

Scottish Crime and Victimization Survey

**Calibration exercise: a comparison of
survey methodologies**

**Research report for
The Scottish Executive**

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1. Introduction

In June 2004, the Scottish Executive commissioned BMRB Social Research to undertake the Scottish Crime and Victimization Survey (SCVS), a new survey of victimisation in Scotland. In two distinct ways the SCVS was a radical departure from the previous sweeps of the Scottish Crime Survey (SCS) that had been undertaken in Scotland since 1993. First, the sample size was increased from 5,000 interviews every three years to an annual sample of 27,000 with continuous interviewing. More importantly, the survey method was changed from a face-to-face survey to a telephone survey.

These changes were the outcome of a fundamental review of the SCS undertaken in 2003.¹ This review set out the limitations of the previous sweeps of the SCS. These related principally to the relatively small sample of 5,000 households, which meant that victimisation could only be reported for Scotland as a whole; that trends were difficult to identify accurately and some of the results for individual types of victimisation were unreliable because of the small number of respondents reporting them. This particularly affected low-incidence but high-profile crimes such as crimes of violence.

The key recommendations of the Review were that the Scottish Crime Survey should:

- switch to computerised data collection
- have a significantly increased sample size, and
- have continuous fieldwork.

There was also a strong recommendation for the survey to have an element of telephone interviewing. Indeed, it was argued that “including a significant telephone component is fundamental, in that the objectives identified in the review cannot be met without doing so”.² Three issues took the review to this conclusion: the recommended increase in sample size could not be achieved at reasonable cost without switching to telephone; it would be difficult to find enough interviewers to conduct an expanded SCS using face-to-face interviewing and other methods (such as postal surveys) were inherently unreliable.

A number of risks were associated with telephone interviewing, such as:

- a reduction in the survey response rate from 70-75% to about 60% seemed likely³

¹ McCaig, E and Leven, T (2003) *Fundamental review of the Scottish Crime Survey*, Edinburgh, Scottish Executive. In the remainder of the text this will be referred to as ‘the McCaig Review’ or simply ‘the Review’.

² Op cit p.52.

³ This was calculated by excluding all non-contacts so was unrealistically reassuring about the response rate penalty of switching to a telephone survey. This response rate is not comparable with the response rates reported below for either the telephone or face-to-face surveys.

- the need for a shorter interview of around 20 minutes rather than the 30-40 minutes of the SCS
- less control over sampling than is offered by the Postcode Address File (PAF) used for face-to-face interviewing because of the weaker link between administrative geography and the coverage of telephone exchanges, and
- poorer quality information and difficulties with sensitive topics.⁴

However, these were not seen as deterrents to using telephone interviewing. Indeed the Review was optimistic about the transition to telephone and expected little change in the measures recorded by a telephone survey compared with face-to-face interviewing: “Our expectation is that the move to telephone will not lead to a variation in victimisation recording rates that would be significant enough to impact on policy studies to a worrying extent”.⁵

Nevertheless, the change in data collection method from face-to-face to telephone interviewing represented the potential for change in the data series established by the Scottish Crime Survey. Reflecting this, the Scottish Executive commissioned MORI Scotland and TNS Social to undertake a parallel face-to-face survey designed as a repeat of the previous waves of the SCS, although with a smaller sample of 3,000 interviews, to provide a measure of victimisation against which the telephone survey could be compared.

This report presents the findings of this comparison exercise. However, the objective is not simply to compare the two surveys as though one was a potential substitute for the other and the only question is which provides the better estimates. The McCaig Review did not question the ability of face-to-face interviewing *per se* to provide accurate estimates, merely whether the sample was large enough to provide the level of precision required and whether a sample of the size required could be achieved at a cost the Scottish Executive might be willing to bear. In general, there is no question that face-to-face victimisation surveys such as the British Crime Survey, based on large random samples with high response rates, are able to provide accurate estimates. The basic question underpinning the calibration exercise reflects the shift in survey method: does a crime survey conducted by telephone provide an accurate estimate of victimisation among the adult population resident in private households in Scotland?

Although the change introduced by a telephone survey is the main focus of the analysis, the question of accuracy does not apply solely to the telephone survey. All surveys are only estimates of what they seek to measure and like all surveys the face-to-face survey is subject to its own errors and biases. Indeed, there is some circularity in gauging the success of the

⁴ It is not clearly spelled out in the Review what is meant by “poorer quality information”. We have taken it to mean higher item non-response than would be expected in a face-to-face survey. Similarly it is not made clear why a telephone survey would have problems with sensitive topics. We have interpreted this as referring to the difficulty of the interviewer establishing rapport with respondents on the telephone and so people being reluctant to discuss sensitive issues such as domestic violence or sexual assault.

⁵ *op cit*, p.60 although the Review does not make clear what level of variation would be required to be “worrying”.

telephone survey by using an approach whose weaknesses the telephone was designed to overcome (and a smaller and less accurate version of it). In assessing the telephone survey we cannot rely solely on the 3,000 face-to-face interviews and where appropriate we also provide comparisons of the telephone survey with the 2003 sweep of the Scottish Crime Survey (5,041 interviews), the 2003 Scottish Household Survey (approximately 15,000 interviews)⁶ and the 2001 Census.

Also, recognising the limited ability of the face-to-face survey to give a definitive answer to the question of accuracy, we have stepped back from a simple comparison of the telephone and face-to-face results and gone back to first principles to ask what features of a survey give us confidence in its estimates, and how far does the telephone survey meet these criteria? Four key aspects of survey accuracy, which can be applied to any survey, provide a structure for our analysis. To provide clear evidence and reassurance of reliability a survey should show:

- a sufficiently large sample
- selection from a sampling frame that covers the population of interest, giving each a known, non-zero probability of selection
- a high response rate from the survey fieldwork, ensuring that the randomness of the sampling is carried into the achieved sample, and
- the absence of observable non-sampling bias in the data, implying that as well as being a reflection of the population on demographic measures, the achieved sample is likely to be a reflection of the population on those measures the survey is intended to estimate.

Thus, the task of the analyses presented here is to assess the extent to which the telephone survey exhibits features that would lead to the conclusion that its estimates of victimisation are likely to be accurate.

The report is structured as follows.

- Chapter 2 provides an overview of the surveys and the sample design used for each
- Chapter 3 looks at sample coverage and survey response rates for both the telephone and face-to-face surveys
- Chapter 4 examines the nature and extent of bias in both surveys and more generally examines the extent to which the surveys provide similar estimates for similar issues

⁶ The Scottish Household Survey is a continuous general household survey of the Scottish population in private residences. Over a two-year sweep over 30,000 face-to-face interviews are conducted across Scotland in respondents' homes. The sampling is designed to be representative of the Scottish population each quarter, to permit analysis of the larger local authorities and groupings of smaller authorities every year and to allow analysis at a local authority level every two years. The survey is conducted by MORI Scotland and TNS Social using computer-assisted personal interviewing and achieves a response rate that is typically 67-68%. More details can be found at www.scotland.gov.uk/shs. At the time of undertaking the analysis, data from 2003 was the most recent publicly available.

- Chapter 5 looks at the key measures of victimisation provided by each survey as well as other measures collected by both surveys.
- Chapter 6 draws together the findings into our conclusions.

In addition, the calculation of response rates for telephone surveys is a complex and contested issue, with few standard definitions and no standard approach across practitioners. The assessment of which telephone numbers are eligible to take part is particularly problematic. Appendix 1 gives details of the method used to estimate the proportion of non-contact telephone lines that should be considered eligible in calculating the survey response rate. Finally, Appendix 2 provides details of the weighting approaches that were used to try to identify bias and correct bias in the telephone survey.

2. An overview of the surveys

This chapter provides a brief overview of the design of the two surveys and discusses differences in survey administration.

2.1 The face-to-face survey

The face-to-face survey was designed to replicate the previous sweeps of the Scottish Crime Survey, the most recent of which was carried out in 2003 by MORI Scotland and TNS Social. The main features of the sample design were as follows.

- The sample was drawn from a geographical area that covers mainland Scotland and the larger inhabited islands.
- Within this area, sample was proportionately allocated to local authorities.
- Primary sampling units (PSUs) based on combinations of Census output areas were selected within each local authority with selection probability proportionate to size.
- Addresses were sampled within PSUs based on a target average response rate of 70%, with assumptions for the proportion of ineligible addresses in each local authority based on the average percentage ineligible observed in the Scottish Household Survey (SHS) between 1999 and 2002.⁷
- Addresses were randomly allocated as either short or long interviews and, within the long interviews, as either version A or version B.⁸
- In households with more than one adult aged 16 years or over, the respondent was selected using a Kish selection procedure.
- Interviewers made up to 6 attempts to secure an interview at sampled addresses.
- Interviews were carried out between July and November 2004.

2.2 The telephone survey

The design of the telephone survey illustrates some of the methodological attractions of telephoning interviewing.

- The sample covers the whole of Scotland, with no geographical exclusions.
- The sample is unclustered and has the potential to reach residences that might be excluded from samples based on the small user PAF.⁹

⁷ The Scottish Household Survey is also carried out by MORI Scotland and TNS Social. At the time of sampling for 2003 and 2004, 2002 was the most recent year for which sample outcome information was available.

⁸ The short interviews comprised only basic demographic information and a self-completion component to collect information on drugs misuse and domestic violence. Neither of these is relevant for comparison with the telephone survey and the short interviews are not discussed further. Version A and Version B indicated different follow-up sections of the questionnaire which came after the incident screening and Victim Forms.

- Interviews are disproportionately spread across the nine Police Force Areas (PFA) in Scotland, with a minimum of 3,000 interviews in each, allowing geographical disaggregation of the data at PFA level for household victimisation and fear of crime.
- The telephone survey involves continuous data collection. To be eligible for the survey, telephone numbers must be those of the main residence of a private household in Scotland. Within each eligible household contacted, only one adult aged 16 or over can be interviewed and a random selection process is used to select one person in households containing two or more adults.
- Rather than 6 calls at sampled addresses, the telephone survey makes a minimum of 10 calls (and often many more) to attempt to make contact. These calls can be spread over several weeks.

2.3 Survey administration

Overall, the surveys were designed to be as similar as possible, consistent with normal practice for each mode of interview.

2.3.1 Respondent selection

Respondents to the face-to-face survey were selected by Kish grid whereas respondents in the RDD survey were selected according to which of the adults in the household had the most recent birthday. The key advantage of ‘last birthday’ over Kish is that the birthday method is less intrusive, requiring no substantive information in advance of respondent selection. It was therefore expected to lead to lower initial refusal than if Kish selection were used. This is borne out by previous research¹⁰ although some studies have found that birthday methods have resulted in various types of bias. Of particular interest, for reasons that will become apparent below, birthday selection methods have, among other things, been found to result in:

- over-representation of women¹¹
- people selecting themselves because they want to do the survey.¹²

2.3.2 Survey instruments and administration

The survey instruments were kept as similar as possible although some differences were inevitable to reflect the requirements of the two modes of interview. Some differences were

⁹ For example, special enumeration districts (military bases, prisons, large communal establishments etc) are excluded from samples drawn from PAF so a private residence in a special ED will be excluded from a PAF sample but might be included in an RDD sample.

¹⁰ For example, Oldendick, Robert W. (1988) “A Comparison of the Kish and Last Birthday Methods of Respondent Selection in Telephone Surveys” in *Journal of Official Statistics*, Vol.4, No.4, 1988. pp. 307-318. More recently Tipping, S and Nicolaas, G (2001) “Respondent Selection procedures for Telephone Surveys” in *Survey Methods Newsletter*, Vol. 21, No.1.

¹¹ Gaziano, C. (2005) “Comparative Analysis of Within-Household Respondent Selection Techniques” in *Public Opinion Quarterly* 69(1):124-157.

¹² cited by anonymous reviewer as O’Rourke D and Blair J (1983) “Improving random selection in telephone surveys”, *Journal of Official Statistics* 10: 428-32.

retained – such as show cards on the face-to-face survey – to retain consistency with normal practice on face-to-face surveys.

The face-to-face sample received advance letters from the Scottish Executive advising them of the survey. Obviously, the telephone sample could not receive an advance letter but the RDD telephone script contained a detailed section of designed to establish the validity of the survey and reassure potential respondents, including offering to send a letter explaining more about the survey.

Broadly, the substantive questions in each survey were the same, consistent with the mode of interview. The main difference was that the face-to-face survey used show cards for some questions such as Question 1 – “I would like to start by asking you about some general social issues in Scotland today. First, do you think unemployment is a problem in Scotland today or not?”. Respondents were shown a card with the options ‘a big problem’, ‘a bit of a problem’ and ‘not a problem’. In the telephone survey, the question was slightly different, with two stages: is the issue a problem or not and if so, how much of problem. Respondents were asked:

“I would like to start by asking you about some general social issues in Scotland today. First, do you think (problem) is a problem in Scotland today or not? IF A PROBLEM: Would you say it is a BIG problem, or a BIT of a problem?”

The CATI script had the distinct advantage of reducing the scope for interviewer error by automating question routing and managing tasks such as identifying and selecting the appropriate incidents of crime for follow-up in the victim modules. These aspects of the face-to-face survey required detailed checking of forms as they were returned for coding and data entry.

Overall, although there were differences in the administration of the two surveys, these were not expected to be significant in terms of the survey measures. The surveys were not designed to test the impact of using or not using show cards. This would have required split samples in the face-to-face survey to try isolate the impact of survey mode on responses to show card questions.

3. Sample coverage and response rates

3.1 Sampling frames

Survey samples are limited by the extent to which the source from which potential respondents are to be selected – the sampling frame – completely enumerates the population that the survey intends to represent. To the extent that the frame does not completely cover the population, there is scope for bias, although the extent to which frame under-coverage leads to bias depends on the extent to which the reason for non-enumeration is related to the survey measures. For example, the small user PAF does not include communal establishments such as student halls. A survey of student living expenses screened from a PAF sample is likely to be systematically biased.

Each potential respondent should, ideally, only be represented once in the sampling frame, giving each person the same probability of being selected.¹³ However, provided the potential for multiple listing is known and information is collected to enable this to be accounted for after the survey, over-coverage is not a significant problem. For example, the fact that some households have more than one telephone line on which they can receive incoming calls increases their probability of inclusion in a telephone survey sample. Provided the number of landlines is established as part of the survey, weighting can restore each household's probability of selection to equality.

3.1.1 The limitations of PAF

Since the mid-1980s the Postcode Address File (PAF) has become the best and most widely used sampling frame for general population surveys. Apart from the SCVS, it is the sampling frame for all major government surveys in Scotland including the Scottish House Condition Survey, the Scottish Household Survey and every recent sweep of the Scottish Crime Survey. PAF is a listing of postal delivery points and the small user subset used for survey sampling is known to include small offices, shops and other non-residential addresses. Vacant properties are also included in the sample. Together this ineligible sample typically represents about 10% of all sampled addresses, imposing a small fieldwork overhead cost to identify and screen them out. Most of the ineligible addresses are readily identified and screened out by interviewers in the field although there remains the possibility of a small proportion that, for example, might be vacant but are not obviously so. The extent of this is unquantifiable but is not considered a significant problem and these will remain part of the eligible sample with the effect that they tend to deflate the survey response rate by a small amount.

¹³ Or more accurately a probability of selection determined only by design features of the survey such as disproportionate stratification.

