

TECHNICAL REPORT

THE NEW ZEALAND
CRIME & SAFETY SURVEY 2006

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MINISTRY OF
JUSTICE
Tāhū o te Ture

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1 Introduction

This report describes the design and methods used in the 2006 New Zealand Crime and Safety Survey (2006 NZCASS hereafter). The survey was conducted for the Ministry of Justice by a consortium comprising the Crime and Justice Research Centre (Victoria University), ACNielsen (who managed the fieldwork), James Reilly (Statistical Insights), and two Māori consultants, Laurie Porima and Nan Wehipeihana (of LLE Research).

The 2006 NZCASS was based on a nationally representative random sample of 5,416 people aged 15 and over living in private households throughout New Zealand (excluding most offshore islands). This includes a 'booster' sample of 1,187 Māori. Those who took part were interviewed at home between February and June 2006 by ACNielsen interviewers. They were asked whether they had been a victim of crimes covered by the survey since 1st January 2005. They were asked about the circumstances and impact of any offences they had experienced, and about a number of other crime-related issues.

This is the third time that a national victimisation survey has been run in New Zealand. The two previous surveys were conducted in 1996 and 2001, under the title of the New Zealand National Survey of Crime Victims (NSCV). ACNielsen carried out both in conjunction with the Institute of Criminology (Victoria University).

What the 2006 New Zealand Crime and Safety Survey does

The 2006 NZCASS offers a wide range of information:

- It measures the amount of crime in New Zealand in 2005 by asking people about crimes they experienced themselves. The survey includes crimes not reported to the police, so it is an important alternative to police records. Victims do not report crime for various reasons. Without the NZCASS, we would have no information on these unreported crimes.
- It looks at how many offences are reported to the police, and the reasons for not reporting. It also looks at how well victims thought the Police responded when a crime report was made.
- The survey can examine trends in survey-measured crime since 1996 and 2001, although there are limits on the comparisons imposed by changes in survey design.¹
- The survey helps identify those most at risk of different types of crime in terms of age, gender, ethnicity, etc. This helps in planning crime prevention programmes. As said, Police figures tell us little on this front.
- It gives information on the nature of victimisation, such as its physical, financial and emotional effects.

1 Initially changes between the 2001 and 2006 surveys were to be minimised to avoid these complications where possible, and much of the sample design and estimation framework reflects this initial emphasis. Significant changes to the questionnaire were later found to be necessary, requiring associated modifications to the sampling and imputation design.

- It gives information on the public's perception of crime problems in their area, and on their concerns about personal victimisation.

Structure of report

This report generally follows a chronological path through the design and methodology employed in the NZCASS. The sample design is described first, followed by questionnaire design and testing. Interviewer and fieldwork procedures are covered next, then the validation and checking of interviews. Response rate calculations are shown, along with an analysis of interview length.

The classifications and standards used in the NZCASS are then detailed, followed by descriptions of the weighting and imputation methods used in the survey.

2 Sampling methodology

Sample surveys such as the NZCASS offer a highly cost-effective alternative to censuses. Confidence in their results rests on the sample design, which ideally aims to produce a probability sample or a close approximation to one. In a probability sample, every item in the population has a known, non-zero chance of being selected. Sampling theory provides a strong foundation for the estimation of population characteristics from a probability sample. Deviations from a pure probability sample are hard to avoid in practice, however, as are measurement problems, and these can affect the accuracy of survey estimates. We attempt to highlight the most significant issues in this report, except where these have already been dealt with in the Key Findings.

Perhaps the simplest probability sampling method is a simple random sample, in which every possible sample is equally likely to be selected. However, this method is not practical for face-to-face surveys of the general New Zealand resident population. The NZCASS has used a multistage design, almost identical to that used in the 1996 and 2001 NSCVs. Areas were selected first. Then households within areas were selected. After this, one respondent within each household was selected. Each of these steps forms a distinct sampling stage.

The final stage in the sample design is the selection of a small number of incidents from those experienced by respondents. Each of these four stages will be detailed further below.

To give more reliable results for Māori, a Māori booster sample was selected independently¹ of the main sample of the general population. The booster sample used a similar multistage sample design, while screening out non-Māori by asking the initial contact whether anyone in the household aged 15 or more would identify as Māori. The target sample size for the booster sample was 1,600 interviews; the main sample was designed to achieve 4,000 interviews.

Survey population

The survey population was the total usually resident, non-institutionalised, civilian population of New Zealand aged 15 years and over. As such it excludes long-term residents of old peoples' homes, hospitals and psychiatric institutions; inmates of penal institutions; those members of the New Zealand armed forces who live in institutional settings; non-New Zealand diplomats and their non-New Zealand staff; members of non-New Zealand armed forces stationed in New Zealand; overseas visitors stationed in New Zealand for less than twelve months; and residents of offshore islands, except Waiheke.

The ABS methodological review of the 1996 NSCV recommended that the official definition of "usually resident" be adopted. This was used for the 2001 sweep, and has been used again in 2006. It mandates respondent self-definition of "usually resident", with a number of exceptions. These are defined in Statistics New Zealand's "Statistical Standard for Usual Residence 1999".

1 Areas for the booster sample were selected independently from the main sample areas, with replacement. If an area was selected for both samples, or twice within one sample, the households to be approached for the second selected area were obtained by following ACNielsen's usual procedure of setting the "start point" for the second area shortly after the last household selected for the first area.

Area selection

ACNielsen maintains a sampling frame that divides New Zealand into agglomerations of meshblocks¹ called Nielsen Area Units (NAUs). This frame was used for both the 1996 and 2001 surveys, and was used again for the 2006 NZCASS. Details of the frame are given in Appendix A3. Offshore islands (except Waiheke Island) were excluded from the survey.

A stratified systematic sample was selected with probability proportional to size, with replacement, for both the main and booster samples using Gambino's (2003) pps package in R (R Development Core Team, 2006). The size measure used for the main sample was the number of households in the Nielsen Area Unit according to the 2001 census. For the booster sample, an estimate of the number of households where at least one Māori person usually lived was used.

Growth or decline in area unit populations since the 2001 census was not accommodated in the sample design or weighting. Although this introduced some potential bias, this is believed to be quite small, as explained later in Appendix A4. Accepting some bias to avoid the fieldwork complications of such accommodations is often felt to be a worthwhile trade-off in face-to-face surveys.

A cluster of interviews was conducted in each selected Nielsen Area Unit. As in the 2001 survey, the number of designated dwellings to be approached per cluster was determined by a combination of anticipated response rate and the desirability of spreading the sample geographically as widely as possible, while retaining the cost efficiencies that cluster interviewing provides. With a main sample size of 4,000, an average of 5 completed interviews per cluster was targeted, providing a good compromise between sample spread and cost efficiency. This meant 800 areas were needed for the main sample, and 320 for a Māori booster sample with a target of 1,600 interviews.

The stratification system employed split the country into both geographical regions and levels of urban/rural density, based upon Statistics NZ definitions. The following tables show the resulting region-by- level-of-urbanisation grid. The sample size given in each row is directly proportional to that row's share of the total number of households in occupied private dwellings (or of Māori households, for the booster sample).

Table 2.1 Stratification of the main sample

Stratum	Percentage of NZ households	Area units selected
Upper North Island, Metropolitan Urban Areas	27%	214
Upper North Island, Other Main Urban Areas	11%	84
Upper North Island, Secondary Urban Areas	2%	16
Upper North Island, Rural/Minor Urban Areas	11%	88
Lower North Island, Metropolitan Urban Areas	10%	60
Lower North Island, Other Main Urban Areas	8%	82
Lower North Island, Secondary Urban Areas	2%	14
Lower North Island, Rural/Minor Urban Areas	4%	36
South Island, Metropolitan Urban Areas	13%	100
South Island, Other Main Urban Areas	3%	24
South Island, Secondary Urban Areas	3%	24
South Island, Rural/Minor Urban Areas	7%	58
Total		800

1 Meshblocks are the smallest geographical unit used by Statistics New Zealand, used in the Census and many other surveys.

Because the proportion of Māori people is relatively small, and they tend to be clustered residentially, some NAUs contained very few Māori people. For the booster sample, trying to interview in these areas can be very unproductive. Having recruited ethnic booster samples on many occasions, ACNielsen have addressed the problem of maximising both cost efficiency and sample representativeness, given the “needle in a haystack” nature of the sampling task.

The solution here was to delete from the sampling frame those NAUs with a low Māori density. As in the 2001 survey, NAUs where less than 5% of dwellings contain Māori were removed from the sampling frame for the booster sample. This accounted for 3% of NAUs, but only 0.2% of Māori households.¹

Table 2.2 Stratification of the Māori booster sample

Stratum	Percentage of NZ Māori households	Area units selected
Upper North Island, Metropolitan Urban Areas	26%	82
Upper North Island, Other Main Urban Areas	13%	42
Upper North Island, Secondary Urban Areas	3%	8
Upper North Island, Rural/Minor Urban Areas	16%	50
Lower North Island, Metropolitan Urban Areas	10%	30
Lower North Island, Other Main Urban Areas	8%	26
Lower North Island, Secondary Urban Areas	2%	6
Lower North Island, Rural/Minor Urban Areas	5%	16
South Island, Metropolitan Urban Areas	9%	28
South Island, Other Main Urban Areas	2%	8
South Island, Secondary Urban Areas	2%	6
South Island, Rural/Minor Urban Areas	5%	18
Total		320

Household selection

The cluster sampling procedure used for this survey (and in the 2001 survey) involved providing the interviewer with a randomly selected start point. This was a numbered house on a named street, and interviewers then needed to call on every fourth dwelling from that point (following a pre-determined walk pattern). This was until an outcome was obtained from every designated dwelling in the cluster.

This dwelling interval reduced the potential “word of mouth” effect from interviewing at adjacent addresses, as was done in 1996, and criticised in the ABS review. It also minimised the clustering effect to some extent. In rural areas consecutive dwellings were approached to minimise travel costs. Based upon a target response rate of 65% and a targeted average number of achieved interviews of five per cluster, this initially meant that eight designated dwellings per cluster were to be approached. However, based on the pilot feedback and response rates, this was revised to nine designated dwellings for the final survey.

Māori booster sample

For the Māori booster sample, the number of designated dwellings approached per cluster was 40. This was based on the assumption that Māori incidence and response rates would be broadly similar

¹ Consistent with this proportion, only one of the 511 Māori respondents in the main sample lived in an NAU outside the sampling frame for the Māori booster sample.

to the 2001 NSCV (or a little lower following the 49% response rate in our 2005 pilot). Because of the screening design used, a dwelling interval of four was not necessary here. Even after eliminating those Nielsen Area Units with very small Māori populations, the great majority of dwellings would still be ethnically ineligible. Thus, every second dwelling was approached in urban areas for the booster sample.¹

Non-private dwellings were disregarded when following the walk pattern, while empty dwellings were recorded but not counted towards the required number of dwellings to be approached.

Respondent selection

Because many types of victimisation are household-based, only one respondent per dwelling was selected. This provides efficient measurement of household victimisation, and avoids potential contamination effects that might arise if more than one person in a household was interviewed. As will be discussed later in Chapter 8, weights for person-based estimates will therefore need to incorporate the number of residents aged 15+ per household to remove any household size biasing effect. This is a routine statistical procedure in household-based survey research.

To select the respondent, we asked for a list of the names and birth month of every household resident aged 15+ from the person who answers the door and then selected for interview the one who has the next birthday. For situations where the next birthday procedure was not workable, the contact sheet included an alternative procedure based on the alphabetical order of first names.

For the Māori booster sample, only Māori aged 15+ were eligible for selection. In the 2001 survey, the booster sample Contact Sheet had the following question after a general introduction: “.....Is there anyone in this household who is Māori and aged 15 years or older?” In the 2006 survey we changed the wording to be more consistent with the Census-style ethnicity question in the body of the questionnaire as follows: “... Is there anyone usually living here aged 15 years or older who might consider themselves Māori? That is, if asked which ethnic group or groups they belong to, they would include Māori.”

Selection of incidents

At four points during the interview (namely at the main screener questions, and at each of the three sets of self-completion screener questions), respondents are asked how many incidents they had experienced since 1 January 2005 of various types of crime. More detailed information is then requested through a “victim form” for some of these incidents. (See Box 3.1 in the next chapter for an outline of the questionnaire.)

A feature of the New Zealand victimisation surveys is the substantial proportion of the reported incidents that were experienced by heavily victimised respondents. For instance, half of all victimisations were experienced by the 6% of people who experienced five or more offences, and

1 Two urban NAUs were selected for the Māori booster sample that contained fewer than 80 dwellings, and were thus too small to allow this procedure to be used. Consecutive dwellings were approached in these areas instead. (Both contained at least 40 dwellings.)

several people experienced more than fifty incidents.¹ Since completing a victim form takes about 10 minutes, it would not be feasible to get heavily victimised respondents to fill in a form for each of the incidents they experienced. Instead, if a respondent recorded more than three incidents at the main interviewer-administered victimisation screener questions, victim forms were completed for three incidents randomly selected by the CAPI software. (If there were three incidents or fewer, victim forms were completed for them all.) The victim forms collect detailed information about people's experience of and feelings about victimisation, and provide critical inputs to the calculation of victimisation rates.

The sample design for selecting incidents aimed to enhance the accuracy of incidence and prevalence rates for key offence types, and provide sufficient victim form information on the characteristics of major offence types, while maintaining some consistency with the approach used in the 2001 survey. Simulations based on 2001 survey data were run for various candidate designs to explore how these affected the number of victim forms completed for various offence types. Designs producing over 400 victim forms for most offence types were preferred.

Incidents were selected independently, without replacement, with selection probabilities proportional to the weight given to the incidents' screener questions. Screener questions fell into three priority categories (low, medium and high, as shown below), with corresponding selection weights 1, 2 and 3.² The probability of selection for a particular incident depended on both the extent of competition from other incidents, and the screener question that the incident was recorded at.

Low priority (weight=1):

- Q29 (things stolen from/off vehicle)
- Q30 (vehicle tampering/damage)
- Q32 (successful burglary)
- Q34 (theft from property – outside, >\$10)
- Q43 (other)

Medium priority (weight=2):

- Q31 (unsuccessful burglary)
- Q35 (theft of household property – inside)
- Q37 (other theft)
- Q39 (threatened to damage)
- Q35.416 (damage – household property)

High priority (weight=3):

- Q28 (vehicle stolen)

1 While the victimisation reported in the survey was highly concentrated among a small proportion of respondents, these reports generally did not seem implausible. There were no obvious gaps in the distribution of incidents experienced, and even the largest numbers of incidents reported seem feasible in the context of an ongoing abusive relationship, for instance, over a reference period lasting a year to 18 months.

2 These weights were chosen partly for simplicity, and partly to maintain some consistency with the 2001 survey (in which weights of 1 and 3 were used).

- Q36 (theft from person)
- Q38 (damage – personal property)
- Q35.40 (assault)
- Q35.41 (threatened to assault)

Denote the selection weight for incident i by w_i . Then the probability of selection for incident j for a particular victim form is $w_j / \sum (w_i)$, where the sum is taken over all incidents available for selection. (Incidents that have already been selected would not be included in the sum.)

Once this incident selection design was implemented in CAPI, extensive simulation tests were conducted (along with code review) to ensure that it worked as intended.

Questions analogous to those in the victim form were asked about the most recent incident recorded at each of the three self-completion questionnaire sections. Some of the heaviest victimisation is recorded in these sections (as would be expected from their coverage of victimisation within ongoing relationships). Because only one “victim form” was allocated to each section, the probability of selecting incidents experienced by these heavily victimised respondents was very low. This results in highly variable incident weights, and may mean that the incidents with missing data are not similar to other incidents, so the accuracy of victimisation estimates could probably be improved by reallocating some of the interview time from interviewer-administered victim forms to self-completion incidents.

A similar approach was taken in the 2001 survey, although randomised incident selection was conducted manually,¹ with a simpler sample design involving just high and low priority incidents. The approach in 1996 was different again, selecting four incidents deterministically² from the main questionnaire. The approach taken for self-completion incidents was the same in all three surveys.

One planned enhancement that was not implemented was the introduction of a mini victim form for respondents reporting more than 3 incidents. This would gather some information about another randomly selected incident; not every question in the full victim form, but just what was needed to determine whether the incident should count towards 2005 victimisation rates. While this proposal survived to an advanced stage of planning, it was eventually dropped because the addition of new questions on other topics meant that questionnaire length had become excessive.

1 An automatic selection process was originally planned for the 2001 survey, but a manual randomised process had to be adopted due to limitations of the CAPI software used then.

2 In the 1996 survey, incidents were selected based on a ranking of offence types, with the most recent incident or incidents being selected when choosing between offences of the same type.

3 Questionnaire development and testing

Questionnaire development

The questionnaire used in the 2006 survey was based on that used for the 2001 NZNSCV, although several changes were made. The final questionnaire, showcards, and the contact sheets are shown in the Appendices, together with a few screenshots to illustrate the look and feel of the self-completion part of the questionnaire. The structure of the final questionnaire is shown in Box 3.1.

Box 3.1 Main topics covered in the 2006 NZCASS

1. Main questionnaire Attitudes to local crime and incivilities Concern about crime Confidence in the Criminal Justice System Neighbourhood Support ** Victimisation 'screener' questions	4. Demographic questionnaire Age Household type Ethnicity Household tenure Employment status Marital status
2. Victim Form Details of victimisation incident Reporting to the police Aspects of the police response Access to victim services ** Costs of crime**	5. Self-Completion I – Offences by partners Assault Threats of assault Vandalism to personal property Threats of vandalism to personal property Psychological Abuse**
3. Experience of E-crime **	6. Self-Completion II – Offences by people well-known Coverage as in Self-Completion I
	7. Self-Completion III – Sexual victimisation Rape Attempted rape Distressing sexual touching Other sexual violence or threats

** *These were new questions.*

As well as the new topics noted above, many changes were made to existing questions. These helped to improve consistency across different sections of the questionnaire, and addressed numerous other concerns with the previous questions. The development process attempted to balance desirable improvements against the need to maintain comparability with previous surveys.

In particular, one early decision was to retain a reference period stretching from the interview date back to the beginning of the previous calendar year. Along with the desire to maintain comparability, this period has other arguments in its favour:

- It starts on a date (New Year's Day) that has resonance for many New Zealanders¹
- It includes the entire previous calendar year

1 There is evidence from psychological research that special events whose dates are easily remembered (such as the New Year) are important in organising autobiographical memory, and from survey methods research that reminding respondents of such 'landmark events' can improve dating of other events (Tourangeau et al., 2000, p. 134).

- It is long enough to provide a substantial sample of incidents
- Including the period up to the interview date (instead of just the 2005 calendar year) avoids tempting respondents to give a 2005 date for a 2006 incident because they want to talk about it (Tourangeau et al., 2000).

Including the entire previous calendar year enables the survey to provide victimisation rates for that period, allowing easier comparison with Police statistics on recorded crime.

There were two particularly important questionnaire changes: first, changes to some of the victimisation screener questions, especially in the self-completion sections; second, changes to the beginning of the Victim Form. The implications of these changes are discussed in the Key Findings report.

Questionnaire preparation and testing

ACNielsen was responsible for questionnaire formatting, pre-testing and piloting of the questionnaire, and CAPI development. Most of the questionnaire development work consisted of discussion and desk review rather than pretesting with respondents.

A series of hard copy paper and laptop electronic pre-tests of the questionnaire were conducted from May through to October 2005. Twenty-one pre-tests were completed by researchers, both Māori research partners, and the National Field Manager. An interviewer completed an extra five interviews to check on the interviewer perspective (i.e. with less understanding of the background). We ensured a spread across age, sex, and ethnic groups. Pre-tests ranged in length from 15 minutes to 5 hours. The client also did a small number of pretests. Advice was taken from a SeniorNet tutor on introducing respondents to using a computer for the CASI section.

