

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Health effects following the Eyjafjallajökull volcanic eruption – a population-based study
AUTHORS	Carlsen, Hanne Krage ; Hauksdóttir, Arna; Valdimarsdttir, Unnur; Gislason, Thorarinn; Einardsóttir, Gunnlaug; Runólfsson, Halldór; Briem, Haraldur; Finnbjörnsdóttir, Ragnhildur; Guðmundsson, Sigurður; Kolbeinsson, Thórir; Thorsteinsson, Throstur; Pétursdóttir, Guðrún

VERSION 1 - REVIEW

REVIEWER	Bernadette M. Longo, PhD Associate Professor University of Nevada - Reno, USA I have no competing interests to declare.
REVIEW RETURNED	11-Aug-2012

THE STUDY	<p>This is a follow-up study on a population exposed to ash and particle pollution following a volcanic eruption.</p> <p>This is a cross-sectional prevalence survey with exposed and unexposed groups. A cohort design would suggest a temporal study (prospective or retrospective) and use of incidence rates. This study measured prevalence of self-reported physical symptoms and diseases, and a measure of mental health. The objective states determining rates (these are typical of a temporal measurement, i.e. incidence), however, prevalence is not a time-based rate. The exposure was measured during the eruption - this study lacks reporting of measurements during the symptom data collection time period of winter. This is of concern since the authors state the ash can still be "resuspended." Are there any environmental measurements for the timeframe when the survey occurred - even to confirm non-exposure? Any anthropogenic fine particle air pollution?</p> <p>This study is a 6-9 month follow-up to initial in depth work with clinical assessment after the eruption. It is not clear why both exposed & unexposed groups were not sampled at the same time since the method was a mailed or internet based survey.</p> <p>The odds ratio was selected to be the measure of effect likely due to the use of regression technique for adjusting confounding. However, the authors refer to "risk" continuously throughout the manuscript. This may be misleading to some readers. Odds are different than rate ratios (classically used for risk). Use of "association" at times may be more appropriate for this prevalence study of self-reported symptoms (not medically diagnosed diseases). In addition, to add to the statistical description - were these data initially stratified and tested for effect modification? What was used for trend analyses?</p>
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	<p>No mention of crude versus adjusted ORs is present in the manuscript. Were the variables for adjusted ORs a priori selected? Strengths of this study include: large sample size, very good response rate, use of standardized questions, and sub-grouping in the exposed area.</p> <p>Improvement is needed in the references for this new area of medical geology. Consider inclusion of work on similar populations at volcanoes with ash fall: Mount Ruapehu (Hickling et al., 1999); Mt. Sakurajima; and the Soufriere Hills. The work at Mt. St. Helens was the first of these kinds of studies, much has been learned since that eruption. Explain to the readers what new knowledge is gleaned by your study.</p> <p>Lastly, there was minimal introduction and discussion on the mental health of the exposed population - referred to as "psychological morbidity." What exactly did this consist of? Anxiety, PTSD, mood disorders? This was one of the higher ORs, yet insufficiently mentioned.</p>
RESULTS & CONCLUSIONS	<p>The message should be more concise. Tell the readers what "new knowledge" has been learned on population health and risks to the millions of people who live near active volcanoes.</p> <p>I would include mention of the current public health implications for the exposed population and what efforts are recommended for physical and mental health prevention (across primary, secondary & tertiary levels). As seen in your high participation for your study, the population is likely eager to learn and improve their health!</p> <p>These authors and Iceland have an opportunity to be the first to prospectively follow a population post eruption and learn a tremendous amount of knowledge on human and environmental interaction and health implications. It is important that sound epidemiology is followed to allow increasing support for the hypothesis.</p>

REVIEWER	<p>Peter Baxter MD FRCP Research Fellow University of Cambridge, UK</p> <p>I have no competing interests</p>
REVIEW RETURNED	29-Aug-2012

