Designing a Low(er)-cost Companion to the National Crime Victimization Survey

W. Sherman Edwards, J. Michael Brick; Sharon Lohr

W. Sherman Edwards, MBA, and J. Michael Brick, Ph.D., are Vice Presidents with Westat, and Dr. Brick is co-Director of Westat's Survey Methods Unit; Sharon Lohr, Ph.D., is Dean's Distinguished Professor of Statistics, School of Mathematical and Statistical Sciences, Arizona State University.
Contact: W. Sherman Edwards, Westat, 1650 Research Blvd, Rockville MS 20850; shermedwards@westat.com

The National Crime Victimization Survey (NCVS) is the nation's primary source of information on criminal victimization. Each year, data are obtained from a nationally representative sample of approximately 40,000 households on the frequency, characteristics and consequences of criminal victimization in the United States. Households are selected in a stratified, multi-stage area sample, in a rotating panel design. Each household is interviewed seven times at six-month intervals; the first interview is always in person; subsequent interviews are conducted by telephone to the extent possible. The survey enables the Bureau of Justice Statistics (BJS) to estimate the likelihood of victimization in the form of rape, sexual assault, robbery, assault, theft, household burglary, and motor vehicle theft for the population as a whole as well as for various subgroups of the population, such as women, the elderly, members of various racial groups, city dwellers, or other groups. The NCVS provides the largest national forum for victims to describe the characteristics and consequences of victimization.

Since the inception of the NCVS, there has been demand for these data at a sub-national level. The three major reviews of the NCVS program (Penick and Owens, 1976; Biderman et al., 1986; Groves and Cork, 2008) all point to the demand local criminal justice administrators have for empirical information to shape policy. In the early years, a series of surveys were conducted in cities (e.g., Hindelang et al., 1978). This included surveys in eight "Impact" cities to assist in evaluating crime prevention programs in those areas. These surveys were conducted outside of the regular NCVS data collection system with designs that differed from the national survey. For example, they included a 12-month reference period and were conducted as cross-sectional surveys over relatively short periods of time. These surveys were not continued, partly because variation in implementation across cities seemed to confound analysts' abilities to compare rates across areas (Penick and Owens, 1976).

Since these early years, the demand for local area estimates has remained strong (Karmen, 2007). A number of states have conducted their own surveys by using a mailed paper questionnaire or by telephone (e.g., Giblin, 2003; Haddon and Christenson, 2005). BJS has tried to meet the demand for information in several different ways. One was to provide both technical assistance and software to conduct victimization surveys. A second was to publish MSA-level estimates for 20 locations using the current sample design (e.g., Lauritsen and Schaum, 2005). BJS has also explored other data collection and analysis strategies to produce local area estimates (LAEs).

In 2010, BJS entered into a cooperative agreement with Westat to design and test a low-cost companion survey to the NCVS that would support local area estimates. For the purposes of this test, "local area" was defined as "Metropolitan Statistical Area" (MSA). This paper will describe the design of the companion survey developed under this cooperative agreement, and discuss the decisions made in developing it. The NCVS Companion Survey (CS) is currently scheduled to be pilot tested in early 2012, with a somewhat larger implementation in 2013. The pilot test will include a methodological experiment to assess design features.

Methods for obtaining local area estimates of victimization rates

The design of the CS has been guided by the goal of improving local area victimization estimates while using resources as efficiently as possible. With that goal in mind, we describe four approaches to producing LAEs. Approaches (a) and (b) use the core NCVS without any new survey data, and are outside the scope of the cooperative agreement and this paper; we mention them here for later comparison with the use of a CS in approaches (c) and (d).

(a) Better direct estimates of victimization using the NCVS. LAEs obtained by direct measurement rely only on the victimization reports of the respondents to the core NCVS. The current NCVS design does not have enough sample size in local areas to provide LAEs of sufficient accuracy; many areas of the country, particularly rural areas, have no sample at all. Thus, to obtain accurate LAEs, one can (1) increase sample sizes in LAEs of interest with the current design, or (2) modify the design to increase its efficiency for estimating victimization rates. Some possible options for modifying the design include: additional stratification, with higher sampling fractions in strata with higher anticipated victimization rates; a two-phase design, in which an inexpensive but fallible screener is used to stratify households for administration of the NCVS instrument; a dual frame design, in which the area frame of the NCVS is supplemented by a (likely incomplete) frame with a high concentration of victims. While these options have some potential (Fay and Li, 2012), their ability to produce LAEs will be very limited using direct methods without increasing sample sizes and data collection costs.