In terms of people excluded from PAF samples there are two issues: the exclusion of communal establishments such as prisons, hospitals, nurses' homes and student halls, and the exclusion of private dwellings within military bases and other sites that would be difficult to access. The 2001 Census shows that 1.7% of the Scottish population live in communal establishments.

Where there are multiple households at a single delivery point – shared flats or bedsits – these will appear on PAF once and interviewers need to randomly sample from eligible households in the field. This under-coverage can be accounted for by weighting. A potentially more serious problem with PAF in Scotland is the listing of tenement flats as a single address when there are, in fact, many individual dwellings. This problem has largely been resolved by two means: the progressive renumbering of addresses to create individual entries on PAF or by the inclusion of an additional field – the Multiple Occupancy Indicator – that allows selection probabilities to be adjusted before sampling. The experience of the Scottish Household Survey has been that the MOI is wrong in only about 2% of sampled addresses.

The general assessment is that PAF provides excellent coverage of private households and given the focus of the SCS on private households, the limitations of PAF do not present any significant problem of bias.¹⁴ For the purposes of the face-to-face survey, there is no issue of frame under-coverage.

Although not a limitation of PAF, face-to-face samples tend to be clustered to achieve some fieldwork efficiencies. The penalty for this clustering is that the standard errors for the survey are larger than for an unclustered sample of the same size.

3.1.2 The limitations of RDD

There is no question that RDD samples represent the best option for telephone surveys. In comparison with published directories, which exclude lines and households that have opted to be ex-directory, RDD ensures that all residential lines have an equal probability of inclusion in the survey sample. However, there are two major limitations imposed by random-digit dialling samples for telephone surveys. The first is that the sample is, by definition, limited to those who can receive telephone calls. Second, mobile phones are also excluded from most RDD samples because there is no link between mobile numbers and terrestrial geography. It is not possible to sample mobile phones located in Scotland without incurring a significant cost in screening a sample of all mobile numbers anywhere in the UK. Thus, RDD samples exclude households with no telephone and those who only have access to a mobile phone. The impact of this is discussed more fully below.

¹⁴ Although it is difficult to say to what extent the focus of the SCS on private households is simply a reflection of the sampling frames that have been available for the survey. Clearly, the omission of significant groups of the population (such as students in halls or prisoners) who might experience higher levels of victimisation would be a problem if the objective were to measure all forms of victimisation.

In addition, because of the way RDD samples are generated, it is not known how many of the numbers generated are working residential lines. This creates a requirement to ‘ping’ all the numbers that have been generated and remove those that are not working lines or where faxes or modems answer the test on the line and then to remove known business numbers by matching with directories. Both ‘pinging’ and directory matching can be done automatically.

The sample that is then issued for interviewing is likely to still contain a significant proportion of business numbers, these mainly being unlisted direct-dial numbers on internal business exchanges. These must be screened out in the course of fieldwork.

Finally, there remains a proportion of numbers that cannot be classified. These are lines that ring out or are always engaged and while they might be eligible residential lines, they might be dedicated computer lines (so not eligible) or they might be public call boxes (ineligible) or perhaps dwellings that are temporarily vacant (also ineligible).

The treatment of these lines varies in the UK. In some studies they are treated as ineligible and so excluded from response rate calculations.¹⁵ This assumes that if contact cannot be made after 10 attempts, the line must somehow be not working or otherwise ineligible.¹⁶ In others,¹⁷ some attempt is made to determine the proportion of lines that should be regarded as eligible on the assumption that there might be valid reasons why an eligible line might be difficult to contact.

In the United States, where telephone interviewing is longer established, this question has been addressed and there are clear recommendations from the American Association of Public Opinion Research¹⁸ on the need to estimate the proportion of the sample of unknown eligibility that should be regarded as eligible. The size of the group of unknown eligibility and the proportion that is taken as eligible can have a significant impact on the survey response. Our approach to estimating this is discussed in Appendix 1.

¹⁵ Most notably for this study, the International Crime and Victimization Survey (ICVS). Other significant studies, such as Ashworth, Wands, Turtle, Lloyd (2000) *Police Public Consultation: Developing a Model Survey*. Home Office, London have followed the practice used on the ICVS of treating all non-contacts treated as ineligible.

¹⁶ In Ashworth et al these lines are discounted as calls boxes, second homes and businesses. In the SCVS, daytime interviewing would have identified and screened out business and as we show in Appendix 1 call boxes and second homes cannot possibly account for the level of non-contact.

¹⁷ for example, Nicolaas, G and Lynn, P (2002) “Random-digit dialling in the UK: viability revisited” in *Journal of the Royal Statistical Society A* Vol.165, Part 2, pp.297-316.

¹⁸ See www.aapor.org. There is no guidance on how the estimate might be arrived at although it is clear that a genuine attempt to make an informed estimate should be made rather than arbitrarily picking a number.

3.2 Mobile telephone and mobile-only households

In considering the accuracy of estimates from telephone surveys, mobile-only households are of particular interest for two reasons:

- the potential for the composition of such households and their experience of victimisation to differ from that of the population as a whole, representing a source of bias in telephone surveys at any point in time
- the possibility that recent trends in the growth of mobile-only households might continue, representing a growing problem of bias and instability in time series data.

In recent years, two broad trends have been apparent. First, the proportion of households with no telephone has declined to the extent that for all practical purposes, coverage is complete. Secondly, the proportion of households with only a mobile telephone has grown quickly in recent years and, according to monitoring data from Ofcom, these households have distinctive characteristics. In this respect the McCaig Review was wrong to see mobile-only households as a problem to be addressed some time in a future that did not need to be considered in 2003.¹⁹

3.2.1 Growth in telephone coverage

Research by ONS between 1998 and 2002 has shown that the proportion of UK households with no telephone declined from 4% in 1998 to 1% in 2002.²⁰ The proportion with only a mobile telephone grew from less than 1% in 1998 to 4% in 2002. This research also showed increases in various telephone-related barriers to contact such as answering machines and call screening devices, which are discussed below.

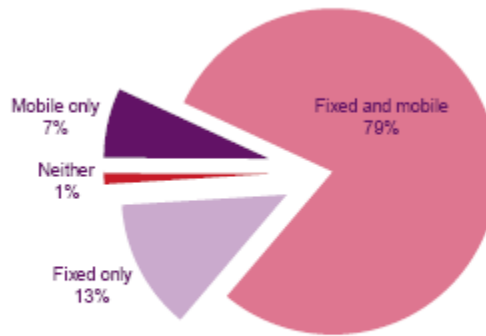
Most recently, the position shown by Ofcom in its most recent report of telephony in the UK from February 2004, is that only 1% of homes in the UK have no telephone and that mobile-only households comprise 7% of all households.²¹

¹⁹ McCaig and Leven, op cit, noted that “while this [non-coverage of mobile-only households] may be an issue for the future, the proportion of homes without a landline phone remains small” p.54.

²⁰ Taylor S (2003) “Telephone surveying for household social surveys” in *Survey Methodology Bulletin* No.52 July 2003, Office for National Statistics.

²¹ Ofcom (2004) *Consumers’ use of fixed telephony* August 2004.

Figure 1 – Telephone access in UK homes, February 2004
Percentage of homes



Note: Data at February 2004

Base: 2131 (Don't knows are excluded)

Source: Ofcom

3.2.2 The characteristics of mobile-only households

As Figure 1 shows, the recent estimate from Ofcom is that 7% of homes in the UK are mobile-only. This is reflected in the results from the face-to-face survey, which found that while 91% of households had at least one fixed line telephone, 7% had only a mobile phone and 2% had no telephone. A more recent estimate, from Jan-March 2005 showed that 11% of households in Scotland had either no telephone (2%) or only a mobile telephone (9%).²² Thus, the likelihood is that a telephone survey in Scotland can only represent about 90% of households and about 1 in 10 are automatically excluded by virtue of having no fixed telephone line.²³

This is only a serious problem if the 1 in 10 differ significantly from the others and, most importantly, if it is likely that they will differ in terms of their experience of victimisation. Both the face-to-face survey and Ofcom's data show that mobile-only households do indeed have distinctive characteristics and are different in ways that might be related to victimisation. They are more likely to be:

- **Young** – the face-to-face survey estimates that 9-16% of respondents aged 16-34 years are in mobile-only households. Ofcom makes similar estimates.
- **Unemployed** – Ofcom estimates that 26% of unemployed households are mobile-only
- **Low-income** – Ofcom estimates that between 10% and 15% of households with an income of less than £17,500 are mobile-only
- **Living in deprived areas** – the face-to-face survey estimates that 16% of households in the most deprived 10% of areas (using the Scottish Index of Multiple Deprivation) had no phone (4%) or were mobile only (12%).

²² The first wave of MORI Scotland's Social Policy Monitor, with a sample of 1,080 households asked the same questions as were used in the face-to-face survey.

- **In single-adult households** – 16% of single adult households in the face-to-face survey had no phone (4%) or were mobile-only (12%).
- **In rented tenures** – 21% of households in rented tenures in the face-to-face survey are mobile-only.

This combination of characteristics suggests that regardless of the quality of the RDD sample, the scale or quality of the fieldwork effort established to translate a high proportion of eligible sample into interviews or its success in doing so, telephone surveys are significantly restricted by the inability of an RDD sample to reach a sizeable proportion of the population whose experience might be important to accurate estimates of the prevalence of victimisation in Scotland.

The extent of victimisation among mobile-only households and households with a fixed line telephone is discussed below.

3.3 Telephone Preference Service

The Telephone Preference Service (TPS) is a central opt-out register where households can register their wish not to receive unsolicited sales and marketing telephone calls. It is a legal requirement that direct marketing companies do not make such calls to numbers registered on the TPS but TPS does not apply to market or social research. However, although it not required, some research companies routinely screen telephone samples and remove TPS subscribers, others vary their practice depending on the survey and other never do.

In this exercise, the RDD survey suffered a major error in that during the sampling process telephone numbers registered with the Telephone Preference Service were mistakenly screened out of the numbers that had been randomly generated for the telephone survey. This error was not detected until after six months of interviewing meaning that TPS subscribers were missing from the sample used for comparison with the face-to-face survey. The effect of this is to remove a group of eligible respondents from the sample for the survey.

There are three questions that need to be considered about this:

- what proportion of eligible households were removed from the sample?
- what is the impact of screening out TPS subscribers on the representativeness of the SCVS sample?
- what is the impact on the survey's estimates of victimisation?

We deal with the first two of these here and address the impact on victimisation in Chapter 5.

²³ These survey estimates are subject to some sampling error – the face-to-face survey estimate suggests that the true value in the population as a whole is likely to be in the range 6.1% to 7.9% while the later MORI Scotland estimate suggests the true value is likely to be in the range of 7.3% to 10.7%.

3.3.1 TPS registration in Scotland

Estimates of the proportion of domestic telephone lines registered with TPS are not routinely published but registration has increased substantially in recent years. In October 2003, 8% of residential GB telephone lines were registered with TPS.²⁴ In June 2004, just before the telephone survey fieldwork started, an estimated 20% of UK lines were registered. The most recent estimate from April 2005 is that 42% of residential lines in Scotland are registered with TPS.

Table 1 shows the number and proportion of TPS subscribers in each of the standard regions of the UK and demonstrates a distinct Northern bias in favour of TPS registration – perhaps exhibiting more of a ‘belt and braces’ approach of being both ex-directory and registered with TPS.

Table 1 – TPS subscription rates in each standard region in the UK

	Listed numbers	Ex-directory	TPS registered	TPS as % of all numbers
North East	483,360	513,552	505,122	51%
Scotland	1,268,731	904,926	912,881	42%
Yorkshire & The Humber	1,039,976	900,344	814,672	42%
North West	1,314,494	1,369,992	1,052,867	39%
East Midlands	811,352	741,238	494,325	32%
South West	1,131,865	959,500	623,053	30%
Wales	657,007	513,576	324,161	28%
South East	1,798,544	2,155,408	1,038,688	26%
Isle of Man	23,983	8,913	8,378	25%
West Midlands	924,042	1,132,962	521,707	25%
Eastern	1,176,255	1,458,669	660,274	25%
Northern Ireland	327,774	297,869	113,400	18%
Channel Islands	73,786	18,080	11,844	13%
London	861,634	1,500,890	302,763	13%
Total	11,892,803	12,475,919	7,384,135	30%

Source: UK Changes. Figures refer to April 2005

By checking the TPS status of achieved interviews from the Jun-Sept sample it was possible for BMRB to estimate that had TPS registrants not been screened out, 24% of the achieved interviews would have been with TPS subscribers, suggesting that the screening removed approximately 22% of households from the telephone survey sample used between July and September 2004 – the fieldwork period being considered in this analysis.

3.3.2 Impact on sample representativeness

Using the sample from January 2005, we can compare the profiles of TPS subscribers and non-subscribers to estimate the impact of screening subscribers out of the June-September RDD sample.

²⁴ UK Changes who provide samples to both MORI and BMRB.

Table 2 compares the main demographic variables from the January 2005 sample, of which 38% were TPS subscribers, with the sample from June-September sample (wholly non-subscribers).

Table 2 – Comparison of TPS and non-TPS subscribers from Jan 2005 with Jun-Sept 2004 sample, 2003 SHS and 2001 Census

	January 2005*	TPS	Non-TPS	Jun-Sept 2004	SHS 2003	Census 2001
Tenure						
Owner/occupied	69.7	86.7	67.9	67.0	65.3	62.6
Social rented	16.8	9.4	22.6	20.9	26.9	27.1
Private rented	5.8	2.5	8.1	6.0	6.0	6.7
Other	1.3	1.4	1.4	1.3	1.7	3.5
Missing	6.3	5.6	5.9	4.8	0.0	0.0
Property type						
Detached house	21.9	28.4	18.6	20.9	21.1	20.4
Semi-detached house	23.2	26.8	21.5	24.2	21.3	23.5
Terraced house	15.7	14.0	16.7	15.7	22.2	20.2
Flat/maisonette	31.2	22.5	36.1	32.3	34.9	35.6
Other	2.6	3.7	2.1	3.4	0.4	0.3
Missing	5.3	4.5	5.0	3.4		
Number of adults in household						
One	37.0	30.3	40.6	34.5	37.9	
Two	49.2	56.1	45.5	49.0	48.3	
Three or more	13.9	13.6	13.9	16.5	13.8	
Number people in household						
One	28.5	25.4	30.4	26.9	31.6	32.9
Two	33.0	38.0	30.7	33.4	34.9	33.1
Three	15.4	13.9	16.4	16.7	14.8	15.6
Four	12.8	13.7	12.4	13.6	13.2	12.9
Five or more	4.9	4.4	5.2	6.0	5.5	5.6
Missing	5.4	4.6	5.0	3.4	0.0	0.0
One or two	61.5	63.3	61.1	60.4	66.5	66.0
Three or more	33.1	32.0	34.0	36.2	33.6	34.0
Missing	5.4	4.6	5.0	3.4	0.0	0.0
Number of cars						
None	24.8	19.9	27.2	27.6	35.3	34.2
One	45.1	45.3	45.3	43.0	44.9	43.4
Two	24.7	29.1	22.4	23.8	17.2	18.6
Three or more	5.3	5.7	5.0	5.6	2.5	3.8
Missing	0.0	0.0	0.0	0.1	0.0	0.0
Base	2,413	1,495	918	9,502	14,880	2,192,247

* includes 13 cases whose TPS status is missing

This shows that TPS subscribers are more likely to be owner-occupiers (almost 90%) living in detached or semi-detached houses, with two or more cars, in two adult households. Compared with an estimate of 63-64% from the 2003 Scottish Household Survey and the 2001 Census, the June-September sample recorded 67% owner-occupation and the January 2005 sample records almost 70% owner-occupation. In January 2005 there is almost a 30% shortfall in the proportion of the sample from social rented tenures (17% compared with 27-28% in the two comparator surveys). Reflecting this, the proportion of households with no car has also deteriorated from 28% in June-September to 25% in January 2005 compared with 34-35% in the comparator surveys.

