain, Heo et al. (2016) on a resolution of tens of km. The resolution used in the study of Muller and

Mendehilson (2009) was not reported, but considering their results suggests a resolution of tens of km. Air chemistry and the formation of particulate matter is strongly dependent on the chemical composition of the emitted pollutants and climatic factors; the above mentioned American studies can therefore not per se be generalized for European conditions. In addition, we have also allowed for the organic PM in our computations; this was not taken into account by Muller and Mendehilson (2009) or Heo et al. (2016).

Third, we have also programmed an internet-based computation tool, which is publicly available. We expect that this concept and the methods included within this easy-to-use tool would be useful also internationally.

Fourth, the manuscript includes a thorough, up-to-date literature review in this field (in the introductory section). Such a detailed review has not previously been presented in the literature. We have also reviewed in detail the current methods for evaluating the health impacts (in section 2.3) and economic impacts (section 2.4).

Although several public health cost studies with similar aims have been conducted in the U.S., reviewed European articles on health costs are much more scarce. Such studies will need to be done and published for several European regions, for various pollutants, and using different methodologies.

This is needed, as the relevant environmental conditions, such as air chemistry, population densities, the structure of emission source categories, the economic costs of life years, and many other relevant factors, are substantially different in various parts of Europe. The range of uncertainty in such estimates is also large, as shown also in the present study. For consolidating such estimates for various regions, pollutants, methods, etc., therefore requires that such evaluations will be presented for several European regions.

Author's changes in manuscript

We have restructured and revised the conclusions section; the revised conclusions contains a more explicit and clear description on what new developments were introduced in this study, in the context of previous literature.

Referee's comment

An example for currently analysed improvements of the methodology is accounting for NO₂ impacts, which the WHO now considers as likely, though less certain than PM_{2.5} impacts.

Authors' response

This is indeed a relevant and timely comment. Especially older diesel cars emit significant amounts of oxides of nitrogen. As diesel cars have become more common, there has been an intense debate about the need to curb NO₂ emissions in Europe. Long-term exposure to NO₂ has been associated with increased mortality in many epidemiological studies.

However, separation of the effects of NO₂ from the effects of particulate matter is challenging, due to their high correlation. Because of major uncertainties concerning concentration response functions, we did not include NO₂ in this study. WHO has also acknowledged that the effects of NO₂ are partly overlapping with PM_{2.5} effects in epidemiological studies (WHO, 2013).

Author's changes in manuscript

We have added a clarification on why the direct health impacts of NO₂ were not addressed in this study, to the second last paragraph of the introductory section.

Referee's comment

Another issue is using exposure (i.e. concentration where people are) instead of concentration in the background, which would involve analysing also indoor sources (smoking, frying, wood heating).

Authors' response

We totally agree that this is an important new research direction. Some of the present authors have studied this matter by detailed modelling for Helsinki (e.g., Kousa et al., 2002, Soares et al., 2014, Kukkonen et al., 2016) and for London (Smith et al., 2016, Vikas et al, 2019). These studies have allowed also for the movements of the population in various micro-environments, and the infiltration of pollution to indoor air. However, performing such modelling for an entire country would require a substantial amount of work. In our view, this could be a topic of another separate study.

Author's changes in manuscript

We have added a paragraph on dynamic exposure modelling to the conclusions section (next to the last paragraph).

Referee's comment

Another field is source apportionment to improve the large uncertainty of atmospheric modelling. In the text, a small chapter should at least mention current work on improving the methodology. A new part is the calculation of health impacts for emissions of different source groups like transport, residential a.s.o. Up to now, monetized health impacts have been estimated differentiated according to height of release and urban/nonurban area (see e.g. <http://ecoweb.ier.uni-stuttgart.de/EcoSenseLE/current/index.php>). Some important (now older, but still valid) guidelines for the methodology could be cited, e.g. the Externe Externalities of Energy Methodology 2005 Update; downloadable at http://www.externe.info/externe_d7/?q=node/30 or the IEHIAS Integrated Environmental Health Impact Assessment System (2011); web based guidance system; accessible under www.integrated-assessment.eu .