(b) Model-based estimation with currently available information. Model-based methods predict the victimization rate from administrative data or other sources, using a regression model. If there is also a direct estimate of victimization for that area from the NCVS, the LAE is a weighted average of the NCVS estimate and the prediction from the regression model; if the NCVS has no sample in the area, the LAE is the regression prediction. If the assumed regression model is correct, the resulting LAE is unbiased under the model and has smaller mean squared error than using just the direct estimate from the NCVS alone. Model-based methods require the presence of high-quality, consistently reported, auxiliary information that are highly correlated with the outcomes (victimization). To date, no such data source has been identified for area-level models of crime victimization. While individual- and household-level characteristics such as gender, age, or moving are associated with higher risk of victimization, such factors do not work well in area-level models---most MSAs, for example, have similar male/female ratios so a variable such as percentage female has little power to predict violent victimization at the MSA level. Explorations of Uniform Crime Report (UCR) data have indicated that the data from police reports have variable bias for predicting NCVS victimization rates across local areas (Lauritsen and Schaum, 2005); in addition, the bias varies across crime types (McDowall and Loftin, 2007). This type of variability greatly diminishes the quality and utility of the data for model-based estimation.

(c) Model-based estimation with additional auxiliary information collected through a survey. The main barrier to producing model-based estimates as described above is the lack of highly correlated auxiliary information. One possible solution is to collect better auxiliary information, for example through a large mail survey in targeted small areas. Such a survey could collect brief information about victimization, attitudes about crime, and similar variables known to be correlated with victimization. Such a mail survey could produce information of interest in its own right, such as attitudes about crime or the police, as well as auxiliary information to be used in producing LAEs of victimization rates and characteristics, at a relatively low cost. A mail survey gives flexibility for adjusting the location to which the sample is allocated over time, so that the targeted local areas can be rotated or varied.

On the other hand, concepts of victimization may differ in the two surveys, and the differences may vary across demographic groups. This is a potential source of differential bias. These biases might be handled by modeling, whereas modeling is less likely to be capable of compensating for differential biases in administrative data across areas. The problem with the administrative data is that the mechanisms that create the differential biases are not known and thus very difficult to model. As with all model-based methods, the quality of LAEs depends on how well the assumed model fits the data. In particular, the model must be trustworthy for areas that have no NCVS sample, since in those areas estimates depend entirely on the model. The method is likely to improve accuracy of LAEs of victimization rates in broad categories; it is less likely to improve LAEs of more detailed characteristics of victimizations.

(d) Blended estimates from two surveys. Under this approach, an independent companion survey (CS) on victimization is conducted in targeted local areas, and estimates from the CS blended with those from the NCVS. If the CS uses a lower-cost data collection design than that of the NCVS, the cost of achieving more precise LAEs can be substantially lower by using a CS than by increasing the NCVS sample size. This approach also provides more information than the mail survey described above on details of victimization that can be used for type-of-crime classification and variables of interest such as weapon use.

If the CS and NCVS share a common conception of victimization and use similar instruments, estimates from the two surveys can be blended directly. In this case, the estimated victimization rate in an area is a weighted average of

the estimated victimization rate from the CS and that from the NCVS. The weights can be either fixed or variable and dependent on the relative accuracy of the CS and NCVS estimates. Blending the surveys has the advantage that any variable measured in both surveys may be estimated using the same relative weights used for estimating victimization rates.

If the nonsampling error structure is the same for the CS and the NCVS, blending the surveys can be done using estimation methods for dual-frame surveys. However, the error structure of a CS may differ substantially from that of the NCVS. For example, if CS data are collected with different modes or have different response rates or interviewer effects, then the sources and directions of bias in the CS and NCVS may differ. Since the CS may have to be collected by a different mode for cost reasons, this change is likely to have many potential effects, so approaches other than direct blending may need to be explored. Lohr and Brick (2011) discuss potential sources of bias in the CS relative to the NCVS, and explore methods for adjusting for bias when combining the surveys. An alternative approach to direct blending is to use the CS as auxiliary information in a model-based approach for LAE, similar to (c).

If an address-based sample is used for the CS, detailed auxiliary information from surveys and administrative records such as the American Community Survey, the Decennial Census, the UCR, and police jurisdictions could be used in the design of the survey, thus improving efficiency relative to the PSU-based NCVS. As with the mail survey in (c), the sampling design is flexible and sample can be easily moved over time to give increased precision in different areas.