Changes to the questionnaire were made and submitted for programming on 28 September 2005. It was received back in two parts for checking programming routines and the administration of the questionnaire on laptops. The first part was received on 13 October 2005, and the remainder on 19 October. The questionnaire was fully tested before proceeding with a pilot survey.

The pilot survey

The pilot survey commenced with a two-day briefing over 9 and 10 November 2005 with fieldwork taking place during 11 to 30 November. Eighteen main sample area units and two Māori booster sample areas were surveyed for the pilot, half of which were intentionally “higher crime” to further test the survey.

As a result of feedback from the pilot, ACNielsen made a series of changes to the survey proper:

- Interviewer briefings reinforced the importance of achieving as many interviews as possible per area.
- ACN monitored interviewers who were not performing well in terms of response rates. Where underperformance could not be explained by the nature of the area they were working in,

under-performing interviewers would be re-trained. If necessary, they would be taken off the project.

- In the pilot when two interviewers and one supervisor saw people at home during the mail drop, they hand delivered the pamphlet, and set up an appointment for a later time. (Note that this is not attempting to do an interview.) This practice was implemented in the main survey.
- ACN thought it would be useful to offer interviewers an incentive with a monthly 'prize draw'. This was done.

4 Fieldwork methods and interviewers

Fieldwork period

Fieldwork for the NZCASS took place between 9 February and 25 June 2006. This period was divided into four fairly equal time blocks (or quarters), with sample areas spread evenly across blocks. This was so that response rates could be calculated as data collection progressed.

Fieldwork procedures

To maximise the chances of obtaining interviews at the designated dwellings in each cluster, it was necessary to institute a 'call-back' regime whereby interviewers returned to dwellings where no contact was made at the first call.

In urban areas, interviewers made an initial trip to the nine homes to be contacted in each cluster in the main sample. This was to post a Ministry of Justice pre-notification card in the mailbox. They then made up to three interviewing trips in each area. This meant a minimum of three knocks on every door. However because dwellings could be approached more than once at each visit, there was effectively the opportunity for up to six door knocks (two per visit), or perhaps even more. No prenotification trip was made in rural areas. This call-back regime achieved a contact rate of over 90% in both the main and booster samples, and increasing the number of call-backs further would have produced rapidly diminishing returns from the resources expended.

Households in the booster sample areas did not receive pre-notification cards in advance (due to the unknown eligibility of households). However, they were given them when the interviewer made personal contact at the door. Four interviewing trips were made to Māori booster areas.

With the booster sample, the lack of prenotification may have reduced cooperation, but we do not have solid evidence of the effect locally (e.g. a large-scale split-sample experiment). Nor are we aware of as much evidence on the prenotification effect internationally for face-to-face interviews as there is for self-completion or even telephone. Anecdotally, there was no strong interviewer feedback on this issue in the NZCASS, but this should be seen in context: there seems no particular reason to expect much feedback about this issue (because with booster sample respondents, we do not expect them to mention something they hadn't seen; with main sample respondents, we expect the effect more to be on the probability of refusal rather than on them explicitly saying much about prenotification).

Fieldwork timing

Most fieldwork was conducted after 2pm on weekdays or at weekends to maximise the chances of finding the selected resident at home at time of call. This procedure minimised the number of calls required to a particular dwelling to obtain the interview. It thus enhanced the chances of interviewing the selected respondent within the number of calls allowable to each dwelling. Interviewers working in rural locations or in lower socio-economic areas were encouraged to visit earlier in the day, as this is when they would be more likely to find people at home.

In addition, one of ACNielsen's field rules is that a return visit to an area must be on a weekend day if the initial visit was on a weekday, and vice versa. Typically, trips were spread over two or more weeks during the interviewing month, with the exception of remote rural areas (where the interviewer would stay 3-4 nights and complete all interviewing in this time, to save travel costs) and areas where all selected households were resolved in a shorter period, so further trips were not necessary.

Koha / Thank you gift

A small thank you gift was given to all respondents. The Ministry of Justice decided that there would be no differentiation of practice according to the ethnicity of participants, in this regard.

Interview mode

As in the 2001 survey, the interview was conducted using CAPI (Computer Assisted Personal Interview); and CASI (Computer Assisted Self Interviewing) whereby the interviewer turns the laptop over to the respondent to complete sensitive sections of questioning. Computer Assisted Interviewing also eliminates data entry costs and ensures survey logic is adhered to. The Conformat CAPI software used in 2006 was new browser-style software. (The CAPI software used in 2001 was extremely problematic and seen as inappropriate for re-use.)

One important advantage of the new software was that the selection of Victim Forms could be computerised, based on answers from the screener questions. In 2001, an awkward paper-based system was used to select offences for Victim Forms when the screeners showed more than three eligible incidents.

General interviewer management

Given the complexity of this survey, interviewers working on the project were given substantial project-specific training and briefing. The company's standard interviewer selection and training procedures are outlined below, together with the nature of specific training and briefing required for the 2006 NZCASS.

As with the 1996 and 2001 surveys, close attention was paid to interviewer selection, bearing in mind some of the sensitive nature of the questionnaire and the stringent ethical considerations which have to apply. ACNielsen's policy was again to use female interviewers wherever possible, and to use Māori interviewers as much as possible for the booster sample. Of the 77 interviewers who worked on the project, 9 were male and 20 identified as Māori.

Recruitment

ACNielsen has a core of trained and experienced interviewers available for this type of project. Extra interviewers were also needed due to the large size of the NZCASS.

All applicants are initially interviewed by a field manager. At this time, they are required to provide personal references and sit an assessment test. If reference checks are satisfactory, they are then formally inducted. This process includes being familiarised with and signing both an employment agreement and a performance agreement. Interviewers are also familiarised with a job description, the company's Health and Safety Handbook, the Field Handbook, and the Field Policy Document.

Māori interviewers

Recruitment of interviewers experienced in interviewing Māori presented difficulties at the start of fieldwork. One cause was the delay from the originally planned date for fieldwork starting in mid-2005. (Potential Māori interviewers were first approached by LLE Research around the mid-2005 date.) Also, many potential recruits were not able to accommodate the near full-time workload required. In the end, very few of the candidates from LLE Research proved suitable. Thus, ACNielsen had to recruit several Māori interviewers in early 2006 just before fieldwork began. There was a particular shortage of Māori booster interviewers in the Auckland area at the start of fieldwork.

General training

Initial general training for interviewers comprises a minimum of six hours basic training.¹ This is generally carried out in three two hour blocks covering four modules:

- Module 1 – Introduction to Market Research
- Module 2 – Introduction to Interviewing
- Module 3 – The Questionnaire
- Module 4 - Probing

A handbook was provided with each module. When basic training was completed, an Interviewer Induction Training Record Form was completed and signed both by the Trainer and the Trainee.

Interviewers are also briefed about every survey on which they work. At the time trainee interviewers are briefed on their first project, a “buddy” and “observer” system is used. This means that trainee interviewers are accompanied on their first foray into the field by a buddy, who observes and provides on-the-spot encouragement, advice and assistance. Briefing and training specifically for the NZCASS is discussed below.

NZCASS training and briefing

The training and briefing for this project were split into two days. During the first day, interviewers were familiarised with the laptops and the questionnaire. These briefings were conducted by ACNielsen regional supervisors some days before the second seminar (except in Christchurch, where they had to be on consecutive days due to larger travel costs). After this session, interviewers could complete practice interviews with family/friends to build up familiarity with the very lengthy questionnaire and use of CAPI.

The second day covered:

- Overview of crime surveys and their use (Ministry of Justice; CJRC Victoria University)
- Feedback and questions resulting from practice interviews

1 ACNielsen has also developed a re-training module, designed as a refresher, or where specific circumstances demand. This include issues of poor response rates, where it has become apparent that procedures and methodology are not being correctly followed, or where there is a special requirement for a particular project.

- Briefing on the hardest parts of the questionnaire (e.g. unusual complexities at the start of the victim form about series incidents or where one incident was coded in more than one screener) by ACNielsen researchers
- Fieldwork method (contact sheets, callsheets, letterbox drop, sampling)
- Cultural awareness and the Māori perspective (led by Māori consultants)
- Turning around refusals
- Interviewer safety.

The turning around refusals component was Refusal Aversion Training (RATS). This training includes the following:

- Identify the source of initial concern
- Select and deliver an averting statement that addresses their concerns
- It's not what you say, it's the way that you say it
- Common themes of resistance.

Interviewers could not start interviewing until both days' training was completed. Hence, interviewing started in some regions before others. The second day's training was conducted in Wellington (7 February), Auckland (9 February), Christchurch (14 February), and Rotorua (16 February). Each briefing was attended by about 20 interviewers.

Overall, a total of 28 hours of training was provided for new interviewers in the 2006 NZCASS, and 22 hours for experienced ACNielsen interviewers.

5 Checks and audits

CAPI range and consistency checks

Several range and consistency checks were embedded in the CAPI questionnaire script to ensure the data was of high quality. For instance, if the value of a stolen vehicle was reported as being \$50,000 or more at Q85 a pop-up window appeared, prompting the interviewer to check that this amount was correct.

Interview checks and validation

Checking and auditing of interviewers' work was undertaken at two levels. This was through (i) area coordinators' checks, and (ii) telephone validation interviews.

Checks by area coordinators covered fieldwork support material - i.e. all area maps, call sheets, respondent selection sheets and analysis forms. Area maps were checked to ensure that all designated dwellings were within map boundaries, that the correct start point was used, that the correct calling procedure was used after the start point, and that the call address record form was completed. Call sheets were checked to ensure that the calling and listing procedures were correctly followed. They were also checked against timesheets. Respondent selection sheets were checked to ensure that the correct respondent was selected.

Telephone audit interviews were conducted for a systematic selection of 25% of each interviewer's work, spread over each NAU the interviewer worked in. These audits confirm that the interview took place according to the prescribed standards. Originally ACNielsen planned that where an interview selected for audit was obtained from a dwelling with no telephone, a mail audit would be undertaken. But on later consideration it was decided that a mail audit was inappropriate for this survey given the confidentiality and safety issues this raised, so no audit interviews were conducted by mail. Sufficient dwellings with telephone numbers were available to adequately check each interviewer's work. If this had not been the case for any individual interviewers, their work would have been checked through a door-to-door audit.

Due to the sensitivity of many questions in this survey, the audit information centred on length of interview, time of interview, and respondent selection procedures (rather than repeating questions from the main questionnaire).

These audits revealed that one interviewer had been falsifying interviews. The affected areas were reworked, which delayed the end of the fieldwork by one week.

Data checks

ACNielsen checked the plausibility of frequency tables and their compliance with skip instructions. They also ran a set of programmed edit checks in SPSS. This uncovered only a small number of irregularities.¹

More detailed checks on consistency naturally arose during more detailed statistical analysis by Statistical Insights. These checks identified a problem with coded Victim Form data, which arose from a known bug in Confirmit. They also identified another 49 cases with inconsistent data. Most of these occurred early in the Victim Form.

Some possible outliers were apparent at the incident screener questions. These were examined to see whether they showed any signs of respondents being unduly creative, but most did not seem implausible. Their treatment is described in Chapter 9 on imputation.

1 The most common (affecting 37 cases) was corrected by reference to contact sheets. It arose from the CAPI validity check programmed for Q147 concerning the number in household aged under 15 years being imprecise.

6 Response rate and interview length

The response rate in the main sample was 59%. For the Māori booster sample, it was 56%. The corresponding figures were 65% and 57% in the 2001 survey and 56% and 66% in the 1996 survey. Response rates have thus varied between each of these surveys. To put this in context, a decline in response rates has been generally evident over this period across a number of surveys, and in a large number of countries. For instance, the response rate for the National Crime Victimization Survey (NCVS) in the United States has dropped by approximately one percentage point per year over the last ten years.¹

Table 6.1 shows an analysis of response rates. Of the issued sample of 7,614 addresses in the main sample, 392 were unoccupied, and for 590 there was no contact made. This gave a contact rate of 92%. Of those households contacted, the main reasons for non-response were unavailability during the survey period (11%) or refusal - either by the selected person (11%) or by the household before a respondent could be selected (10%). Non-contact accounted for most of the other failures.

Table 6.1 Analysis of response rates

	Main sample	Māori booster sample
A. Total dwellings approached	7614	13137
B. Unoccupied dwellings	392	382
C. Total occupied dwellings approached (= A - B = D + E)	7222	12755
D. Contacted dwellings (=D1+D2+D3 for booster sample)	6632	12147
D1. Known eligible contacted dwellings (i.e. with one or more Māori adults) (=J+K+L+M+N)		1947
D2. Known ineligible contacted dwellings		9788
D3. Dwellings with unknown eligibility (=H+I)		412
D4. Estimated eligible contacted dwellings (=D*D1/(D1+D2))		2015
E. Non-contact (dwellings where no contact made after call regime completed)	590	608
E1. Estimated eligible non-contact dwellings (=E*D1/(D1+D2))		101
F. Estimated eligible occupied dwellings (=D4+E1, only calculated for booster sample)		2116
G. Contact rate (=D/C for main sample, =D4/F for booster sample)	92%	95%
H. Unapproachable house	65	158
I. Household refusal	685	254
J. Respondent not interviewable	174	40
K. Respondent refusal	756	346
L. Respondent unavailable throughout survey period	708	367
M. Interview not completed	15	7
N. Successful interview ²	4229	1187
O. Conversion rate (=N/D for main sample, =N/D4 for booster sample)	64%	59%
P. Response rate (=N/C for main sample, = N/F for booster sample)	59%	56%

1 The response rate for individuals in the 2005 NCVS was 84%, down from 90% in 2000, on top of household response rates of 91% and 93% respectively.

2 Respondents who only refused to complete the self-completion section were counted as complete interviews.

A corrected response rate is shown in Table 6.1 for the Māori booster sample. The corrected figure incorporates an estimate of the proportion of ineligible dwellings (i.e., those with no resident Māori person aged 15+) among those dwellings at which no contact was made after the call regime had been completed, or where the household's eligibility might otherwise have not been established. The corrected figure is thus an estimate, but is a better estimate of the actual response rate than the pessimistic figure of 40% obtained by simply dividing the number of complete interviews by the total number of occupied dwellings approached.

There was a large difference in the raw household refusal rate between the main and booster samples (9% versus 2% respectively), but this is easily explained. First, the booster contact sheet (in Appendix A7) shows that the determination of whether there is anyone ethnically eligible was done early on, before consent to a full interview was explicitly sought. Hence the appropriate base for comparisons is not all dwellings approached, but eligible dwellings contacted. (For the booster sample, this excludes the majority of dwellings approached.) The household refusal rates are then similar for both samples (10-11%). Second, and more subtly, it is possible that some households with Māori effectively refused by not admitting to having Māori in the household, and were therefore recorded as ineligible rather than refusals. This would be consistent with the lower than expected incidence of Māori households noted below.

The rate of unoccupied dwellings also differed somewhat between the two samples. Holiday homes make up a large proportion of this category, and are tightly clustered. For example, just 4 area units accounted for 40% of the holiday homes encountered in the main sample. This clustering may increase sampling error enough to account for the difference. Also, many areas dense in holiday homes were less likely to be selected for the Māori booster sample. For example, three of the four area units mentioned above were in the South Island and had a low density of Māori households.

While the drop in the response rate was somewhat disappointing, there were nonetheless a number of initiatives adopted to encourage participation in the survey:

- A Ministry of Justice pre-notification card was dropped into the mailbox of all the nine homes to be contacted in each cluster in the main sample. Households in the booster sample areas did not receive pre-notification cards in advance (due to the unknown eligibility of households), but got them when the interviewer made personal contact at the door.
- Households contacted were given a letter signed by the Secretary for Justice with details about the survey. This letter was also available in Māori, Cook Island Maori, Samoan, Tongan, Chinese (both full form and simplified), Japanese and Korean translations.
- New promotional material for the interviewers and respondents was designed to give the NZCASS an attractive 'image brand'.
- Promotional activity took place in newspapers, websites, and on the radio to increase awareness of the survey and provide evidence of authenticity.
- Interviewers with low response rates were identified and re-trained and monitored.
- A prize draw for interviewers who achieved excellent response rates took place each quarter.
- Respondents were offered a summary of the results of the survey, due to be provided in 2007.
- Considerable effort was made to recruit Māori interviewers to work in the Māori booster sample areas.

The response rate was highest during the first quarter of fieldwork, and lowest during the third quarter. It was also lower in the most deprived areas (the lowest quintile), in the upper North Island and particularly in Auckland. Response rates did not differ markedly based on interviewer experience, which was surprising. Details of this are given in Appendix A1.

The final sample of 5,416 was reasonably close to the 5,600 originally expected.¹ There were more Māori in the main sample than expected, whereas fewer interviews were obtained with the Māori booster sample (1,187) than hoped for (1,600).

Although the response rate for the booster sample was slightly lower than for the main sample, this was not the largest contributor to the shortfall. A lower than expected incidence of Māori households was the main cause. The proportion of booster sample households approached that reported being eligible (i.e. that a Māori person aged 15 or more lived there) was only 22.6% (=2967/13137). This was substantially lower than the eligibility rate in the 2001 survey of 27.0% (=1115/4132). The relatively blunt screener question may have contributed to this.² Interviewers reported that some households that clearly appeared to have Māori residents claimed to be not eligible. In all, 1,698 Māori interviews were completed, adding those from the main sample to those from the booster sample.

Comparing the response rate data for the Māori booster sample in 2001 and 2006 revealed three differences large enough to possibly be of practical interest:

- A higher proportion of dwellings reported no eligible adult, as discussed above.
- Fewer unoccupied dwellings were encountered in 2001 (2.9% of dwellings versus 4.2%). Although the difference in holiday homes mentioned above partly explains why this is lower than the rate in the main sample, we see no obvious explanation for the difference between the rates for the 2001 and 2006 Māori booster samples. (The rate of unoccupied dwellings in the main sample was 5.1% in both surveys.)
- A higher proportion of (estimated) eligible dwellings were classified as unapproachable. This may have resulted from increased emphasis being placed on interviewer safety in more recent years.

Response to the Self-Completion components

Of those who completed the interview in other respects, 6.4% (=349/5416) refused to complete the Self-Completion component.³ This compares with around 4% in the 2001 survey. Refusals were disproportionately from older respondents (11% for those aged 60 years or older compared with 5% for those aged 15–59). The longer interview length in the 2006 survey before the Self-Completion components started, compared with 2001, may have been a factor in the higher Self-Completion refusal rate. Another may have been the stronger emphasis put on the consent process at the

1 There were rather more interviews completed (5,476), but about 1% were deleted from the dataset due to irregularities in procedure, incomplete data etc.

2 The wording of the question in 2006 was “Is there anyone usually living here aged 15 years or older who might consider themselves Māori? That is, if asked which ethnic group or groups they belong to, they would include Māori.” The 2001 wording was “Is there anyone in this household who is Māori and aged 15 years or older?” This was changed to better match the official ethnicity standard.

3 By way of comparison, 5% of people in the 2004-05 British Crime Survey refused similar self-completion components. The New Zealand rate therefore is not unusual, especially as the BCS only requested self-completion from those aged under 60 years (Grant et al., 2006: 53).

beginning of the survey informing respondents they can stop the interview at any time. When respondents were asked why they refused, the most common reasons given were computer concerns, not wanting to answer, and being too busy or too tired.

Interview length

The average interview length was 52 minutes, five minutes longer than in 2001. There was a wide variation, ranging from less than 30 minutes for some interviews to more than 70 minutes for others. A quarter of the interviews took longer than an hour. Interview length is derived from questionnaire length (recorded on the laptop), plus ten minutes added as the average time for getting settled and disengaging with the respondent at the close of the interview.¹ Table 6.2 shows details.

