INNOVATIONS IN THE ANALYSIS OF CRIME SURVEYS

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This paper presents several recommendations for the conduct of victimization surveys. It does not simply focus on their "analysis," for it would be mistake to hope that more sophisticated analytic methods can somehow overcome neglecting to gather key data elements in the first place. It discusses four ways in which the utility of large-scale household victimization surveys could be enhanced: by bringing geography back into the picture, improving the treatment of multiple victimization, more consciously constructing the surveys around a conceptual framework which enhances their explanatory power and substantive appeal, and not doing them at all.

BRINGING GEOGRAPHY BACK IN

A general feature of large-scale efforts like the US National Crime Survey (NCS) and the Australian Victimization Survey (AVS) that they deal with their respondents as a selection of atomized individuals, disconnected from the place from which they come. Their personal backgrounds, opinions, and experiences are related to one another in a self-contained analytical fashion that denies or disguises that reality. In the NCS there are many victims but there is no such thing as a "bad neighborhood." This is far removed from the everyday world of the rest of criminology; for the discipline -- as for the ordinary person on the street -- where things happen (along with who did them) is one of the surest ways of explaining why they happened. Area effects are strong: research on selected small areas which includes both household-level measures of crime avoidance and prevention and measures of area-level risks suggests that the effects of neighborhood vastly outweigh whatever difference that people can hope to make by acting in their own defense (Greenberg, et al, 1982). Contextual variables like area stability and cohesion, neighborhood unemployment rates, and residential mobility, generally explain victimization better than do individual-level measures of lifestyle (Mayhew and Hough, 1991). The atomized nature of mass surveys also severs the link between research and policy, for the relationship between peoples' experiences or opinions and features of the jurisdictions which serve them tend to disappear from view.

Place-Based Studies

There are several approaches to bringing geography back into the analysis of victimization surveys. The place-based approach of surveying individual areas with samples large enough to characterize victimization rates is the most obvious, and sometimes appropriate. Area-based programs often are evaluated using surveys. For example, Trevor Bennett's (1990) evaluation of Neighbourhood Watch employed interviews with 300-400 respondents in each of four areas -- two program neighborhoods, one control area, and one potential target of displacement. Research surveys often focus on selected neighborhoods to maximize variation on key area-level factors. For example, Taub, Taylor and Dunham (1984) selected areas with rising and falling real estate prices, in an examination of the impact of crime on residential investment decisions. For my book Disorder and Decline (Skogan, 1990) I assembled 40 such surveys from 7 cities; I believe this and Ostrom's 60-area telephone survey (see Smith and Jarjoura, 1988) are the largest collections of place-based crime surveys.

However, none of them was very useful for assessing victimization. Even in the highest-crime US neighborhoods personal violence visits individuals only infrequently within some reasonable time frame like one year. In my work surveys of 300-400 neighborhood respondents in not-very-desirable places, less than 0.5 percent of those interviewed typically report instances of sexual assault, less than 3 percent a robbery, and less than 2 percent a purse-snatching. In the last instance that translates into about four crimes per area, not a robust base for either studying victims or drawing conclusions about changes in victimization rates over time. Victim surveys require the brute force of numbers.

The brute force solution to the geography problem is to survey huge numbers of people in particular jurisdictions. One prominent examples are the US city surveys, which were conducted by the Census Bureau in the early 1970s, and paid for by the Law Enforcement Assistance Administration (LEAA). They aimed at interviewing everyone twelve years of age and older in 12,000 households in each of 26 cities; this would have yielded 687,000 interviews, if everyone had been at home. I have never been able to reconstruct exactly why these 26 were chosen. LEAA did spend millions of dollars in eight of them (the "High Impact Cities"), and there was a vague sense that the surveys would somehow help evaluate those expenditures. Others apparently were surveyed because they were large, or someone thought they were interesting. Another small city (Compton, California) was given the full survey treatment because a top administrator at LEAA thought it was an interesting place, but the results of that survey were never released.[1]