Potential Sampling and Data Collection Approaches for an NCVS Companion Survey

The current NCVS is a longitudinal survey with seven interview waves for each sampled household, and attempts interviews with every person age 12 or older in the household. As mentioned earlier, the concept of a CS is that it must be much less expensive per completed household to administer than the current NCVS. A number of factors contribute to the cost of the NCVS: the large sample size, in-person interviewing, and the complexity of the instruments are perhaps the three most significant.

The sample size is dictated by precision requirements for survey estimates. Victimization, particularly for violent crimes, is a fairly rare event, so estimating trends in victimization rates by type of crime requires a large sample. Estimating characteristics of crime victims requires an even larger sample. However, if there were some reasonably reliable alternative way to identify potential crime victims, using screening or a dual frame design as described earlier, the sample size for the detailed interview could be reduced.

Telephone interviewing is considerably less expensive than in-person interviewing, primarily because of the in-person interviewers' travel time and associated expenses. Mail surveys generally are less expensive yet, depending on the design, and Web surveys generally are the least expensive alternative. However, using the Web and mail pose serious challenges for the current survey instruments. The NCVS instrument is complex, and relies on interviewer mediation to classify reported crimes, so it may not lend itself to mail or Web administration without interviewers. Further, there is as yet no sampling frame or survey design to support a Web-only cross-sectional survey of the general population.

For these reasons, BJS and Westat made several *a priori* decisions regarding the design of the CS. First, computer-assisted telephone interviewing (CATI) will be used to obtain victimization data in the CS. This decision does not rule out using mail or Web for other purposes, as we shall see. Second, the CS will be cross-sectional and only interview adults (18 and older). Third, to limit burden in large households, the CS will sample no more than two adults in each household. Finally, the CS design will be driven by local area data needs, and will be independent of the NCVS design in local areas. In particular, households will be selected independently in the two surveys. While it is possible that a household could be selected for both surveys, the probability of such an event is extremely low.

Given these decisions, three approaches to conducting a CS in local areas such as MSAs were considered. All three assume centralized telephone interviewing to collect data to support blended estimates; one would also provide data to support model-based LAEs. The approaches differ in terms of sampling frames and methods of initial contacts

with households. In-person follow-up for selected nonresponse is feasible with any of these approaches, although it is more limited with the first of the three approaches described below[1].

Random-digit-dial (RDD) survey. Traditional RDD designs using only landline frames are becoming increasingly rare as their coverage of the household population continues to decline. The design we consider includes samples drawn from numbers assigned to both landline and cellular service, with cell numbers screened to identify cell-only households. The choice of screening of cell phones rather than interviewing all sampled cell numbers is related to the local area requirements. Given this design, we would anticipate a screening response rate of 30-40% in large MSAs, and 70-80% for the substantive interview, for a net response rate of 20-30%. The RDD methodology is well-tested. Instrument design is relatively straightforward, and in most cases the entire data collection can be done on one or two contacts with the household. However, the potential for bias due to undercoverage and nonresponse is high, and there is limited ability to stratify geographically within MSAs. Further, the cell sample would be less geographically efficient than the landline sample.

Address-based sample (ABS) with mail survey to obtain telephone numbers. This "telephone harvest" ABS approach begins with selection of a sample of addresses from a vendor-enhanced version of the USPS Delivery Sequence File. ABS allows geographic stratification within MSAs, and has very good coverage. We would then obtain telephone numbers for these addresses from vendor services. For those addresses without a telephone number, we would attempt to obtain one by mail using 2 or 3 mailings. The content of the mail piece would be limited and essentially non-substantive. We would then proceed with telephone interviewing in much the same way as for the RDD. The telephone instrumentation would be very similar to that of the RDD approach. During the telephone interview, the respondent would be asked to verify that the residence is at the sampled address since a proportion of the vendor numbers are not correct. For any sample address that is matched but the telephone number obtained is incorrect (about 20% will not even be working numbers at residences), the address will be placed into the mail process to obtain a telephone number.

The response rate for this design is likely to be comparable to or even lower than RDD. We estimate we would obtain vendor telephone numbers for about 50% of sampled addresses (this and many of the other assumed rates are likely to vary across local areas in the sample), and about 40% of those mailed a screener would provide a telephone number. About 20% of vendor-acquired telephone numbers would not be working or residential, and we estimate about 10-15% will be working but not actually be for the sampled address. Thus, about a third of the addresses with vendor-provided numbers would be cycled through the mail process, and we again assume about 40% response. In the end, we assume we would have good telephone numbers for about 60% of the addresses. Assuming a 40-50% screening rate and 80% for the substantive household interview[2], the net response rate would be about 20%. It is also likely that there will be differential nonresponse for those with and without valid matching telephone numbers.

