Author's response to reviews

Title: Economic Evaluation of the Benefits of Reducing Acute Cardiorespiratory Morbidity Associated with Air Pollution

Authors:

Dr David M Stieb (dave_stieb@hc-sc.gc.ca)
Paul De Civita (pau_decivita@hc-sc.gc.ca)
Reed Johnson (frjohnson@rti.org)
Matthew P Manary (matthewmanary@yahoo.com)
Aslam H Anis (anis@hivnet.ubc.ca)
Robert C Beveridge (drbob_beveridge@bigfoot.com)
Stan Judek (stan_judek@hc-sc.gc.ca)

Version: 2 Date: 29 Nov 2002

PDF covering letter
November 29, 2002

Philippe Grandjean
University of Southern Denmark
David Ozonoff
Boston University School of Public Health
Editors in-chief
Environmental Health: A Global Access Science Source

Dear Drs. Grandjean and Ozonoff

RE: Economic Evaluation of the Benefits of Reducing Acute Cardiorespiratory Morbidity Associated with Air Pollution

Thank you for your prompt attention to our paper, and for the helpful peer reviewer comments. I apologize for the slight delay in returning the revised manuscript. We have detailed our response to the reviewer comments below. Please also find attached the revised manuscript for your consideration.

Sincerely,

David M. Stieb, MD, M Sc, FRCP
Air Health Effects Division, Health Canada, Room 722, 275 Slater St., postal locator 3807B
Ottawa, ON, Canada, K1A 0K9, dave_stieb@hc-sc.gc.ca
(613) 957-3132 (voice); (613) 954-7612 (fax)
Dr. Ulla Slothuus Skjoldborg

1. More information on the data sources.

We have included additional material, as suggested, regarding our assessment of the quality of the data from the primary data sources, on pages 8-10.

2. More discussion of the potential gain from using our approach vs. “standard” methods

As we point out in the background section, there is in fact no standard approach. We have added a phrase in the last paragraph of this section to more clearly contrast the status quo with the particular features of our approach i.e. greater consistency and comprehensiveness.

3. More on the regression model.

It’s not clear what is added by duplicating the regression model statement in the text relative to table 4 or what additional discussion is required. The model specification and results are unambiguously presented in the table and our interpretation of these results is provided in the first paragraph of the results section.

4. More thorough descriptions of the figures.

We have moved descriptive information from the figure into a new table, which makes the figures simpler and provides a clearer explanation of the characteristics of estimates shown in the figures.

5. Figure 3 does not have a heading.

The title for figure 3 appears in the file containing the manuscript text. Figures 1 and 2 have subheadings (eg. asthma symptom days, restricted activity days), because there is more than one outcome presented in these figures, in contrast to figure 3 which describes only 1 outcome: acute respiratory symptom days.

6. Unclear what the purpose is of comparing results to earlier estimates

We have added a sentence on page 17 indicating that we might have expected our estimates to exceed earlier estimates by virtue of our more comprehensive approach. We have also added material which addresses this expectation with reference to the sensitivity of valuation estimates for less severe outcomes to the specified severity, and the probably small incremental difference our approach may have made for outcomes associated with a low valuation. We believe the outcomes involving hospital care represent a special case where differences between national health care systems appear to outweigh the impact of our more comprehensive approach and have made this more explicit in the discussion.
Dr. Jonathan Levy

1. Too many significant figures.

We have rounded all valuation estimates to two significant figures.

2. Provide a clear rationale for why incremental refinement of morbidity (vs. mortality) valuation is important

We have added material acknowledging the dominance of mortality and chronic morbidity effects in benefits assessments in the first paragraph, but point out that from a broader public health perspective, morbidity effects remain important and will continue to be evaluated in these assessments.

3. Need to clarify why the specific health outcomes were selected.

We have clarified in the first paragraph of the methods section that we identified these conditions based on the available evidence.

4. Valuation versus QALY/DALY measures.

We have added material to the third paragraph of the methods section, acknowledging QALYs/DALYs as alternative measures of preferences, but noting particular limitations.

5. Uncertainty related to the differences in impact between susceptible subgroups and the general population.

We have added a sentence acknowledging this issue on page 13.

6. Foreshadowing health care system influences.

We have added a sentence on page 5, flagging the issue of extrapolation of U.S. data to other countries.

7. Indictment of recent literature too broad regarding concerns about statistical analysis.

Based on a recent U.S. EPA workshop on this issue (November 4-6), it appears that all time series studies using GAMs are potentially affected, particularly where there are strong seasonal cycles. However, we have added clarification to the effect that the impact of these problems is still being evaluated.

8. Should be clear that mortality and long-term morbidity effects are omitted.

A statement has been added at the end of this section to emphasize this.
9. Too many significant figures.

See response to point 1.