

**Comparability of the Crime  
Surveys in the UK:  
A Comparison of Victimisation  
and Technical Details**

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## **1. INTRODUCTION**

1.1 The ability to compare crime rates using recorded offences is limited because crimes are defined and recorded in different ways across the different jurisdictions within the UK.

1.2 Crime surveys offer a more standardised approach to comparing crime rates and trends. The population of the UK is covered by three separate crime surveys; the British Crime Survey (BCS) which covers England and Wales; the Scottish Crime Survey (SCS) and the Northern Ireland Crime Survey (NICS). The geographic coverage of these surveys reflects the three jurisdictions within the UK. This paper aims to map differences between the BCS, SCS<sup>1</sup> and NICS which may influence the estimates of victimisation. Although this paper concentrates on victimisation rates the analysis presented could be expanded to other substantive areas, for example fear of crime or perceptions of the criminal justice system. This paper is based primarily on an analysis of information contained in the published technical reports for each survey. To that extent we have been limited by how the ranges of topic covered, and level of detail given, varied between technical reports. While these differences do not directly affect the comparability of the different surveys, they limit researchers' ability to conduct comparative research because it is not immediately apparent the extent to which the surveys may vary in terms of methodology and content.

1.3 The issues covered in this paper can be grouped into three broad categories:

- The sample: who and how many people are asked?
- The questions: what are people asked about? Are they asked in the same way?
- Coding: how are the answers people provide turned into data?

1.4 Apart from a brief overview of the historical development of the different surveys, this paper will focus on the most recent surveys for which technical documentation was available at the time of writing<sup>2</sup>. It is important to note that other differences may exist when comparing surveys from other years and that the

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1 Starting in 2007, Scotland will be undertaking a new survey, called the Scottish Crime and Justice Survey (SCJS). Although it is hard to cover the new survey in detail in the report, reference will be made to several salient differences between the SCS and SCJS.

2 BCS 2005-06, NICS 2005 and SCS 2006

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comments made in this paper are largely based on a reading of the appropriate technical reports rather than comparisons of practical analysis.

## **2 GENERAL HISTORICAL DEVELOPMENT OF DIFFERENT SURVEYS**

2.1 While this paper is primarily concerned with the comparability of estimates of victimisation rates across the most recently available BCS, SCS and NICS, a cursory review of the historical development of the different surveys serves to illustrate some of the reasons why comparability of estimates may be problematic. The first BCS was conducted in 1982 and included data covering England, Wales and Scotland. Fieldwork in Scotland was conducted by the same company as that in England and Wales and used an identical questionnaire. The absolute sample size was smaller in Scotland although larger in relative terms (5,000 as opposed to 10,000). Fieldwork in Scotland only covered the southern part of the country limiting the ability to undertake national comparisons with England and Wales. While the BCS was re-run in 1984, using a similar methodology to that in 1982, Scotland did not participate. The 1988 BCS was broadly comparable to that conducted in 1982 and once again included areas of Scotland south of the Caledonian Canal. At this time, comparisons between Scotland and the rest of Great Britain were limited by difficulties in ensuring the reliability of the data referring to Scotland. This meant that while results from the 1988 BCS in England and Wales were available quite early, similar results for Scotland were not published until 1992. No crime survey data for Northern Ireland was collected in the 1980s.

2.2 The first BCS of the 1990s was conducted in 1992 and once again involved a core sample of around 10,000 respondents, this time drawn exclusively from England and Wales. The first independent SCS was conducted in 1993 (a year later than the BCS due to delays associated with the development of the survey). The sampling frame for this survey was extended to cover the whole of Scotland but once again a sample size of around 5000 was employed. Although similar to the 1992 BCS questionnaire, the 1993 SCS included several differences aimed at reflecting the differing context associated with the Scottish criminal justice system. The SCS sampling strategy and questionnaire design remained little altered for the 1996, 2000, and 2003 SCS. The BCS, now exclusively concentrating on England and Wales, was repeated regularly after 1992 (94, 96, 98, 2000) with a steadily increasing sample size. The core-sample size for the 2000 BCS was around 20,000. Booster samples aimed at providing more accurate estimates for ethnic minorities and young people were also regularly included. The interview strategy of the BCS was changed in 1994 to one using Computer Assisted Personal Interviews (CAPI) and Computer Aided Self Interviewing (CASI) rather than paper questionnaires. The increased

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sample size of the BCS also allowed respondents to be split into smaller sub-samples who were asked questions about different topics (victimisation questions are always asked of the full sample).

2.3 A series of ad hoc crime surveys were conducted in Northern Ireland beginning in 1994/95<sup>3</sup>. Further surveys were conducted in 1998, 2001 and 2003/4. All these surveys had achieved sample sizes of just over 3000 and were conducted using CAPI and CASI. The questionnaire used for NICS was closely modelled on that used for the BCS but the smaller sample size meant it was not possible to split the sample to cover as wide a range of topics as the BCS.

2.4 In 2001 the BCS moved to a continuous survey, sampling throughout the year, a move followed by NICS in 2005. The current BCS has a sample of around 47,000 (per annum) while the NICS has a target sample of 4000 cases per year. As in the past, the NICS questionnaire is designed to closely mirror that of the BCS.

2.5 The 2004 SCS involved two separate surveys, a large sample telephone survey and a smaller face-to-face calibration survey. Following concerns about the reliability of the data collected in the telephone survey the SCS reverted to personal interviews, now based on CAPI and CASI, a methodology that reflects those of the BCS and NICS. The last SCS was conducted in 2006. The next sweep of the SCS, currently being developed (SCJS), will see the survey follow the BCS and NICS in moving to a continuous collection methodology and will also see an increase in sample size which may allow for the splitting of respondents into smaller sub-samples to cover a wider range of topics.

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<sup>3</sup> Prior to this date questions concerning experience and perceptions of crime and security issues were included in other surveys such as the Community Attitudes Survey.

### 3 THE SAMPLE

3.1 There are many issues around the design of surveys which can influence the reliability and comparability of the estimates they provide (for instance, sample size, sampling design, the characteristics of the population surveyed and the amount of sample coverage). If estimates of victimisation from different surveys are to be comparable it is important that they are representative of comparable populations (or at least that any differences are so negligible as to have no impact on any aggregate level estimates produced) and that their sampling strategies do not introduce bias to their estimates (or at least that any bias is estimated and accounted for). For instance, at first glance, it appears that if one survey included students living in halls of residence and another did not then this could affect the comparability of victimisation rates, as the students who live in such residences are likely to experience different patterns of victimisation from the rest of the population. However only a very small proportion of the population aged in their late teens and early twenties would reside in university halls. Therefore, the different treatment they receive from different surveys is only likely to have a very marginal effect on overall estimates of victimisation. Similarly, the nature of any stratification or weighting in the sampling strategy may influence the precision with which estimates are made using the different surveys, and it will be necessary to account for these differences when establishing whether significant differences exist between jurisdictions.