In terms of household size, the number of adults in responding households shows some improvement after the inclusion of TPS households. The number of adults in households in the January 2005 sample is very close to the estimate from the SHS.

Overall, the impact of including TPS households in the sample seems to be to exacerbate some of the demographic bias observed in the June-September sample, which, as we discuss below, already showed bias in favour of owner-occupiers and against households with no car. Adding TPS households brings the distribution of the number of adults in households closer to the SHS.

The exclusion of TPS households means that the June-September sample had a significant problem of sample under-coverage. Approximately 10% of households were excluded because they have no phone or were mobile-only and a further 22% of households were missed because TPS subscribers were screened out. Combining the two suggests that almost one-third of all Scottish households were not covered by the sample used for the telephone survey.

The exclusion of TPS households is likely to partially compensate for the exclusion of mobile-only households since the two groups seem to be very unlike each other in demographic terms. Where respondents in mobile-only households tend to be in rented tenures and in relatively deprived areas, TPS subscribers are more likely to be owner-occupiers and relatively more affluent.

3.4 Survey response rates

Survey response rates are a key measure of survey quality. The response rate indicates the scope for the achieved sample to differ from the population as a whole through differential non-response. For example, if those who are regularly not at home are at higher risk of victimisation than those who stay at home, a high level of non-contact indicates significant scope for bias since those most readily interviewed might be expected to be people least likely to be out of the house and, consequently, at lower risk of victimisation. In this scenario, estimates derived from a survey with a low response rate or a high non-contact rate will tend to underestimate victimisation. Alternatively, if people who have not experienced victimisation are more likely to refuse to participate, perhaps feeling that the survey is not relevant to them, estimates will tend to overstate victimisation. Of course, both might be true, making it a matter of chance whether the victimisation rates from a survey with a low response rate are accurate.

The importance of the response rate also helps to explain differing practice in relation to the treatment of non-contact in calculating response rates. If lines that cannot be contacted can be dismissed as ineligible, the response rate that can be claimed for the survey is (quite artificially) bolstered, making the survey appear to be more representative than it is or at least to have less scope for bias than it actually does.

3.4.1 Response to the face-to-face survey

The face-to-face survey achieved 3,034 interviews compared with an interview target of 3,038 (99.9%). This was achieved from a total of 5,018 addresses issued for long interviews, giving an unadjusted response rate of 60.5%. After removing ineligible addresses, the response rate from eligible addresses was 67.3%, 2.7% lower than the target of 70%. Thus, although the number of achieved interviews was close to the target, the response rate was 96.4% of the target. This reflects levels of deadwood varying across local authorities from that implied by the SHS experience.

The response rates to the face-to-face survey are summarised in Table 3.

Table 3 – Face-to-face survey response rate

	Issued sample	% of all sample issued	% of sample potentially eligible
Interview	3,034	60.5	67.3
No contact	593	11.8	13.2
Refused	749	14.9	16.6
Other eligible	5	0.1	0.1
Unsure if eligible	32	0.6	0.7
Missing, presumed eligible	85	1.9	2.1
Total eligible	4,508	89.8	100.0
Ineligible	510	10.2	
Total	5,018	100.0	

This overall response rate disguises variation between different types of area as measured by the Scottish Executive’s urban/rural classification and the Scottish Index of Multiple Deprivation, as Table 4 and Table 5 show.

Table 4 – Face-to-face survey response rate by urban/rural classification

Excludes ineligible addresses

	Large urban areas	Other urban	Small accessible towns	Small remote towns	Accessible rural	Remote rural	Total
Interview	61.0	69.5	74.1	80.6	73.2	73.8	67.4
No contact	17.0	11.3	8.8	7.8	9.9	11.1	13.2
Refused	18.3	17.1	14.1	10.7	14.1	14.3	16.6
Other eligible	0.1	0.1	0.2		0.4		0.1
Unsure if eligible	1.3	0.3	0.5		0.4		0.7
Contact sheet missing	2.3	1.7	2.3	1.0	2.1	0.7	2.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Base	1,837	1,314	441	103	533	279	4,507

One valid address did not match the lookup files for urban-rural classification

In terms of the urban/rural classification, response in the large urban areas – the four cities of Glasgow, Edinburgh, Aberdeen and Dundee – is substantially lower although this was anticipated in the sample design and response rate rates meaning that large urban areas are not under-represented in the achieved sample, making up 39% of the achieved sample compared with 40% of all eligible addresses.

Table 5 – Face-to-face survey response rate by SIMD deciles

Excludes ineligible addresses

	Most deprived					Least deprived					Total
	1	2	3	4	5	6	7	8	9	10	
Interview	62.5	69.1	63.7	65.0	68.7	71.1	73.5	69.3	71.1	65.4	67.4
No contact	15.7	11.4	14.5	17.5	10.0	12.3	12.5	11.0	10.4	13.9	13.1
Refused	17.1	16.3	18.5	15.3	18.2	14.6	13.4	16.0	17.3	18.4	16.6
Other eligible		0.4					0.3			0.4	0.1
Unsure if eligible	1.4	1.1	0.3	0.2	0.9	1.2		0.8		0.6	0.7
Contact sheet missing	3.4	1.7	3.0	2.0	2.1	0.7	0.3	3.0	1.3	1.2	2.0
Total	100	100	100	100	100	100	100	100	100	100	100
Base	650	528	399	503	428	405	313	400	394	483	4,503

Five valid addresses did not match the lookup file for SIMD

As with large urban areas, the lower response rates in deprived areas were largely anticipated in the sampling with the result that compared with 14% of eligible addresses in the most deprived areas, 12% of the achieved interviews in these areas.

Nevertheless, while the sample design might have been able to anticipate lower response and avoid under-representation – where response rates are lower, there is greater scope for bias and since both rurality and relative deprivation are linked to victimisation, this would be expected to have some impact on the estimates from the survey.

3.4.2 Response to the telephone survey

The telephone survey achieved 9,509 interviews compared with a target of 10,000 interviews (94.6%). This was achieved from a total of 43,504 telephone numbers issued to interviewers (after pinging and the removal of known business numbers), giving an unadjusted response rate of 21.9%. However, ineligible lines make up a much higher proportion of the issued sample than is generally the case in face-to-face surveys and after lines whose ineligibility has been established are removed from the total, the response rate increases to 38.4%. The full breakdown is shown in Table 6.

Table 6 – Telephone survey response rates

	Issued Sample	% of all sample issued	% of sample potentially eligible
Interview	9,509	21.9	38.4
No contact	6,227	14.3	25.1
Refused	7,953	18.3	32.1
Other eligible	162	0.4	0.7
Contact but unsure if eligible	914	2.1	3.7
Total eligible	24,765	56.9	100.0
Ineligible	18,739	43.1	
Total	43,504	100.0	

At first glance this looks more like the response rate of a good postal survey – a method dismissed in the McCaig Review as inherently unreliable – but this still presents only a partial picture of response to the telephone survey because of the high proportion of lines whose eligibility for the survey cannot be determined. This includes not only those lines where contact

has been made but the eligibility the person or households could not be established (the fifth line of Table 6) but also the non-contact category (line 2) since without making contact with someone at the end of a telephone line, it cannot be clearly established if it is a residential number.

In the face-to-face survey, only 2% of the issued addresses were recorded as unknown eligibility and whether they are considered eligible or not can only affect the response rate by 0.3 percentage points. Counting these addresses as eligible gives a response rate of 67.5% whereas including them all as ineligible increases the response rate to 67.8%. In the telephone survey 32% of the issued sample that was not excluded as non-working or a business line was of unknown eligibility. How these are treated can have a significant impact on the survey response rate and this is discussed further below.

3.4.3 Unknown eligibility

Whereas the calculation of response rates for random face-to-face surveys is both well established, straightforward and consistently defined by survey practitioners, practice is less well established for telephone surveys.

In a face-to-face survey it is relatively simple for interviewers to decide if addresses are eligible for inclusion in the survey. First, they have to find the address and, standing outside, can observe if the address is obviously demolished, derelict, vacant or non-residential. If in doubt, the interviewer can attempt to determine if the address is occupied directly by approaching occupants or indirectly through neighbours. In no one answers, signs of occupation can be looked for – lights may be on, there may be sounds of occupation, there might be curtains at the windows, name plates on doors, furniture in rooms etc. In short, the number of instances when an interviewer on a face-to-face survey might be unsure of an address's eligibility should be small.

In contrast, if a telephone line is permanently engaged or ringing without answer it might be non-working or it might be a working residential line connected to the internet or to a call screening device. The interviewer or dialler knows nothing more about the line and cannot deduce or assume anything about its status. Once a telephone line has been dialled many times over an extended period and at different times of day and always with no clear outcome i.e. always ringing or engaged or answered by a machine, it is reasonable to question whether the line is eligible for the survey. However, there are many circumstances when an eligible line might never be answered, just as there are clearly circumstances when face-to-face interviewers cannot make contact at occupied addresses in spite of many calls over an extended period.

- The occupants are always out – this would include people who work away from home, work shifts, have long working hours or active social lives (eligible).

- If the line is primarily used for internet access and frequently engaged or if it is left unanswered for voice calls because friends and family use a mobile number, the line would still be eligible. If the line is used solely for internet access and does not even have a telephone attached, the line would be ineligible.
- The property is unoccupied but the line is still receiving incoming calls for example, it could be a rented flat between occupants (generally would be eligible but not while unoccupied).
- The line is at a second home or holiday home (ineligible).
- The line is at an occupied address but the household uses call screening (eligible).

In the telephone survey, if the entire sample whose eligibility was unknown were assumed to be eligible, the response rate would be 38.4%. However, if the entire unknown eligible sample is assumed to be ineligible, the response rate would be 54%. This would still be low by the standards of large-scale social surveys. The likelihood is that the proportion is neither zero nor 100% but apart from that, little is known about what proportion of these lines should be considered eligible. Yet, a reasonable estimate is important to the final survey response rate.

Our best estimate is that 73% of the lines that had no contact after 10 or more attempts or which were permanently engaged can be considered ineligible. The rationale for this is explained in Appendix 1 but this estimate gives a response rate of 49% for the telephone survey.

Table 7 – Telephone survey response rates

	Issued Sample	% of all sample issued	% of eligible sample
Interview	9,509	21.9	48.6
No contact	6,227	3.9	8.6
Refused	7,953	18.3	40.7
Other eligible	162	0.4	0.8
Contact but unsure if eligible	914	0.6	1.3
Total eligible	24,765	44.9	100.0
Ineligible	18,739	55.1	
Total	43,504	100.0	

3.5 Comparing response to the telephone and face-to-face surveys

Table 8 below compares the response rate from the face-to-face survey and our best estimate of the response to the telephone survey and shows how the two differ (F-T = face-to-face rates minus telephone rates). Clearly, the fieldwork outcomes from the two surveys are substantially different. Most notably:

- The telephone response is substantially lower than the face-to-face response.

- The principal cause of the difference is a much higher refusal rate – 41% of eligible households refuse to take part in the telephone survey compared with just under 17% in the face-to-face survey.
- The non-contact rate for the telephone survey is lower.

Table 8 – Comparison of face-to-face and telephone survey response rates

	Face-to-face (F)	Telephone (T)	Difference (T-F) percentage points
Interview	67.3	48.6	-18.7
No contact	13.2	8.6	-4.6
Refused	16.6	40.7	+15.5
Unknown eligibility, assumed eligible	0.7	1.3	+0.6
Other eligible	0.1	0.8	+0.7
Contact sheet missing, presumed eligible	2.1	0.0	-2.1
Total eligible	100.0	100.0	

This comparison suggests that one of the principal (methodological rather than cost) advantages of a telephone survey envisaged by the McCaig Review – improved contact rates – has been achieved to some extent. The Review argued that:

[while] the telephone approach will have a higher refusal rate than face-to-face it has the potential to improve on initial contact rates provided that the advantages of cheap contact are taken up. Most face-to-face surveys will have a callback requirement of three or four times but with telephone this can extend to ten or more with a higher proportion being undertaken at evenings and weekends. There can be much more controlled and effective cover of different times of day with the possibility of returning to non-contacts after a month or so to pick up households that were absent in the first contact period. It can be hypothesised that reducing non-contacts is more important than reducing refusals when estimating victimisation levels.²⁵

It is not spelled out in the review what the hypothesised benefits of refusal over non-contact might be.

3.6 Conclusion

Population coverage is an essential feature of any survey sample and our analysis suggests that while PAF gives good coverage of the population in private households, small proportion of the adult population living in communal establishments is excluded. This represents 1.9% of the adult population. Frame under-coverage excludes an estimated 9% of households in Scotland from the telephone survey. Although this means that the telephone survey can still cover 91% of households, the fraction that is not represented has distinctive characteristics that are likely to be related to victimisation, meaning that it represents a source of bias in the telephone survey. We would expect the omission of these groups to tend to understate victimisation.

²⁵ McCaig and Leven, op cit, p.57.

In addition to the exclusion of households that cannot be covered by an RDD sample that excludes mobile phones, the mistaken exclusion of TPS subscribers has further reduced the survey coverage by 22% of households. In total, the sample for the telephone survey excluded around 30% of households in Scotland. Our analysis of the characteristics of TPS respondents shows that they have distinctive characteristics, being 90% owner-occupiers and more likely to live in multi-adult households, in detached and semi-detached properties and to have one or more car. These characteristics will have implications for assessing the extent to which non-response (rather than sample under-coverage) has affected the profile of the telephone survey sample and the estimate of victimisation.

The telephone survey is also achieving a response rate that is substantially lower than the face-to-face survey, caused by a substantially higher refusal rate of 41% compared with 17%. While there is certainly scope for the face-to-face survey to be biased, the combination of frame under-coverage, exclusion of TPS and low response means that it is unlikely that the sample achieved by this sweep of the telephone survey would have avoided substantial bias.

4. Evidence of bias

In this chapter we develop the previous chapter's conclusion about the scope for bias in the surveys by examining each for evidence of bias. We compare the sample characteristics of the telephone survey with the corresponding characteristics from the face-to-face survey. As we noted earlier, both surveys have the potential for bias so we also compare the surveys with corresponding data from the 2001 Census and the Scottish Household Survey.²⁶

4.1.1 Comparison of survey respondents and the Scottish population

Table 9 shows the extent to which the samples for both surveys reflect the structure of the Scottish population as measured by the 2001 Census. There is evidence that the survey samples under-represent some age groups and over-represent others. For example, the face-to-face survey shows a pattern typical of face-to-face surveys of adults, with under-representation of young adults and over-representation of older adults.

