

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Heat related respiratory hospital admissions in Europe in a changing climate: A health impact assessment
AUTHORS	Åström, Christofer; Orru, Hans; Rocklöv, Joacim; Strandberg, Gustav; Ebi, Kristie; Forsberg, Bertil

VERSION 1 - REVIEW

REVIEWER	Kristen Malecki, PhD, MPH Assistant Professor (CHS), Department of Population Health Sciences University of Wisconsin, Madison USA There are no competing interests in reviewing this article that I am aware of.
REVIEW RETURNED	21-Sep-2012

THE STUDY	Overall I feel this is an important study and contribution to the field and warrants publication. The only minor limitation to this study that I see is the use of the HYDE model from the Netherlands for estimating populations at risk is not clearly defined and needs additional description. It is unclear in the methods why these population data were selected as they limit the ability to look at variations in older adults. If this was a first step towards a more detailed or refined assessment that will be published or explored in a subsequent paper this should be indicated. Also, it is hard to imagine that there are not any data across Europe at a smaller than country scale that would allow for some analyses stratified by age - for the older populations. While it is clearly identified as a limitation of this paper - it would be helpful to then understand why this choice of population data was made relative to a different population source.
RESULTS & CONCLUSIONS	I feel the messages are overall very clear - one suggestion is to really emphasize that these results suggest that investment in early warning and preventive systems may be warranted particularly in Southern Europe. In addition, I think it is important to discuss in the conclusions and discussion (as the authors did while describing the measure in the methods but not later on) the influence of humidity and temperature in measuring heat related effects - and how much in the current projections a contribution that dew point/humidity plays relative to overall temperature and may explain the North/South gradient in changing conditions is something that the author group may consider to add to the discussion. E.g. in estimating health impacts from heat - temperature alone is not always an issue it is the temperature *humidity that create the dangerously high heat days and should be discussed. Finally, this is an important application of previous research - the results from previous work are used in a risk assessment framework to project future public health risks from previous studies. This work is critically important in translating the

	<p>health impacts of various climate projections to future scenarios and helping policy makers and other relevant stakeholder audiences to start to understand potential future implications. The title of the article is framed as a "health impact assessment" - in the United States a "health impact assessment" refers to a process by which you start with a policy change and then identify future potential health impacts integrating ideas from all stakeholder groups. The relevance and utility of these findings for decision-making around adaptation and mitigation is not mentioned in the discussion. Some discussion regarding future directions that includes further refinement of these methods in particular for identifying and making projections for future vulnerable sub-populations will be important for regions not only in Europe but across the board and should be mentioned as part of the conclusions/discussion.</p>
GENERAL COMMENTS	<p>I think this is a valuable contribution to the field and should be published. My thoughts/suggestions on minor issues within the manuscript as stated above were also included as "sticky notes" in the pdf version of my review.</p>

REVIEWER	<p>Lorenzo Cecchi, MD Interdepartmental Centre of Bioclimatology University of Florence - Italy</p>
REVIEW RETURNED	<p>02-Oct-2012</p>