3.2 Table 1 provides an overview of the sampling design for the three UK crime surveys; focusing on size, target population and selection method. For the most part the sampling strategies employed appear very similar and this suggests that comparison between the surveys should be possible. Despite this it is worth further exploring those differences that do exist to identify if they may impact on estimates of victimisation.

3.3 It is important to note that the analysis presented in this paper refers only to those surveys listed in Table 1. Many aspects of sampling appear to remain constant over time, for instance, comparing NICS 2005 with NICS 2007/08<sup>4</sup> shows that the percentage of addresses found to be eligible for the survey is 89.8 rather than 89.4 and the response rate is 65.0% rather than 67.5%. However, surveys do occasionally undergo more radical redesigns which could influence comparability not only between the different surveys but also within one jurisdiction over time. The case of Scotland, where a new survey, known as the Scottish Crime and Justice Survey (SCJS), was introduced in 2008 is an example of this. Data and detailed technical

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<sup>4</sup> Data provided directly by NICS researchers.



information about SCJS will not be available until autumn 2009. Therefore, any detailed assessment of how this survey compares can not yet be made. However, several differences (compared to SCVS 2006) are worthy of note, in particular the move to continuous sampling (as was already the case with BCS and NICS), and changes to the clustering structure which was employed when identifying target addresses (notable here is the decision to use slightly different levels of clustering in urban and rural areas). The impact of these changes will require further investigation once data from the first year of SCJS is released.

## **Target Population and Sampling Frame**

3.4 The target population for all three samples is identical, covering those aged 16 and over who live in private households while excluding those who live in institutions such as prisons, care homes and university halls of residence. One notable difference between the surveys is that the BCS often include booster samples for certain sub-sections of the population (for instance ethnic minorities and young people). These booster samples are selected using a distinct sampling strategy and are not used when calculating overall victimisation rates. As such, they do not affect the comparability of victimisation across surveys and are not discussed in this report.

3.5 A key issue in determining the accuracy with which an estimate arrived at through a survey will reflect a population is how complete, and unbiased, the sample frame is (DeVaus, 2002, pp73-75). Both the BCS and SCS use the Royal Mail's Small User Postcode Address File (PAF) as their sampling frame<sup>5</sup>.

3.6 The PAF is widely used as a sampling frame for large scale, nationally representative surveys and is the most comprehensive listing of addresses available in the UK (Foster, 1994). PAF is updated monthly to maintain its accuracy and, while no sampling frame is likely to be 100% accurate, Foster (1984) shows that any inaccuracies in PAF appear to be unbiased in terms of the demographic characteristics of those who are missing.

3.7 The NICS sample is drawn from the Land and Property Services list of private addresses. The Central Survey Unit of the Northern Ireland Statistics and Research Agency (NISRA) uses the Land and Property Services (LPS) domestic property

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<sup>5</sup> With the introduction of the Scottish Crime and Justice Survey (SCJS), the sampling frame for Scotland will change to take account of the Multiple Occupancy Indicator (an additional field within PAF). This is believed to better reflect the nature of the housing stock within Scotland which includes a higher proportion of flats. Its use should help improve the information available for any subsequent weighting of the data

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database for most of its surveys. Historically, partly because of coverage issues with the PAF in rural areas of NI and partly because it is actively used in the calculation of rates bills, meaning it has been viewed as the most accurate and comprehensive register of addressees available. Overall, both sampling frames (PAF in England and Wales and Scotland and LPS in Northern Ireland) appear to provide good levels of coverage capturing a very high proportion of addresses relevant to their respective surveys. Overall, the different surveys use of alternative sampling frames can be seen as a result of them employing the most accurate sampling frame available for their jurisdiction (and reflects the best practice employed by other large-scale household surveys in their countries).

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**Table 1: Overview of Sampling Design in UK Crime Surveys**

Aspect of Sampling	British Crime Survey 2005/06	Northern Ireland Crime Survey 2005	Scottish Crime Survey 2006
Target Sample Size	46000 (minimum 1000 in each police force area)	4000	5000
Eligible Percentage	91.7	89.4%	90.1%
Response Rate	75.2%	67.5%	70.5%
Actual Sample	47479	3,692	4948
Sampling Frame (Households)	Small User Postcode Address File.  Excludes those in institutions such as halls of residence, care homes, prisons or the armed forces.	Land and Property Services list of private addresses.  Excludes those in institutions except those who live in private households on such sites.	Small User Postcode Address File.  Excludes those in institutions such as halls of residence, care homes, prisons or the armed forces.
Sampling Frame (Individuals)	Random adult aged 16 and over in chosen household (no replacements allowed). See also youth booster sample below.	Random adult aged 16 and over in chosen household (no replacements allowed).	Random adult aged 16 and over in chosen household (no replacements allowed).
Stratification	Postcodes sectors assigned to Police Force Area giving two levels of clustering.  Population density and proportion of those aged 16-74 in non manual occupations	n/a	Postcodes sectors assigned to Police Force Area giving two levels of clustering.  Population density and proportion of those aged 16-74 in non manual occupations
Clustering	PSU was postcode sectors (sectors with less than 500 addresses joined with neighbours). 32 addresses per PSU. 50% PSU from previous survey maintained but with different addresses.  Only 16 addresses issued for PSUs where non-white population above 26% in 2001 census. – compensated for by duplication of PSUs	n/a	PSU was postcode sectors (sectors with less than 500 addresses joined with neighbours). 22 addresses per PSU.
Timing of Survey	Continuous – April 2005 to June 2006. Allows estimates each quarter.	Continuous – January 2005 to December 2005	June to December 2006 with a reference period since April 2005
Booster Sample	Booster sample for non-white respondents – selected through focussed enumeration (target 3000 interviews).  Booster sample for those aged 16-24. Selected from core households where the “main respondent” was not aged 16-24 and other people of this age are present in the property (target 2000 interviews).	None	None

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3.8 Ideally, any consideration of how accurate and effective a sampling frame is should consider both the extent to which it includes all the addresses relevant to a samples target population (i.e. its coverage), and the extent to which addresses it lists are found to be appropriate for the survey, for instance they are not derelict or demolished. While exact data is hard to find, as outlined above, both PAF and LPS are considered amongst the best sampling frames for their countries in terms of coverage. Looking at the percentage of investigated addresses which were found to be ineligible also suggests that there is no major difference in the effectiveness of the Northern Ireland sampling frame as opposed to that for the BCS or SCS (89.4% eligible addresses in contrast to 91.7 and 90.8%). It is possible that Northern Ireland's slightly lower eligibility rate is due to a combination of the absence of a statutory requirement to notify demolitions and the fact that, historically, no rates are payable on vacant or derelict properties. Table 2 provides a breakdown of why addresses were found to be ineligible across the different surveys. The figures for the BCS and SCS are taken from the relevant technical reports while those for NICS were provided via personal communication. Overall, the percentages in Table 2 appear to broadly reflect the differences in the overall eligibility rates between surveys. Despite some variation in the relative prevalence of the different reasons no major differences can be identified between the surveys.

