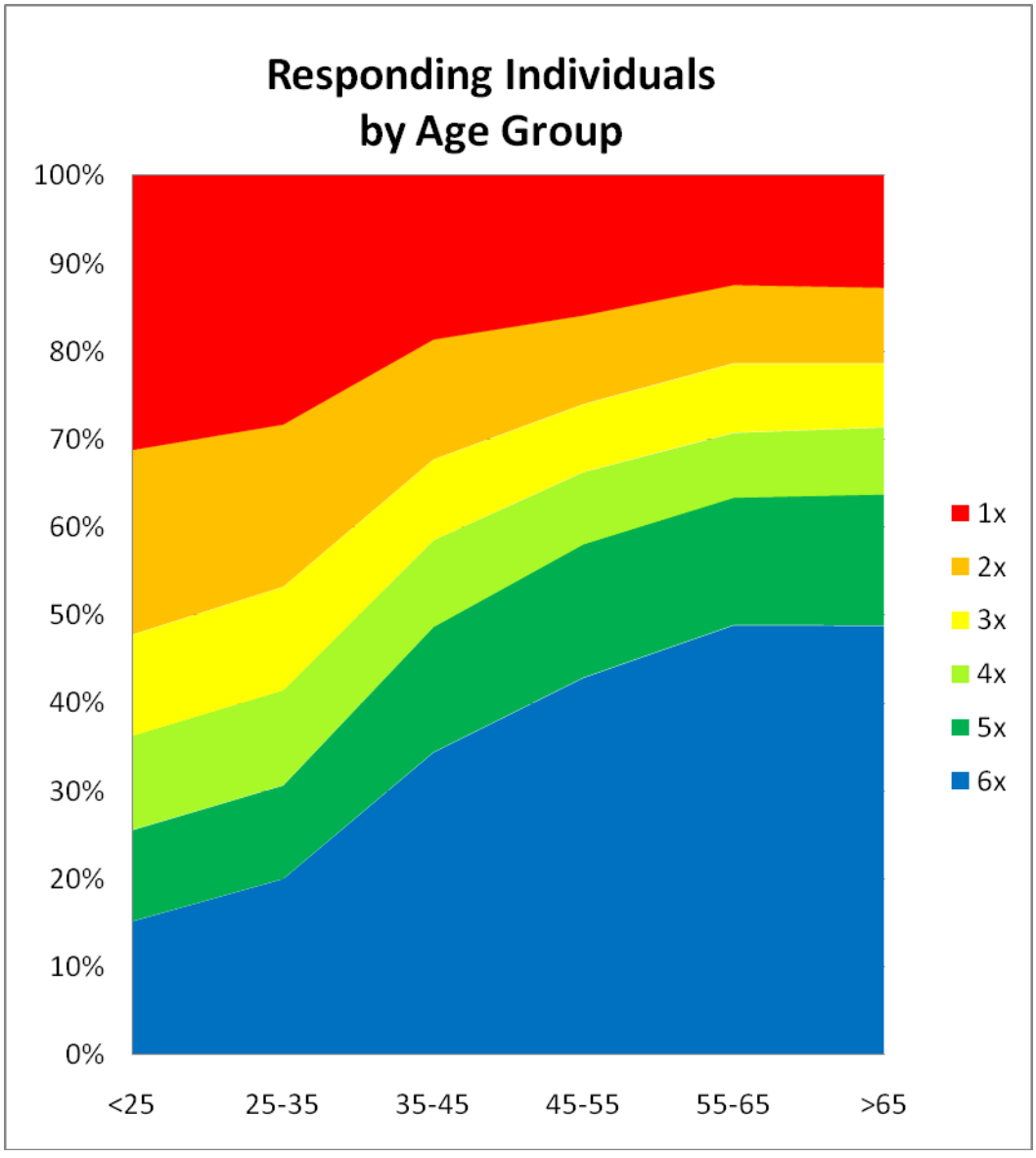
NORC Methodological Research to Support the Redesign of the National Crime Victimization Survey



Fall 2009

NORC Methodological Research to Support the Redesign of the National Crime Victimization Survey:

Analysis of Possible Non-Response Bias
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Presented to:
U.S. Department of Justice
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Erika Harrell
Program Manager
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Presented by:
National Opinion Research Center (NORC)
at the University of Chicago
1155 East 60th Street
Chicago, IL 60603
(773) 256-6000
(773) 753-7886 (Fax)

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Chapter 1

Background and Study Scope

This Report is divided into 4 main chapters, plus a summary (Chapter 5) and several appendices that contain details about the work NORC did on NCVS nonresponse issues. Included, too, is supporting information that can be used to broaden interpretations by NVCS experts beyond the conclusions offered here.

We have chosen, for the most part, to footnote technical details and references, as the report proceeds. But to those who want to approach this material in a nonsequential fashion we will also provide a glossary separately, on request.

1.1 Background

The measurement of crime and the validity and reliability of crime statistics have long been of concern to social scientists.[1] For much of the twentieth century the Uniform Crime Reports (UCR) produced by the Federal Bureau of Investigation (FBI) were considered "almost sacrosanct" as a source of official crime statistics in the United States.[2] However, by the late twentieth century there were a large number of studies questioning the extent to which UCR statistics can be treated as an accurate and adequate measure of crime.

To address these concerns, in 1973 the Bureau of Justice Statistics (BJS) introduced the National Crime Victimization Survey (NCVS, formerly NCS), which is fielded by the US Census Bureau. The purpose of that survey was "to learn more about crimes and the victims of crime [and] to measure crimes not reported to police as well as those that are reported.[3]" Data are collected twice a year from a nationally representative sample to obtain information about incidents of crime, victimization, and trends involving victims 12

[1] For example, see Biderman, A. 1967. "Surveys of Population Samples for Estimating Crime Incidence," *Annals of the American Academy of Political and Social Science,* 374 (1967): 16-33; and Biderman, A. 1981. "Sources of Data for Victimology," *The Journal of Criminal Law and Criminology,* 72 (1981): 789-817.

[2] Savitz, L. 1967. *Dilemmas in Criminology* (New York: McGraw Hill, 1967), 31.

[3] Bureau of Justice Statistics, *Report to the Nation on Crime and Justice,* 2nd ed., NCJ-105506 (Washington, DC: US Department of Justice, 1988), 11.

years of age and older and their households. The survey has long been considered a leader in making methodological advances.[4] The survey underwent an "intensive methodological redesign" in 1993 to "improve the questions used to uncover crime, update the survey methods, and broaden the scope of the crimes measured.[5]"

The UCR and the NCVS differ in that they "are conducted for different purposes, use different methods, and focus on somewhat different aspects of crime" (BJS 2004:1). So inevitably there are discrepancies between estimates derived from these two different measures of crime. Nonetheless, "long-term [NCVS and UCR] trends can be brought into close concordance" by analysts familiar with the programs and data sets.[6] This is not surprising in that the NCVS was designed "to complement the UCR program.[7]" So while the NCVS and UCR programs each were designed to collect different data, each offers data that are criminologically relevant, together they "provide a more complete assessment of crime in the United States.[8]"

The conclusion is that both programs are essential to the measurement of crime in the United States. So, while we will concentrate mainly on the NCVS, we make a modest parallel effort involving the UCR and highlight the possible joint use of both of these statistical series, in a manner that parallels the use by the Bureau of Labor Statistics (BLS) of employment and unemployment data, from the Current Population Survey and the Current Employment Program.

1.2 Types of Nonresponse

Operationally, two major components of survey nonresponse are conventionally considered – nonresponse due to noncontact and nonresponse due to refusal. The literature demonstrates that both noncontact rates and refusal rates have been on the rise

[4] Scheuren, F., *What Is a Survey?* 2000. www.whatisasurvey.info (accessed September 30, 2009).
[5] For example, see Bureau of Justice Statistics, *The Nation's Two Crime Measures*, NCJ-122705 (Washington, D.C.: US Department of Justice, 2004), 1.
[6] BJS 2004:2. Ibid., 2.
[7] BJS 2004:2. Ibid.
[8] Lauritsen, J.L. and Schaum, R.J. 2005. "*Crime and Victimization in the Three Largest Metropolitan Areas, 1980-98*," NCJ 208075 (Washington, D.C.: Bureau of Justice Statistics), http://www.ojp.usdoj.gov/bjs/pub/pdf/cv3lma98.pdf (accessed September 30, 2009).

in the recent decade and that, in face-to-face surveys**,** refusals can now be a larger component of nonresponse than noncontacts.[9] "Uncorrectable" nonresponse bias may arise mainly from noncontact nonresponse, since typically in such settings-- like the first wave of the NCVS--we have very little to go on in adjusting for the nonresponse.[10] Refusal nonresponse, on the other hand, often rises after a first contact, when some information is known about the respondents. What we know about the nonrespondents allows us to usefully distinguish among three models, first proposed by Rubin:[11]

Ignorable nonresponse: If the probability that a household or a within-household individual selected for the NCVS sample does not depend on the vector of information known about the sampling unit (such as geographic region, household income, race, gender, age, etc.), the response of interest (such as variables about victimization status), or the survey design, then the nonresponses are ignorable and can be treated as "missing completely at random" (MCAR). These nonresponses would be essentially selected at random from the sample and, therefore, can be ignored as a source of bias. They do, however, increase costs and raise concerns about the credibility of survey estimates.[12]

Conditional ignorable nonresponse: If the probability that a household or a within-household individual selected for the NCVS sample depends on the vector of information known about the sampling unit but not on the response of interest, the nonresponse can be treated as missing at random (MAR), given covariates. The nonresponse can be conditionally ignorable since we may use models to explain the

[9] See Atrostic, B. K. *et al.* 2001. "Nonresponse in U.S. Government Household Surveys: Consistent Measures, Recent Trends, and New Insights," *Journal of Official Statistics* 17: 209-226.

[10] Also, in some surveys like the CPS, a household that was not at home may be an indicator that the household members could be working. Temporary absent nonresponders in the CPS might, on the other hand, be on vacation**.**

[11] Rubin, D. 1978. "Multiple Imputations in Sample Surveys: A Phenomenological Bayesian Approach to Nonresponse," *Proceedings of the Survey Research Methods Section, American Statistical Association* (1978): 20-28. See also D. Rubin, "Inference and Missing Data," Biometrika 63, no. 3 (1976): 581-592.

[12] It is important to note that so far we have been talking about the bias of a single univariate variable*.* We will continue to do so but caution that, as mentioned in Scheuren, F. 2005. "*Seven Model Motivated Rules of Thumb or Equations," http://www.niss.org/sites/default/files/Scheuren.pdf (accessed on September 30, 2009*, most of the time all forms of nonresponse are present, sometimes for different variables, sometimes for different time periods.

nonresponse mechanism, and the nonresponse can be ignorable after the model accounts for it.[13]

Nonignorable nonresponse: If the probability of nonresponse depends on the value of a response variable such as victimization status and cannot be completely explained by the value of the vector of information known about the sampling units (household or individuals within a household), then the nonresponse is nonignorable or not missing at random (NMAR). Theoretically, by using additional covariates, perhaps from an augmented frame or from an earlier wave of the same survey, models can help in this situation. Make no mistake about the NMAR case, though; it can seldom be dealt with satisfactorily for the entire vector of survey variables. There are many cases, however, where, relative to sampling error, the mean square error (MSE) increase over the sampling variance (VAR) is small, i.e., $\{MSE/VAR\}^{1/2}$ lies within a narrow range not much larger than if there had been no nonresponse,[14] and hence confidence intervals are not unduly lengthened.

In the present Report we distinguish between the concerns about bias that a raw response rate might engender and measuring the bias arising from nonresponse after adjusting for it, using whatever is known about the selected units.[15] Different survey approaches may lead to a higher response rate for a similar cost. As pointed out in

[13] Obviously the more we know about the unit selected for study, perhaps from a strong frame or previous successful contacts, the more likely this form of nonresponse may be successfully modeled.

[14] This point is developed further in Scheuren, F, 2005. "*Seven Model Motivated Rules of Thumb or Equations.*" *http://www.niss.org/sites/default/files/Scheuren.pdf (accessed on September 30, 2009)*, in which the following related works are cited: W. G. Cochran, "*Sampling Techniques",* 3rd ed. (New York: John Wiley, 1977); and M. H. Hansen, W. N. Hurwitz, and W. G. Madow, "*Sample Survey Methods and Theory*", 2 vols. (New York: Wiley, 1953).

[15] In our treatment here we have largely focused on unit nonresponse concerns, as distinct from item nonresponse. In a complex survey like the NCVS, the line between these two forms of missingness gets blurry. There is a gray area where methods like multiple imputation (Rubin, D. 1978. "Multiple Imputations in Sample Surveys: A Phenomenological Bayesian Approach to Nonresponse," *Proceedings of the Survey Research Methods Section, American Statistical Association* (1978): 20-28) that grew up mainly to handle item nonresponse can be used to handle unit nonresponse just as well or do even better than weighting approaches. For a discussion of this, see the exchange between Little (Little, R. J. A. 1988. "Missing-Data Adjustment in Large Surveys," *Journal of Business & Economic Statistics* 6, no. 3 (1988): 287-296) and Scheuren. Scheuren, F. 1988. "Missing-Data Adjustments in Large Surveys: Comment," *Journal of Business & Economic Statistics* 6, no. 3 (1988): 298-299.

Scheuren (2005), unit nonrespondents, *m*, can be divided up into three parts (MCAR, MAR, and NMAR), all usually present in any given survey; that is --

$$\boldsymbol{m = m_{MCAR} + m_{MAR} + m_{NMAR}.}$$

For our work with the NCVS, it is important to learn the size of *m* overall, and, conditional on that value, how to minimize $\boldsymbol{m_{NMAR}}$.

The NORC efforts carried out so far have been confined to studies of unit nonresponse. Based on our prior work[16] we have working hypotheses on the relative sizes of the quantities m_{MCAR}, m_{MAR}, and especially m_{NMAR}. Of course, we do not expect to test all of our working hypotheses but shall state them for the record in any case.

1.3 Working Hypotheses

The challenges of nonresponse are both very hard and very common. Seldom, like now, though do we get a chance to explore the nature of unit nonresponse in depth. Still, because nonresponse is chronic, practitioners (like us) have had a chance to develop working hypotheses that have proven of value in settings similar to the ones we face with in the National Crime Victimization Survey (NCVS).

Of course, in our proposal NORC spelled out areas we thought worthy of study. These were and we paraphrase –

1. A still new modeling approach, labeled "Capture/Recapture," that uses two or more waves of the NCVS to estimate the average propensity to respond by NCVS wave.[17] We cover this in Chapter 2.
2. As an extension of this method, we will divide respondents at one wave between those who continued to remain respondents and those who later became nonrespondents. Particularly important will be differences between the first wave and later wave responders/nonresponders.[18] This is covered in Chapter 3.

[16] Scheuren, F. 2007. "Paradata Inference Applications" (presentation, International Statistical Institute, 56th Session, Lisbon, August 22-29).

[17] For example as set out in Scheuren, 2007. Ibid.

[18] See also the ideas in Kish and Hess (Kish, L. and Hess, I. 1958. "On Noncoverage of Sample Dwellings," *Journal of the American Statistical Association* 53 (1958): 509-524), plus the classic Hansen and Hurwitz (Hansen, M. H. and Hurwitz, W. N. 1946. "The Problem of Non-response in Sample Surveys," *Journal of the*

3. One standard method involves examining differences between easy and hard-to-get NCVS respondents – both households and individuals within selected households – using, say, the 2002-2006 or other available. We will do this primarily by looking at the survey by wave – also covered in Chapter 3.

4. Another common method we employ is to compare response rates and dispositions among key subgroups. To implement this analysis, we will use the log linear models. Again we will use two or more completed panels of 2002-2006 NCVS data. See also Chapter 3

5. The final method that NORC proposed compares respondents and nonrespondents directly by using sample frame variables or external data that can be matched to the survey (In particular from the Uniform Crime Reports.[19] This is dealt with in Chapter 4.

NORC's results for each of these areas are detailed in separate later chapters of this Report. But, first, let us set out our "priors" -- maybe a better phrase would be our points of view, or working hypotheses regarding unit nonresponse:

1. Survey practice regarding nonresponse, including in the NCVS, continues to use methods that grew up in an era of low unit and item nonresponse (the 1940/50s). This is a mistake. These methods need now to be augmented.

2. Organizations, like the US Census Bureau, that pioneered these earlier approaches, notably the application of implicit quasi-randomization methods[20] have stayed with them too long.

3. Costs of attempting to patch these older approaches (e.g., as by refusal conversion) have continued to grow and with no satisfactory way of measurably assessing whether or not they remain effective.

4. There is a very general belief that in a survey setting we need to use more modern methods. But where to start? And how to preserve the many good approaches that still seem to work.

American Statistical Association, 41, no. 236 (1946): 517-529, republished (in part) in *The American Statistician* 58, no. 4 (2004): 292-294.) paper on nonresponse, including the reprint by Scheuren (2004) in the American Statistician. ("with Introduction by Fritz Scheuren on the Topic of Nonresponse or "Missingness").

[19] If the frame variables or external data are related both to respondents' decisions to participate in a survey and to the survey variables of interest, then they are ideal as covariates in that they all researchers to lower both the survey variance and bias. This method, thus, yields potentially useful information on the nature of nonresponse and the potential size of nonresponse bias.

[20] Oh, H.L. and Scheuren, F. 1983. "Weighting Adjustment for Unit Nonresponse," in *Incomplete Data in Sample Surveys: Vol. 2*, *Theory and Bibliographies*, eds. W. G. Madow, I. Olkin, and D. B. Rubin (New York: Academic Press).

5. The NCVS has many aspects that offer "handles" to pull existing Census practice up to a more cost effective and inferentially supportive paradigm. We have partially explored some of these, to the extent our scope allowed.

6. The idea of using paradata more in making estimates has been growing but the actual paradata being obtained has not kept pace with the rhetoric. In fact, paradata remains an area that is "underdesigned" for inference.[21]

7. No surprise, then, that this would be true for the NCVS too! The Census collected paradata in the NCVS were designed to measure or monitor various survey subsystems and we found them very hard to use for addressing broader inference questions.

8. Why? The existing NCVS paradata created by the Census Bureau, have not adhered fully to Deming's dictum of being designed to "do systems thinking." In any case, right now in the NCVS, the Census Bureau is not looking enough at inference overall.

9. Perhaps the best example of this failure is that the Census Bureau has really not used the excellent longitudinal structure of the NCVS to improve cross-section estimates, which seem to be the main focus currently for BJS

10. Another idea that cannot be directly examined for the NCVS is the use of more frame data. The only exception, and this is a big one, is that we have explored augmenting the frame by linking in the Uniform Crime Reports by county. But more on this below (in Chapter 4).

11. Augmenting the NCVS frame might be of special use in reducing the mean square error of the first wave impact of the NCVS nonresponse. This is an especially important problem, given the nonresponse at the first survey wave of the NCVS is quite sizable and may well be nonignorable to an important degree.[22]

12. We can only speculate that, because of the weak frame variables now being used in the adjustment of first wave nonresponse, a number of other approaches might lead to improvements. Key here is finding a way to add new variables in an affordable way.

13. Which improvements to try requires serious modeling, not simply extending the quasi-randomization approach to a larger set of covariates, as useful as that might be.

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[21] The Recent FCSM session (a copy of the papers provided earlier to BJS) bears this out.

[22] A special study, not in our scope, would have been required to examine this directly in the NCVS, but the work of other researchers supports our conjecture (e.g., Sisto, J. and Rendtel, U., "Nonresponse and Attrition Effects on Design-Based Estimates of Household Income," in Harmonisation of Panel Surveys and Data Quality: CHINTEX: The Change from Input Harmonisation to Ex-Post Harmonisation in National Samples of the European Community Household Panel, Implications on Data Quality, M. Ehling *et al*. (Wiesbaden: Statistisches Bundesamt, 2004).

14. NORC cannot offer a modeling approach to nonresponse, without reminding the reader that we are not believers in the notion that a "best model exists and can be found. "

15. Rather, we have become believers in providing multiple estimates (e.g., as with a Bayesian approach) – indeed, in providing a distribution of answers. This approach is still new to Census surveys, although quite common when, say, making demographic (e.g., population) projections (as in the Census P-20 Series).

16. Much more is to be said about the value of making models explicit, defending them and offering ways for BJS customers, including BJS staffers, to link up their analysis models with the models that data producers, like the Census Bureau, might come up with.[23]

17. Usually, though, the biggest challenge, especially when a program is operational, is coming up with a way to make changes continuously and affordably.

18. Deming, again, tells us that perhaps the most important quality attribute is "constancy of purpose" or the ability for leaders to stay focused and work for the long run improvements.

19. Deming does not think well of managers, as distinct from leaders. Perhaps he attributes short run thinking to managers, not leaders.

20. Frankly and perhaps too bluntly the NORC recommendations made here are for leaders, not managers. What NORC recommends BJS try are small, affordable yet continuous changes (a Kaisen approach). This would allow them to operate adaptively -- making a revolution, one small step at a time! In the summary chapter, Chapter 5, NORC highlights some items that we recommend BJS began doing right away.

1.4 Working Tools

The tools NORC recommends are of several types. Some are implied already in our discussion of working hypotheses above. Some others will allow us to move the focus onto the implementation process we have in mind. It is very apparent to us and we began with this observation that the NVCS nonresponse problem is very complex, cannot be separated from other survey- going activities and will require novel use of new tools or old tools used in new ways. A sampling is given below:

[23] For example, see Scheuren, F. 2005. "*Seven Model Motivated Rules of Thumb or Equations,*" *http://www.niss.org/sites/default/files/Scheuren.pdf (accessed on September 30, 2009).*

First, we talked about the need for more use of a redesigned paradata system for the survey as a whole. This might be of most importance to second and subsequent waves.

Second, we mentioned an augmented frame for the survey, to the extent affordable. This might be particularly valuable for first wave nonresponse, where so little else is known.

Third, any proposed modern "solution" would need explicit use of nonresponse models and the display of alternative estimates that allow survey analysts to look at sensitivity issues routinely.

Fourth, we recommend that there be a direct use of earlier survey waves in making inferences at later waves. Familiar examples here might be the use of multiple survey weights and, for some variables; multiple imputation might be tried too.

Fifth, a redesigned data collection strategy that cuts way down on the reliance that now exists in the NCVS on callbacks. Callbacks implicitly assume all nonresponse is nonignorable when, as our work suggests (See Chapter 2) for the NCVS this may be far from true.

Sixth, we do not advocate eliminating callbacks but replacing by a focused but smaller, more intensive field effort to address the interpretation that nonignorable nonresponse poses.

Seventh, we strongly suggest the collection of more data about interviewers and a direct use of the interviewer-to-interviewer variability for inference. This is arguably paradata but so important that it deserves special mention.

Eighth, we would also look at the existing reinterview program (a look we were not commissioned to do). It might be integrated into the main NCVS estimates, partly for interpretative reasons and partly for improvement reasons.

Ninth, we believe the greatest value NORC will offer here is our ability to triangulate or borrow insights from each method to create an overall view of the nonresponse surface in the recent rounds of the NCVS.

Tenth, we expect to make recommendations about how to better measure the biasing portion of nonresponse in the NCVS. With these measures it may be possible to redirect resources to better target future survey efforts and to better measure biases and their effect on survey inference. The results will be used to inform better post-survey adjustment procedures.

Eleventh, Little and Zhang in the new book edited by Peter Lynn makes four recommendations which we see as key to a redirected nonresponse program. [24]

- At the design stage, record values of covariates that are predictive of nonresponse, and condition on these in imputing the missing values
- Consider following up a subsample of nonrespondents to recover at least the key missing information on these cases. These data can then be used to multiply-impute the information on incomplete cases that are not followed up.
- When there are various mechanisms of missingness, attempt to determine which of the missing values of a variable are likely to be MAR and which are likely not to be MAR, and then use MAR methods to multiply-impute the former. This reduces the scope of the NMAR problem for this variable, as compared with an analysis that fits a NMAR model to all the missing values.

In a separate appendix we provide still other improvement suggestions that were provided earlier but, for the most part, will not be developed here, accept on a limited basis in the summary Chapter 5

[24] Lynn, P. 2009. "*Methodology of Longitudinal Surveys,"* (New York: Wiley).

Chapter 2

Capture-Recapture Model of Potential Bias

2.1 Introduction

In this chapter, we examine a capture/recapture approach to estimating the fraction of the nonresponse that is potentially nonignorable. In each wave of the NCVS after the first, interviewers attempt to interview both prior nonrespondents and previously interviewed cases. Given this interview approach, we are then able to fit the following model.

1. Construct for each NCVS subgroup of interest 2x2 tables, with cell entries given by the values ***a***, ***b***, ***c***, and ***d*** – where the ***a*** cases had been interviewed twice, the entries ***b*** and ***c*** once each, and the entry ***d*** is for those not interviewed at all.
2. Under the assumptions of the capture/recapture model -- assumptions equivalent to ignorability — we can estimate the capturable or ignorable portion of the ***d*** cell, denoted d_I, as $d_I = bc/a$. The remainder ($d - d_I$) is then potentially nonignorable.[25]

In NCVS wave 1?	*In NCVS wave 2?*	
	yes	no
yes	***a***	***b***
no	***c***	***d***

This method, under a model, separates the occasional nonresponder from the chronic nonresponder, thereby making it possible to estimate the portion of nonresponse that is potentially nonignorable.[26] The name "capture/recapture" comes from the famous and often used dual systems approach to estimating undercoverage in censuses. The application of the old dual systems idea was first described in 2001 but can be expanded to

[25] The nonrespondents can be further subdivided into refusals and noncontacts, but the simpler model is presented here to explain the concept.

[26] Only the Wave 1/Wave 2 example has been used. This method can be employed with each pair of adjacent waves and has been in Appendix A.

cover a survey, like the NCVS, that has 7 waves.[27] Now, of course, there may be dependency across waves that would need to be modeled before the results were used. We do not believe, based on earlier applications[28] that this will be an insurmountable barrier, if handled properly.

What we are doing is treating those households[29] that respond on some occasion(s) but not others as missing at random (MCAR or MAR), while the "never responders" are more likely to be nonignorable (NMAR). The base and follow-up interviews for NCVS can, thus, be used under this model to estimate the portion of nonresponse that is potentially nonignorable. [30] Typically, in longitudinal surveys, and the NCVS would seem to be no different, attrition or chronic nonresponse becomes more and more common in later waves. In some longitudinal surveys, once a refusal occurred in an earlier wave, no further attempts were made in later waves. This is not the case with the NCVS, and we have used that fact in a manner similar to that used in Vaughan and Scheuren.[31]

2.2 NCVS Longitudinal Data and Interview Status across Waves

Each month the U.S. Census Bureau selects respondents for the NCVS using a "rotating panel" sample design. Households are randomly selected and all age-eligible individuals become part of the panel. Once in the sample, respondents are interviewed

[27] Scheuren, F. 2001. "Macro and Micro Paradata for Survey Assessment," in *1999 NSAF Collection of Papers*, by Tamara Black *et al.* and J. Michael Brick *et al.*, 2C-1 – 2C-15 Washington, D.C.: Urban Institute, http://anfdata.urban.org/nsaf/methodology_rpts/1999_Methodology_7.pdf. See also http://www.unece.org/stats/documents/2000/11/metis/crp.10.e.pdf (both accessed on October 2, 2009). Assessing the New Federalism Methodology Report No. 7.

[28] Scheuren, F. 2007. "Paradata Inference Applications," presented at the 56th Session of the International Statistical Institute, Lisbon, August 22-29.

[29] We do not know enough about the use of this model for the sampling of individuals within households, so we have not offered it for use here. A future study of this would be recommended, if enough resources were available. There are other priorities that would be placed higher, however. See Chapter 6.

[30] The fact that a household never responds does not mean that it is biasing and nonignorable. It could have characteristics very similar to those of respondents; hence we have characterized this group as only potentially nonignorable. Still, it is better that we use this unit nonresponse rate than a rate which treats all of the nonrespondents as potentially nonignorable.

[31] Vaughan, D. and Scheuren, F. 2002. "Longitudinal Attrition in SIPP and SPD," *Proceedings of the Survey Research Methods Section, American Statistical Association* (2002): 3559-3564.

every six months for a total of seven interviews over a three-year period.[32] For example, we constructed a longitudinal file for the households that came into the NCVS sample as the new incoming units to be interviewed for the first time in 2003. Two cohorts of NCVS households were setup, with first cohort containing households starting to be approached for interviews for the first time within the first six months of 2003, and the second cohort containing households starting to be interviewed for the first time within the second six months of 2003. Each of the households in these two cohorts can stay in the sample to be interviewed seven times for seven waves, till the first half of 2006 and the second half of 2006 respectively.

Noninterviews may occur at any of the waves for any of the households approached for interviews. A sample unit for which an interview could not be obtained is classified as one of three non-interview types, namely, Type A, Type B, and Type C noninterviews[33].