RESULTS & CONCLUSIONS	<p>The authors need to qualify their conclusions as their results also show, e.g., a small gradient in the percentage of current smokers in the low to high exposure groups; the exposed area is a heavy farming area but we are not told about the control area occupations - and the farmers in the exposed area may do more arable farming which places them outside and doing a lot ploughing, etc, grain harvesting, etc., which will expose them to dusts other than volcanic ash (mentioned briefly on page 13). In addition, the dust from the glacier floods is a serious air pollution issue in the exposed area (mentioned) and has not been adequately researched to my knowledge, and this exposure during dry periods may be sufficient to cause the problems found to be significant. The responders in the exposed areas may have been biased by being in the ash fall area - indeed the previous survey may even have helped to increase the subjects' bias, including the psychological responses. We don't know how much the farmers, etc., have lost money because of the</p>
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	<p>eruption and some may have lost their farms or converted them to vacation sites, and become depressed for those reasons and also keen to add weight to their feelings. We don't have enough information in this questionnaire survey (such as occupations) to conclude that the findings are due to volcanic ash, but it is a reasonable hypothesis, and there is a dose- response relationship for some respiratory question responses to back it up.</p> <p>I agree there is enough suspicion to warrant recommending follow up studies at this eruption, and they should include lung function studies. We do not know if we can confidently reassure the population that the heavy respirable particle levels during and since the eruption could have been sufficient to trigger long term airway problems, e.g., making asthma worse or even contributing to COPD later on in life in a small but significant number of people (see also their reference 26). There is a dearth of information in this field which is of growing medical interest and long-term studies are needed to get to the bottom of the questions this work raises. Iceland provides a unique opportunity for this work given their record systems - other countries with frequent volcanic eruptions are not able to do it for cost and infrastructure reasons being in low resource regions - and the information would help develop mitigation measures at future eruptions.</p> <p>Specific comments:</p> <p>Page 4, line 30-32. What acids and when you say small enough what do you mean, e.g., respirable or thoracic? The grain size range is in Ref 2. (Horwell et al.) of the previous paper by Carlsion et al.</p> <p>Page 5, lines 6-8. It's NOT known if "long term" exposure to SO₂ and H₂S causes chronic bronchitis and cardio-respiratory symptoms. References 10 and 11 are hardly authoritative and in fact studies of workers in industry are few on this subject. Perhaps refer to WHO guideline documents for SO₂ and sulphate aerosol in non-volcanic air pollution, the latter as particulate may have a role as suggested. Long term? How long term? Gas flows – when have they occurred on Iceland? They are rare in volcanic areas and there are no studies of their effects on mental health....</p> <p>Page 8. Venn diagram – this does not seem to be mentioned in the text again and it is not clear what it is meant to show. The diagram would benefit from percentages being added.</p> <p>Page 13, line 17. Who in the lowlands has been exposed to lava flows on Iceland recently? Heimay 1973?</p> <p>Page 13, line 50. Mount St Helens eruption –increases in ER visits lasted 2 weeks (not months as in this paper) post eruption and a major proportion of these were in people with a previous diagnosis of asthma or bronchitis. Should be reference 24, not 25. See, in addition, Baxter et al., 1983, Arch. Environ Health 38: 138-143.</p> <p>Page 14, line 6-18. These animal studies were looking at silicosis because of the raised crystalline silica content of the ash at Mount St Helens, not respiratory symptoms or acute inflammation. So this material is not relevant here.</p>
REPORTING & ETHICS	Formal ethics committee approval not mentioned in text

VERSION 1 – AUTHOR RESPONSE

Reviewer: Bernadette M. Longo, PhD
Associate Professor
University of Nevada - Reno, USA

I have no competing interests to declare.

This is a follow-up study on a population exposed to ash and particle pollution following a volcanic eruption.

1. This is a cross-sectional prevalence survey with exposed and unexposed groups. A cohort design would suggest a temporal study (prospective or retrospective) and use of incidence rates.

Response: The personal identification numbers assigned to every individual at birth or immigration and the population registries in Iceland allow us to identify a complete study base with virtually no loss to follow-up. Using these resources we identified two cohorts of individuals living in the exposed and non-exposed areas at the time the volcanic eruption started; these were then contacted 6-9 months later and asked about prevalent symptoms. Thus, we are conducting a cohort study but, as the reviewer points out, we have only assessments of prevalent symptoms. We have made changes throughout the manuscript to highlight this point.