None of this was useful, or necessary. The major products of the US city surveys were two books by Michael Hindelang (1976; Hindelang, Gottfredson and Garofalo, 1978) describing patterns of personal and household victimization. They simply merged all of the data, and not a single analytic table in either book reports things separately by city; the summary chapters never mention city differences or effects. Statistical reanalyses by Miethe et al (1987) and others have dealt with this mass of data by drawing and merging samples of the samples for each city, but they also simply analyzed the resulting pool of data and ignored city distinctions. These studies all could have been done using national data. With the exception of an article on city differences in fear of crime (Liska, 1982) and a small group of articles contrasting victimization and UCR rates for the 26 cities, little ever came of these very expensive city-level collections. They also proved useless for evaluation purposes, despite the fact that LEAA had half of them resurveyed two years later. What the High Impact Cities did with their millions proved difficult to track, the Census Bureau's relentlessly standard survey instrument did not include any questions tailored to local conditions, and it proved hard to think of what could serve as a control group for Cleveland. The same confidentiality issues that shroud the national surveys applied to these as well (see below), so there was no link between area-based programs within the cities and the results of the surveys.

When it is possible to gather or retrospectively gather survey data for multiple places, the results can be striking. Figure 1 presents a plot of the relationship between neighborhood affluence and an index of how politely people report that they are treated by the police. The data are for 8,100 respondents living in 35 neighborhoods, and they demonstrate starkly the class bias

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[1] Compton was indeed an interesting place. The commercial victimization rate there was greater than 1,000 per 1,000 establishments, and as a result the Census Bureau's specially-written computer programs could not process the data. In 1992, part of Compton was burned down by rioters.

in this measure of the quality of police service, which is largely organized along geographical lines. The 35 surveys displayed in Figure 1 all were conducted to evaluate policing experiments; they measured victimization half-heartedly, and focused their attention on fear of crime, neighborhood satisfaction, and attitudes toward the police.

Figure 1: Area Affluence and Police Civility

Another place-based approach is to oversample selected areas, or at least make use of samples that naturally represent huge metropolitan areas. Mike Hough of the Home Office Research Unit spent considerable effort before the 1984 British Crime Survey (BCS) trying to arrange additional oversamples of cities and local council areas; he could not get any of them to foot the bill, but the success of police-sponsored city and state surveys in Australia suggests that the climate for oversampling may be better elsewhere. Large "natural" sample clusters are likely in nations with one or a few primate cities, rather than in decentralized countries like the US or Germany. In the 1988 BCS, enough respondents lived in the (former) Greater London Council area that I could compare their experiences with the police to that of Londoners surveyed by the Institute for Policy Studies some years before (Skogan, 1990b). This was facilitated by the fact that the BCS built studiously upon past research.

Places Like This

The "places-like-this" strategy recommends the use of analytic rather than "actual" geography. Respondents are classified by the kind of place in which they live; these can be used in turn to explain patterns of victimization and fear, or the surveys can be used to generate new data on those kinds of places.

Perhaps the best example of the places-like-this approach to geography is the ACORN component of the BCS. To develop ACORN ("A Classification of Residential Neighborhoods), a marketing firm clustered 40 variables for all 130,000 census enumeration districts in Great Britain. This clustering identified 38 distinctive types of places (for example, those dense with "furnished flats-mostly single people"). These in turn form 11 macro-clusters (the above example falls among "high status non-family areas"). Other examples of ACORN clusters include "villages with wealthy older commuters" (which clusters with other "affluent suburban housing") and "tenement flats lacking amenities (among those characterized by "older terraced housing"). Every British postcode is matched through its enumeration district number to a distinctive ACORN area, so that many kinds of data can be geocoded in this fashion. Every BCS respondent is tagged with an ACORN identifier.