Tables 2.1 and 2.2 summarize the statuses of the households in the two cohorts across the seven waves starting from 2003. Take table 1 for example, among the 9,363 "incoming" households in the first cohort of 2003; there were 6,898 interviewed in the first wave, 1,372 were Type B non-interviews, 416 were Type C non-interviews, and the rest were Type A non-interviews (336 refusals, 236 with no one at home, and 105 for other Type A reasons). In each of the subsequent waves, some households were not linked for reasons such as their moving out of the sample. These, so called "not matched" cases were excluded from this analysis and excluded in the paired 2x2 capture/recapture analysis.

[32] *National Crime Victimization Survey, 2007 [Record-Type Files]: Codebook* (Ann Arbor, MI: Inter-university Consortium for Political and Social Research, 2009), http://www.icpsr.umich.edu/cgi-bin/bob/archive2?study=25141&path=NACJD&docsonly=yes (accessed on October 5, 2009).

[33] Type A non-interviews consist of households occupied by persons eligible for interviews but from whom non interviews were obtained because, for example, no one was found at home in spite of repeated visits, the household refused to give any information, the unit cannot be reached due to Type B non-interviews are for units which are unoccupied or which are occupied solely by persons who have a usual residence elsewhere (URE). Type C cases are ineligible addresses arising because of impassable roads, serious illness or death in the family, or the interviewer is unable to locate the sample unit. Because Type A non-interviews are considered avoidable, every effort is made to convert them to interviews. The "every effort" is extremely conservative and expensive strategy, especially given that much of the missingness may be ignorable.

Table 2.1: Summary of Interview Status of Households Starting in the First Six-Months of 2003

Wave	Not Matched	Interviewed	Type A			Type B	Type C	Total
			Refused	No One Home	Other			
1	.	6,898	336	236	105	1,372	416	9,363
2	641	6,806	330	205	104	1,230	47	9,363
3	667	6,789	363	181	91	1,245	27	9,363
4	703	6,783	383	164	87	1,224	19	9,363
5	1,276	6,226	423	169	87	1,169	13	9,363
6	1,662	5,903	385	155	65	1,185	8	9,363
7	4,266	4,043	250	117	37	643	7	9,363

Note: The period from Wave 1 to Wave 7 spans from 2003Q1Q2 to 2006 Q1Q2.
Source: NCVS 2003-2006

Table 2.2: Summary of Interview Status of Households Starting in the Second Six-Months of 2003

Wave	Not Matched	Interviewed	Type A			Type B	Type C	Total
			Refused	No One Home	Other			
1	.	6,924	339	275	108	1,383	468	9,497
2	740	6,881	306	183	92	1,250	45	9,497
3	803	6,748	352	192	86	1,287	29	9,497
4	1,306	6,307	370	216	73	1,201	24	9,497
5	1,694	5,964	359	174	84	1,199	23	9,497
6	4,485	3,861	290	121	35	692	13	9,497
7	4,446	3,964	232	82	55	698	20	9,497

Note: The period from Wave 1 to Wave 7 spans from 2003Q3Q4 to 2006 Q3Q4.

2.3 Fraction of Nonresponse That Is Ignorable

A key promising feature of the capture-recapture method for NCVS nonresponse analysis is its capacity to estimate the fraction of nonresponse that is ignorable and how the fractions of ignorable nonresponse can vary for various subgroups. To test the fraction of nonresponse that is ignorable, we examined the interview statuses for the whole range of the pairs of 2x2 waves, with the current wave tabulated by each of all the subsequent waves.

Table 2.3 and Table 2.4 show the capture-recapture analysis results on the interview status across waves among cohort 1 and cohort 2 households respectively. The last columns under [u/(b+c+d)]*100 calculate the fractions of nonresponses that may not

be ignorable. For any of the 2x2 pair of the waves, the fraction of nonresponse that is not ignorable falls into the range between about 10% to slightly less than 40%. That is, the majority of the nonresponses can be treated as ignorable. The results also reveal that the farther apart the two waves were the proportion of nonignorable nonresponses would be smaller.

The capture/recapture approach separates nonresponse cases into two forms of missingness -- ignorable and potentially nonignorable. This is, of course, under an independence model. The ignorable portion, by definition, is not biasing but does increase the sampling error because the number of respondents is reduced. It also raises the average cost per usable respondent too. The balance of the missingness is only potentially nonignorable. The balance, too, could be ignorable, if a more refined model were used. The interpretation of the capture/recapture results is based on the notion that some nonresponse is chronic, coming from units that never respond and some nonresponse is or behaves as if it were "random," coming from units that would respond or even do respond another time. In our treatment here we are using the model results as a lower bound on the ignorable nonresponse.

2.4 Ignorable Nonresponses and Returning Interviews by Subgroups

As an extension of the capture/recapture method, we divide respondents at one wave between those who continued to remain respondents and those who later became nonrespondents. The panel data of NCVS have considerable information about nonrespondents who participated in some earlier wave. There are data available on demographic and victimization characteristics; therefore, it is possible to discern differences between these individuals and those who continued to respond. In addition, study of later wave nonrespondents helps not only to develop nonresponse weighting adjustments[34] but also to gain an understanding of the causes of panel attrition[35] Tables

[34] Oh, L. and Scheuren, F. 1983. "Weighting Adjustment for Unit Nonresponse," in *Incomplete Data in Sample Surveys: Vol. 2*, *Theory and Bibliographies*, eds. W. G. Madow, I. Olkin, and D. B. Rubin (New York: Academic Press. 1983).

[35] Kalton, G. *et al.*. 1992. "Characteristics of Second Wave Nonrespondents in a Panel Survey," *Proceedings of the Survey Research Methods Section, American Statistical Association*: 462-467.

B2.1 – B2.7 in Appendix B present the capture recapture analysis on all household respondents and also by gender, race, and age. For each group, the summarized percentage of nonresponse that is ignorable is calculated. The extent of the returning interviews was also assessed.

A summary of the fraction of nonresponse that is ignorable is in Table 2.5. Overall, more than 80 percent of the nonresponses in NCVS can be regarded as 'ignorable." Proportionately, more nonresponses by male, black, and young (age 25 or less) eligible interviewees are ignorable. The largest of variation occur for the race/ethnicity, with eligible black interviewees having proportionately more ignorable nonresponses (84.81% vs. 80.43%).

Table 2.3 Capture-Recapture Analyses of Household Cohort 1[1] -- Interviews Across Waves

Wave by Wave	A	b	c	D	$d_i=bc/a$	$u=(d-d_i)$	[u /d]*100 [2]	[u /(a+b+c+d)]*100	[u/(b+c+d)]*100
ic12	6,156	304	294	275	14.52	260.48	94.7%	3.706%	29.838%
ic13	6,013	355	344	218	20.31	197.69	90.7%	2.853%	21.558%
ic14	5,912	404	384	178	26.24	151.76	85.3%	2.206%	15.710%
ic15	5,419	452	350	163	29.19	133.81	82.1%	2.096%	13.866%
ic16	5,086	411	354	143	28.61	114.39	80.0%	1.908%	12.598%
ic17	3,473	288	264	80	21.89	58.11	72.6%	1.416%	9.194%
ic23	6,134	289	291	279	13.71	265.29	95.1%	3.794%	30.884%
ic24	6,008	358	321	236	19.13	216.87	91.9%	3.133%	23.702%
ic25	5,457	420	304	201	23.40	177.60	88.4%	2.783%	19.200%
ic26	5,111	390	332	169	25.33	143.67	85.0%	2.394%	16.124%
ic27	3,491	256	242	109	17.75	91.25	83.7%	2.227%	15.034%
ic34	6,142	288	268	291	12.57	278.43	95.7%	3.984%	32.873%
ic35	5,559	382	272	237	18.69	218.31	92.1%	3.385%	24.502%
ic36	5,183	354	294	192	20.08	171.92	89.5%	2.854%	20.467%
ic37	3,548	238	216	130	14.49	115.51	88.9%	2.796%	19.779%
ic45	5,685	329	214	298	12.38	285.62	95.8%	4.377%	33.961%
ic46	5,276	313	240	246	14.24	231.76	94.2%	3.815%	29.007%
ic47	3,589	217	186	150	11.25	138.75	92.5%	3.350%	25.091%
ic56	5,250	251	275	297	13.15	283.85	95.6%	4.674%	34.490%
ic57	3,571	187	218	176	11.42	164.58	93.5%	3.964%	28.328%
ic67	3,646	156	163	206	6.97	199.03	96.6%	4.772%	37.910%

Note: a. Count of households interviewed in both designated waves

b. Count of households interviewed in the first designated wave but not in the second designated wave

c. Count of households interviewed in the second designated wave but in the first designated wave

d. Count of eligible households not interviewed in both designated waves

[1]Based on households in cohort 1 with the first rotation in the sample in the first 6 month in 2003

[2]Percentages in this column denote the percentages of potentially nonignorable missing households

Source: NCVS 2003-2006 Longitudinal File

Table 2.4: Capture-Recapture Analyses of Household Cohort 2[1] -- Interviews Across Waves

Wave by Wave	A	b	c	D	d_i=bc/d	u=(d-d_i)	[u/d]*100 [2]	[u /(a+b+c+d)]*100	[u/(b+c+d)]*100
ic12	6,214	251	313	284	12.64	271.36	95.5%	3.842%	32.000%
ic13	5,937	376	380	207	24.07	182.93	88.4%	2.651%	18.996%
ic14	5,468	429	378	185	29.66	155.34	84.0%	2.405%	15.660%
ic15	5,122	422	377	152	31.06	120.94	79.6%	1.991%	12.717%
ic16	3,298	316	251	95	24.05	70.95	74.7%	1.792%	10.718%
ic17	3,392	251	265	88	19.61	68.39	77.7%	1.711%	11.323%
ic23	6,147	307	243	264	12.14	251.86	95.4%	3.618%	30.942%
ic24	5,644	401	269	203	19.11	183.89	90.6%	2.822%	21.064%
ic25	5,245	410	291	150	22.75	127.25	84.8%	2.087%	14.953%
ic26	3,358	309	197	104	18.13	85.87	82.6%	2.164%	14.077%
ic27	3,442	250	218	85	15.83	69.17	81.4%	1.731%	12.507%
ic34	5,690	330	262	264	15.20	248.80	94.2%	3.801%	29.066%
ic35	5,260	344	275	222	17.98	204.02	91.9%	3.344%	24.259%
ic36	3,370	281	214	133	17.84	115.16	86.6%	2.880%	18.337%
ic37	3,438	218	230	114	14.58	99.42	87.2%	2.485%	17.690%
ic45	5,324	290	273	281	14.87	266.13	94.7%	4.315%	31.532%
ic46	3,372	236	210	174	14.70	159.30	91.6%	3.991%	25.694%
ic47	3,421	181	236	149	12.49	136.51	91.6%	3.424%	24.119%
ic56	3,470	200	178	212	10.26	201.74	95.2%	4.969%	34.193%
ic57	3,487	162	217	164	10.08	153.92	93.9%	3.819%	28.346%
ic67	3,532	126	207	205	7.38	197.62	96.4%	4.855%	36.732%

Note: a. Count of households interviewed in both designated waves

b. Count of households interviewed in the first designated wave but not in the second designated wave

c. Count of households interviewed in the second designated wave but in the first designated wave

d. Count of eligible households not interviewed in both designated waves

[1]Based on households in cohort 2 with the first rotation in the sample in the second 6 month in 2003

[2]Percentages in this column denote the percentages of potentially nonignorable missing households

Source: NCVS 2003-2006 Longitudinal File

Table 2.5 Ignorable Nonresponses by Subgroups

	Percent of Nonresponses that are Ignorable	Total Counts of Ignorable Nonresponses
All	81.10	2762
Male	84.04	1327
Female	83.43	1435
Black	84.81	469
Other	80.43	2294
Age 25 or Younger	84.11	323
Age 26 or Older	83.74	2441

2.5 Discussion

The longitudinal approach has been regarded as essential to study the performance of the justice system as a whole and it has been recommended that strategies for improving longitudinal structures, including improving the linkage capacity of existing data to fielding panel surveys of crime victims.[36] NORC heartily concurs, as we found at many points in our analyses where some research objectives had to be accomplished only indirectly, if at all.

The capture-recapture method proposed for NCVS has implications for the survey sponsor in that it can test whether there is evidence for a potentially serious nonresponse bias arising from the unobserved fraction of the refusals. It also has implications for the expensive refusal conversion process and the extent to which that process should be pursued based on its seemingly small bias reduction potential. Finally, the raw weighted nonresponse rate measure in NCVS could be recalibrated to reflect only the potentially nonignorable portion of the nonresponse. Like most surveys, the raw NCVS nonresponse rate continues to be used as a quality and credibility measure when, in fact, matters are far more nuanced. This one simple change could allow BJS to focus resources elsewhere, for example at the fall-off in reported crime incidences as the survey proceeds, wave by wave.

[36] Groves, R.M. and Cork, D.L. 2009. "*Ensuring the Quality, Credibility, and Relevance of U.S. Justice Statistics."* Washington, D.C.: National Academies Press.

2.6 Imputation and Its Impact on the 2002 NCVS New Sample Cohort

In a separate analysis, we constructed a longitudinal file using the "incoming" household cohort starting in the first half of 2002 and assess the impact of imputations of the nonresponses. During the first 6 months of 2002, a total of 9,484 households were selected in the NCVS sample. As part of the NORC's study on the possible nonresponse bias, we keep track of the changes of the interview status each time when the same households were in the subsequent surveys in the next three years, through the constructions of the longitudinal file which was based on the 2002-2005 NCVS. Table 2.6 shows the detailed survey response status by the waves.

Table 2.6: Tracking the Interview Status of the Same Sampled Household Respondents from 2002-2005

Survey Response Status	Wave 1	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6	Wave 7
Type A							
Interviewed	7004	6827	6745	6671	6658	6597	6131
Refused	348	290	329	381	388	397	365
No One at Home	232	163	156	160	156	185	153
Other Type A	114	96	84	94	88	83	58
Type A Subtotal	***7698***	***7376***	***7314***	***7306***	***7290***	***7262***	***6707***
*Refusal Rate**	*4.52%*	*3.93%*	*4.50%*	*5.21%*	*5.32%*	*5.47%*	*5.44%*
Type B	1298	1223	1249	1251	1252	1246	1176
Type C	488	32	27	22	21	28	18
Total	9484	8631	8590	8579	8563	8536	7901
Not Matched	NA	853	894	905	921	948	1583

Note: Refusal rate = Refused/Type A Subtotal. NA: Not applicable.
Source: NCVS 2002-2005

Indeed, some households were interviewed at an early wave but turned out to be a type A nonresponse case in the subsequent wave. Table 2.7 shows the survey response status for each of the waves after the initial wave, among those interviewed households in the immediate previous wave.

Table 2.7: Interview Status for Wave t by Interview Status for Wave (t+1) in a 2002 Household Cohort

Interviewed at Wave t	Interview Status at Wave (t+1)						
	Interviewed	Refused	No one at Home	Other Type A	Type B	Type C	Total
t=1	6181	107	78	58	316	10	6750
t=2	6187	118	89	67	304	3	6768
t=3	6045	158	97	69	323	4	6696
t=4	6062	124	92	67	287	2	6634
t=5	6031	113	116	56	289	5	6610
t=6	5606	96	96	45	261	4	6108

Source: NCVS 2002-2005

As shown, in Table 2.7, there were a total of (107+78+58) households interviewed in wave 1, which were Type A nonrespondents later on. These households in various waves were highlighted with bold in the appendix tables. If, as the capture/recapture model suggests, most of the wave missingness in the NCVS is ignorable, then elaborate strategies to address/reduce bias seem overkill and less expensive methods might be tried.

We do not have the scope in this exploratory study to do more than illustrate a simple way to impute nonresponse at later waves by using response achieved at earlier waves. In particular, one strategy is to have the values of survey variables of interests be imputed with the values of the same variables from the previous waves.

Table 2.8 shows the imputation rates for wave 2 to wave 7 household respondents. The crime incidence by the household respondents is listed, both before and after the imputations took place. Notice how similar the results are, suggesting that an imputation strategy, with its smaller variance, would be competitive with the current reweighting strategy. We are just touching on this rather large subject as a way of emphasizing the advise that is found in the work of Rod Little, mentioned in Chapter 1.

Table 2.8: Characteristics of Household-Level Crime Incidents Before and After the Imputations

Wave	Imputation Rate[1]	Incident Rate[2]	
		Before Imputation	After Imputation
t=1	None Used	0.1626214	0.1626214
t=2	5.96%	0.1306577	0.1333805
t=3	6.19%	0.1214233	0.1214718
t=4	6.20%	0.1178234	0.1177086
t=5	5.25%	0.1136978	0.1135273
t=6	3.31%	0.1041382	0.1054699
t=7	1.64%	0.1050400	0.1050400

Note: [1] "imputation rate" is calculated as (refused + no one at home + other type A). [2] "Incident #" refers to the total crime incident reports filled by the household respondent.

Source: NCVS 2002-2005.

The detailed wave-specific distributions of the number of crime incidents reported by household respondents were listed in Table 2.9. Weighted estimations after the imputations were also listed.

Table 2.9: Distribution of Number of Household-Level Crime Incidents before and after the Imputations

Wave	Total Crime Incident Reports Filled by the Household Respondents											
	0	1	2	3	4	5	6	7	8	9	10	Missing
Before Imputation (Unweighted)												
t=1	6119	710	128	30	9	2	5	1	0	0	0	2480
t=2	6132	557	104	18	12	2	1	0	0	1	0	1804
t=3	6116	495	95	26	9	4	0	0	0	0	0	1845
t=4	6061	489	85	25	7	1	2	1	0	0	0	1908
t=5	6092	442	82	29	6	6	0	0	0	0	1	1905
t=6	6057	433	84	15	5	1	0	0	2	0	0	1939
t=7	5632	404	67	18	3	4	1	2	0	0	0	1770
After Imputation (Unweighted)												
t=1	6119	710	128	30	9	2	5	1	0	0	0	2480
t=2	6340	583	109	20	13	3	1	0	0	1	0	1561
t=3	6135	498	95	26	9	4	0	0	0	0	0	1821
t=4	6075	490	85	25	7	1	2	1	0	0	0	1893
t=5	6102	442	82	29	6	6	0	0	0	0	1	1895
t=6	6076	434	84	15	5	1	0	0	2	0	0	1918
t=7	5632	404	67	18	3	4	1	2	0	0	0	1770
After Imputation (Weighted)												
t=1	13600365	1550072	281692	64471	21802	4344	14878	1858	0	0	0	0
t=2	12789236	1143400	213270	34796	30135	6596	1768	0	0	2318	0	732192
t=3	12844381	999030	188123	53540	22608	7228	0	0	0	0	0	1269887
t=4	12975230	946867	157061	50376	10270	0	4620	2542	0	0	0	1520087
t=5	13125963	883166	172880	53690	12179	11075	0	0	0	0	2112	1330438
t=6	13052627	874324	174859	36472	10381	1867	0	0	6956	0	2004	1318334
t=7	12113583	795064	133856	29935	6026	7478	2134	4205	0	0	0	1233171

Notes: (1) Non-matched cases were not included after wave 2. (2) Weighted numbers are rounded. (3) Weights for the wave t were used for wave (t+1) in the above table.

Source: NCVS 2002-2005.

Chapter 3

Response Analysis of Early vs. Late and Key Subgroups

3.1 Introduction

In the proposal, the second intended method to examine bias due to nonresponse would use a level-of-effort approach by contrasting respondents with different levels of recruitment effort. NORC has applied this approach in nonresponse bias analysis[37] and has found it effective in estimating the direction and the size of nonresponse bias. For the NCVS, we had proposed to compare survey data for 1) respondents who required less than three contact attempts/visits vs. respondents who required three or more visits to complete the survey, and 2) respondents who answered the survey request readily without refusal conversion effort vs. respondents who required refusal conversion effort. Unfortunately, the number of attempts to obtain an interview is not a data field readily available for use – nor is the amount of effort required to convert an initial refusal. These data may be available on a raw audit file kept by Census on a sample of the interviews. NORC did ultimately receive a copy of a Raw Audit File, but the amount of effort to decipher the variables and their meanings did not fit in with the requirements for this study. Thus, as a proxy, we use differences in estimates between respondents who were amenable and did not refuse the survey request and those who refused the survey request at least once but were converted in a later wave.

Several years of data are used to examine stability and trends of the patterns, the specific data used are shown in Appendix Table C.3.1. Overall, the household and person level public use files for 2002-2006, and 2007, as well as the linked household internally created file for 2002-2006 are used. Due to the longitudinal nature of the data collection, previous responses can be used in the same way as frame data to make nonresponse or missing data adjustments.

[37] See Skalland, B. *et al*. 2006. "A Non-Response Bias Analysis to Inform the Use of Incentives in Multistage RDD Telephone Surveys," *Proceedings of the Survey Research Methods Section, American Statistical Association*: 3705-3712.

Throughout this Chapter, results of logistic regression models are presented. We make no claim that the model results are any "best" predictors of nonresponse; instead, the purpose of the logistic models is threefold: (1) determining pockets or particular interactions of characteristics that correlate with response, (2) investigating the correlation of crime victimization estimates and response patterns, (3) comparing response patterns across longitudinal data versus annual collection efforts to build on the natural structure of the data.

3.2 Early vs. Late and Easy vs. Hard Responder Comparisons

The Census Bureau employs a rotating panel longitudinal sample to use for the NCVS interviews. Each selected household is included in the sample seven times over a period of three and a half years. Until 2006, the first interview was used as a bounding interview and not released on the public use file. Beginning in 2006, the first 'unbounded' interviews were phased in and included for release. NORC was given access to the internal files, and created two household level longitudinal cohort files for years 2002-2006 -- including the first or unbounded interview. Employing these data, we look at the frequency of response, by analyzing the distribution of wave response by key demographic variables. In particular, our exploratory analysis focuses on the panel survey response issue of continued response and dropout issues – that is, that initial respondents do not continue to respond through all waves of the survey. There are two issues to address – (1) which initial respondents are most likely to drop out and (2) after all data are collected, what is the best way to adjust for the non-response. The exploratory analysis focuses on singling out characteristics of drop outs. Using the cohort file NORC created, we looked at initial responding households that entered the survey in the second half of 2002 and computed how many waves they participated in.

Table 3.2: Number and Percent of Responding Households by Number of Waves Participation

Number of Waves Response	Number of Responders	Percent of Initial Responders
7 (all)	3722	53
6	1425	20
5	940	14
4	388	6
3	207	3
2	130	2
1 (only wave 1)	148	2
Total	6960	100

There is much literature about the differences in response rate by age[38]. Chart 3.1 is a stacked line graph that shows the percent of respondents in the age group that participated – shown is the number of waves they participated in, given that they participated in the first wave. The deep blue color shows the percent of respondents that participated in all 7 waves. The red (warning!) color shows those respondents that only participated in one wave. The percent adds up to 100 for each age group. It is clear that the younger age groups are less likely to continue responding. However even for the youngest age group, nearly 80% of the respondents did participate in at least 5 of the survey waves. Similar Charts for Educational Attainment and Reported Income are included in Appendix C, as charts C.3.1 and C.3.2.

[38] ***Ibid.***

Chart 3.1: Percent of Responding Households in Age Group by Number of Waves Participation, Internal Cohort File 2002-2006

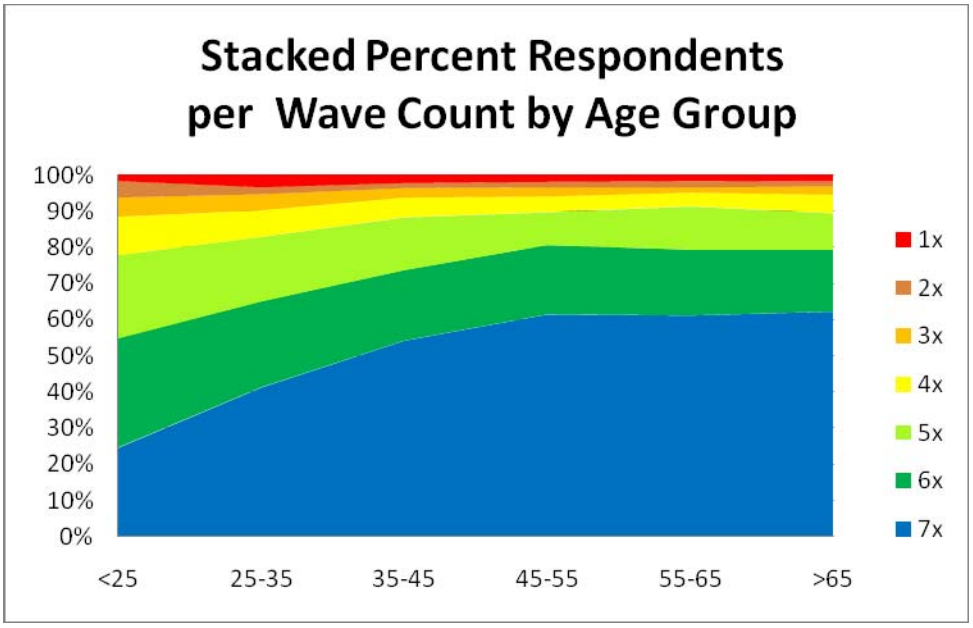
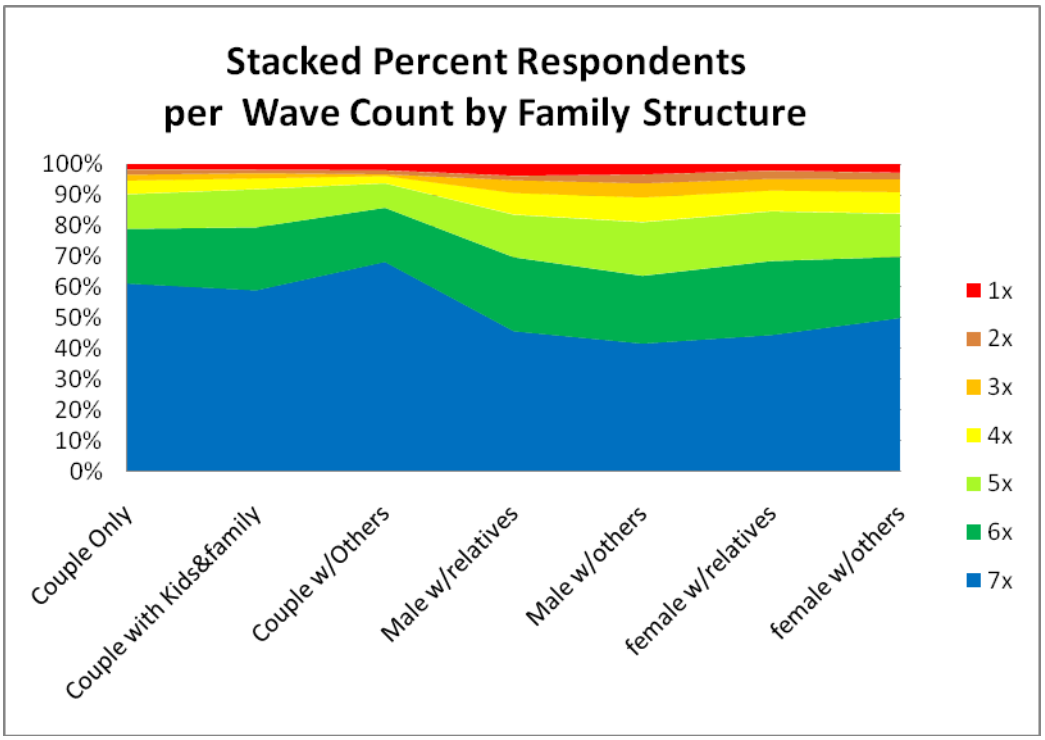


Chart 3.2 below, contains the stacked chart for different categories of household structure. Response appears higher for households with couples, versus households without couples.