**Table 2: Reasons for Ineligible Addresses Across UK Crime Surveys**

<b>Reason for Ineligibility</b>	<b>British Crime Survey 2005/06</b>	<b>Northern Ireland Crime Survey 2005</b>	<b>Scottish Crime Survey 2006</b>
Addresses not traced	0.8%	0.6%	1.0%
Not build/ does not exist	0.1%	0.4%	0.1%
Derelict/demolished	0.4%	1.2%	0.9%
Empty/vacant	4.6%	5.4%	4.6%
Second home/not main residency	0.8%	0.6%	1.1%
Business/industrial	1.0%	0.9%	1.5%
Institution/communal establishment	0.2%	0.1%	0.2%
Other	0.4%	1.2%	0.3%

Figures show percentage of total addresses issued for survey

### ***Non-Response***

3.9 Another potential source of bias is in terms of who chooses to answer the survey, which may influence the accuracy and comparability of estimates of victimisation. To some extent the use of weighting can help to adjust for non-response. However, while weighting may help to correct for non-response it can lead to biased estimates where either rare events or uncommon groups of respondents are concerned because it assumes that the few examples included in the dataset are representative of the wider population (see McVie, Norris and Raab, 2006). Although, it would be useful to compare reasons for non-response and the characteristics of those who are choosing not to answer across the surveys these data are recognisably difficult to collect and the three technical reports vary substantially in how much information they provide about non-responders. The technical report for NICS gives an overall response rate of 67.5%, slightly below that for the other two surveys, but gives no details of the reasons for non-response. Both the BCS and SCS reports provide some details about why respondents did not respond, for instance refusal, illness, missed appointment. The BCS supplements this with a breakdown of non-response by geographical region, type of property and the nature of the area in which they live. The BCS report notes that non-response was greatest amongst those living in flats (due to access difficulties) and those living in areas which had litter lying around, vandalism or poor condition housing. As wider analysis has linked the likelihood and nature of victimisation to an individual's social context it is possible that these patterns of non-response may influence the estimates which are achieved. It would therefore be useful to create a table, similar to Table 2, which considered the reasons for non-response.

3.10 Lynn (1998) suggests that one strong way to establish if non-response introduces potential bias is to compare the characteristics of those who do respond with those who choose not to. This can be difficult given that, by definition, little information is known about non-respondents. Different techniques for comparing respondents and non-respondents to crime surveys has been an area extensively investigated with reference to the BCS, for instance in the report prepared for the Home Office by Lynn in 1997 "Collecting Data About Non-respondents to the British Crime Survey". Given the varied level of detail which is currently presented about non-response across the three surveys it is likely that a much improved picture could be developed if the suggestions made by Lynn for assessing non-response in the BCS were repeated across all three surveys.

3.11 The work of Freeth (2005) presents analysis aimed at evaluating how non-response in the BCS (from 2001) varied depending on the characteristics of the respondent and their household. The work of Freeth (2005) uses census data to try and establish the characteristics of both those addresses where contact could not be made, and also those respondents who were contacted but refused to participate in

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the survey. Logistic regression is used to identify those characteristics which are associated with a greater risk of non-contact or non-response and this analysis provides a template which could be replicated with reference to the NICS and SCS. Freeth (2005, pp38-41) concludes that there is a relationship between those factors associated with non-response and many of the key estimates provide by the BCS (notably levels of victimisation). It should be expected that the weighting strategies employed by the different surveys will help address these concerns. However, further analysis in this area, and in particular how patterns of non-response vary between across the different surveys, would be beneficial.

### ***Sample Size***

3.12 Putting aside issues about the representativeness of the sampling frame or bias introduced through sampling (see the discussion below about stratification and design effects) then the accuracy of any estimates will be affected by the size of the sample, the variability of estimates and the sample design. Accepting confidence intervals of 95% then the approximate margin of error associated with NICS is +/-1.6, for SCS is +/-1.4 and for BCS is +/-0.5<sup>6</sup>. Although this comparison can be considered simplistic, as it assumes that all three samples are based on simple random sampling (which is only employed in NICS), it does provide some evidence that the different surveys may be subject to different amounts of error due to their sample size and that this should be acknowledged when presenting figures, for instance by adding confidence intervals to the estimates. The sample size is another area where the new SCJS will vary from the 2006 SCVS. The sample for SCJS will be approximately three times the size of those previously employed.

### ***Sample Design Effects***

3.13 As described in Table 1, all three surveys employ a form of random sampling. The samples for the BCS and SCS are selected using a multi-staged sample selection process employing both stratification and clustering procedures. The NICS sample is drawn from its sampling frame using simple random sampling. The Land and Property Services list, used as the sampling frame for the NICS, is ordered by geographical region, for instance district council area and electoral ward. Therefore, the simple random sample drawn tends to be roughly in proportion to the number of domestic properties in each geographic region. Discussions with NISRA indicate that their use of a simple random sample means that there are no design effects associated with NICS. It is not the case that complex sample designs (such as those

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<sup>6</sup> Based on sample size calculator at <http://www.surveysystem.com/sscalc.htm>

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used in the BCS and SCS) should be seen as less than optimal than a simple random sample, rather they allow for the maximum degree of accuracy (or precision) to be achieved within the resource constraints of the surveys (Lynn, 1998). Indeed, because stratification and clustering are systematic departures from simple random sampling it is possible to estimate the effect of the sample design on any data collected. However, if the resultant figures are to be compared reliably it is important that the likely impact of any sampling design features are taken into account. It is therefore important that adequate, and clearly accessible, information about the size of design effects are provided for each survey or confidence intervals published alongside key estimates.

3.14 Since the calculation of margins of error in survey estimates (i.e. simple standard error) assumes simple random sampling, standard errors need to be corrected where estimates are made from surveys employing complex (non-random) sample designs. These are commonly known as complex standard errors. The impact of the sampling strategy on the precision of estimates is measured using design effects. Design effects give the ratio of complex standard errors to those based on a simple random sample. The greater this ratio the more impact the sampling strategy is having on the results. While many BCS reports make reference to design effects (for instance Home Office, 2001, gives design effects for questions relating to drug use for all surveys between 1994 and 2000) such information does not appear in the 2005-06 Technical Report (communication from John Flatley indicates that the Home Office do themselves calculate design weights and use them as the basis for publishing confidence intervals). Comparing design effects for a range of common questions across the different surveys could help provide an indication as to whether the different approaches to stratification in the different surveys are influencing the precision with which estimates of victimisation can be compared.