The analogous system in the United States classifies all 36,000 postcodes into 40 clusters. These clusters summarize hundreds of measures of social rank, mobility, ethnicity, family life cycle, and housing style. They were originally formed using measures from the 1970 Census of Population, but postcode data have since been matched with the results of hundreds of large scale marketing surveys, television viewing diaries, lists of new-car buyers and the membership of mass organizations, and more recent Censuses, to considerably enrichen and refine the clusters. They range income from "Blue Blood Estates" (Beverly Hills, CA) to "Hard
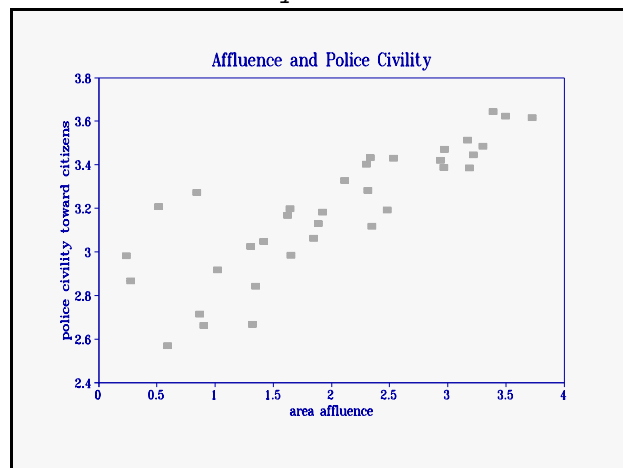
Scrabble" (the poorest rural settlements). The newest clusters identify concentrations of Asians, middle-class retirees in rustic villages, and upper-income black neighborhoods (cf, Weiss, 1989).

Not surprisingly, because the ACORN scheme summarizes a number of significant social and economic features of their life circumstances, BCS respondents' ACORNs are good predictors of a variety of crime-related phenomena. Those who live in similar circumstances (in "places like this") share a great deal in common. Area is linked in clear ways to victimization; in 1988 the poorest council housing areas had six times the victimization rate of better-off retirement areas (Mayhew, et al, 1989). ACORN is related to fear of crime; in 1984, 55 percent of residents of multi-racial areas thought it was likely that they would be a burglary victim, as compared to 40 percent in affluent suburban housing areas and 19 percent of agricultural areas (Hough and Mayhew, 1985). Participation in Neighbourhood Watch was highest in affluent suburban areas (24 percent), and lowest in multi-racial areas and the poorest council housing estates (7 percent) (Mayhew, et al, 1989). Relations between the public and the police were better in better-off areas; 30 percent of the residents of affluent suburban areas thought the police did a "very good job," but only 18 percent of those living in poverty stricken council housing estates shared that view (Skogan, 1990).

Figure 2 presents the distribution of these ratings of police performance for all eleven macro ACORN areas, illustrating the overall spread of those ratings.

Multi-Level or Contextual Models

Contextual models take geography into account in analytic but not very descriptive fashion. Rather than using surveys to describe areas or perhaps to array them analytically (as in Figure 1), they use places to characterize the environment within which each respondent is placed. Place becomes a measured causal factor entered in a logistic regression equation predicting individual-level victimization. While place-bases studies demand many respondents for each area, the ideal contextual design draws each respondent from a _different_ place.

Figure 2: ACORN Areas and Ratings of Police Performance
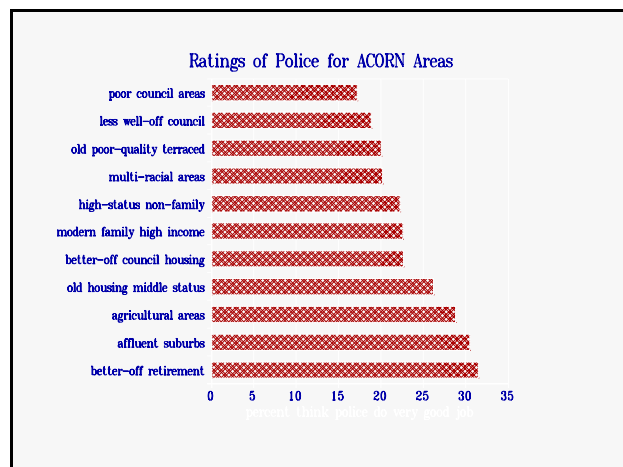
One common approach is to use the cluster sample design that is inherent in all "national" personal-interview surveys in order to "make" area-level data that then can be brought into the analysis. In national surveys, small geographical areas are first selected, and then groups of respondents are chosen _within_ those areas for interviewing. Because respondents are therefore clustered together rather than spread all over the nation, surveys have "sample design effects" which lower their accuracy. But this can also present opportunities for area-level analysis.[2] For example, Lynch and Cantor (1992) calculated the mean response of all the interviewees living in

