

RESEARCH ISSUES

Patricia Mayhew¹

My task is to bring together some remarks on research issues, in particular in the light of the discussion over the last two days, and the papers in the session on Wednesday.

James Lynch remarked that the credibility of the International Crime Survey (ICS) results will ultimately depend on the degree to which they can be defended with a sufficient degree of confidence. He also said that a priority for secondary analysis was early examination of the reliability of results. I want to take up these points about reliability, and also later - and very briefly - address a few broader points about research methods in the context of the ICS.

Let me start with three remarks.

- 1) Firstly, the reason we went ahead with the ICS was to get underway an exercise for which there had been criminological demand, if not enough criminological energy to provide the supply. We saw the survey as a case of "nothing ventured nothing gained".
- 2) Secondly, the essence of the ICS, in our view, lay in its standardisation. If the same questions were asked, in roughly the same way, then any inherent deficiencies of measurement would at least be equalised. In my view, this point has not been adequately emphasised at this Conference. We were aware that any type of survey would be imperfect - it would have one leg shorter than the other, but at least everyone in the field would have one leg shorter than the other. This is not quite the whole story of course, and I shall have to return to this.
- 3) Thirdly, there were pragmatic considerations with regard to the design of the ICS. This explains many of the features of the survey. Thus, first, sample sizes were dictated by costs. It was simply unrealistic to think that there would be an adequate number of countries represented if costs were set too high. Second, the choice of telephone interviewing offered reduced costs, and the promise of greater standardisation of survey administration.

Reliability issues

Three of the main methodological issues which have attracted attention both at this Conference, and more generally in the way the ICS has been received are:

- 1) the accuracy of victimisation risks;
- 2) the effect of mode of interview;
- 3) the effect of response rates.

¹ Senior Principal Research Officer, Research and Planning Unit, Home Office, London, United Kingdom.

I do not want to go into any of these issues in any great detail, but it is nonetheless important to try and "lay a few ghosts to rest".

Victimisation risks

The point estimates of victimisation risks in different countries should not be seen as the most important information the ICS offers; far from it. But ironically, the figures which inevitably attract most attention at this stage in the ICS programme are those which purport to show how much crime there is of different types, in different countries. These figures, of course, are subject to sampling error, and the dimensions of this error can, and should, be specified. However, potentially more important, is the degree to which the figures are subject to response bias. Many people may assume that risk estimates are fragile because people in different countries may differ in their preparedness to tell interviewers about what had happened to them, and/or as regards how well they perform the interview "exam". In my view, these worries have not been firmly substantiated - though I speak more in terms of industrialised countries. I would not rule out some differential response effect operating in relation to sexual incidents for instance, but there is no *prima facie* reason to believe that in industrialised countries, people differ much in their ability to remember incidents for instance. As I have said earlier, to the extent that respondents are bad respondents, they may well be bad everywhere.

Richard Block raised the point with regard to response bias that forward telescoping was likely to mean that the annual ICS rates were overestimates. I am not entirely sure I agree with this. It may be true of more serious crime, but there is also much methodological evidence to suggest that survey undercounts crime - because of deliberate under-reporting to interviewers of certain types of offences, and because victimisations simply get forgotten. More offences may fall out of the interview than are telescoped into the recall period.

I think a further point needs making in relation to victimisation risks - and surprisingly it is not one that has been emphasized much here. It is about the usefulness of national risks. There is of course likely to be as much variation within countries as between them, and this should not be forgotten. Jan van Dijk has already mentioned that national risks reflect degree of urbanisation, such that comparisons of city risks do not always paint the same picture as national risks. (In Australia, for instance, about three-quarters of residents live in cities; Australian city risks appear much less out of step with those in European cities than in risks in Australia overall.)

The point about area differences also applies to comparison between surveys in developing cities and those in the industrialised countries. The developing cities themselves differ in relation to size and it is difficult to see what "size of place" unit from the industrialised countries will be the best one to take for comparisons - if indeed these comparisons should be made at all.

Telephone interviewing

The second methodological issue is telephone interviewing. I believe that on balance the choice of CATI was justified - because of costs and standardisation

considerations, as I have said, and because the surveys in the industrialised countries were done where telephone ownership was at a sufficiently high enough level for gross bias in sample representativeness to be avoided.