Authors' response

Emission and atmospheric modelling in this study has been substantially improved, compared with previous studies, especially concerning the national very fine resolution emission inventory and numerous recent refinements of the SILAM model. Some recent references have been added regarding the SILAM, FRES and UDM-FMI models, which illustrate the most recent advancements of these models.

We agree with the reviewer that a discussion and references on the EcoSenseLE and ExterneE need to be added to the literature review and to the relevant sections in the text (e.g., Bickel and Friedrich, 2005). Clearly, these are very important research methods and activities in this area, and certainly need to be discussed and referenced.

Author's changes in manuscript

We have added discussion on the methodology of Bickel and Friedrich (2005), and the EcoSenseLE tool to the introduction (4th paragraph).

We have updated the references to the FRES model to include two latest published articles (section 2.1, first paragraph). The references regarding the SILAM model and its applications have been updated in section 2.2., first paragraph and section 2.2.1, first paragraph. One recent reference has been added on the evaluation of the UDM-FMI model (section 2.2.2).

We have also added to the conclusions section a discussion on model improvements in the present study, as per reviewer request, especially regarding the spatial resolutions and organic particulate matter. We have mentioned that one important chemical constituent of the PM_{2.5} that has been neglected, according to our knowledge, by all of the previous published health cost studies has been here taken into account, viz. the organic particulate matter. We could take this fraction realistically into account, due to recent developments of the SILAM model. However, we felt that a more deep and thorough discussion on the improvement of chemical transport and other dispersion models would be outside the scope of this study.

Referee's comment

The result is indeed useful for consultants or decision makers in Finland, that want to identify the most efficient measures for reducing health effects from fine particles. They may use the given unit cost figures for estimating the health benefit caused by reduced emissions, and could also use the tool provided. The editors should decide, whether the publication fits into the scope of their journal or whether a journal more oriented towards environmental policy application might be a better choice

Authors' response

In the authors' view, the paper would fit best to the scope of ACP.

Author's changes in manuscript

No changes made.

References

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Referee # 2 (Ståle Navrud)

Referee's comment

Interactive comment on “Modelling of the public health costs of fine particulate matter and results for Finland in 2015” by Jaakko Kukkonen et al.

Ståle Navrud (Referee) stale.navrud@nmbu.no Received and published: 12 May 2020

This paper is an excellent and state-of-the-art application of the impact pathway approach (also termed the damage cost approach by economists) to assess public health costs of fine particulate matter (PM_{2.5}) in ambient air from domestic primary and precursor emissions of PM_{2.5} in Finland in 2015. The paper provides a very transparent overview of the assumptions and calculations in all steps of the impact pathway model they develop; from emissions from different sources through dispersion and exposure to concentration-response functions and the economic valuation of the selected health endpoints.

Authors' response

Thank you for these positive comments.

Author's changes in manuscript

No changes made.

In the economic valuation of premature death due to PM2.5 the authors argue for using the number of lost life years and the Value of a Life Year (VOLY) rather than valuing all people at the Value of Statistical Life (VSL). This implies assigning a lower value to the elderly, that make up the majority of people dying prematurely due to PM2.5. The authors state in line 402-407: “The monetized estimates in the computations of the economic impacts in this study are based on the average value of a life year (VOLY), instead of the value of statistical life (VSL). The VOLY-based approach has been commonly used as a measure to assess a decrease in mortality risk (Im et al., 2018), whereas the VSL-based approach, despite its disadvantage, is in line with EPA’s standard procedure and recommendations (Wolfe et al., 2019)”. The “despite disadvantages” phrase should be deleted. They should instead say choosing between the VSL and VOLY approaches is also an ethical question in terms of whether one should assign the same economic value to all adults, independent of age; and that a constant VOLY assumes that people value a life year the same independent of age.

Authors' response

There was an unfortunate error in our text in the manuscript. In the method section 2.4. (lines 402-407 in the original manuscript), we should write that both VOLY and VSL -based methods were used. This is how we actually used the methods in the ‘results’ section, and how we described the conclusions.