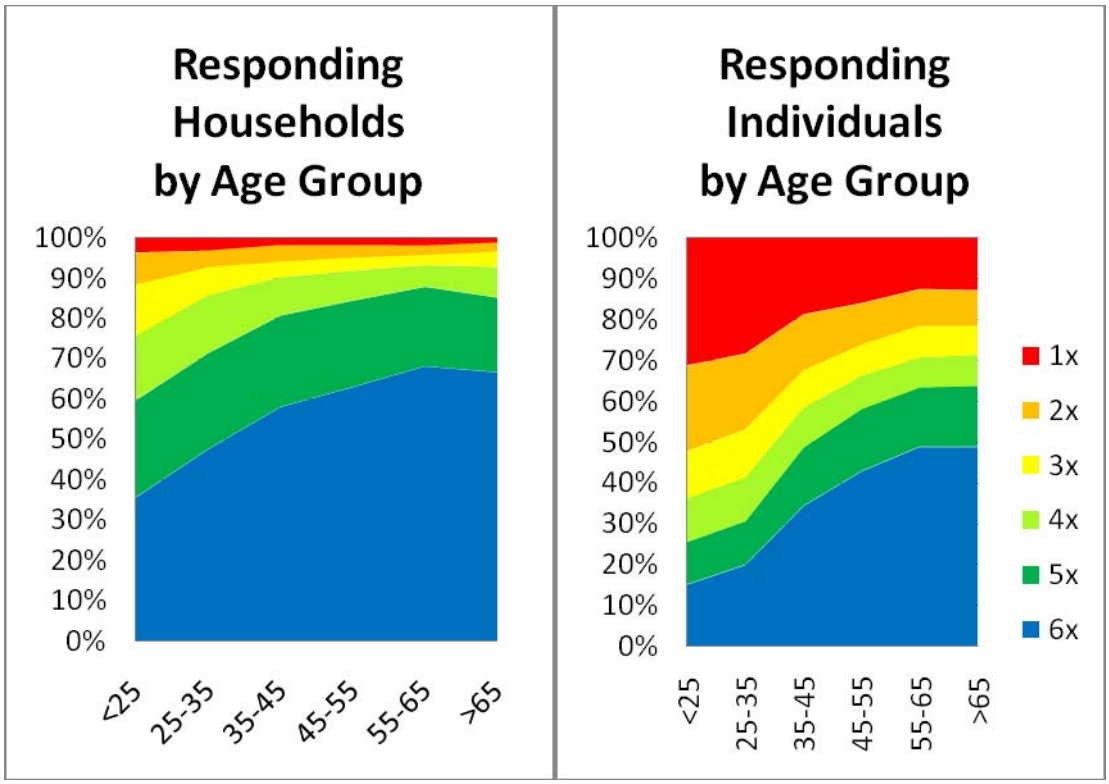
Chart 3.2: Percent of Responding Households by Household Structure by Number of Waves Participation, Internal Cohort File 2002-2006



In order to also investigate at the individual response level, Public Use Files (PUF) at the individual level were downloaded from the ICPSR site managed by University of Michigan.[39] These person level files were merged together in order to look at person level cohorts beginning in the first half of 2002. Since the first bounding interview is not included in the Public Use Files, the analysis here focuses on the results from Waves 2 through 7 for both the person level and household cohorts. By focusing on the panel/rotation group that was initially interviewed in the first half of 2002 (panel/rotation in 13,23,33,43,53,63), we are able to include all possible responses from that group for the remaining waves. The patterns are similar for the households and individual characteristics we examined. Chart 3.3 below is a double chart that compares household and person level stacked number of waves responded to. Similar charts for Education Attained, Hispanic Origin, and Race are included in Appendix C, Tables C.3.3 – C.3.5.

[39] NCVS public use data and documentation are available at http://www.icpsr.umich.edu/NACJD/NCVS/ (accessed on June - September, 2009).

Chart 3.3: Percent of Responding Households and Individuals in Age Group by Number of Waves Participation, for PUF 2002-2006



3.3 Modeling Continued Response and Characteristics of Drop Outs

The descriptive charts are informative re overall trends, but we also developed logistic regression models to explore interactions between the variables. For this exercise, we use the household cohort files, representing the cohorts beginning in the second half of 2002. As in the household graphs above, we only use records that responded to the first, bounding wave, and include their continued response. For prediction variables, indicators and grouped variables were developed for the following variables of interest. Also, interactions for race and Hispanic origin with the other variable groups were introduced.[40]

[40] *Since not all units responded to the first wave, the value used for the independent variable was taken from the earliest wave response available.*

Table 3.3: Variable Groups Input to Logistic Model of Response/Drop Out

Gender	Rural/Urban
Race: Black or Asian	Region
Hispanic Origin	Homeowner
Age	MSA Status
Marital Status	Family Structure
Education	Number of Crime Incidents
Household Income	

Two models were developed looking at the extremes of response, first we modeled continued response, or those households that responded to at least 6 waves. Correspondingly, we also developed a model to explore drop outs – that is, those that only responded to 3 waves or less. The logistic models were run with a stepwise procedure with the cut-off SLS=0.02. The model variables and their ranking are shown in the table below, the direction is also indicated. The specific logistic results are included in Appendix C. The concordance for both models was around 65%. Homeowner showed as the most important variable in both models. The interaction of Race=Black, with at least 1 crime incidence reported was significant for both models. This is something that should be investigated further. Income and Age came in with the expected direction of correlation. That is, age and income are both positively correlated with response. There was a good amount of overlap for the variables that showed up significant for the two models.

Table 3.4: Model Variables Shown in Order of Importance for the Logistic Continued Response/Drop Out Models

	Drop Out (3 or Less Wave Responses)		**Continued Response (6 or More Wave Responses)**	
Homeowner	**-1**			+1
Married		**-2**		+3
black*Incidence Reported		+3	**-9**	
Rural		+4		
Age Bounded (20,50)	**-5**			+2
Asian*Married		+6		
Rank of Household Income	**-7**			+8
South		+8		
Family Structure = Male w/others			**-7**	
Hispanic Origin				+4
Midwest				+5
Post College				+6

3.4 Differential Response Rates and Dispositions by Subgroups

Although, the NCVS data collection is based on a longitudinal sample design with the possibility of responding to the survey seven times in three and a half years, the NCVS releases estimates and public use files with an annual focus. To reflect this we too focus on annual response patterns. In particular, we investigate the data collected during 2002 and, for a more recent comparison, 2007. Instead of focusing only on one cohort, which is basically one-sixth of the total sample, we are able to include much more data. For the annual estimates, the selected units have the possibility of responding during January to June, and then separately again during July to December. For analysis of patterns of disposition outcomes, the entire annual data file is used. We also use the entire file for general patterns of geographic[41] and race for the Type A refusal nonresponse analysis. For the more detailed socio-crime related analysis which includes more detailed data collected for the survey, we investigate the response pattern of those responding Jan-June and/or July-December, for this analysis we only include the four cohorts that have the opportunity to respond in both periods.[42]

We analyze the differential response by beginning at the top examining the disposition patterns of sampled households and tunneling through to the detailed analysis of individual respondents. At the top of the analyses is the detailing of the disposition codes by the available geographic data – region, msa/not msa, place size, type of living quarters and land use (rural/urban). The first level of response is at the sampled household. As a benchmark, the resulting dispositions are compared for year 2002 and 2007 in terms of percent of total sampled units during January through December of the respective year. There is about a 4% decrease in overall percentage of interviewed household, almost half of this is due to an increase in the percent of vacant sampled units. There were also small 0.5% increases in Type A reasons – No One at Home, Refusals and Other. Overall the results appear fairly consistent for the two years. The detailed data is included as Appendix Table C.3.8. Delving a bit deeper, we looked at disposition across

[41] *Region, msa status, size of area, living quarters.*

[42] *That is, we omit the cohort that is finishing up in the Jan-June time frame, and the cohort that has its first interview in the July-Dec timeframe.*

geographic characteristics available on all sampled household units: region, land use, msa status, place size code, type of living quarters. Disposition code has been collapsed to the main categories. The results for urban/rural are shown in Table 3.5 below. There is a pattern of higher refusals in urban areas, and more vacant units in rural areas.

Table3.5: Major Disposition Outcomes for Sampled Units, by Urban/Rural

		Year 2002		Year 2007	
		Urban	Rural	Urban	Rural
Type A	No one home	2.06%	0.90%	2.33%	1.42%
	Refused	4.11%	2.86%	5.01%	3.37%
	Other Type A	1.05%	0.63%	1.39%	0.84%
Type B	Vacant-regular	8.60%	14.60%	10.52%	15.95%
	Other Type B	2.84%	6.73%	3.75%	6.62%
Type C	Demolished, converted to business	0.27%	0.58%	0.62%	1.11%
	Interviewed Household	81.07%	73.70%	76.38%	70.68%

Dropping out the Type B and Type C units, we focus on responders and Type A non responders. We are able to look at non response reason & responder results by these same geographic variables with the addition of race (black/non-black). The overall results are shown in Table 3.6. Overall, blacks appear less responsive, with more "No One Home" and "Refusals".

Table 3.6: Response Outcomes for Black and Non Black for Year 2002 and 2007

	Year 2002		Year 2007	
	Non Black	Black	Non Black	Black
Duplicate or Language problems	0%	0%	0.08%	0.08%
No one home	1.9%	3.2%	2.4%	3.5%
Temporarily absent	0.6%	0.6%	0.4%	0.3%
Refused	4.4%	5.0%	5.5%	5.9%
Other occupied	0.5%	0.5%	1.1%	1.3%
Respond	92.6%	90.7%	90.5%	88.8%
Total	100%	100%	100%	100%

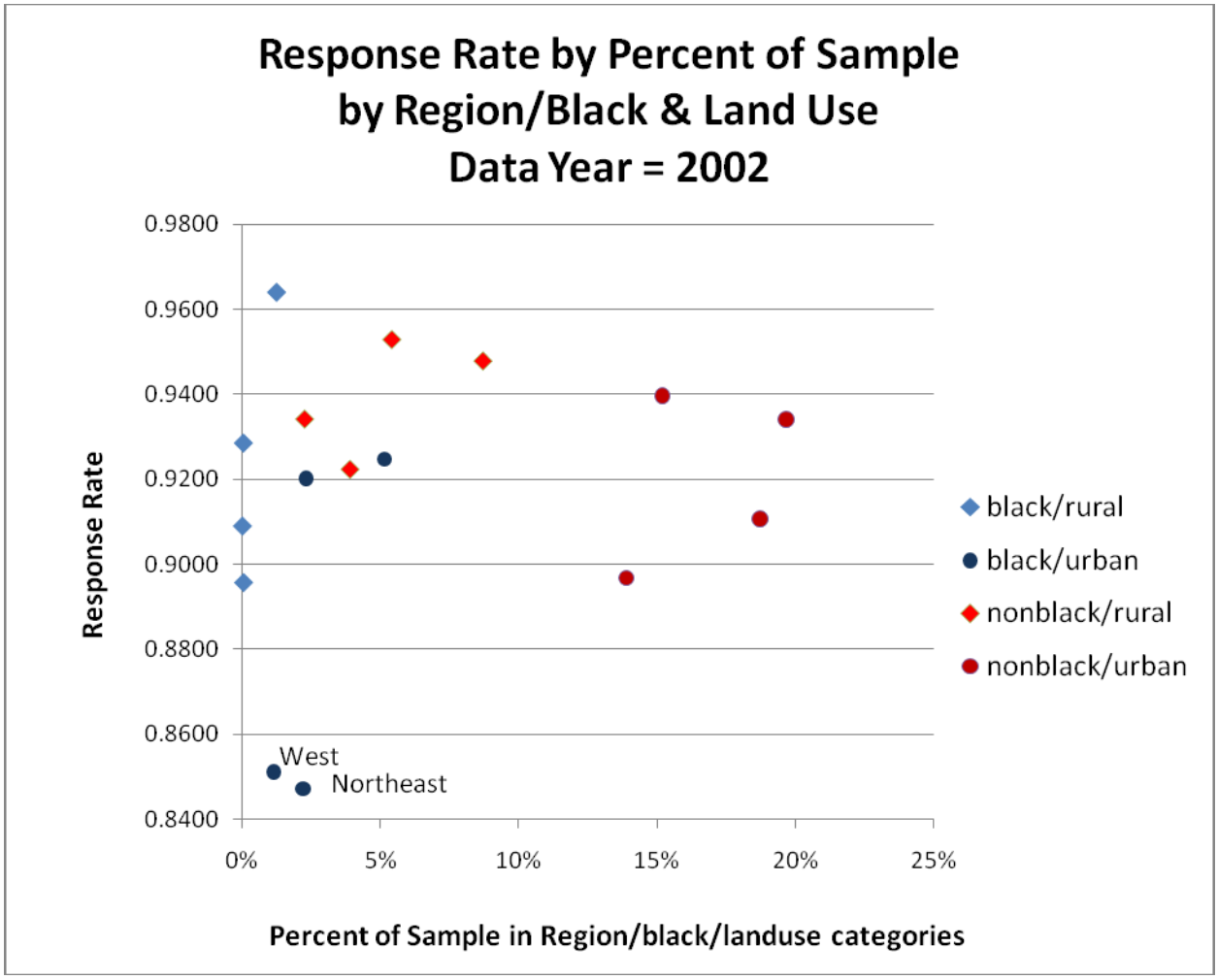
The response rates are shown separately for Region/black/nonblack in Table 3.7. Note there is a lower response rate for blacks in the North East and West for the year 2002, whereas the black response rate decreases for the Midwest region for 2007.

Table 3.7: Response Outcomes for Black/Non Black, by Region

		Response Rate	
		2002	2007
North East	Black	85%	85%
	Non-black	90%	87%
Midwest	Black	92%	86%
	Non-black	94%	93%
South	Black	93%	92%
	Non-black	94%	92%
West	Black	85%	83%
	Non-black	91%	89%

The lower response rate for the blacks in the Northeast and Midwest appears to be mainly due to low response in urban areas for those regions, as shown in Chart 3.4 below where response rate is graphed against percent of sample. Each point represents a group identified by Region, Urban/Rural, and Black/nonblack. The two points in the lower left corner, show the much lower response rate obtained for Black respondents in the Northeast and West urban areas.

Chart 3.4: Response Rate by Percent of Sample, 2002



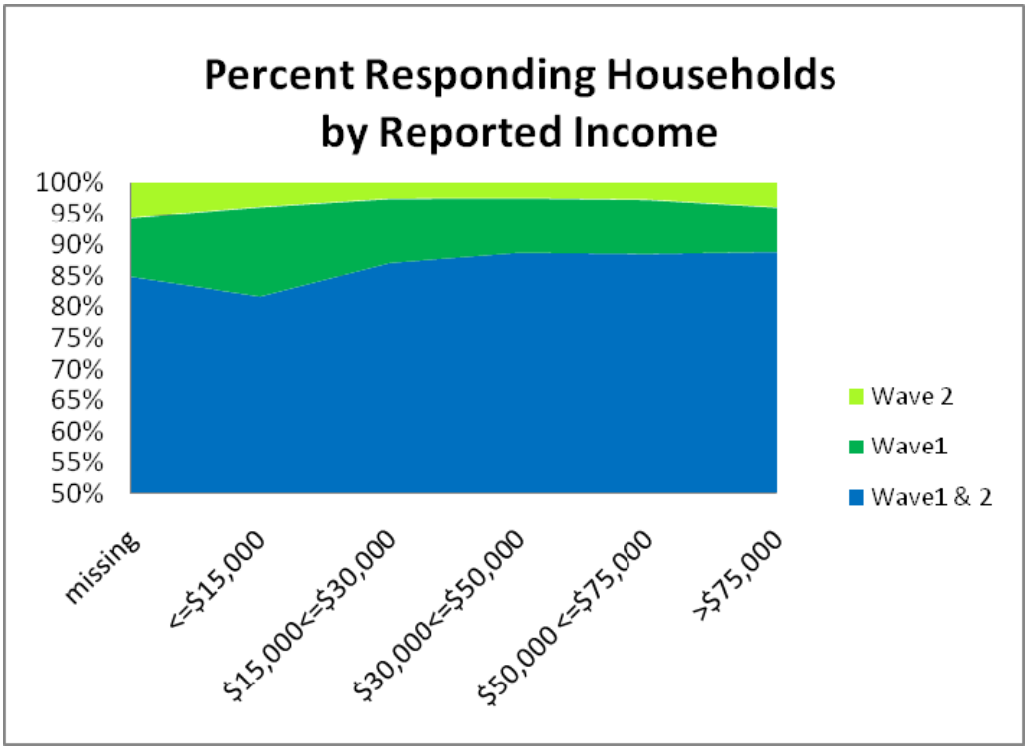
3.5 More on Responder Differences

We now turn to look at the differences in responders, where we have more detailed data as well as survey outcomes that allows a more intense view of the impacts of differential nonresponse. The question at this point becomes, what differential not missing at random non response remains that can be accounted for with models or other factors based on prior waves response.

The Public Use Files are structured to allow analysts to compute annual estimates, either in a collection year, or as the data year. We are working with the two waves that are put together to compute estimates for a collection year. Sampled units have an option of

responding to either the first or second, or preferably, to both waves in a given year. To get a feeling for the patterns, we first examine patterns of responding households for the data collection year. Response pattern per wave 1 and wave 2 by income is shown below in Chart 3.5, the corresponding graph by Education is included as Appendix Table C.3.11.

Chart 3.5: Percent Responding Households by Income, 2002



One method to examine the response impact, is to compute the restricted estimates by the response pattern (Jan-June only, both Jan-June & July-Dec, and July – Dec only) results, shown in Table 3.8 below, are based only on those households with the possibility of responding in both Jan-June and July-Dec 2002. That is, like the above graphs, the panels that were being rotated out or rotated in are not included.[43] There is not a noticeable difference in the restricted estimates for the different groups of responders.

[43] The population percentages, and the proportion of crime reported are weighted estimates, using the collection year weight available on the public use file.

Table 3.8: Restricted Results for Annual 2002 Estimates: Proportion of Households Reporting Crime Incident

	Nonresponse July-Dec	Respond July-Dec
Nonresponse Jan-June	% population Crime Incident	2% 0.0867
Response Jan-June	3% 0.0920	94% 0.0842

Using the more detailed data on the responders, we develop logistic regression models to predict nonresponse. In this situation, we separate the annual file into responders (responded in both time periods) and nonresponders (did not respond in one time period). We develop models for both 2002 and 2007. The results are similar as those where we used all of the wave responses to predict drop outs, or loyal responders. The concordance for the 2002 model is 62.7, for the 2007 model it is slightly higher at 68.7. One must note that there are 8% nonresponders in the 2002 data, and 14.5% for 2007. This difference is because the first (the unbounded) interview is included for analysis on the later public use file.[44] The logistic regression results are shown in Appendix Tables C.3.10 and C.3.11. [45]

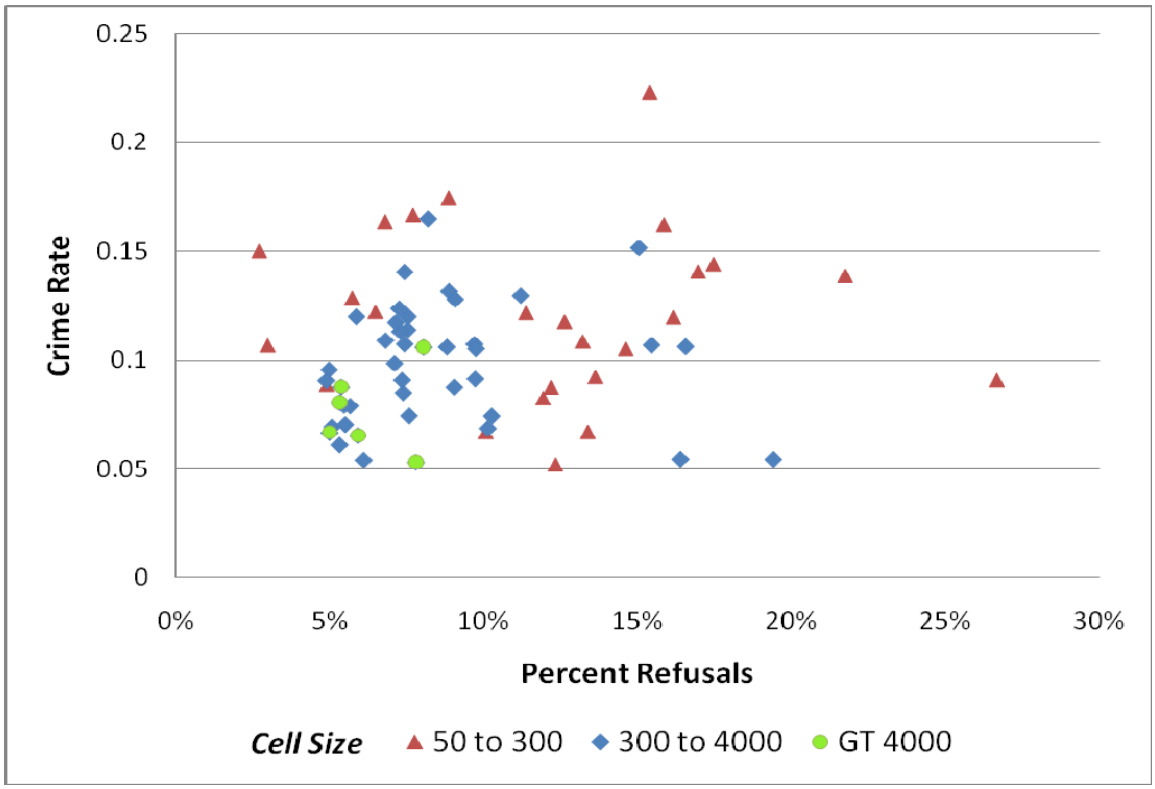
Stepping back from the detailed file, we consider broader patterns of nonresponse, including the Type A refusals, and their relationship to victimization estimates. The pattern in Chart 3.6 suggests something we already saw in our modeling work in Chapter 2; that it is plausible to believe that much of the nonresponse is not biasing. In Chapter 2 we assessed this from a process perspective. Here we are looking at refusal rates by crime rates and see little pattern. Again we caution against overpromising relative to low bias for the NCVS but consider the outcome encouraging. One last point: The nonresponse rate from the first round is not included for the 2002 results, but in the later public use files

[44] Beginning in 2006, the first bounding interviews are included on the Public Use Files.

[45] Another possible method for addressing nonresponse is to impute missing units using their prior survey data. Such an analysis was performed, the results are shown in Appendix Table C.3.12.

(e.g., for 2007) the crime rate estimates shown are cumulative of all rounds.[46] Similar plots are included in Appendix D, along with the table data.

Chart 3.6: Refusal Rate vs Crime Rate in Groups Defined by Region, Place Size & Race (black/non-black) [only groups with at least 50 individuals included in graph], Year 2002



[46] Beginning in 2006, the first bounding interviews are included on the Public Use Files.

Chapter 4
An Analysis of the NCVS and UCR Crime Statistics at the County-Level, 2003-2006

4.1 Introduction

The U.S. Department of Justice administers two statistical programs to measure the magnitude, nature, and impact of crime in the Nation: the Uniform Crime Reporting (UCR) Program and the National Crime Victimization Survey (NCVS). The UCR and the NCVS differ in that they "are conducted for different purposes, use different methods, and focus on somewhat different aspects of crime."[47] So inevitably there are discrepancies between estimates derived from these two different measures of crime. Nonetheless, "long-term [NCVS and UCR] trends can This is not surprising in be brought into close concordance" by analysts familiar with the programs and data sets[48] that the NCVS was designed "to complement the UCR program."[49] So while the NCVS and UCR programs each were designed to collect different data, each offers data that are criminologically relevant, and together they "provide a more complete assessment of crime in the United States" [50] than either could produce alone.[51]

The conclusion that both surveys are essential to the measurement of crime in the United States underscores the importance of the current request by BJS for proposals to conduct methodological research to support a present-day redesign of the NCVS.[52] More broadly, these are challenging times for survey research generally given dramatic and fast-

[47] BJS 2004:1. Bureau of Justice Statistics. 2007. *National Crime Victimization Survey: MSA Data, 1979-2004* [Computer file]. Ann Arbor, MI: Inter-University Consortium for Political and Social Research.

[48] ***BJS 2004-2***

[49] ***ibid.***

[50] Lauritsen, J.L. and Schaum, R.J. 2005. "*Crime and Victimization in the Three Largest Metropolitan Areas, 1980-98."* Washington, D.C.: Bureau of Justice Statistics, http://www.ojp.usdoj.gov/bjs/pub/pdf/cv3lma98.pdf (accessed September 30, 2009).

[51] Rand, M. R. 2009." *Criminal Victimization*, *2008*." Washington, D.C.: U.S. Bureau of Justice Statistics, http://www.ojp.usdoj.gov/bjs/pub/pdf/cv08.pdf (accessed on October 4, 2009).

[52] Federal Bureau of Investigation. 2008. *The Nation's Two Crime Measures. Uniform Crime Report, Crime in the United States, 2007."* Washington, D.C.: U.S., *http://www.fbi.gov/ucr/cius2007/documents/crime_measures.pdf* (accessed on October 4, 2009).

paced technological, social, and cultural change. It is also challenging how the UCR data may facilitate in improving the NCVS estimation counts at the local level.[53]

In order to better understand and utilize the relationship between the NCVS and UCR at the sub-national level, we examined the NCVS crime victimization estimates and the UCR arrest. Specifically, we attempted to estimate the victimization totals at the county level and compare all the NCVS county estimates with the count records from the UCR. For illustration, we focused on the 2003-2006 period, used four-year pooled NCVS and UCR, and examined summated measures of victimizations and crimes so that the NCVS and UCR measures can be better comparable.

The National Crime Victimization Survey (NCVS) Series, previously called the National Crime Surveys (NCS), has been collecting data on personal and household victimization through an ongoing survey of a nationally-representative sample of residential addresses since 1973. During the 2003-2006, household residents from all the 50 states plus the Districtrict of Columbia participated in the surveys. There are wide variations in terms of the numbers of the counties that were in the NCVS samples in this period. The top five states with the largest number of counties involved in the NCVS data collections were Texas (52 Counties), Virginia (47 counties), Ohio (44 counties), Georgia (39 counties), and New York (37 counties). Only one county within the following states had residents participating in NCVS during 2003-2006: Hawaii, New Hampshire, Vermont, and Wyoming.

4.2 Data Sources

This analysis examined the differences and the relationships at the county level between the National Crime Victimization Surveys and the Uniform Crime Reports (UCR) in the period of 2003-2006. New weights were developed for this analyses so that the county-level annual NCVS estimations of the totals can be produced. UCR information were

[53] McDowall, D. and C. Loftin, C. 2007. "What Is Convergence and What Do We Know About It?" in *Understanding Crime Statistics: Revisiting the Divergence of the NCVS and UCR*, eds. J. P. Lynch and L. A. Addington. New York: Cambridge University Press.

retrieved from the annualized county-level UCR data only for those counties in the NCVS samples in the same year. (See Appendix E, tables E4.1-E4.4.)

Because the BJS designed the NCVS to complement the UCR Program, the two programs share many similarities. As much as their different collection methods permit, the two measure the same subset of serious crimes, defined alike. Both programs cover rape, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Rape, robbery, theft, and motor vehicle theft are defined virtually identically by both the UCR and the NCVS. (Although rape is defined analogously, the UCR Program measures the crime against women only, and the NCVS measures it against both sexes.)

There are significant differences between the two programs: (1) the two programs were created to serve different purposes; (2) the two programs measure an overlapping but nonidentical set of crimes; (3) The NCVS includes crimes both reported and not reported to law enforcement. The NCVS excludes, but the UCR includes, homicide, arson, commercial crimes, and crimes against children under age 12. The UCR captures crimes reported to law enforcement but collects only arrest data for simple assault and sexual assault other than forcible rape. (3) the NCVS and UCR definitions of some crime differ. For example, the UCR defines burglary as the unlawful entry or attempted entry of a structure to commit a felony or theft. The NCVS, not wanting to ask victims to ascertain offender motives, defines burglary as the entry or attempted entry of a residence by a person who had no right to be there. [54]

4.3 Measurement

The National Crime Victimization Survey covers all of the index offenses covered by the Uniform Crime Reports, except for homicide and arson. Therefore, when comparing the total counts of crime victimizations and arrests, we exclude murder and arson from the UCR total count measure.

[54] Federal Bureau of Investigation. 2008. "*Crime in the United States, 2008* ." Washington, D.C.: U.S. Federal Bureau of Investigation, 2008, http://www.fbi.gov/ucr/cius2008/about/index.html (accessed on October 4, 2009).

Due to skewed distributions of the untransformed raw counts and "outliers" found in the scatterplots, separate alternative scatterplots were made using the logarithm transformations of the crime totals (log(counts +1)). Further scatterplots were shown with some peculiar counties (i.e., counties with no crime victimization reported, that is, NCVS county level crime incident count=0, and counties with no arrest reported, that is, UCR county level arrest count=0 for the 2003-2006 period) excluded.

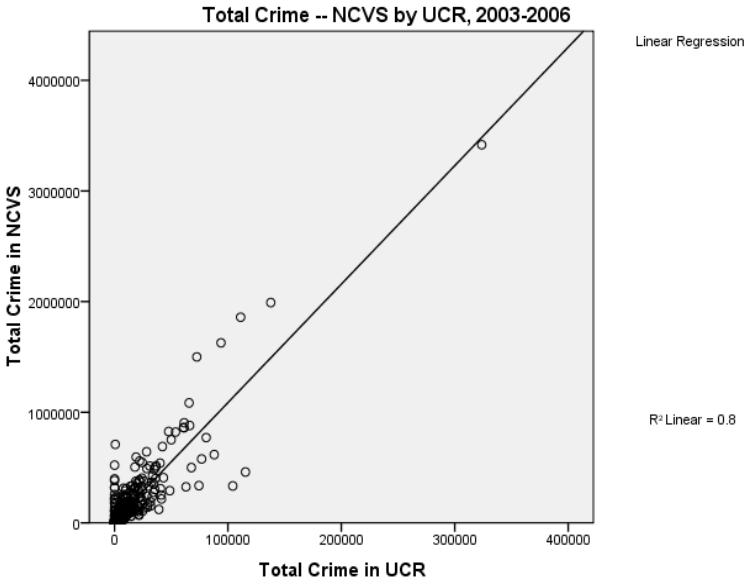
In this analysis, the crimes included in the totals from the NCVS included: Rape, Robbery, Assault, Burglary, Motor Vehicle Theft, Purse Snatching, and Theft; and the crimes included in the totals from the UCR included: Rape, Robbery, Assault, Burglary, Motor Vehicle Theft, and Larceny.