### ***Weighting***

3.15 All three surveys make use of weighting to try and ensure the make-up of their sample reflects that of the wider population. As with the other areas of sample design discussed in this paper, the level of detail provided varies between surveys and this can make direct comparison difficult. The Technical Report for NICS describes a one stage weighting process where the only adjustment made is to account for the fact that as only one respondent is selected at each address, those living in larger households have less chance of selection. This weighting process, which increases the importance attached to those respondents from larger household, is commonly known as selection weighting. The NICS technical report

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makes no apparent reference to weighting to adjust for differing rates of non-response across different types of respondent.

3.16 Both the BCS and SCS technical reports provide details of weighting procedures which are aimed at taking account of the probability of selection within households (same as NICS), non-response bias and the stratified nature of their sample. For instance the weighting to be used for analysis of individual level victimisation in the BCS core sample includes one component to account for the likelihood of an individual being selected within a household, a component to account for the likelihood of a dwelling being selected where an address contains multiple dwellings, a component to account for variations in response rates between inner city areas and other geographies and a component to account for the unequal probability of selection caused by stratification at police force area level (including the rotation of 50% of primary sampling units from the previous years survey). Calculation of weights for analysis of household victimisation in the BCS follows the same procedure as the weighting for individual analysis but does not include a component for the probability of an individual being selected within a household as this is not necessary. In addition to the BCS design weights described here the Home Office has created additional calibration weights to adjust for the different probabilities of non-response associated with the characteristics of individual respondents (e.g. age and gender) and all these weights are deposited with UKDA for use by researchers undertaking secondary analysis.

3.17 Although aiming to deal with broadly the same issues as the BCS, the SCS follows a different weighting strategy notably meaning that individual weights are not simply a function of household weight adjusted for the probability of an individual being selected. Within each PFA, household weights include two design weight factors one aimed at ensuring the distribution of households in the sample matches the wider population in terms of household size (i.e. 1 person, 2 people etc) and one which, after the first weight is applied, adjusts the distribution further to try and gain an accurate match in terms of ACORN classification. The final household weight is based on multiplying together this final non-response weight with the probability of a given dwelling having being selected at a given address. Individual level weights in the SCS consist of two parts, a design weight (equal to the probability of a given individual being selected in a given dwelling multiplied together with the probability of that dwelling having being selected at a given address), and a non-response weight which was calculated so that the sample (after the design weight was applied) matched the wider population profile in terms of age, gender, ACORN classification and police force area.

3.18 In addition to the basic procedures used to calculate household and individual weights described above all three surveys scaled their final weights to ensure that the weighted sample size matched that of the unweighted sample. This



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should ensure that measures of estimating precision (for instance standard errors) are not affected by the introduction of weights appearing to result in a changed sample size. BCS and SCS also capped their weights to try and ensure that any extreme cases did not have undue influence on results (such capping is not required in NICS given that its weighting strategy is unlikely to result in extreme weights for cases). Although both the SCS and BCS technical reports state that this capping had little impact on results, it is possible that as capping weights can introduce bias to estimates employing different strategies for capping could influence the comparability of results between surveys, although given capping affects a very small proportion of actual cases the impact of this is likely to be negligible. Assessing if this is the case when comparing victimisation rates between surveys would require more information about the capping procedure used than is currently given in the technical reports and/or some practical tests to quantify the impact of capping weights.

### ***Representiveness of Sample***

3.19 Differences in weighting procedures between surveys is of secondary interest providing the final weights do not introduce undue bias into any estimates which are to be compared and the weighted sample for each survey accurately reflects its region's population. One way of assessing how well a sample (after weights have been applied) reflects a population is to compare the demographic breakdown of the sample (on characteristics such as age, gender, household type) to knowledge about the whole population, for instance from a census. At the time of writing, such details were not included in the SCS documentation (they have subsequently been added) while the tables provided for the BCS and NICS are not directly comparable as they include different groupings for some characteristics (notable bands of age). Information about the age and gender balance of the 2006 SCS was provided directly by the survey contractors, BMRB, and along with similar information for the BCS and NICS this is presented in Appendix 1. The overall impression is that, when considering age and gender, the weighed samples of the three surveys are close to the populations they are aiming to represent, suggesting that after weighting their samples are, with regards to these characteristics highly representative. However, the differences in how this information is presented by the different surveys means it is not possible to extend this analysis to consider other characteristics, such analysis could prove useful to further compare the representiveness of the different surveys.

3.20 Relating back to the idea of non-response (discussed above) Appendix 1 also includes details of age and gender splits across the different samples before any weighting is applied. Although very limited in scope these figures provide some

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evidence that patterns of non-response may influence the representiveness of the data. In particular, it appears that prior to weighting all three surveys have samples which under-represent respondents who are young and male. As discussed above, further work is required to look at how patterns of non-response vary depending on the characteristics of the chosen respondent and how, this might influence the robustness and representiveness and comparability of the different data sets.

3.21 An important point to bear in mind is that the concept of representiveness is always a relative one: we can only assess the representiveness of a sample in comparison to known characteristics of the wider population. In turn, this requires not only that similar information is available for both the sample and the population but also that it is sufficiently detailed: in general, the more items of similar information that are available, the greater our ability to estimate representiveness. This provides one area where further work (maybe similar to the work of Freeth, 2005, with reference to non-response) could help ensure more reliable comparisons across different surveys.

### ***Questionnaire***

3.22 If estimates are to be comparable across surveys it is important that the questions asked in different jurisdictions are comparable. This will mostly likely involve having no differences in the wording of questions, or the ways in which respondents are asked to respond, which could influence the answers that are provided. One possible exception to this would be if an identical incident was commonly referred to using different terms in different jurisdictions. In this case, an alternative wording may be necessary to achieve comparability. Information about victimisation is collected in two stages. Firstly, all respondents are asked a series of screener questions aimed at establishing if they have experienced particular forms of victimisation in the (12 month) period before interview. Secondly, those respondents who indicate they have experienced victimisation are asked to provide further information to allow the incident(s) to be fully coded in the same way as the police would have recorded the incident, and to provide more information about the nature of victimisation. This is done via “victim forms”<sup>7</sup>.

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<sup>7</sup> Questions about victimisation for potentially sensitive crimes, notably domestic violence, have been asked in self-completion modules at different points in the history of the surveys. These are excluded from this paper as they can follow a different structure to the questions considered here and the data are less readily available for analysis (i.e self-report data from the BCS is not always released with the public dataset).