ABS with mail screener and telephone interviewing. This approach may be called the "two-phase ABS hybrid." The sample selection would be the same as that for the ABS design above, but would involve mailing every sampled household a brief screener questionnaire. The content of the screener could (a) support model-based estimation, (b) provide data that are expected to be highly correlated with victimization incidents in the household to support stratification for the second phase (telephone) survey, and (c) yield telephone numbers for a large portion of those returning the survey. Nonresponders for whom telephone numbers are obtained from a vendor would also be available for the telephone interview. The telephone follow-up would proceed essentially the same way as in the other ABS approach.

A key aspect of this approach is subsampling after the screener based on likelihood of victimization. The households in the first-phase sample are partitioned into two strata---high likelihood of victimization and low likelihood of victimization---based on answers to screener questions: households in the high likelihood stratum are sampled with certainty, and households in the low likelihood stratum are sampled with a smaller sampling fraction. The goal is to increase the number of victimizations reported without increasing the number of second-phase telephone interviews

[1] There is currently no plan to do in-person follow-up.

[2] The only "screening" that would occur on the telephone would be identifying ineligible addresses, such as businesses and households with no members 18 or older.. The "substantive household interview" is the current screening instrument (NCVS-1) administered to the household respondent who in most households would also be a sampled adult.

conducted. If the sensitivity and specificity of the screener are both high, the two-phase design will produce more accurate estimates of victimization rates and characteristics of victims with reduced costs (McNamee, 2003). Even if either the sensitivity or specificity is relatively low, the two-phase design should yield more victim households in the sample, which will improve precision for estimates of victim characteristics.

The two-phase ABS hybrid approach produces two levels of information that can be used to improve LAEs: the CS at phase 2 can be blended with the NCVS as described in estimation approach (d), and the mail survey at phase 1 can provide high-quality auxiliary information for model-based LAEs of victimization. The design allows comparison of estimation approaches (c) and (d), by comparing results that use the mail screener alone in model-based estimation of victimization rates with results obtained using both phases of the survey. The two-phase hybrid design also allows exploration of multivariate relationships between victimization and attitudes about crime.

We expect that the number of addresses with good telephone numbers would be very comparable to that from the telephone harvest ABS approach, around 60 percent. However, because more of the sampled households would have completed the mail screener, we believe that the telephone response rates would be higher, in the range of 50-60 percent screened and 90 percent for the substantive interview, or as high as 30 percent net. This is comparable to or higher than the expected rate for an RDD design.

Relative cost of these designs. In assessing the feasibility of the different options, relative cost is an important component. We use the cost of a completed interview for landline RDD as the base, and assume the same number of completed interviews across different approaches. With the RDD landline cost as 1, we estimate the cost per cell-only household RDD interview at about 4, and the cost for a two-frame design where 11% of the completed interviews are with cell-only households is about 1.4. We estimate the per-complete cost for the telephone harvest ABS approach as being about 1.1 times that for a landline RDD case. Thus, this approach is about 20% less expensive than the dual-frame RDD.

The two-phase ABS hybrid approach is more difficult to cost due to subsampling. First, we assume no subsampling and then introduce the second-phase subsampling. In this approach mailing is more expensive than the telephone harvest ABS approach because the entire sample is sent the mail screener. The second phase telephone interview would be somewhat less expensive because almost all of those followed up would have already cooperated to the screener. On balance, assuming no subsampling after the screening, the per-complete cost would be about 1.2 times that of a landline RDD complete, or about 10% more expensive than the telephone harvest ABS approach for a given number of completed interviews. This is still substantially less than the dual-frame RDD design.

We now factor in the subsampling of the low likelihood stratum where we take half of this group for the telephone interview. Assume the high likelihood stratum is 20% of the respondents and all of these are interviewed, and we wish to maintain the total number of completed telephone interviews. This design would nearly double the initial sample (and the mailing costs), and increase the total cost by about 20%. This brings the cost of this design up to about the level of the RDD design. Notice that in this design we expect to have many more interviews with victims, so an alternative approach is to attempt to obtain the same number of completed telephone interviews with victims. If this were the metric, then the subsampling design is much less expensive if the screener instrument is effective at identifying victims.