Table 6.2 Interview duration

Interview duration	Percentage of interviews	Minutes
Up to 30 minutes	4	
30 – 39 minutes	23	
40 – 49 minutes	27	
50 – 59 minutes	20	
60 – 69 minutes	11	
70 minutes or more	16	
Median		48
Mode		43
Mean		52

The average questionnaire length for non-victims was 34 minutes, and 53 minutes for victims (the longer time for victims being entirely expected). Table 6.3 shows details.

Table 6.3 Average interview duration by number of Victim Forms completed

Number of Victim Forms completed	Average interview duration (minutes)
No Victim Forms	34
One Victim Form	46
Two Victim Forms	56
Three Victim Forms	65
One or more Victim Forms	53

¹ The 5,274 cases used to calculate interview length is less than the total number of interviews because some interviews (those where one section of the questionnaire was recorded as taking 60 minutes or longer) were excluded. This most likely occurred because of a clear break in interviewing, such as finishing the interview on a different day. Thus, the average interview length is probably a little understated since a break is more likely to have been taken in unusually long interviews.

7 Classifications and coding

Classifications and standards

Using standards and classifications increases the value of survey data, as outlined in the Official Statistics System Statement of Principles (Statsphere 2006: 8). A number of official classifications and standards were used in the 2006 NZCASS, including:

- Statistical Standard for Ethnicity 2005
- Standard Classification of Ethnicity (2 digit, with one addition; see below)
- Statistical Standard for Occupation 2002
- New Zealand Standard Classification of Occupations 1995 (4 digit)
- New Zealand Socio-economic Index
- Statistical Standard for Marital Status 1999 (using a combined legal and social marital status classification)
- New Zealand Index of Deprivation – NZDep 2001 (not official, but independent and widely used)
- The MOSAIC geodemographic classification (not official, but independent and widely used)
- Statistical Standard for Usual Residence 1999
- New Zealand Standard Areas Classification (for Territorial Authorities, used to define broad regions, and Urban Areas)
- All of these except MOSAIC (PMP Micromarketing, 2006) and NZDep (Salmond and Crampton, 2002) are documented by Statistics New Zealand on their website (www.stats.govt.nz).

Coding

Information given in relation to the main income earner's occupation had to be coded for socio-economic classification. For the 2006 survey, socio-economic classification was done using the New Zealand Socio-economic Index (or NZSEI). This was a two-stage process. First, ACNielsen coders translated the open-ended responses to questions about occupation and main tasks in the occupation (questions Q157-Q160) into the New Zealand Standard Classification of Occupations 1990 (Statistics New Zealand, 2001), following the Statistical Standard for Occupation 2002. Second, these codes were converted into NZSEI scores using a concordance available from Statistics New Zealand. NZSEI scores range from 10 to 90, and were collapsed into six groups for tabulation purposes.

Other coding completed by ACNielsen was:

- Backcoding from 'Other Specify' comments into the existing code frames (i.e. correcting interviewer errors in code selection). Given that no new codes were created, we did not create extra coding documentation for this. Instead, coders relied on the existing code frame and

interviewer instructions. (Only a few cases were unclear based on these documents; they were referred to researchers for decisions.)

- Coding of ethnicity responses other than the main groups presented (as in the Census) into level 2 of the Statistical Standard for Ethnicity (2005). In addition, a separate code was assigned to responses of New Zealander and/or Kiwi, even though the official standard only includes this code at level 4. These were combined with European responses in the Key Findings and Topic Reports.

In all cases, open-ended responses were typed directly into the laptop, and subsequently coded by coding staff in the office rather than by interviewers in the field.

Classifying incidents

The classification of offences was a separate exercise done after the end of fieldwork. In essence, incidents needed to meet legal definitions of criminal behaviour to be counted. The coding principles were agreed with New Zealand Police. Full details of the offence coding procedures are given by Cameron and Mayhew (2006); some highlights relevant for analysis and interpretation are reviewed below.

Classification of offences is based on the responses in the Victim Forms and Self-Completions, including a short description in the respondent's own words. It also draws to some extent on which screener question was answered affirmatively.

There were some changes made to the coding exercise for the 2006 survey compared to the two previous ones. The main ones were:

- Greater reliance on information from the Victim Form. In 1996 and 2001, the offence code decision was made principally on which screener question the offence came from, and the offence description given by the victim in the Victim Form. In 2006, a substantial amount of other data from the Victim Form itself was inspected. The number of offences that could not be coded in 2006 was smaller than in the two previous surveys where many incidents were not coded because of doubts about their 'relevance' (see above), or which offence code they should attract. In 2001, 22% of incidents for which a Victim Form was completed were not coded either because they were 'out of scope', or an offence code could not be allocated.¹ In 2006, the figure was 15%.
- Double offence codes. In New Zealand, an incident of crime can give rise to more than one offence being coded by the police if two dissimilar or unrelated offences occurred at the same time. (If one offence, say damage to a car, is simply part of another, say, theft from a car, or the means of committing it, then only the major offence is coded.) There was discussion with New Zealand Police on the issue of double coding at the start of the coding exercise. The result of this was that more crime incidents from the 2006 survey were given two offence codes (6%) than in 2001 (1%). The increase appeared to relate to burglaries; if combinations of burglary with vehicle offences are excluded, the 2006 proportion also drops to 1%. Estimated risks of victimisation take account of both offences, except for combinations of burglary with bicycle theft or vehicle offences, which are treated as burglaries only. Thus, most of the second codes were

1 The figure in 1996 was 9%, although this may not be comparable due to the very different incident selection framework used in that survey, and the large number of victim forms required by the design that were missing in the 1996 survey.

discounted in the calculation of victimisation rates from the 2006 survey, resulting in little difference from 2001.

In order to make better comparisons between the results from the 2006 and 2001 surveys, two approaches were taken with respect to coding 2006 offences. They were coded first on the basis of the best information in the Victim Form. They were also coded as it was judged they *would have been coded* using the procedures used in the 2001 survey. As would be expected, more 2006 incidents were deemed 'out of scope' when 2001 coding procedures were followed.¹ This had the most noticeable effect with regard to incidents reported in the Self-Completion components. The comparison of rates of victimisation in 2005 compared to those in 2000 (from the 2001 survey) in Chapter 2 of the Key Findings report draws on the '2001-like' coding approach.

Table 7.1 shows the 2006 survey offence codes alongside the corresponding codes from the 1996 and 2001 surveys. It also shows whether they attract the personal (P) or household (H) weight, which depends on the nature of the offence. A few changes to the weights between 2001 and 2006 should be noted. Offences excluded from the survey count are also shown.

1 22% of 2006 incidents were coded as out of scope using 2001 procedures, as against 15% under 2006 coding procedures. The most marked effect was in relation to incidents from the Self-Completions. Using 2001 coding procedures on the 2006 survey data, 24% of these incidents were not coded, whereas it was a much lower 7% under 2006 procedures. In the 2001 survey, just over 30% of incidents from the first and second Self-Completion components were not coded.

Table 7.1 Offence codes

	1996 Offence codes	2001 Offence code	2006 Offence codes	2006 Weight	Not counted
Sexual violation of women	1	1	1	P	
Sexual violation of men	2	2	2	P	
Incest	3	3	3		✓
Indecent assault	4	4	4	P	
Indecent exposure	5	5	5		✓
Grievous assaults	6	6	6	P	
Other assaults	7	7	7	P	
Abduction/kidnapping	8	8	8	P	
Robbery	9	9	9	P	
Theft from person	10	10	10	P	
Burglary (old definition) ¹	11	11	11	H	
Burglary (new definition)	-	-	41	H	
Theft from inside home (right to be there)	12	12	12	H	
Theft from outside home, over \$10	13	13	13	H	
Taking/conversion motor vehicle	14	14	14	H	
Unlawful interference / getting into motor vehicle	15	15	15	H	
Theft from motor vehicles	16	16	16	H	
Taking /conversion / unlawful interference with bicycle ²	17	17	17	H	
General theft of personal property	18	18	18	P	
Arson	19	19	19	H	
Wilful damage to household property (new) ³	20	20	27	H	
Wilful damage to personal property (new)	20	20	28	P	
Threatening to kill / assault / threatening behaviour	21	21	21	P	
Threatening to damage personal property (new)	-	-	29	P	
Extortion / blackmail	22	22	22		✓
Unlawfully in building (no intent to commit offence)	23	23	23		✓
Peeping Toms, lurking etc	24	24	24		✓
Fraud	-	25	25		✓
Damage to motor vehicles ³	20	26	26	H	
In scope, but not able to tell which offence			85		✓
Not an offence	88 ⁴	88	86		✓
Offence not in scope			87		✓

- 1 The legal definition of burglary changed between the 2001 and 2006 surveys. It now covers thefts from enclosed yards, which may have been included under Code 13 before, and does not require forced entry. Thus, a new code has been added to handle the new definition. The old code has been retained to allow comparisons with previous surveys.
- 2 Bicycle theft was treated as a personal offence in 1996 and 2001, so (in theory) thefts of bicycles belonging to other household members were excluded. Also many bicycles were stolen from outside the house, and should in principle have been given Code 13, but were coded as bicycle theft. In 2006, bicycle theft is to be treated as a household offence.
- 3 In previous surveys, Code 20 covered wilful damage in general. This included damage to both household and personal property in 2001. Damage to motor vehicles was also included in Code 20 in 1996.
- 4 'Not relevant' codes were only added to the 1996 data during analysis of 2001 survey data. Prior to that, these incidents were simply omitted from the dataset.

8 Survey weights

Weights are usually applied to sample survey data during its analysis to adjust for factors such as differential selection probabilities, non-response patterns and sample skews relative to population figures. The NZCASS is no exception.

The sample design for the NZCASS covered four levels: Nielsen Area Units, households, people, and victimisation incidents. Weights have been calculated for the NZCASS data at three of these levels; households, people, and incidents. These weights incorporate adjustments for each of the factors listed above.

Household weights

Wells (1998) gives formulae for the calculation of weights for a screening booster sample design. These only provide for person weights, however, and household weights are needed to analyse household offence in the NZCASS. Initial household weights were calculated as the reciprocal of each household's estimated probability of inclusion in the sample, across all three samples. (Person weights will be calculated using a similar process for consistency.) The inclusion probability for household i was calculated as follows:

$$\begin{aligned} p_i &= P(\text{household } i \text{ included}) \\ &= P(\text{household } i \text{ selected and eligible}) \\ &= \sum_{s=1}^2 p_i^s r_i^s \end{aligned}$$

where p_i^s is the probability that household i was selected for sample s and r_i^s is the probability that household i was eligible for sample s . (The main sample and Māori booster sample are indexed by $s = 1$ and 2 respectively.)

Since Nielsen Area Units are selected for the main sample with probability proportional to the number of households they contain, and a fixed number of occupied households are approached in each NAU, each household has approximately¹ the same probability of being approached for the main sample. This probability can be calculated as the number of NAUs selected multiplied by the number of households approached per NAU, divided by the number of households. Specifically,

based on the 2001 Census data used for sampling, $p_i^1 = \frac{800 \times 9}{1342644} = 0.00536255$.

Similarly, the household selection probability for the Māori booster samples was calculated as the number of dwellings selected for the relevant sample in each selected area multiplied by the number of areas selected for that sample, divided by the estimated number of eligible dwellings in the relevant sampling frame (i.e. in NAUs with a high enough incidence of Māori households).

1 Some households may have a greater or lower chance of selection due to changes in the number of occupied dwellings in that NAU since the 2001 Census, but since these changes were not known when survey weights were calculated, this could not be taken into account in calculating the weights described here. There is some resulting potential for bias in the results. The magnitude of any such bias cannot be firmly established until the 2006 Census figures are released at meshblock level, but an analysis based on preliminary Census figures at area unit level indicated that the potential bias from this source would be negligible relative to sampling error.

Specifically, this was $p_i^2 = \frac{320 \times 40}{1329489} = 0.00962776$ for the Māori booster sample (or zero outside the Māori booster sampling frame).

The second element of the household inclusion probability calculation is the probability that a household was eligible for each sample. All households in the dataset were eligible for the main sample, i.e. the probability of eligibility for the main sample is 1. However household eligibility for the Māori samples was not known for non-Māori respondents in the main sample (unless the household was in a low incidence NAU and thus was not in the relevant sampling frame, or the household only contained one person aged 15 or more). The probability r_i that household i was eligible for the Māori booster sample was 0 if the NAU was outside the frame for the Māori booster sample; otherwise it was 1 if the respondent was Māori; otherwise it was 0 if the number of people aged 15 or more in household was 1. For the remaining households, the desired probability was estimated as

$$r_i = \frac{uv}{1 - (1-u)v} \text{ where } u \text{ is the proportion of people in the household aged 15 or more who would}$$

be eligible for the Māori booster sample, averaged over all households known to be of mixed eligibility, and v is the proportion of households in the Māori booster sampling frame that were eligible. Although this formula estimating r_i for the remaining households (i.e. those in the Māori booster sample frame where the respondent was not Māori and did not live alone) was consistent with that used in the 2001 survey, it is only an approximation and could be improved in two ways; by conditioning the calculation of u and v on the number of people in the household aged 15 or more (combining the larger household sizes, which have low sample sizes), and by replacing the units in the denominator by a factor w being the proportion of main sample households in the Māori booster sampling frame which have at least two residents aged 15 or more.¹

The initial household weight was calculated as the reciprocal of the household inclusion probabilities described above. The resulting weights had an average value of 130.0, a coefficient of variation of 0.345, and ranged from 66.7 up to 186.5.

A non-response adjustment was made to these initial household weights. The response outcome data from each NAU was modeled using logistic regression. First a model was fitted using the following predictor variables: interviewer ethnicity (Māori or non-Māori), sample (Māori booster or main sample), interviewer experience (in years), the 2005 crime rate in that Police Station Area (on a truncated log scale), broad region, level of urbanisation, and deprivation index (NZDep2001). Then the non-response model was selected by sequentially removing predictor variables that were not statistically significant, until only significant variables remained. The final predictors were the sample, broad region, and level of urbanisation. Initial household weights were then divided by the predicted values from this model, which ranged from 0.457 to 0.692. The resulting weights had an average of 225.0 and a coefficient of variation of 0.335, and ranged from 96.4 to 354.0.

1 A derivation of the formula with the latter improvement is given in Appendix A5.

Table 8.1 Parameter estimates for non-response model

Variable	Level	Parameter estimate	Std. Error
Intercept		0.10748	0.04638
Sample	Booster	-0.27991	0.05542
Urbanisation	Other metro cities	0.11754	0.09613
Urbanisation	Other main urban areas	0.01404	0.07274
Urbanisation	Secondary urban areas	0.30585	0.10641
Urbanisation	Rural/minor urban areas	0.35064	0.07085
Region	Lower North Island	0.35032	0.06883
Region	South Island	0.13368	0.07566

Each of the predictor variables in this model were also related to survey measures such as victimisation rates. Of these, sample membership generally has the strongest relationship. For instance, the incidence rate for any victimisation in the main sample was 109 offences per 100 people, with a 95% confidence interval of (98, 120), while the rate in the booster sample was 215 offences per 100 people, with a 95% confidence interval of (193, 237).

The household weights resulting from the non-response adjustment were then post-stratified by level of urbanisation, based on the estimated number of households in each category as at 30 June 2006.

Table 8.2 Population targets for household weight calculation

Urbanisation	Number of households
Auckland	414400
Wellington/Christchurch/Dunedin	337021
Other main urban areas	342613
Secondary urban areas	103450
Rural/minor urban areas	360815

The final household weights after post-stratification ranged from 119.8 to 475.6, with an average of 287.7 and a coefficient of variation of 0.351. These weights were used for analyses of household characteristics, and in particular to calculate incidence and prevalence figures for household offences.

Person weights

Person weights were calculated in a similar way to household weights. The only differences were that the initial household weights were multiplied by the number of people living in the household who were eligible to be interviewed¹ (to adjust for only one person from each household being interviewed), and that the person weights were raked² by combinations of age, sex and ethnicity

1 If the number of eligible household members was greater than six, a value of six was used instead. This affected 14 respondents.

2 Raking (Deming and Stephan, 1940), also known as rim weighting, enables the simultaneous control of marginal distributions for several benchmark variables. It was implemented here using Lumley's (2006) *rake* function.

instead of by urbanisation. (The population targets used are shown in the following table.)
Adjustment for non-response used the same non-response model as for households.

The initial inverse probability person weights ranged from 66.7 to 1119.0, with an average of 332.4 and a coefficient of variation of 0.577. After the non-response adjustment, the person weights had an average of 575.8 and a coefficient of variation of 0.585.

The final person weights after raking ranged from 53.4 to 3208.0, with an average of 588.1 and a coefficient of variation of 0.722. The lowest weights were for Māori women aged 40-59, with their final weights being slightly lower than their inverse probability weights. In contrast, the highest weights were for Asian men aged 15-24, and these weights were almost 3 times higher than inverse probability weights for these respondents.

Person weights were used in the calculation of incidence and prevalence figures for personal offences, and for the analysis of self-completion lifetime prevalence data and of most data from the main questionnaire. No further adjustments were made to account for non-response to the entire self-completion component.

Table 8.3 Population targets for person weight calculation

Weighting Control Group	Estimated population 30 June 2006
Males 15-24	309030
Males 25-39	409400
Males 40-59	549190
Males 60-69	165870
Males 70+	154620
Females 15-24	295080
Females 25-39	435750
Females 40-59	569040
Females 60-69	172210
Females 70+	204430
Māori Males 15-39	122834
Māori Males 40-59	59454
Māori Males 60+	18373
Māori Females 15-39	128801
Māori Females 40-59	64921
Māori Females 60+	21157
Non-Māori	2849080
Pacific	186000
Non-Pacific	3078620
Asian	308000
Non-Asian	2956620

Incident weights

Incident weights were derived from person weights by dividing them by the selection probability for that incident (given that the current respondent had been selected). The probability that a given incident from the main questionnaire was selected for a victim form depended on whether it was a high, medium or low priority incident, as well as how many low, medium and high priority offences were experienced by that participant. It was calculated simply by enumerating the relevant parts of the probability space through a branching process and adding up the probabilities for each of the appropriate nodes. However, if the incident selection probability was less than 0.1, a value of 0.1 was used instead.¹ This avoided the high variability that would result from using the incident probabilities from heavily victimised respondents, at the cost of introducing the potential for some bias.² Some exploration of potential and likely levels of bias suggested that 0.1 was within the range of reasonable values to use from a statistical standpoint. Since this value was used in 2001, it was adopted in the 2006 NZCASS for consistency.