GENERAL COMMENTS	<p>GENERAL/MAJOR COMMENTS</p> <p>This study aims at assessing the possible effect of climate on admissions for respiratory diseases under different climate change scenarios over Europe. Main findings show an increase of admissions in most of European countries, the larger occurring in the south and the smaller in the east. The paper is well written and easily readable.</p> <p>Results of the study are fascinating and they could support the implementation of adaptation measures able to reduce the effects of changes on human health in Europe. However, several limitations affect results of the analysis:</p> <ol style="list-style-type: none"> 1) The uncertainties and high variability of climate change scenarios are well known and briefly mentioned in the discussion by the way. The key role of local conditions is accepted as well. The question is: is it a study at European level feasible (and reliable)? 2) Health related data are poorly characterized. Other than the lack of stratification by age (which is of great importance in health-related diseases), no information about the type of respiratory diseases, which lead to hospital admissions, are available. On the basis of the European epidemiological data, it is arguable that most of admissions are related to chronic respiratory diseases (COPD and asthma). However, this issue should be better discussed in the paper. 3) Both point 1 and 2, introduce a several uncertainties, which affect the reliability of results. In summary, both climatic and health-related datasets have limitations while the study is well conducted both in terms of design and analysis.
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	<p>In conclusion, discussion could be implemented with a wider critical view on these limitations.</p> <p>SPECIFIC/MINOR COMMENTS</p> <p>Page 4, line 33-35. A prediction on global impact on mortality under projected climate change scenarios is really difficult. Maybe, possible reduction of both mortality and hospitalization during milder winter should be taken into account too.</p> <p>Page 6, line 7: Author could explain the reason(s) why WHO instead of EUROSTAT database has been chosen for this study</p> <p>Page 11, line 40-60. Among social adaptations, authors could include and discuss the beneficial effects of warning systems which have been implemented in several countries. Two recent Italian papers have shown this effects and could be cited and discussed (Schifano P, Leone M, De Sario M, De'donato F, Bargagli AM, D'Ippoliti D, Marino C, Michelozzi P. Changes in the effects of heat on mortality among the elderly from 1998-2010: results from a multicenter time series study in Italy. Environmental Health 2012, 11:58; Morabito M, Profili F, Crisci A, Francesconi P, Gensini GF, Orlandini S: Heat-related mortality in the Florentine area (Italy) before and after the exceptional 2003 heat wave in Europe: an improved public health response? Int J Biometeorol 2012, 56:801–810.)</p>
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REVIEWER	<p>Kim Knowlton, Senior Scientist, Health & Environment Program; and Co-Deputy Director, Science Center; Natural Resources Defense Center (NRDC). Also Assistant Clinical Professor, Environmental Health Sciences, Mailman SPH, Columbia University; USA.</p> <p>No competing interests.</p>
REVIEW RETURNED	15-Oct-2012

THE STUDY	<p>Is the research question clearly defined? A central tenet of the research question is not clearly stated in the manuscript, namely the mechanisms by which “extreme heat has a large impact on mortality and morbidity, with a large relative increase for respiratory diseases” [from the “Objectives” on manuscript p.2]. Although citations #3-5 are provided to support this statement, there is less clarity how the authors draw the link from “global warming in the near future” [from “Article focus” on manuscript p.3] to the “heat</p>
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related hospital admissions” [also from “Article focus”]. The language is not sufficiently specific re: whether authors propose the link between climate change, extreme heat, and respiratory hospital admissions (RHAs), the health outcome measure quantified in Results & Tables 1-2, is via rising air pollution concentrations. The authors may be saying there’s a more direct mechanism with heat itself and respiratory admissions, independent of air pollution; and this needs to be clarified. This could be easily accomplished, for example, with some additional clarification on manuscript p.4, line 33-35; in 1st sentence authors could include a brief description & citation(s) of proposed effect extreme heat has on respiratory illness that increases hospital admissions. Michelozzi et al (2009) pp.387-388 discuss some possible pathways and means by which heat increases respiratory admissions. Though this paper is referenced early in this manuscript (#3), a brief re-statement of this info and/or other info motivating the study, in Intro of paper would more clearly define the research question for readers, and would be entirely appropriate. Vigilance in being clear about respiratory hospital admissions vs. “heat-related” hospital admissions in the manuscript would be helpful. Respiratory admissions are the focus here, but it seems sometimes that the broader category of “heat-related” effects of climate change get center stage. Please bound the discussion a bit more, as it is a fine piece of work with an ambitious scope.

Are the methods adequately described?

Methods need a bit more info in lines 21-24 on p.6, to provide context and specifics from reference #3, the PHEWE project (and please spell out acronym, Assessment and Prevention of Acute Health Effects of Weather Conditions in Europe, for readers, even though it is described in citation #3 Michelozzi et al. (2009). It is a key reference for this manuscript, as it was the first study to show that high temperatures increase risk of respiratory hospital admissions (morbidity), as well as mortality. Page 6, lines 43-48, was it a generalized heat morbidity function, or a more specific heat-RHA relationship coefficient from the PHEWE study that was applied in this study?