The CS Pilot Test Design

Because of the relative statistical and cost advantages of ABS over RDD and the likelihood that these advantages will only grow over time, the CS will be tested using an ABS design. There is not a clear choice in the literature as yet between the two ABS designs described in the previous section, so the CS pilot test will conduct a split-ballot experiment. One group of sampled addresses will receive a short version of a mail screener only if a telephone number is not available from directory services. This is the “telephone harvest ABS” approach. In the other group sampled addresses will all receive a mail screening interview that will include questions (1) to support oversampling of households likely to include a crime victim for the second-phase survey, (2) to provide information in the first-phase survey that might support model-based LAEs without further data collection, and (3) to collect telephone numbers for telephone administration of the NCVS instruments. This is the “two-phase ABS hybrid” approach. The

sample will be selected so as to yield approximately the same number of completed telephone interviews with each approach.

The two approaches are summarized as flow charts in Figures 1 and 2. The telephone harvest ABS approach (Figure 1) is simpler conceptually; an initial sample is selected, and the data collection process proceeds for all sample units until some final resolution. For the two-phase ABS hybrid (Figure 2), there are two subsampling points within the design. Sampled addresses with a completed screener and a telephone number, regardless of whether the number was provided in the screener or obtained from a directory service, will be classified into two risk strata; addresses in the "low risk" stratum will be subsampled, arbitrarily at a 50% rate, while all of those in the high risk stratum will be retained. Addresses with a telephone number but no completed screener will also be subsampled.

Starting with a goal of at least 3,000 completed victimization screeners with sampled individuals for each approach, we developed the sample sizes shown in Table 1. There are a number of assumptions in these tables; some are based on evidence from similar surveys or the NCVS itself, while others, notably the response rates, are educated guesses, and are in fact outcome measures for the pilot test. After the mail screener phase, essentially every household for which we have a telephone number, whether from the vendor match or from the mail screener, is eligible for the telephone follow-up. We assume that there will be no difference between the experimental treatments in the proportion of sampled addresses that wind up with a telephone number. However, because in the two-phase telephone hybrid approach both "low risk" mail screener respondents and screener nonrespondents with vendor-provided telephone numbers will be subsampled for the telephone follow-up, the proportion of addresses that are followed up by telephone is smaller than for the telephone harvest approach.

Two key assumptions that will be tested in the pilot regarding the two-phase hybrid ABS approach are: (1) that the response rate by telephone will be higher than for the telephone harvest approach, because a large portion of the sample has already cooperated with the mail survey, and (2) that it will have a 20-30 percent higher yield of victims because of the use of mail screener data for stratification.

Pilot Evaluation Plan

There are many obvious design differences between the NCVS as conducted by the Bureau of the Census and the Companion Survey as described here. These differences, summarized in Table 2, raise the possibility that estimates from the CS may differ systematically from NCVS estimates. Evaluating differences between the CS and NCVS will be an important part of the evaluation of the pilot. Possible sources of differences in the estimates include:

- Coverage of the address-based frame;
- Mode and interviewer differences;
- House differences;
- Within-household sampling;
- Bounding and time-in-sample effects;
- Recall period: 12 months (CS) instead of 6 (NCVS);
- Nonresponse to the mail questionnaire; and
- Nonresponse to telephone survey.

Victimization rates are often sensitive to mode and response rates, and the redesigns of the NCVS in 1993 and 2006 resulted in substantial changes in estimated victimization rates (Kindermann et al., 1997; Rand, 2007). The design changes in the CS may likewise result in rates that differ from those observed in the core NCVS. These changes may or may not result in better estimates of "true" victimization rates than the core NCVS; if there are changes, however, the CS and NCVS may be biased relative to each other.

The first goal of the pilot study is to determine whether producing blended estimates from NCVS and CS data, using either of methods (c) or (d), is feasible. The primary mechanism for this assessment will be an analysis of relative bias between the two surveys, including comparison of victimization estimates and of population characteristics. If

the results of this assessment are positive or inconclusive, the next goal is to determine which of the two data collection approaches produces better data for blending for a given cost. Methods will include:

- Comparison of response rates at different stages of data collection and for different geographic areas and population subgroups;
- Cost per completed household interview and per identified victim of violent and property crime;
- Comparison of victimization rates for major type of crime classes;
- Comparison of number of crimes reported to police between approaches and with UCR by jurisdiction;
- Cost relative to standard error for estimating victimization rates and characteristics of victims from the CS; and
- Poststratification and weighting methods to produce blended estimates with NCVS.

