

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Sensor Recorded Changes in Rates of Hand Washing with Soap in Response to the Media Reports of the H1N1 Pandemic in Britain
<b>AUTHORS</b>	Diana S Fleischman, Gregory D Webster, Gaby Judah, Mícheál de Barra, Robert Aunger Valerie Curtis

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Senior Research Scientist Dr Shona Hilton MRC Social & Public Health Sciences Uni 4 Lilybank Garden Glasgow, G12 8RZ Tel: 0141 357 753 Fax: 0141 337 2389
<b>REVIEW RETURNED</b>	07/04/2011

<b>THE STUDY</b>	<p>Intro P5 line 39 to 44. Need to reference this paragraph with studies that have analysed newspaper/ media coverage (eg Hilton 2010 and Rubin 2009)</p> <p>Line 44 title should be bold</p> <p>Methods Specify all the key words. The search terms described seems quite a crude method of analysing newspaper coverage- are there any weaknesses in using this method over doing a more throughout analysis of newspaper content.</p> <p>Other key refs</p> <p>Duncan B. How the media reported the first days of the pandemic (H1N1) 2009: Results of EU-wide media analysis. Vol 14: Eurosurveillance; 2009:1-3</p> <p>Hilton S, Hunt K. UK newspapers representations of the 2009-2010 outbreak of swine flu: one health scare not over-hyped by the media? [Epub ahead of print]. Journal of Epidemiology &amp; Community Health 2010.</p> <p>Hilton S, Smith E. Public views of the UK media and government</p>
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	reaction to the 2009 swine flu pandemic. BMC Public Health 2010; 10:697.
<b>RESULTS &amp; CONCLUSIONS</b>	<p>Discussion</p> <p>I think the findings need to compare with those of other findings that examine public views of the swine flu threat. How does your findings compare with Rubin's and Hilton's studies which both found people did not feel anxious and did not think they had changed their hand washing. Given that swine flu risk perception of risk was greatest over the spring and summer, and became less as people became more familiar with swine flu through direct and indirect contact and given that many people thought swine flu was not likely to cause them to any real harm how does this relate to your findings – you need to discuss this more.</p> <p>You do not mention the impact of the increased use of anti bacterial hand sprays and lotions had on hand hygiene.</p>
<b>GENERAL COMMENTS</b>	None.

<b>REVIEWER</b>	Professor Lynn B. Myers, Department of Psychology, Brunel University Uxbridge, Middlesex, UB8 3PH
<b>REVIEW RETURNED</b>	11/04/2011

<b>THE STUDY</b>	<p>There is no actual research question, just an opportunistic investigation of hand washing during pandemic flu outbreak. There is no measurements of hand washing before the pandemic, which is a major limitation.</p> <p>A leaflet was distributed to every household in the UK during May 2009 "Important information about swine flu." what would have been useful is to compare hand washing rates before and after the leaflet was distributed.</p> <p>Apart from measuring hand washing in men's and women's toilets in a service station there were no other measures. Consequently, there were difficulties in interpreting the findings. For example, women's higher use of hand washing compared to men's - this could be a spurious finding. It could be that children go with their mothers/female caregivers into the toilets more than with male caregivers into men's toilets and they are more likely to wash their hands.</p> <p>The standard of English is often poor For example, totally objective, notoriously poor, millions of data points, keenly aware etc. There are far too many "we's". For example, throughout the result section. The term rest room is not used in the UK. Typing errors e.g. diarrhea. Sometimes not clear e.g.: (a) time can be confounded with either the independent or dependent variables. The authors need to explain what this means and why.</p>
<b>RESULTS &amp; CONCLUSIONS</b>	Much of this is described in previous section. These are the limitations of the research question, the presentation of results which are difficult to follow and gender differences could be due to the limitation of design of study.
<b>REPORTING &amp; ETHICS</b>	There is concern that the authors have not discussed any ethical issues. It appears that they have not gained ethical approval, although this could be considered an audit. However, this is not

	discussed. They could have asked another group of people how they would feel about having their hand washing practices anonymously recorded.
<b>GENERAL COMMENTS</b>	None

### VERSION 1 – AUTHOR RESPONSE

Thank you for the opportunity to revise and improve our manuscript. The following is a list of changes that have been made to the manuscript in line with the recommendations of Reviewer 1 and Reviewer 2

- The formatting changes suggested by the reviewers have been changed and the English has been corrected and improved throughout the manuscript
  - Other key references suggested by the reviewers have been added where appropriate.
  - Weaknesses in the keyword method used have been addressed in the discussion as well as the limitations involved in the fact that we did not begin measuring hand washing until after the pandemic began
  - Some information about how our results compare to self-reported anxiety and intention to hand wash have been added in the discussion.
  - The interpretation of Reviewer 1 about children going into the women's rather than the men's toilets influencing hand washing has been included.
  - The confusing language regarding how time could be confounded with the dependent or independent variables has been clarified with an example
  - A paragraph about the ethics of the study is included
- We hope that the changes made address the concerns of the reviewers.