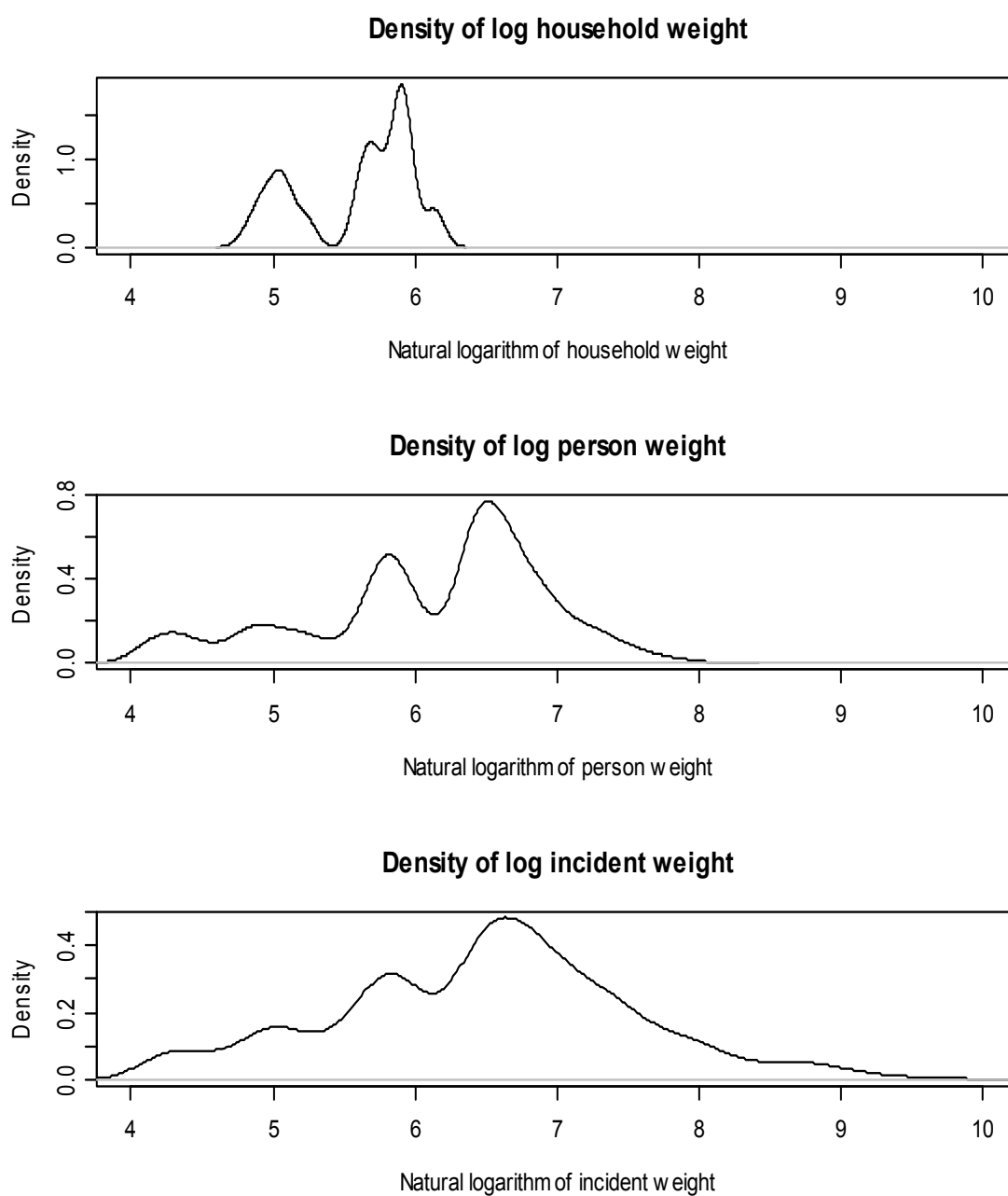
Incidents from the self-completion questionnaire had a much simpler selection probability, being just the reciprocal of the number of incidents reported at all screeners in that section.

Isolated missing values for the number of incidents (e.g. from a “Don’t wish to answer” response to a particular screener question) were imputed with the value 1, as was done in 2001. While it would be correct to derive a second set of incident weights from the household weights rather than the personal weights, and use these for analyzing household incidents, this would have introduced some bias in comparisons between 2001 and 2006 results because incident weights were derived from personal weights for all incidents in the 2001 survey. To maintain comparability with the approach used in 2001, personal weights were used for all incidents in the 2006 NZCASS. Although this will have introduced some bias in the 2006 results, all these results have been expressed in terms of percentages of incidents, which avoids the large bias that would occur if these weights were used to estimate total numbers of incidents. This method also eliminates the bias that would be introduced in comparisons of 2001 and 2006 results if household weights were used in 2006.

1 This truncation of the distribution of probabilities affected 176 incidents. Although these incidents made up only 4% of sampled incidents, they would have accounted for 34% of the total of all incident weights if this truncation had not been applied. After truncation, they accounted for only 16% of the incident weight total.

2 The term “bias” is being used here in a technical sense, meaning the extent to which the average of the results would not agree with the true population figures (if these were known), supposing that the survey was conducted many times in the same circumstances but different samples were selected according to the same design.

Figure 8.1 Densities of logarithms of the household, person and incident



Weights

The density plots above show that incident weights are much more variable than person weights, which in turn are much more variable than household weights. This reflects the large variation in selection probabilities at each of the last two stages of the NZCASS sample design. Since increased variation in weights can substantially decrease the effective sample size, the next comprehensive redesign of the survey should consider ways to reduce this variation.

9 Imputation

Since some respondents are heavily victimised, a proportion of incidents will have no victim forms. In fact, most incidents have no victim forms. Victim forms were completed for only 33% of the incidents reported in the NZCASS. Most of the data collected in victim forms was analysed using the incident weights described in Chapter 8, which provide an effective method of analysis when each victim form can be viewed in isolation. However, direct calculation of incidence and prevalence rates required information about all the incidents experienced¹ by each survey participant, including three items collected on each victim form: whether the incident occurred during the year 2005; whether the incident was an offence within the scope of the survey (termed being relevant); and which detailed offence codes the incident falls under. Another item that can now be derived from victim form data (subject to some assumptions) is how likely the incident was to have been reported a second time at another screener question.

Information on these items was required for all incidents, including the 67% of incidents without victim forms, to enable the direct calculation of incidence and prevalence figures. However this data is missing for the incidents without victim forms, necessitating some form of imputation² for this missing data. The process of imputation also affects the sampling error of the results, although for some imputation methods it is hard to figure out how much. Multiple imputation (Rubin, 1987) has been used in the NZCASS to quantify this effect, via Lumley's (2004) 'mitools' package. Ten imputations have been used throughout.

Imputing the number of incidents

If the number of incidents at any screener question was missing (i.e. the respondent said "Don't know / Can't remember" or "Don't wish to answer"), a value of 1 was imputed. In other words, it was assumed that the respondent reported being a victim of just one incident. This is likely to be a poor assumption in many of these cases, but it was hoped that this provided a reasonable compromise between overcounting for those who weren't victims and undercounting for those who really experienced more than one such incident. It was suspected, however, that most of these responses would be from victims, since non-victims would presumably not have trouble remembering the answer and would have less reason to be averse to answering the question. This suggests that the approach used here probably underestimates the true level of victimisation, and that other common imputation methods (such as a random hot-deck) would also suffer from a similar problem.³ Hot-deck imputation would also introduce more variability. The primary reasons for imputing a value of 1 were that this approach was used in the 2001 survey, and that no clearly superior method was identified.

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- 1 Strictly speaking, this is most critical for prevalence rates, since incidence figures could be calculated directly from the incident weights. These figures would be less reliable than those based on imputation, however, and since imputed values have been produced to enable calculation of prevalence rates, it makes sense to use the same values to calculate incidence rates as well.
 - 2 Imputation is a commonly used remedy for missing data, which involves filling in the missing values with allowable values for the variable in question. Many imputation methods have been devised; for an overview, see Seastrom et al. (2002) at <http://nces.ed.gov/statprog/2002/appendixb3.asp>.
 - 3 This assumes the donor pool would consist of all respondents with complete data for that screener question. Another possibility is to restrict the donor pool to those reporting some incidents at that screener, which would probably then err in the other direction.

The number of incidents was missing in 100 places for the screener questions in the main questionnaire, coming from 72 respondents. The missing values were not uniformly distributed across these 15 screener questions; roughly a third of them (35) affected Q31 (attempted break-ins), accounting for 10% of the positive responses to this screener question. Missing information was more common in the self-completion sections, with 281 missing values accounting for approximately one third of the 837 non-negative responses. Missing information was most prevalent at the sexual victimisation screener questions, where slightly more respondents failed to provide information than provided a specific positive number of incidents. Even within this section there was substantial variation between the screener questions, with missing information being twice as likely as complete positive information for the first screener question (which asked about forced sexual intercourse, the most serious crime covered in this section).

If mean imputation or random hot-deck imputation was used instead of imputing a value of 1, still assuming that all the missing responses were from victims (and restricting the mean or the donor pool to victims accordingly), this would roughly double the estimated incidence of rape. The estimated prevalence would also be somewhat higher, though not as much. The estimated incidence and prevalence of all offences would increase slightly.

Another cause of missing information for the self-completion screener questions was the complete refusal of some 6% of respondents to answer the self-completion questionnaire. No overt imputation has been conducted to correct for this, i.e. it is effectively assumed that these people experienced no offences of the types covered by the self-completion screener questions. This will have led to underestimation of the true victimisation rates for these offence types, although the bias will not have been large due to (1) the small level of self-completion non-response, and (2) its skew towards older respondents.

Date imputation

For each incident without a victim form from the main screeners, the calendar year in which the incident occurred was imputed randomly assuming that it had an equal chance of occurring on each day between 1 January 2005 and the date of the interview. That is, the year each incident occurred was imputed as being in 2005 with probability equal to 365 divided by the number of days between 1 January 2005 and the date of the interview. This was done independently across incidents and for each of the ten imputations conducted per incident. This is the same method as used in the 1996 and 2001 surveys.

For self-completion incidents, the same method was used, except when the incident with date information from that section occurred during 2005. Since that incident is the last incident in that section to have occurred, all the others are then imputed as occurring during 2005. This is the same method as used for the first self-completion section in the 2001 survey (dates were not gathered in the other sections). The assumption of even spread is not ideal even when the last incident occurred in 2006, because knowing this provides some additional information about when the other incidents are likely to have occurred, but the 2001 method has been continued for consistency.

The assumption of even spread also does not account for recall bias. An investigation of the known incident dates (described in Appendix A6) suggested that this is likely to have had a substantial

effect on the victimisation risk estimates from this survey, and even stronger effects in the previous surveys.¹ However, no easy method of correcting for this has been apparent.

Relevance imputation

Different types of offences have widely varying relevance rates (and varying proportions of missing data). These are shown in the following tables, broken down by source question (i.e. the screener question at which that incident was enumerated). Here “relevant” means having an offence code other than 85, 86 or 87.

Table 9.1 Missing forms and relevance rates by main screener question

Source question	Description	Percentage of incidents without victim forms	Relevance rate (for completed victim forms)
Q28	Vehicle Theft	39	87
Q29	Theft from Vehicle	34	90
Q30	Damage to Vehicle	37	72
Q31	Attempt to break in	42	63
Q32	Burglary	54	90
Q34	Theft from property	49	84
Q35	Theft from inside home	53	87
Q35.416 ²	Household damage	42	84
Q35.40	Theft from person	40	80
Q35.41	Other theft	47	64
Q36	Assault	46	81
Q37	Threat of assault	55	66
Q38	Other Damage	48	55
Q39	Threat of damage	79	51
Q43	Other Incidents (Main Questionnaire)	68	38
Q167.420	Assault (by current partner)		
Q167.422	Threat of assault (by current partner)	88	92
Q167.424	Damage (by current partner)		
Q167.426	Threat of damage (by current partner)		
Q228	Assault (by person well known)		
Q230	Threats (by person well known)		
Q232	Damage (by person well known)	86	90
Q234	Threat of damage (by person well known)		
Q434	Forced sexual intercourse		
Q436	Attempted forced sexual intercourse	84	³
Q438	Distressing sexual touching		
Q440	Other sexual violence, incl. threats		

1 Similar patterns of bias have been observed in the 2005 Irish International Crime Survey (van Dijk, Manchin, van Kesteren, Nevala and Hideg 200; pages 9-11).

2 Questionnaire numbering: Q416 followed Q35; hence it is referred to as Q35.416. This protocol is repeated throughout the survey documentation.

3 Incident descriptions were not collected for any sexual incidents. All the last incidents from this section were coded as valid offences, and the relevance imputation method used here will apply this procedure to the rest of these incidents. This is different from the 2001 method, where it was assumed that these incidents had similar relevance rates to other incidents from the self-completion questionnaire.

The relevance rates for the main screeners vary from 38% to 90%, so the screener question should be a useful predictor of relevance status. An imputation model was chosen by stepwise selection, starting with a model that included (for incidents from the main questionnaire) the screener question, household composition, household size, tenure/landlord, gender, age, marital status, employment status, ethnicity, urbanisation, NZSEI, and the NZDep2001 score. This reduced to a model with the screener question, age, ethnicity, household size, tenure/landlord, NZSEI, and the NZDep2001 score as predictors. Details of the model are shown in Table 9.2. According to the le Cessie-van Houwelingen normal test statistic, there was some lack of fit ($Z = -2.5$), but once the complex sample design is allowed for, this is unlikely to be statistically significant. (The associated Brier score was 0.16, while the Somer's D and gamma statistics were both 0.41.) This model was used to multiply impute relevance status for incidents from the main screener questions without victim forms.

Table 9.2 Relevance imputation for incidents from main screeners

Predictor variable	Level (relative to base level, for categorical variables)	Parameter estimate	Standard error
(Intercept)		-0.6001	0.5819
Screener	Things stolen from/off vehicle	-0.3883	0.2880
Screener	Vehicle tampering/damage	0.8152	0.2599
Screener	Unsuccessful burglary	1.3279	0.2723
Screener	Successful burglary	-0.3430	0.3189
Screener	Theft from property-outside	0.1737	0.2690
Screener	Theft from property-inside	0.0304	0.3014
Screener	Theft from a person	0.5388	0.3311
Screener	Other theft	1.3038	0.2695
Screener	Damage	1.6720	0.3278
Screener	Threatened to damage	1.8636	0.3776
Screener	Assault	0.5180	0.2945
Screener	Threatened to assault	1.2535	0.2632
Screener	Other	2.2848	0.2828
Screener	Damage to HH property	0.1623	0.2683
Age	25-39	0.3451	0.1109
Age	40-59	0.3899	0.1158
Age	60 or older	0.4574	0.1551
NZSEI		-0.0050	0.0025
NZDep01		-0.0011	0.0005
European		-0.3469	0.0935
Māori		-0.1925	0.0935
Household size		0.0962	0.0456
Tenure and landlord	Rented - private landlord	-0.1863	0.0910
Tenure and landlord	Rented - other landlord	-0.4400	0.1324
Tenure and landlord	Other	0.3002	0.2126

For self-completion incidents, the source screener question for the last incident is not collected. This was imputed randomly within each section with probability proportional to the number of incidents reported at each screener question. Then the same process was used to choose a relevance imputation model, with the same candidate variables (except of course for the screener questions). Certain screener questions (mainly sexual) had no variation in relevance status, and were omitted from the screener variable.

Offence code imputation

Offence codes were imputed using a “hot deck” imputation method (the approximate Bayesian bootstrap¹), with imputation classes defined by source screener question. This technique will reproduce the distribution of offence codes from each screener, on average. In contrast, mode imputation was used in the 1996 and 2001 surveys. The old technique would have depressed the estimated rates for offences like bicycle theft that do not have dedicated screener questions, and overstated the rates for other offences that did have their own screener question.

Duplicated incidents

A new set of questions were added to the 2006 survey early in the victim form to establish whether the current incident was actually the same as in one of the previous victim forms. If so, this meant that the respondent included the same incident in their reported counts at two different screener questions, and the rest of the victim form was skipped. However, this question only detects duplication between the three (or fewer) incidents for which victim forms were completed. By estimating the rate of duplication per potential clash, and assuming independence, unobserved duplications were imputed for other incidents. The estimated duplication rate per potential clash was 3.75%, and approximately 15% of incidents from the main screeners with no victim form were projected to be duplicates. These were deleted to help avoid over-reporting through failure to follow the “apart from...” instructions.

These new questions were not added to the self-completion questionnaire, so there is no data on duplications here. The rate from main screener incidents was applied to all self-completion incidents, and around 17% were estimated to be duplicates.

Heavy victimisation cut-off

After imputation, a cut-off was applied to improve the reliability of the estimated rates. The number of valid offences (after removing out-of-scope or duplicated incidents, and those not in 2005) from the main questionnaire was not allowed to exceed 30. Any further offences above this value were not included in the victimisation estimates. The same cut-off was applied independently to incidents from the self-completion components.²

No cut-off was applied in 2001, but the introduction of this cut-off was partly prompted by the easing in 2006 of controls on how many incidents could be reported at each screener question.

1 See Rubin (1987), p. 124.

2 Averaged across imputations, this cut-off ruled out 5.5% of incidents from the main questionnaire that would otherwise have been counted. These came from 22 respondents. The cut-off had a greater effect on self-completion incidents, ruling out 17.5% of these on average (again restricting consideration just to those incidents that would otherwise have been counted). These came from just 25 respondents, however.

Comparable imputations

Date, offence code, and relevance imputations were repeated for the 2006 data using 2001 procedures and offence codes, and a new set of results produced. These were meant solely for comparison purposes, since many of the procedures have been improved upon.

10 Variance estimation and significance tests

While sample surveys like the NZCASS provide a practical and cost-effective means of collecting information on victimisation, the survey results are inherently subject to random sampling variation. The size of this variation must be estimated and considered to interpret the results sensibly. Variance estimation for the NZCASS is complicated by the survey's complex sample design and the large amount of missing data. A balanced repeated replication method (Wolter, 1985) was used to accommodate the sample design and weighting¹, and the effect of imputation was estimated using multiple imputation.

Balanced repeated replication

Balanced repeated replication (BRR), like other resampling methods, uses the variation between the results for many sample “replicates” to estimate sampling variances (excluding imputation effects). BRR essentially uses an experimental design to determine a small “balanced” set of half-samples that contain all the information needed² to calculate a variance estimate as

$$\hat{v}_{BRR}(\hat{\theta}) = \frac{1}{28} \sum_{k=1}^{28} (\hat{\theta}_k - \hat{\theta})^2$$

where $\hat{\theta}$ is the statistic of interest, calculated using the original survey weights, and $\hat{\theta}_k$ is the same statistic calculated using the k^{th} set of replicate weights. A slight generalisation of the simple BRR formula above has been used here, using Fay's method with $\rho = 0.5$. The replicate weights have also been modified to include a finite population correction (FPC) following McCarthy's approach.

The strata from the main and booster samples have been treated as separate, giving 24 strata, and NAUs have been merged to form half-samples within strata using Lumley's (2006) survey package. The weighting process has been run³ using each of the 28 sets of BRR replicate weights, including the FPC and Fay's adjustments, as input. This accounts for the effect of the full weighting framework.

One way of summarising how much the sampling variance is affected by the sample design, weighting and any imputation is to calculate how much smaller a simple random sample with the same variance would be. This is expressed as the ratio of the actual sample size to the size of this simple random sample, and called the design effect. Design effects can vary substantially from one statistic to another, even though these come from the same survey and are based on the same weights.

Design effects were calculated for the proportion giving each response for a wide range of variables, for person, household, and incident weights. Over 500 such statistics were analysed using person and household weights. For person weights, the design effects had a lower and upper quartile of

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- 1 Standard linearisation software was considered as an alternative, but did not seem capable of reflecting the complex weighting framework used here.
 - 2 At least for linear statistics.
 - 3 However, the same non-response adjustment model was used across all replicates, i.e. model selection was not rerun for each replicate. This may lead to sampling errors being slightly underestimated.

0.83 and 2.09 respectively, and an average of 1.64. Design effects were also calculated for the same measures using person weights but restricting the sample to Māori. The design effects for Māori were slightly smaller than for the full sample, having a lower and upper quartile of 0.80 and 1.84 respectively, and an average of 1.51. For household weights, the design effects (for the full sample) were generally smaller again, having a lower and upper quartile of 0.68 and 1.51 respectively, and an average of 1.22. For incident weights, design effects were calculated for over 250 statistics. The lower and upper quartiles were 1.70 and 4.06 respectively, and the average design effect was 3.19.

Some significance tests conducted for chapters 4 and 5 of the Key Findings report used design effects of 2 and 4 (round numbers close to the upper quartiles mentioned above) for analyses of people and incidents respectively.

Multiple imputation

The effect of imputation on the reliability of victimisation rates has been accounted for using multiple imputation (Rubin, 1987). Each stochastic imputation step was repeated 10 times, using parameter values drawn from their maximum likelihood distribution¹. To produce each variance estimate, the 10 resulting imputed datasets were analysed using each of the 28 sets of replicate weights, producing 280 results. For a particular imputed dataset, say the j^{th} one, the results from all the BRR weights were combined using the BRR formula above to give the complete-data variance estimate $\hat{v}_{BRR,j}(\hat{\theta})$.