During 2003-2006, of all the counties where NCVS data were collected, a total of 46 counties showed zero number of arrests. All these 46 counties had considerable large amount of crime victimization incident reports in the same time period (see Table E4.5). A total of 56 counties had zero crime victimization incidents reported during 2003-2006, although many of them made many arrests for criminal offenses (see Table E4.6).

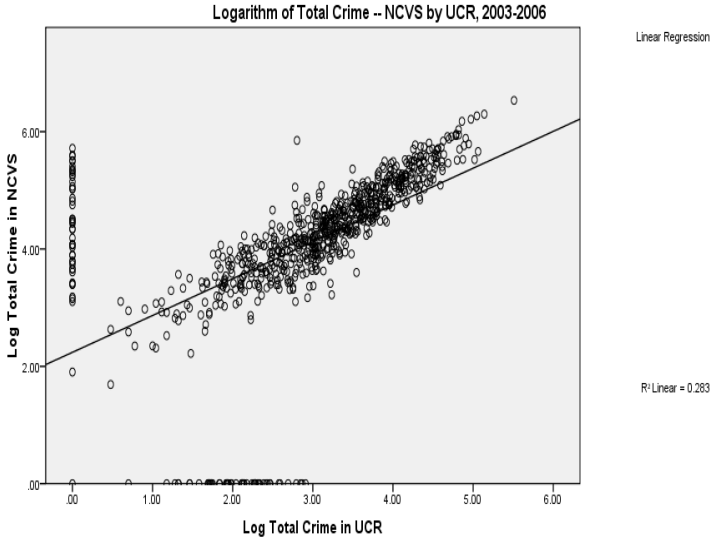
4.4 Results

Estimations and counts were obtained for each of the four years in 2003-2006. The combined totals at the county level were thereafter obtained through the summations of the year-specific totals in NCVS and UCR respectively. Only the results for the combined 2003-2006 are shown here. The year-specific scatter plots are also available in the NORC workpapers and appendices.

Figure 1 shows the scatter plot of the total victimizations in NCVS by the total crimes in UCR. Significant positive relationship was observed. The R^2 of the linear regression model was 0.80.



As the distribution of the victimization counts at the county level appears to be skewed, we made a logarithmic transformation on the outcomes without dropping any cases. Figure 2 shows the scatter plot.



Because of the logarithm transformations of the crime totals (log(counts +1)), counties with zero count of victimizations could still be shown; actually, the scatter plot in Figure 2 demonstrated that there were quite a few zero-type of counties from both NCVS

and UCR. Not surprisingly, the R^2 as a fit statistics of the regression model dropped dramatically due to these outliers.

4.5 Outliers

The counties with either victimization counts being zero or crime arrest counts being zero – were carefully examined next. Of course, these zero-counties are only an example of the data problems that a careful analysis might find

1. *UCR "zero-type "counties.* Among all the counties where NCVS data were collected during 2003-2006, a total of 46 counties were found to have "zero" number of arrests for any of the six major index crimes (murder was excluded). As shown in Table 4.1, 3/5 of these counties were located in the State of Florida, and 1/3 of these counties were located in the State of Illinois. Minnesota and Virginia each had one "zero-type" county.

2. *NCVS "zero-type" counties.* During 2003-2006, there existed 55 counties where NCVS data were collected but there were no victimization incidents reported. Virginia had the largest number of "zero-type" of counties (n=12), followed by Texas (n=6), Louisiana (n=4). Table 4.2 list all states which had at least one "zero-type" county.

Table 4.1: Distribution of Counties Where UCR Crime Counts During 2003 – 2006 Were Zero

State	Frequency	Percent
Florida	28	61
Illinois	16	35
Minnesota	1	2
Virginia	1	2
ALL	*46*	*100*

Table 4.2: Number of Counties Where NCVS Crime Counts During 2003 – 2006 Were Zero, by State

State	Frequency	Number of Total Counties
Virginia	12	12
Texas	6	6
Louisiana	4	4
Colorado, Georgia, Missouri, Tennessee	3	12
Iowa, Kentucky, Mississippi, Wisconsin	2	8
Alabama, Illinois, Indiana, Michigan, Minnesota, Nebraska, Nevada, New Mexico, New York, North Carolina, Oklahoma, Pennsylvania, Utah	1	14
All		56

Did the UCR "zero-type" counties have larger than 0 amount of victimization incidents reported in NCVS? or vice versa? The answer is yes to both. Details, including the counties involved are shown in the Appendix Tables E4.5 and E4.6, whereas the inconsistencies found between the UCR and NCVS may need further investigations, we excluded these "zero-type" counties from the subsequent analyses.

4.6 Relationship between the NCVS and UCR

Figure 4.3 shows the scatter plot of the total victimizations in NCVS by the total crimes in UCR among the counties which had non-zero amount of victimization incidents and criminal offense arrests.

Figure 4.3: Scatterplot of the Total Crime Counts, NCVS by UCR, for Counties Which Had At Least One Victimmization Incident and One Official Arrest, at the County Level, 2003-2006

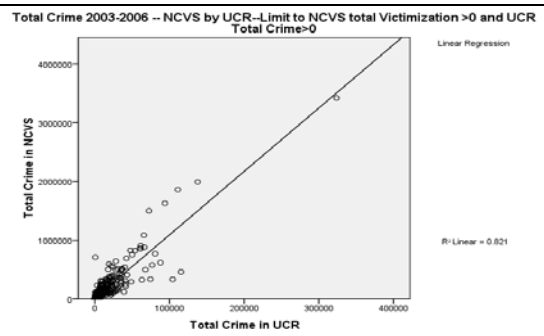
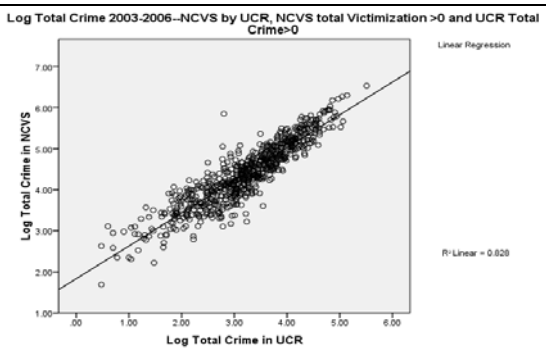
Figure 3a: Raw Totals	**Figure 3b:** Logarithm of Raw Total
	

Figure 4.4: Logarithms of Total Counts of Crime Incidents – NCVS by UCR, in 2003 – 2006, By Region

Excluding counties where total victimization incident count =0 or arrest count =0

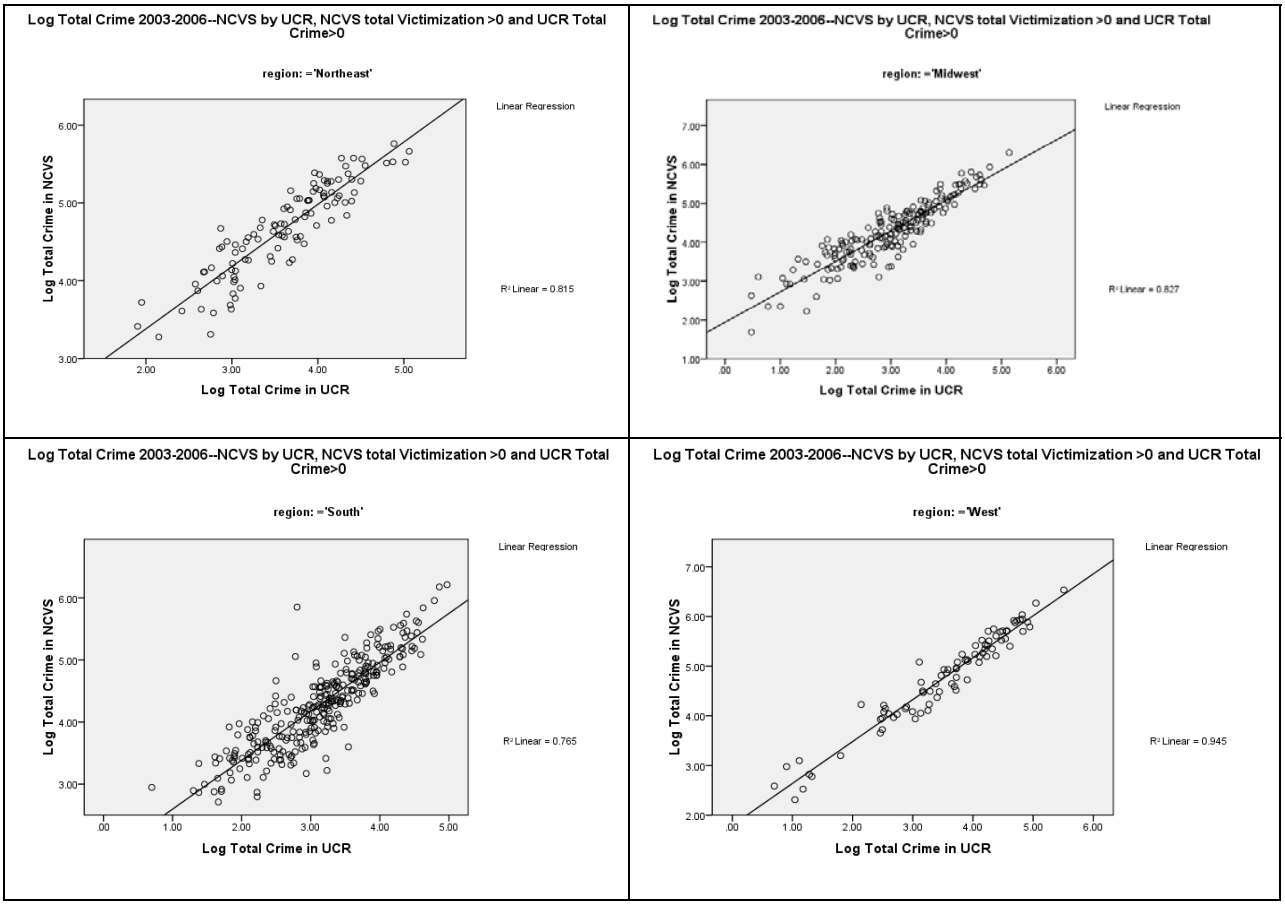


Figure 4.4 shows the scatter plots of the log transformations of the NCVS victimization incident count by UCR arrest count for each of the four regions separately.[55] Strong positive significant relationships were observed for each of the four regions.

Table 4.3 shows a summary of the R-squares in the linear regression models with the weighted estimations of the total counts of the crime victimization incidents reported in NCVS as the dependent variable and the total counts of arrests reported by the county-level UCR as the independent variable.

[55] Region-specific scatter plots on raw totals, region-specific scatter plots on raw totals with zero-type outliers excluded, and region-specific scatter plots on log transformations with raw totals are listed in the Appendix B, Tables B4.1-B4.3.

Table 4.3: R-squares in the regression analysis of the Arrests Reported by UCR and the Crime Victimizations Captured by the NCVS

Crimes	ALL	Region			
		Northeast	Midwest	South	West
Total crime	0.80	0.660	0.898	0.653	0.929
Logarithm of total crime counts	0.283	0.602	0.338	0.132	0.694
Total crime with restrictions to Total Victimization and Crime > 0	0.821	0.658	0.903	0.747	0.928
Logarithm of total crime counts with restrictions to Total Victimization and Crime > 0	**0.828**	**0.815**	**0.827**	**0.765**	**0.945**

Note: In the regression models depicted by the scatter plots, the square root of R-square is the same as the correlation coefficients. Overall, and across each of the census regions, the correlations (r) between the NCVS estimates and the UCR estimates are very high. The R^2 was 0.828 (r=.9+) at the national level, and ranged from 0.765 (r= +.8) to 0.945 (r= +.95) at the regional level.

There are variations across the four census regions in terms of the extent the magnitudes of the UCR arrest counts can explain the variability of the crime victimizations reported by householders. Regardless whether we transformed the crime and victimization counts or whether we eliminated the outliers such as those counties which had no or extremely high level of victimizations, the West Region had the highest level of R^2. (i.e., R^2 = .929 before any transformation and truncation; R^2 = .945 after the exclusion of outliers and the transformation).

In the past, the UCR and the NCVS have been used at the national level to assess their correlations on specific index crimes (BJS, 2007; Lauritsen and Schaum, 2005; McDowall and Loftin, 2007). Both high and low correlations have been observed. A high correlation between UCR and NCVS trends would suggest that either data series would serve as a reasonable proxy for some analytical purposes (NRC, 2008:72). In addition to definitional difference on certain crimes (FBI, 2008), there are conjectures on what would make the UCR and NCVS differ such as the matters concerning the public's willingness to report crime to the police and the way police departments record crime, how these factors may vary across regions or other geographic units remains an important questions that shall need further investigation which is beyond the scope this study.

Chapter 5

Summary and Recommendations

5.1 Summary

In this report we stated a number of working hypotheses about unit nonresponse (Chapter 1) and then set out to test some of them (Chapters 2 and 3). Chapter 4 was devoted mainly to the possible joint use of the NCVS, with the uniform crime reports (UCR).

Scope limitations restricted us mainly to exploratory methods on NCVS rounds after the first. We could not analyze nonresponse in the first round directly. Confirmatory results were not obtained but several results were suggestive of what a fuller analysis might find.

Bottom line, there was little evidence for nonresponse bias after the first round of the survey. Chapters 2 and 3 had information suggestive of this at the household level. In an appendix to Chapter 3, we display a logistic regression to characterize the person level nonresponse. Here, in this regression, we do find some evidence of differential nonresponse that might be biasing, if not addressed. However, the person level nonresponse is weight adjusted to age and race controls in the NCVS and these seem to be the categories that are the main drivers in any potential nonresponse bias.

5.2 Recommendations for Immediate Action

We have repeatedly expressed concerns about the first round being potentially biasing. A discussion of this and two other process recommendations are highlighted below.[56]

[56] We have, however, yet to address systematically the suggestions BJS wanted us to look at. These will be covered in our briefing later this month.

- *Nonresponse during first attempted contact*. The literature on panel surveys cited earlier suggests that the first round is where the potential for nonresponse bias is the most severe, largely because there are so few covariates to model and adjust with.[57] Doing more here in the NCVS, especially adding to the frame seems an obvious action step. Bringing forward additional data from the UCR or the previous census would be good. A close examination of the paradata picked up when there is a noncontact or a refusal in the first round outcome might be made. In NORC's Survey of Consumer Finances, for example, neighborhood information is obtained. Some pairing of cases ahead of time, e.g., having two linked interviews in the same ultimate cluster could be a sensible precaution for household, person, and item nonresponse.
- *Reinterviews to check on response quality and nonresponse bias*. The scope of the NORC proposal kept us from looking at the Census Bureau's reinterview program. We would recommend time be spent studying how successful this effort is and whether it could be harnessed to study a small sample of nonresponse cases from each round of the NCVS, especially but not exclusively the first round.[58] Since the focus will be on bias examination a very high response rate will be needed for these reinterviews, making this an expensive undertaking in time and money. To limit the effort, a real-time MIS might be set up and results posted routinely. Stopping rules could be developed after the program started and after efforts to optimize resources were attempted.
- *Imputation Experiments.* In Chapter 1 we mentioned ideas about how to plan and carryout nonresponse adjustments that were mixtures of reweighting and imputation. These seem to offer the best general approach to NCVS

[57] With only a limited number of covariates the nonresponse may, *ceteris paribus*, be more often nonignorable.

[58] BJS already has staff conversant in such methods and their limitations (e.g., Sinclair, M.D. 1994. "Evaluating Reinterview Survey Methods for Measuring Response Errors," (Ph.D. dissertation, George Washington University).

missingness, whether of whole households, persons or individual items. This too should be tried in a limited way.

In Appendix G six broad areas are covered: (1) Paradata construction and related issues. (2) Longitudinal NCVS Constructions and Related Analyses, (3) Addressing bias and variance reduction, (4) Bounding procedures, (7) Examining the organization of crime event information and (6) Design Elements. These might be addressed here but will not. Instead, they will be taken up selectively when NORC and BJS get together later this month.[59]

[59] The three suggestions that BJS considered useful in the short term were (1) Improving paradata construction and related interviewer information, (2) Improving information related to contact with respondents and (3) Analyses of longitudinal cohorts.

Appendix A

Table A2.1: Capture-Recapture Analyses of Household Cohort 11 -- Refusals across Waves

Wave by Wave	a	b	c	d	d_i=bc/a	u=(d-d_i)	[u /d]*100 [2]	[u /(a+b+c+d)]*100	[u/(b+c+d)]*100
ic12	150	147	157	6375	153.86	6221.14	97.6%	91.099%	93.145%
ic13	122	174	219	6415	312.34	6102.66	95.1%	88.061%	89.639%
ic14	108	190	252	6328	443.33	5884.67	93.0%	85.558%	86.923%
ic15	101	171	287	5825	485.91	5339.09	91.7%	83.632%	84.977%
ic16	82	185	279	5448	629.45	4818.55	88.4%	80.390%	81.505%
ic17	41	133	188	3743	609.85	3133.15	83.7%	76.325%	77.095%
ic23	169	135	170	6519	135.80	6383.20	97.9%	91.280%	93.540%
ic24	150	155	216	6402	223.20	6178.80	96.5%	89.250%	91.227%
ic25	127	142	265	5848	296.30	5551.70	94.9%	86.990%	88.756%
ic26	110	162	253	5477	372.60	5104.40	93.2%	85.045%	86.633%
ic27	60	119	175	3744	347.08	3396.92	90.7%	82.892%	84.124%
ic34	193	140	168	6488	121.87	6366.13	98.1%	91.088%	93.675%
ic35	156	144	237	5913	218.77	5694.23	96.3%	88.283%	90.471%
ic36	129	163	232	5499	293.15	5205.85	94.7%	86.433%	88.325%
ic37	81	134	153	3764	253.11	3510.89	93.3%	84.968%	86.667%
ic45	209	125	192	6000	114.83	5885.17	98.1%	90.180%	93.164%
ic46	174	147	190	5564	160.52	5403.48	97.1%	88.946%	91.569%
ic47	94	116	137	3795	169.06	3625.94	95.5%	87.541%	89.574%
ic56	218	156	143	5556	102.33	5453.67	98.2%	89.802%	93.146%
ic57	118	122	107	3805	110.63	3694.37	97.1%	88.978%	91.581%
ic67	144	92	85	3850	54.31	3795.69	98.6%	91.002%	94.256%

Note: a. Count of refusal households in both designated waves

b. Count of households refusing in the first designated wave but not in the second designated wave

c. Count of households refusing in the second designated wave but in the first designated wave

d. Count of eligible households not refusing in both designated waves

[1]Based on households in cohort 1 with the first rotation in the sample in the first 6 month in 2003

[2]Percentages in this column denote the percentages of potentially nonignorable missing households

Source: NCVS 2003-2006 Longitudinal File

Table A2.2: Capture-Recapture Analyses of Household Cohort 2[1] -- Refusals across Waves

Wave by Wave	a	b	c	d	d_i=bc/d	u=(d-d_i)	[u/d]*100 [2]	[u /(a+b+c+d)]*100	[u/(b+c+d)]*100
ic12	145	154	143	6620	151.88	6468.12	97.7%	91.591%	93.511%
ic13	118	171	221	6390	320.26	6069.74	95.0%	87.967%	89.498%
ic14	96	181	252	5931	475.13	5455.88	92.0%	84.456%	85.730%
ic15	80	176	255	5562	561.00	5001.00	89.9%	82.348%	83.447%
ic16	58	115	218	3569	432.24	3136.76	87.9%	79.211%	80.388%
ic17	45	131	177	3643	515.27	3127.73	85.9%	78.272%	79.163%
ic23	158	118	172	6513	128.46	6384.54	98.0%	91.719%	93.849%
ic24	118	147	223	6029	277.81	5751.19	95.4%	88.249%	89.876%
ic25	94	150	236	5616	376.60	5239.40	93.3%	85.948%	87.294%
ic26	67	95	205	3600	290.67	3309.33	91.9%	83.421%	84.855%
ic27	50	111	165	3669	366.30	3302.70	90.0%	82.671%	83.719%
ic34	170	138	171	6067	138.81	5928.19	97.7%	90.562%	92.977%
ic35	136	143	195	5627	205.04	5421.96	96.4%	88.870%	90.896%
ic36	92	105	178	3623	203.15	3419.85	94.4%	85.539%	87.554%
ic37	69	119	149	3663	256.97	3406.03	93.0%	85.151%	86.645%
ic45	183	133	152	5700	110.47	5589.53	98.1%	90.621%	93.392%
ic46	111	105	158	3618	149.46	3468.54	95.9%	86.887%	89.372%
ic47	92	124	126	3645	169.83	3475.17	95.3%	87.163%	89.221%
ic56	148	82	126	3704	69.81	3634.19	98.1%	89.512%	92.898%
ic57	108	112	105	3705	108.89	3596.11	97.1%	89.234%	91.691%
ic67	144	130	72	3724	65.00	3659.00	98.3%	89.902%	93.199%

Note: a. Count of refusal households in both designated waves

b. Count of households refusing in the first designated wave but not in the second designated wave

c. Count of households refusing in the second designated wave but in the first designated wave

d. Count of eligible households not refusing in both designated waves

[1]Based on households in cohort 2 with the first rotation in the sample in the second 6 month in 2003

[2]Percentages in this column denote the percentages of potentially nonignorable missing households

Source: NCVS 2003-2006 Longitudinal File

Table A2.3: Capture-Recapture Analyses of Household Cohort 1[1] -- Other Type A across Waves

Wave by Wave	a	b	c	d	d_i=bc/a	u=(d-d_i)	[u /d]*100 [2]	[u /(a+b+c+d)]*100	[u/(b+c+d)]*100
ic12	24	273	248	6484	2821.00	3663.00	56.5%	52.113%	52.291%
ic13	22	274	210	6424	2615.45	3808.55	59.3%	54.957%	55.132%
ic14	20	278	202	6378	2807.80	3570.20	56.0%	51.908%	52.059%
ic15	15	257	212	5900	3632.27	2267.73	38.4%	35.522%	35.606%
ic16	12	255	181	5546	3846.25	1699.75	30.6%	28.358%	28.414%
ic17	13	161	126	3805	1560.46	2244.54	59.0%	54.678%	54.852%
ic23	22	282	207	6482	2653.36	3828.64	59.1%	54.750%	54.922%
ic24	28	277	200	6418	1978.57	4439.43	69.2%	64.126%	64.386%
ic25	14	255	215	5898	3916.07	1981.93	33.6%	31.055%	31.123%
ic26	13	259	183	5547	3645.92	1901.08	34.3%	31.674%	31.743%
ic27	13	166	117	3802	1494.00	2308.00	60.7%	56.320%	56.499%
ic34	18	315	200	6456	3500.00	2956.00	45.8%	42.295%	42.404%
ic35	13	287	213	5937	4702.38	1234.62	20.8%	19.141%	19.180%
ic36	16	276	169	5562	2915.25	2646.75	47.6%	43.944%	44.061%
ic37	20	195	114	3803	1111.50	2691.50	70.8%	65.138%	65.455%
ic45	17	317	209	5983	3897.24	2085.76	34.9%	31.961%	32.044%
ic46	12	309	183	5571	4712.25	858.75	15.4%	14.136%	14.164%
ic47	17	193	119	3813	1351.00	2462.00	64.6%	59.440%	59.685%
ic56	24	350	163	5536	2377.08	3158.92	57.1%	52.016%	52.222%
ic57	19	128	119	3793	801.68	2991.32	78.9%	73.696%	74.042%
ic67	18	218	115	3820	1392.78	2427.22	63.5%	58.193%	58.445%

Note: a. Count of other type A households in both designated waves

b. Count of households being other type A in the first designated wave but not in the second designated wave

c. Count of households being other type A in the second designated wave but in the first designated wave

d. Count of eligible households not being type A in both designated waves

[1]Based on households in cohort 1 with the first rotation in the sample in the first 6 month in 2003

[2]Percentages in this column denote the percentages of potentially nonignorable missing households

Source: NCVS 2003-2006 Longitudinal File

Table A2.4: Capture-Recapture Analyses of Household Cohort 2[1] -- Other Type A across Waves

Wave by Wave	a	b	c	d	d_i=bc/d	u=(d-d_i)	[u/d]*100 [2]	[u /(a+b+c+d)]*100	[u/(b+c+d)]*100
ic12	30	269	217	6546	1945.77	4600.23	70.3%	65.141%	65.419%
ic13	21	268	223	6388	2845.90	3542.10	55.4%	51.335%	51.491%
ic14	19	258	247	5936	3354.00	2582.00	43.5%	39.969%	40.087%
ic15	17	239	222	5595	3121.06	2473.94	44.2%	40.737%	40.851%
ic16	6	167	129	3658	3590.50	67.50	1.8%	1.705%	1.707%
ic17	10	166	107	3713	1776.20	1936.80	52.2%	48.468%	48.590%
ic23	22	254	219	6466	2528.45	3937.55	60.9%	56.566%	56.745%
ic24	21	244	242	6010	2811.81	3198.19	53.2%	49.075%	49.233%
ic25	15	229	215	5637	3282.33	2354.67	41.8%	38.626%	38.722%
ic26	5	157	135	3671	4239.00	-568.00	-15.5%	-14.315%	-14.333%
ic27	10	151	110	3724	1661.00	2063.00	55.4%	51.640%	51.769%
ic34	28	280	225	6013	2250.00	3763.00	62.6%	57.485%	57.732%
ic35	24	255	211	5611	2241.88	3369.13	60.0%	55.223%	55.441%
ic36	8	189	136	3665	3213.00	452.00	12.3%	11.306%	11.328%
ic37	14	174	100	3712	1242.86	2469.14	66.5%	61.729%	61.945%
ic45	20	296	216	5636	3196.80	2439.20	43.3%	39.546%	39.675%
ic46	11	205	130	3646	2422.73	1223.27	33.6%	30.643%	30.728%
ic47	14	202	98	3673	1414.00	2259.00	61.5%	56.659%	56.859%
ic56	15	215	123	3707	1763.00	1944.00	52.4%	47.882%	48.059%
ic57	13	207	100	4058	1592.31	2465.69	60.8%	56.320%	56.488%
ic67	22	252	93	3703	1065.27	2637.73	71.2%	64.809%	65.161%

Note: a. Count of other type A households in both designated waves

b. Count of households being other type A in the first designated wave but not in the second designated wave

c. Count of households being other type A in the second designated wave but in the first designated wave

d. Count of eligible households not being type A in both designated waves

[1]Based on households in cohort 2 with the first rotation in the sample in the second 6 month in 2003

[2]Percentages in this column denote the percentages of potentially nonignorable missing households

Source: NCVS 2003-2006 Longitudinal File.

