Reviewer’s report

Title: Assessing the acceptability of rat trap use over pesticides for health risk reduction in poor urban communities through a survey

Version: 1 Date: 5 February 2012

Reviewer: Leslie London

Reviewer’s report:

This is a very interesting paper, which provides some novel new insights into a problem that is both invisible but important. Publication of the paper will help to shed light on this issue and with relatively minor changes, the paper will be scientifically sound and contribute to new knowledge in this area. I would recommend publication with relatively minor revisions, attention to four issues and with some addition of supplementary materials.

Major revisions

1. The main puzzle in this paper is about baseline use of pesticides for rat control. On the one hand, there are data for baseline use of pesticides buried in Table 1, as the answer to a retrospective question about use of pesticides prior to being given the trap, and no information is provided anywhere in the paper about prior use of rat traps. It is not clear if respondent households were asked at baseline about use (I suspect they were) but what would be really interesting would be to see the change in intention after use of the traps. Unless it is the case that all respondents at baseline were users or potential users of pesticides and none were users or potential users of traps, then the change in what people thought would be important to note. That would, of course, add a cohort component to the study, even though the units are households, not individual respondents. The authors should clarify if such data are available, and if not, explain the extent to which they think the finding at the follow up reflect a true change in use or intended use.

That said, the comparison of reported previous use (78%) to intended future use (29%) at the follow up (in table 1) is still impressive and worth making more of in the discussion, since it is not only the “acceptability of rat trap use” that is the focus of this paper but their acceptability “over pesticides.” The change from 78% to 29% certainly does show some preference for traps “over pesticides”, all the caveats of a cross-sectional study notwithstanding. It would have been even more elegant to have a baseline prevalence for intention to use traps.

2. The Title should reflect more closely what the project was about: It was not about ‘poor urban communities’ in general, but rather about findings from two specific poor urban communities (and I would add “in South Africa”). I do not think it necessary to have ‘through a survey’ in the title since that is the method. In short, I think a title “The acceptability of rat trap use over pesticides for health
risk reduction in two poor urban communities in South Africa” would best capture the study.

3. I don’t think the conclusion can ignore the need for better sanitation and waste control as solving the problem upstream. Vermin control at the end point can only be partially successful. While intervening on sanitation and garbage collection isn’t the focus of the paper, simply acknowledging this issue would make the paper more cogent as a Public health contribution. So, the use of rat traps could be more strongly argued if it were to take place in the context of comprehensive public health programmes to address problems of waste in poor communities.

4. The authors should include as supplementary files, the questionnaire used and some of the 2X2 tabulations relating to the associations for which extremely high OR’s were presented – if only to assure the reader these Odds Ratio’s are not spurious.

Minor revisions

1. While the study concerns itself with the effects of a very specific intervention, I fear that the context in which the study was done, that of a seeming increase in urban poisonings of children with pesticides, attributable to intoxication with street-sold pesticides, referred to in references 9 and 10, is somewhat lost. Simply pointing to the potential contribution of a preventive intervention such as rat traps and the potential benefits of reducing harm in the form of acute pesticide poisoning in children, without claiming more than can be claimed from this study, would still help to situate this study better, and highlight its importance.

2. In the abstract,
   a. A comment is made that ‘rodent infestations are an increasing problem’. This may be true but the paper does not present it as ‘increasing’ – rather just as a public health problem (It does warn that the problem may increase in future with Global Warming). Either the literature review in the introduction should justify why the problem can be described as (currently) getting worse, or the abstract text should be amended to remove ‘increasing.’
   b. The Description of results used the term ‘predictor’ of trap use. Given that this is essentially a cross-sectional study, I would be more sanguine in attributing a time sequence (‘predict’) and simply use the term ‘association with …’. See earlier comment re lack of data at baseline.

3. Methods
   a. First line on page 6: I would describe the sampling as “Households were the units of sampling rather than individuals” rather than “Households formed the study sample…”
   b. Systematic random sampling could only identify the household, not the adult interviewed, and it should be described in that way. Further, the authors should indicate how they selected an adult from each selected household. Was it the first adult to open the door, the head of household? Convenience selection?
   c. Sample size: It would be expected to see something in the narrative of how
Sample Size was estimated of an a priori prevalence estimate and an acceptable margin of error (not only mention of the size of the population).

d. Note that in the next sentence “The loss to follow up …”, the authors speak about respondents moving away, when it is actually families or households moving elsewhere. Given you did not need the same respondent at follow up visit, it is inconsistent to talk about ‘respondents’ moving away.

4. Analysis

a. It would be useful to be able to see the questionnaire to understand how use in future of traps was determined. This could be added as a supplementary file.