Once this was done for each imputed dataset, and the results were combined using Rubin's standard combining rules:

$$\hat{v}_{MI,BRR}(\hat{\theta}) = \left(\frac{1}{10} \sum_{j=1}^{10} \hat{v}_{BRR,j}(\hat{\theta}) \right) + \left(1 + \frac{1}{10} \right) \text{var}[\hat{v}_{BRR,j}(\hat{\theta})]$$

where $\text{var}[\hat{v}_{BRR,j}(\hat{\theta})]$ is the variance among the BRR variance estimates for the 10 imputed datasets. Confidence intervals were calculated using a t distribution with the appropriate degrees of freedom.

The variance estimates for victimisation rates assume that the imputation and analysis models are congenial (Meng, 1994). Model misspecification can cause multiple imputation to produce biased variance estimates.

The design effects for victimisation rates have been calculated for over 30 offence types. These design effects include the effect of imputation, and have an average of 2.0, and lower and upper quartiles of 1.2 and 2.2 respectively. The imputation effect alone usually increases their variances by a factor between 1.2 and 1.8 (the lower and upper quartiles). Sexual offences have particularly large imputation effects, ranging from approximately 2 to 10.

1 Except for the duplication adjustment, where a simple pq/n formula was used for the parameter variance.

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Appendices

A1 Response rate by interviewer experience

Table A1.1 Fieldwork outcomes and response rate by interviewer experience for the main sample

Main sample	Interviewer experience				Total
	1 – 3 months	4 months to 2 years	2 to 6 years	Over 6 years	
A. Dwellings approached	1,639	1,743	1,326	2,907	7,614
B. Empty/Holiday home	80	118	68	127	392
C. Ineligible	0	0	0	0	0
D. No contact	137	171	69	214	590
Eligible dwellings approached	1,560	1,625	1,258	2,780	7,222
Eligible dwellings contacted	1,423	1,454	1,189	2,567	6,632
Contact rate	91.2	89.5	94.5	92.3	91.8
Unapproachable house	19	15	14	17	65
Household refusal	153	163	155	215	685
Respondent not interviewable	33	29	38	75	174
Respondent refusal	161	169	115	306	750
Respondent unavailable	128	137	156	287	708
Interview not completed	4	3	1	7	15
E. Successful interviews	926	937	708	1,659	4,229
Conversion rate	65	64.4	59.5	64.6	63.8
Response rate	59.3	57.6	56.3	59.7	58.6

A2 Sample and population profiles

The following tables compare the NZCASS sample profile, unweighted and after each stage of the weighting process, with the corresponding population proportions across various demographic characteristics.

Table A2.1 Sample and population profiles for variables used in post-stratification and raking

	Unweighted sample	Probability weights only	After non-response adjustment	Final weights (after post-stratification / raking)	Population (2001 Census or 2006 estimates / projections)
	%	%	%	%	%
Urbanisation					
Auckland	22.4%	23.6%	26.2%	26.6%	26.6%
Other metropolitan cities	18.1%	19.2%	18.4%	21.6%	21.6%
Other main urban areas	24.2%	24.2%	24.8%	22.0%	22.0%
Secondary urban areas	7.6%	7.5%	6.9%	6.6%	6.6%
Rural/minor urban areas	27.8%	25.5%	23.7%	23.2%	23.2%
Age by gender					
Males 15-24	5.1%	7.3%	7.3%	9.5%	9.5%
Males 25-39	9.9%	9.8%	9.8%	12.5%	12.5%
Males 40-59	14.4%	14.8%	14.8%	16.8%	16.8%
Males 60-69	5.8%	5.5%	5.5%	5.1%	5.1%
Males 70+	5.5%	4.9%	4.9%	4.7%	4.7%
Females 15-24	7.8%	9.2%	9.4%	9.0%	9.0%
Females 25-39	16.8%	15.3%	15.4%	13.3%	13.3%
Females 40-59	19.8%	20.9%	20.8%	17.4%	17.4%
Females 60-69	6.6%	6.0%	5.9%	5.3%	5.3%
Females 70+	8.4%	6.2%	6.1%	6.3%	6.3%
Maori by age by gender					
Maori Males 15-39	5.6%	4.2%	4.3%	3.8%	3.8%
Maori Males 40-59	3.7%	2.4%	2.5%	1.8%	1.8%
Maori Males 60+	2.0%	1.0%	1.1%	0.6%	0.6%
Maori Females 15-39	11.6%	7.4%	7.7%	3.9%	3.9%
Maori Females 40-59	6.1%	4.0%	4.1%	2.0%	2.0%
Maori Females 60+	2.3%	1.3%	1.3%	0.6%	0.6%
Non-Maori	68.6%	79.7%	78.9%	87.3%	87.3%
Pacific					
Pacific	4.1%	5.4%	5.6%	5.7%	5.7%
Non-Pacific	95.9%	94.6%	94.4%	94.3%	94.3%
Asian					
Asian	5.1%	7.8%	8.2%	9.4%	9.4%
Non-Asian	94.9%	92.2%	91.8%	90.6%	90.6%

As expected, the weighted sample profiles for these variables match the population figures precisely.

Table A2.2 Sample and population profiles for other household characteristics

	Unweighted sample	Probability weights only	After non-response adjustment	Final weights (after post-stratification / raking)	Population (2001 Census or 2006 estimates / projections)
	%	%	%	%	%
Number of usual residents in household					
One usual resident	17.5%	22.0%	21.7%	21.5%	21.9%
Two usual residents	34.1%	34.7%	34.2%	34.1%	32.2%
Three usual residents	17.4%	16.1%	16.3%	16.4%	15.8%
Four usual residents	15.7%	14.7%	14.9%	15.0%	14.3%
Five usual residents	8.9%	7.6%	7.7%	7.8%	7.1%
Six or more usual residents	6.5%	4.9%	5.1%	5.2%	4.4%
Tenure					
Owner-occupied	64.3%	68.1%	67.4%	67.4%	67.8%
Rented	32.6%	28.9%	29.6%	29.7%	29.1%
Provided free	3.1%	3.0%	3.0%	2.9%	3.1%
Presence of motor vehicle in household					
Have motor vehicle in household	90.9%	91.2%	91.1%	91.1%	89.9%
No motor vehicle	9.1%	8.8%	8.9%	8.9%	10.1%
Broad region					
Upper North Island	51.0%	48.3%	51.6%	50.5%	50.2%
Lower North Island	25.9%	26.0%	23.8%	23.5%	24.0%
South Island	23.1%	25.7%	24.6%	26.0%	25.8%
NZ Deprivation Index					
Quintile 1 (least deprived)	15.6%	17.5%	17.5%	17.9%	20.4%
Quintile 2	16.3%	18.0%	17.8%	17.9%	19.9%
Quintile 3	19.3%	21.1%	20.7%	20.5%	20.6%
Quintile 4	21.9%	21.8%	21.8%	21.9%	21.0%
Quintile 5 (most deprived)	26.9%	21.5%	22.1%	21.7%	18.1%

In general, the sample profiles on the household variables in Table A2.2 exhibited a close match with population figures. The biggest differences are that the sample contains too few households in the least deprived areas (quintiles 1 and 2), too many in the most deprived areas (quintiles 4 and 5), and too many larger households. These differences have been reduced by the weighting process, but not eliminated.

Table A2.3 Sample and population profiles for other personal variables

	Unweighted sample	Probability weights only	After non-response adjustment	Final weights (after post-stratification / raking)	Population (2001 Census or 2006 estimates / projections)
	%	%	%	%	%
Legal and social marital status (combined)					
Legally married, or in a civil union	43.8%	50.7%	50.5%	50.8%	50.8%
Defacto relationship or living together	14.2%	13.8%	13.8%	13.1%	11.5%
Single / Never married	21.8%	22.0%	22.4%	23.7%	24.6%
Widowed	9.1%	5.7%	5.5%	5.2%	5.7%
Divorced / Separated	11.0%	7.8%	7.7%	7.2%	7.5%
Employment status					
Employed	55.7%	59.0%	58.9%	60.1%	59.8%
Student ⁽¹⁾	6.5%	9.2%	9.5%	10.6%	8.1%
Occupation					
Legislators, Administrators and Managers	12.9%	14.3%	14.4%	15.0%	13.2%
Professionals	13.4%	14.3%	14.3%	14.7%	14.7%
Technicians and Associate Professionals	12.0%	12.6%	12.8%	12.7%	11.7%
Clerks	7.7%	7.4%	7.4%	7.1%	13.3%
Service and Sales Workers	15.8%	14.0%	14.1%	13.4%	14.8%
Agriculture and Fishery Workers	9.9%	9.9%	9.4%	9.5%	8.4%
Trades Workers	10.8%	11.3%	11.4%	12.4%	8.9%
Plant and Machine Operators and Assemblers	10.4%	9.9%	9.8%	9.5%	8.8%
Elementary Occupations	6.9%	6.2%	6.2%	5.7%	6.2%
Broad region					
Upper North Island	51.0%	50.9%	54.2%	53.5%	50.8%
Lower North Island	25.9%	24.9%	22.6%	22.3%	24.3%
South Island	23.1%	24.2%	23.2%	24.2%	25.0%
Urbanisation					
Auckland	22.4%	26.4%	29.2%	30.5%	28.7%
Other metro cities	18.1%	19.3%	18.4%	19.2%	21.5%
Other main urban areas	24.2%	22.5%	23.1%	22.5%	21.3%
Secondary urban areas	7.6%	6.4%	5.9%	5.6%	6.3%
Rural/minor urban areas	27.8%	25.4%	23.4%	22.2%	22.1%
NZ Deprivation Index					
Quintile 1 (least deprived)	15.6%	18.2%	18.3%	19.5%	21.0%
Quintile 2	16.3%	17.5%	17.2%	18.3%	19.8%
Quintile 3	19.3%	20.4%	20.0%	20.6%	20.1%
Quintile 4	21.9%	21.1%	21.2%	20.9%	20.4%
Quintile 5 (most deprived)	26.9%	22.8%	23.3%	20.6%	18.6%

1. The "Student" figure taken from the Census is the number of those who studied 20 hours or more a week.

The largest differences on the personal variables shown in Table A2.3 are that there are too few clerks and too many trades workers in the sample. This could indicate some difference in occupation coding procedures, although both the NZ Census and NZCASS used the same standard.

Other differences of note include an excess of people in the most deprived areas and the corresponding shortfall in the least deprived areas, too many people in Auckland and the northern North Island, too many de facto couples, too few singles, and too many students. The last three at least might be explained by definitional differences (who counts as a “student” is not clearly defined in the NZCASS question) or by missing data (nearly 10% of people did not state their legal or social marital status in the Census). Deprivation shows the same skews for people as were evident for households in Table A2.2, while the regional skews for people have occurred despite there being little difference in the household profiles.

A3 ACNielsen area sampling frame

In nearly all cases, house-to-house interviewing by ACNielsen has required national coverage. This includes many large-scale descriptive surveys undertaken for a range of government and quasi-government agencies, SOEs and universities on a range of topics with social and economic policy implications, as well as the industry-commissioned media use surveys (newspaper and magazine readership, television viewing, and radio listening) which ACNielsen has conducted for many years.

From all of these types of organisations, there has been the requirement for statistically reliable data. This led ACNielsen to some years ago develop and standardise a reliable and replicable national sampling frame that could be linked closely with official population statistics. This was meant for two reasons:

- To project the survey data up to total population (i.e., report data in terms of thousands and tens and hundreds of thousands of persons) rather than reporting simply in percentage terms.
- To be able to break survey data down to relatively small and concise geographical areas.

ACNielsen's national sampling frame – the “Nielsen Area Unit” (“NAU”) – uses geographical units that are intermediate in size between the two smallest types of unit defined by Statistics NZ.

The smallest of Statistics NZ geographical units are “Meshblocks”. (These are used for Census data collection purposes, and are defined so that they can be amalgamated into most other “official” territorial geographical constructs.¹ They can also be defined in terms of a density of population continuum, which consists of Main Urban Areas, Secondary Urban Areas, Other Urban Areas, and Rural Areas.) Statistics NZ has split the country into approximately 38,000 Meshblocks, which contain on average around 100 residents living in about 35 dwellings. This is too small a unit for survey research purposes, because random selection procedures could mean that a particular Meshblock might be selected a number of times and be “worked out” in a relatively short time, especially where the sample frame is in continuous use for many surveys.

The next smallest Statistics NZ geographical construct is called the Area Unit. There are approximately 1,850 of these. They contain on average an amalgamation of around 20 Meshblocks, and so usually cover around 2,000 residents living in about 700 dwellings. However, 1,700 Area Units is insufficient to fulfil ACNielsen's practical requirement to be able to have a large number of areas in use at any one time.

The Nielsen Area Unit (NAU) frame combines on average around seven Meshblocks, with an average population of around 700 living in around 230 dwellings. These units have been created in such a way that they will always conform, in combination, with Statistics NZ Area Unit boundaries. They can therefore be used as building blocks for all of the territorial geographical and density of population constructs described above.

1 These include Area Units, local authority Wards, Territorial Local Authorities (Cities and Districts), and Regional Council Regions.

All NAU areas have been mapped using Statistics NZ Meshblock maps that show individual property boundaries. The first time an NAU is selected for a survey, a particular property is designated as the interviewer's "start point", the first dwelling to be called upon. This original start point is selected by using a grid pattern overlay template and a random number selection procedure that produces grid coordinates. Interviewers record all houses called upon on a sheet attached to the inside of the map folder, and the next time the map is used for a survey the start point is the house adjacent to the last house at which contact was made on the previous survey.

Multi-Stage Sampling

Multi-Stage Sampling meets the practical need to impose a staged, sequential selection procedure on the process that interviewers use in recruiting the sample. It is needed as there is no comprehensive list of all New Zealanders that can be used to select a simple random sample from.

Providing a staged, sequential and systematic guideline serves two purposes. In addition to giving interviewers a practical framework for selecting respondents, it also preserves the central tenet of probability sampling necessary to allow calculation of sampling error margins. That is, it ensures all members of the population retain a known chance of being included in the sample.

The ACNielsen procedure has three stages. It imposes random or quasi-random guidelines for:

- (a) selecting area units within stratification grid cells (strata)
- (b) selecting streets and dwellings within area units, and
- (c) selecting respondents within dwellings.

Clustered Sampling

Clustered sampling is where a number of dwellings are selected to be sampled in a patterned way around a single "start point", where a start point is defined as a numbered house on a named street, or in rural areas as a systematically-defined dwelling on a known or named road. The purpose of cluster interviewing is to reduce cost through efficiency gains in interviewers' travel time and mileage.

Within ACNielsen, clusters are generally defined in terms of the number of interviews, for example five, to be obtained around each start point. This, technically, is a quota, because it requires interviewers to keep calling on and replacing dwellings until they have obtained their quota of five interviews. This means an indeterminate (and possibly large) number of addresses are called upon, which can of course reduce overall response rate, and thereby increase non-response bias.

For the 2006 NZCASS, as with the previous crime survey, a cluster approach was employed providing interviewers with a number of dwelling addresses to be approached - a 'fixed number of addresses' approach. The cluster was completed when a "final outcome" was achieved for each address. In most cases a final outcome was an interview, a refusal, or a non-contact – after the specified number of calls back to the address has been achieved. (The number of callbacks is dealt with elsewhere.) The 'fixed number of addresses' clustering approach differs from the alternative of 'quota clusters', which centres on achieving a fixed number of interviews. The latter approach was used in the 1996 survey. It attracted some criticism in the ABS methodological review insofar as replacement households might differ in key victimisation respects from households that had refused an interview. The 'fixed number of addresses' approach means that an indeterminate number of interviews are achieved in each cluster, but it also gives the possibility of a better overall response rate. This is because it reduces the impact on the overall response rate of low-response area units, or of interviewers who are perhaps less persuasive in obtaining respondent cooperation.

A4 Effect of area unit population changes

Household figures from the 2001 Census have been used in the selection of area units for the 2006 NZCASS, but the populations of some areas would have changed significantly by the time fieldwork for the survey was conducted. As a result, the sample design would not have been completely self-weighting, while the weighting method assumes that it was. This introduces some potential for bias in the results. However, based on the analysis described below, this bias appears to be small enough to be of little concern.

Approximate estimates of the bias have been calculated as the difference between the survey result and a "corrected" figure. The "corrected" figures were calculated by adjusting the survey weights based on preliminary 2006 Census data released by Statistics New Zealand at area unit level, and recalculating the survey results using these adjusted weights. The original survey weights were multiplied by a factor reflecting the relative increase or decrease in the number of occupied dwellings in that area unit between 2001 and 2006.

The results of these calculations are reassuring. For instance, the prevalence of any victimisation reported in the Key Findings is 38.7%, and so is the "corrected" estimate, at least to 1 decimal place. More precisely, the reported prevalence was 38.7089%, while the corrected prevalence was 38.7183, giving an estimated bias of 0.009 percentage points. For comparison, the standard error for this prevalence rate was much higher at 0.834 percentage points.

Approximate bias estimates have also been calculated for the prevalence and incidence of all household offences, personal offences, confrontational offences and burglaries. The estimated bias for incidence figures is generally a little higher than for prevalence rates (because incidence figures are higher than the corresponding prevalence figure), but even the largest bias estimate of -0.019 offences per 100 households (for the incidence of household offences) is fairly small. For comparison, the standard error for this incidence figure is 2.676 offences per 100 people.

To put this in context, one rule of thumb is that biases should generally be taken seriously if they are over one quarter the size of the standard error. This bias seems to be much smaller, being roughly one hundredth the size of sampling error.

A few minor caveats are appropriate. The adjustment factor is based on changes for Statistics New Zealand's area units, not Nielsen Area Units, and the true factors at NAU level would probably be more variable since NAUs are only one third as large. The 2006 Census figures used are only preliminary. Also, the 2006 figures are available for 2006 area units, not 2001 area units. While details of which 2001 area unit each NAU falls into were available, the concordance between 2006 area units and NAUs was not. Although a concordance between 2001 and 2006 area units is available, this left a few ambiguous cases where the 2001 area units have been split. These were dealt with by combining area units to remove the ambiguity, which may also have made the adjustment factors slightly less variable than they should be. Finally, the adjustment factors were applied directly to the final post-stratified weights, but ideally they should be applied to the inverse probability weights, and later stages of the weighting process should be rerun using these adjusted weights. However, none of these issues seem serious enough to affect the general conclusion here, which is that the bias from growth suburbs appears too small to be of real concern.

The bias is small primarily because there was virtually no correlation between victimisation and area unit growth. The correlation coefficient between the number of offences experienced by a respondent and the relative increase in the number of dwellings within the area unit where they reside was only 0.0003. Also, most area units did not change drastically in size between 2001 and 2006. The median relative increase in the number of dwellings was 3.9%, the lower and upper quartiles were 0.4% and 10.5% respectively, and the 10th and 90th percentiles were -4% and 22% respectively.

A5 Derivation of eligibility probability estimate

In the Household weighting section of Chapter 8, an estimated probability of eligibility for the Māori booster sample r_i was presented for main sample households in the Māori booster sample frame where the respondent was not Māori and did not live alone, along with suggestions for its improvement. A derivation of the improved formula follows below.