2. This study measured prevalence of self-reported physical symptoms and diseases, and a measure of mental health. The objective states determining rates (these are typical of a temporal measurement, i.e. incidence), however, prevalence is not a time-based rate.

Response: Thank you for pointing this out, we have altered "rates" to "prevalence" throughout the manuscript.

3. The exposure was measured during the eruption - this study lacks reporting of measurements during the symptom data collection time period of winter. This is of concern since the authors state the ash can still be "resuspended." Are there any environmental measurements for the timeframe when the survey occurred - even to confirm non-exposure?

a. Any anthropogenic fine particle air pollution?

Response: The initial exposure during the volcanic eruption was followed by intermittent but frequent exposures due to resuspension of the ash. Thus, the exposure lasted from the onset of the eruption until the time the survey occurred. Air quality was monitored throughout the period. We obtained this information from the Environment Agency of Iceland who has written an internal report on these measurements. We have added this information and a reference to the report (methods: p. 8). The anthropogenic sources of pollution are negligible, but have been mentioned in the methods chapter, page 7).

4. This study is a 6-9 month follow-up to initial in depth work with clinical assessment after the eruption. It is not clear why both exposed & unexposed groups were not sampled at the same time since the method was a mailed or internet based survey.

Response: This was simply due to logistic reasons and the capacity of our staff. We sent the questionnaires to the exposed group between November 19th and December 28th 2010, and to the unexposed group from January 26th to February 4th 2011. The winter in Iceland is if anything harsher in the weeks following Christmas. According to the chief epidemiologist, the influenza outbreaks during this winter occurred between December and March, peaking in late February. We have added a reference about this, and mention it in the discussion (p16).

5. The odds ratio was selected to be the measure of effect likely due to the use of regression technique for adjusting confounding. However, the authors refer to "risk" continuously throughout the manuscript. This may be misleading to some readers. Odds are different than rate ratios (classically used for risk). Use of "association" at times may be more appropriate for this prevalence study of self-reported symptoms (not medically diagnosed diseases).

Response: Thank you for pointing this out. We have changed the wording accordingly.

6. In addition, to add to the statistical description - were these data initially stratified and tested for effect modification?

a. What was used for trend analyses?

b. No mention of crude versus adjusted ORs is present in the manuscript.

Response:

We chose to present the crude percentages of symptoms-sufferers for each exposure group, as this is an easily understandable measure along with adjusted odds ratios. Our models are only adjusted for age, gender, education and smoking status and do not divert largely from crude estimates. We have added a sentence clarifying this in the results section (page 12). We have not conducted a trend analysis as the aim is not to test or reveal a causal association but to maintain a conservative style both in statistical analyses and interpretation. Nevertheless, we are certainly willing to reconsider our position on the editor's request.

c) Were the variables for adjusted ORs a priori selected?

Response: Yes, the variables were selected a priori, we have now added this information in the method section (see Methods, database and coding, page 10).

7. Strengths of this study include: large sample size, very good response rate, use of standardized questions, and sub-grouping in the exposed area.

Response: Thank you!

8. Improvement is needed in the references for this new area of medical geology. Consider inclusion of work on similar populations at volcanoes with ash fall: Mount Ruapehu (Hickling et al., 1999); Mt. Sakurajima; and the Soufriere Hills. The work at Mt. St. Helens was the first of these kinds of studies, much has been learned since that eruption. Explain to the readers what new knowledge is gleaned by your study.

Response: Thank you, we had cut down the list of references at one point, but will add these as suggested.

9. Lastly, there was minimal introduction and discussion on the mental health of the exposed population - referred to as "psychological morbidity." What exactly did this consist of? Anxiety, PTSD, mood disorders? This was one of the higher ORs, yet insufficiently mentioned.