The telephone survey shows a slightly different profile. The earlier discussion of the characteristics of mobile-only households might be expected to have resulted in under-representation of young adults in comparison with the Census population. In spite of this there is very little difference. Males aged 20-24 years and 45-54 years are under-represented in the telephone survey as are all adults aged 65 years or more. Females aged 35-64 years are over-represented.

²⁶ This analysis was undertaken with data weighted only to account for design elements. The telephone survey was weighted to account for disproportionate sampling by Police Force Area and to account for some households having more than one landline for receiving calls. Respondents were also weighted by the inverse of the number of adults in the household to account for the fact that as the number of adults in a household increases, each adult has a correspondingly reduced probability of selection. The face-to-face survey was only weighted to account for the number of adults in households. The Scottish Household Survey has no corrective weights applied. This does not mean that the SHS is unbiased but it is the closest comparator available and represents what might be achieved from a face-to-face crime survey of a similar size and design.

Table 9 – Comparison of respondent age/sex profiles and 2001 Census

Shows percentage of all respondents in each age/sex group

	Face-to-face (%)	Telephone (%)	Census (%)	Face-to-face difference		Telephone Difference	
Male – 16-19 years	2.8	3.1	3.0	-0.2		0.1	
Male – 20-24 years	2.8	3.2	3.8	-1.0	*	-0.6	*
Male – 25-34 years	6.9	7.9	8.3	-1.4	*	-0.4	
Male – 35-44 years	9.2	9.9	9.4	-0.1		0.5	
Male – 45-54 years	7.7	7.7	8.5	-0.8		-0.8	*
Male – 55-64 years	7.8	6.2	6.6	1.2	*	-0.4	
Male – 65-74 years	5.8	4.0	4.9	0.9	*	-0.9	*
Male – 75+ years	3.6	1.5	2.9	0.6	*	-1.4	*
Female - 16-19 years	2.6	3.3	2.9	-0.3		0.3	
Female - 20-24 years	2.9	3.6	3.8	-0.9	*	-0.3	
Female - 25-34 years	7.4	9.4	9.0	-1.5	*	0.5	
Female - 35-44 years	10.7	12.8	10.0	0.7		2.8	*
Female - 45-54 years	10.5	11.5	8.6	1.9	*	2.9	*
Female - 55-64 years	7.7	8.5	7.1	0.7		1.4	*
Female - 65-74 years	6.3	5.1	6.1	0.2		-1.0	*
Female - 75+ years	5.1	2.2	5.2	-0.1		-3.0	*

* indicates that difference is significant at 95% confidence limits

What is also apparent is that differences of similar magnitude in both surveys (such as for males aged 45-54 years) are significant for the telephone survey but not for the face-to-face survey. This is a reflection of the telephone survey's larger sample, which should lead to greater precision. Differences that are likely to occur by chance in the relatively small face-to-face survey should not be expected in the telephone survey.

The gender balance of each sample is shown in Table 10. This compares the proportion of men within each age group for both surveys in comparison with the corresponding figures in the 2001 Census. This shows evidence of systematic bias against men in almost all age groups in the telephone survey (except men aged 75 years and over), although among men aged under 25 years the difference is not significant. The proportion of men in the samples of respondents aged between 25 and 64 years is significantly lower than in the Census.

In only one of the age groups in the face-to-face sample is the proportion of men significantly different from the proportion in the corresponding group in the Census. This shows bias against men aged 45-54 years.

Table 10 – Gender balance of survey samples

Difference between proportions of men in each age group compared with 2001 Census

	Calibration	Telephone (Jun-Sept)	Census	Calibration difference	Telephone difference	
16-19 years	51.5	49.0	50.7	0.8	-1.7	
20-24 years	48.6	47.3	49.6	-1.1	-2.3	
25-34 years	48.0	45.7	48.1	-0.1	-2.4	*
35-44 years	46.4	43.5	48.3	-2.0	-4.8	*
45-54 years	42.1	40.0	49.5	-7.3	-9.4	*
55-64 years	50.3	42.3	48.2	2.1	-5.9	*
65-74 years	48.1	44.2	44.8	3.3	-0.6	
75+ years	41.1	40.8	36.2	4.9	4.6	

* indicates that difference is outside 95% confidence limits for the survey.

The discussion of respondent selection above (section 2.3.1) indicated that there was potential for the ‘last birthday’ selection method used in the telephone survey to lead to over-representation of women. This would tend to be supported by the evidence here. However, the respondent selection method was changed in January 2005 to use the method recommended by Rizzo et al²⁷ with no significant improvement in the gender balance of the sample, suggesting differential participation by men rather than problems with respondent selection linked to the use of the ‘birthday’ method.

Table 11 – Percentage of men each age group in the telephone samples from Jun-Sept 2004 and Jan 2005

	Telephone Jun-Sept 2004	Telephone January 2005	Census
16-19 years	49.0	39.8	50.7
20-24 years	47.3	54.9	49.6
25-34 years	45.7	40.5	48.1
35-44 years	43.5	44.8	48.3
45-54 years	40.0	44.4	49.5
55-64 years	42.3	44.1	48.2
65-74 years	44.2	44.7	44.8
75+ years	40.8	46.2	36.2

4.1.2 Comparing key demographic variables

Table 12 shows the results of comparing the telephone survey with three sources of comparator data: the face-to-face survey, the 2003 results from the Scottish Household Survey and the 2001 Census. Although this highlights differences between the surveys, there are a number of factors that need to be remembered in assessing the results.

- apart from the face-to-face and telephone surveys, the other data relate to different time periods and some variables, such as tenure, might be expected to change (albeit slowly) over time

²⁷ Rizzo, L. Brick, JM and Park, I (2004) “A minimally intrusive method of for sampling persons in random digit dial surveys” in *Public Opinion Quarterly* 68:2.

- while the intention of bringing in other data sources is to avoid over-reliance on the face-to-face survey (there is no reason to assume that all differences between the face-to-face and telephone surveys is caused by bias in the telephone survey), these surveys – including the census – will themselves have some degree of error.
- the data sources are different sizes and the significance or otherwise of differences will be affected by sample sizes.

Nevertheless, the comparisons are useful because they give us an indication of whether differences between the telephone survey and the face-to-face survey are also observed with comparison to the other sources and in the same direction. For example, in comparison with the face-to-face survey, the telephone survey appears to under-represent households in social rented accommodation. This is also seen in comparison with the SHS and with the 2001 Census. On this basis, we would conclude that the telephone survey is biased against social rented households. This is unlikely to be caused by missing data in the telephone survey – 4.8% of telephone survey respondents refused tenure information – since analysis of other variables (such as MOSAIC classification, SIMD classification) suggests that the households with missing tenure are more broadly spread across social groups.

Where the comparisons across the three sources are not consistent (for example, the proportion of respondents in detached houses) we could not confidently conclude that the telephone survey was biased and if the results from the telephone survey were consistent with the other sources, it would seem more likely that the face-to-face survey is biased on this measure.

We need to bear in mind that the sample being compared in Table 12 excludes TPS subscribers. As we showed in Table 2, TPS subscribers have particular characteristics, being more likely to be owner-occupiers, living in detached or semi-detached houses, with two or more cars, in two adult households. The comparisons here do not take account of how the inclusion of TPS subscribers might have affected the profile of the telephone sample although we would expect it to have exacerbated bias against social rented households, against single adult households and against no car households.

Finally, we also need to bear in mind that the face-to-face survey, the SHS and the Census will include mobile-only households and as set out in section 3.2.2, the absence of these households from the telephone survey will have an impact on the demographic profile. For example, the proportion of social rented households in the face-to-face survey would drop from 26% to 23% if mobile-only households were filtered out. This gives some measure of the impact of the telephone survey being unable to include mobile-only households.

Table 12 – Demographic comparisons – telephone compared with face-to-face, SHS and 2001 Census

	Tel (T)	F2F (F)	Diff (T-F)	SHS (S)	Diff (T-S)	Census (C)	Diff (T-C)
Tenure							
Owner/occupied	67.0	66.6	0.4	65.3	1.7 *	62.6	4.4 *
Social rented	20.9	26.1	-5.3 *	26.9	-6.0 *	27.1	-6.2 *
Private rented	6.0	5.5	0.6	6.0	0.0	6.7	-0.7 *
Other	1.3	1.8	-0.5 *	1.7	-0.4 *	3.5	-2.2 *
Missing	4.8						
Property type							
Detached house	20.9	24.5	-3.6 *	21.1	-0.2	20.4	0.5
Semi-detached house	24.2	22.3	1.9 *	21.3	2.9 *	23.5	0.8
Terraced house	15.7	21.3	-5.6 *	22.2	-6.5 *	20.2	-4.6 *
Flat/maisonette	32.3	31.8	0.5	34.9	-2.6 *	35.6	-3.2 *
Other	3.4	0.0	3.4 *	0.4	3.0 *	0.3	3.1 *
Missing	3.4						
Number of adults in household							
One	34.5	37.2	-2.7 *	37.1	-2.6 *		
Two	49.0	48.5	0.5	48.7	0.3		
Three or more	16.5	14.3	2.2 *	14.2	2.3 *		
Number people in household							
One	26.9	31.3	-4.4 *	31.7	-4.8 *	32.9	-5.9 *
Two	33.4	35.2	-1.8	35.1	-1.7 *	33.1	0.4
Three	16.7	15.7	1.0	14.9	1.8 *	15.6	1.1 *
Four or more	19.6	17.8	1.8 *	18.3	1.3 *	18.5	1.1 *
Missing	3.4						
One or two	60.4	66.5	-6.2 *	66.8	-6.4 *	66.0	-5.6 *
Three or more	36.2	33.5	2.7 *	33.2	3.0 *	34.0	2.1 *
Number of cars							
None	27.6	30.5	-2.8 *	33.0	-5.4 *	34.2	-6.6 *
One	43.0	41.2	1.8	44.6	-1.6 *	43.4	-0.4
Two	23.8	22.7	1.0	19.5	4.3 *	18.6	5.2 *
Three or more	5.6	5.7	-0.1	3.0	2.6 *	3.8	1.8 *
Missing	0.1						
Employment status of respondent (aged 16-74)							
In full-time education	6.3	6.0	0.3	5.7	0.6 *	7.3	-1.0 *
Working full-time	51.3	44.8	6.5 *	44.9	6.4 *	46.9	4.4 *
Working part-time	14.4	13.0	1.5 *	12.0	2.4 *	11.1	3.3 *
Care - home or family	6.0	8.1	-2.1 *	8.0	-2.0 *	5.5	0.5 *
Perm retired from work	13.6	18.7	-5.1 *	18.9	-5.3 *	13.9	-0.3
Unemployed	2.7	3.8	-1.1 *	3.7	-1.0 *	4.0	-1.2 *
Perm sick or disabled	3.8	4.3	-0.5	5.1	-1.3 *	7.4	-3.6 *
Other (specify)	1.8	1.4	0.4	1.7	0.1	3.9	-2.1 *
Marital status							
Single (never married)	29.5	26.6	2.9 *			30.6	-1.1 *
Married	52.2	55.8	-3.6 *			49.8	2.4 *
Separated	3.7	3.4	0.3			3.6	0.1
Divorced	7.0	5.6	1.4 *			7.0	0.0
Widowed	5.8	8.3	-2.5 *			9.1	-3.3 *
Lowest floor of accommodation							
Basement	0.7	0.5	0.2			1.3	-0.6 *
Ground floor	77.8	82.8	-5.0 *			75.4	2.4 *
First floor	7.8	9.1	-1.3 *			13.3	-5.5 *
Second floor	3.3	4.0	-0.7			5.1	-1.8 *
Third or fourth floor	2.7	2.3	0.4			3.5	-0.8 *
Fifth or higher	1.0	1.3	-0.3			1.4	-0.4 *
Missing	6.6		6.6				6.6

* indicates significant differences

These comparisons show considerable consistency in there being significant differences between the telephone survey and each of the other sources indicating bias in terms of:

- **Tenure** – the telephone survey appears to be biased against social rented tenures and we would expect the inclusion of TPS subscribers to exacerbate this. Also, over three-quarters of no phone or mobile-only households are in social rented tenures.
- **Property type** – terraced houses are under-represented and the inclusion of TPS subscribers would tend to make this worse. TPS subscribers would also increase the proportion of households in semi-detached houses and substantially reduce the proportion of households in flats/maisonettes.
- **Number of adults in households** – single adult households are under-represented compared with the face-to-face survey and the SHS. TPS subscribers are less likely to be in single adult households suggesting that this difference would be wider if the sample had included TPS households. This is one of the key characteristics of mobile-only households.
- **Total household size** – one person households are under-represented and households with three or more people are over-represented. TPS subscribers are less likely to be in single person households and less likely to be in households of three or more persons so including them in the sample would exacerbate the under-representation of single person households and reduce the over-representation of households with three or more people.
- **Number of cars** – the telephone survey under-represents households with no car and over-represents households with two or more cars. TPS subscribers are more likely to have two or more cars and much less likely to have no car suggesting that these biases would be worse had the TPS subscribers not been screened out.
- **Employment status** – among respondents aged 16-74 years, the telephone survey over-represents adults in full-time and part-time employment and under-represents retired people and people who are permanently sick or disabled. Including TPS subscribers would make very little difference to the proportion of respondents in employment but it would increase the proportion of retired respondents so would reduce bias against retired people.
- **Marital status** – TPS subscribers are more likely to be married so had they been included, the profile of the telephone sample would be more like the face-to-face survey but less like the Census.

It appears that the main impact of excluding over 20% of households by screening out TPS has been to make the achieved telephone sample *less* biased than it might otherwise have been. In most cases, the characteristics of the TPS subscribers would be expected to exacerbate the biases apparent in the sample. It is difficult to be more precise about this because as Table 2

also shows, there is some instability in the profiles of the July-September sample and the January 2005 sample which seems to result from increasing missing data – up from around 3% of respondents to 5% – and from the growth of TPS subscription between June 2004 and January 2005, which alters the characteristics of the non-TPS sample in January compared with July-September.

4.2 Attitudinal differences in the surveys

This section makes comparisons between the telephone survey and the face-to-face survey on various attitudinal variables. Differences between the two surveys on these variables are less clearly evidence of bias than are provided by consistent differences in demographic characteristics and any differences that are seen are likely to be influenced by differences in the profiles of the two samples.

There were two sets of attitude questions in each survey and both were asked at the same points in the interview. For both sets of respondents the first substantive question of the survey was the battery of questions about “problems in Scotland” followed by questions on crime in the local area, perceptions of safety and worries about various types of crime.

The second block of questions on how common different types of crime are in the respondent’s area comes in one of the follow-up modules asked of half the sample in the face-to-face survey and a quarter of the sample in the telephone survey.

Table 13 shows that there are significant differences between the two samples on a number of the measures of the extent of problems in Scotland. Telephone respondents are more likely to say that standards of education, alcohol abuse, standards of housing and standards of public transport are a ‘big problem’ in Scotland. They are less likely to say that unemployment is a ‘big problem’.