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[2] National telephone surveys can be totally unclustered and thus have smaller design effects; alas, they are therefore totally without geography.

the same NCS segment (a small area from which individual units are sampled) to a question about whether "neighbors watch one another's houses when no one is at home," and entered this mean in each individual's
data. They used it as an indicator of the extent of "guardianship by neighbors," in order to predict whether or not respondents were burgled (they found guardianship helps). They also took advantage of the fact that interviews are conducted in NCS sample segments for 3½ years by processing more than 1,000,000 records to build up victimization rates for each segment; these were then used to typologize areas by their past dangerousness, to predict household victimization in a logistic regression model (past dangerousness hurt a great deal).

An example of a contextual analysis of fear of crime is presented in Table 1. It examines the results of interviews with 8,175 residents of 7 US cities (this is the same data source examined in Figure 1, analyzed at the individual level). Table 1 includes many of the individual-level factors that have been demonstrated to be related to fear of crime: safety is higher among younger males who have not been victimized, those who do not live with a family, and those who speak good English and own single family homes. In addition, Table 1 includes two area-level measures which were aggregated from the data itself for each of the 35 neighborhoods from which the respondents were drawn (there were an average of 234 respondents per area). These measure area levels of victimization and the percentage of the population which is nonwhite. As Table 1 indicates (note the highlighted variables), net of the measured personal factors, city dwellers of all backgrounds felt more fearful when they lived in high crime and minority neighborhoods.

This approach is not without its problems. For example, Sampson and Wooldredge (1987) used the responses of the 47-odd people interviewed in each BCS sampling area (electoral wards of about 5,000 residents) to characterize their ward, and then added this ward-level data to each respondent's data. Because they were asked about concepts close to the core of criminological theory these ward-level indicators were on the mark with regard to explaining victimization. However, 47 cases was a shaky basis for making those area-level estimates; the contextual data doubtless are very error-prone and biased their findings toward nonsignificance. The fact that those respondents shared the same ward-level scores introduced further error into the statistical analysis as well. In addition, contextual models assume that area-level measures are not simply surrogates for unmeasured or badly measured individual-level variables.

The alternative approach is to use independently derived area level data to characterize each respondent's environment. This typically comes from Census enumerations, and thus provide a firmer basis for estimating area-level variables (they will be based on all residents or a healthy sampling of them). However, these measures rarely are on the mark with regard to criminological theory. They serve well to represent poverty and inequality, but are hopeless for scoring areas on such factors as accessibility, guardianship, or cohesion. As a result the research is likely to understate area effects, due to concept misrepresentation and the underspecification of area-level variables because of their limited availability. The research may or may not reflect the theoretically appropriate level(s) of geography, a shortcoming they share with the aggregating-up approach to contextual analysis.

An important empirical issue is what kind of geography accounts for crime. Lynch and Cantor (1992) argue that at least two levels are involved: a) the immediate block face, which sets the design features which shape factors like access, visibility, and opportunities for natural surveillance, and b) a larger neighborhood context which supplies factors like the extent of income inequality, propinquity to commercial and recreational zones, and the strength of community organizations.