Response:

While the paper focuses on physical symptoms (respiratory etc.), one of our findings concerns psychological morbidity as measured by The General Health Questionnaire (GHQ). As now described in the methods section, GHQ is a "non-specific screening tool for psychological morbidity which measures anxiety, loss of self-confidence and social dysfunction" (Hankins et al, 2008). This tool was developed to screen for psychological morbidity and is not intended for diagnosing specific psychiatric problems. We have now added more information on GHQ in the methods section (see questionnaire

section, paragraph, page 10).

In the study we also included other measurements of psychological morbidity (stress, PTSD), which are being analyzed and await further investigations.

10. The message should be more concise. Tell the readers what "new knowledge" has been learned on population health and risks to the millions of people who live near active volcanoes.

Response: We have now made some alteration to make the message clearer, while sticking firmly to our data.

11. I would include mention of the current public health implications for the exposed population and what efforts are recommended for physical and mental health prevention (across primary, secondary & tertiary levels).

Response: We have changed the wording to emphasize the lessons learnt, in particular concerning the services and care that needs to be provided to the population. This paper describes results from our quantitative study; clinical/social public health recommendations would be the subject of another paper.

12. These authors and Iceland have an opportunity to be the first to prospectively follow a population post eruption and learn a tremendous amount of knowledge on human and environmental interaction and health implications. It is important that sound epidemiology is followed to allow increasing support for the hypothesis.

Response: Thank you, this is precisely the reason why we have embarked on the study, with a long-term perspective in mind.

Reviewer: Peter Baxter MD FRCP
Research Fellow
University of Cambridge, UK

I have no competing interests

1. The authors need to qualify their conclusions as their results also show e.g., a small gradient in the percentage of current smokers in the low to high exposure groups; the exposed area is a heavy farming area but we are not told about the control area occupations - and the farmers in the exposed area may do more arable farming which places them outside and doing a lot of ploughing, etc, grain harvesting, etc., which will expose them to dusts other than volcanic ash (mentioned briefly on page 13).

Response:

Thank you for a good point. The gradient in percentage of smokers is non-significant (multinomial logistic regression and row by column Chi²), so we disregarded it.

The farming in the two areas, the exposed and non-exposed, is to a large extent sheep/dairy farming, with the exception of a few farms in the exposed region, where barley and canola are cultivated, almost on an experimental scale. Haymaking and grain production both require ploughing and harvesting, exposing the workers to dust. A study on Icelandic farmers and a control group sampled from the national registry found no difference in respiratory symptoms among farmers and controls or among farmers in different regions, we have added a reference to this study.

The fact that our data on chronic illnesses are comparable between the two areas, further supports that the working environment is similar with respect to respiratory health.

We have added comments about this on pages 14 and 15(discussion section).

2. In addition, the dust from the glacier floods is a serious air pollution issue in the exposed area (mentioned) and has not been adequately researched to my knowledge, and this exposure during dry periods may be sufficient to cause the problems found to be significant.

Response:

In the Discussion we mention the lack of information about the health effects of the normal air quality in South Iceland, which often is subject to severe dust storms. However, we argue that the dose-response character of symptoms with respect to distance from the volcano suggests that the symptoms are associated with the eruption but not “regular” South Iceland dust storms.

3. The responders in the exposed areas may have been biased by being in the ash fall area – indeed the previous survey may even have helped to increase the subjects’ bias, including the psychological responses.

Response: We understand the reviewer’s concern that having taken part in a study some months before could affect the answers in the current study. The number of participants in the previous study (spring 2010) was 207 while 1148 in took part in the fall of 2010. Given that, and the 6-month time span in between, we feel that it is highly unlikely that participating in the spring-study would explain our results in the fall study. Regarding concerns that being in the ash fall area would affect psychological responses, we agree, and it was indeed one of the aims of the study to ascertain whether the exposed group was more likely to report psychological symptoms following this unusual experience.

4. We don’t know how much the farmers, etc., have lost money because of the eruption and some may have lost their farms or converted them to vacation sites, and become depressed for those reasons and also keen to add weight to their feelings.

Response: We agree that loss of money, damages etc. may indeed be a part of the explanation (eruption->damages->psychological morbidity) and this should be addressed in future studies. We have added a comment on this at the end of the discussion (page 17).