### VERSION 2 - REVIEW

<b>REVIEWER</b>	Eric Lau School of Public Health, The University of Hong Kong University  I have no competing interests
<b>REVIEW RETURNED</b>	27/06/2011

<b>THE STUDY</b>	Data collected from motorway service station may not be representative of normal washing behaviour
<b>GENERAL COMMENTS</b>	Please give reason of the missing data of two months in women.  Partial correlation adjusted for calendar day was used in the analysis. While the authors attempted to adjust for the seasonal trend, adjustment by day can only remove linear trend but not seasonal trend in general, such as having a peak level of handwashing during summer. The case number of H1N1 may also be an important confounder which should be addressed in the discussion.

<b>REVIEWER</b>	Anne Presanis Statistician Medical Research Council Biostatistics Unit Cambridge UK  I declare no competing interests.
<b>REVIEW RETURNED</b>	27/06/2011

## THE STUDY

This paper attempts to answer the question of whether hand washing behaviour changed during the period of the 2009 pandemic H1N1/A influenza outbreak. While the research question is interesting, I am not convinced the study design and statistical methods employed are appropriate and adequate for answering the question.

The authors have conducted an observational study, measuring daily the ratio of uses of soap to entries to toilets in one motorway service station, from May 21st 2009 to January 4th 2010. In the same time period, they have also collected counts of news articles and blog posts mentioning the keywords "H1N1" or "swine flu". Given the nature of the study, very little information is available on the population observed, other than sex, determined by entry to either the men's or women's toilets. There is therefore no way of understanding whether the observed population is at all representative of the population of the country as a whole. The statistical methods employed to analyse the relationship between handwashing rates and media coverage are purely descriptive, looking only at the correlation between the two.

Specific comments:

1. The authors look at two correlation coefficients, the Pearson (zero-order) correlation coefficient and the partial correlation. As the authors acknowledge and explain well, it is important to control for time, as it is likely to be a confounder for both the handwashing rates and media coverage. The use of the partial correlation controlled for time is therefore appropriate. However, I'm not sure why the authors report the Pearson correlation at all, since time is clearly a confounder.

2. The authors' reporting of the correlation coefficients in Table 1 is unclear, for several reasons:

a) It would be easier to read if the row header contained the same headings as the column header, rather than the numbers, i.e. "Wash, Men, Women, etc."

b) More importantly, neither in the caption nor in the methods section do the authors define the hypothesis test for which they report the p-values. The statement "All correlations are significant at the  $p < .05$  level (two-tailed)" is meaningless without defining the null hypothesis – are they testing the null hypothesis that there is 0 correlation? If that is the case, how have they carried out the hypothesis test? Via a permutation test? bootstrapping? a conditional independence test in the case of the partial correlations?

c) Do "Ns = 209" and "ns = 154" refer to the sample sizes? The number of days for which the handwashing rate is available? If so, why is there a period of time (August-October 2009) for which the data for the women's toilets are missing (seen in Figures 3 and 4 also)? Have the authors considered that this missing data may seriously bias the measured correlations, if for example handwashing rates in the period with missing data were not correlated with media coverage? Although the authors mention in the results section comparing the strength of correlations for men and women on days where the data exist for both, it is not clear whether the correlations reported in the table are based on the whole time period or only on days where the data exist for both men and women.

	<p>d)The authors do not explain anywhere what the difference is between the “predicted correlations” (boldfaced in the table) and the remaining correlations.</p>
<b>RESULTS &amp; CONCLUSIONS</b>	<p>3. Further to the comments in point 2, I am not convinced by the authors' interpretation of the partial correlations. Although the partial correlations between handwashing rates and media coverage are significantly different from 0 (if that is indeed the hypothesis test carried out), the largest of these, between women's handwashing rates and blog posts, is only 0.56, which is not a particularly strong correlation. I am not sure the measured correlation justifies describing handwashing rates as “significantly related to H1N1 media coverage” (line 15, page 10). Nor is the description “the strong relationship between blogs and handwashing for women” (line 51, page 12) justified by the reported correlation coefficient.</p> <p>4.Although the authors mention some of the limitations of their study in the discussion, I am not persuaded that they have either designed the study well or analysed the data appropriately. The missing data in a large portion of the time under study for women could in theory have a big effect on the reported correlations. As the authors themselves acknowledge, correlation does not imply causation, and a big limitation in answering the research question is that the study began a month after the pandemic itself began. Have the authors considered that time series analyses may have been more appropriate than merely looking descriptively at correlations? There is a large literature on time series analyses accounting for time-varying covariates, e.g. Hay &amp; Pettitt, Biostatistics, 2001.</p>
<b>GENERAL COMMENTS</b>	<p>Minor comments:</p> <p>1.A number of the papers referenced in the main body of the text do not appear in the list of references at the end, e.g. Hilton &amp; Hunt 2010.</p> <p>2.Page 6, line 6: “A longitudinal conducted” - should this be “A longitudinal study conducted”?</p> <p>3.In general, in the introductory section on previous literature, page 6, the reporting of results from the literature is poor from a statistical point of view, with percentages given but no indication of sample sizes.</p> <p>4.Page 8, line 5: The phrase “the proportion of entries to soap usage” is unclear – do the authors mean “the proportion of entries who used soap” or “the ratio of entries to soap usage”?</p> <p>5.Page 9, start of the Results section: The description of the outcome measures is unclear. In forming the “single daily index of hand-washing”, did the authors average the rates in the men's and women's toilets by weighting by the “population size”, i.e. the number of entries to each? In which case the single daily index is simply the overall number of soap uses / overall number of entries. Or did the authors average the men's and the women's rates by summing these and dividing by 2? In which case the authors are implicitly assuming an equal number of entries to the men's and the women's toilets. During the period where data are missing for the women's toilets, do the authors use only the men's data to form the “single daily index”? Also “hand-washing indexes” should be “hand-washing indices”.</p> <p>6.Page 13, start of last paragraph: “The data in this study are correlation” - this is unclear, correlations are statistics, not data.</p>