Since the pilot will not have an optimal design, we also plan to estimate the costs and response rates that would have resulted under a more efficient design, such as optimal allocation for the two-phase ABS hybrid approach or an improved subsampling approach within households. Separately, we plan to analyze the effectiveness of the two-phase ABS hybrid screener at identifying households with victimizations. We will estimate specificity and sensitivity for different crime types and examine associations between screener questions and type-of-crime classification for NCVS.

We also plan to evaluate the potential of the two-phase ABS hybrid mail screener to support model-based local area estimates, by:

- Examining census block-level variables as predictors of (violent) victimization;
- Analyzing associations between the two-phase ABS hybrid screener questions and victimization, focusing on the non-victimization-related questions such as attitudes toward police, fear of crime, routine activities, employment, etc.;
- Fitting LAE models using phase 1 data from the two-phase ABS hybrid screener as auxiliary information, in addition to variables identified from census; and
- Developing a theory for using both phases of a two-phase survey as auxiliary information in LAE, then comparing reductions in MSE with blended estimate described earlier.

Next Steps

The larger goals of this research are to develop a methodology that will result in producing LAEs of crime victimization and characteristics that are of acceptable quality and cost. In so doing, the CS approach must be compared and contrasted with other possible methods of producing LAEs. If the CS is superior to other methods, then the research should identify the specific methodology for the CS.

To accomplish these larger objectives, we have some immediate courses of action based on the pilot test results. If the pilot demonstrates promise for blended estimates or the results are inconclusive, the next step planned is a larger test with a redesigned sample and modified instruments. The larger test would use the basic data collection approach that proved more effective in the pilot. If the pilot response rate or response bias is a particular concern, the next test may include experiments to increase response, such as the use of monetary incentives that might differentially improve response rates. If blended estimates from approach (d) do not have reduced mean squared error when compared with estimates from the NCVS alone, the next steps may include further work on assessing the components of the survey design that are associated with the relative bias. They may also include further exploration of the CS to provide correlates for model-based LAEs.

Figure 1. NCVS-CS Pilot Test: Telephone Harvest ABS Design

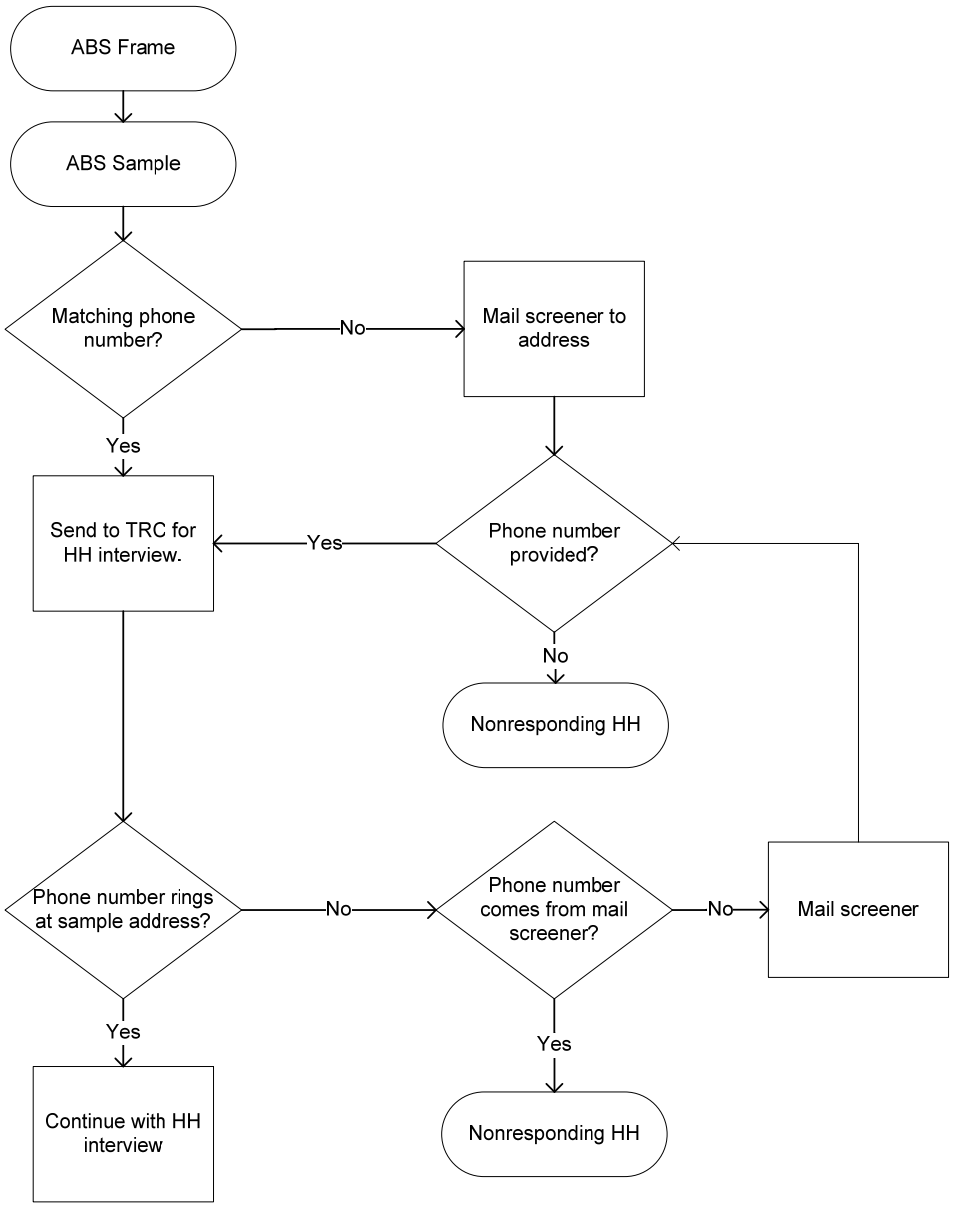
ABS Frame
ABS Sample
Matching phone number?
No
Mail screener to address
Yes
Send to TRC for HH interview.
Yes
Phone number provided?
No
Nonresponding HH
Phone number rings at sample address?
No
Phone number comes from mail screener?
No
Mail screener
Yes
Continue with HH interview
Yes
Nonresponding HH