### ***Screener Questions***

3.23 Table 4 lists the different forms of victimisation for which screener questions are included within the three UK crime surveys. For each survey the table indicates the question number used to ask about a particular form of victimisation. The overarching picture is one of questions which are broadly comparable across surveys. All the surveys pretty much cover the same forms of victimisation with the only differences appearing to be that there is no screener question referring to sexual attacks in the NICS and that the SCS does not differentiate between whether or not crimes concerning a respondent's home occurred at their present or previous address. Where questions about comparable crimes are asked, nearly all questions use the same text. Similarly, the order in which the different questions are asked is broadly the same across surveys. This reduces the likelihood of respondents' answers being influenced by the order in which they are asked to consider different incidents. As an aside, it should be noted that the order of questions referring to other topics do vary between surveys and this may reduce comparability in these areas.

3.24 In terms of differences in question text, it should be noted that in questions where the BCS refers to "home" the SCS commonly uses "home/flat" although this may be expected to have little impact on the comparability of data. The only other difference concerns the wording of the question concerning sex attacks which is included in the BCS and SCS. The alternative question texts are shown in box 1 below.

*Box 1: Wording difference between BCS and SCS for question SEXATTAK (not in NICS)*

BCS: During the last 12 months have you been sexually interfered with, assaulted or attacked, either by someone you knew or by a stranger?

SCS: Has anyone, including people you know well, interfered with, assaulted or attacked you in a sexual way or exposed themselves to you?

3.25 One issue which could potentially influence the comparability of responses to the screener questions between surveys is the period that respondents are asked to consider when answering. Both the BCS and NICS have dynamic reference periods which are intended to cover the previous 12 months rounded to the start of the month in which the interview occurs. In contrast to the dynamic reference periods of the BCS and NICS, SCS has a fixed reference period dating back to April 2005 (as shown in Box 2 below). This fixed starting date may mean that Scottish respondents are reporting crimes over a period longer than 12 months (e.g. someone interviewed towards the end of the fieldwork period would be considering a period of around 19 months). Although victimisation estimates are based only on incidents that were

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reported to have occurred during the 12 months since April 05, it is possible that the extended reference period has some impact on the ability of respondents to accurately recall events<sup>8</sup>. This discrepancy will be removed with the introduction of SCJS. From this point forward, the survey in Scotland will employ a rolling twelve month reference period similar to those in the BCS and NICS.

3.26 The BCS Technical Report makes reference to providing respondents with a “life-events calendar” listing key events during the reference period to try aid accurate recall. Although no mention of a similar instrument is made in the NICS Technical Report, communication with the NICS team shows that their fieldworks do make use of a similar resource. No calendar instrument appears to be employed in the SCS. If this additional aid to memory works as intended, and is not employed in the SCS, this may have some effect on victimisation between surveys, although the impact is likely to be minor.

### *Box 2: Alternative Reference Period for Victimisation Screener Questions in SCS*

The preamble to the victimisation questions is slightly different in each survey:

BCS: I’m now going to ask you about things that may have happened over the last 12 months, that is since the first of [DATE]....

SCS: The next few questions are about things that may have happened to you... I am only interested in things which have happened

- To you personally or other members of your household
- Since [1 April 2005]

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<sup>8</sup> See work concerning apparent inconsistencies between 1 year and 5 year victimisation rates (van der Veen, 1992)

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**Table 4: Victimization Screener Questions Across UK Crime Surveys**

Question	BCS Q#	SCS Q#	NICS Q#	Comment
<b>Vehicle Crime</b>				
MotTheft	1	1	1	Theft of motor vehicle
MotStole	2	2	2	Theft from motor vehicle
CarDamag	3	3	3	Damage of motor vehicle
BikTheft	4	4	4	Theft of bicycle
<b>Previous Home</b>				
PrevThef	5	-	5	Theft, or attempted, from previous home <i>Previous home within last year, not differentiated in SCS.</i>
PrevDam	6	-	6	Deliberate damage to previous home <i>Previous home within last year, not differentiated in SCS.</i>
PrevTry	7	-	7	Attempted break-in to previous home <i>Previous home within last year, not differentiated in SCS.</i>
PrevStol	8	-	8	Theft, or attempted, from previous home <i>Previous home within last year, not differentiated in SCS.</i>
ProSide	9	-	9	Theft from outside previous home <i>Previous home within last year, not differentiated in SCS.</i>
PrDeface	10	-	10	Deliberate defacing item outside previous home <i>Previous home within last year, not differentiated in SCS.</i>
<b>Present Home</b>				
HomeThef	11	-	11	Theft, or attempted, from new home
YrHoThef	12	5	12	Theft, or attempted, from current home
YrHoDam	13	6	13	Deliberate damage to current home
YrHoTry	14	7	14	Attempted break-in to current home
YrHoStol	15	8	15	Theft, or attempted, from current home
YrOSide	16	9	16	Theft from outside current home
YrDeface	17	10	17	Deliberate defacing item outside current home
<b>Personal</b>				
PersThef	18	11	18	Theft of personal items being carried
TryPers	19	12	18	Attempted theft of personal items being carried
OthThef	20	13	20	Theft of item left somewhere
DelibDam	21	14	21	Deliberate damage to personal items
DelibVio	22	15	22	Deliberate violence against you
ThreVio	23	16	23	Threat of violence against you
SexAttak	24	17	-	Sexual assault against you <i>Not present in NICS<sup>9</sup> Will not be present in SCJS<sup>10</sup></i>
<b>Household</b>				
HhldViol	25	18	24	Deliberate violence from household member against you

<sup>9</sup> 'SexAttak' was removed from the NICS mainly because the numbers of incidents being generated from this sensitive question were so small (and unreliable) that they could not be used to generate an accurate estimate. Collecting the data for the sake of completeness was not considered a sufficient reason to retain the question. Should the respondent wish to disclose such an incident, it is possible that this will be captured within 'DelibVio' in Table 4

<sup>10</sup> For similar reasons to the NICS this question will no longer be asked in Scotland following the introduction of the SCJS

## 4 VICTIM FORMS

4.1 For those respondents who do indicate they have been a victim of crime during the reference period a series of further questions are then asked to gain more information about the incident(s). The BCS Technical Report notes, “the victim form is the key to the estimate of victimisation...” (p23). Besides providing detailed information for possible analysis, the aim of the victim form is to provide sufficient detail about an incident to allow it to be accurately coded. The Victim Forms associated with all three surveys collect information about three broad issues (indeed the description in the SCS and BCS technical reports is almost word for word identical). Firstly, an exact month for the incident is collected to ensure that it has occurred within the time frame being used for calculating victimisation rates. Secondly, an open ended description of the incident is collected (this allows the respondent to describe exactly what happened and is important for the subsequent coding procedure). Finally, a series of closed questions are used to collect key characteristics of the offence. The answers to these question may contribute to the coding process but also allow for additional analysis to be undertaken looking at issues such as the context in which an offence occurred.

