Reviewer's report

Title: Cost-effectiveness of interventions for increasing the possession of functioning smoke alarms in households with pre-school children: a modelling study

Version: 1 Date: 21 January 2014

Reviewer: Rob Anderson

Reviewer's report:

This is a well described, and apparently well conducted, model-based economic evaluation of an important type of injury prevention initiative in developed countries. I am not well qualified to comment on the technical aspects of the execution of the model assumptions in R using Monte Carlo simulations, but the methods seem to adhere to the guidance on Good Practice on decision analytic modelling to which they refer in their paper.

Major compulsory revisions

I would suggest only one major compulsory revision. It is that the authors both revisit and better justify the derivation of the base case value for the probabilities of a fatality following a fire, with and without a functioning smoke alarm. In the past the relative risk of a fatality without vs with a smoke alarm has always been claimed to be 2; but I have never been able to trace an original source to properly inform this. These authors cite Table 2.4 in a recent UK report on Fire Statistics. I have looked at, and reanalysed the data from this table in Excel, and I am not clear where they get their values of 5 per thousand and 10 per thousand from. My analysis (attached) suggests a substantially smaller difference in the rate of fatality fires between households with and without a functioning smoke alarm (using either the 1010-11 or the 2011-12 data). My estimation of the risk of fatal casualties per thousand from the data in this table are 6.1 or 5.9 (if house had a smoke alarm which operated/worked) and 7.9 or 7.3 in houses where a smoke alarm was absent or present but did not work. This gives a substantially lower effectiveness of between 1 and 2 fatalities avoided per 1000 fires.

Furthermore, I think these rates will be even smaller because the denominator in this table is not persons in houses with fires but fires - I think the analysis therefore implicitly assumes that all fatalities in these data were/are in 0 to 4 year-olds. My doubt about the validity of these particular parameter input values, is underlined by a general lack of sufficient detail about how important parameters have been obtained and derived. Once the authors have got to the bottom of and justified what the correct fatal casualty probabilities should be, this should also be one of the key one-way sensitivity analyses conducted.

Minor essential revisions
1. Fuller justification of the choice of sources and derivation from sources of key input parameter values, possibly in an Appendix. In particular, I think the methods and findings of the Cooper et al MTC/meta-analysis deserve summarising more fully; including some discussion of the counter-intuitive results from that study in terms of additional strategy components being associated with lower probabilities of possessing a functioning smoke alarm (e.g. when going from Intervention (3) E + FE to (4) E + FE + HI or from intervention (3) to (5))?

Also, in allocating the fixed cost of an intervention scheme (of £61,009) presumably there is an assumed number of households that such a scheme would cover; and therefore how much the share per household would be. I can’t see this and assume that £61K would probably be a huge underestimate of the true fixed/overhead cost of a national programme of the kinds described? What is the assumption about the scale/coverage of an intervention scheme that has been built into the analysis?

2. The table of parameters (A1) could have more informative column titles (e.g. "Data" should say "Point estimate (Std error)").

3. Tables of results of cost-effectiveness analyses must have columns for incremental costs and incremental QALYs. Also, to avoid many decimal places, I would suggest presenting the results for 1000 or 100,000 households.

4. Some attempt to show a basic breakdown of the origins of the estimated QALY gains (e.g. between fatal and non-fatal fire-injuries avoided) and costs under each intervention would be useful.

5. top p.7 should refer to NICE public health methods guidance, to avoid confusion.

2.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I have no financial competing interests.

My only potential non-financial competing interest is that I was closely involved in an earlier model-based economic analysis of similar public interventions. This in fact makes me more aware of (and more sympathetic to) the considerable challenges that this modelling team would have faced in obtaining relevant data and developing a workable and credible model structure to address the decision
problem.