Figure 2. NCVS-CS Pilot Test: Two-phase ABS Hybrid Design

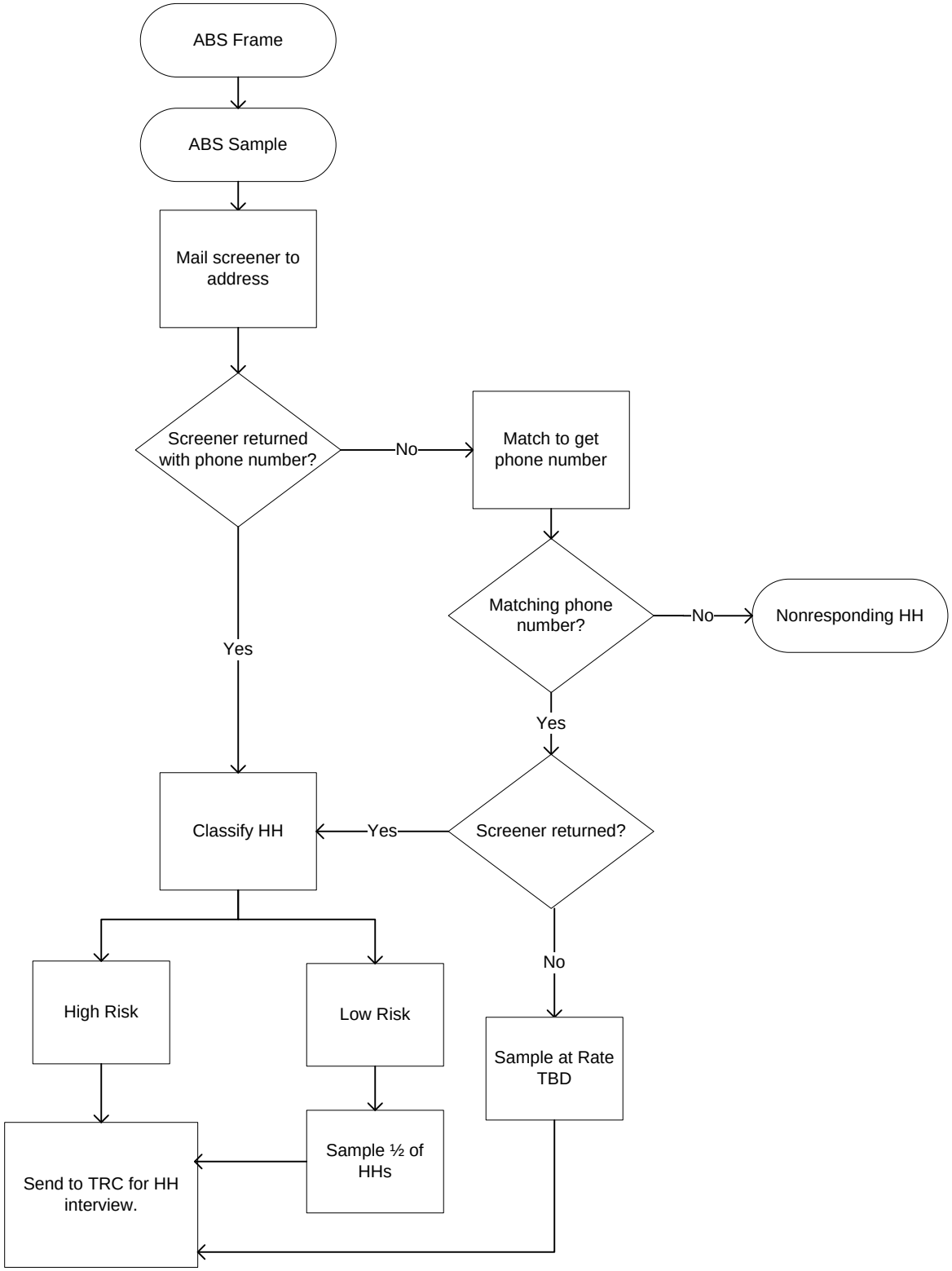

Table 1. Expected Sample Performance, NCVS-CS Pilot Test

	Two-phase ABS Hybrid		Telephone Harvest ABS	
	Rate	N	Rate	N
Occupied addresses sampled	88%	12,320	88%	11,000
Mail screeners sent	100%	12,320	52%	5,720
Screeners returned with phone	40%	4,928	30%	1,716
Total addresses with phone number	64%	7,934	64%	6,996
Addresses subsampled for follow-up	58%	4,620	100%	6,996
Household interviews completed	50%	2,310	35%	2,449
Individual interviews completed	1.35	3,125	1.35	3,313
Crimes reported:				
Property	18%	563	14%	464
Personal	2.4%	75	2.0%	66

Table 2. Comparison of design features, NCVS and Companion Survey

	NCVS	Companion Survey
Frame	Area	ABS
Clustering	Geographic	None
Sample Design	Rotating panel (7 waves)	Cross-sectional (1 wave)
Within-household sampling	All aged 12+	2 adults (aged 18+)
Initial contact	In person	Mail/telephone
Data collection	In person/telephone	Telephone
Reference period	6 months, bounded	1 year, unbounded

References

Biderman, A.D., Cantor, D., Lynch, J.P., and Martin, E. (1986). *Final report of the National Crime Survey redesign.* Washington, DC: Bureau of Social Science Research.

Fay, R., and Li, J., (2012). *Rethinking the NCVS: Subnational Goals Through Direct Estimation*, presented at the Federal Committee on Statistical Methodology Research Conference, Washington DC, January 2012.