4.2 While the victim forms associated with all three surveys cover the same broad issues their exact content does vary. The BCS consists of two designs of victim form, a long form and a short form. The long form design is used for the first three victim forms a respondent is asked to complete and the shorter form for incidents 4 to 6. The long design includes additional questions aimed at gaining a more complete understanding of an incident, the differences between the long and short forms do not influence the process of coding offences. It is rather that the long-form collects additional information which can be used for other analysis. The NICS also employs both long and short victim forms, although both of these are more strictly comparable to the short form used in the BCS. The NICS have excluded questions which are included on the BCS long form where they believed they would not generate usable data given the smaller sample size of the NICS. In addition, removing these questions allowed for the inclusion of additional topics within NICS, in particular around issues of performance measurement (personal communication with Brian French). The 2006 SCS used only one design of victim form (in contrast to some previous sweeps where mixtures of long and short forms appear to have been used). The SCS Technical Report draws attention to how this form as remained broadly unaltered over time. The exact contents of the SCS victim form has diverged from that of the BCS since 1993. However, these changes do not affect the basic information collected about victimisation.

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4.3 The maximum number of victim forms respondents are asked to answer varies between surveys. Six forms are completed in the BCS and NICS while only 5 are completed in the SCS. Given that the information recorded on the victim forms is used as the basis for coding offences and calculating victimisation statistics allowing respondents in different surveys to answer varying numbers of victim forms may reduce the comparability of estimates between regions. However, it is important that this is not overstated as in reality very few respondents complete the maximum allowable number of victim forms. For instance, only 0.9% of those in the SCS sample completed 5 victim forms (2.9% of victims) while only 0.2% of the BCS sample (1% of victims) completed 5 or 6 victim forms.

4.4 It is highly likely that some, although not many, respondents in each survey will have experienced more types of victimisation than can be covered in the maximum number of victim forms they are allowed to complete. In this case, victim forms are asked about offences based on a priority ranking. If different surveys placed offences in different orders of importance than this could influence which types of victimisation respondents are asked about and reduce comparability between surveys. Information on the exact order in which crimes are prioritised across the different surveys is not present in all the technical reports. However, for all three surveys the order is broadly the inverse of that in which screener questions are asked. Table 4 indicated that the screener questions were asked in a comparable order across the surveys and as such there is no reason to expect the order in which crimes are prioritised for victim forms to vary between surveys. In addition, it should be noted that this is only likely to affect a very small proportion of the of the offences reported in the screener questions (commonly less than 1%) and as such any differences will most likely have only negligible effects on aggregate estimates of victimisation.

4.5 Measures of victimisation are commonly expressed in two forms: prevalence rates (the percentage of individuals or households who have being victimised at least once) and incidence rates (a count of the number of victimisations per 10,000 people or households). The calculation of incidence rates is likely to be affected by how repeat victimisation is recorded. All three surveys have traditionally asked respondents who have suffered a given type of victimisation more than once if they believe the incidents were related and as such can be treated as a “series”. This approach is historical and was done largely to reflect the way in which the criminal justice systems treated repeat victimisation of a person (or property) by a single perpetrator as a single ongoing incident. The extent to which these figures are now comparable to official figures should be considered to see if this approach to counting is still relevant following the introduction of new police recording procedures. In the calculation of incidence rates, repeat victimisation is capped to ensure that no extreme values unduly influence the overall estimate. It has being argued that this causes crime surveys to under-estimate the real level of

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victimisation (for a discussion of this see Pease and Farrell 2007)<sup>11</sup>. In terms of ensuring comparability between the different UK crime surveys, the important issue is that capping should occur in the same way. This is the case, as all surveys cap counts at 5 incidents per series. , It remains possible that comparisons based on capped incidence rates may not reflect the true victimisation rates in each jurisdiction if repeat victimisation (above the capping level) is more prevalent in one of the jurisdictions. However, the data presented in the technical reports, and discussions with the different survey management teams suggest that this is likely to be the case.

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<sup>11</sup> There are of course several good reasons for employing capping when estimating levels of victimisation using survey data. In particular, the use of uncapped data could lead to spurious estimates of victimisation because such victims find it difficult to recall the exact number of incidents they have suffered. The resultant fluctuations in estimates of victimisation between surveys could indeed reduce comparability, not just between jurisdictions, but also within a single jurisdiction over time.



## 5 CODING OF OFFENCES

5.1 The objective of offence coding is to assign those incidents which individuals report during a crime survey to a category comparable to those used in police recorded crime and to enable the calculating of victimisation rates by offence. This classification is based on the information respondents were asked to provide during the “victim form” part of the survey. Each survey has a set of coding instructions which explain how to use the information on the victim forms to arrive at a final offence classification. Since only one form of crime is recorded for each victim form, the most important issue in the calculation of victimisation estimates is which offence is most important when an incident involves a respondent being the victim of more than one “crime”. For instance, should a burglary which has involved violence be recorded as a burglary or an assault? If the coding instructions used by different surveys vary in their interpretation of such incidents then it could be expected to influence the final estimates of victimisation levels.

5.2 The coding procedures used across the surveys are generally similar, involving specially trained coders whose work is regularly checked for quality control purposes and difficult to code cases are referred to the respective government agency (i.e Home Office, Scottish Executive etc).

### BCS Compared to NICS

5.3 Comparing the coding instructions across the three surveys suggests they are broadly similar. The NICS coding instructions are a near identical match to those for the BCS with a couple of minor differences, and these could be expected to have very little if any, impact on comparability between the two surveys. Indeed during discussions, the NICS team suggested that the majority of these minor differences between the BCS and NICS may well be the result of a recent update to the BCS coding instructions. The NICS team commonly review their own coding rules in view of changes to the instructions associated with the BCS to try and maintain comparability between the two surveys.

5.4 Occasional presentational differences exist between the BCS and NICS. For example, the BCS uses bold print and includes some additional examples of what should be included in a category, compared to NICS. One example is that when

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referring to criminal damage the BCS makes explicit reference to including recycling bins in the definition of “a home.” Although the impact of these differences is likely to be negligible at most, further analysis which confirms this is the case would help ensure comparability across surveys.