Appendix B

Table B2.1: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 ALL Household Respondents

Waves (t by t+1)	Interviewed at Both Waves	Interviewed at Wave t but not Wave t+1	Interviewed at Wave t+1 but Not t	Type A Nonresponses at Both t and t+1	Expected (Modeled) Value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	6156	294	304	275	14.52	yes	94.72
t=2 by t+1=3	6134	185	289	92	8.72	yes	90.53
t=3 by t+1=4	6142	171	288	97	8.02	yes	91.73
t=4 by t+1=5	5685	138	329	95	7.99	yes	91.59
t=5 by t+1=6	5250	193	251	86	9.23	yes	89.27
t=6 by t+1=7	3646	111	156	51	4.75	yes	90.69
All t by (t+1)	**33013**	**1092**	**1617**	**696**	**53.22**	**yes**	**92.35**

Total ignorable	*(1092+1617+53)=2762*
% of nonresponse that is ignorable	*2762/(1092+1617+696)=81.1%*

Waves (t by t+1)	Type A Nonresponses at Both t and t+1	Never Interviewed Again	Interviewed at Least Once	Percent Returned to be Interviewed
t=1 by t+1=2	275	179	96	34.9
t=2 by t+1=3	92	39	53	57.6
t=3 by t+1=4	97	48	49	50.5
t=4 by t+1=5	95	61	34	35.8
t=5 by t+1=6	86	70	16	18.6
All t by (t+1)	**645**	**397**	**248**	**38.4**

Table B2.2: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 MALE Household Respondents

Waves (t by t+1)	Interviewed at both waves	Interviewed at wave t but not wave t+1	Interviewed at wave t+1 but not t	Type A Nonresponses at both t and t+1	Expected (modeled) value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	3288	105	166	71	5.30	yes	92.53
t=2 by t+1=3	3223	100	130	50	4.03	yes	91.93
t=3 by t+1=4	3212	75	147	48	3.43	yes	92.85
t=4 by t+1=5	2967	78	161	42	4.23	yes	89.92
t=5 by t+1=6	2747	94	122	41	4.17	yes	89.82
t=6 by t+1=7	1855	49	77	23	2.03	yes	91.16
All t by (t+1)	**17292**	**501**	**803**	**275**	**23.21**	**yes**	**91.56**

Total ignorable	*(501+803+23)=1327*
% of nonresponse that is ignorable	*(501+803+23)/(501+803+275)=84.04%*

Waves (t by t+1)	Type A Nonresponses at Both t and t+1	Never Interviewed Again	Interviewed at Least Once	Percent Returned to be Interviewed
t=1 by t+1=2	71	29	42	59.2
t=2 by t+1=3	50	26	24	48.0
t=3 by t+1=4	48	26	22	45.8
t=4 by t+1=5	42	27	15	35.7
t=5 by t+1=6	41	38	3	7.3
All t by (t+1)	**252**	**146**	**106**	**42.1**

Table B2.3: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 FEMALE Household Respondents

Waves (t by t+1)	Interviewed at Both waves	Interviewed at Wave t but not Wave t+1	Interviewed at Wave t+1 but not t	Type A Nonresponses at Both t and t+1	Expected (modeled) Value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	2868	189	138	98	9.09	yes	90.72
t=2 by t+1=3	2911	85	159	42	4.64	yes	88.95
t=3 by t+1=4	2930	96	141	49	4.62	yes	90.57
t=4 by t+1=5	2718	60	168	53	3.71	yes	93.00
t=5 by t+1=6	2503	99	129	45	5.10	yes	88.66
t=6 by t+1=7	1791	62	79	28	2.73	yes	90.23
All t by (t+1)	**15721**	**591**	**814**	**315**	**29.90**	**yes**	**90.51**

Total ignorable	*(591+814+30)=1435*
% of nonresponse that is ignorable	*(591+814+30)/(591+814+315)=83.43%*

Waves (t by t+1)	Type A Nonresponses at both t and t+1	Never interviewed again	Interviewed at least once	Percent Returned to be Interviewed
t=1 by t+1=2	98	44	54	55.1
t=2 by t+1=3	42	13	29	69.0
t=3 by t+1=4	49	22	27	55.1
t=4 by t+1=5	53	34	19	35.8
t=5 by t+1=6	45	32	13	28.9
All t by (t+1)	**287**	**145**	**142**	**49.5**

Table B2.4: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 BLACK Household Respondents

Waves (t by t+1)	Interviewed at Both Waves	Interviewed at Wave t but not Wave t+1	Interviewed at Wave t+1 but not t	Type A Nonresponses at Both t and t+1	Expected (modeled) value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	786	53	50	35	3.37	yes	90.37
t=2 by t+1=3	793	35	52	12	2.30	yes	80.87
t=3 by t+1=4	791	31	47	15	1.84	yes	87.72
t=4 by t+1=5	738	27	50	15	1.83	yes	87.80
t=5 by t+1=6	677	31	40	11	1.83	yes	83.35
t=6 by t+1=7	532	15	26	8	0.73	yes	90.84
All t by (t+1)	**4317**	**192**	**265**	**96**	**11.90**	**yes**	**87.60**

Total ignorable	*(192+265+12)=469*
% of nonresponse that is ignorable	*(192+265+12)/(192+265+96)=84.81%*

Waves (t by t+1)	Type A Nonresponses at Both t and t+1	Never Interviewed Again	Interviewed at Least Once	Percent Returned to be Interviewed
t=1 by t+1=2	35	25	10	28.6
t=2 by t+1=3	12	7	5	41.7
t=3 by t+1=4	15	7	8	53.3
t=4 by t+1=5	15	10	5	33.3
t=5 by t+1=6	11	9	2	18.2
All t by (t+1)	**88**	**58**	**30**	**34.1**

Table B2.5: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 OTHER Household Respondents

Waves (t by t+1)	Interviewed at Both Waves	Interviewed at Wave t but not Wave t+1	Interviewed at Wave t+1 but not t	Type A Nonresponses at Both t and t+1	Expected (Modeled) Value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	5370	241	254	240	11.40	yes	95.25
t=2 by t+1=3	5341	150	237	80	6.66	yes	91.68
t=3 by t+1=4	5351	140	241	82	6.31	yes	92.31
t=4 by t+1=5	4947	111	279	80	6.26	yes	92.17
t=5 by t+1=6	4573	162	211	75	7.47	yes	90.03
t=6 by t+1=7	3114	96	130	43	4.01	yes	90.68
All t by (t+1)	**28696**	**900**	**1352**	**600**	**42.10**	**yes**	**92.98**

Total ignorable	*(900+1352+42)=2294*
% of nonresponse that is ignorable	*(900+1352+42)/(900+1352+600)=80.43%*

Waves (t by t+1)	Type A Nonresponses at both t and t+1	Never interviewed again	Interviewed at least once	Percent Returned to be Interviewed
t=1 by t+1=2	240	154	86	35.8
t=2 by t+1=3	80	32	48	60.0
t=3 by t+1=4	82	41	41	50.0
t=4 by t+1=5	80	51	29	36.3
t=5 by t+1=6	75	61	14	18.7
All t by (t+1)	**557**	**339**	**218**	**39.1**

Table B2.6: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 Household Respondents Who Were 25 or Younger

Waves (t by t+1)	Interviewed at Both Waves	Interviewed at Wave t but not Wave t+1	Interviewed at Wave t+1 but not t	Type A Nonresponses at Both t and t+1	Expected (modeled) Value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	412	45	36	28	3.93	yes	85.96
t=2 by t+1=3	453	20	33	10	1.46	yes	85.43
t=3 by t+1=4	471	18	25	11	0.96	yes	91.31
t=4 by t+1=5	442	15	45	4	1.53	yes	61.82
t=5 by t+1=6	407	22	26	13	1.41	yes	89.19
t=6 by t+1=7	289	7	21	5	0.51	yes	89.83
All t by (t+1)	**2474**	**127**	**186**	**71**	**9.79**	**yes**	**86.22**

Total ignorable	*(127+186+10)=323*
% of nonresponse that is ignorable	*(127+186+10)/(127+186+71)=84.11%*

Waves (t by t+1)	Type A Nonresponses at both t and t+1	Never interviewed again	Interviewed at least once	Percent Returned to be Interviewed
t=1 by t+1=2	28	15	13	46.4
t=2 by t+1=3	10	5	5	50.0
t=3 by t+1=4	11	5	6	54.5
t=4 by t+1=5	4	3	1	25.0
t=5 by t+1=6	13	7	6	46.2
All t by (t+1)	**66**	**35**	**31**	**47.0**

Table B2.7: Capture – Recapture Analysis of the NCVS Non-Responses Among 2003 Cohort 1 Household Respondents Who Were 26 or Older

Waves (t by t+1)	Interviewed at Both Waves	Interviewed at Wave t but not Wave t+1	Interviewed at Wave t+1 but not t	Type A Nonresponses at Both t and t+1	Expected (modeled) Value of Type A Nonresponse	Actual > Modeled?	Percent Difference
t=1 by t+1=2	5744	249	268	141	11.62	yes	91.76
t=2 by t+1=3	5681	165	256	82	7.44	yes	90.93
t=3 by t+1=4	5671	153	263	86	7.10	yes	91.75
t=4 by t+1=5	5243	123	284	91	6.66	yes	92.68
t=5 by t+1=6	4843	171	225	73	7.94	yes	89.12
t=6 by t+1=7	3357	104	135	46	4.18	yes	90.91
All t by (t+1)	**30539**	**965**	**1431**	**519**	**44.94**	**yes**	**91.34**

Total ignorable	*(965+1431+45)=2441*
% of nonresponse that is ignorable	*(965+1431+45)/(965+1431+519)=83.74%*

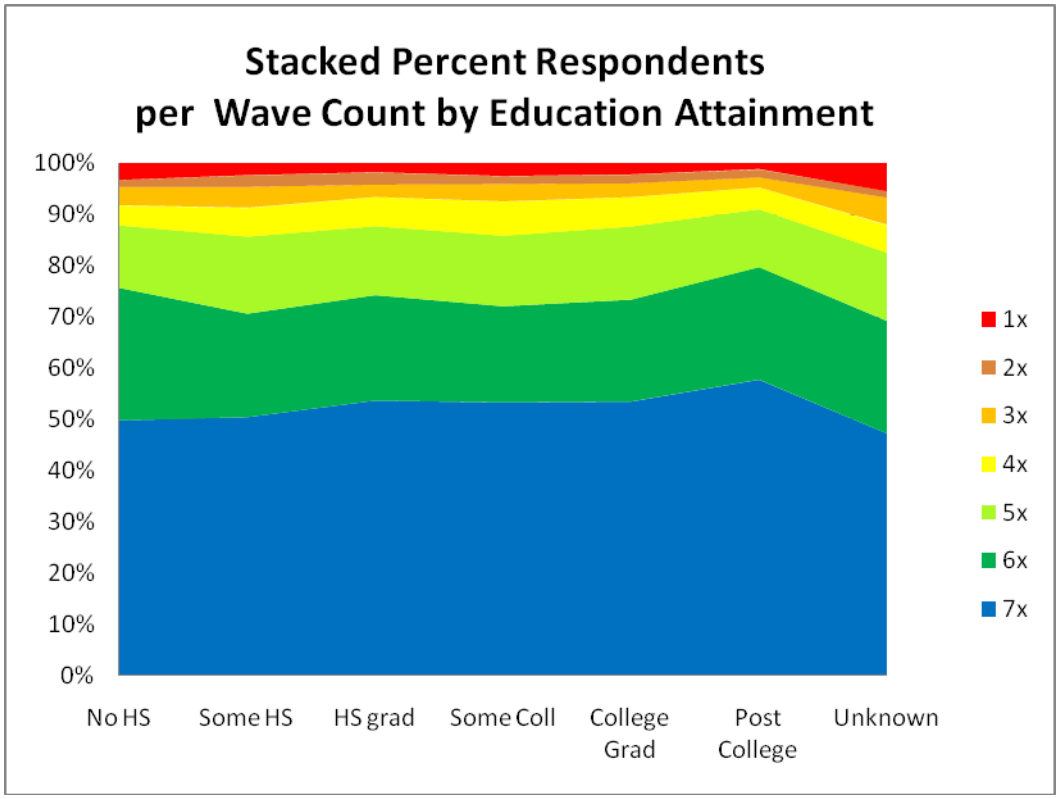
Waves (t by t+1)	Type A Nonresponses at Both t and t+1	Never Interviewed Again	Interviewed at Least Once	Percent Returned to be Interviewed
t=1 by t+1=2	141	58	83	58.9
t=2 by t+1=3	82	34	48	58.5
t=3 by t+1=4	86	43	43	50.0
t=4 by t+1=5	91	58	33	36.3
t=5 by t+1=6	73	63	10	13.7
All t by (t+1)	**473**	**256**	**217**	**45.9**

Appendix C

Table C.3. 1: Data Level, Analyses Performed and Data Used

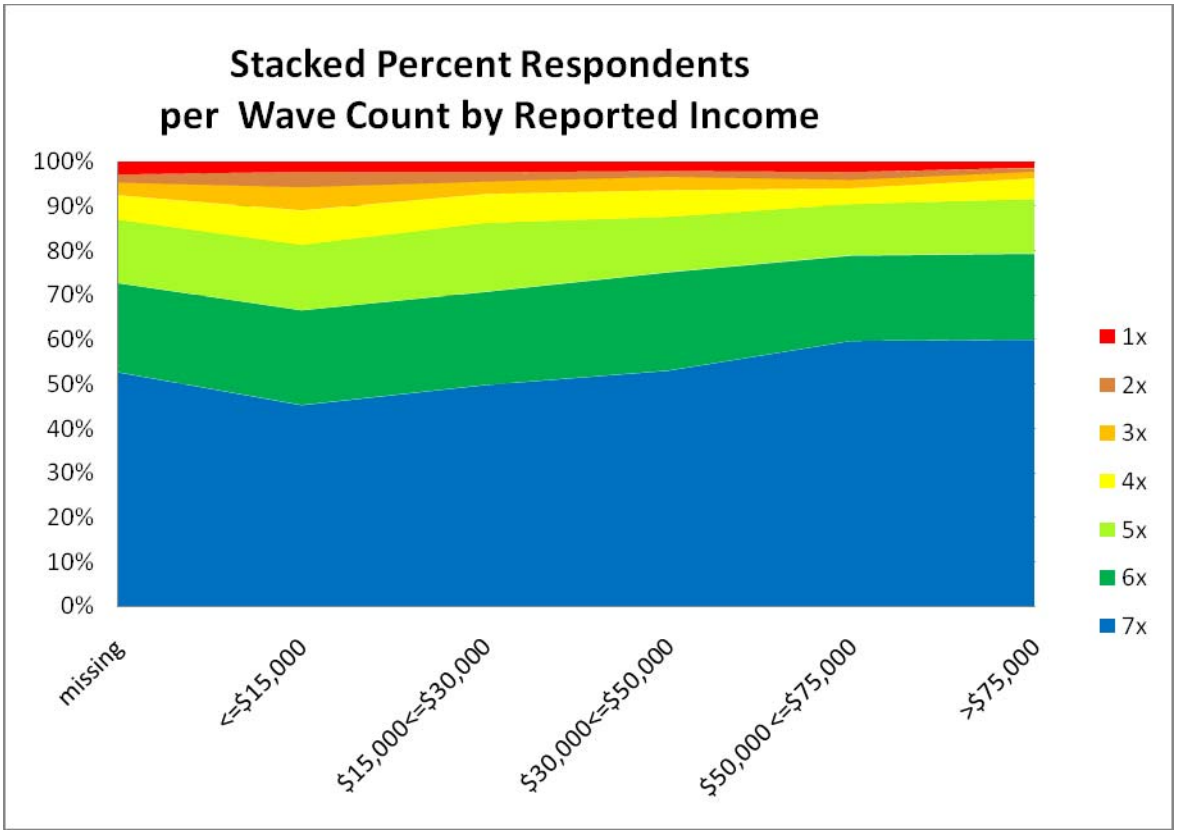
Level	Basic Analyses	Data Used
All Sample Households	Disposition by geographic detail	Public Use Files: 2002, 2007
All Contacted Households	Type A Nonresponse and response by geographic detail and Race	Public Use Files: 2002, 2007
Interviewed Households for Annual Analyses	Response to collection year by geographic, socio-demographic and victimization responses	Public Use Files: 2002, 2007
Interviewed Individuals for Annual Analyses	Response to collection year by socio-demographic and crime incidents	Public Use Files: 2002, 2007
Interviewed Cohort Households	1) Wave Response to collection year by socio-demographic and crime incidents 2) Logistic Model predicting chronicle responders 3) Factors reflecting differential nonresponse	Linked Household Public Use Files: 2003 - 2006: Internal Created File 2002-2005
Interviewed Cohort Individuals	Wave Response to collection year by socio-demographic and crime incidents	Linked Individual Public Use Files: 2003 – 2006

Chart C.3.1: Percent of Respondents in Education Attainment Group by Count of Waves Responding



Total Respondent Count by Education Category						
No HS	**Some HS**	**HS grad**	**Some Coll**	**College Grad**	**Post College**	**Unknown**
439	483	2095	1596	1227	896	224

Chart C.3.2: Percent of Respondents by Household Income Group by Count of Waves Responding



Total Respondent Count by Household Income Category					
missing	**<=$15,000**	**$15,000 <=$30,000**	**$30,000 <=$50,000**	**$50,000 <=$75,000**	**>$75,000**
1942	848	1054	1129	880	1107

Chart C.3.3: Percent of Respondents by Education Attainment Group by Count of Waves Responding

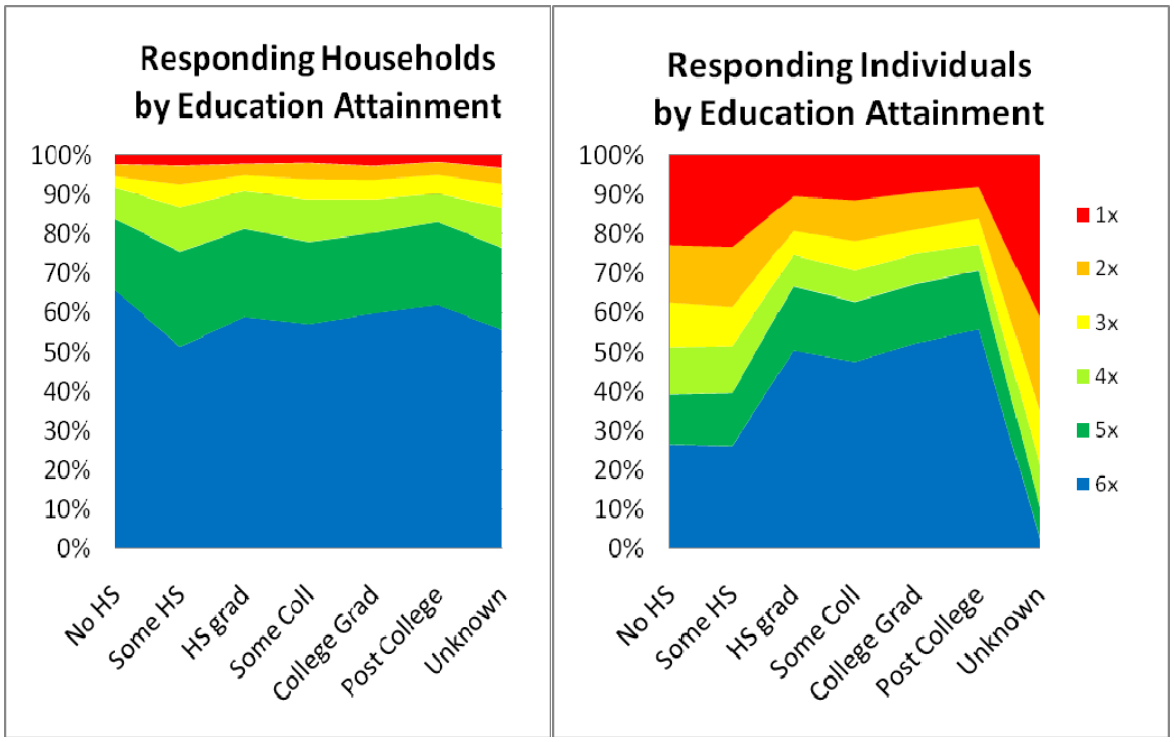


Chart C.3.4: Percent of Respondents by Hispanic Origin by Count of Waves

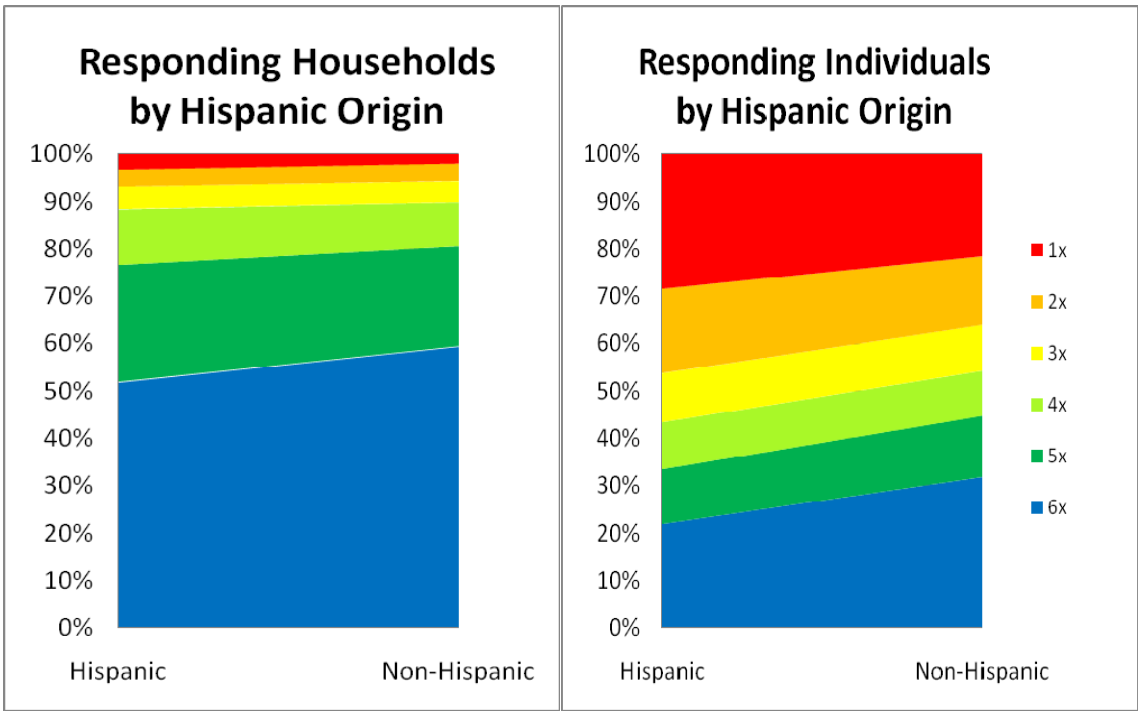
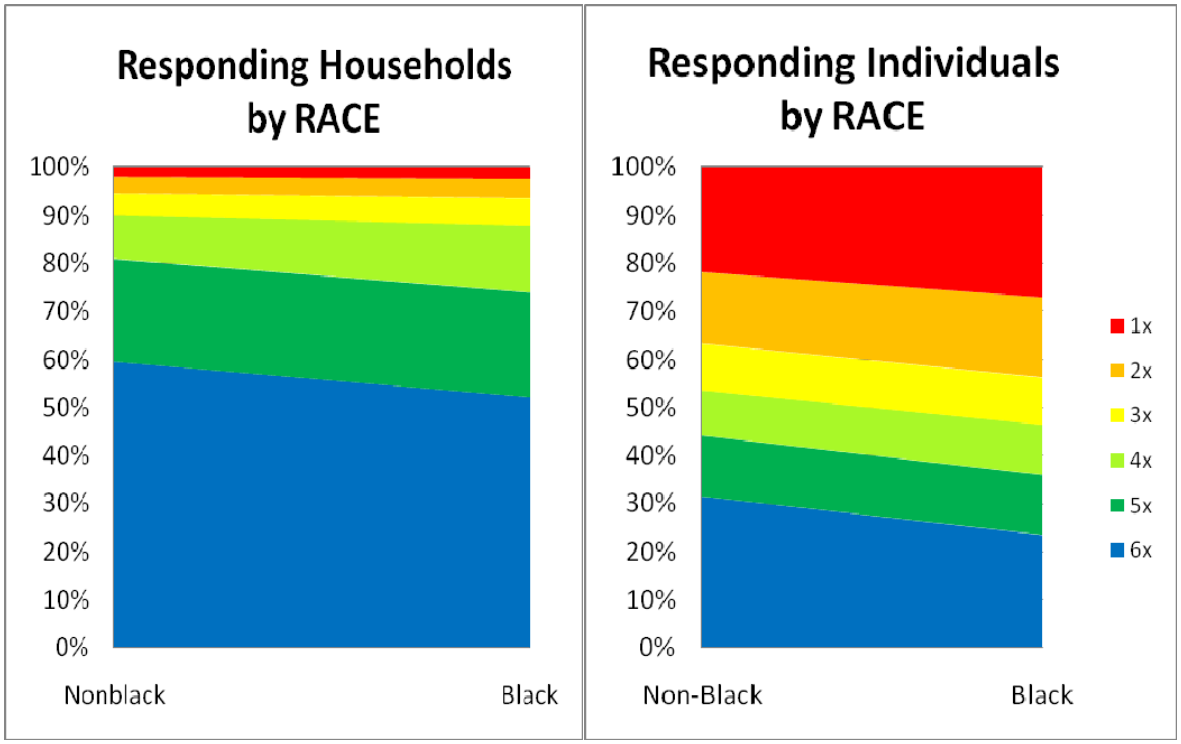


Chart C.3.5: Percent of Respondents by Race by Count of Waves



Tables C.3.2 – C.3.4: Household Logistic Regression Results Modeling Response to 6 or more Waves of Survey

Tables C.3.2: Response Profile

Response Profile	
Response to 6 or More Waves	**Frequency**
1	5147
0	1813

Tables C.3.3: Results Table

	Estimate	Std Error	Wald Chi-Sqr	PR > ChiSq
Intercept	-0.6873	0.1242	30.6106	<.0001
Age Bounded (20,50)	0.0244	0.00265	84.7242	<.0001
Hispanic Origin	0.3618	0.0968	13.9637	0.0002
Married	0.2291	0.0656	12.1943	0.0005
Post College	0.259	0.0924	7.8659	0.005
Rank of HH Income	0.0429	0.017	6.3592	0.0117
Black*Incidence Reported	-0.4826	0.2021	5.6998	0.017
Midwest	0.2367	0.0689	11.8113	0.0006
Homeowner	0.6351	0.0658	93.2682	<.0001
Family Structure = Male w/others	-0.2278	0.0811	7.8839	0.005

Tables C.3.2: Association of Predicted Probabilities and Observed Responses

% Concordant	66	Somers' D	0.328
% Discordant	33.2	Gamma	0.331
Pairs	9331511	c	0.664

Tables C.3.5 – C.3.7: Household Logistic Regression Results Modeling Drop Outs (Responded to 3 or Less Waves of Survey)

Tables C.3.5: Response Profile

Response Profile	
Drop Out	**Frequency**
1	485
0	6475

Tables C.3.6: Results Table

	Estimate	Std Error	Wald Chi-Sqr	PR > ChiSq
Intercept	-1.4041	0.1879	55.85	<.0001
Age Bounded (20,50)	-0.0177	0.00435	16.62	<.0001
Married	-0.563	0.1082	27.09	<.0001
Rank of HH Income	-0.0795	0.0295	7.25	0.0071
black*Incidence Reported	0.9018	0.2581	12.21	0.0005
Asian*Married	0.8144	0.2875	8.02	0.0046
Rural	0.4311	0.1151	14.02	0.0002
South	0.2574	0.098	6.89	0.0086
Homeowner	-0.4362	0.1118	15.22	<.0001

Tables C.3.7: Association of Predicted Probabilities and Observed Responses

% Concordant	65.3	Somers' D	0.323
% Discordant	33.2	Gamma	0.328
Pairs	3140375	c	0.662

Table C.3.8: Disposition Results for Sampled Households: Year 2002 and Year 2007

		Year 2002	Year 2007	Year 2002	Year 2007
		Overall	Overall	Sample	Sample
Type A	Duplicate (2000 sample design only)	0.00%	0.01%	0	13
	Language problems	0.00%	0.06%	0	63
	No one home	1.78%	2.12%	1902	2343
	Temporarily absent	0.53%	0.29%	568	321
	Refused	3.81%	4.64%	4070	5127
	Other occupied	0.42%	0.91%	444	1002
Type B	Temporarily occupied by persons with usual residence elsewhere	1.21%	1.33%	1290	1468
	Vacant-regular	10.04%	11.75%	10716	12969
	Vacant-storage of household furniture	0.79%	1.10%	839	1219
	Unfit or to be demolished	0.43%	0.46%	460	505
	Under construction, not ready	0.44%	0.62%	465	684
	Converted to temporary business or storage	0.25%	0.14%	264	160
	Unoccupied site for mobile home, trailer, or tent	0.34%	0.39%	360	436
	Permit granted, construction not started	0.07%	0.07%	73	79
	Other	0.26%	0.28%	274	310
Type C	Demolished	0.10%	0.23%	103	250
	House or trailer moved	0.07%	0.11%	70	126
	Outside segment	0.01%	0.01%	6	9
	Converted to permanent business or storage	0.04%	0.08%	45	83
	Merged	0.03%	0.05%	33	55
	Condemned	0.01%	0.02%	8	18
	Built after April 1, 2000	0.01%	0.02%	11	18
	Unused line of listing sheet	0.02%	0.09%	22	95
	Permit abandoned or other	0.07%	0.12%	71	134
	Removed during subsampling	0.00%	0.00%	0	0
	Unit already had a chance of selection	0.00%	0.01%	0	15
	Interviewed household	79.31%	75.09%	84682	82905
	Overall Total	100.00%	100.00%		

Chart C.3.6: Percent Responding Households by Education, 2002

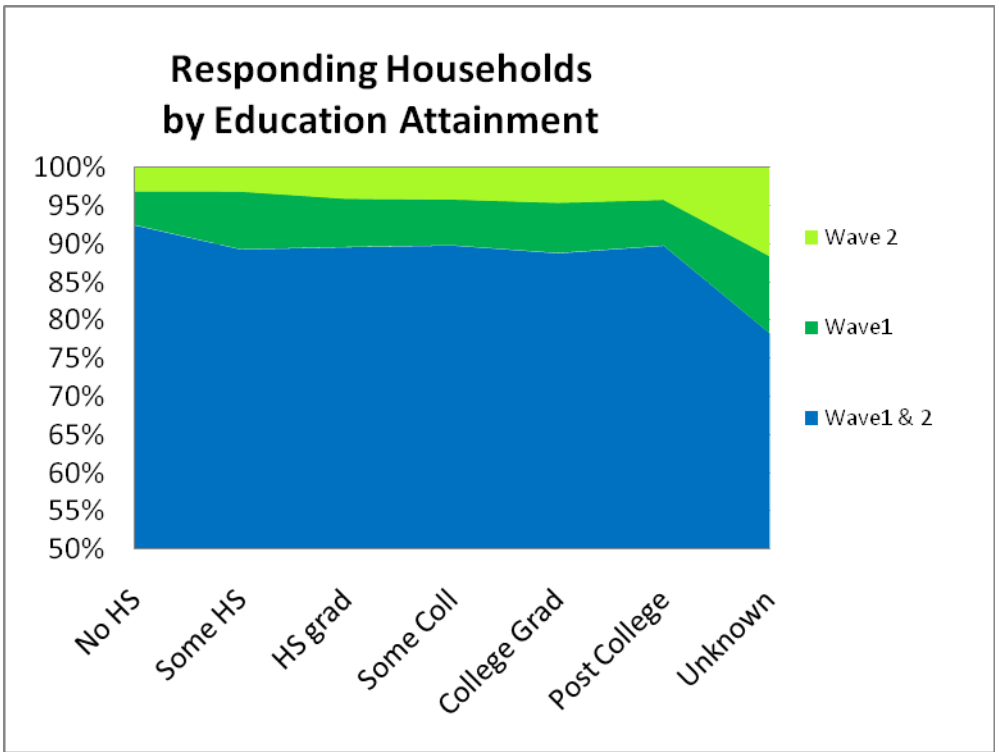


Table C.3.10: Logistic Regression: Year 2002 – Predicting Household Non-Response to One of the First Two Interviews. (Respondents = 29,381, Nonrespondents = 3,492)