Groves, R.M. and Cork, D.L. (2008). *Surveying victims: Options for conducting the National Crime Victimization Survey.* Washington, DC: National Academies Press.

Hindelang, M., Gottfredson, M., and Garofalo, J. (1978). *Victims of personal crime: An empirical foundation for a theory of personal victimization.* Cambridge, MA: Ballinger.

Karmen, A. (2007). *Crime Victims: An Introduction to Victimology.* Belmont, CA: Wadsworth.

Kindermann, C., Lynch, J., and Cantor, D. (1997). *Effects of the Redesign on Victimization Estimates.* Washington, DC: US Department of Justice, Bureau of Justice Statistics.

Lohr, S. and Brick,. J. M. (2011). Blending estimates from surveys with possible bias. Paper presented at Foundations and Frontiers: A Conference Celebrating the Contributions of Mary Thompson to the Statistical Sciences, Waterloo ON.

McDowall, D. and 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,* ed. J.P. Lynch and L.A. Addington. Cambridge: Cambridge University Press, 93-121.

McNamee, R. (2003). Efficiency of two-phase designs for prevalence estimation. *International Journal of Epidemiology*, 32, 1072-1078.

Penick, B. and Owens, M. (1976). *Surveying Crime.* Washington, DC: U.S. Government Printing Office.

Rand, M. R. (2008). Criminal victimization, 2007. Technical Report NCJ 231327. Washington, DC: US Department of Justice, Bureau of Justice Statistics.