5. We don’t have enough information in this questionnaire survey (such as occupations) to conclude that the findings are due to volcanic ash, but it is a reasonable hypothesis, and there is a dose-response relationship for some respiratory question responses to back it up.

Response: We agree, and have taken care to interpret our findings with care. We have rephrased our conclusions on p.17.

6. I agree there is enough suspicion to warrant recommending follow up studies at this eruption, and they should include lung function studies. We do not know if we can confidently reassure the population that the heavy respirable particle levels during and since the eruption could have been sufficient to trigger long term airway problems, e.g., making asthma worse or even contributing to COPD later on in life in a small but significant number of people (see also their reference 26).

7. There is a dearth of information in this field which is of growing medical interest and long-term studies are needed to get to the bottom of the questions this work raises. Iceland provides a unique opportunity for this work given their record systems - other countries with frequent volcanic eruptions are not able to do it for cost and

infrastructure reasons being in low resource regions - and the information would help develop mitigation measures at future eruptions.

Response: We thank the reviewer for encouraging remarks on our study and hope to be able to follow up these findings.

Specific comments:

1. Page 4, line 30-32. What acids

Response: Those were salts. We changed it in the text (see page 6).

and when you say small enough what do you mean, e.g., respirable or thoracic? The grain size range is in Ref 2. (Horwell et al) of the previous paper by Carslon et al.

Response: We have made an effort to clarify the grain size in the text (see page 6).

2. Page 5, lines 6-8. It's NOT known if "long term" exposure to SO₂ and H₂S causes chronic bronchitis and cardio-respiratory symptoms. References 10 and 11 are hardly authoritative and in fact studies of workers in industry are few on this subject. Perhaps refer to WHO guideline documents for SO₂ and sulphate aerosol in non-volcanic air pollution, the latter as particulate may have a role as suggested.

Response: We have modified the sentence to reflect that this has been found in the two studies mentioned.

3. Long term? How long term? Gas flows – when have they occurred on Iceland? They are rare in volcanic areas and there are no studies of their effects on mental health....

Response: We have now adjusted the text according to the reviewer's comment, referring to psychological morbidity as a general term instead (see Introduction page 6).

4. Page 8. Venn diagram – this does not seem to be mentioned in the text again and it is not clear what it is meant to show. The diagram would benefit from percentages being added.

Response: It is mentioned on page 13 as figure 2; we have also added a sentence in the results section (page 12). We have changed the figure, so that the percentages are shown rather than the crude numbers.

5. Page 13, line 17. Who in the lowlands has been exposed to lava flows on Iceland recently? Heimaey 1973?

Response: We have deleted the word "lava" from the text (see introduction, page 7).

6. Page 13, line 50. Mount St Helens eruption –increases in ER visits lasted 2 weeks (not months as in this paper) post eruption and a major proportion of these were in people with a previous diagnosis of asthma or bronchitis. Should be reference 24, not 25. See, in addition, Baxter et al., 1983, Arch. Environ Health 38: 138-143.

Response: Thank you for pointing this out. This has been corrected.

7. Page 14, line 6-18. These animal studies were looking at silicosis because of the raised crystalline silica content of the ash at Mount St Helens, not respiratory symptoms or acute inflammation. So this material is not relevant here.

Response: Thank you, this part has been deleted.

8. Formal ethics committee approval not mentioned in text.

Response: This has been added to the methods chapter (see, page 11).

VERSION 2 – REVIEW

REVIEWER	Bernadette Mae Longo, Ph.D Associate Professor Division of Health Sciences University of Nevada-Reno, USA I have no competing interests to declare.
REVIEW RETURNED	26-Sep-2012

THE STUDY	Under Objectives - please correct "rates" to "prevalence" Otherwise the Abstract is accurate. English: Please correct page 6, sentence 42. Do not start the sentence with a number%.
GENERAL COMMENTS	A very nice study and contribution! All suggested revisions were well addressed by the authors. Please see above for corrections to the manuscript in two areas. I would suggest the bioethics approval sentence be mentioned in the previous section on data collection.