Table 13 – Attitudes about social problems in Scotland

	Telephone (T)	Face-to-face (F)	Difference (T-F)	
Q1 – Percentage saying each is a big problem in Scotland				
Unemployment	38.3	43.2	-4.9	*
Standards of education	14.0	10.5	3.5	*
Standards of health care	33.8	33.8	0.0	
Racial discrimination	19.2	20.0	-0.8	
Alcohol abuse	62.9	60.3	2.6	*
Crime	56.7	58.7	-2.0	
Drug abuse	75.3	76.6	-1.3	
Standards of public transport	41.4	20.1	21.3	*
Standards of housing	29.6	21.2	8.4	*

Table 14 shows some slightly contradictory views in the two surveys to the extent that although telephone respondents are more likely to have lived in their local area for 10 years or more and significantly less likely to feel that crime in their area has increased in the past two years, they are less likely to be concerned about personal safety (Q4, 5 and 6) in their local area but more

likely to worry about every type of crime asked about (Q7). One partial resolution of the apparent contradiction might lie in the perception that rather than having got worse or better, telephone survey respondents report that crime in their local area has stayed the same – perhaps as safe or unsafe as it ever was.

The findings on fear of crime in their local area are consistent with the findings from section 4 that the telephone survey substantially under-represents older people of both sexes and older women in particular. Fear of crime is generally associated with older people and older women in particular.²⁸

The other possibility is that the lower fear of crime is a reflection of tenure imbalance – the other main demographic bias identified at section 4.1.2. If owner-occupiers are more likely to feel safe etc compared with social rented tenants the over-representation of owner-occupiers in the telephone survey would then lead to the difference between the two surveys. However, while this might offer some explanation, both the owner-occupiers and social rented tenants in the telephone survey are more likely than their face-to-face counterparts to ‘feel safe’.

Table 14 – Worries about crime and personal safety in local area

	Telephone (T)	Face-to-face (C)	Difference (T-F)	
Q2 - Lived in local area				
Less than 1 year	4.2	6.1	-1.9	*
Less than 5 years	19.7	23.7	-4.0	*
10 years or more	67.8	62.1	5.7	*
Q3 - Change in crime in local area in past two years				
More	28.4	40.7	-12.3	*
About the same	62.3	52.8	9.5	*
Less	9.3	6.5	2.8	*
Safety and fear of crime				
Q4 - Safe walking alone in local area after dark	68.1	65.9	2.2	*
Q5 - Safe alone in home at night	93.8	92.4	1.4	*
Q6 - Worries a lot about being a victim of crime	5.3	6.4	-1.1	*
Q7 - Worry about crime				
Worries - car or vehicle stolen	45.5	38.6	6.9	*
Worries - things stolen from car or other vehicle	45.5	38.7	6.8	*
Worries - car /vehicle damaged by vandals	57.5	53.8	3.7	*
Worries - home damaged by vandals	37.4	34.4	3.0	*
Worries - home broken into	49.0	42.8	6.2	*
Worries - being mugged and robbed	42.1	33.8	8.3	*
Worries - being physically assaulted in public	41.6	33.8	7.8	*
Worries - attack because of ethnicity/religion	16.5	11.8	4.7	*
Worries - being sexually assaulted	28.0	20.7	7.3	*
Q8 - Impact of fear of crime (mean score on 1-10 scale)	3.27	3.10	0.2	

²⁸ See, for example, McVie et al (2004) *Scottish Crime Survey 2003* Scottish Executive Social Research

	Telephone (T)	Face-to-face (C)	Difference (T-F)	
How common crime is in local area (% saying common)				
(asked of a quarter of telephone respondents and half of face-to-face respondents)				
People having their cars /other vehicles stolen	28.0	21.9	6.1	*
People having things stole from their vehicle	32.2	28.4	3.8	*
Deliberate damage to property or vehicles	41.3	38.3	3.0	*
Homes being broken into	28.0	26.8	1.2	
People being mugged or robbed	19.2	16.2	3.0	*
People being assaulted / attacked in the street	31.0	23.8	7.2	*
Racial / religious assaults	10.3	6.7	3.6	*
Sexual assault	6.6	5.2	1.4	*
Base (full sample)	9,462	3,034		

* indicates significant differences

Even if we could explain the differences in the ‘safety’ findings, there remain the contradiction between ‘safety’ and ‘worry’ – telephone respondents feel safer in their local area and at home than do face-to-face respondents but they are more likely to worry about each type of crime. Possibly telephone survey respondents make a clearer distinction between safety **in their local area** and worry about crime **in general**.

4.3 Conclusion

Compared with the face-to-face survey, the telephone survey shows significant differences on a number of demographic variables. We acknowledged from the outset that there was scope for bias in the face-to-face survey but comparison of the telephone survey with the 2003 Scottish Household Survey and 2001 Census underlines the differences and, in some cases, indicates that the difference is greater than the face-to-face survey suggests. While some of the demographic bias might be a reflection of the exclusion of mobile-only households, the demographic characteristics of TPS subscribers suggests that had these households been included, the demographic bias in the telephone survey would, in general, have been greater.

The differences in the attitudinal variables are more complex and simply indicate that the two surveys have significantly different responses on these measures. We cannot say on the basis of this that one survey is more biased than the other in what they record. While some of the differences between the two surveys are what might be expected given the age, sex and tenure biases in the telephone survey sample, this does not explain the apparent contradiction between perception of ‘safety’ and the greater reporting of ‘worry’ about crime among telephone survey respondents.

5. Victimization

The estimation of victimisation is the principal purpose of the Scottish Crime and Victimization Survey. In this chapter we look at the victimisation data collected by the face-to-face and telephone surveys in an effort to determine the extent to which the change to a telephone survey might have impacted on the estimates obtained.

At this stage we are still using data that has only had basic design weights applied. No account has been taken of the biases identified either at the sampling stage, through the exclusion of mobile-only and TPS households from the telephone survey, or at the data collection stage where both surveys will, in the various ways described above, have experienced response bias. In Appendix 2 we consider the ability of corrective weighting to take account of these.

5.1 How victimisation is recorded

Both the face-to-face and telephone surveys adopted a similar approach to identifying incidents of victimisation. Respondents were asked a series of ‘screening’ questions which identified whether the household/individual had experienced any incidents of any of 18 possible types of crimes within the survey reference period. In both surveys, the reference period was “since 1 April 2003”.

Where respondents reported any incidents, these were followed up with a Victim Form (Victim Module in the telephone survey but we will refer to both as Victim Forms) which collects a detailed description of the incident and associated information such as whether the incident was reported to the police. To limit the burden on respondents, a maximum of five Victim Forms was completed for each respondent. In the face-to-face survey, five full victim forms were completed but in the telephone survey three full forms and two shortened forms were completed, again to reduce the burden on respondents. The limit of five forms results in some incidents not being followed up but this affects only a small number of respondents.

Each form can represent either a single incident or a series of incidents. A series of incidents occurs when the same type of offence is committed against the respondent over a period of time, probably by the same people. In a series, the incidents are both similar and related. For the purposes of analysis, series forms are weighted by the number of incidents they represent up to a maximum of five.

A key purpose of the Victim Form is to probe more fully on when incidents happened in order to determine whether the incident reported in the form actually fell within the year that is used for analysis of victimisation. For these surveys, the reference period for analysing victimisation is April 2003 to March 2004.

5.2 Victimization in the surveys

Table 15 shows a number of comparisons of the level of victimisation recorded in the telephone and face-to-face surveys. In each case, the telephone survey records higher levels of victimisation.

- A higher proportion of respondents report any incidents (line 2)
- Respondents who report any incidents report more of them, completing more Victim Forms per person than similar respondents in the face-to-face survey (line 4)
- A lower proportion of incidents in the telephone survey are counted as eligible for inclusion in the analysis (line5) ²⁹ but this still leaves relatively more victim forms per respondent (line 7)
- Each eligible Victim Form represents more incidents in the telephone survey (line 10), suggesting that the telephone survey has recorded higher levels of victimisation than the face-to-face survey.

Table 15 – The recording of victimisation in both surveys

	Face-to-face survey	Telephone
1 Total respondents	3,034	9,509
2 Percentage reporting any incidents	30%	37%
3 Number of victim forms completed	1,306	5,791
4 VFs per 'reporting' respondent	1.44	1.67
5 Proportion of VFs eligible	64%	52%
6 Number of eligible Vfs	836	3,006
7 Eligible VFs per respondent	0.28	0.32
8 % of eligible VFs representing series incidents	26%	31%
9 Total incidents from eligible VFs	1,186	4,572
10 Incidents per eligible VF	1.41	1.52
11 Incidents per respondent	0.39	0.48

5.2.1 Prevalence rates

The telephone survey records more eligible incidents per respondent interviewed but it also records a slightly different profile of victimisation in terms of the proportion of all incidents that are coded into crime categories.³⁰ In general, compared with the face-to-face survey, the telephone survey records more crime and this difference is particularly marked in relation to personal crime rather than property crime.

²⁹ It is possible that this is incorrect. An error in the telephone survey script meant that no information was collected about how many of the incidents reported in a series occurred within the survey reference period. Whether any incidents occurred in the reference period was known and in most cases the number of periods in which incidents occurred equalled the total number of incidents. The incidents could therefore be accurately assigned to periods. In around 20% of cases it was necessary to estimate the number that might have happened in the reference period by assigning incidents evenly across all the periods in which incidents were known to have occurred. For example, if 5 incidents occurred in two periods, each period was given 2.5 incidents.

³⁰ Both companies used the same coding manual and a 5% sample of each company's coding was submitted to the surveys' project manager at the Scottish Executive for independent verification.

Table 16 shows the prevalence rates recorded in each survey – the percentage of respondents reporting any incidents of each type in the survey reference period – and compares the telephone survey with both the face-to-face survey and the 2003 Scottish Crime Survey. This is the last reported full sweep of the crime survey, with over 5,000 respondents compared with just over 3,000 in the face-to-face survey.

Table 16 – Prevalence rates for telephone, face-to-face and 2003 SCS

	Tel (T)	F2F (F)	2003 (S)	Diff (T-F)	Diff (T-S)	Diff (F-S)
Household crime						
Motor vehicle vandalism	4.7	4.5	5.8	0.2	-1.1 *	-1.3 *
Property vandalism	3.5	2.7	4.4	0.8 *	-0.9 *	-1.7 *
Theft from motor vehicle	2.6	2.6	3.0	0.0	-0.4	-0.4
Attempted theft of/from mv	0.6	0.5	1.1	0.1	-0.5 *	-0.6 *
Theft of motor vehicle	0.3	0.5	0.7	-0.2	-0.4 *	-0.2
Bicycle theft	0.8	0.8	1.2	0.0	-0.4 *	-0.4
Housebreaking	2.6	1.8	2.9	0.8 *	-0.3	-1.1 *
Other household theft	3.3	3.2	3.3	0.1	0.0	-0.1
Any household crime	15.5	14.1	18.3	1.4	-2.8 *	-4.2 *
Personal crime						
Petty assault	4.1	2.1	2.4	2.0 *	1.7 *	-0.3
Serious assault	0.5	0.4	0.8	0.1	-0.3 *	-0.4 *
Sexual offences	0.4	0.2	0.0	0.2	0.4 *	0.2 *
Theft from person	1.2	0.3	0.5	0.9 *	0.7 *	-0.2
Robbery	0.4	0.2	0.4	0.2	0.0	-0.2
Other personal theft	2.3	1.7	2.2	0.6 *	0.1	-0.5
Threats	2.9	1.8	1.6	1.1 *	1.3 *	0.2
Any personal crime	7.2	4.0	5.4	3.2 *	1.8 *	-1.4 *
	9,509	3,034	5,041			

* Indicates difference significant at 95% confidence limits. Eligible incidents only

There are three important points to note from this:

- the telephone survey generally records higher prevalence rates than the face-to-face survey. Where differences are significant, the telephone survey is consistently higher than the face-to-face survey but the prevalence rate for household crime is not significantly different. The rate for personal crime is greater by over 60%.
- the telephone survey records lower prevalence of household crime than the 2003 Crime Survey – a drop of 15%. Prevalence of personal crime is 33% higher than that recorded in 2003.
- the face-to-face survey also records lower prevalence of household crime than the 2003 Crime Survey although the drop is much larger at 23%. Personal crime is 26% lower than in 2003.

5.2.2 Incidence rates

Incidence rates express the number of eligible incidents recorded in the surveys as a number of incidents per 10,000 units of population. These units can be households, adults, car owning households or bicycle owning households depending on the offence. The rate itself is the

product of multiplying the number of eligible incidents by a grossing factor equal to the number of 10,000s of units in the population.

Table 17 – Comparison of incidence rates – telephone, face-to-face and 2003 Scottish Crime Survey

	Tel (T)	F2F (F)	2003 (S)	Sig diff (T-F)	Sig Diff (T-S)	Sig Diff (F-S)
Motor vehicle vandalism	785.4	768.0	825.9			
Property vandalism	709.6	550.4	830.5			
Theft from motor vehicle	356.2	309.8	407.8			
Attempted theft of/from mv	76.2	65.9	125.7			
Theft of mv	35.8	62.6	90.9			
Bicycle theft	124.9	85.7	131.9			
Housebreaking	345.4	263.7	397.5			
Other household theft	512.4	431.8	499.2			
Petty assault	705.9	420.2	435.4		*	
Serious assault	69.0	49.3	114.8			
Sexual offences	56.2	24.6	1.8		*	
Theft from person	122.7	32.4	59.4			
Robbery	50.7	15.8	48.4			
Other personal theft	283.9	248.3	260.9			
Threats	523.8	283.1	174.0	*	*	
Any household crime	2,945.7	2,537.9	3,309.2			*
Any personal crime	1,232.4	765.92	1,094.8	*	*	
	9,509	3,034	5,041			

Comparison of the incidence rates shows that even though they broadly reflect what was seen in terms of prevalence rates, none of the differences in the household incidence rates are statistically significant when offences are considered separately. The rates for ‘any household crime’ shows that the telephone survey is not significantly different from either of the two face-to-face surveys but the difference between the face-to-face survey and the 2003 survey is significant.

Apart from ‘threats’, the personal incidence rates recorded by the telephone and face-to-face survey are not significantly different although the overall ‘any personal crime’ category is significantly higher in the telephone survey. Compared with the 2003 survey, the personal incidence rates in the telephone survey are significantly higher for petty assault, sexual offences, personal theft and threats. Overall, ‘any personal crime’ is higher in the telephone survey in comparison with both the face-to-face survey and the 2003 survey.

5.3 Recorded crime

Trends in recorded crime from 2002 and 2003/4 tend to confirm that while the face-to-face survey appears to be understating household crime, the telephone survey appears to overstate personal crime (compared with 2003 SCS figures). In summary:

- the face-to-face survey shows a fall in vandalism of 20% compared with a fall of 10% in the telephone survey and an increase of 9% in recorded incidents of vandalism

- the telephone and face-to-face surveys both show a fall in vehicle crime of 25% and 30% respectively, which is broadly consistent with the trend in recorded crime showing a fall of 19%
- the fall in bicycle theft of 30% recorded by the face-to-face survey is not consistent with the trend in recorded crime, which is static. The fall of 13% recorded by the telephone survey is more consistent with recorded crime
- housebreaking in the face-to-face survey shows a fall of 29% and the telephone survey a fall of 5%, against a recorded decrease of 17%
- recorded crimes show a drop of 4% in personal theft (which combines theft from the person and other personal theft) while the face-to-face survey shows a drop of 13%. The telephone survey shows an increase of 12%
- recorded incidents of assault rose by 4% whereas the face-to-face survey shows a fall of 15%. The telephone survey shows an increase of 41%
- robbery has fallen by 15% in the recorded crime statistics whereas the telephone survey shows an increase of 5% and the face-to-face survey shows a fall of 67%.