The underlying conclusion of methodology work is that telephone interviews can give similar results to face-to-face ones, given a similar quality of fieldwork. Methodological work under the ICS programme also supports this. This is not to say that no problems have occurred. The acceptability of telephone interviews varies by country and in England and Wales, for instance, the telephone mode undoubtedly caused problems (it seems to have done so, too, in New Zealand). Also, it was not possible to conduct interviews by telephone in all countries. In a few countries, face-to-face interviews were chosen (by Japan for instance), or were not feasible (in Northern Ireland and some parts of Spain). There is scope for further work here to test whether response bias due to mixed-mode interviewing is something we should be concerned about in the context of the ICS questionnaire.

Response rates

The third methodological point relates to response rates - something which has not got much attention here, but which is no doubt in the back of many people's minds. The level of response is one issue, though to my mind it may be the variability of response which is potentially more difficult.

There are two arguments about low response: first, that those with something to say - i.e. more heavily victimised - are disproportionately over-represented; and, second, that those who are easiest to reach by telephone are over-represented - i.e. the residentially most stable and less victimised. On balance, the first view has more supporters. I believe a strong argument could be mounted that non-response in telephone interviews may have less to do with the representativeness of who is contacted than simply the realities of people's lives. The power to halt, or refuse, a call is in the hands of the respondent. Calls are made predominantly in the early evening, when the potatoes, rice, pasta are coming up to the boil, or are about to be put on the table. Refusal could just as well reflect meal times, television programmes, keep-fit classes as any particular bias as regard victimisation. I would like to see a methodological test of response rates in which calls were deliberately made at the time of peak-viewing TV programmes - Coronation Street, the Bill Cosby Show, Il Prezzo è Giusto, or Neighbours. I would not be surprised if the attractions of such programmes lowered response considerably.

ICS methodological work has not ignored the non-response issue in any case. The result of the work done is that the victimisation rate among those who initially refused but accepted after a second call was no different from that among a sample of those who accepted the first time round.

Other research issues

Let me finish by briefly considering a few other research issues regarding the future design and analyses of the ICS.

Design

A problem with victimisation surveys is that their designers inevitably get locked into questions and survey administration decisions that were made at the beginning of the programme in order to maintain (a) consistency over time and (b) consistency for countries who may enter the survey at different points. To this extent, the ICS survey instrument we have at the moment is one which cannot be radically redesigned. This needs to be accepted.

This said, future design decisions could take on board some changes. For example:

- a) one might consider oversampling particular people - e.g. those in cities - to increase the "take" of victims. It would not, in my view, be particularly feasible to interview children. The ICS questionnaire is not appropriate, and there could be difficulties as regards obtaining parental permission;
- b) one might certainly decide to expand some questions (e.g. on the police), though in practice this may well be at the cost of cutting others.

Again, some of the points that have been raised at this Conference have, in fact, been taken up in ICS methodological work. For instance, as Jan van Dijk mentioned, we tried out - with no great success - some self-reported offending questions. We have also tested the usefulness of advance letters, and the effect of different types of introductory statements at the beginning of the survey.

Analysis

James Lynch's paper focused on the best ways to analyse the ICS data, and the priority that needs to be given to different tasks. His comprehensive discussion leaves little else to add that would not go over the same ground. What I would say, though, is that more secondary analysis of the ICS results has been done than is probably widely known. It was our intention initially to publish a secondary analysis book which would have brought results more to the fore. For various reasons (of the familiar human kind), this fell by the wayside, though hopefully some of the work will be retrieved. The sort of analysis that was done was important mainly in showing the uses to which ICS data could be put, aside from league tables. One example is Jan van Dijk's analysis of how constant the correlates of risk are across country. In all participating countries, risks were increased by higher socio-economic status, younger age, and living in a large city - independent of each other. The similarity of results was more notable than the few variations.

In sum, then, it should not be thought that those most closely involved in the ICS were, or are, ignorant of the important problems surrounding the reliability of the ICS. However, we were concerned not to let the perfect be the enemy of the good. A fair amount of methodological work has already been done on the critical issues of mode effects and response bias, and further work will continue in the future. Finally, we believe the potential of the ICS will be more clearly apparent when more secondary analysis, that strays beyond the confines of league tables, has been completed.