The probability of eligibility for the Māori booster sample for a main sample household in the Māori booster sample frame where the respondent was not Māori and did not live alone is

$$\begin{aligned} \frac{\sum_T I_{2+} I_{NM} I_E}{\sum_T I_{2+} I_{NM}} &= \frac{\sum_T I_{2+} I_{NM} I_E I_M}{\sum_T I_{2+} I_{NM}} \\ &= \frac{\sum_T I_{2+} I_{NM} I_E I_M}{\sum_T I_{2+} I_{NM} ((1 - I_E) + I_E)} \\ &\approx \frac{\sum_T I_{2+} I_E I_M \pi_{NM}}{\sum_T I_{2+} ((1 - I_E) + I_E I_M \pi_{NM})} \end{aligned}$$

where I_{2+} indicates whether a household has 2 or more residents aged 15 or more, I_{NM} indicates whether the respondent in that household is non-Māori, I_E indicates whether a household is eligible for the booster sample (i.e. has at least one Māori resident aged 15 or more), I_M indicates whether the adults in that household are of mixed eligibility, and π_{NM} is the proportion of Māori adults among adults in mixed eligibility households in the Māori booster sample frame.

This can then be estimated as

$$r_i = \frac{uv}{w - (w - u)v}$$

where u is the proportion of people in the household aged 15 or more who would be eligible for the Māori booster sample, averaged over all households known to be of mixed eligibility, v is the proportion of households in the Māori booster sampling frame that were eligible, and w is the proportion of main sample households in the Māori booster sampling frame which have at least two residents aged 15 or more.

A6 Investigation of incident dates

Investigation of Incident Dates in New Zealand's 2001 and 2006 Crime Victim Surveys

James Reilly

24 August 2006

In both of the 2001 and 2006 crime victim surveys, a question early in the victim form asked for the approximate date of the incident. Since the estimates of victimisation rates related to the last full calendar year (called the *reference year* here), the date information was needed to establish which incidents fell into this year.

Dates were imputed for incidents without victim forms, making the assumption that incidents were spread evenly from the beginning of the reference year up to the date of the interview (as was also done in the earlier 1996 survey). An even spread would be expected if there was little change in victimisation levels within and between years.

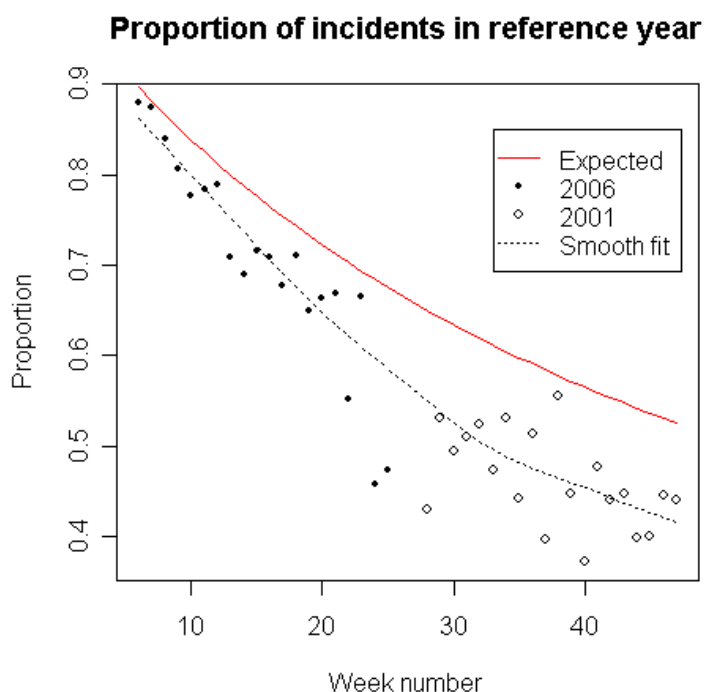


Figure 1. Proportion of victim form incidents falling in the reference year, by interview date. Results are shown for both the 2001 and 2006 surveys, along with a smooth curve fitted to this data. They fall below the expected proportion (the solid red line) throughout, with the shortfall growing larger later in the year.

However, the dates recorded in the victim forms show that the proportion of incidents reported in the reference year is lower than would be expected if they were evenly spread. An investigation has been conducted reviewing possible causes for this and the effect it may have had on victimisation

rates. The results will help inform the interpretation of 2005 victimisation rates and comparisons between 2000 and 2005.

In the 2001 survey, only 46% of victim forms recorded that the incident occurred in the reference year, substantially lower than the expected 58%. The shortfall was smaller in the 2006 survey, where 72% of victim forms recorded that the incident occurred in the reference year, compared to the expected 78%. These proportions varied during the course of fieldwork, as shown in the following graph. Depending on the cause of the shortfall, it may have reduced the estimated victimisation levels in 2000 by 20% or more.

These figures relate to the dates recorded in the main victim forms only, not the dates for incidents recorded in the self-completion questionnaire sections. Because the date was recorded only for the most recent incident in each self-completion section, this affects the distribution of the dates, making the situation significantly more complex. In contrast, the incidents for the main victim forms were selected at random within offence type, which should have produced a representative selection of dates.

Possible causes for the shortfall include:

- Seasonal crime patterns
- Increase in crime levels after the reference year
- Recency effect
- Recall problems
- Some combination of the above

Seasonal crime patterns

Seasonality would induce fluctuations around the red line over the course of a year. We observe steady divergence over a ten month period, so this hypothesis does not explain the pattern seen in the data. Seasonal fluctuations would also have to be much stronger than those seen in recorded crime over 2004-2005 (which varied from approximately 31,000 to 36,000 offences a month) to produce effects as large as those observed here.

Increase in crime levels

While an increase in crime levels could produce the observed pattern of dates, the steady divergence from the expected proportion would require an ongoing increase in crime. If this increase was linear, the magnitude of the shortfall would have required crime levels in 2001 to be roughly 25% higher on average than in 2000, peaking 50% higher late in 2001. However recorded crime levels for the two calendar years are very similar (427,230 offences in 2000 versus 426,526 in 2001).

Recency effect

Recency effects are a widely recognised source of bias in retrospective surveys. Respondents often remember events as having occurred more recently than they truly did. This effect, also known as forward telescoping, leads to overestimates of rates when questions asking about all events in a period leading up to the interview. For example, newspaper readership is often estimated based on recall of readership in the last seven days, and the recency effects are known to occur in this setting. The crime survey situation is more complex, since we focus on incidents in the reference year. Recency bias is defined here as a tendency for incidents to be recalled as too recent, affecting both the reported incident dates and the numbers reported at the screeners. Some incidents that actually occurred during the reference year would be reported in the following year (between the end of the reference year and the interview date), and some incidents from the year before the reference year would be reported in the reference year. In other words, some incidents occurring in 2005 would be reported in 2006, and some 2004 incidents would be reported as occurring in 2005. These effects will balance out to some degree, although the former effect is expected to be stronger.

Although it is unclear how large the net effect would be, a thought experiment may help. Think of two adjacent years, 10 years ago. If we asked about the proportion of incidents in each year, we would expect a net recency effect near zero, i.e. equal numbers in each year. Then think of asking about adjacent periods 7 years ago, 5 years ago, and so on. As the years move closer to the present, we would expect to get greater numbers in the more recent year if recency effect became larger as periods get closer to present. For two periods running up to the present, with the most recent varying in size, this tendency could produce a pattern generally similar to that shown in the graph above. For example, the 50% ratio we observed 7 months into the interview year could reflect a 10 month period during which incidents were attributed to 2006 (Sep 05 – June 06, i.e. an extra 4 months) and a 10 month 2005 attribution period (Nov 04 – Aug 05, i.e. an extra 2 months, less the 4 months misattributed to 2006). However it seems that the recency effect would need to be strong and change sharply to explain the extent of the variation seen in these crime surveys.

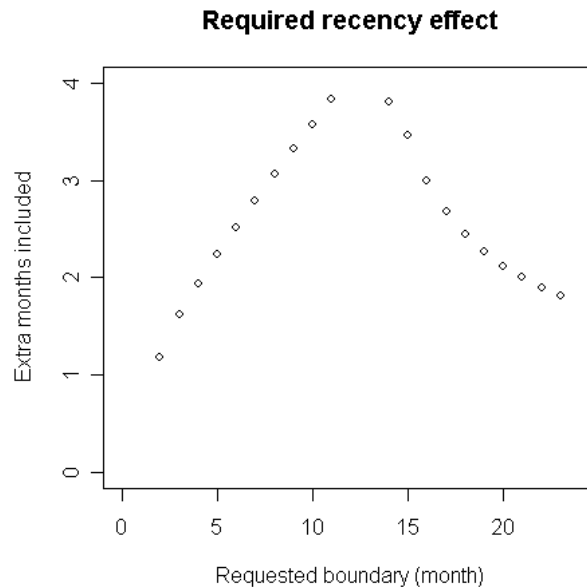


Figure 2. Model for the recency effect. This graph shows an approximate numerical model for the recency effect which would be consistent with the observed results, but requires both a strong peak effect and sharp changes in the extent of the effect. Models without the peak are possible, but require a stronger effect.

Under this interpretation, our incident counts would include some unwanted incidents (from 2004, but reported as in 2005) and exclude some we want (from 2005, but reported as in 2006). The shortfall pattern seems to require that the latter dominate, so our 2005 counts would be too low. The “even spread” imputation procedures used for the 2001 survey would not have accounted for this effect, leading to underestimated victimisation rates.

Recall bias

Another possible explanation for the shortfall is recall bias. People do not have perfect memories, so not every incident will be reported. Problems with recall generally get worse as one goes further back in time, so we would expect more incidents to go unreported from the reference year than from the more recent period leading up to the interview. This might explain the observed shortfall pattern. Recall problems would also be expected to affect less serious incidents more than highly serious incidents, since serious incidents are likely to be remembered better. In contrast, it is not obvious why recency effects or seasonality might exhibit this behaviour. Figure 3 shows that incidents reported to the police suffer from much less of a shortfall than other incidents, which supports the recall bias theory. A slightly more complex model for the proportion reported in the reference year, which also includes whether the respondent regarded the incident as a crime, also confirms this effect.

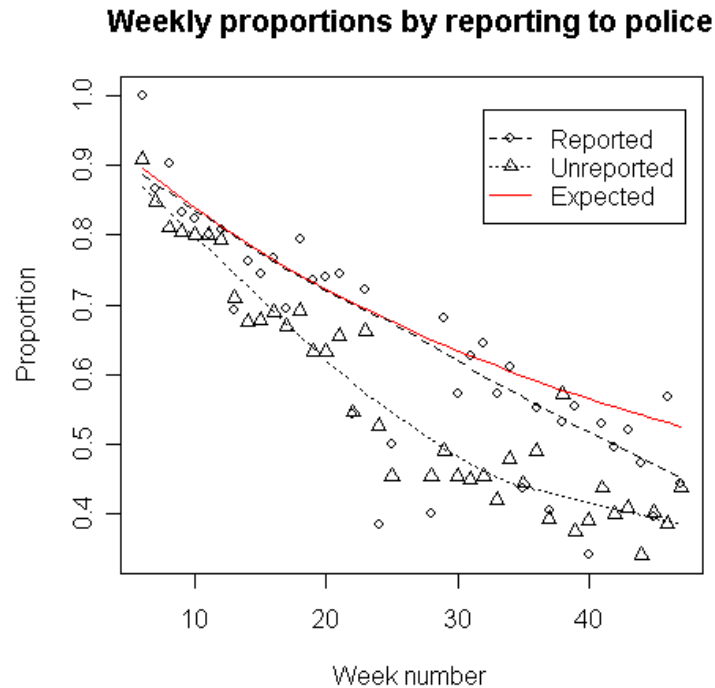


Figure 3. Proportion of incidents in the reference year, split by whether the incident was reported to the police. This graph shows that there is very little shortfall from the expected proportion for incidents that were reported to the police, at least until late in the year. For the first half of the year, virtually the entire shortfall appears to be confined to incidents that were not reported.

The shortfall only provides an indirect measure of any recall bias, since a proportion of incidents occurring after the reference year will also be omitted. This lowers the base that the proportion is calculated from by an unknown amount, although it should be quite small for interviews conducted early during the year, and so the shortfall at that point may not be far from the actual recall bias. The data indicates that the shortfall there may be quite small, perhaps only 5%. If recall bias increases steadily over time, this suggests that the level of recall bias in the base would be even smaller, and so the shortfall would be a reasonable approximation to level of recall bias in the reference year. Victimization estimates for 2000 would have been deflated by about 20-25% on average, while victimisation estimates for 2005 would only be 8-10% too low overall.

Of course, some offence types would be more affected than others. It might be possible to refine the above estimates by modeling the proportion in the reference year based on various factors (such as reporting to police, whether seen as a crime, offence type).

Self-completion incidents would have also been affected by failure of the even spread imputation assumption, and rates for the types of offences reported there would have been more severely underestimated.

Combinations of the above

While recall bias appears to be the only plausible single cause of the shortfalls, it is certainly possible that some combination of the above effects is to blame. This is much more difficult to quantify, however, and recall bias would probably remain the dominant effect. For these reasons, it is sensible to rely predominantly on the recall interpretation.

Conclusion

Recall bias seems the most likely explanation for the observed incident date patterns. Seasonality cannot explain the observed patterns. Although a recency explanation is mathematically possible, it requires a very substantial and rapidly changing effect, making this explanation seem implausible. A combination of effects is also possible, although it would probably be dominated by recall bias. In the absence of other theories, the recall interpretation should lead our thinking.

Under this interpretation, recall problems appear to have led to moderately substantial underestimation of victimisation rates for non-sexual offences committed by strangers in 2000, deflating these by over 20% on average. However, due to fieldwork occurring earlier during the year, recall problems will cause the 2005 rates to be underestimated by less than half this much, causing an increase in estimated rates of over 10%. These effects will be heavier for less serious incidents, and lighter for more serious incidents.

Future surveys would ideally be conducted as close to the reference period as possible, since a few months can make a substantial difference to the expected amount of bias. However the same fieldwork period as the preceding survey should be used if comparability is required.

Appendix to dates investigation

A statistical model predicting the proportion of incidents reported in the reference year, based on the expected proportion for the interview date and indicators of the perceived seriousness of the incidents. The model was fitted using 2006 survey data.

Variables:

in2005 = whether the incident was reported in the reference year (2005)

Q119 = whether the police knew about the incident (reverse scale)

Q137_a_crime = whether the respondent perceived the incident as a crime

lpredRefYr = the naïve expected proportion of incidents in the reference year, based on the interview date, transformed to a logit scale.

Call:

```
glm(formula = in2005 ~ Q119 + Q137_a_crime + lpredRefYr,
     family = binomial(),
     data = vfdate)
```

Deviance Residuals:

Min	1Q	Median	3Q	Max
-2.0675	-1.3601	0.7074	0.8490	1.1032

Coefficients:

	Estimate	Std. Error	z value	Pr(> z)
(Intercept)	-0.19661	0.21640	-0.909	0.3636
Q119	-0.21156	0.08425	-2.511	0.0120 *
Q137_a_crime	0.23821	0.08134	2.929	0.0034 **
lpredRefYr	1.08792	0.11601	9.378	<2e-16 ***

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

(Dispersion parameter for binomial family taken to be 1)

Null deviance: 4240.0 on 3598 degrees of freedom

Residual deviance: 4129.4 on 3595 degrees of freedom
AIC: 4137.4

Number of Fisher Scoring iterations: 4

A7 Contact sheets

The following pages show the contact sheets for the main and booster samples.

Date:

--	--

--	--

0	6
---	---

Interviewer No:

--	--	--	--	--

Laptop No:

--	--	--	--

Interviewer Name:

Area Name:

Area Unit No:

--	--	--	--

Household number:

--	--	--

Telephone No:

Calls to obtain:

Address:

Interview start time:

--	--	--	--

Interview Finish Time:

--	--	--	--

INITIAL INTRODUCTION

I'm ... from ACNielsen, the survey company. We are doing a New Zealand-wide survey on behalf of the Ministry of Justice to get an accurate measure of New Zealanders' feelings of safety and experience of crime.

You may have received a card (like this) telling you we were going to call. This is a very important survey, because its results will help Government departments plan how to deal with crime more effectively.

I have here a letter, which gives you more information about the survey. We can go through it now if you like, or you can read it later. ***Hand over introductory letter. Work through if appropriate.***

When it is clear that the initial contact is co-operative, proceed to respondent selection on the next page.

Does respondent want a summary of research findings?

yes ☐ 1 no ☐ 2

Preferred name and postal address:

address same as above ☐ 1

or E-mail address:

RESPONDENT SELECTION

It is very important that we interview a cross-section of New Zealanders. To do this, can you tell me the first name of everyone aged 15 and over who usually live here? ...and in what month is their birthday?

Probe: Is that everyone aged 15 and over who live here?

Probe to Yes

Note: Include children who board at school but return for holidays, and children in joint custody who spend equal or more nights here than elsewhere. Exclude tertiary students who live elsewhere while studying.

Usual residents of Household (aged 15+ years)			
	First name	Month of Birth (1-12)	✓ when selected
1			
2			
3			
4			
5			
6			
7			
8			
9			
10			

- The interview is with the person who has the next birthday. Is ...(NAME)... available now?

If not available now, establish when they will be available and make appointment. Give household member the prenotification card and appointment card.

*If initial doorstep respondent cannot give you all the other household members' months of birth:
write the **first names** of all household members in **alphabetical order** (eg Bruce before Charlie)
select the name beginning with the letter earliest in the alphabet (eg Bruce not Charlie).*

RESPONDENT INTRODUCTION

Repeat initial introduction if respondent and initial contact are different people.

*Please ensure that the initial contact you spoke to, hands over the **information letter** to the selected respondent before the interview takes place (or provide another copy).*

INTERVIEWER'S CONSENT FORM

Just before we start the survey, I would just like to remind you...use **Showcard "Consent"** ... the Ministry of Justice is doing this survey to find out more about New Zealanders' feelings of safety and experience of crime.

I need to check you know that:

- you don't have to take part in the New Zealand Crime and Safety Survey unless you want to
- you can stop the interview at any time
- your answers are private. Your answers will be put together with those of other people to show the results
- if you have any questions you can phone one of the 0800 numbers provided on the card
- you can get a summary of the results from the survey.

✓

Once all boxes ticked the respondent understands the purposes of the research and has agreed to be interviewed.

Respondent's first name (ask if necessary):

.....

Interviewer's signature:

.....

Here are some biscuits as a thank-you.

NEW ZEALAND CRIME AND SAFETY SURVEY 2006 CONTACT SHEET (BOOSTER SAMPLE)

Job No: C141300144

Date:

06

Interviewer No:

Laptop No:

Interviewer Name:

Area Name:

Area Unit No:

Household number:

Telephone No:

Calls to obtain:

Address:

Interview start time:

Interview Finish Time:

IRREGULARITIES

Anything wrongly entered into computer?
Note question number & comments here.

.....

.....

.....

.....

.....

.....

.....

.....

.....

.....

.....

.....

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.....

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.....

.....

.....

RESEARCH FINDINGS

Does respondent want a summary of research findings?

yes 1 no 2

Preferred name and postal address:

address same as above 1

or E-mail address:

INITIAL INTRODUCTION

I'm ... from ACNielsen, the survey company. We are doing a New Zealand-wide survey on behalf of the Ministry of Justice to get an accurate measure of New Zealanders' feelings of safety and experience of crime.

For this study we need to obtain a good spread of different types of people. Is there anyone usually living here aged 15 years or older who might consider themselves Māori? That is, if asked which ethnic group or groups they belong to, they would include Māori.

- Yes (including "possibly") → Continue.
- No Māori in h/hold → Record code 80 on call sheet. Terminate.
- Household refuses before ascertaining eligibility → code 67 on call sheet.

This is a very important survey, because its results will help Government departments plan how to deal with crime more effectively. I have here a letter, which gives you more information about the survey. We can go through it now if you like, or you can read it later. **Hand over introductory letter. Work through if appropriate.**

When it is clear that the initial contact is co-operative, proceed to respondent selection on the next page.

RESPONDENT SELECTION

It is very important that we interview a representative selection of Māori New Zealanders. To do this, can you tell me the first name of all Māori aged 15 and over who usually live here?
...and in what month is their birthday?

If needed: By "Māori" we mean that if asked which ethnic group or groups they belong to, they would include Māori.

Probe: Is that all Māori aged 15 and over who live here?