**VERSION 2 – AUTHOR RESPONSE**

Dear BMJ open

Thank you for the opportunity to revise and further improve our manuscript. The revision is uploaded including tracked changes. What follows is a list of revisions made and explanations of some of our statistical design and techniques.

Responses to specific comments and questions

- While the authors attempted to adjust for the seasonal trend, adjustment by day can only remove linear trend but not seasonal trend in general, such as having a peak level of handwashing during summer.

o We could not control for seasonal trends because we had less a year of data. Controlling for such temporal trends demands that one have multiple cycles of the time scale in question. This is why we could -- and did -- control for weekly trends but not season trends. For example, if one had 45 minutes of continuous data recorded at the level of seconds, one could look at or control for minute trends, but not hourly trends

- "I'm not sure why the authors report the Pearson correlation at all, since time is clearly a confounder."

o We agree that the partial correlations are likely more accurate than the zero-order correlations, and we have stressed this fact in our manuscript; however, we have chosen to present both types of correlations (zero-order and partial) side-by-side for at least two reasons. First, presenting one type of correlation and not the other opens the article to unnecessary criticisms from interested readers. Specifically, if we presented only the partial correlations, readers might falsely accuse us of "hiding" the "pure" zero-order correlations, presumably because they're non-significant (which they're not); we can preempt such criticism by presenting the complete picture (i.e., both sets of correlations). Second, such transparency allows readers to actively compare the two types of correlations and make up their own minds regarding where the partial correlations are appropriate.

- "Why is there a period of time (August-October 2009) for which the data for the womens' toilets are missing?"

o Missing data is a result of the door sensors not functioning to record entry. Without the door sensor we still had soap usage but did not know how many people used the toilets and thus could not produce a proportion of soap usage. This description is now included.

- Have the authors considered that this missing data may seriously bias the measured correlations....?

o Yes, we have considered how gaps in data collection may impact our findings and this is addressed in the discussion section

- "I am not convinced by the authors' interpretation of the partial correlations.... I am not sure the measured correlation justifies describing handwashing rates as "significantly related to H1N1 media coverage" (line 15, page 10). Nor is the description "the strong relationship between blogs and handwashing for women" (line 51, page 12) justified by the reported correlation coefficient.

o The measured correlation is significantly different from zero so we are justified in describing handwashing rates as significantly related to H1N1 media coverage. The description of the correlation as "strong" is standard and based on Cohen (Cohen, J. (1988). Statistical power analysis for the behavioral sciences (2nd ed.). New Jersey: Lawrence Erlbaum) who describes correlations of .5-.7 as "large" and is often interpreted in behavioural science literature as "strong".

- Have the authors considered that time series analyses may have been more appropriate than

merely looking descriptively at correlations?

o We agree and would have preferred to use time-series analytic techniques; however, because time-series analyses typically assume a continuous stream of data collected at regular time intervals without any gaps, and because we had some missing data due to technical errors, this crucial assumption could not be met. Consequently, we chose more traditional statistical technique—correlation and partial correlation controlling for time—that is generally more robust given some missing data.

We hope that you find the manuscript improved.

Thank you,

Diana Fleischman

#### VERSION 2 - REVIEW

<b>REVIEWER</b>	Eric Lau Research Assistant Professor The University of Hong Kong Hong Kong  I have no competing interests
<b>REVIEW RETURNED</b>	08/09/2011

The reviewer filled out the checklist only and made no further comments.

## Correction

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Fleischman DS, Webster GD, Judah G, *et al.* Sensor recorded changes in rates of handwashing with soap in response to the media reports of the H1N1 pandemic in Britain. *BMJ Open* 2011;1:e000127.

The Results section of the Abstract should read: “Hand-washing rates were positively related to both H1N1 coverage in blogs and the news; however, these relationships were stronger for women than for men.”

*BMJ Open* 2012;2:e000127corr1. doi:10.1136/bmjopen-2011-000127corr1