5.4 Other factors affecting victimisation in the telephone survey

In the preceding chapters we discussed three features of the telephone survey that might be having an influence on victimisation rates. These were:

- the exclusion of no-phone and mobile-only households from the survey coverage
- an administrative error that screened out TPS subscribers from the survey sample
- a substantially higher refusal rate than the face-to-face survey, which opens the possibility that people refusing to participate had different experiences of victimisation.

The face-to-face survey gives an indication of the victimisation experience of no-phone and mobile only households. TPS subscribers were not screened out of the samples from January 2005 and we have been provided with data from January with TPS subscribers flagged, allowing analysis of the experience of TPS subscribers. The June-September 2004 achieved interviews have also been flagged to indicate whether respondents took part in the main sweep of fieldwork or whether their interview took place as part of the re-contacting of respondents who had initially refused. The victimisation experience of these 'soft' refusals gives an indication of the extent to which the experience of refusers might differ from more willing participants.

5.4.1 No-phone and mobile-only households

With only just over 200 respondents without a fixed telephone line, for the purpose of this analysis, no-phone and mobile-only have been collapsed into a single group and compared with households with a fixed telephone line (and possibly also a mobile phone). The results are

shown in Table 18, which shows that there are no significant differences in the victimisation of the two groups in the face-to-face survey. It might be, however, that in a larger sample such as the annual sample for the telephone survey, that differences such as those recorded here for motor vehicle theft, housebreaking and serious assault would be significant.

Table 18 – Comparison of prevalence rates among households with and without a fixed telephone line

	No landline	Landline	All	Chi-sq	Sig
Motor vehicle vandalism	4.2	4.6	4.5	0.09	
Property vandalism	3.0	2.7	2.7	0.09	
Theft from motor vehicle	2.3	2.6	2.6	0.11	
Attempted theft from motor vehicle	0.4	0.5	0.5	0.12	
Theft of motor vehicle	0.8	0.5	0.5	0.40	
Bike theft	0.8	0.8	0.8	0.00	
Housebreaking	3.0	1.7	1.8	2.21	
Other household theft	3.0	3.2	3.2	0.02	
Petty assault	2.0	2.2	2.1	0.03	
Serious assault	1.0	0.4	0.4	1.94	
Sexual assault	0.0	0.1	0.1	0.29	
Theft from the person	0.5	0.3	0.3	0.18	
Robbery	0.0	0.2	0.2	0.43	
Other personal theft	1.7	1.9	1.9	0.01	
Threat	4.0	1.6	1.8	5.89	*
Any household crime	14.7	14.0	14.1	0.10	
Any personal crime	5.0	4.2	4.3	0.23	
Base - household	265	2,769	3,034		
Base - individuals	202	2,832	3,034		

5.4.2 TPS respondents

Comparison of prevalence rates for TPS and non-TPS respondents from January 2005 shows only one significant difference – the rate of serious assault among non-TPS respondents is significantly higher than among non-TPS. Even the broad categories of ‘any household crime’ and ‘any personal crime’ show no significant differences.

Table 19 – Prevalence rates among TPS and non-TPS subscribers in January 2005

	Non-TPS	TPS	All	Chi-sq	Sig
Motor vehicle vandalism	6.3	7.1	6.6	0.50	
Property vandalism	5.2	5.4	5.3	0.03	
Theft from motor vehicle	3.1	2.0	2.7	2.52	
Attempted theft from motor vehicle	1.2	1.2	1.2	0.00	
Theft of motor vehicle	0.3	0.7	0.4	2.81	
Bike theft	0.9	1.6	1.1	2.14	
Housebreaking	2.6	2.7	2.6	0.01	
Other household theft	3.7	3.8	3.7	0.01	
Petty assault	3.8	4.3	4.0	2.72	
Serious assault	1.1	0.1	0.7	8.04	*
Sexual assault	0.3	0.1	0.3	2.70	
Theft from the person	0.9	0.9	0.9	1.86	
Robbery	0.9	0.6	0.8	2.23	
Other personal theft	3.2	2.7	3.0	2.02	
Threat	4.3	3.5	4.0	2.19	
Any household crime	18.6	19.5	18.9	0.32	
Any personal crime	9.9	8.7	9.5	2.12	
Base	1463	891	2,356		

Again, we would caution that this may be a consequence of only having access to one month's sample – only 2,356 cases – and that a larger sample may show significant differences.

5.4.3 Refusal in the telephone survey

The June-Sept 2004 sample was flagged to show whether respondents had taken part in the initial round of fieldwork or whether they had been interviewed as part of a refusal sample that was re-contacted in the hope of converting them to interviews. To the extent that the experience of initial refusers is different from more willing participants, this gives an indication of the impact of the telephone survey's high refusal rate on victimisation.

Table 20 shows that on the two broad measures of victimisation – any household and any personal crime – refusers have significantly lower prevalence rates than those who took part in the initial fieldwork. Several individual categories show significant differences and in all but one (theft of a motor vehicle) refusers have lower prevalence rates than main respondents. Combining some of the smaller categories that are not separately significant, such as motor vehicle and property vandalism into any vandalism shows that the refusers also have significantly lower prevalence rates.

The implication of this is that 'hard' refusers – those who were firm refusals at the outset or who refused to take part in the recontact survey – might be expected to have at least similar prevalence rates as the refusers who were converted, and perhaps lower still.

Table 20 – Prevalence rates among initial refusers and main survey participants – June-Sept sample

	Main	Refusers	All	Chi-sq	Sig
Motor vehicle vandalism	4.8	3.8	4.7	1.72	
Property vandalism	3.6	2.4	3.5	3.30	
Theft from motor vehicle	2.8	1.3	2.6	6.88	*
Attempted theft from motor vehicle	0.6	0.2	0.6	2.11	
Theft of motor vehicle	0.3	0.8	0.3	5.80	*
Bike theft	0.8	0.7	0.8	0.17	
Housebreaking	2.6	2.3	2.6	0.25	
Other household theft	3.3	3.6	3.3	0.24	
Petty assault	4.3	2.4	4.1	7.21	*
Serious assault	0.5	0.1	0.5	2.61	
Sexual assault	0.4	0.3	0.4	0.10	
Theft from the person	1.2	0.8	1.1	1.00	
Robbery	0.5	0.1	0.5	2.42	
Other personal theft	2.4	2.1	2.3	0.29	
Threat	3.0	2.2	2.9	1.67	*
Any household crime	15.7	13.1	15.5	4.17	*
Any personal crime	7.5	4.6	7.2	9.93	*
Base	8,693	870	9,509		

The main conclusion to be drawn from this is that refusal from the telephone survey is biased and that non-victims are less likely to participate in the survey. This tends, overall, to inflate the survey's measure of victimisation and, taking account of the victimisation rates show in Table 17, seems to inflates measures of personal crime more than household crime.

5.5 Conclusion

Analysis of victimisation is limited by the small number of cases of victimisation arising in the face-to-face survey, which makes it difficult to identify any significant differences between the telephone and face-to-face surveys and between the face-to-face survey and the 2003 SCS data. Compared with the 2003 survey, the prevalence and victimisation rates for 'any household crime' in the face-to-face survey are lower. The rates for the telephone survey are higher for personal crime.

Analysis of the exclusion of households without a landline and households subscribing to TPS does not show any bias in terms of measures of victimisation although given the small samples involved, we cannot conclude that this means these have no effect, just that no effect is observed in this data.

Refusal to the telephone survey does appear to have a significant effect on victimisation, biasing the victimisation rates upwards. It is possible that the effect shown above is weaker than might actually be happening in practice i.e. if these refusers have lower rates than non-refusers, it is possible that the rates among those who could not be converted are lower still.

To the extent that the telephone survey is biased in favour of victims, this would be particularly problematic since it suggests that there can be no scope to weight the telephone survey to correct this bias. In Appendix 2 we outline two approaches to developing a weighting solution although we conclude that neither has been successful.

6. Conclusions

At the outset we established four key attributes of a survey which, if satisfied, would provide reassurance that estimates derived from the survey were likely to be an accurate reflection of the population. In this section we restate those attributes and, drawing on the evidence from preceding chapters offer our conclusions about the extent to which the telephone survey appears to be meeting these requirements.

1. A large sample, randomly selected

There are no issues with the sample sizes or sampling method used for the telephone survey.

2. Selection from a sampling frame that completely covers the population of interest

RDD samples, unless they include all mobile phones, automatically exclude a sizeable sub-group of the population that has no telephone or only has access to a mobile phone. This group has quite distinctive characteristics, which are important for two reasons. First, in its own right, the groups who are mobile-only have characteristics that suggest they are more likely to experience victimisation. We have not been able to demonstrate whether victimisation differs among mobile-only households but their exclusion from the survey undermines confidence in the accuracy of the key survey estimates. Secondly, the groups who are excluded by being mobile-only tend also to be groups most likely to be affected by non-participation in the survey. Although this seems to be true of both the telephone survey and the face-to-face survey, the lower response rate to the telephone survey creates more opportunity for this to affect the telephone survey. Analysis of the face-to-face survey does not show significant differences in victimisation for no-phone and mobile only households but the sample size is too small to completely rule out bias in the telephone survey.

More generally, although outwith the scope of this exercise, the emerging evidence of an increasing role for mobile phones as the primary means by which many adults make and receive calls is likely to present increasing problems for RDD surveys in the future.³¹

The exclusion of TPS subscribers was an unfortunate error. While we cannot demonstrate bias on the basis of the sample from January 2005, which contained TPS subscribers, we cannot rule it out.

3. Achieving a high response rate to ensure that the randomness of the sampling is carried into the achieved sample

Based on our estimate of the eligibility of telephone lines where no contact was made, the telephone survey achieved a response rate of approximately 49% and recent figures show that this has been declining. The main reason for the lower response rate is a higher refusal rate – 40% of eligible respondents refuse making the telephone refusal rate 140% higher than the face-to-face survey. Non-contact is lower than the face-to-face survey but in spite of the optimism of the Review, our interpretation of the survey's victimisation rates is that non-victims have been more likely to refuse to take part, leading to the paradoxical situation that although the exclusion of mobile-only households and evidence of bias within the data would lead to an expectation of lower recorded victimisation, the rates recorded by the survey are higher than those recorded by the face-to-face survey and the rates for personal crimes are generally higher than those recorded by the last full sweep of the SCS in 2003. Thus, we feel the Review was wrong to suggest that higher refusal was unlikely to have an impact on the survey measures. Indeed, refusal seems to be the principal cause of a bias in favour of victims.

4. The absence of observable bias

The telephone survey appears to differ from all comparator sources of data to an extent that cannot be explained by sampling error. To that extent we can conclude that the telephone survey appears to be systematically biased. Although one element of this bias will be the type of non-response bias that affects all surveys, there appears to also be a systematic bias against people who have not experienced any form of victimisation.

On the basis of these criteria, we have not found sufficient evidence to conclude that the telephone survey is likely to be accurately measuring victimisation. We have been unable to devise a weighting approach that satisfactorily corrects the many demographic biases that are observable in the data but even if these biases could be corrected, our conclusion is that the telephone survey suffers a more fundamental and irresolvable bias resulting from what appears to be a tendency for non-victims to be more likely to refuse to participate.

³¹ For example, research by Ofcom shows that 21% of adults use a mobile as their main way of making and receiving calls. (Ofcom (2005), *The telecommunications market 2005* chart 3.46 p.135).

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Appendix 1 – Estimating e

Response rates in telephone surveys are complicated by the fact that a large proportion of the telephone numbers generated in the sampling process are not valid numbers, being either business numbers, fax and computer lines or non-working lines. A number of measures can be taken to eliminate invalid numbers such as pre-dialling the numbers to test the validity of lines (known as ‘pinging’), matching the generated numbers against published directories and dialling for interview. These identify the majority of non-working and ineligible lines. However, a proportion of numbers remain that ring when dialled but are never answered or where the line is constantly engaged. These lines may fall into one of five categories:

- they are non-working lines but dialling does not result in the characteristic ‘number unobtainable’ tone
- they are working lines but are non-residential, including telephone boxes, business lines, fax machines etc
- they are working residential lines but the property is between occupants or not a main residence and therefore regularly unoccupied
- they are working residential lines but are regularly connected to the internet or used for calls for long periods or the occupants are always out
- they are working residential lines but are behind a call screening device such as caller ID.

The additional potential category of the number representing a working residential line but the dwelling containing no eligible respondents does not apply in this case since all households would be eligible to participate in the SCVS.

Understanding the composition of these lines is important in determining the response rate for the survey. As the American Association for Public Opinion Research argues, “In estimating e , one must be guided by the best available scientific information on what share eligible cases make up among the unknown cases and one must not select a proportion in order to boost the response rate.”³²

This section attempts to estimate the proportion of eligibility unknown lines from the June-September sample of the SCVS that should be considered eligible for calculating the survey response rates.

³² The American Association for Public Opinion Research. 2004. *Standard Definitions: Final Dispositions of Case Codes and Outcome Rates for Surveys*. 3rd edition. Lenexa, Kansas: AAPOR.

The unadjusted response rate for the June-September sample was 38.4% although a high proportion of the sample would fall into the category of unknown eligibility. Depending on the extent to which unknown eligibility lines are actually eligible, the response rate for the survey might be up to 54%.

The key to estimating the proportion of unknown eligibility that should be considered eligible is the proportion of the sample registered with the Telephone Preference Service. The logic of the estimation is as follows.

- The proportion of all residential lines registered with TPS is known (at April 2005) to be 42%.
- Given the sampling method used for the SCVS, the sample, after deadwood has been removed, should be a random subset of all residential lines in Scotland and should therefore have a TPS registration rate of 42%. We know that:
 - the sample drawn for January 2005 did not have TPS numbers screened out and TPS listed numbers made up 19% of the initial sample
 - after the January 2005 sample had been fully worked, 42% of the sample was identified as deadwood.
- This implies that TPS numbers comprised 33% of the sample that was not identified as deadwood (i.e. $19 \div (1 - 0.42)$).
- If the non-deadwood sample is a random subset of all residential lines, the only reason for the proportion of TPS lines to be lower than the known national proportion is that the non-deadwood sample still contains some ineligible lines. The real response rate is given by multiplying the apparent response rate (38%) by a factor equal to that required to bring the non-deadwood TPS rate into line with the known TPS rate.

$$factor = 38\% \times \left(\frac{42}{33}\right)$$

- This gives a 'real' response rate of 48%

This can be expressed more generally in the following equation, based on the AAPOR terminology.