Are the abstract/summary/key messages/ limitations accurate?

The Abstract/summary/key msgs/limitations could use more specific info: Methods p.2, lines 24-25, don’t authors mean “heat-related respiratory hospital admissions under a changing climate are projected...” (this addition is an important, focusing detail); lines 30-31, which GHG emissions were applied? (say A1B & A2); key message p. 3 lines 16-17 say what “30 year annual mean” i.e. what time frame? (“in 20 years” or “around 2035” or “2021-2050”); and p.3 lines 19-20 “in 10s of thousands” [an important finding] would be clarified by saying relative to what reference year (i.e. 1995, or 1981-2010) These details will add clarity to the important findings of the work.

Are the statistical methods described?

Statistical methods are not described, in terms of the specific tests applied to calculate the RR coefficients, 95% confidence intervals and thresholds used, from the PHEWE project. They could be quickly mentioned in reference; or it may be the assessment of the editors that the existing reference to Michelozzi et al. (2009) and the PHEWE study is sufficient. This reviewer would like some more in-manuscript specific info to not have to search outside the paper for the relevant information.

	<p>Does anything raise questions about the work? No, this work has reported data adequately, no questions occur to the reviewer.</p>
GENERAL COMMENTS	<p>The information mentioned on manuscript p.8, lines 5-6 (the unpublished data on each country available from authors) may be suitable as supplemental information.</p> <p>p.9, Table 2. aren't these "the percentage of the annual expected number of RHAs in each region"?</p> <p>p. 11, lines 48-51: not the last 50 years actually. ("Studies from the U.S show that the heat related health burdens decreased over the last 50 years, indicating that some adaptation is taking place.") Davis et al.'s dataset covered only 1964-1998, which is ~35 years of data, and did not include the most recent 15 years. It may be more judicious to say "Studies from some recent decades in the U.S. show.."</p>

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

Comment 1: While it is clearly identified as a limitation of this paper - it would be helpful to then understand why this choice of population data was made relative to a different population source.

Reply: The population data used (HYDE) was the only gridded population data that had an equal or higher spatial resolution than the climate data at the time of analysis (more refined data may have become available later). This is now commented in the Discussion section.

Comment 2: I think it is important to discuss in the conclusions and discussion (as the authors did while describing the measure in the methods but not later on) the influence of humidity and temperature in measuring heat related effects - and how much in the current projections a contribution that dew point/humidity plays relative to overall temperature

Reply: The temperature variable AT max used in the PHEWE Study (see reference list) and in this study takes humidity into account. However, the influence of humidity on heat-health impacts is often discussed. Most population level studies from the US and Europe found that no daily temperature measure was consistently better in predicting mortality (E.g see Hajat S, Armstrong B, Baccini M, et al. Impact of high temperatures on mortality: is there an added heat wave effect? *Epidemiology* 2006;17:632e8). The effect on hospitalisations does not seem to differ from mortality in this respect. Although we acknowledge that this is unexpected in regards to heat physiology, we feel that this have been sufficiently addressed in the study.

Comments in the pdf:

1. Is this in excess or total when presenting the CA results? P.4 l.16-17
2. How much of the results can be attributed to changes of relative humidity rather than heat alone?
3. It is not clear how the population projections at the larger grid scale are employed at the smaller grid scale to match the climate data.
4. Is the one climate projection - the middle of the road projection or the extreme scenario?
5. It may also be useful to consider running a sensitivity analyses for older adults using the RR for older adults

Replies:

1. The impact described is a relative impact, which can be interpreted as the excess morbidity.

2. The apparent temperature is rather complex combination of temperature and relative humidity so it would be hard to distinguish the main contributor to the increased health burden. Global warming is affecting both.
3. The reviewer seems to have misunderstood the data description. The population data is on a higher resolution, or a smaller grid, than the climate data and we have tried to clarify this better in the data description in the text.
4. The mean estimate is the mean increase over the five climate change projections which spans from a high to a middle of the road emission scenario.
5. This is a good idea and is touched upon in the discussion where we give suggestions on future impact assessments with age stratified population data.

