Reviewer's report

Title: Parameter and model uncertainty in a life-table model for fine particles (PM2.5)

Version: 1 Date: 19 December 2006

Reviewer: Brian Miller

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General:

It is becoming increasingly realised that, in performing a health impact assessment for possible changes in particulate pollution, the effect on mortality is best modelled through life-table calculations. However, life-tables involve prediction into the future, and require many assumptions to be made. The authors are to be commended for an attempt to address the extent to which variations in these assumptions will affect the outputs from the model. It is important that people do these impact assessments, and discuss the uncertainty involved in them. However, this paper will be hard for readers who are not close to the topic to understand, partly because of the language and terminology employed, and partly because mortality effects are always less well understood than many other health effects. It is therefore worth being doubly careful and pedantic about the language used to describe them.

Most of the points made below are about the language used in the text and the extent to which the description of the work might be improved. This implies fairly major work on the text. The work itself appeals to a number of subjective judgments, which others might make differently, but in the main I haven’t considered these, leaving it to the authors to justify their own choices.

Major compulsory revisions:

Introduction

The word “estimate” is used in three different senses in the paper, and they are not clearly distinguished, and are therefore a source of potential confusion. They are
1) Estimation of parameters from data sets e.g. relative risks from cohort studies
2) Application of life table models to quantify changes in mortality patterns in the future
3) Choosing (assuming) the value of an input value for the model.
I’d suggest reserving “estimate” for 1), using “predict” or “project” for 2), and “assume” for 3). Or any set of words that make the distinction clearly.

Para 3 suggests a “problem” in LT methods; but LT methods are the only ones that correctly model change in population age structures following change in hazard rates. That this need to be done carefully is no more a problem here than anywhere.

The definition given of “lag” is potentially confusing. It might be better to describe a leg as the time elapsed between a change in exposure and the ensuing change in the hazard rate. (In more general E-R functions, this could be a more complex function of time than simply a constant gap).

I wouldn’t describe the LT method used as “conditional”, except in the tautological sense that all life tables are conditional at any time point on the events preceding that time. They do, however, allow the prediction of life expectancy conditional on reaching a given age, which is what I think is meant here.

Methods: overview

I don’t recognise “importance analysis” as a term. The authors might usefully adopt terminology from the literature on assessing variability on modelling, i.e. “sensitivity analysis” for local influence of single input variables, and “uncertainty analysis” for partitioning the variation in the output values. This would give a context for further distinguishing uncertainty as imperfect knowledge of a parameter (e.g. estimated with error from a study) and a distribution of population variation in that parameter.

Methods: scenarios
It is very difficult to accept the suggestion that there is “no uncertainty” in the baseline scenario (i). This involves projecting recent mortality rates over a century into the future, a very strong (and uncertain) assumption. The authors might usefully discuss this, or at least qualify their claim.

The last para “The exposure distributions…” is very difficult to understand. What are the units, and what variable is correlated across them?

Dose-response sub-model

The idea that one may model the uncertainty distribution in the exposure-response conditional on a subjective belief dichotomy as to whether the relationship exists seems novel, and close to Bayesian in intent. The effect would be, I think, the same as an unconditional distribution with a spike of probability at a RR value of 0. (This opens up the possibility of an infinite number of unconditional distributions with weights distribute according to prior belief or assumptions: the input distribution does not have to be one of the standard statistical functions such as Gaussian or gamma.) Some further discussion of this would be useful: statements like “plausibility is an inherently dichotomous variable and the true uncertainty is always zero or one” will be hard for readers to understand or (perhaps) agree with. Of course, judgment on plausibilities or indeed on the shape of a prior distribution for a parameter will be personal and itself variable.

Discount

Surely ALL benefits from reduced fine particle exposure occur in the future, or at least after the reduction is made?

Discounting is best described not as part of the life-table methodology, but as part of a scheme for assigning different values to LT predictions. Since LTs produce as output the life years to be lived at each age and in each calendar year, different values can be assigned by year and age. Discounting differentiates life years only by calendar year. The authors do not discuss the possibility of assigning different values to a life-year depending on its age.

Impact indicators

The paper might be read as suggesting that life expectancy and life years are alternative outputs. In fact, life expectancy is measured in (life-)years, and is the sum of those life-years experienced and/or predicted. Thus, for any age-specific sub-cohort, the change in life-expectancy from a change in hazards from a particular age is exactly the change in average life-years experienced from that age (because the history of getting to that age is assumed to be the same under the two scenarios). The paper doesn’t give that impression.

I don’t have the Nevalainen and Pekkanen paper to hand. However, impacts on independent cause groups can be combined because hazard rates are additive. If the authors are making some different (and presumably more complex) assumption, perhaps they should describe it briefly.

Results

It’s true that the distribution of baseline mortality rates could be very different in developing countries than in the west, but it’s the cause-specific rates that are important.

The assessment in this paper was done only for the population current in 2002, and the authors note that a long lag in an older population will minimise the effect. This ignores that future generations should inherit cleaner air and therefore experience lower overall mortality rates, and so underestimates the total benefit to society.

Discussion

For reasons discussed above, authors should not state “discount rate has a large impact on life-table results”; it has a large impact only on the total value placed on those results.
While it is strictly true that death implies a “health loss”, I don’t think that phrase (used multiple times) is a good description of a change in mortality.

Discretionary Revisions (which the author can choose to ignore)

**What next?:** Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.