phase 1 to phase 2 in rehoused and not rehoused groups, including the interaction effect (the difference in the intervention group minus that in the control group) with an appropriate confidence interval. Although for the other outcomes considered, these analyses lead to similar conclusions to the naive approach of interpreting significant as real and non-significant as null, this would not generally be the case, and should not be regarded as an adequate basis for inference.

In similar vein, the authors argue that in the rehoused group there was a significant change in damp in respondents aged over 50 ($p = 0.04$) but not in those aged 50 or under ($p = 0.08$). The difference between these two $p$ values is small and could well be entirely attributable to the fact that the majority of the sample, 59 per cent, fell in the over-50 group. Had the authors chosen to dichotomize age at 60, say, they might well have concluded that the effect of intervention was confined to the lower age group. The correct approach here is to examine whether there is evidence for an interaction between group (intervention or control) and the explanatory factor of interest, here age.

The above deficiencies point to the need for us in public health, as a highly numerate discipline, to seek to maintain an exemplary standard in peer-reviewing and reporting research findings.

References


Yours faithfully

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Reply

Sirs,

Robert Newcombe's comment on our study raises the issue of whether there is such a thing as a perfect analysis. Of course there is not, and choosing a technique involves balancing a number of considerations. We chose McNemar's test because it is particularly appropriate for before and after designs in which each participant is used as its own control and in which the measurements are made on either a nominal or an ordinal scale. It also adopts an $H_0, H_1$ approach, and the significance level and interpretation are based upon probability.

Confidence intervals provide an alternative to significance testing. With the exception of damp, Newcombe's analysis leads to the same inferences as those we made using McNemar's test. Where we identified a change as significant, the confidence intervals also do not cross the zero boundary. In the case of damp, the change is in the same direction for both groups, and we did not interpret this in the paper as of any substantive significance.

The use of probability as an indicator of statistical significance and basis for inference is well established. However, we acknowledge that the use of confidence limits provides a sense of magnitude and additional information that may be of importance. This is more likely to be the case when making decisions about medical interventions than housing interventions.

Regarding Newcombe's point about effects by age group, these were explored by age band, although only the significant finding for dichotomizing at age 50 was reported in the paper.

Yours faithfully

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Contact tracing and population screening for tuberculosis – who should be assessed?

Sirs,

We have read the paper on this subject by Underwood et al. with great interest and were particularly interested in their outcome data from screening new entrants to the United Kingdom. Such screening is recommended as part of the national programme for control and prevention of TB, but few data are available on the effectiveness of screening programmes and Underwood et al. have shown no cases of TB diagnosed in 322 new entrants screened and only 10 (3.1 per cent) were given chemoprophylaxis.

In 1999 in Croydon we set up a holistic clinic for new entrants, where as well as TB screening, the wider needs of new entrants could be met. Refugee Health Visitors gave advice on benefits,