5.5 The numbering of codes attached to victim forms also appears consistent between the NICS and BCS. The only notable difference appearing to be that the BCS subdivides cases of criminal damage in terms of the value of damage caused (above or below £20) where as this is not coded in the NICS (or SCS). Given this difference refers to definitions within a broad category (Criminal Damage) as opposed to a difference which is likely to see a case move between broad categories (i.e. Assault instead of Robbery) this will have no impact on the comparison of broad patterns between the two surveys (the BCS categories could simply be collapsed to reflect the data collected by NICS). However, such difference may be important to users who wish to study more precise types of victimisation. Finally, when considering theft, the NICS (and SCS) includes locked communal areas of flats as part of the home, while this appears explicitly excluded in the BCS instructions (potentially causing the figures for Scotland and Northern Ireland to appear inflated compared to those from the BCS). However, this difference will most likely only affect a very small proportion of the cases reported. Further analysis, which considered what proportion of theft reported to the NICS and SCS had occurred in communal areas would allow for any effect to be accurately quantified.

### SCS Compared to BCS and NICS

5.6 The coding manual for the SCS does diverge to a greater extent from those for the BCS and NICS. These differences reflect real differences in the nature of legal proceedings in Scotland Compared to the rest of the UK. These differences have become more pronounced since 2003 as prior to this coding was largely based on English Law (the 2003 SCS Report gives this as the explicit reason for changing the definition of Housebreaking in such a way that it was no longer comparable with BCS). The other major area of difference between SCS and BCS is the coding of assault cases. This once again reflects the different way these cases are handled by the Scottish criminal justice system. For instance, the Scottish Coding Manual states that the level of intent to cause injury is not a relevant consideration in coding assaults. A full list of variations between the coding instructions for SCS and those for the BCS are given in the SCS Offence Coding Manual (and reproduced in Appendix 2). Another notable difference is the inclusion of crimes committed by people who are mentally ill and police officers. Although likely to be relatively few in number, the inclusion of such offences has the potential to increase Scottish victimisation

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rates relative to those for the rest of the UK. Similarly, several definitional changes are likely to influence the comparability of estimates between surveys.

5.7 The SCS instructions vary from those for the other surveys in the relative importance they attach to some offences, and this could impact on the comparability of estimates between surveys. For instance, the SCS Coding Manual notes that in cases including both vandalism and assault, the assault component will generally be considered more serious unless it is clear that the damage to property was clearly the most serious aspect of the incident (in BCS the priority appears the other way round with the emphasis being placed on showing that the assault was the most serious aspect in order to change the coding). Finally, the SCS introduces several codes which combine together offences which would have been coded as only one type of crime in the BCS. For example, the SCS includes code 37 for “Rape and Housebreaking” when such a crime would be coded simply as “Rape” in the BCS.

5.8 Given their common origins (the 1982 BCS) it is not surprising that many of the coding instructions are word for word identical. However, over the years slight changes appear to have been introduced resulting in quite different documents, particularly when the SCS is considered. One clear example of this is how “assaults” are coded across the surveys (reflecting the differing ways such incidents are handled by the respective criminal justice systems). Such differences may influence the ability to directly compare rates between surveys, particularly for specific types of victimisation.

5.9 Given the strong similarities between the coding instructions used by the different surveys, most issues around comparability between surveys seems more likely to be the result of how these instructions are interpreted by individual coders rather than differences in the instructions themselves. All three surveys have provisions in place to try and ensure that different coders working on their surveys are coding consistently and it might prove an informative exercise to benchmark the coding across different surveys.

5.10 On the basis of the information available it is difficult to assess whether differences in coding practice influence the final estimates of victimisation in a systematic way. This is an area which would benefit from further investigation. One approach to trying to estimate the impact of these differences could be to take the victim forms from one survey (say the SCS) and have them coded by coders who work on the other surveys (according to their coding guidelines). The difference between the original estimate, and the estimate based on the alternative coding instructions should provide some indication as to the impact of using different coding regimes.

## 6 CONCLUSIONS

6.1 This paper aimed to map differences across the BCS, NICS and SCS which may impact on their estimates of victimisation. There are clearly many similarities between the surveys, notably in terms of the populations they attempt to measure and the wording of the questions they employ. It is therefore important not to overplay the extent, and likely impact of differences between the surveys. However, it is possible to identify several differences which may well influence the comparability of estimates between surveys. In particular, differences between the instructions for coding offences into categories are likely to restrict comparability especially where the SCS is concerned. It is however important to note that many of these differences reflect the need for the coding employed by the different surveys to reflect their own (unique) criminal justice systems.

6.2 Given the potential for differences in coding to restrict comparability (particularly where Scotland is concerned), and remembering that each surveys coding is undertaken to try and achieve comparability with its own criminal justice system, one question worth greater consideration is whether it is legitimate to try and compare victimisation rates between areas where the criminal justice systems treat offences in different ways?

6.3 A second aspect concerning how victimisation data is collected by the different surveys revolves around differences in the number of victim forms used and the level of detail collected about specific incidents. This may influence both the preciseness with which offences are coded and indeed the number of events which are used to compute estimates of victimisation. Given how relatively few respondents complete the maximum number of victim forms, it seems likely that the use of an additional victim form in Scotland will have only a marginal impact of the comparability of data. However, further work is required to accurately quantify the impact of differences in the use of victim forms.

6.4 Beyond the coding of offences, there may be scope for further investigating the other issues raised in this paper. While it has been possible to make some first impressions that the surveys are comparable in terms of their target populations and representativeness these areas clearly merit more investigation which would be assisted by producing comparable technical data for the different surveys. For instance, an accurate assessment of the representativeness of the final datasets was limited due to the lack of comparable information about the profile of the survey samples compared to the wider population. Additionally, comparable evidence about patterns of non-response is limited and an area in need of further investigation.

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6.5 Those running all three surveys use several strategies to highlight the significant findings within their reports. For instance, BCS reports commonly see statistically significant differences highlighted within tables, commentary which concentrates on those differences which are statistically significant and the inclusion of reference tables of confidence intervals for key estimates such as victimisation rates. However, comparisons between the results from the different surveys would be improved if results were presented in a more consistent manner taking into account the impact of issues such as sample size and sampling strategy. For instance it might be that either standard errors or confidence intervals could be routinely included in tables rather than relegated to appendices.

6.6 In conclusion, it is also worthy of note that while the differences between surveys with regards to estimating victimisation are relatively minor, data concerning other topics are likely to enjoy even greater comparability between jurisdictions. For instance it may be possible to ask comparable questions on topics such as confidence in the police or perceptions of safety but comparisons between regions (with less concern with the need for answers to be coded in a jurisdiction specific manner). Providing such figures are presented along with information on the appropriate standard errors or confidence intervals, to show if any differences persist once issues of survey methodology are accounted for, then comparisons between surveys should be straight-forward. Finally, it should be noted that the different surveys continue to develop (for instance the introduction of SCJS in Scotland) and that it is important to consider how a change implemented in one survey may affect comparability with those used in other parts of the UK.