Analysis of Maximum Likelihood Estimates					
Parameter	**DF**	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**
Intercept	1	-1.3188	0.0890	219.5131	<.0001
Black	1	0.2448	0.0520	22.1376	<.0001
Age	1	-0.0165	0.00211	60.9230	<.0001
Age > 60	1	-0.1808	0.0557	10.5345	0.0012
No High School	1	-0.3449	0.0912	14.3111	0.0002
Some College	1	-0.1261	0.0440	8.2266	0.0041
Unknown Household Income	1	0.3350	0.0395	71.8170	<.0001
Midwest	1	-0.2213	0.0453	23.8861	<.0001
Homeowner	1	-0.3518	0.0411	73.2911	<.0001
Family Structure: male with others	1	0.4922	0.0508	93.8105	<.0001
Family Structure: female with relatives	1	0.1696	0.0590	8.2728	0.0040
Family Structure: female with others	1	0.3724	0.0508	53.7224	<.0001
Not MSA	1	-0.1875	0.0576	10.5904	0.0011

Association of Predicted Probabilities and Observed Responses			
Percent Concordant	62.5	**Somers' D**	0.264
Percent Discordant	36.1	**Gamma**	0.268
Percent Tied	1.5	**Tau-a**	0.050
Pairs	102598452	**c**	0.632

Table C.3.11: Logistic Regression – Year 2007 Predicting Household Non-Response to One of the First Two Interviews (Respondents = 28,378 Nonrespondents = 4,799)

Analysis of Maximum Likelihood Estimates					
Parameter	**DF**	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**
Intercept	1	-0.2155	0.1027	4.3987	0.0360
Age	1	-0.0297	0.00194	232.8924	<.0001
Young: Age 25-35	1	-0.1188	0.0478	6.1910	0.0128
Hispanic Origin	1	-0.2638	0.0630	17.5553	<.0001
Married	1	-0.2248	0.0501	20.1593	<.0001
Widowed	1	-0.4027	0.0694	33.6292	<.0001
Never married	1	-0.1910	0.0517	13.6182	0.0002
Unknown Household Income	1	0.8210	0.0362	515.5699	<.0001
Household Income < $20,000	1	0.1478	0.0490	9.0893	0.0026
Number of Crime Incidents	1	0.1173	0.0301	15.2213	<.0001
Hispanic Origin, south & urban	1	0.4626	0.0965	23.0001	<.0001
Black, Midwest, & urban	1	0.3989	0.1034	14.8838	0.0001
Midwest	1	-0.2162	0.0426	25.7871	<.0001
Homeowner	1	-0.6664	0.0374	317.6972	<.0001
Family Structure: Male with others	1	0.2840	0.0503	31.8971	<.0001
Family Structure: female with others	1	0.1657	0.0524	9.9810	0.0016

Association of Predicted Probabilities and Observed Responses			
Percent Concordant	68.7	**Somers' D**	0.386
Percent Discordant	30.2	**Gamma**	0.390
Percent Tied	1.1	**Tau-a**	0.095
Pairs	136186022	**c**	0.693

Table C.3.12: Proportion of Individuals Reporting at Least One Crime Incidence, Using Data from the Public Use File, at the Person Level, from Quarters 3 and 4 of 2002 and Quarters 1 and 2 of 2003

	Missing Imputed	Percent Difference: (imputed - respondents)/respondents	Respondents Only
Overall	0.0438	-10	0.0486
Nonblack	0.0426	-12	0.0483
Black	0.0525	0	0.0526
ratio	1.2329		
Female	0.0448	7	0.0420
Male	0.0429	-18	0.0523
	0.9569		
Hispanic Origin	0.0492	-2	0.0502
Non-Hispanic	0.0431	-11	0.0485
	0.8769		
Age LT 25	0.0637	0	0.0637
Age 25-35	0.0522	10	0.0474
Age 35-45	0.0445	1	0.0442
Age 45-55	0.0396	1	0.0391
Age 55-65	0.0283	1	0.0281
Age GT 65	0.0145	-2	0.0148
Black - Age LT 25	0.0678	-6	0.0725
Nonblack - Age LT 25	0.0629	-1	0.0633
Black - Age 25-35	0.0598	21	0.0492
Nonblack - Age 25-35	0.0510	8	0.0472
Black - Age 35-45	0.0436	-2	0.0444
Nonblack - Age 35-45	0.0505	18	0.0428
Black - Age 45-55	0.0429	11	0.0384
Nonblack - Age 45-55	0.0392	-12	0.0444
Black - Age 55-65	0.0393	-11	0.0443
Nonblack - Age 55-65	0.0271	2	0.0265
Black - Age GT 65	0.0204	-11	0.0230
Nonblack - Age GT 65	0.0139	-1	0.0141

Table C.3.13: Logistic Regression – Year 2002 Predicting Individual Non-Response to Wave 3 Given Response to Wave 2 (Respondents = 12,205 Nonrespondents = 2336)

Analysis of Maximum Likelihood Estimates					
Parameter	**DF**	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**
Intercept	1	-2.2967	0.0502	2092.8849	<.0001
Age < 25	1	0.9020	0.0611	218.1104	<.0001
Age 25 to 35	1	0.8840	0.0644	188.1504	<.0001
Age 35 to 45	1	0.3051	0.0686	19.7707	<.0001
Black	1	0.1711	0.0670	6.5207	0.0107
High School Graduate	1	0.2737	0.0540	25.6782	<.0001
Some College	1	0.2753	0.0592	21.6153	<.0001
Crime Incidence for Age < 25	1	0.7326	0.1462	25.1125	<.0001

Association of Predicted Probabilities and Observed Responses			
Percent Concordant	57.7	**Somers' D**	0.249
Percent Discordant	32.8	**Gamma**	0.275
Percent Tied	9.5	**Tau-a**	0.067
Pairs	28510880	**c**	0.625

Table C.3.14: Logistic Regression – Year 2007 Predicting Household Non-Response to Wave 2 Given Response to Wave 1 (Unbounded) (Respondents = 4,658 Nonrespondents = 1689)

Analysis of Maximum Likelihood Estimates					
Parameter	**DF**	**Estimate**	**Standard Error**	**Wald Chi-Square**	**Pr > ChiSq**
Intercept	1	-1.1039	0.0839	173.1195	<.0001
Crime Incidence	1	0.2881	0.0832	11.9810	0.0005
Household Income Unknown	1	0.1948	0.0699	7.7768	0.0053
Income < $20,000	1	0.2801	0.0776	13.0263	0.0003
Black & Rural	1	-1.2075	0.4807	6.3109	0.0120
Homeowner	1	-0.5070	0.0680	55.5369	<.0001
Family Structure: Male with others	1	0.2458	0.0780	9.9462	0.0016
Central City	1	0.1691	0.0618	7.4826	0.0062
Age < 25	1	0.7695	0.1074	51.3658	<.0001
Age 25 - 35	1	0.3816	0.0810	22.1803	<.0001
Age 35 - 45	1	0.2463	0.0775	10.1025	0.0015

Odds Ratio Estimates			
Association of Predicted Probabilities and Observed Responses			
Percent Concordant	60.8	**Somers' D**	0.253
Percent Discordant	35.5	**Gamma**	0.263
Percent Tied	3.7	**Tau-a**	0.099
Pairs	7867362	**c**	0.627

Appendix D

Table D.3.1a: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, Northeast

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
Northeast	Not in a place	Nonblack	5271	4860	411	8%	0.0533
Northeast	Not in a place	Black	151	133	18	12%	0.0827
Northeast	Under 10,000	Nonblack	2599	2402	197	8%	0.0745
Northeast	Under 10,000	Black	89	76	13	15%	0.1053
Northeast	10,000-49,999	Nonblack	3525	3168	357	10%	0.0688
Northeast	10,000-49,999	Black	284	249	35	12%	0.0522
Northeast	50,000-99,999	Nonblack	1423	1277	146	10%	0.0744
Northeast	50,000-99,999	Black	176	156	20	11%	0.1218
Northeast	100,000-249,999	Nonblack	732	661	71	10%	0.1074
Northeast	100,000-249,999	Black	213	184	29	14%	0.0924
Northeast	250,000-499,999	Nonblack	229	192	37	16%	0.1198
Northeast	250,000-499,999	Black	138	108	30	22%	0.1389
Northeast	500,000-999,999	Nonblack	172	149	23	13%	0.0671
Northeast	500,000-999,999	Black	60	44	16	27%	0.0909
Northeast	1,000,000-2,499,999	Nonblack	341	324	17	5%	0.0957
Northeast	1,000,000-2,499,999	Black	225	214	11	5%	0.0888
Northeast	2,500,000-4,999,999	Nonblack	2033	1700	333	16%	0.0547
Northeast	2,500,000-4,999,999	Black	727	586	141	19%	0.0546

Table D.3.1b: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, Midwest

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
Midwest	Not in a place	Nonblack	4980	4731	249	5%	0.0668
Midwest	Not in a place	Black	156	137	19	12%	0.0876
Midwest	Under 10,000	Nonblack	3503	3325	178	5%	0.0695
Midwest	Under 10,000	Black	135	131	4	3%	0.1069
Midwest	10,000-49,999	Nonblack	5127	4854	273	5%	0.0806
Midwest	10,000-49,999	Black	345	326	19	6%	0.0706
Midwest	50,000-99,999	Nonblack	1798	1710	88	5%	0.0906
Midwest	50,000-99,999	Black	199	179	20	10%	0.0670
Midwest	100,000-249,999	Nonblack	1363	1283	80	6%	0.1200
Midwest	100,000-249,999	Black	201	188	13	6%	0.1223
Midwest	250,000-499,999	Nonblack	758	703	55	7%	0.1238
Midwest	250,000-499,999	Black	208	192	16	8%	0.1667
Midwest	500,000-999,999	Nonblack	686	636	50	7%	0.1132
Midwest	500,000-999,999	Black	332	307	25	8%	0.1140
Midwest	1,000,000-2,499,999	Nonblack	59	55	4	7%	0.1636
Midwest	1,000,000-2,499,999	Black	260	253	7	3%	0.1502
Midwest	2,500,000-4,999,999	Nonblack	641	542	99	15%	0.1070
Midwest	2,500,000-4,999,999	Black	326	277	49	15%	0.1516

Table D.3.1c: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, South

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
South	Not in a place	Nonblack	9913	9326	587	6%	0.0655
South	Not in a place	Black	1491	1400	91	6%	0.0543
South	Under 10,000	Nonblack	3657	3457	200	5%	0.0793
South	Under 10,000	Black	846	801	45	5%	0.0612
South	10,000-49,999	Nonblack	5362	5074	288	5%	0.0877
South	10,000-49,999	Black	1006	949	57	6%	0.0790
South	50,000-99,999	Nonblack	1440	1337	103	7%	0.1174
South	50,000-99,999	Black	406	376	30	7%	0.0851
South	100,000-249,999	Nonblack	2144	1998	146	7%	0.1091
South	100,000-249,999	Black	633	588	45	7%	0.0986
South	250,000-499,999	Nonblack	1856	1716	140	8%	0.1200
South	250,000-499,999	Black	652	593	59	9%	0.0877
South	500,000-999,999	Nonblack	831	750	81	10%	0.1053
South	500,000-999,999	Black	545	492	53	10%	0.0915
South	1,000,000-2,499,999	Nonblack	807	747	60	7%	0.1406
South	1,000,000-2,499,999	Black	280	264	16	6%	0.1288

Table D.3.1d: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, West

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
West	Not in a place	Nonblack	2993	2773	220	7%	0.0909
West	Not in a place	Black	53	46	7	13%	0.1087
West	Under 10,000	Nonblack	1749	1619	130	7%	0.1075
West	10,000-49,999	Nonblack	4474	4114	360	8%	0.1060
West	10,000-49,999	Black	171	142	29	17%	0.1408
West	50,000-99,999	Nonblack	2851	2600	251	9%	0.1062
West	50,000-99,999	Black	143	121	22	15%	0.2231
West	100,000-249,999	Nonblack	2457	2234	223	9%	0.1280
West	100,000-249,999	Black	214	187	27	13%	0.1176
West	250,000-499,999	Nonblack	1866	1657	209	11%	0.1298
West	250,000-499,999	Black	183	154	29	16%	0.1623
West	500,000-999,999	Nonblack	1269	1165	104	8%	0.1648
West	500,000-999,999	Black	113	103	10	9%	0.1748
West	1,000,000-2,499,999	Nonblack	417	348	69	17%	0.1063
West	2,500,000-4,999,999	Nonblack	1183	1078	105	9%	0.1317
West	2,500,000-4,999,999	Black	126	104	22	17%	0.1442

Figure D.3.2: Refusal and Crime Rate by Region, Place Size and Race, Year =2007
[only groups with at least 50 households included in graph]

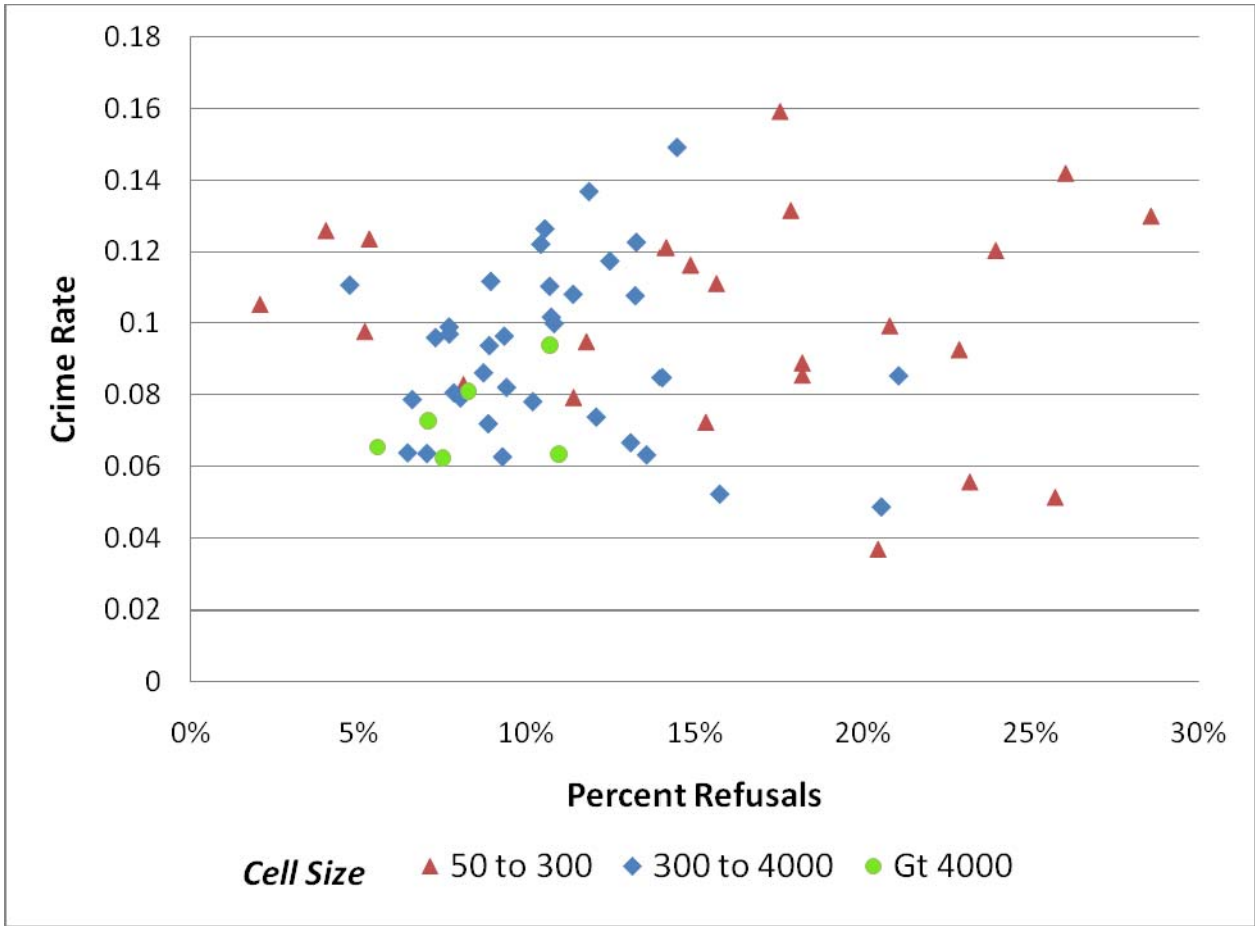


Table D.3.2a: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, Northeast

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
Northeast	Not in a place	Nonblack	5150	4585	565	11%	0.0635
Northeast	Not in a place	Black	142	108	34	24%	0.1204
Northeast	Under 10,000	Nonblack	2451	2223	228	9%	0.0625
Northeast	Under 10,000	Black	101	86	15	15%	0.1163
Northeast	10,000-49,999	Nonblack	3524	3045	479	14%	0.0631
Northeast	10,000-49,999	Black	301	261	40	13%	0.1226
Northeast	50,000-99,999	Nonblack	1212	1053	159	13%	0.0665
Northeast	50,000-99,999	Black	110	90	20	18%	0.0889
Northeast	100,000-249,999	Nonblack	587	516	71	12%	0.0736
Northeast	100,000-249,999	Black	114	101	13	11%	0.0792
Northeast	250,000-499,999	Nonblack	164	126	38	23%	0.0556
Northeast	250,000-499,999	Black	98	83	15	15%	0.0723
Northeast	500,000-999,999	Nonblack	140	100	40	29%	0.1300
Northeast	500,000-999,999	Black	48	36	12	25%	0.1111
Northeast	1,000,000-2,499,999	Nonblack	273	262	11	4%	0.1260
Northeast	1,000,000-2,499,999	Black	194	190	4	2%	0.1053
Northeast	5,000,000 or more	Nonblack	1791	1423	368	21%	0.0485
Northeast	5,000,000 or more	Black	661	557	104	16%	0.0521

Table D.3.2b: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, Midwest

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
Midwest	Not in a place	Nonblack	5951	5620	331	6%	0.0653
Midwest	Not in a place	Black	105	78	27	26%	0.0513
Midwest	Under 10,000	Nonblack	3613	3379	234	6%	0.0636
Midwest	Under 10,000	Black	143	117	26	18%	0.0855
Midwest	10,000-49,999	Nonblack	4849	4506	343	7%	0.0728
Midwest	10,000-49,999	Black	448	392	56	13%	0.1173
Midwest	50,000-99,999	Nonblack	2049	1891	158	8%	0.0989
Midwest	50,000-99,999	Black	197	181	16	8%	0.0829
Midwest	100,000-249,999	Nonblack	1284	1170	114	9%	0.0718
Midwest	100,000-249,999	Black	188	178	10	5%	0.1236
Midwest	250,000-499,999	Nonblack	564	497	67	12%	0.1368
Midwest	250,000-499,999	Black	263	232	31	12%	0.0948
Midwest	500,000-999,999	Nonblack	481	436	45	9%	0.0963
Midwest	500,000-999,999	Black	407	364	43	11%	0.1264
Midwest	2,500,000-4,999,999	Nonblack	565	446	119	21%	0.0852
Midwest	2,500,000-4,999,999	Black	219	162	57	26%	0.1420

Table D.3.2c: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, South

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
South	Not in a place	Nonblack	10241	9471	770	8%	0.0624
South	Not in a place	Black	1404	1291	113	8%	0.0790
South	Under 10,000	Nonblack	3871	3615	256	7%	0.0786
South	Under 10,000	Black	780	725	55	7%	0.0634
South	10,000-49,999	Nonblack	5316	4876	440	8%	0.0810
South	10,000-49,999	Black	1133	1044	89	8%	0.0805
South	50,000-99,999	Nonblack	2203	2033	170	8%	0.0969
South	50,000-99,999	Black	484	461	23	5%	0.1106
South	100,000-249,999	Nonblack	2152	1964	188	9%	0.0860
South	100,000-249,999	Black	797	726	71	9%	0.0937
South	250,000-499,999	Nonblack	1136	1053	83	7%	0.0959
South	250,000-499,999	Black	270	256	14	5%	0.0977
South	500,000-999,999	Nonblack	1427	1278	149	10%	0.1221
South	500,000-999,999	Black	659	600	59	9%	0.1117
South	1,000,000-2,499,999	Nonblack	1058	905	153	14%	0.1492
South	1,000,000-2,499,999	Black	274	226	48	18%	0.1593

Table D.3.2d: Refusal and Crime Rate by Region, Place Size and Race, Year =2002, West

Region	Place Size	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
West	Not in a place	Nonblack	2342	2103	239	10%	0.0780
West	Not in a place	Black	39	31	8	21%	0.1935
West	Under 10,000	Nonblack	2303	2086	217	9%	0.0820
West	Under 10,000	Black	28	21	7	25%	0.0000
West	10,000-49,999	Nonblack	4148	3704	444	11%	0.0940
West	10,000-49,999	Black	160	135	25	16%	0.1111
West	50,000-99,999	Nonblack	2315	2066	249	11%	0.1016
West	50,000-99,999	Black	178	141	37	21%	0.0993
West	100,000-249,999	Nonblack	3489	3091	398	11%	0.1081
West	100,000-249,999	Black	269	231	38	14%	0.1212
West	250,000-499,999	Nonblack	1735	1505	230	13%	0.1076
West	250,000-499,999	Black	185	152	33	18%	0.1316
West	500,000-999,999	Nonblack	1046	934	112	11%	0.1103
West	500,000-999,999	Black	70	54	16	23%	0.0926
West	1,000,000-2,499,999	Nonblack	775	691	84	11%	0.0999
West	1,000,000-2,499,999	Black	43	42	1	2%	0.1905
West	2,500,000-4,999,999	Nonblack	920	791	129	14%	0.0847
West	2,500,000-4,999,999	Black	137	109	28	20%	0.0367

Figure D.3.3 – Refusal and Crime Rate by Region, MSA Status, Land Use and Race, Year =2002 [only groups with at least 50 households included in graph]

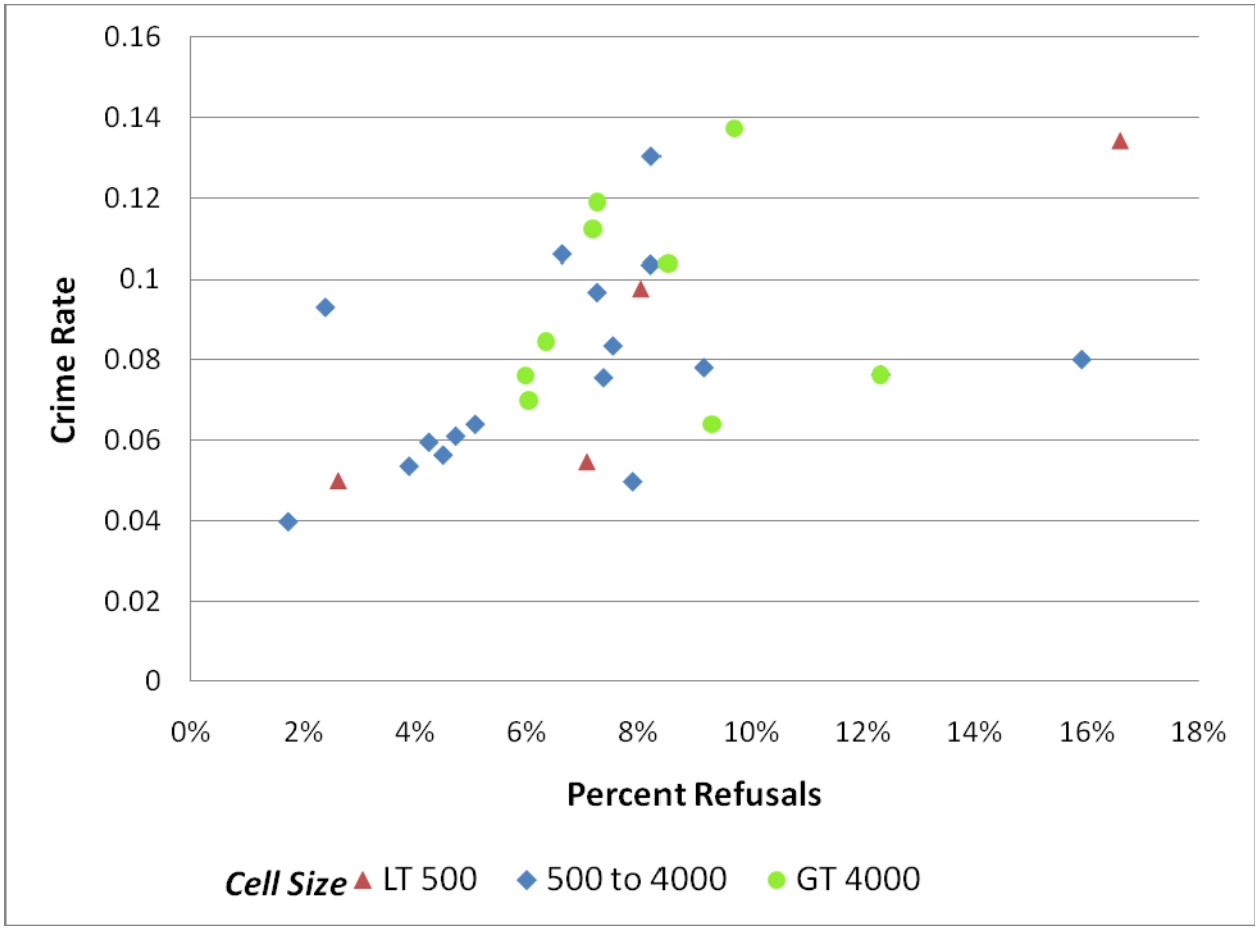


Table D.3.3a: Refusal and Crime Rate by Region, MSA Status, Land Use and Race, Year =2002, Northeast & Midwest

Region	MSA Status	Land Use	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
Northeast	Central city	Urban	Nonblack	4451	3903	548	12%	0.0764
Northeast	Central city	Urban	Black	1440	1211	229	16%	0.0801
Northeast	MSA but not central city	Urban	Nonblack	7952	7213	739	9%	0.0641
Northeast	MSA but not central city	Urban	Black	574	495	79	14%	0.0788
Northeast	MSA but not central city	Rural	Nonblack	2556	2354	202	8%	0.0497
Northeast	MSA but not central city	Rural	Black	42	37	5	12%	0.0541
Northeast	Not MSA	Urban	Nonblack	323	297	26	8%	0.0976
Northeast	Not MSA	Urban	Black	1	1	0	0%	0.0000
Northeast	Not MSA	Rural	Nonblack	1043	966	77	7%	0.0756
Northeast	Not MSA	Rural	Black	6	6	0	0%	0.0000
Midwest	Central city	Urban	Nonblack	4742	4401	341	7%	0.1122
Midwest	Central city	Urban	Black	1471	1350	121	8%	0.1304
Midwest	MSA but not central city	Urban	Nonblack	7785	7319	466	6%	0.0761
Midwest	MSA but not central city	Urban	Black	596	551	45	8%	0.0835
Midwest	MSA but not central city	Rural	Nonblack	2750	2610	140	5%	0.0640
Midwest	MSA but not central city	Rural	Black	33	30	3	9%	0.1000
Midwest	Not MSA	Urban	Nonblack	1409	1375	34	2%	0.0931
Midwest	Not MSA	Urban	Black	53	50	3	6%	0.0800
Midwest	Not MSA	Rural	Nonblack	2229	2134	95	4%	0.0595
Midwest	Not MSA	Rural	Black	9	9	0	0%	0.2222

Table D.3.3b: Refusal and Crime Rate by Region, MSA Status, Land Use and Race, Year =2002, South and West