We also agree that ‘despite disadvantages’ should be removed, and that selection between these two measures is partly an ethical question.

Author's changes in manuscript

We have corrected the error mentioned above (the revised manuscript version reads that we use both VOLY and VSL).

We have deleted the phrase “despite disadvantages“, in relation to VSL. We have also somewhat restructured the 2nd and 3rd paragraphs of the section 2.4 to be more logical.

Also the authors further down in the paper (line 669-672) say that they have performed calculations using both VOLY and VSL (see also table 4), in order to show the difference in calculated economic benefits from emission reductions; which is of course very good that they do. However, in line 402 (see citation above) they state that they only used VOLY. This has to be corrected to made consistent with the calculations/sensitivity analyses they actually performed.

Authors' response

We thank the reviewer for finding this mistake in the original manuscript.

Author's changes in manuscript

We have corrected the mistake.

In line 399 after the sentence “This also facilitates numerical comparisons with those studies” they should add that: “Ideally a country-specific VSL (and a derived VOLY from this VSL) for Finland should be applied in this analysis, which could be based on value transfer (Navrud and Ready 2008) from the most recent global meta analysis of stated preference studies of VSL (Lindhjem et al 2011). However, this would preclude the direct comparison of results with similar impact pathway models e.g. Holland et al (2005).

Authors’ response

We agree with the reviewer on this comment and thank him for providing this text.

Author's changes in manuscript

We have added this comment to the manuscript (2nd paragraph of section 2.4).

In the appendix the authors use VOLY as the abbreviation for Value of a Life Year , whereas in the text also VLY is used (e.g. in the paragraphs including lines 670, 685 and 705). This should be consistent, and I suggest using VOLY all the way through the paper.

Authors’ response

We agree.

Author's changes in manuscript

All such acronyms have been corrected to read ‘VOLY’.

They find that the economic benefits om emission reductions are clearly largest for the emission reductions for the source categories that have low emission heights, and are located in more densely populated regions (i.e. vehicular traffic, non-road and machinery and residential wood combustion urban versus rural areas). In the Abstract they say “It was found that economically the most effective measures would be the reduction of the emissions in urban areas of (i) road transport, (ii) non-road vehicles and machinery, and (iii) residential wood combustion”. However, in economics cost-effectiveness, that this can statement can interpreted as, is a measure of e.g. reduced number of kg of PM 2.5 emissions per euro of abatement costs. Thus, the authors should rather say that “ the economic benefits in terms of avoided public health costs is largest for measures that will reduce of (i) road transport, (ii) non-road vehicles and machinery, and (iii) residential wood combustion”. Whether these measures have the largest net benefits (Economic health benefits minus the abatement costs) depends of course on the costs of reducing emissions from theses sources.

Authors’ response

We totally agree with the reviewer, this is an important point.

Author's changes in manuscript

We have corrected this mistake in the abstract.

This also has to be corrected when using the word “effective” both in the abstract and in line 399-805.

Authors' response

We have checked the whole manuscript for the correct usage of terminology in this respect.

Author's changes in manuscript

We have revised the text at several instances, to be in accordance with the correct terminology, as per reviewer request.

There could also be other economic benefits than to public health from reducing the PM2.5 emissions, such as reduced soiling and corrosion of residential and commercial buildings, historic monuments and cultural heritage buildings as well as reduced environmental damages. The authors very briefly mentions this at the end of the paper, but they could also mention that there are ways to value these economic benefits and refer to e.g. Navrud and Ready (2002, 2007) and Watt et al (2009).

Authors' response

We agree on this comment.

Author's changes in manuscript

The first paragraph in the introduction has been extended to include these comments and references.

References:

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Author's changes in manuscript

The above mentioned references were added to the reference list.

Other minor revisions**Author's changes in manuscript**

In the affiliations on the first page, we have added the acronym 'SYKE', as this institutes prefers to write its name in this manner. The name of the third institute has also been updated. A second affiliation has been added to Prof. Lanki.