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## APPENDIX 1: SAMPLE PROFILES FOR BCS 2005-06, NICS 2005 AND SCS 2006

### Comparison of the BCS Achieved Core Sample with the Population by Sex and Age, 2005-06

	Unweighted sample %	Weighted Sample %	Mid-2004 Population Estimates %
<b>Age</b>			
16-19	3.4%	5.5%	6.5%
20-24	4.7%	6.3%	8.0%
25-34	14.3%	14.7%	16.3%
35-44	19.3%	19.4%	19.0%
45-54	15.6%	17.1%	15.8%
55-64	17.2%	16.6%	14.5%
65-74	13.4%	11.5%	10.4%
75-84	9.4%	7.1%	7.1%
85 and over	2.6%	1.7%	2.5%
<b>Gender</b>			
Men	44.8%	47.0%	48.5%
Women	55.2%	53.0%	51.5%
<i>Bases:</i>	47,796	47,479	43,102,500
Source: Mid-2004 Population Estimates, Office of National Statistics			

### Comparison of the NICS Achieved Core Sample with the Population by Sex and Age, 2005

	Unweighted sample %	Weighted Sample %	Population Estimates %
<b>Age</b>			
16-24	9.2%	13.2%	16.4%
25-34	16.4%	16.0%	18.8%
35-44	20.4%	20.1%	19.2%
45-59	24.8%	23.1%	22.5%
60 and over	31.0%	25.8%	23.1%
<b>Gender</b>			
Men	45.4%	48.1%	47.9%
Women	54.6%	51.9%	52.1%
Source: Population Estimates, 2001 Census			

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**Comparison of the SCVS Achieved Core Sample with the Population by Sex and Age, 2006**

	Unweighted Sample	Weighted Sample	Mid-2006 Population
	%	%	Estimates
			%
<b>Men</b>			
16-24	9.7	15.1	16.7
25-34	13.4	16.2	15.3
35-44	18.4	19.6	18.7
45-54	17.8	17.3	17.1
55-64	17.5	14.2	15.0
65 and over	23.0	17.5	17.2
<i>Bases:</i>	2,191	2,360	2,031,200
<b>Women</b>			
16-24	8.2	12.7	14.7
25-34	15.1	15.6	14.2
35-44	17.7	18.8	18.4
45-54	14.4	16.2	16.4
55-64	17.4	14.2	14.4
65 and over	27.1	22.3	21.9
<i>Bases:</i>	2,797	2,628	2,229,500
All men	43.9	47.3	47.7
All women	56.1	52.7	52.3
<i>Bases:</i>	4,988	4,988	4,260,700
Source: Mid-2006 Population Estimates, Office of National Statistics			



## APPENDIX 2: CODING DIFFERENCES BETWEEN OFFENCE CODING IN SCS AND BCS TAKEN FROM SCS 2006 CODING MANUAL

The coding process is very similar to that used in BCS. However, some of the actual coding rules are quite different.

The main differences are outlined here:

### General

Crimes committed by people who are mentally ill will count as valid crimes (in BCS these do not count).

Crimes committed by police officers will also be coded (for BCS these are referred to the Home Office).

### Assault

Removed code 13 – common assault

In Scotland, there are two assault codes:

Code 11 Serious assault

Code 12 Minor assault

Serious assault is defined as an assault which leads to an overnight stay in hospital or where any of the following injuries were caused (regardless of any hospitalisation)

*Fractures: meaning the breaking or cracking of a bone. It should be noted that a nose is cartilage not bone so therefore a broken nose should not be classified as a Serious Assault unless it meets one of the other criteria.*

*Internal Injuries*

*Severe Concussion*

*Loss of Consciousness*

*Lacerations requiring sutures which may lead to impairment or disfigurement*

*Any other injury which may lead to impairment or disfigurement.*

Unlike England, the intention to cause severe injury is not relevant to the coding.

If serious assault occurs due to fire raising, then code 14 is used – Fire raising and serious assault

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If serious assault occurs with housebreaking, code 15 is used – Serious assault and housebreaking.

If the only crime is attempted assault on a person other than the respondent, this is coded 19. This would be considered “out of scope” and would not be included in any estimates. In the BCS this is referred to the Home Office.

### **Sexual offences**

If rape occurs with housebreaking, then code 37 is used (rape and housebreaking). Otherwise, rape takes precedence over all other codes.

If serious assault with a sexual motive occurs with housebreaking, code 38 is used (serious assault with sexual motive and housebreaking). Otherwise, serious assault with sexual motive takes precedence over all other codes (except rape).

Serious assault with sexual motive is defined in same way as serious assault (i.e. leading to the above list of injuries and/or an overnight stay in hospital).

A new code has been introduced for indecent exposure (code 36). This takes precedence over threats and vandalism, but not over other crimes (similar to indecent assault).

### **Robberies and thefts from a person**

If a serious assault with sexual motive takes place as part of a robbery, then the serious assault with sexual motive takes priority (code 32).

Robbery takes priority over serious assault, but serious assault takes priority over attempted robbery.

### **Housebreaking and thefts**

Housebreaking defined as unauthorised entry to someone’s home, garage or outhouse, involving breaking in through a locked door or using a non-standard method of entry (e.g. a window) with the intent to steal

Other thefts from respondent’s home (e.g. via unlocked door, getting in by false pretences etc) coded as theft in a dwelling (or, in the case of unconnected outhouses and garages – theft from outside dwelling).

Other intentions (in absence of intent to steal) would be coded as those crimes (e.g. break in to vandalise would be coded as vandalism).

Communal areas count as part of the home if they are normally secure (could lead to double counting of an offence).

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As mentioned above, housebreaking can occur alongside other crime codes:

Code 15 Serious assault and housebreaking

Code 37 Rape and housebreaking

Code 38 Serious assault with a sexual motive and housebreaking

(However, if the break in was with the intention of committing rape, and nothing stolen, this would be coded as rape).

Thefts of pedal cycles – all bicycles count (including children’s bikes).

Attempted theft of property belonging to household member (away from the home) is coded as 69 (this is referred to Home Office for BCS).

There is also a code 79 which is “other attempted theft falling outside survey coverage”.

### **Vandalism**

Fire raising not referred – but is a case of vandalism.

Fire raising that causes the respondent serious injury is coded as 14 – Fire raising and serious assault.

Otherwise, fire-raising has the same priority as other vandalism (housebreaking, robbery and theft take priority).

Codes 81 and 83 have been removed, as there is no split depending on the cost of the damage. Thus, there is just one vandalism to a motor vehicle code (82) and one vandalism to the home code (84).

Vandalism includes cases where the damage can be repaired just by labour of the victim without outside cost.

Unlike BCS, vandalism only takes priority over assault if the damage is component is clearly the more serious aspect of the incident. (In BCS, assault has to be clearly the more serious incident).

### **Threats**

Threats made over the telephone are coded as a crime; however, obscene phone calls are out of scope, unless sexual threats are involved.