Region	MSA Status	Land Use	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
South	Central city	Urban	Nonblack	6742	6252	490	7%	0.1190
South	Central city	Urban	Black	2519	2336	183	7%	0.0967
South	MSA but not central city	Urban	Nonblack	10160	9515	645	6%	0.0846
South	MSA but not central city	Urban	Black	1748	1588	160	9%	0.0781
South	MSA but not central city	Rural	Nonblack	4919	4622	297	6%	0.0699
South	MSA but not central city	Rural	Black	395	367	28	7%	0.0545
South	Not MSA	Urban	Nonblack	1118	1065	53	5%	0.0610
South	Not MSA	Urban	Black	454	442	12	3%	0.0498
South	Not MSA	Rural	Nonblack	3071	2951	120	4%	0.0535
South	Not MSA	Rural	Black	743	730	13	2%	0.0397
West	Central city	Urban	Nonblack	7418	6699	719	10%	0.1373
West	Central city	Urban	Black	576	498	78	14%	0.1687
West	MSA but not central city	Urban	Nonblack	8915	8156	759	9%	0.1038
West	MSA but not central city	Urban	Black	482	402	80	17%	0.1343
West	MSA but not central city	Rural	Nonblack	1168	1072	96	8%	0.1035
West	MSA but not central city	Rural	Black	8	7	1	13%	0.0000
West	Not MSA	Urban	Nonblack	828	773	55	7%	0.1061
West	Not MSA	Urban	Black	4	4	0	0%	0.5000
West	Not MSA	Rural	Nonblack	930	888	42	5%	0.0563
West	Not MSA	Rural	Black	3	3	0	0%	0.3333

Figure D.3.4: Refusal and Crime Rate by Region, MSA Status, Land Use and Race, Year =2007 [only groups with at least 50 households included in graph]

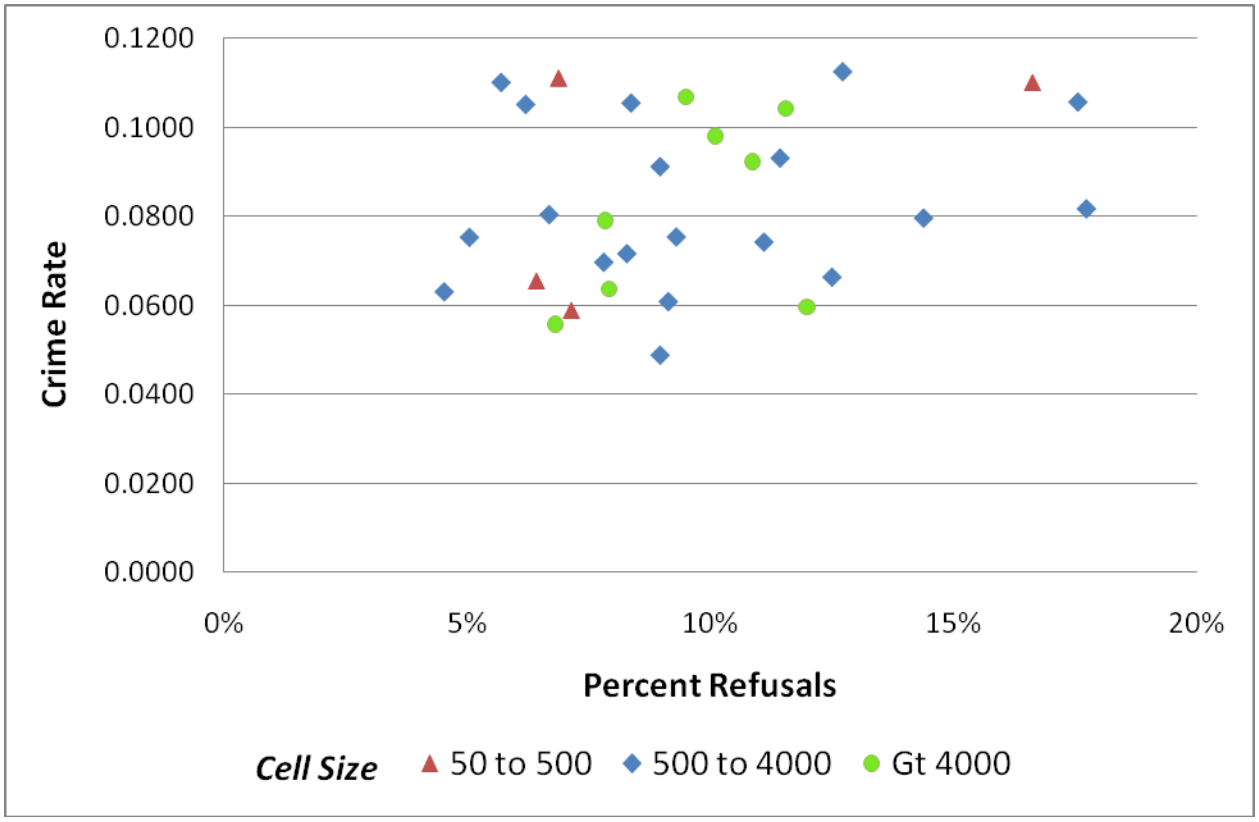


Table D.3.4a: Refusal and Crime Rate by Region, MSA Status, Land Use and Race, Year =2007, Northeast & Midwest

Region	MSA Status	Land Use	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
Northeast	Central city	Urban	Nonblack	3944	3289	655	17%	0.0696
Northeast	Central city	Urban	Black	1202	1049	153	13%	0.0753
Northeast	MSA but not central city	Urban	Nonblack	8161	7182	979	12%	0.0596
Northeast	MSA but not central city	Urban	Black	541	446	95	18%	0.1054
Northeast	MSA but not central city	Rural	Nonblack	1704	1509	195	11%	0.0663
Northeast	MSA but not central city	Rural	Black	8	3	5	63%	0.0000
Northeast	Not MSA	Urban	Nonblack	525	481	44	8%	0.0873
Northeast	Not MSA	Urban	Black	11	9	2	18%	0.2222
Northeast	Not MSA	Rural	Nonblack	958	872	86	9%	0.0608
Northeast	Not MSA	Rural	Black	7	5	2	29%	0.4000
Midwest	Central city	Urban	Nonblack	4541	4082	459	10%	0.0980
Midwest	Central city	Urban	Black	1271	1112	159	13%	0.1124
Midwest	MSA but not central city	Urban	Nonblack	7174	6605	569	8%	0.0636
Midwest	MSA but not central city	Urban	Black	542	464	78	14%	0.1056
Midwest	MSA but not central city	Rural	Nonblack	2241	2113	128	6%	0.0487
Midwest	MSA but not central city	Rural	Black	13	7	6	46%	0.1429
Midwest	Not MSA	Urban	Nonblack	1857	1763	94	5%	0.0930
Midwest	Not MSA	Urban	Black	116	108	8	7%	0.1111
Midwest	Not MSA	Rural	Nonblack	3543	3382	161	5%	0.0716
Midwest	Not MSA	Rural	Black	28	13	15	54%	0.0769

Table D.3.4b: Refusal and Crime Rate by Region, MSA Status, Land Use and Race, Year =2007, South and West

Region	MSA Status	Land Use	Race	Sample Size	Number Respondents	Number Refusals	Refusal Rate	Crime Rate
South	Central city	Urban	Nonblack	7420	6715	705	10%	0.1068
South	Central city	Urban	Black	2399	2200	199	8%	0.1100
South	MSA but not central city	Urban	Nonblack	10357	9544	813	8%	0.0790
South	MSA but not central city	Urban	Black	2038	1855	183	9%	0.0803
South	MSA but not central city	Rural	Nonblack	3669	3382	287	8%	0.0630
South	MSA but not central city	Rural	Black	475	441	34	7%	0.0590
South	Not MSA	Urban	Nonblack	1881	1755	126	7%	0.0752
South	Not MSA	Urban	Black	563	528	35	6%	0.0795
South	Not MSA	Rural	Nonblack	4077	3799	278	7%	0.0558
South	Not MSA	Rural	Black	326	305	21	6%	0.0656
West	Central city	Urban	Nonblack	8436	7461	975	12%	0.1043
West	Central city	Urban	Black	671	552	119	18%	0.1051
West	MSA but not central city	Urban	Nonblack	7606	6779	827	11%	0.0923
West	MSA but not central city	Urban	Black	403	336	67	17%	0.1101
West	MSA but not central city	Rural	Nonblack	809	735	74	9%	0.0816
West	MSA but not central city	Rural	Black	16	14	2	13%	0.2143
West	Not MSA	Urban	Nonblack	1062	944	118	11%	0.0911
West	Not MSA	Urban	Black	10	8	2	20%	0.2500
West	Not MSA	Rural	Nonblack	1160	1052	108	9%	0.0741
West	Not MSA	Rural	Black	9	6	3	33%	0.0000

Appendix E

Appendix E4.1: Crime Victimization Rates by Census Divisions, in Year 2006

Crime Victimization	ALL	Rates by Census Division									
	Count	All Divisions	New England (1)	Middle Atlantic (2)	East North Central (3)	West North Central (4)	South Atlantic (5)	East South Central (6)	West South Central (7)	Mountain (8)	Pacific (9)
Assault	3308527	1.338%	1.185%	1.099%	1.247%	0.918%	0.966%	0.905%	2.055%	1.682%	1.876%
Burglary	2433133	0.984%	0.520%	0.783%	1.177%	0.787%	0.709%	0.856%	1.484%	1.252%	1.124%
Motor Vehicle Theft	801504	0.324%	0.052%	0.142%	0.386%	0.250%	0.256%	0.204%	0.321%	0.590%	0.549%
Purse Snatching	148124	0.060%	0.097%	0.069%	0.042%	0.002%	0.035%	0.009%	0.083%	0.091%	0.107%
Rape	185893	0.075%	0.028%	0.048%	0.058%	0.039%	0.070%	0.101%	0.133%	0.120%	0.085%
Robbery	512377	0.207%	0.190%	0.180%	0.202%	0.151%	0.182%	0.053%	0.279%	0.245%	0.296%
Theft	10502638	4.248%	3.555%	3.016%	3.891%	3.248%	3.236%	2.525%	6.269%	5.832%	6.224%
All Crime	17892196	7.237%	5.626%	5.338%	7.002%	5.395%	5.454%	4.652%	10.625%	9.812%	10.262%
Total Persons	247244481		11575994	33630342	39861140	18144654	46877523	15433425	25649354	17413243	38658806

Source: NCVS, 2006

Appendix E4.2: Arrests Reported in the Uniform Crime Reports (UCR) in Counties where the National Crime Victimization Survey (NCVS) Data Were Collected, in Year 2006

Crime Arrest	All	Census Division								
		New England (1)	Middle Atlantic (2)	East North Central (3)	West North Central (4)	South Atlantic (5)	East South Central (6)	West South Central (7)	Mountain (8)	Pacific (9)
Aggravated Assaults	285453	14742	53632	25691	14297	31547	9957	31591	12783	91213
Burglaries	178766	6818	30041	18177	8227	23467	9589	20984	9493	51970
Larcenies	664528	28140	124780	86123	48684	81174	35728	95032	53070	111797
Murders	9359	226	2085	1146	396	1259	597	1158	524	1968
Motor Vehicle Thefts	95238	2012	16675	15021	5348	8158	2942	7920	7991	29171
Rapes	13826	735	2968	1602	843	1384	610	2182	846	2656
Robberies	94235	3736	24871	11833	3835	11462	4178	8535	4354	21431
Combined except Murder	1332046	56183	252967	158447	81234	157192	63004	166244	88537	308238
All Combined	1341405	56409	255052	159593	81630	158451	63601	167402	89061	310206
Total County Population Where Agencies Report Arrests	197426932	10135367	32027981	29064515	9149697	34997248	7709295	22548902	12275104	39518823

Source: UCR, 2006

Appendix E4.3: Crime Victimization Rates by Census Divisions, in Year 2005

Crime Victimization	ALL	Rates by Census Division									
	Count	All Divisions	New England (1)	Middle Atlantic (2)	East North Central (3)	West North Central (4)	South Atlantic (5)	East South Central (6)	West South Central (7)	Mountain (8)	Pacific (9)
Assault	3211231	1.313%	1.124%	1.404%	1.387%	0.707%	0.967%	0.579%	1.699%	1.820%	1.748%
Burglary	2413114	0.987%	0.894%	0.603%	1.079%	0.783%	0.848%	0.575%	1.716%	1.476%	1.015%
Motor Vehicle Theft	861993	0.353%	0.278%	0.238%	0.336%	0.225%	0.249%	0.158%	0.536%	0.582%	0.530%
Purse Snatching	165341	0.068%	0.018%	0.101%	0.059%	0.053%	0.052%	0.016%	0.160%	0.031%	0.065%
Rape	140607	0.058%	0.018%	0.080%	0.026%	0.084%	0.034%	0.024%	0.115%	0.101%	0.058%
Robbery	544742	0.223%	0.061%	0.345%	0.294%	0.151%	0.217%	0.086%	0.238%	0.293%	0.174%
Theft	10390651	4.250%	3.260%	3.450%	3.948%	2.874%	3.511%	2.077%	6.424%	6.738%	5.496%
All Crime	17727679	7.250%	5.653%	6.223%	7.129%	4.877%	5.877%	3.515%	10.889%	11.040%	9.086%
Total Persons	244504326		12917601	31587129	37522638	19583401	46137495	17626940	24590162	13091564	41447396

Source: NCVS, 2005

Appendix E4.4: Arrests Reported in the Uniform Crime Reports (UCR) in Counties where the National Crime Victimization Survey (NCVS) Data Were Collected, in Year 2005

Crime Arrest	All	Census Division								
		New England (1)	Middle Atlantic (2)	East North Central (3)	West North Central (4)	South Atlantic (5)	East South Central (6)	West South Central (7)	Mountain (8)	Pacific (9)
Aggravated Assaults	277205	11835	51281	27498	13699	33970	11379	24299	11458	91786
Burglaries	170587	6307	28133	17460	7330	24985	8199	17560	10788	49825
Larcenies	686897	25504	131468	83408	47457	94373	33244	94308	60892	116243
Murders	9155	196	1852	1281	377	1347	570	1118	463	1951
Motor Vehicle Thefts	103310	2255	17914	16795	5985	9444	3148	7684	8314	31771
Rapes	14272	635	3034	2276	1060	1662	519	1861	746	2479
Robberies	86434	3111	23740	11017	3932	11549	3834	6988	3494	18769
Combined except Murder	1338705	49647	255570	158454	79463	175983	60323	152700	95692	310873
All Combined	1347860	49843	257422	159735	79840	177330	60893	153818	96155	312824
Total County Population Where Agencies Report Arrests	191800897	10061367	32989936	27386916	8831912	35592394	6803765	20327739	11335722	38471146

Source: UCR, 2005

Appendix E4.5: List of Counties Where the UCR Crime Counts Were Zero

State FIPS Code	stcnty	U_TOT_CRIME	U_CPOPARST	N_Tot_Crime	N_Total_Persons
12	12001	0	227627	32149	680570
12	12005	0	639119	73363	2193835
12	12007	0	28592	2643	84798
12	12011	0	7112336	521811	6631430
12	12013	0	13514	1266	224326
12	12019	0	657963	28948	560490
12	12025	0	2375298	226252	3873287
12	12031	0	3336768	320539	2991557
12	12041	0	47355	4622	764319
12	12045	0	14211	1519	283774
12	12053	0	441405	6064	263190
12	12057	0	4447774	397454	3695300
12	12061	0	500985	30002	3843254
12	12071	0	2067339	218434	4198683
12	12075	0	111860	11803	1420317
12	12083	0	308560	32425	841905
12	12086	0	4833033	206209	4327658
12	12089	0	255081	4055	135314
12	12091	0	546541	22105	1600441
12	12095	0	4002656	105496	2739235
12	12097	0	864096	27350	869680
12	12099	0	5016717	316203	4368781
12	12101	0	1628891	172185	1297853
12	12103	0	3783580	382554	2892441
12	12105	0	2117942	185779	2673323
12	12109	0	604735	11950	249382
12	12115	0	1446896	256446	4206164
12	12117	0	1592307	56428	1410605
17	17007	0	50637	7867	136840
17	17023	0	51070	5483	1109103
17	17027	0	133360	2468	152920
17	17045	0	58184	6237	1521160
17	17063	0	46051	0	31053
17	17083	0	88108	4524	76068
17	17093	0	257714	10687	421130
17	17097	0	710486	65361	618147
17	17111	0	1220427	106435	959169

Appendix E4.5: List of Counties Where the UCR Crime Counts Were Zero

State FIPS Code	stcnty	U_TOT_CRIME	U_CPOPARST	N_Tot_Crime	N_Total_Persons
17	17115	0	110759	21696	777064
17	17117	0	49769	11658	71664
17	17119	0	1068433	116289	1000259
17	17133	0	121077	7829	124619
17	17163	0	1027004	140007	882645
17	17179	0	517644	15427	847377
17	17203	0	147407	2522	214663
27	27101	0	8911	79	217353
51	51780	0	0	1418	285360

Appendix E4.6: List of Counties Where the NCVS Crime Counts Were Zero

State FIPS Code	stcnty	U_TOT_CRIME	U_CPOPARST	N_Tot_Crime	N_Total_Persons
1	1007	85	21662	0	21858
8	8039	20	23219	0	24285
8	8047	68	5025	0	13157
8	8093	29	17269	0	0
13	13015	619	92095	0	84969
13	13083	205	48449	0	35402
13	13227	245	29705	0	6597
17	17063	0	46051	0	31053
18	18013	14	15254	0	17316
19	19085	20	15968	0	30999
19	19129	53	15365	0	27260
21	21077	49	8198	0	9371
21	21211	740	108950	0	68035
22	22037	185	19737	0	9651
22	22047	301	30697	0	32487
22	22075	134	27483	0	0
22	22091	83	9724	0	10734
26	26037	84	68387	0	124808
27	27077	4	4451	0	155718
28	28093	276	35530	0	23021
28	28137	134	26452	0	17129
29	29049	86	25421	0	13510
29	29177	491	96439	0	112691

Appendix E4.6: List of Counties Where the NCVS Crime Counts Were Zero

State FIPS Code	stcnty	U_TOT_CRIME	U_CPOPARST	N_Tot_Crime	N_Total_Persons
29	29219	193	28939	0	24272
31	31025	18	25874	0	22980
32	32023	378	41830	0	55396
35	35057	83	17739	0	18570
36	36041	52	15826	0	167985
37	37053	139	23573	0	7490
40	40117	49	17004	0	0
42	42025	815	181474	0	128450
47	47047	148	34896	0	17436
47	47057	289	65091	0	43104
47	47173	51	19319	0	14568
48	48019	53	20554	0	20324
48	48055	128	36502	0	15982
48	48221	187	49288	0	44933
48	48259	137	28482	0	11254
48	48325	195	43880	0	33614
48	48473	705	138583	0	31630
49	49043	186	36142	0	2797
51	51033	112	25818	0	12288
51	51075	177	74641	0	86437
51	51093	89	33750	0	11502
51	51099	67	20843	0	14680
51	51101	55	14879	0	15541
51	51109	50	30307	0	2523
51	51145	28	26863	0	32010
51	51181	37	7083	0	11340
51	51187	93	35910	0	9206
51	51630	260	20939	0	5476
51	51683	394	113456	0	22192
51	51830	212	35192	0	36913
55	55047	97	19330	0	118133
55	55093	216	41853	0	21941

Appendix F

Figure F4.1: Total Counts of Crime Incidents – NCVS by UCR, in 2003 – 2006, By Region

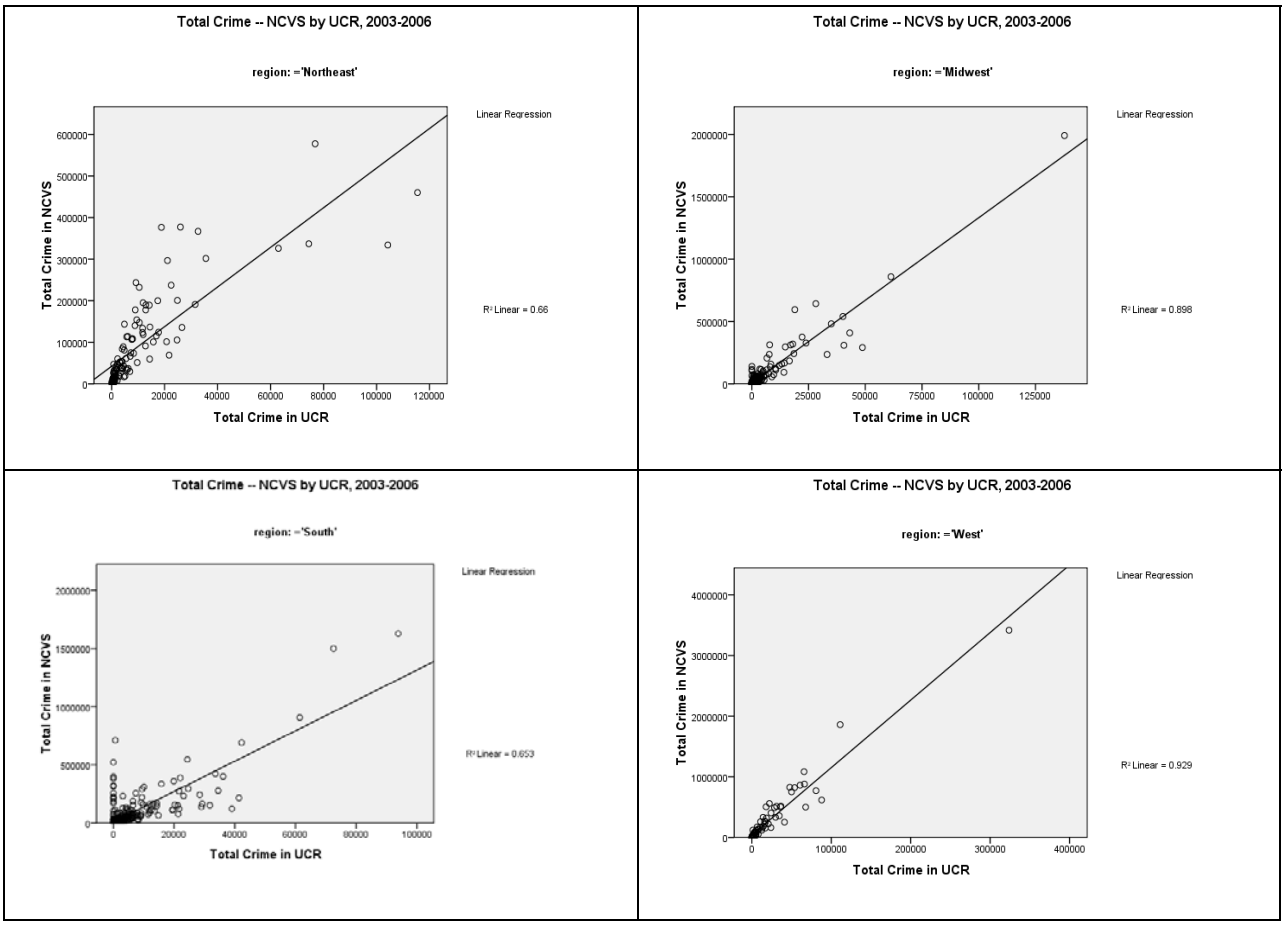


Figure F4.2: Logarithms of Total Counts of Crime Incidents–NCVS by UCR, in 2003–2006, By Region

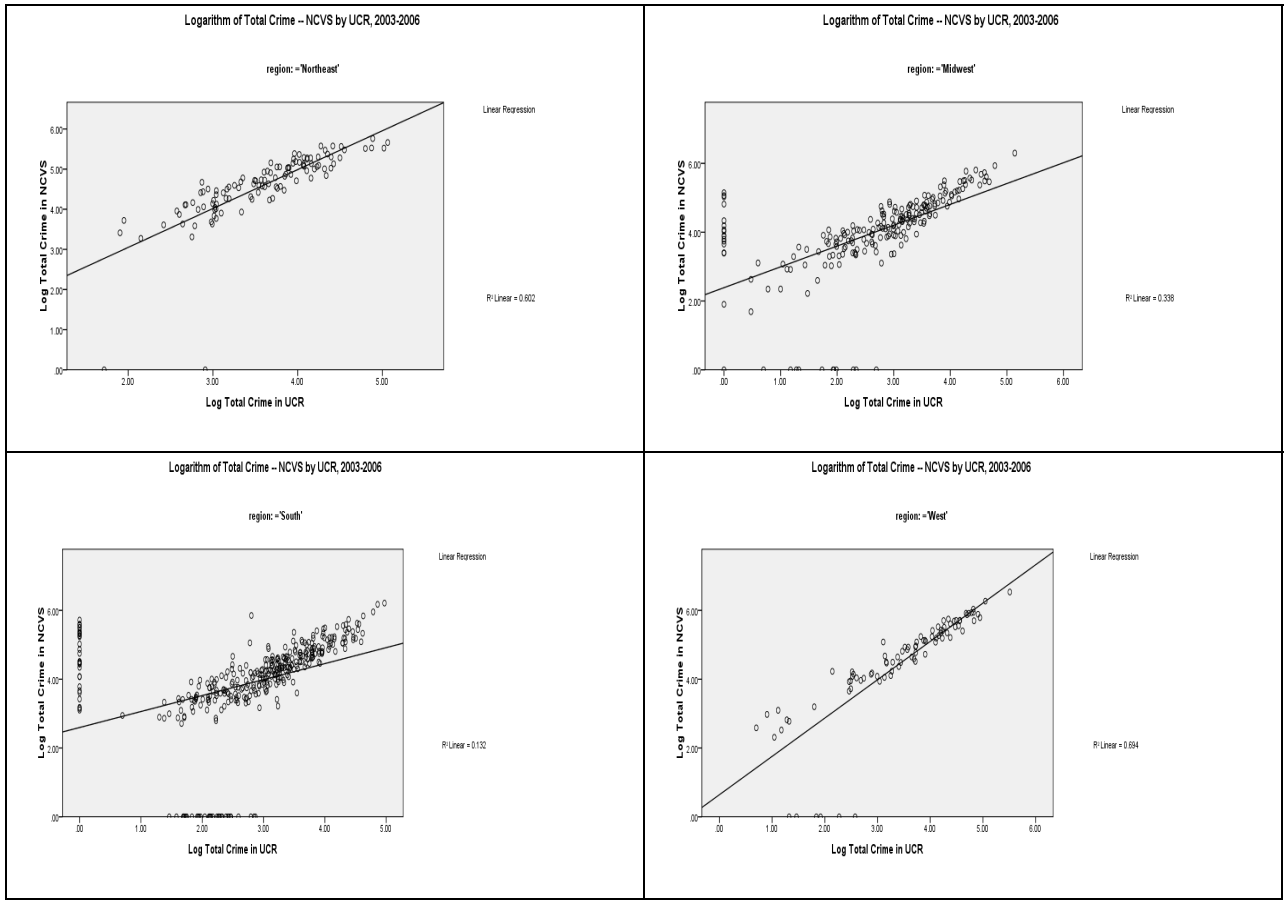


Figure F4.3: Total Counts of Crime Incidents – NCVS by UCR, in 2003 – 2006, by Region
Excluding counties where total victimization incident count =0 and arrest count =0