Reviewer 2

Comment 1: The uncertainties and high variability of climate change scenarios are well known and briefly mentioned in the discussion by the way. The key role of local conditions is accepted as well. The question is: is it a study at European level feasible (and reliable)?

Reply: The study uses pooled estimates of heat and respiratory admission representative of the regions studied combined with population data and climate change scenarios. It describes a macro perspective on respiratory disease and climate change. Although, such information has limitations from a local perspective, it is key to European and national stakeholders to understand and inform decision making. Local studies that could address special circumstances are also needed. This is addressed in the Discussion and Conclusions sections.

Comment 2: Health related data are poorly characterized. Other than the lack of stratification by age (which is of great importance in heat-related diseases), no information about the type of respiratory diseases, which lead to hospital admissions, are available. On the basis of the European epidemiological data, it is arguable that most of admissions are related to chronic respiratory diseases (COPD and asthma). However, this issue should be better discussed in the paper.

Reply: The type of respiratory hospital admissions (ICD codes) that the PHEWE study was based on was added to the Methods section. Studies show that COPD patients are at high risk during heat waves, and that COPD is increasing. We have addressed this in the discussion section.

Comment 3: Page 4, line 33-35. A prediction on global impact on mortality under projected climate change scenarios is really difficult. Maybe, possible reduction of both mortality and hospitalization during milder winter should be taken into account too.

Reply: The paper aims to address the problems with possible increases in RHAs related to heat and the possible need to further implement warning systems, hospital availability during summers etc. We do not try to estimate the overall change.

Comment 4: Page 6, line 7: Author could explain the reason(s) why WHO instead of EUROSTAT database has been chosen for this study.

Reply: We were familiar with WHO database and it had the ICD 9 coded hospital admissions as in the PHEWE (Eurostat uses ICD 10 codes).

Comment 5: Page 11, line 40-60. Among social adaptations, authors could include and discuss the beneficial effects of warning systems which have been implemented in several countries.

Reply: This is an important issue and we have added a short discussion regarding the possible health

benefits from the implementation of a heat warning system.

Reviewer 3

Comment 1: A central tenet of the research question is not clearly stated in the manuscript.

Reply: The sentence was changed to explicitly state that climate change is expected to increase the frequency of hot days, which are known to be associated with increased respiratory disease.

Comment 2: The language is not sufficiently specific re: whether authors propose the link between climate change, extreme heat, and respiratory hospital admissions (RHAs), the health outcome measure quantified in Results & Tables 1-2, is via rising air pollution concentrations.

Reply: This concern has been addressed by describing the modelling procedure in the PHEWE Study (Michelozzi et al) where a sensitivity analysis shows that the inclusion of air pollution in the models has a small or no effect on the estimated risk ratio coefficient for temperature, which would make it reasonable that the increase would be attributable to heat.

VERSION 2 – REVIEW

REVIEWER	Kristen Malecki Assistant Professor University of Madison, School of Medicine and Public Health United States I have no conflict of interest or competing interests.
REVIEW RETURNED	04-Dec-2012