5. Results

a. I would cite income in values already converted to US$ and indicate they are converted values. (e.g. ‘… equivalent to US$xx…’). Similarly on page 11 in discussion, when pricing illegal pesticides, use the US$ equivalent (in cents!).

b. Again, I would like to see the results qualified by ‘reported’ such that “Most of the study respondents reported using the traps …”. Applies throughout.

c. I have to take a double take at Odds Ratios of 14.6 and 83.6. BMC allows for data from analyses to be included as Appendices and readers might want to reassure themselves these are not artefacts in estimating measures of effect. What this implies is that there is almost complete discordance in the 2X2 table for these contingencies and it would be useful to be able to eyeball this to confirm. I suggest that the authors be asked to confirm the veracity of these Odds Ratios and to include some of the preliminary bivariate analyses which would presumably have had some 2X2 contingency tables for the associations for which these astronomical Odds Ratios were produced.

d. Related to the above, is the question whether there was co-use (or co-intention to use) of pesticides and traps. It would be useful to see the %s of respondents who were prepared to use both.

6. Discussion

a. The fact that pesticide use at follow up was so strongly associated with intention to use pesticides in future implies that users who remain users have very strong allegiance to pesticides. Does this mean that once you are convinced that pesticides are the best way to deal with pests, you are always going to be convinced? In other words, if you start using pesticides, can you stop? This brings me back to the earlier question about change in practice – are those who used pesticides at follow up the same who used pesticides at baseline? Were there baseline users who at follow up had switched?

b. A second question related to the above is co-use of traps and pesticides. This didn’t get presented in the data and it would be interesting to see. Did respondents report co-use and, if so, how common was it, and did it affect intention to use – either to use pesticides or traps

c. The discussion ends with a very strong note that cost will ‘determine’ sustainability. This may be true, but I am not sure the data presented in this study justify such a strong statement. The data pointed to gender, to experience of the
trap’s success and to willingness to buy at a vendor. “Determine” is a very linear causative relationship when I think the authors have data and would agree that risk perceptions and choices for vermin control are multifactorial – so cost may be important, even very important, but I am not sure “ultimately determine” does justice to their study.

d. The discussion of limitations talks about study power as lacking. It is true that one will almost never have sufficient power to show modest associations in the absence of a very large sample, such as a population-based survey. But I am not sure this is such a problem in this study. There were POR’s that were astronomical in this analysis – OR’s over 10 and one of over 80. There was no power lacking there, since both were statistically significant. And what ‘weaker predictors’ would have been relevant?

Also, I would have thought that the limitations could have mentioned the design issue of respondent versus household (as in 4b. above). Respondents at the follow up may not have been entirely familiar with how traps were used, or may not have been responsible for any pest control, so this may have resulted in misclassification. In the absence of any details of how the adults were selected from the households, it is difficult to see if this could have been a problem or not.

Discretionary Revisions

1. Methods

a. The text “The same fieldworkers … were successfully located” could do with some editing down. For example, it would read more concisely as “The same fieldworkers follow up households in November 2009 to administer the acceptability survey. Of the 199 original respondents, 175 households were successfully located.”

b. The study is essentially a cross-sectional study in a population who were given an intervention to use. I would not mention the absence of a control group since that is likely to confuse readers. You did make a comparison between users/intenders and non-users/intenders, so it gets confusing to state you had no control group. What I would suggest is that you explain clearly and upfront the study design used, since it is a bit buried and takes a little bit of reflecting to understand what is being compared.

2. Analysis

a. In defining the outcome variables, strictly speaking what was being measured was reported use or reported intention to use, rather than actual use or actual intention. The text should ideally reflect this. (1st paragraph page 7)

b. The text of the 1st sentence in the last paragraph on page 7 ends a bit awkwardly. It is not clear what the subject of the sub-clause is meant to be. Can the authors make this clearer in the text? (‘and also used to guage acceptability …’) Perhaps splitting the sentence will be best.

3. Results

a. I would cite income in values already converted to US$ and indicate they are
converted values. (e.g. ‘… equivalent to US$xx…). Similarly on page 11 in discussion, when pricing illegal pesticides, use the US$ equivalent (in cents!).

4. Discussion

a. At the head of page 10, the authors describe respondents ‘reported’ problems with wooden traps. This is presumably information they shared outside of the main study methods, since this finding does not emerge from the Results. If so, can you use a different verb as ‘reported’ implies that it was data you solicited in the questionnaire (for example, use ‘described’). In this way, it is clear it is not part of your results.

b. The sentence “… in order for the intervention to be accepted …” needs a bit of clarity. When the authors describe what “… is currently used and available,” do they mean commercially available in stores?

c. The comment that females had more problems using the traps could be tested from the data that the researcher have – a simply 2X2 comparison of problems in use by gender. Rather than it ‘appeared that more females had problems…’, one can say whether it is or isn’t the case.

d. Given the above point, the recommendation for an informal vendor to demonstrate rat trap use would presumably be strengthened by the demonstrator being a female vendor herself?

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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

Declaration of competing interests:

Conflict of interest declaration: The authors are colleagues in my department; I have served on the advisory panel for Dr Rother’s project from which this student thesis is drawn. However, I have not been a co-investigator in the study, nor been involved in any of the research, management or financial decisions. I have no financial interests in the research.

I have conducted this review, to the best of my critical abilities. I do not believe my academic links to Dr Rother and Prof Ehrlich have influenced my judgement, but I will leave it to the editor’s to assess whether my review should be considered free of conflict of interest.