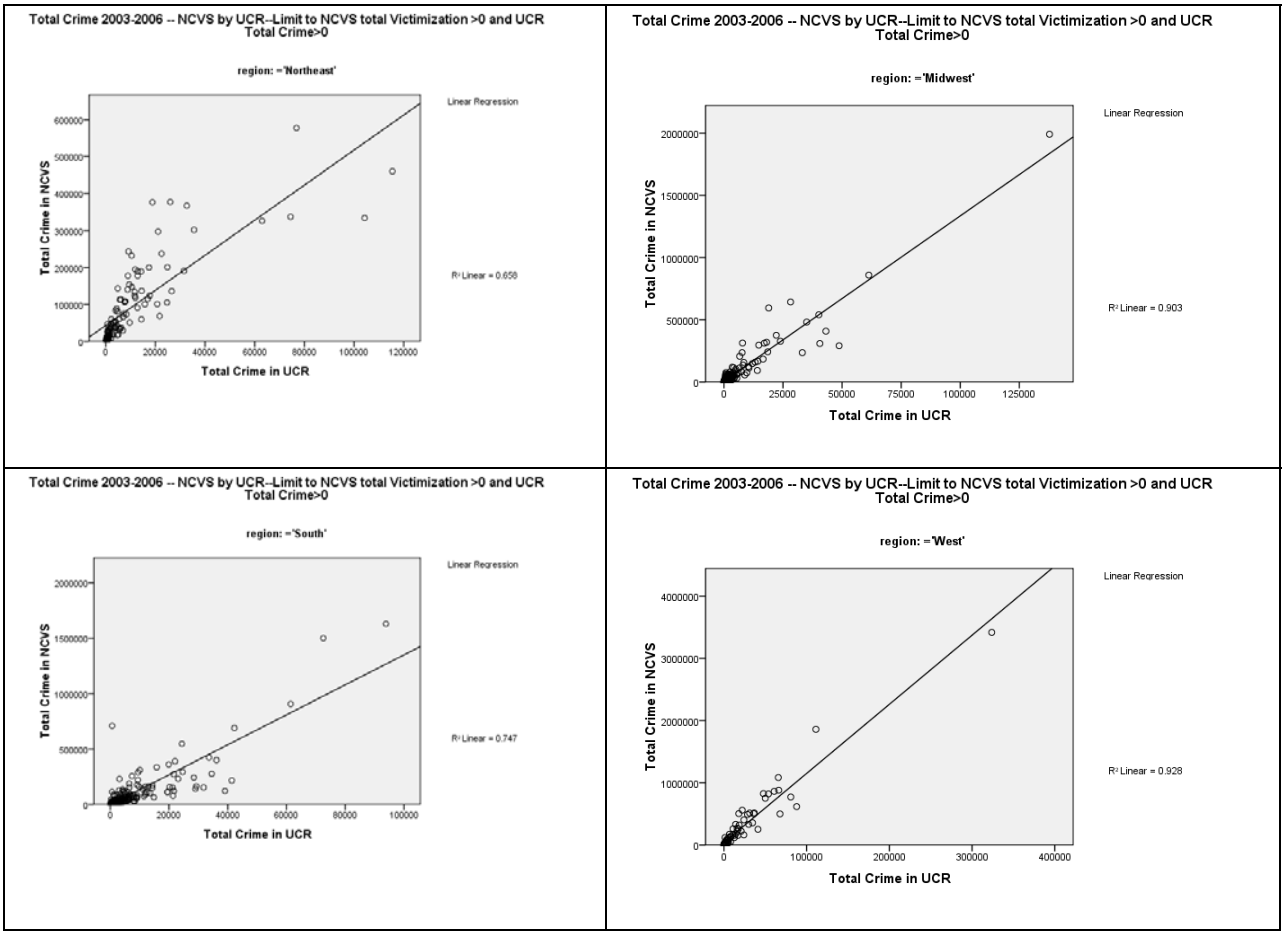
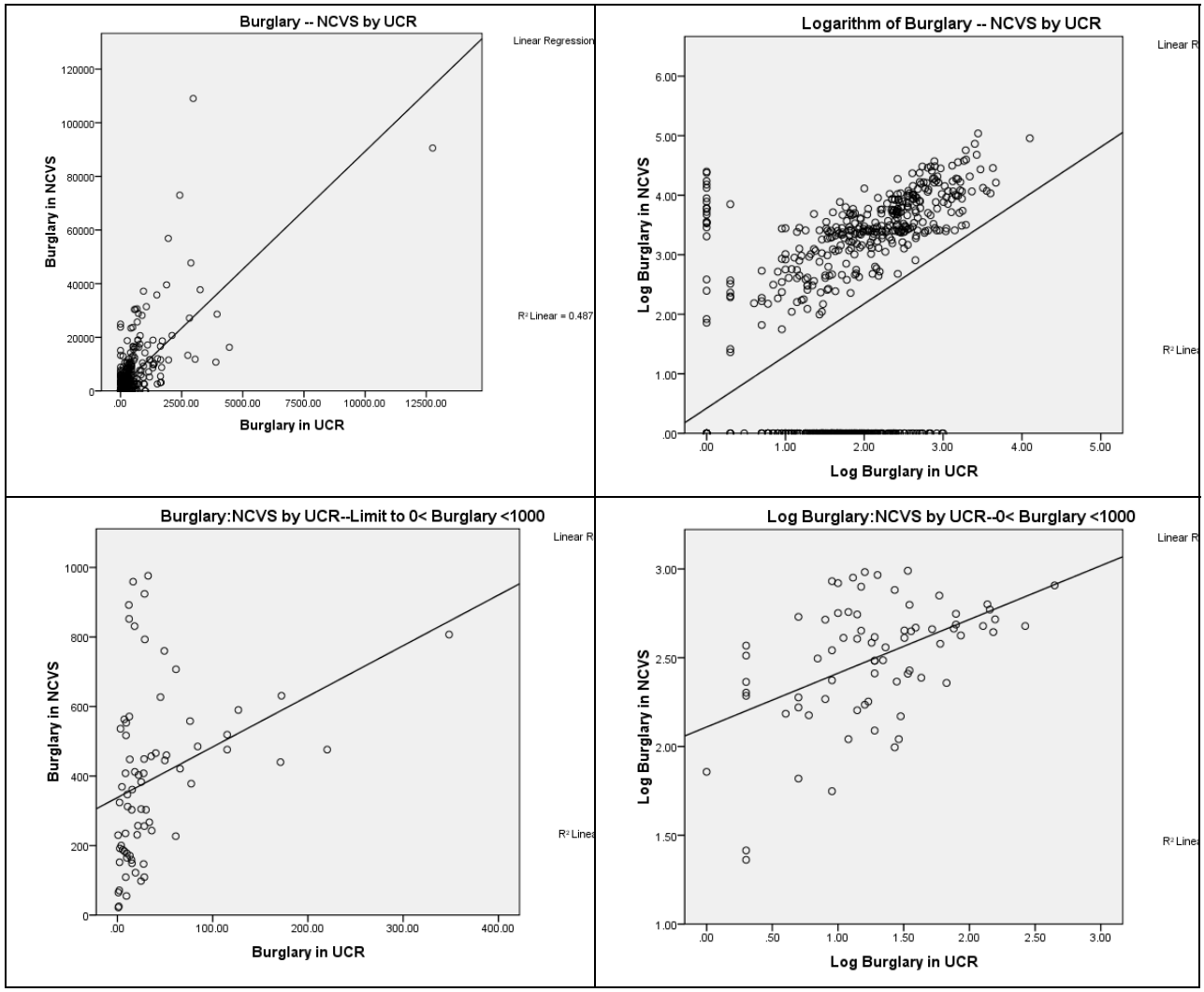


Figure F4.4: County-level Burglary Crime Victimization Estimates in NCVS by Burglary Offense Arrests in UCR, 2006



Appendix F4.1: Crime Victimization Incidents at Selected Areas – Unweighted

Table F4.1.1: Crime Victimization by YEAR					
Bronx, NY					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	2	0	2
Robbery	1	1	4	3	9
Assault	8	5	11	9	33
Purse Snatching/Pocket Picking	0	1	0	0	1
Burglary	3	5	3	1	12
Motor Vehicle Theft	1	0	0	1	2
Theft	19	9	14	11	53
Total	32	21	34	25	112
Frequency Missing = 2938					

Table F4.1.2: Crime Victimization by YEAR					
Cook County					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	2	0	1	0	3
Robbery	16	4	11	1	32
Assault	20	31	26	17	94
Purse Snatching/Pocket Picking	9	8	3	1	21
Burglary	36	49	23	38	146
Motor Vehicle Theft	15	12	9	8	44
Theft	135	118	103	91	447
Total	233	222	176	156	787
Frequency Missing = 13630					

Table F4.1.3: Crime Victimization by YEAR					
Kings (Brooklyn), NY)					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	2	7	3	2	14
Assault	9	6	8	9	32
Purse Snatching/Pocket Picking	2	0	3	0	5
Burglary	12	6	2	4	24
Motor Vehicle Theft	2	2	1	1	6
Theft	25	24	21	21	91
Total	52	45	38	37	172
Frequency Missing = 6152					

Table F4.1.4: Crime Victimization by YEAR					
Manhattan					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	2	2	0	0	4
Assault	11	7	5	4	27
Purse Snatching/Pocket Picking	0	1	0	0	1
Burglary	2	1	3	4	10
Motor Vehicle Theft	1	0	0	1	2
Theft	32	25	18	14	89
Total	48	36	26	23	133
Frequency Missing = 3215					

Table 4.1.5: Crime Victimization by YEAR					
Other					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	84	73	61	94	312
Robbery	200	175	170	219	764
Assault	1686	1571	1319	1720	6296
Purse Snatching/Pocket Picking	70	75	68	59	272
Burglary	1493	1481	1324	1477	5775
Motor Vehicle Theft	451	462	386	363	1662
Theft	6308	6227	5309	6054	23898
Total	10292	10064	8637	9986	38979
Frequency Missing = 698734					

Table F4.1.6: Crime Victimization by YEAR					
Queens, NY					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	2	1	1	0	4
Assault	5	4	8	6	23
Purse Snatching/Pocket Picking	1	2	0	1	4
Burglary	2	0	5	5	12
Motor Vehicle Theft	2	1	3	0	6
Theft	28	15	19	21	83
Total	40	23	36	33	132
Frequency Missing = 5122					

Table F4.1.7: Crime Victimization by YEAR					
Richmond (Staten Island), NY					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	0	0	0	0	0
Assault	0	5	1	0	6
Purse Snatching/Pocket Picking	0	0	1	0	1
Burglary	0	0	0	0	0
Motor Vehicle Theft	1	0	0	0	1
Theft	6	3	9	3	21
Total	7	8	11	3	29
Frequency Missing = 1290					

Appendix F4.2: Crime Victimization Incidents at Selected Areas -- Weighted

Table F4.2.1: Crime Victimization by YEAR					
Bronx, NY					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	11222	0	11222
Robbery	3525.3	3964	16988	7444.2	31921
Assault	30720	12467	34407	28506	106100
Purse Snatching/Pocket Picking	0	2009.2	0	0	2009.2
Burglary	7013.2	10088	7246.1	3176.6	27524
Motor Vehicle Theft	2455.7	0	0	2953.6	5409.3
Theft	44469	20990	37510	32604	135573
Total	88183.2	49518.3	107373	74684.7	319759

Table F4.2.2: Crime Victimization by YEAR					
Cook County					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	4741.7	0	2293.6	0	7035.3
Robbery	43937	11369	41296	3887.6	100489
Assault	53439	80658	83334	62013	279444
Purse Snatching/Pocket Picking	23123	21021	10195	2514.5	56854
Burglary	78417	105845	62576	109027	355865
Motor Vehicle Theft	34186	28637	26864	23395	113082
Theft	310313	252023	256998	255492	1074826
Total	548157	499553	483557	456328	1987595

Table F4.2.3: Crime Victimization by YEAR					
Kings (Brooklyn), NY)					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	6626.1	19072	9269.3	11135	46103
Assault	25948	15127	28418	27758	97251
Purse Snatching/Pocket Picking	7302.4	0	8386.1	0	15689
Burglary	32397	13283	4984.5	11821	62485
Motor Vehicle Theft	5263.9	4826.6	2779.8	2379.8	15250
Theft	58896	49130	54896	57826	220747
Total	136433	101439	108734	110920	457525

Table F4.2.4: Crime Victimization by YEAR					
Manhattan					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	5514.6	5894.7	0	0	11409
Assault	37628	17266	13651	10696	79240
Purse Snatching/Pocket Picking	0	2514.6	0	0	2514.6
Burglary	4835.8	2235.3	7675.2	11560	26306
Motor Vehicle Theft	2226.6	0	0	2557.7	4784.3
Theft	73614	52602	46664	38686	211566
Total	123819	80512.6	67990.2	63498.6	335820

Table F4.2.5: Crime Victimization by YEAR					
Other					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	243632	197019	187509	322314	950473
Robbery	546659	461567	536933	714888	2260046
Assault	4430054	4267294	4180266	5560462	1.84E+07
Purse Snatching/Pocket Picking	185342	221487	247161	198477	852466
Burglary	3582029	3478690	3647753	4130200	1.48E+07
Motor Vehicle Theft	1019022	1040342	1039877	1061089	4160330
Theft	1.46E+07	1.46E+07	1.41E+07	1.67E+07	6.00E+07
Total	2.46E+07	2.42E+07	2.39E+07	2.87E+07	1.02E+08
Frequency Missing = 396010.16212					

Table F4.2.6: Crime Victimization by YEAR					
Queens, NY					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	5565.5	2213.6	2576	0	10355
Assault	14143	10620	24858	22763	72384
Purse Snatching/Pocket Picking	2178.9	5958	0	3386.8	11524
Burglary	4481.2	0	9333.3	13282	27097
Motor Vehicle Theft	4398.7	1908.5	8006.4	0	14314
Theft	67595	33809	46238	60097	207739
Total	98362.7	54509.4	91011.3	99528.4	343412
Frequency Missing = 4654.9130483					

Table F4.2.7: Crime Victimization by YEAR					
Richmond (Staten Island), NY					
	YEAR				Total
	2003	2004	2005	2006	
Rape/Sexual Assault	0	0	0	0	0
Robbery	0	0	0	0	0
Assault	0	12009	1281.6	0	13291
Purse Snatching/Pocket Picking	0	0	2368.2	0	2368.2
Burglary	0	0	0	0	0
Motor Vehicle Theft	2589.4	0	0	0	2589.4
Theft	13437	6623.5	21230	8262.5	49553
Total	16026.4	18632.7	24879.5	8262.48	67801.1

Appendix F4.3: UCR Arrest Rates for Selected Areas, 2003 (per 1,000)

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	0.087909	0.11231	0.54179	1.04038	0.61927	3.21165	1.56485
Bronx, NY	0.078519	0.12915	1.12127	1.85803	1.07064	4.76175	1.29225
Kings (Brooklyn), NY	0.078641	0.12865	1.12114	1.85714	1.06952	4.76201	1.29173
New York (Manhattan), NY	0.081683	0.12928	1.13648	1.89028	1.07924	4.80192	1.29599
Queens, NY	0.078535	0.12896	1.1218	1.86833	1.07182	4.78394	1.29226
Richmond (Staten Island), NY	0.078282	0.1283	1.13074	1.8592	1.07203	4.77305	1.29166

Appendix F4.4: UCR Arrest Rates for Selected Areas, 2004 (per 1,000)

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	0.080988	0.10493	0.54503	1.0506	0.55887	3.0076	1.38578
Bronx, NY	0.074887	0.13289	1.13431	2.13867	1.13211	5.24794	1.40449
Kings (Brooklyn), NY	0.07529	0.13336	1.13656	2.13856	1.13215	5.24748	1.40488
New York (Manhattan), NY	0.075454	0.13393	1.14816	2.17685	1.15005	5.38365	1.40533
Queens, NY	0.075113	0.13334	1.13736	2.15382	1.13469	5.26456	1.40536
Richmond (Staten Island), NY	0.075669	0.13404	1.14801	2.14036	1.1372	5.25795	1.40529

Appendix F4.5: UCR Arrest Rates for Selected Areas, 2005 (per 1,000)

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	0.072	0.10209	0.57924	1.06917	0.5281	2.80048	1.31808
Bronx, NY	0.10458	0.13573	1.21267	2.54031	1.27646	5.94989	1.30316
Kings (Brooklyn), NY	0.10438	0.13621	1.20738	2.5401	1.2763	5.94866	1.30289
New York (Manhattan), NY	0.10522	0.13862	1.223	2.56887	1.28475	6.04506	1.30428
Queens, NY	0.1045	0.1359	1.20918	2.54753	1.27735	5.97952	1.30337
Richmond (Staten Island), NY	0.1044	0.13636	1.21874	2.54401	1.27627	5.95094	1.30183

Appendix F4.6: UCR Arrest Rates for Selected Areas, 2006 (per 1,000)

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	0.05846	0	0.60328	0.94216	0.52275	2.51796	1.0902
Bronx, NY	0.14074	0.14442	1.29612	2.80076	1.43022	5.70173	1.17748
Kings (Brooklyn), NY	0.14098	0.14418	1.29762	2.79949	1.42978	5.69871	1.17747
New York (Manhattan), NY	0.14099	0.14537	1.30897	2.84165	1.44996	5.7848	1.17739
Queens, NY	0.141	0.14412	1.29709	2.80656	1.43005	5.73003	1.18063
Richmond (Staten Island), NY	0.14138	0.14349	1.30616	2.80644	1.43065	5.69939	1.17744

Appendix F4.7: UCR Arrest Counts for Selected Areas, 2003

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	472	603	2909	5586	3325	17244	8402
Bronx, NY	107	176	1528	2532	1459	6489	1761
Kings (Brooklyn), NY	195	319	2780	4605	2652	11808	3203
New York (Manhattan), NY	127	201	1767	2939	1678	7466	2015
Queens, NY	176	289	2514	4187	2402	10721	2896
Richmond (Staten Island), NY	36	59	520	855	493	2195	594

Appendix F4.8: UCR Arrest Counts for Selected Areas, 2004

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	433	561	2914	5617	2988	16080	7409
Bronx, NY	102	181	1545	2913	1542	7148	1913
Kings (Brooklyn), NY	188	333	2838	5340	2827	13103	3508
New York (Manhattan), NY	120	213	1826	3462	1829	8562	2235
Queens, NY	169	300	2559	4846	2553	11845	3162
Richmond (Staten Island), NY	35	62	531	990	526	2432	650

Appendix F4.9: UCR Arrest Counts for Selected Areas, 2005

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	383	543	3081	5687	2809	14896	7011
Bronx, NY	141	183	1635	3425	1721	8022	1757
Kings (Brooklyn), NY	259	338	2996	6303	3167	14761	3233
New York (Manhattan), NY	167	220	1941	4077	2039	9594	2070
Queens, NY	233	303	2696	5680	2848	13332	2906
Richmond (Staten Island), NY	49	64	572	1194	599	2793	611

Appendix F4.10: UCR Arrest Counts for Selected Areas, 2006

Area	Murder	Rape	Robbery	Aggressive Assault	Burglary	Larceny	Motor Vehicle Theft
Cook County, IL	310	0	3199	4996	2772	13352	5781
Bronx, NY	191	196	1759	3801	1941	7738	1598
Kings (Brooklyn), NY	352	360	3240	6990	3570	14229	2940
New York (Manhattan), NY	225	232	2089	4535	2314	9232	1879
Queens, NY	316	323	2907	6290	3205	12842	2646
Richmond (Staten Island), NY	67	68	619	1330	678	2701	558

Appendix G

NORC at the University of Chicago
Ideas for the NCVS July 24, 2009

Introduction

It is important to establish valid measures of crime, and it is also important to be able to observe changes in trends and patterns of crime. So naturally as lessons are learned from the experience of implementation and as new data collection methods and technologies are developed there have been and will continue to be recommendations to modify or redesign the NCVS to improve the quality of the data and data collection. However, for a program like this to be able to continue to contribute to policy and practice, any modification or redesign needs to be cognizant of and attentive to impacts on trends and trend analysis from changes in design or methodology.

Based on extensive study and review by a consortium of experts and a report issued by the NAS, in 1992 the NCS was redesigned and became the NCVS. In 2008 the National Research Council of the NAS issued a report with new recommendations for a redesign of the NCVS. Naturally any such redesign is a long term process. As part of the longer term process BJS has been supporting methodological studies of the NCVS. Currently NORC is conducting two such studies: *The Analysis of Possible Non-Response Bias in the National Crime Victimization Survey* (2008-BJ-CX-K062) and *An Examination of a Twelve Month Reference Period in the National Crime Victimization Survey* (2008-BJ-CX-K071).

At a meeting between the NORC team working on the non-response bias study and the BJS staff working on the project, the question was raised about what could be done over the next 12 to 18 months that could immediately be implemented for the NCVS without a break in the current series. On July 23 a meeting was held at NORC including members of the teams working on the two NCVS methodological studies. The meeting was attended by Chet Bowie, Fritz Scheuren, Norman Bradburn, Zhiwei Zhang, James Carr, Lisa Lee, and Henry Brownstein. We discussed this question and our response follows.

Ideas for Implementation over the Coming Months

1: Paradata Constructions and Related Issues

a. Interviewer Information

- *ID*: Work with the census bureau staff to ensure the analytic utility of the interviewer IDs and make immediate suggestions on how the interview IDs should be assigned, organized, stored during the survey cycles.

- *Demographic information*: work with the Census Bureau DSD to formulate an operational plan to get the data; assess the data; and make recommendations on data coding and storing and database management.

- *Interviewer experience information*: work with the Census Bureau to figure out the reliability and the validity of this measure and how the existing coding can be transformed into useful information such as experience in NCVS and experience in all surveys overall, and make suggestions how it may be coded in the future.

- *Information unavailable but potentially useful for consideration in future paradata collection*: interviewers' attitudes and confidence ("can convince almost anyone to respond"); interviewers' education, occupational aspiration; use of cultural knowledge and local knowledge of the sample neighborhood; way of adapting to the NORC at the University of Chicago 2 Ideas for the NCVS July 24, 2009 situation at the doorstep; behaviors during the interviewer-respondent interactions such as any intentional or unintentional departure by interviewers from the designed guidelines or instructions, observed respondents' burden during the face-to-face interviews (is the respondent in a hurry to do something else?)

b. Frame Information

Current NCVS internal files contain some geographic information which were originally used for sample design such as PSU, county, and tract identifiers. A number of census tract level characteristics such as median age, median housing value, percent Hispanic, etc. were available in the existing NCVS internal files. These variables are very helpful for the study of the possible nonresponse bias and other victimization-related researches but not all pertinent and influential variables that may affect the survey processes are included. One example of such variables is the "percentage of residents who moved in the past five years" which may influence the local pattern of the mobility and the percentage of movers in the NCVS. It is recommended to make a systematic effort to add variables by using the geographic identifiers. Practically, it is relatively convenient to do so because the analysis will be conducted at the Census Bureau and almost all of the variables to be added will come from the Census Bureau's database and many of which are publically available. This merging effort can be made smoother because of the knowledgeable staff on the site.

Immediate:

Assess the level of geographic units and prepare a list of socio-demographic variables from the Census and merge them into the existing NCVS internal files.

Long-term:

Assess the utility of the American Community Survey in facilitating and improving the survey process.

Merge the 2010 NCVS with the 2010 decennial Census.

c. Cleaning the Survey History Record Information for Better Use

The Census Bureau has internal paradata association with the NCVS. These include (1) the Call-history Data for CATI (i.e., for NCVS till June 2007); and (2) Audit Trail Data for CAPI which is currently used in the NCVS in July 2007 and onward. These data are probably extremely under-utilized. The data were also not designed special for the formal paradata analytic. We recommend the following steps:

- Assess and document fully the availability and coverage of these data.
- Assess the variables codings and the extent of the recodes
- Perform recodes so that the data can be used as paradata to support methodological studies of NCVS
- Perform special analyses and recommend further analyses using these data.
- Recommend how the future paradata in this area should be recorded and how the data should be collected and managed.

d. Contact Information

Other than Call-history and Audit Trail Data, further information on the detailed ways of approaching and contacting the households and respondents are lacking. We recommend NORC at the University of Chicago 3 Ideas for the NCVS July 24, 2009 collaborating with the Census staff to develop a list of interesting information that future paradata collections may use.

2: Longitudinal NCVS Constructions and Related Analyses

The purpose of this effort would be to build upon what has been done by NORC and to convert NCVS Cross-sectional Files into Longitudinal Files for the full 2003-2009 period, thereby increasing the analytic utility of the surveys.

a. Immediate

- Handle the limitations and contingencies such as movers.
- Revise a previous data structure plan to cover the major contingencies (cases not matched, movers) and document the method and process of how the longitudinal files are created
- Conduct disclosure analysis to assess the feasibility of making the longitudinal cohort files as (i) PUF and (ii) as internal files for other staff to use and make recommendations for NJS to review.

b. Short-term

- Apply the updated program to create the 2007 July-Dec ember NCVS Cohort Longitudinal file.

- Collaborate with the Census Bureau in the file creation process and have the linkage variables double checked for each year to ensure quality control which will be necessary due to survey redesign or possible internal program modifications and changes.

- Create codebooks for the longitudinal files

- Revise the program to make them as macro engines for multiple year NCVS processing and can be readily applied to any cohorts after the 2007 July-December.

c. Related Analyses

- Make the followings comparisons:

    - Compare the 2003 Jan-June Cohort to 2003 July-Dec Cohort
    - Compare the 2003 July-Dec cohort to 2007 July-Dec Cohort

- On the following domains:

    - Type A Nonresponses
    - Type Z nonresponses
    - Crimes
    - Movers
    - Unbounded wave vs. all other waves
    - repeated victimizations (i.e., # of victimizations >2 in the three years in the survey)
    - Spatial distributions of the above
    - HLM model in the causal relationship
    - Latent class analysis of the status changes
    - NORC at the University of Chicago 4 Ideas for the NCVS July 24, 2009

3: Addressing Bias and Variance Reduction

a. Immediate Implementation

- Routine housekeeping could be conducted to be able to successfully match waves over time in order to do any of the other ideas recommended.

- Earlier interviews could be used to longitudinally impute (not reweight) data for later NCVS wave nonresponse.

- UCR data could be used, after light editing and longitudinal imputation, for ratio estimation of the NCVS.

b. Implementation Following Some Research

- The screener could be further examined to increase interviewer compliance with BJS goals for the survey.

- The NCVS paradata system could be redesigned to aim it simultaneously at continued successful survey departmental success, while also instrumenting it more fully for inference.

- Processing could be sped up so analysis comes more closely, almost simultaneously, with data collection.

4: Bounding Procedures

As part of the NCVS 12-month reference period contract, NORC examined the percentage of crime incidents reported for each month of the 6-month reference period. The data showed that for the first few months of the reference period, the percentage of crimes reported drops as time between the interview and the incident increases. However, there is a slight increase in the percentage of crimes reported for the final two months of the reference period. This pattern suggests that telescoping is occurring. That is, some incidents that occurred outside the reference period are being reported as having occurred within the reference period.

To address problems of telescoping, many panel surveys employ bounding procedures. The interviewer typically reminds the respondent of the information he/she reported in the prior round of the survey before conducting the current interview. However, in the NCVS, respondents are not reminded of incidents that were reported in prior interviews. Rather, the interviewer compares reports from the current and prior rounds and, if the interviewer feels there may be a duplicate report, s/he must ask the respondent questions to determine whether the incidents are duplicates. This procedure relies on the interviewer's judgment on whether to ask the respondent about a possible duplicate report.

The current procedures for bounding interviews in the NCVS could be revised. A procedure that involves the respondent in de-duplicating reports may be more effective in limiting forward telescoping. NORC at the University of Chicago 5 Ideas for the NCVS July 24, 2009.

5: Examining the Organization of Crime Event Information

Relatively little is known about how people store and organize information about crime events in memory. One way of studying this issue is through respondents' verbal descriptions of the crimes they have experienced. These verbal protocols could be analyzed with a focus on the types of event information that are spontaneously reported, and the order and organization of the crime event information.

The NCVS screener provides an opportunity to collect verbal protocols on crime events. For each crime elicited by a screener question, the respondent is asked to describe what happened. Although respondents might typically provide only a brief description of the incident, selected respondents could be asked to describe their crime experience in greater detail. This information may be useful in deciding what kinds of retrieval cues are most effective and help inform future modifications to the screener.

6: Questions about Design Elements Suggesting Possible Adjustments

a. A move to a 12-month reference period will have implications for the survey beyond the question of incident reporting. With a longer reference period, telescoping will be more important. At present we understand very little about how the adjustment for using the first (unbounded) interview was arrived at or how it will have to be adjusted with a 12-month reference period. Also, we have not been able to get a clear idea about how much information the interviewer has to use in deciding whether an incident reported in the current period has been reported in the last period. Apparently the determination as to whether it is a new or old incident is made by the interviewer at the end of the interview rather than by directly asking the respondent. The wording of the question also poses a problem because reporting incidents that occurred in the last 12-months is different than reporting "since the last interview." Not all respondents are interviewed at the same report in the fixed reporting period. We know from our analysis that there are patterns of reporting influenced by how far from the calendar date of the reporting period the interview is actually taking place. This problem will be greater when the reference period is 12-months. A 12-month reporting period will have implications for other aspects of the survey such as series crimes. The number of crimes in a specified period to qualify as a series crime will have to be adjusted. What is the proper number?

b. With a 12-month reference period, more incidents will be reported thus triggering more incident reports and increasing the burden on the respondent. It may reduce the willingness to cooperate in later rounds of the survey. One way to reduce the burden is to subsample the frequent crimes and get incident reports on only a sample of them. Research should be done on how to do this. What crimes incidents should be subsampled? What should the sampling rate be? Can one use the UCR to supplement the data on the types of crimes that are subsampled?

c. There is little research on the incident forms. Is all of the information there necessary? Is it necessary for all incidents? Reducing the detail or number of incident forms for NORC at the University of Chicago 6 Ideas for the NCVS July 24, 2009 respondents would reduce the respondent burden and should lead to better cooperation and better reporting of incidents.

d. The NCVS is one of the major sources of data on the cost of crime, at least of direct costs to victims. With a longer reference period, costs that were not reported with a 6-month reference period because they fell outside the reporting period may now be reported. Some research is needed to know how to disentangle greater reported costs due to the increased number of incidents reported from those due to the longer period during which costs for a particular crime, e.g. prolonged health care expenses.

Conclusion

We thank you for this opportunity to present our ideas to you. The ideas presented above are suggestions and do not all necessarily rise to the level of recommendations. They are firmly rooted in a long history of involvement with the NCVS and ongoing interest and work in the methodological and operational aspects of the survey. But they were conceived as ideas for this particular purpose within a 24 hour period. So if BJS is interested, any or all of these are things we could develop further for or with BJS.

The redesign of NCS in the 1980s was lead by Al Biderman with a scientific advisory group that was constituted to set the research addenda. Different groups such as NORC, SRC at Michigan, and individual scholars in different universities, worked with them to conduct a number of studies and experiments covering all aspects of the survey from sampling to questionnaire to reporting. The result was a coordinated comprehensive set of recommendations for the redesign. If BJS is planning such a coordinated approach to the current redesign NORC would be honored to be able to participate in that effort and to contribute in any way we can.