Probe to Yes

Note: Include children who board at school but return for holidays, and children in joint custody who spend equal or more nights here than elsewhere. Exclude tertiary students who live elsewhere while studying.

Usual residents of Household (Māori, aged 15+ years)			
	First name	Month of Birth (1-12)	✓ when selected
1			
2			
3			
4			
5			
6			
7			
8			
9			
10			

The interview is with the person who has the next birthday. Is ...(NAME)... available now?

If not available now, establish when they will be available and make appointment. Give household member the prenotification card and appointment card.

*If initial doorstep respondent cannot give you all the other household members' months of birth: write the **first names** of all household members (Māori, 15+) in **alphabetical order** (eg Bruce before Charlie) select the name beginning with the letter earliest in the alphabet (eg Bruce not Charlie).*

RESPONDENT INTRODUCTION

*Repeat initial introduction if respondent and initial contact are different people.
Please ensure that the initial contact you spoke to, hands over the **information letter** to the selected respondent before the interview takes place (or provide another copy).*

For this study we need to obtain a good spread of different types of people. Can I just check, if asked which ethnic group or groups you belong to, would you include Māori?

- Yes → Continue
- No → Ask to speak to the next person on the list.
- If respondent refuses before eligibility can be confirmed → code 75 on call sheet
- If no Māori in household → Terminate. Record as respondent not eligible Code 81 on call sheet.

INTERVIEWER'S CONSENT FORM

Just before we start the survey, I would just like to remind you... **use Showcard "Consent"** ... the Ministry of Justice is doing this survey to find out more about New Zealanders' feelings of safety and experience of crime.

I need to check you know that:

- you don't have to take part in the New Zealand Crime and Safety Survey unless you want to
- you can stop the interview at any time
- your answers are private. Your answers will be put together with those of other people to show the results
- if you have any questions you can phone one of the 0800 numbers provided on the card
- you can get a summary of the results from the survey.

✓

Once all boxes ticked the respondent understands the purposes of the research and has agreed to be interviewed.

Respondent's first name (ask if necessary):

.....

Interviewer's signature:

.....

Here are some biscuits as a thank-you.

A8 Showcards

Showcard: Consent

You don't have to take part in the New Zealand Crime and Safety Survey unless you want to

You can stop the interview at any time

Your answers are private. Your answers will be put together with those of other people to show the results

If you have any questions you can phone one of the 0800 numbers provided on the card

You can get a summary of the results from the survey

Showcard **A**

A very big problem	1
A fairly big problem.....	2
Not a very big problem	3
Not a problem at all	4

Showcard **B**

Very safe	1
Fairly safe	2
A bit unsafe	3
Very unsafe	4

Showcard **C**

Very worried	1
Fairly worried	2
Not very worried	3
Not at all worried	4
Not applicable.....	5

Showcard **D**

Q15, Q16, Q17, Q18

- Been at the movies, a restaurant, bar or other entertainment
- At night
- In your local town or city centre

Showcard E

Q21

Have kept name and telephone number of
our street contact person 1

Have put crime prevention stickers / posters
in window or on letterbox 2

Have received newsletters or material from
Neighbourhood Support or Rural Support in
last 12 months 3

Been to a Neighbourhood Support or Rural
Support meeting / street event in the last 12
months..... 4

Reported something suspicious in the area to
the police in the last 12 months..... 5

Showcard **F**

Excellent.....	1
Good.....	2
Fair	3
Poor.....	4
Very poor.....	5

Showcard **G**

- Must have happened since 1st January 2005
- Must have happened to you personally,
or to your household property
(Note: 'household' means the people living
with you)
- Must have happened in New Zealand
- Can be both serious things and small things too

Showcard H

Property was stolen, or taken without permission, even if you later got it back	1
(Apart from anything actually stolen) There was an attempt made to steal anything (else).....	2
Property was damaged. That is buildings, vehicles, and / or other property. This includes damage in the course of theft or attempted theft	3
The person or any of the people who did it, actually used force / violence on anyone in anyway, even if this resulted in no injury.....	5
The offender threatened to harm something or someone	6
Any sexual element.....	7

Showcard I

Pushed in past person who opened door (or tried to).....	1
Door was not locked.....	2
Door was open	3
They had key	4
Forced lock / broke lock (or tried to).....	5
Broke / cut out / removed panel / window of door or panel / window beside door (or tried to).....	6
By false pretences (pretending to be someone he / she wasn't)	7
Other	8

Showcard J

Window was open / could be pushed open....	1
Forced window lock / catch (or tried to)	2
Broke / cut out glass (or tried to)	3
Other	4

Showcard K

Vehicles etc

Truck / car / van	1
Motorcycle	2
Vehicle parts / accessories	3
Bicycle	4

Household items etc

Furniture / linen / other household goods.....	5
Kitchen equipment / silverware	6
Food / alcohol	7
Personal effects / jewellery	8
Tools	9
Plants / garden tools & equipment	10
Sporting equipment	11

Cash and cards etc

Purse / wallet	12
Cash / cheque book / credit cards / other documents (e.g. Savings account book, passport).....	13

Electronic equipment etc

TV, VCR, DVD, stereo equipment, iPod, Walkman.....	14
CDs, tapes, video games	15
Mobile phone	16
Computer, computer equipment	17

Other	18
--------------	----

Showcard L

Damage done as part of the offender getting in / trying to get in your home	1
Damage done as part of the offender stealing / trying to steal things (for example, from locked cupboards or rooms)	2
Other damage	3

Showcard **M**

Anger / annoyance	1
Shock	2
Fear	3
Depression	4
Anxiety / panic attacks	5
Difficulty sleeping	6
Crying / tears	7
Loss of confidence / feeling vulnerable	8
Shame / guilt	9
Increased use of alcohol or drugs or medication	10
More cautious / aware	11
Other - please state	96
No, no emotional reaction	95

Showcard **N**

Very satisfied	1
Satisfied	2
Neither satisfied nor dissatisfied	3
Dissatisfied	4
Very dissatisfied	5

Showcard **O**

Very helpful	1
Fairly helpful	2
Not very helpful	3
Not at all helpful	4

Showcard **P**

A crime	1
Wrong, but not a crime	2
Just something that happens	3

Showcard Q

The computer has been affected by a virus, worm or spyware 1

The computer has been hacked into without your permission 2

You bought something over the Internet or by email where you believe you were a victim of fraud 3

On a web page, you unintentionally encountered material that you found highly offensive 4

You received email messages that said things that you found harassing or threatening 5

Showcard **R**

The mobile phone has been used by someone else for a purpose that offended you 1

You have received a phone call or text message that said things that you found highly offensive 2

You have received a picture that you found highly offensive 3

You have received a phone call or text message that you found harassing or threatening 4

Showcard **S**

One person living alone	1
Solo parent with child / children	2
Couple without children / children not living at home	3
Couple with child / children	4
Extended family / whānau	5
Family, other combination	6
Flatmates	7
Other	96

Showcard **T**

15-19 years	1
20-24 years	2
25-29 years	3
30-34 years	4
35-39 years	5
40-44 years	6
45-49 years	7
50-54 years	8
55-59 years	9
60-64 years	10
65-69 years	11
70-74 years	12
75 years and over	13

Showcard **U**

New Zealand European	1
Māori	2
Samoan	3
Cook Island Maori	4
Tongan	5
Niuean	6
Chinese	7
Indian	8
Other (such as Dutch, Japanese, Tokelauan) please state	96

Showcard **V**

Private person, trust or business.....	1
Local Authority / City Council	2
Housing New Zealand Corporation	3
Other	96

Showcard **W**

Currently in paid employment or self-employed	1
Retired	2
Home duties	3
Unemployed, receiving unemployment benefit	4
Unemployed, not receiving unemployment benefit	5
Receiving sickness benefit	6
Receiving invalid's benefit	7
Receiving other benefit	8
Student	9
Other	96

Showcard **X**

Managing quite well,
able to save or spend on leisure 1

Just getting by,
unable to save if wanted to 2

Getting into difficulties 3

Showcard Y

Legally married (If legally married but separated, select Divorce / Separated)	1
Civil union	2
Defacto relationship or living together	3
Single / Never married	4
Widowed	5
Divorced / Separated	6

A9 Selected CAPI screenshots

This selection of screenshots aims to demonstrate the “look and feel” of the CAPI user interface for different question types, especially for the self-completion section (where such layout might have the greatest impact on responses).

Administration Section



Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]

Tools Help

Cancel Postpone Previous Next Forward Language Messaging Synchronize Now Log out

Sample type:

☒ Main sample
☐ Booster sample

Laptop Number:

Please select your answer

Supervisor Area:

Select region:

☒ Auckland/Northland
☐ Waikato/BOP/EastCoast
☐ Wellington/HB/NP/PN
☐ South Island

Select Quarter:

☒ Quarter 3

<< >>

© 2006 ACNielsen |

Ready Console name: AC Nielsen 34 User name: martin You are online

Start 1 save - Micros... EasiQuest NZCSS 26 Jan ... Screendumps Vi... Screendumps B... Interviewer C... 10:25 a.m.

“Main” questionnaire (i.e. for all respondents, before screeners)

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

File Edit View Favorites Tools Help

Back Forward Stop Home Search Favorites Media Print Mail News RSS Feeds

Address http://survey.confirmat.com/wix/p313091703.aspx Go Links

MAIN QUESTIONNAIRE

Q1.
CODE SEX OF RESPONDENT

☐ Male
☐ Female

Q2. How long have you lived at this address?

☐ Less than 1 year
☐ 1 to < 2 years
☐ 2 to < 5 years
☐ 5 years to < 10 years
☐ 10 years or more

<< >>

© 2006 ACNielsen |

Done Internet

Start Screendumps Briefing Qu... New Zealand Crime an... 1:08 p.m.

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

Address: <http://survey.confirmat.com/wix/p313091703.aspx>

Q6 What sort of crime problems do you think there are in this neighbourhood?

PROBE What others? PROBE TO NO / NOTHING
IF NEEDED, READ: 'This neighbourhood' means the streets around you. (RURAL) 'This neighbourhood' means your 'district'

DO NOT READ. CODE ALL THAT APPLY.

<input type="checkbox"/> Burglary / break-ins	<input type="checkbox"/> Theft of cars
<input type="checkbox"/> Vandalism / graffiti	<input type="checkbox"/> Theft from and damage to cars
<input type="checkbox"/> Petty thefts	<input type="checkbox"/> Dangerous driving / speeding / hoons in cars
<input type="checkbox"/> Assault	<input type="checkbox"/> Drink driving
<input type="checkbox"/> Youths on the street / youths fighting	<input type="checkbox"/> Drinking / drunken behaviour / under-age drinking
<input type="checkbox"/> Street attacks	<input type="checkbox"/> Selling drugs / growing or manufacturing drugs
<input type="checkbox"/> Domestic violence	<input type="checkbox"/> Drug use
<input type="checkbox"/> Sexual crimes	<input type="checkbox"/> Other (specify) <input type="text"/>
<input type="checkbox"/> Prowlers	<input type="checkbox"/> Don't know

<< >>

© 2006 ACNielsen |

Done Internet
 Start [Icons] Screendumps Briefing Qu... New Zealand Crime an... 1:22 p.m.

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

Address: <http://survey.confirmat.com/wix/p313091703.aspx>

USE SHOWCARD A

Q8. Using the categories on Showcard A, can you tell me how much of a problem you think the following things are in your neighbourhood?

PROBE IF NEEDED, READ: 'This neighbourhood' means the streets around you. (RURAL) 'This neighbourhood' means your 'district'

READ OUT

	1 A very big problem	2 A fairly big problem	3 Not a very big problem	4 Not a problem at all	[NOT ON SHOWCARD] Don't know
Drunks, glue sniffers or people high on drugs on the streets	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Rubbish and litter lying about on streets or empty sections	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Broken windows, graffiti, or other deliberate damage to property	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Speeding cars	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Teenagers hanging around on the streets	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

<< >>

© 2006 ACNielsen |

Done Internet
 Start [Icons] Screendumps Briefing Qu... New Zealand Crime an... 1:24 p.m.

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

Address: <http://survey.confirmat.com/wix/p313091703.aspx>

FEAR OF AND WORRY ABOUT CRIME

Q9. Now some questions about how worried you are about crime. First, how much is **your own** quality of life affected by **fear of crime**, on a scale from 0 to 10, where 0 is no effect and 10 is a total effect on your quality of life?

☐ 0
☐ 1
☐ 2
☐ 3
☐ 4
☐ 5
☐ 6
☐ 7
☐ 8
☐ 9
☐ 10

Q10. Do you ever walk alone in your neighbourhood after dark?

WALKING THE DOG IS CLASSIFIED AS WALKING ALONE.
PROBE IF NEEDED, READ: 'This neighbourhood' means the streets around you and includes parks. (RURAL) 'This neighbourhood' means your 'district'

☐ Yes
☐ No

Done

Start | Screendumps Briefing Qu... | New Zealand Crime an...

Internet 1:25 p.m.

Screeners Questions before Victim Form

Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]

Tools Help

Cancel Postpone Previous Next Forward Language

Messaging Synchronize Now Log out

VICTIMISATION SCREENER QUESTIONS

USE SHOWCARD G. LEAVE SHOWCARD IN VIEW FOR SCREENERS I.E. TILL Q44

I'd now like to ask you about some things that might have happened to you or your household since 1st January 2005, that is, since the beginning of last year, in which you may have been the victim of a crime or offence. This doesn't mean that crimes that may have happened before this time are unimportant, but we want to get the most recent information on people's experience of crime.

Showcard G lists the sorts of incidents we are interested in. These...

- Must have happened since **1st January 2005**
- Must have **happened to you personally**, or to **your household property**
- Must have happened in **New Zealand**
- Can be both **serious** things and **small** things too.

It is often difficult to remember exactly when things happen, so take the time you need.

Q27. First, has anyone **now** in this household owned or had the regular use of a car, motorcycle, van or truck at any time since 1st January 2005?

CODE YES IF ANYONE HAS OWNED OR HAD REGULAR USE OF A CAR, MOTORCYCLE, VAN OR TRUCK SINCE THE 1ST JANUARY 2005, BUT NO LONGER DOES

☐ Yes
☐ No

Ready Console name: AC Nielsen 122 User name: martin Last online time: 2/1/2006 3:10 PM

Start | Interviewer Console - ... | Screendumps Victim Form ...

9:20 AM

Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]

Tools Help

Cancel Postpone Previous Next Forward Language Messaging Synchronize Now Log out

Q28. Since 1st January 2005 have you or anyone else now in your household had their car, motorcycle, van or truck stolen or taken away without permission? How many times?
 PROBE IF NEEDED, READ: This excludes legal towaways
 NONE = 0, DON'T KNOW / CAN'T REMEMBER = 98

<< >>

© 2006 ACNielsen

Ready Console name: AC Nielsen 122 User name: martin Last online time: 2/1/2006 3:10 PM

Start Interview Console - ... ScreenDumps Victim Form ... 9:21 AM

Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]

Tools Help

Cancel Postpone Previous Next Forward Language Messaging Synchronize Now Log out

Q33. Since the beginning of 2005, has anyone stolen anything from outside your home or holiday home, such as from your front gate or garden or shed. First, how many times was anything worth less than \$10 stolen? For example, milk cartons or newspapers.
 NONE = 0, DON'T KNOW / CAN'T REMEMBER = 98
 AFTER READING THIS QUESTION, FLOW ONTO THE NEXT ONE WITHOUT A PAUSE I.E. TREAT THIS AND THE NEXT QUESTION AS A SINGLE QUESTION

Q34. And now, how many times was anything worth more than \$10 stolen?
 NONE = 0, DON'T KNOW / CAN'T REMEMBER = 98
 PROBE IF NEEDED: This is still about anything stolen from outside your home or holiday home, such as from your front gate or garden or shed, since the beginning of 2005. Please count all incidents where things more than \$10 were stolen, even if this was at the same time as when something less than \$10 was stolen.

Q35. And since 1st January 2005 has any of your household property been stolen from inside your home or garage by someone who was allowed to be there? For example, a workman doing a job, or a visitor or a boarder or someone living at home. This includes a holiday home if you have one. How many times?
 NONE = 0, DON'T KNOW / CAN'T REMEMBER = 98

<< >>

© 2006 ACNielsen

Ready Console name: AC Nielsen 122 User name: martin Last online time: 2/1/2006 3:10 PM

Start Interview Console - ... ScreenDumps Victim Form ... 9:24 AM

Victim Form

Alternate question if only one victim form, version 1a (if based on any screeners Q28-Q41, Q416 i.e. if=1)

The screenshot displays the 'Interviewer Console' window for the 'New Zealand Crime and Safety Survey 2006'. The window has a title bar with the text 'Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]'. Below the title bar is a menu bar with 'Tools' and 'Help'. A toolbar contains buttons for 'Cancel', 'Postpone', 'Previous', 'Next', 'Forward', 'Language' (with a dropdown menu showing 'ENG'), 'Messaging', 'Synchronize Now', and 'Log out'. The main content area has a blue header bar. Below it is a grey bar with the text 'VICTIM FORM'. The main text area contains the following content: 'Q44. Now, I'd like to get a few more details from you about the theft of a vehicle.', 'Can I just confirm that this incident happened in New Zealand [and](#) after 1st January 2005?', and two radio button options: 'Yes' and 'No'. Below the options are two navigation buttons: '<<' and '>>'. At the bottom left of the main area is the copyright notice '© 2006 ACNielsen'. The bottom status bar shows 'Console name: AC Nielsen 122', 'User name: martin', and 'Last online time: 2/1/2006 3:10 PM'. The Windows taskbar at the very bottom shows the 'Start' button, several open applications including 'ScreenDumps Victim Form ...' and 'Interviewer Console - ...', and the system clock showing '10:19 AM'.

Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]

Tools Help

Cancel Postpone Previous Next Forward Language ENG Messaging Synchronize Now Log out

VICTIM FORM

Q44. Now, I'd like to get a few more details from you about the theft of a vehicle.

Can I just confirm that this incident happened in New Zealand [and](#) after 1st January 2005?

☐ Yes

☐ No

<< >>

© 2006 ACNielsen

Console name: AC Nielsen 122 User name: martin Last online time: 2/1/2006 3:10 PM

Start ScreenDumps Victim Form ... Interviewer Console - ... 10:19 AM

Alternate question text for four or more offences, version 3a

The screenshot shows a web application titled "Interviewer Console - [p318919618 - New Zealand Crime and Safety Survey 2006]". The interface includes a toolbar with buttons for "Cancel", "Postpone", "Previous", "Next", "Forward", and "Language". There are also icons for "Messaging", "Synchronize Now", and "Log out". The main content area is titled "VICTIM FORM" and contains the following text:

Q44. Now I'd like to get a few more details about three of the incidents. The computer selected them randomly, to make sure we get a wide spread of information about the things that happen to people. To start, please think about the 2nd unlawful entry into your home / garage.

Can I just confirm that this incident happened in New Zealand and after 1st January 2005?

☒ Yes
☐ No

Navigation buttons "<<" and ">>" are located below the question. The footer shows "© 2006 ACNielsen" and a small icon. The status bar at the bottom indicates "Ready", "Console name: AC Nielsen 122", "User name: martin", and "Last online time: 2/1/2006 3:10 PM".

Demographics

The screenshot shows a web application titled "New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer". The address bar shows "http://survey.confirmat.com/wix/p313091703.aspx". The main content area is titled "USE SHOWCARD U" and contains the following text:

Q151. Please use this card to tell me which ethnic group or groups you belong to.
CODE ALL THAT APPLY

☐ [1] New Zealand European
☒ [2] Māori
☐ [3] Samoan
☐ [4] Cook Island Maori
☐ [5] Tongan
☐ [6] Niuean
☐ [7] Chinese
☐ [8] Indian
☐ [96] Other (such as Dutch, Japanese, Tokelauan) Please state
☐ [NOT ON SHOWCARD] Refused

Navigation buttons "<<" and ">>" are located below the question. The footer shows "© 2006 ACNielsen" and a small icon. The status bar at the bottom indicates "Done", "Internet", and "1:51 p.m.".