THE STUDY	The HYDE data provide the foundation for population risk estimates and while limitations regarding the ability to adjust for age were recognized, some understanding of the selection of these data for use as the population denominator data- beyond the fact that they were scaled to grid coordinates corresponding to the climate data is still needed. Similarly, reference for the RHA estimates were provided and stated as being derived from the WHO European Health for All Database, however, no information on how this database ascertains cases was described or mentioned in the text, more detail, even a sentence or two on case-ascertainment and coverage of this dataset would be helpful to understand any potential for bias in using these data as opposed to others mentioned by authors in the response to reviewers (Eurostat). In their reply to "reviewer 2" authors indicated that this was the data they were familiar with and it used ICD-9 codes as in PHEWE compared to EUROSTAT which uses ICD-10 codes but this information was not included in the body of the text, nor was the benefit of using the select ICD-9 codes described. This is described within the PHEWE paper but has relevance for this analysis and should also be included/repeated in this paper because it has potentially important implications regarding the validity of future projections.
RESULTS & CONCLUSIONS	There are no confidence intervals in Table 2 or measures of significance- given the uncertainty surrounding most climate projections, the range of anticipated effects or some representation of statistical significance would be helpful to include in this table, in addition - it is hard to determine both from the text as well as the table which climate models were run under which green house gas

	<p>scenarios (e.g. is the HadCM3 model run with A2 greenhouse gas emission scenario?) Slight revisions to table 2 would help the reader.</p> <p>There is very little discussion on why the estimates for the high green house gas emission scenarios were not higher than the middle of the road estimates for number of heat days and this was not mentioned and/or addressed at all as part of the discussion.</p> <p>Line 50, pg 12 of 36 of the pdf refers to table 1 but I think the authors meant to refer to Table 2.</p>
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VERSION 2 – AUTHOR RESPONSE

The HYDE data provide the foundation for population risk estimates and while limitations regarding the ability to adjust for age were recognized, some understanding of the selection of these data for use as the population denominator data- beyond the fact that they were scaled to grid coordinates corresponding to the climate data is still needed.

--We understand the reviewers comment. However the reviewer did not suggest any better data. The need to have a fine spatial resolution of the population data is now mentioned in the manuscript. The only other dataset, to our knowledge, that might have similar spatial resolution is the Eurostat data. The Eurostat population data on the NUTS-3 level does only separate people over 65 while the NUTS-2 regions has 5-year intervals. The NUTS-2 regions are however too large to be used in this assessment and using the NUTS-3 data would not improve the study much since the age stratification would not match the strata in the PHEWE study.

Similarly, reference for the RHA estimates were provided and stated as being derived from the WHO European Health for All Database, however, no information on how this database ascertains cases was described or mentioned in the text, more detail, even a sentence or two on case-ascertainment and coverage of this dataset would be helpful to understand any potential for bias in using these data as opposed to others mentioned by authors in the response to reviewers (Eurostat).

In their reply to "reviewer 2" authors indicated that this was the data they were familiar with and it used ICD-9 codes as in PHEWE compared to EUROSTAT which uses ICD-10 codes but this information was not included in the body of the text, nor was the benefit of using the select ICD-9 codes described. This is described within the PHEWE paper but has relevance for this analysis and should also be included/repeated in this paper because it has potentially important implications regarding the validity of future projections.

--An explanatory text regarding the choice of health data is added in the manuscript.

There are no confidence intervals in Table 2 or measures of significance- given the uncertainty surrounding most climate projections, the range of anticipated effects or some representation of statistical significance would be helpful to include in this table, in addition - it is hard to determine both from the text as well as the table which climate models were run under which greenhouse gas scenarios (e.g. is the HadCM3 model run with A2 greenhouse gas emission scenario?) Slight revisions to table 2 would help the reader.

--The intervals in table 1 on attribution are based on the highest and lowest national mean estimate in each of the regions and does not display the uncertainty introduced by the different climate change scenarios. The information that such intervals would add is provided in table 2.

--Table 2 is slightly changed to clarify which models were run under which climate change scenario.

--There is very little discussion on why the estimates for the high greenhouse gas emission scenarios were not higher than the middle of the road estimates for number of heat days and this was not mentioned and/or addressed at all as part of the discussion.

--This is now discussed in the manuscript.

Line 50, pg 12 of 36 of the pdf refers to table 1 but I think the authors meant to refer to Table 2.

--Correct, changed

VERSION 3 - REVIEW

REVIEWER	Malecki, Kristen University of Wisconsin
REVIEW RETURNED	21-Dec-2012

- The reviewer completed the checklist but made no further comments.