Assuming partial interviews are counted in the total, the minimum response rate is RR2

$$RR_2 = \frac{I + P}{(I + P) + (R + O) + (UH + UO)}$$

where I = Complete interviews, P = Partial interviews, R = Refusals, O = Other eligible households, UH = Unknown if an eligible household, UO = Other unknown eligibility

This is the same as saying that the response rate is the number of interviews ($I = I+P$) as a proportion of the potentially eligible sample, which is the issued sample (S) minus known ineligible sample (i).

$$RR_2 = \frac{I}{S - i}$$

Using the TPS registration rates we can express the relationship between RR_2 and RR_3 in terms of three values – the TPS rate in the population (T_p), the TPS rate in the issued sample (T_i) and the proportion of the issued sample that is identified as ineligible (i).

$$RR_3 = RR_2 \left(\frac{T_p \times (1 - i)}{T_i} \right)$$

By this method, there is no need to identify e in order to calculate the response rate. However, since response rates are generally reported alongside the proportion of the sample that falls into different categories of eligibility and ineligibility, it is useful to calculate e in order to reapportion sample between the potentially eligible and ineligible categories.

$$RR_3 = \frac{I}{(I + P) + (R + O) + e(UH + UO)}$$

If we call $I + P + R + O$ the known eligibles (E_k) and $UH + UO$ the unknown eligible sample (E_o) we can say that the response rate RR_3 is

$$RR_3 = \frac{I}{E_k + eE_o}$$

therefore

$$e = \frac{I - (RR_3 \times E_k)}{RR_3 \times E_o}$$

Appendix 2 – Corrective weighting

In Chapter 3 we identified the exclusion of mobile-only households as a potential source of bias in the telephone survey. We also compared the response rates to both surveys and considered the potential for differential non-participation, particularly in terms of refusal to introduce bias. In Chapter 4 we examined a number of demographic variables and found that, in comparison with the face-to-face survey, the Scottish Household Survey and the 2001 Census, the telephone survey showed evidence of bias. We also noted that the telephone survey respondents differed significantly in terms of their response to attitudinal variables. The victimisation data outlined in Chapter 5 has borne out this view – victimisation is higher in the telephone survey and more so for personal crimes than for property crimes. Evidence of higher victimisation among respondents to the telephone survey who initially refused to participate also pointed in the direction of a bias in favour of victims of crime.

Non-response weighting is always problematic because while we have a great deal of information about people who respond to the survey, we know very little about those who do not. We can examine the characteristics of respondents and compare these with external data sources (as we do in Chapter 4) and infer the nature of non-response. While this identifies something of the nature and extent of bias on these variables, the real problem of non-response lies in the variables related to those characteristics that the survey seeks to produce estimates – in this case victimisation – about which, by definition, we know little.

We attempted two approaches to weighting. The first is traditional demographic weighting to bring the profile of the sample into line with reliable population estimates. This approach assumes that there is no difference between responders and non-responders in terms of the key survey measure of victimisation. Analysis in the main body of the report clearly concludes that this is not the case, making this approach methodologically unacceptable. The second approach is more unusual: we accepted the presence of bias in terms of victimisation and developed an approach that sought to measure this and calculate weights that would correct it.

Demographic weighting

In the preceding chapters we identified bias in the telephone survey in terms of the following variables:

Tenure – owner-occupiers are over-represented

Property type – households in detached houses are under-represented

Number of people in the household – small households are under-represented

Number of cars or vans in the household – households with no cars or vans are under-represented

Number of bicycles – households with no bicycles are under-represented

The age/sex profile of respondents – men were generally under-represented and older people of both sexes were also under-represented

Respondents' employment status – working households are over-represented.

The crime survey needs separate weights for the analysis of household variables (including household victimisation) and for individual variables, including personal offences. It is not possible, therefore, to develop one set of corrective weights. Correction relevant to the household (such as tenure, property type or the number of adults in the household) needs to precede correction of individual attributes. The sequence of weighting therefore needs to be:

- design weights to correct unequal selection probabilities for households – this addresses disproportionate sampling between PFAs and weighting for the number of landlines available for receiving incoming calls
- corrective weighting for household characteristics – this addresses bias in household variables and, when combined with household design weights, provides a single household weight to use in calculating victimisation rates for household offences.
- design weights for individual characteristics – this addresses unequal selection probabilities related to the number of adults in households.
- corrective weighting for individual characteristics – addressing bias on individual variables and, when combined with the three preceding weights, provides a single individual weight to use in calculating victimisation rates for personal offences.

This gives some indication of the complexity of the task since the effect of each stage of weighting needs to be carried into the next stage. For example, after the proportion of respondents in each PFA has been corrected, corrective weighting needs to be carried out **within** each PFA to stop the corrective weighting from changing the proportions. These combinations of design and corrective weights then form their own weighting classes within which each subsequent stage of weighting needs to be carried out.

In general terms, weighting needs to be carried out within weighting classes that are both comprehensive – all respondents are in a class – and mutually exclusive – each respondent is in only one class. This can combine a number of variables but this type of weighting is subject to practical constraints. First, the reference data need to exist in the same classes so that weights can be calculated. Second, the number of classes needs to be kept within manageable limits. For example, weighting involving three variables each with two categories results in eight weighting classes – the number of possible permutations. The addition of a third variable with two categories increases this to 16 classes. With many variables and many categories, the number of

possible permutations quickly expands to a level that is unmanageable (at least for manual calculations in SPSS) and leads to the potential for empty cells where no weight can be calculated or small cells leading to large weights.

The telephone survey is affected by both of these issues. First, Census data, which would provide ideal reference values for many demographic variables, tends not to be available in complex combinations and probably not in combinations that will reflect the weighting requirements of the survey. Second, the combinations of design weights and corrective weights at both household and individual levels quickly leads to a very large number of weighting classes, as outlined below.

The Census has a further limitation in that while it represents the best estimate of demographic variables around the time it is undertaken, we might expect some change in variables such as tenure and car ownership over the course of four or five years that make continuous reference back to characteristics in 2001 problematic. The need for complex weighting classes and for reference data that changes to reflect underlying change in the population suggested that using the SHS as a source of reference data would be more appropriate than the Census.

Although the SHS is not as accurate as the Census (and comparison with Census data, such as that in Table 12, shows the extent to which it differs) it is generally close to the Census on most variables and closer than the telephone survey appears to be. Using it as a source of reference statistics would be a compromise but one with some advantages. It is based on a very large sample and estimates can be produced at PFA level. Its estimates also have the capacity to move year-on-year in line with underlying change in the population.

The weighting process

To assess which variables would be best for corrective weighting we examined correlation within the group of demographic variables. The variables with the greatest number of significant correlations with other demographic variables were prioritised over those with fewer and those with strong correlations were prioritised over those with weaker correlations. Our objective was to reduce the number of weighting variables to a few that could make a significant contribution to correcting the range of biases evident in the data. For household variables, we prioritised the number of people in the household and tenure. These were collapsed to broader categories: two categories for the number of people ('one or two' and 'three or more') and three tenure categories (owner-occupied, social rented and other) to provide a manageable number of weighting classes – 48 when PFA is included.

As would be expected, this weighting is effective in correcting bias on the variables used to produce the classes but it also appears to be effective in bringing respondent employment status closer to Census estimates of the proportion of working adult aged 16-74 years (bringing the survey estimate down from 51.3% working full time to 47.8% compared with Census estimate

of 46.9%). However, although the proportion of working adults has been improved by weighting, the proportion of retired people among adults aged 16-74 has been pushed higher than the Census estimate when there had previously been a close match between the telephone survey and the Census. Also, the distribution of property types is worse after weighting and this weighting does not appear to have made any significant impact on the bias against households with no car. This is shown in Table 21.

Table 21 – Survey estimates before and after weighting for tenure and number of people in household compared with Census estimates

	Before corrective weighting	After corrective weighting	Census
Tenure			
Owner/occupied	70.3	64.6	62.6
Social rented	21.9	27.3	27.1
Private rented	6.1	6.4	6.7
Other	1.7	1.7	3.5
Property type			
Detached house	20.9	24.5	20.4
Semi-detached house	24.3	25.6	23.5
Terraced house	15.6	16.0	20.2
Flat/maisonette	32.5	30.4	35.6
Other	3.4	3.5	0.3
Number of adults in household			
One	35.7	38.1	
Two	48.5	47.7	
Three or more	15.8	14.2	
Number people in household			
One	27.9	29.8	32.9
Two	34.0	36.0	33.1
Three	17.4	15.6	15.6
Four	14.3	12.8	12.9
Five or more	6.4	5.8	5.6
One or two	61.9	65.8	66.0
Three or more	38.1	34.2	34.0
Number of cars			
None	27.7	28.2	34.2
One	43.0	42.7	43.4
Two	23.8	23.5	18.6
Three or more	5.5	5.5	3.8
Employment status of respondent (aged 16-74)			
In full-time education	6.3	4.2	7.3
Working full-time	51.3	47.8	46.9
Working part-time	14.4	14.0	11.1
Looking after the home or family	6.0	6.2	5.5
Permanently retired from work	13.6	17.7	13.9
Unemployed and seeking work	2.7	2.9	4.0
Permanently sick or disabled	3.8	5.0	7.4
Other (specify)	1.8	2.2	3.9

The main individual characteristic that needs to be weighted is the age/sex profile of respondents. This would, ideally, be done within each of the weighting classes used for household correction but with 2 categories of sex and 8 age categories, the resulting 768

weighting cells ($48 * 2 * 8$) becomes unwieldy and there are likely to be empty cells that further complicate the weighting. The options to avoid this are:

- calculate the age/sex weights independently of the household weighting classes and apply them to the sample as a whole. This risks losing some of the correction already achieved
- reduce the number of age/sex categories, although even if only four categories of age were used there would still be 394 weighting cells.

The simplest solution was the former approach. The disadvantage of the second approach is that it relies on using the SHS as the source of reference data and we know that the SHS under-represents young adults and over-represents older adults. This perhaps makes it unsuitable for this task.

Using the first approach does indeed lose some of the correction that had previously been gained, as Table 22 shows. However, on some, the correction of the age/sex profile takes many of them further from the Census estimate than they were before (highlighted)

Table 22 - Survey estimates before and after weighting for tenure and number of people in household and after addition of age/sex weighting compared with Census estimates

	Before corrective weighting	After corrective weighting at household level	After additional age/sex weighting	Census
Tenure				
Owner/occupied	70.3	64.6	67.8	62.6
Social rented	21.9	27.3	24.5	27.1
Private rented	6.1	6.4	5.9	6.7
Other	1.7	1.7	1.7	3.5
Property type				
Detached house	20.9	24.5	27.1	20.4
Semi-detached house	24.3	25.6	27.1	23.5
Terraced house	15.6	16.0	16.3	20.2
Flat/maisonette	32.5	30.4	26.1	35.6
Other	3.4	3.5	3.4	0.3
Number of adults in household				
One	35.7	38.1	23.9	
Two	48.5	47.7	51.4	
Three or more	15.8	14.2	24.7	
Number people in household				
One	27.9	29.8	18.7	32.9
Two	34.0	36.0	37.2	33.1
Three	17.4	15.6	18.4	15.6
Four	14.3	12.8	17.0	12.9
Five or more	6.4	5.8	8.7	5.6
One or two	61.9	65.8	55.8	66.0
Three or more	38.1	34.2	44.2	34.0
Number of cars				
None	27.7	28.2	24.6	34.2
One	43.0	42.7	40.4	43.4
Two	23.8	23.5	26.9	18.6
Three or more	5.5	5.5	8.1	3.8

	Before corrective weighting	After corrective weighting at household level	After additional age/sex weighting	Census
Number of bicycles				
Any	40.9	41.1	43.7	
None	59.1	58.9	56.3	
Employment status of respondent (aged 16-74)				
In full-time education	6.3	4.2	6.2	7.3
Working full-time	51.3	47.8	50.2	46.9
Working part-time	14.4	14.0	13.0	11.1
Looking after the home or family	6.0	6.2	5.6	5.5
Permanently retired from work	13.6	17.7	15.9	13.9
Unemployed and seeking work	2.7	2.9	3.0	4.0
Permanently sick or disabled	3.8	5.0	4.1	7.4
Other (specify)	1.8	2.2	1.9	3.9

This leads us to conclude that simultaneously correcting the many biases in the telephone survey will require a more sophisticated approach than simply using population-based ratios for single variables or even combinations of a number of variables. Rim weighting is able to calculate weighting factors based on a number of individual variables with know population totals. For example, rim weights based on age, sex and housing tenure would be calculated by a computer program iteratively adjusting the weighting factors applied to each case in a way that brings the sample distribution of each of these variables into line (or as close as possible) with the population distribution.

The second conclusion is that since we cannot find a weighting solution, we are unable to look at the impact of corrective weighting on victimisation.

Weighting to correct for bias in victimisation

In the main report we concluded that the telephone survey over-represents victims resulting in higher rates of victimisation than would be expected given the exclusion of mobile-only households and demographic differences between the RDD sample and comparator data. Comparison of the face-to-face survey with 2003 data and recorded crime data suggested that that survey was under-representing victims.

This approach tries to estimate the extent to which the surveys might be biased in favour of, or against, households with characteristics that are related to experiencing both household and personal victimisation.

The approach

If the telephone survey is systematically biased towards victims, corrective weighting that attempted to address this by matching survey and reference demographics would be unlikely to correct this bias. The underlying assumption that responders were like non-responders in terms of key survey estimates – experience of victimisation – was not true.

Reflecting on this, we attempted an alternative approach that might address any ‘victim’ bias more directly. If we could establish that the RDD sample was biased in favour of victims and estimate by how much, we would be able to use this as the basis for corrective weighting. The approach (for correction of household victimisation) was as follows:

1. respondents to the telephone survey were assigned to one of two groups based on their experience of any incidents of household victimisation (any or none)
2. SPSS Answertree was used to assign households to groups whose characteristics were related to experience of victimisation
3. the resulting combinations of characteristics were used to assign households to weighting classes
4. the same classes were created in the face-to-face survey and also in the Scottish Household Survey, which would be used as reference data for both samples
5. comparison of the proportion of households in each class in both datasets allowed us to calculate weights.

This approach relies on two assumptions. First, that the SHS is suitable for reference data i.e. that it is not substantially biased. This would be problematic if the SHS and the face-to-face survey were similarly biased on demographic characteristics and the telephone survey was not. This would lead to the paradoxical situation of the face-to-face survey requiring no corrective weighting and the telephone survey requiring substantial “correction”. Table 12 shows that the SHS is broadly in line with 2001 Census estimates. We know the SHS under-represents young people in general and young men in particular so before calculating weights for personal victimisation, we corrected the age/sex profile of the SHS sample using the 2001 age/sex profile of adults in Scotland. The second key assumption is that participation in the SHS is not related to household or personal victimisation. If it were, it could not provide a basis to calculate weights related to victimisation.

Results

Table A1 shows that in the telephone survey the weighting classes that are most likely to experience household victimisation (index > 100) are being weighted down, with an average weight of 0.88. This suggests that overall, the telephone survey over-represents victims of household crime. This table also suggests that overall the face-to-face survey does not tend to over or under-represent households with these characteristics. For both surveys there are some groups that attract quite large weights.

The same exercise was run for personal victimisation using the variables included in the household correction and a range of individual respondent characteristics. The results of this are shown in Table A2. This suggests that the telephone survey also over-represents individuals with characteristics related to higher victimisation but with an average down-weighting factor of only 0.96 this weighting is unlikely to have any significant impact on victimisation rates.

Table A2 also suggests that the face-to-face survey under-represents individuals with victimisation-related characteristics and with an average weight of 1.15, this weighting should increase the victimisation rates for personal incidents recorded in the face-to-face survey.

7.1.1 Impact on sample demographic profiles

Table 4 shows that the addition of household corrective weighting in the telephone survey has some beneficial impact on characteristics such as:

- the proportion of households in social rented tenures
- the proportion of households in flats
- the number of single adult households
- the proportion of one or two person households
- the proportion of households with no car.