Self-Completion

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

File Edit View Favorites Tools Help

Back Forward Stop Search Favorites Media Print

Address <http://survey.confirmat.com/wix/p313091703.aspx> Go Links

SELF COMPLETION SECTIONS

The next section is concerned with people's experiences as victims of some other kinds of crime, which we have not discussed yet. Some of the questions asked are very personal. Because of this we are asking you to continue the survey by entering your answers directly into the computer.

This is specially designed to be easy for anyone to complete, even if they don't use a computer.

- Please be as honest as possible. It is important that we have a complete picture of what happens to people.
- Your answers are totally confidential and will not be seen by me unless you ask me to help you.
- Your answers will be put together with those of other people to show the results.
- Even if you have not experienced any incidents of the type you will be asked about, we still need to know that. In this case, you will only need to answer a few questions.

We have some practice questions to start with, and I can help you get started if you want. I'll now pass the computer to you.

IF RESPONDENT SAYS NO INCIDENTS IT IS ACCEPTABLE FOR YOU TO GO THROUGH THE QUESTIONS WITH THEM.
PLEASE NOTE: MALES SHOULD ALSO BE FILLING IN THIS PART OF THE QUESTIONNAIRE

☒ Happy to continue
☐ Hesitant
☐ Refused totally

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Done Internet

Start | ScreenDumps Briefing Qu... | New Zealand Crime an... | 2:01 p.m.

Question numbering: Q411 below followed Q163; hence it is referred to as Q163.411 elsewhere in the survey documentation.

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

Address: <http://survey.confirmat.com/wix/p313091703.aspx>

PRACTICE QUESTIONS

Q411. Do you drink tea?

IF YOU ARE UNSURE HOW TO FILL IN ANY OF YOUR ANSWERS, FEEL FREE TO ASK THE INTERVIEWER TO HELP YOU.

☐ Yes

☐ No

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Address: <http://survey.confirmat.com/wix/p313091703.aspx>

SECTION 1: VIOLENCE BY YOUR PARTNER

Q417. Most of the following questions are about things that might have happened to you since 1st January 2005, but some questions relate to things that may have happened earlier.

- We only want to know about things which have happened **to you personally**.
- We don't just want to know about serious things - we want to know about small things too.

If you are unsure how to fill your answers in, feel free to ask the interviewer to help you.

In this section, we are interested in learning about New Zealanders' experience of violence by people who were **partners at the time it happened**. International research suggests that this is much more common than violence by strangers. We need to know if this is so in New Zealand too.

First, can you please say whether you are currently in a marital or similar partnership.

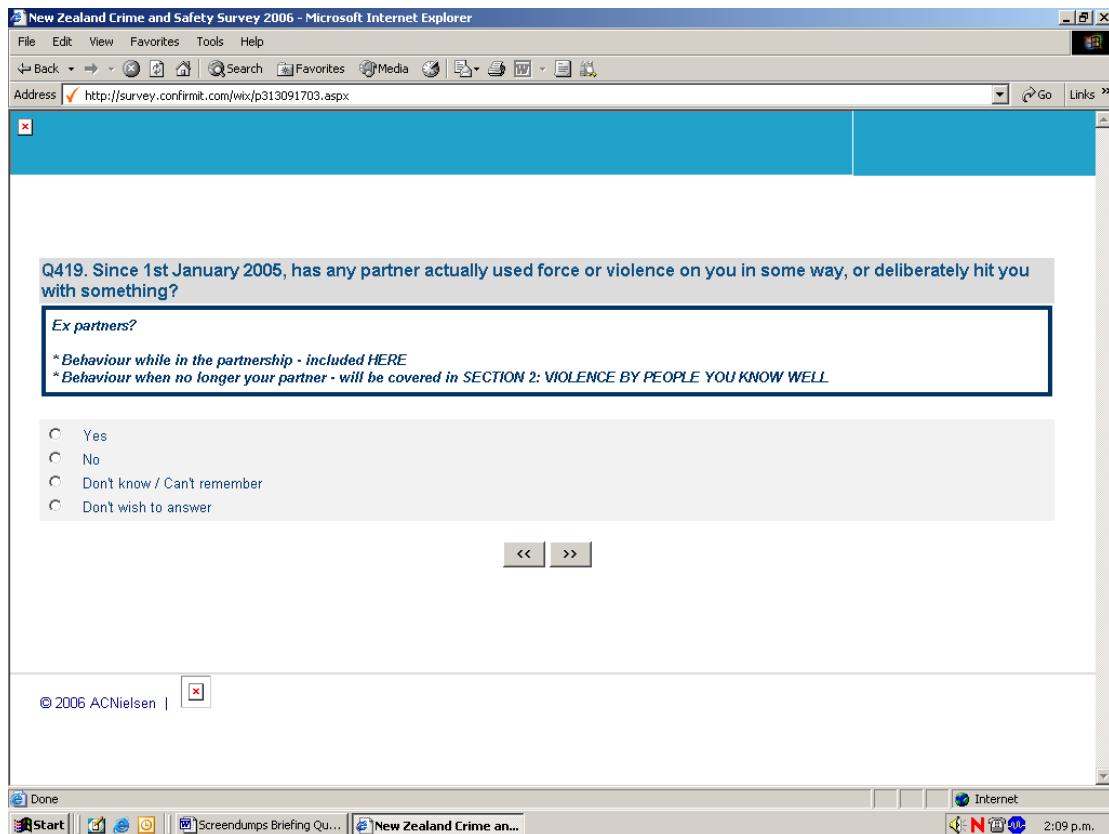
☐ Yes, have a spouse or partner of the opposite sex

☐ Yes, have a partner of the same sex

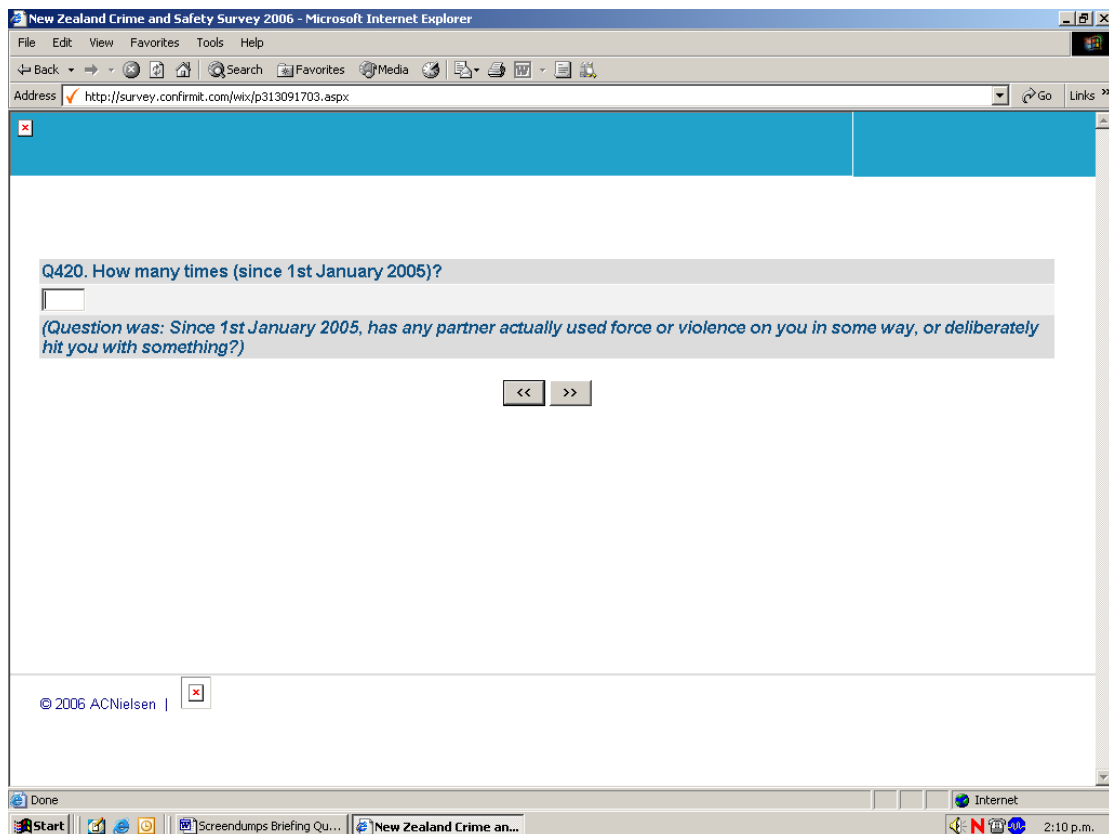
☐ No, do not currently have a spouse or partner

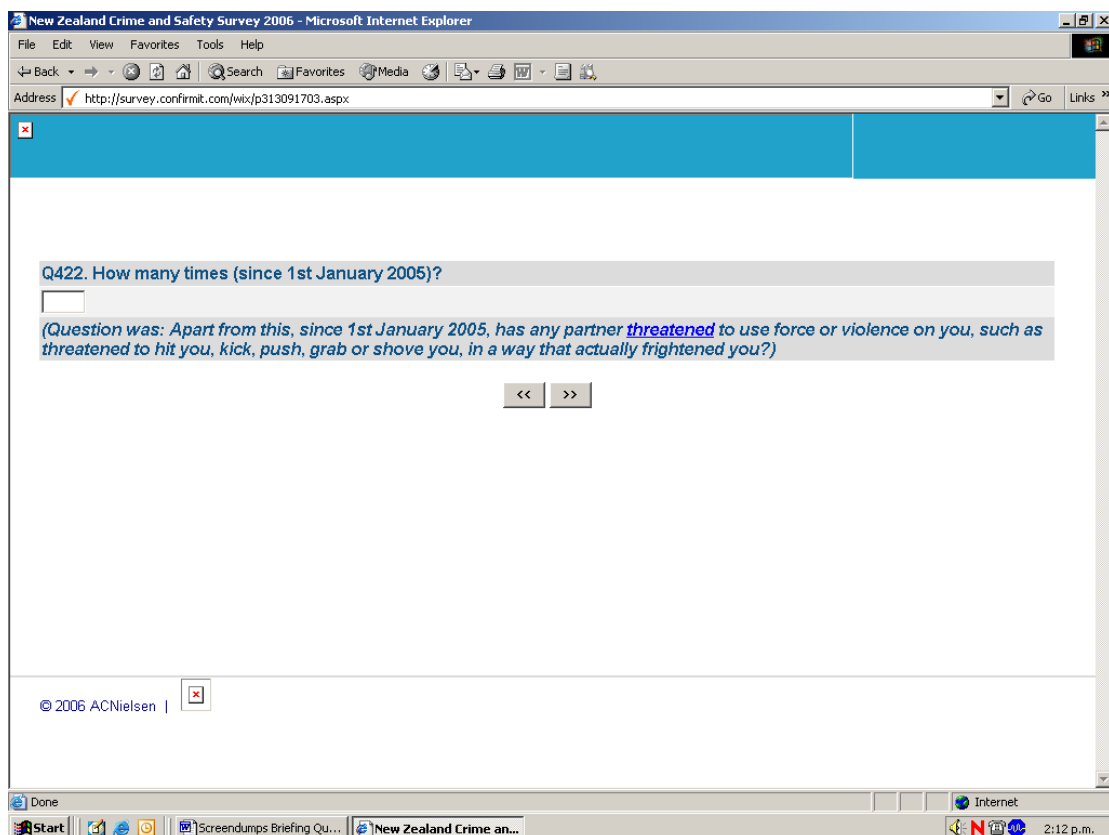
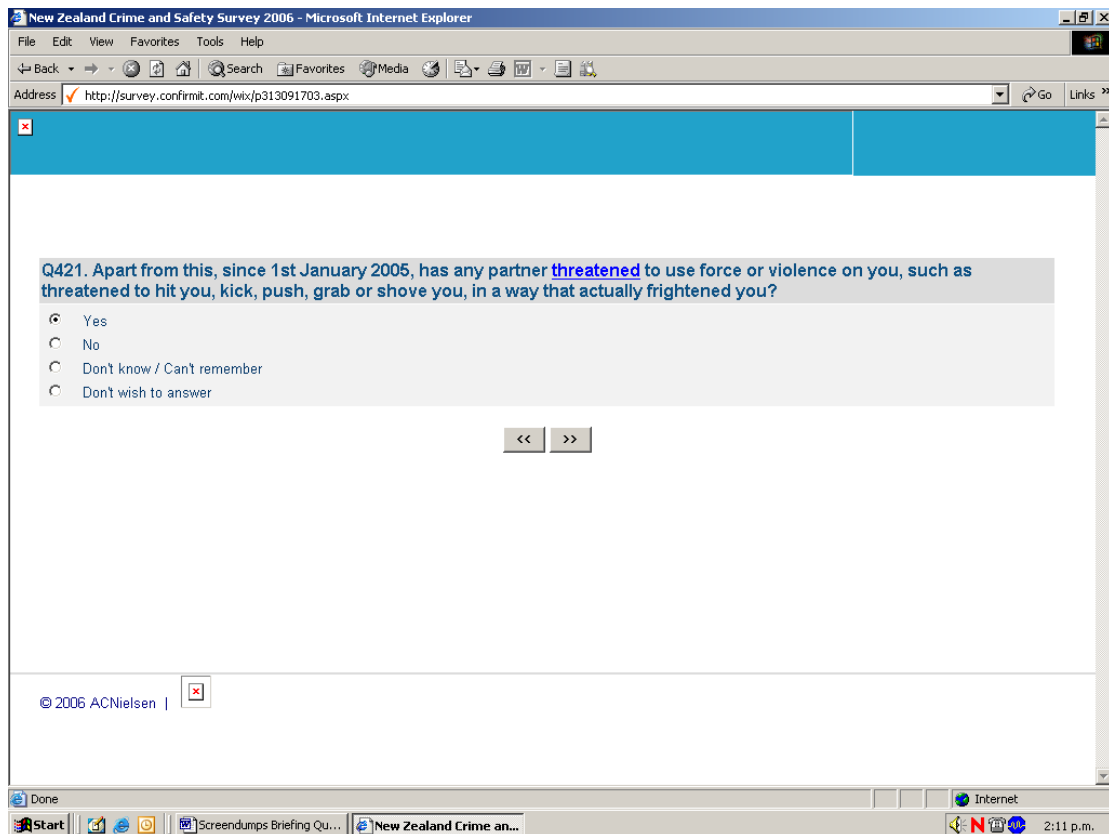
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Note: In the screen below, the text in parentheses following "Question was" is important because this is a separate screen to the closely-related preceding question. The 2001 survey used a similar approach.





New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

Address: <http://survey.confirmat.com/wix/p313091703.aspx>

Q217. Who did you talk to?
PLEASE SELECT ALL THAT APPLY.

<input type="checkbox"/> Immediate family member(s) or ex-partner	<input type="checkbox"/> Citizens Advice Bureau
<input type="checkbox"/> Other relative(s)	<input type="checkbox"/> Church / Church group / Salvation Army
<input type="checkbox"/> Friend(s) / neighbour(s)	<input type="checkbox"/> Neighbourhood Support (Neighbourhood Watch) / Rural Support
<input type="checkbox"/> Work colleague / employer / fellow student	<input type="checkbox"/> Iwi or other Māori organisation
<input type="checkbox"/> Doctor, nurse, psychologist, psychiatrist, counsellor	<input type="checkbox"/> Pacific organisation
<input type="checkbox"/> Victim Support	<input type="checkbox"/> Someone else (not the police)
<input type="checkbox"/> Rape Crisis / HELP (Sexual Abuse Centre) / Women's Refuge	<input type="checkbox"/> Don't know / Can't remember

<< >>

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Address: <http://survey.confirmat.com/wix/p313091703.aspx>

Q218. Did you approach them first, or did they contact you?

	Approached them first		Contacted by them	
Victim Support	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Rape Crisis / HELP (Sexual Abuse Centre) / Women's Refuge	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Citizens Advice Bureau	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Church / Church group / Salvation Army	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Neighbourhood Support (Neighbourhood Watch) / Rural Support	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Iwi or other Māori organisation	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Pacific organisation	<input checked="" type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

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Variant question (if respondent approached organisation)


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Address: <http://survey.confirmat.com/wix/p313091703.aspx>

Q219. How helpful was...?

	Very helpful	Fairly helpful	Not very helpful	Not at all helpful
Victim Support	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Rape Crisis / HELP (Sexual Abuse Centre) / Women's Refuge	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Citizens Advice Bureau	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Church / Church group / Salvation Army	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Neighbourhood Support (Neighbourhood Watch) / Rural Support	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Iwi or other Māori organisation	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Pacific organisation	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

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Start Screendumps Briefing Qu... New Zealand Crime an... 2:40 p.m.

Variant question (if organisation contacted respondent)


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Address: <http://survey.confirmat.com/wix/p313091703.aspx>

Q220. How helpful was...?

	Very helpful	Fairly helpful	Not very helpful	Not at all helpful	Didn't accept / want their help
Victim Support	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Rape Crisis / HELP (Sexual Abuse Centre) / Women's Refuge	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Citizens Advice Bureau	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Church / Church group / Salvation Army	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Neighbourhood Support (Neighbourhood Watch) / Rural Support	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Iwi or other Māori organisation	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Pacific organisation	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

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Done Internet
Start Screendumps Briefing Qu... New Zealand Crime an... 2:41 p.m.

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Address http://survey.confirmat.com/wlx/p313091703.aspx Go Links

Q223. Which of the following best describes what happened?

☐ A crime
☐ Wrong, but not a crime
☐ Just something that happens
☐ Don't know

Q224. How serious an incident do you think this was? On a scale of 1 to 20 - with 1 indicating a very minor incident like theft of a newspaper from the gate, to 20 for the most serious crime, murder - how would you rate this incident?

☐ 1 ☐ 11
☐ 2 ☐ 12
☐ 3 ☐ 13
☐ 4 ☐ 14
☐ 5 ☐ 15
☐ 6 ☐ 16
☐ 7 ☐ 17
☐ 8 ☐ 18
☐ 9 ☐ 19
☐ 10 ☐ 20

Q225. Apart from any financial losses you might have had, what would be a reasonable amount of money as compensation for the upset, and any disruption you and/or your household suffered?

A rough estimate will do.

PLEASE TYPE IN \$ AMOUNT BELOW. DO NOT TYPE COMMAS OR \$ SIGN.

Done Internet

Start ScreenDumps Briefing Qu... New Zealand Crime an...

2:42 p.m.

Confirmation screen appearing only if the amount given in Q225 was above \$10,000:

New Zealand Crime and Safety Survey 2006 - Microsoft Internet Explorer

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Q225 Is the amount \$23423 correct?

☐ Yes
☐ No

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Start Inbox - Microso... EasiQuest Microsoft Excel H:\CUSTOMISE... ScreenDumps B... New Zealand ...

4:37 p.m.

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Address http://survey.confirmat.com/wlx/p313091703.aspx Go Links

Q226. We now have some questions about other situations that sometimes happen in relationships. Does your **current partner...**

	Frequently	Sometimes	Never	Don't wish to answer
Prevent you from having your fair share of the household money?	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Prevent you from seeing friends and relatives?	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Follow you or keep track of your whereabouts in a way you feel is controlling or frightening?	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Call you names, insult you, or behave in a way to put you down or to make you feel bad?	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
Get angry if you speak with other women?	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

	Frequently	Sometimes	Never	Not applicable	Don't wish to answer
Upset you by harming or threatening to harm your children?	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

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Start Screendumps Briefing Qu... New Zealand Crime an... 2:43 p.m.

The next two screenshots show an unusual use of CAPITALS for emphasis (in addition to the usual colour/bolding change)