It has a negative effect on:

- the proportion of households in remote rural areas

However, there remain substantial unresolved gaps in the demographic profile compared with the SHS. We estimate that the combination of design and corrective weighting has removed 70% of the difference between the reference data and the unweighted telephone survey data. Most of this is done by the design weighting (66%) with corrective weighting only adding another 4% improvement.³³

The addition of individual corrective weighting has improved the proportion of adults living in rented tenures, the proportion of adults with no car, the proportion adults in flats, the proportion in one or two adult households, the proportion of adults in one or two person households, the proportion of adults working full time and the proportion permanently retired from work. We estimate that while the individual design weight removes 51% of the differences between the telephone survey and the SHS, the addition of corrective weighting increases this to 68%.

For the face-to-face survey, as would be expected given the small average weight for household correction, this weighting has almost no impact on the demographic profile of the sample.

Individual weighting has the paradoxical effect of making a number of measures worse than before – taking them further away from the reference data – and taking very few measures closer.

³³ Calculated as the percentage reduction in the mean square error between the reference data and the unweighted data and the reference data and the weighted data.

However, it needs to be borne in mind that the weighting is not aimed at correcting the surveys' demographic profiles but at correcting any bias in victimisation. Since it acts on combinations of variables there might be instances where, for example, a combination of tenure and car ownership is weighted up and another combination of tenure and urban/rural classification is weighted down. The impact on the tenure profile might be neutral.

If we were confident that the weighting was adequately addressing bias in victimisation, it might be reasonable to conclude that in terms of what the survey seeks to measure, the remaining demographic anomalies could be ignored.

Impact of weighting on victimisation rates

The implications of tables A2 and A3 are that the victimisation rates from the telephone survey should fall for household incidents and be unchanged for personal incidents. The face-to-face survey should have largely unchanged household victimisation rates but higher personal victimisation rates.

Table A5 shows that the rate for all property offences recorded in the telephone survey has fallen, although only by a small amount. The rate for personal offences has also increased very slightly. For the face-to-face survey, the rate for property offences has fallen slightly and there has been an increase in the rate for personal offences.

On individual offence categories, the corrective weighting has narrowed the gap between the surveys in 10 of the 15 categories but by a substantial amount in only a few.

Compared with the 2003 SCS, the telephone survey rates remain lower for property offences (-13%) and higher for personal offence (33%) while the face-to-face survey remains lower still for property offences (-25%) and also lower for personal offences (-13%).

Conclusion

Even if demographic weighting had worked in correcting the sample profile, it would need to be rejected on the grounds that the basic assumption it relied on – of there being no difference between responders and non-responders – was patently untrue. While we think the approach focusing on correcting bias in victimisation might have had some theoretical potential, it has done little to change the victimisation rates in either survey and the 'victim' bias remains unresolved by this approach.

Description	Telephone survey			Victim index (all = 100)	Calibration survey		
	Unweighted n	Weighted n	Weight		Unweighted n	Weighted n	Weight
Any cars, missing info on urb/rur and proptype	103	97	0.95	213			
Any cars, urban areas, missing tenure or not owned outright	3211	2761	0.86	150	926	931	1.01
Any cars, not large urban or remote rural, not owner occupier	388	342	0.88	124	126	115	0.92
No car, more than 2 people in household, one adult	426	352	0.83	111	97	119	
No car, not detached house	312	295	0.95	105	3	3	1.00
No car, single adult	118	168	1.43	104	51	57	1.11
Any car, urban area, owned outright	821	1005	1.22	98	365	339	0.93
Any car, not urban, not remote rural, owner	980	801	0.82	92	318	270	0.85
Any car, remote rrural, three or more people in household	115	95	0.82	87	33	32	0.97
Any car, not urban, not remote rural, owned outright	478	522	1.09	67	195	176	0.90
No car, single adult, semi-detached	969	1498	1.55	56	449	505	1.13
Any car, detached house	138	130	0.95	55	1	1	1.00
No car, two adults in household, no children	617	677	1.10	54	166	228	1.37
Any car, more than two people in household, more than two adults	206	180	0.87	48	55	61	1.10
Any cars, rural, three or fewer in household	336	282	0.84	44	143	95	
No car, one person, not detached house	289	304	1.05	37	106	102	0.97
	9509	9509		100	3034	3034	
Overall			average weight				average weight
Nodes with likelihood > 100%	4558	4016	0.88		1203	1225	1.02
Nodes with likelihood < 100%	4950	5493	1.11		1831	1809	0.99

Table 3 - Adult Correction	Telephone survey			Victim index (all = 100)	Calibration survey		
	Description	Unweighted n	Weighted n		Weight	Unweighted n	Weighted n
Age missing, 3+ adults	87	38	0.44	300			
<25 years, male, large urban	265	298	1.12	271	81	99	1.23
25-34, not owner, most deprived decile	118	117	0.99	268	40	39	0.96
Age missing, 1 or 2 adults, top 40% deprived	135	169	1.26	217			
<25 years, male, not large urban	327	320	0.98	182	89	106	1.19
35-44, retired or UE or sick, male	72	116	1.61	180	37	39	1.05
Aged < 25, not male	628	617	0.98	167	160	205	1.28
35-44, work FT or FTE, no urb/rur class	100	94	0.94	166	3	3	1.00
25-34, not owner, not most deprived decile	374	423	1.13	161	123	141	1.14
35-44, retired or UE or sick, not male	83	81	0.98	140	27	27	0.99
35-44, working FT or full-time ed (FTE), urban or accessible small	948	883	0.93	136	273	293	1.08
45-54, not detached or semi, work FT or FTE	433	382	0.88	131	113	127	1.12
25-34, male, owner	524	514	0.98	125	135	171	1.26
35-44, work PT or looking after home, dep dec < 7	341	264	0.77	116	100	88	0.87
Age missing, 1 or 2 adults, not top 40% deprived	78	87	1.11	111			
25-34, not male, owner	591	521	0.88	107	133	173	1.30
45-54, not detached or semi, not (work FT or FTE)	321	328	1.02	94	112	109	0.97
35-44 years, detached house	1022	850	0.83	77	317	282	0.89
35-44, work FT or FTE, not urban or accessible small town	289	230	0.80	77	86	77	0.89
55-74, FT or FTE, not remote rural	431	391	0.91	72	165	130	0.79
55-74, not (FT or FTE), top 20% dep	330	418	1.27	70	136	139	1.02
35-44, work PT or looking after home, dep dec 7+	251	191	0.76	50	87	63	0.73
55-74, not (FT or FTE), not top 20% dep	1312	1402	1.07	30	526	466	0.89
55-75, FT or FTE, remote rural	103	35	0.34	27	17	11	0.67
Aged 75+	344	740	2.15	16	273	246	0.90
	9509	9509			3034	3034	
Overall			average weight				average weight
Nodes with likelihood > 100%	5105	4925	0.96		1315	1511	1.15
Nodes with likelihood < 100%	4404	4584	1.04		1719	1520	0.88

Table 4 - Weighting outcomes	TELEPHONE SURVEY							CALIBRATION SURVEY				
	Household data				Individual data			Household data		Individual data		
	Reference SHS	Unweighted	Design weight	Corrective weight	Reference SHS	Design Weight	Corrective weight	Unweighted	Corrective weight	Reference SHS	Design Weight	Corrective weight
Property type												
1.00 Detached house	19.8	25.8	20.9	21.1	22.2	24.4	23.4	24.5	23.8	22.2	27.6	26.1
2.00 Semi-detached house	21.9	25.3	24.2	23.3	23.6	25.9	25.3	22.3	21.9	23.6	23.0	22.5
3.00 Terraced house	22.2	15.4	15.7	15.3	22.5	16.3	16.4	21.3	21.3	22.5	21.4	21.9
4.00 Flat/maisonette	35.8	27.1	32.3	33.7	31.4	26.8	28.4	31.8	33.0	31.4	28.0	29.5
5.00 Other	0.3	3.4	3.4	3.6	0.3	3.5	3.5	0.0	0.0	0.3	0.0	0.0
Total	100.0	97.0	96.6	97.0	100.0	96.9	97.0	100.0	100.0	100.0	100.0	100.0
Missing data	0.0	3.0	3.4	3.0	0.0	3.1	3.0	0.0	0.0	0.0	0.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Number of adults in household												
1	37.0	33.1	34.5	38.6	22.7	18.3	21.0	37.2	38.0	22.7	21.0	20.9
2	48.8	50.2	49.0	46.4	53.6	51.9	51.1	48.5	48.3	53.6	53.6	52.8
3	10.1	11.2	11.0	10.1	15.1	17.5	16.5	10.2	9.7	15.1	16.2	16.6
4	3.6	4.5	4.4	4.0	7.1	9.3	8.7	3.5	3.4	7.1	7.5	7.9
5	0.5	0.9	0.8	0.7	1.2	2.1	2.0	0.6	0.5	1.2	1.5	1.6
6	0.1	0.1	0.2	0.1	0.3	0.5	0.5	0.1	0.1	0.3	0.2	0.3
7	0.0	0.0	0.0	0.0	0.1	0.2	0.2	0.0	0.0	0.1	0.0	0.0
9	0.0	0.0	0.0	0.0	0.0	0.1	0.1	0.0	0.0	0.0	0.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Number of adults - summary												
One or two	85.7	83.3	83.5	85.0	76.2	70.2	72.1	85.7	86.3	76.2	74.6	73.7
Three or more	14.3	16.7	16.5	15.0	23.8	29.8	27.9	14.3	13.7	23.8	25.4	26.3
Total household size												
1	31.3	26.0	26.9	31.4	19.2	14.3	16.6	31.3	31.8	19.2	17.6	17.3
2	34.7	33.9	33.4	33.3	37.2	33.8	34.8	35.2	35.3	37.2	37.4	36.2
3	14.9	16.9	16.7	14.9	18.0	20.3	19.3	15.7	15.3	18.0	19.5	20.2
4	13.5	13.9	13.6	12.0	17.6	18.8	17.2	13.2	13.0	17.6	18.5	19.0
5	4.2	4.7	4.5	4.0	5.9	7.0	6.5	3.4	3.4	5.9	5.3	5.5
6	1.0	1.1	1.1	0.9	1.6	1.8	1.7	0.8	0.8	1.6	1.2	1.3
7	0.2	0.2	0.3	0.2	0.3	0.5	0.5	0.3	0.3	0.3	0.4	0.4
8	0.1	0.1	0.1	0.0	0.2	0.2	0.2	0.0	0.0	0.2	0.0	0.0
9	0.0	0.1	0.1	0.0	0.0	0.2	0.2	0.0	0.0	0.0	0.0	0.0
10	0.0	0.0	0.0	0.0	0.0	0.1	0.1	0.0	0.0	0.0	0.0	0.0
Total	100.0	96.9	96.6	97.0	100.0	96.9	96.9	100.0	100.0	100.0	100.0	100.0
Missing data	0.0	3.1	3.4	3.0	0.0	3.1	3.1	0.0	0.0	0.0	0.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Total household size - summary												
One or two	66.0	59.9	60.4	64.7	56.4	48.1	51.4	66.5	67.1	56.4	55.0	53.5
Three or more	34.0	37.0	36.2	32.3	43.6	48.8	45.5	33.5	32.9	43.6	45.0	46.5

Table 4 - Weighting outcomes

	TELEPHONE SURVEY							CALIBRATION SURVEY				
	Household data				Individual data			Household data		Individual data		
	Reference SHS	Unweighted	Design weight	Corrective weight	Reference SHS	Design Weight	Corrective weight	Unweighted	Corrective weight	Reference SHS	Design Weight	Corrective weight
Number of cars available to household												
.00 None	35.3	25.2	27.6	32.2	28.7	22.1	24.8	30.5	34.1	28.7	26.3	26.4
1	44.9	42.8	43.0	40.3	46.0	40.2	40.6	41.2	38.7	46.0	38.3	38.1
2	17.2	25.6	23.8	22.2	21.3	28.7	26.3	22.7	21.7	21.3	27.2	27.1
3.00 Three or more	2.5	6.3	5.6	5.3	4.0	8.9	8.3	5.7	5.5	4.0	8.2	8.4
Total	100.0	99.9	99.9	100.0	100.0	99.9	99.9	100.0	100.0	100.0	100.0	100.0
Missing data	0.0	0.1	0.1	0.0	0.0	0.1	0.1	0.0	0.0	0.0	0.0	0.0
Total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Employment status of random respondent (aged 16-74)												
1 In full-time education	5.8	4.4				6.3	6.6				5.4	6.5
2 Working full-time	44.2	49.5				50.9	48.5				44.6	46.0
3 Working part-time	11.8	14.3				14.5	13.9				13.1	12.3
4 Looking after the home or family	8.5	6.2				6.1	5.8				8.1	7.9
5 Permanently retired from work	18.9	16.4				13.5	15.6				18.9	17.2
6 Unemployed and seeking work	3.7	2.7				2.8	3.1				4.1	4.3
7 On a government work or training sche	0.1	0.1				0.2	0.2				0.2	0.2
8 Permanently sick or disabled	5.7	4.1				3.8	4.3				4.2	4.1
9 Temporarily unable to work because of	0.8	1.1				0.9	1.1				1.3	1.4
10 Other	0.5	0.4				0.4	0.4				0.0	0.0
Total	100.0	99.3				99.3	99.4				100.0	100.0
Missing data	0.0	0.7				0.7	0.6				0.0	0.0
Total	100.0	100.0				100.0	100.0				100.0	100.0

Table 5 - impact of weighting on victimisation rates

	Telephone (T1) design only	Telephone (T2) corrective	Calibration (C1) design only	Calibration (c2) corrective	2003 SCS (S)	Difference (T1-C1)/C1	Difference (T2-C2)/C2	Weight impact
Motor vehicle vandalism	785.4	737.5 -	768.0	720.9 -	825.9	2%	2%	None
Property vandalism	709.6	735.7 +	550.4	543.6 -	830.5	29%	35%	Worse
Theft from motor vehicle	356.2	330.9 -	309.8	291.7 -	407.8	15%	13%	Better
Attempted theft of/from mv	76.2	73.3 -	65.9	62.2 -	125.6	16%	18%	Worse
Theft of mv	35.8	40.8 +	62.6	59.1 -	90.9	-43%	-31%	Better
Bicycle theft	124.9	104.1 -	85.7	89.1 +	131.9	46%	17%	Better
Housebreaking	345.4	357.8 +	263.7	269.9 +	397.5	31%	33%	Worse
Other household theft	512.4	500.9 -	431.8	435.5 +	499.1	19%	15%	Better
Petty assault	705.9	730.0 +	420.2	483.4 +	435.4	68%	51%	Better
Serious assault	69.0	77.7 +	49.3	58.7 +	114.8	40%	32%	Better
Sexual offences	56.2	59.6 +	24.6	25.9 +	1.8	128%	130%	Worse
Theft from person †	384.7	380.0 -	270.3	302.8 +	59.4	42%	25%	Better
Robbery	50.7	50.3 -	15.8	18.3 +	48.4	220%	175%	Better
Other personal theft †	21.9	22.6 +	10.3	11.3 +	260.9	112%	100%	Better
Threats	523.8	535.6 +	283.1	310.3 +	473.2	85%	73%	Better
All property	2945.7	2881.1 -	2537.9	2471.9 -	3309.2	16%	17%	Worse
All personal	1812.2	1855.7 +	1073.7	1210.6 +	1394.0	69%	53%	Better
All offences	4757.9	4736.8 -	3611.6	3682.5 +	4703.2	32%	29%	Better