A substantial problem facing researchers pursuing either analysis path is the intractability of Census Bureaus with regard to data confidentiality. The best victim survey (the BCS) wisely refuses to let its Bureau get involved at all, and tenders the job to a private contractor. The Bureaus all refuse to release public micro-data with low-level identifiers that might compromise the confidentiality they guarantee respondents; in the US, that level is set at 250,000 residents. The "places like this" analytic strategy offers a way around this problem if Bureaus can be convinced to attach cluster-type identifiers to individual records, for particular places are not identified at all. The contextual model offers an opportunity for Bureaus to attach selected analytic variables to each record, although in my experience they like to argue that more than a few variables allows the identification of individuals' small areas and thus compromises their anonymity. Either approach inevitably stifles creative follow-on analysis of surveys, for their designers surely will fail to include analytic variables recommended by new theoretical perspectives.

A promising approach to environmental analysis is to make better use of interviewers in personal-visit surveys. In my surveys of big-city neighborhoods interviewers rate home security features on a seven-item check list, and stand at the front door and note whether they can see things like litter, graffiti, a broken window, a seemingly abandoned car, or three or more males gathered in a group. They also note land use features in the vicinity of the address, including the unit's location *vis* busy streets, cul-de-sacs, and business establishments open to the public. A recently-completed survey of Seattle by Terry Miethe (for a description see Miethe, 1991) asked respondents to review a long list of potentially criminogenic establishments and check off those located within three blocks of their home; these included schools, convenience stores, bars, shopping strips, motels, and fast food restaurants. Lynch and Cantor (1992) employed interviewer ratings of the visibility of each dwelling from the street to predict burglary victimization (it does not help, net of the dangerousness of the area).

ACCOUNTING FOR REPEAT VICTIMIZATION

Nowhere is the complex relationship between methodological issues and the substance of victimization research more apparent than in the domain of multiple victimization. The issues are that:

> 1) The methodological (and conceptual) problems involved in counting and sorting out those repeat incidents are many and complex.

> 2) A relatively small number of multiple or repeat victims account for a disproportionate share of the overall victim count.

> 3) Repeat victims present a unique opportunity for intervention, for they are readily identifiable and provide considerable leverage over the total count of crime.

The disproportionate influence of the distributional "tail" of repeat offenders on the total crime count is well known; less understood (because the data usually are not very adequate) is that repeat victims have the same impact. In the 1977 National Crime Survey (NCS), assault victims recalling more than one incident in a six-month recall period (a very short time frame) constituted 20 percent of assault victims but contributed 46 percent of all assault victimizations (Skogan, 1981). My own tabulations of the short-lived NCS commercial victimization survey indicates that victimization of business establishments was even more heavily concentrated among a small pool of repeat victims. Shapland, et al (1991) found that multiple victimization drove the total crime count in English industrial estates, and that on the worst estates businesses could expect to be victimized five times per year.

The influence of the distributional tail of victimization can be seen at the area level. In an important paper, Trickett and his associates (1992) explore the question of what makes a "high crime area" a high crime area. They contrast the effects of higher victimization prevalence rates (the percentage of persons or households in an area who are victimized) with the effects of higher victimization incidence rates (which add up the number of crimes, including repeats, and divide by the number of people or households in the area). They find that what makes "high crime areas" in England and Wales is that residents are repeatedly victimized; repeat victimization rises much more rapidly than the spread of victimization to new targets.

However, accurately assessing the magnitude of repeat victimization is a tricky business. Some repeat victims are so overcome by their plight that they are unable to separately enumerate or describe what has happened to them; ironically, their condition sometimes makes it impossible to account for them accurately in victim surveys. In the NCS these are called "series" crimes, and 13 percent of them involve an estimated 11 or more incidents (Dodge, 1987). The ability of victims to describe these incidents and estimate their number is extremely sensitive to differences in the skill and tenacity of interviewers, adding to the already-large interviewer effects which plague victim surveys. Repeat victims also move more frequently than others, and therefore are lost from most over-time studies (Reiss, 1977). Some examples of more frequent repeat victimizations include episodes of domestic violence, thefts from cars, petty thefts by nonstrangers, crimes that occur as a function of one's occupation, and schoolyard intimidation. Rather than fitting our model of crimes as discrete events, these more closely resemble ongoing circumstances or conditions of people's lives (Dodge, 1987).

The analysis of repeat victimization is also thwarted by the way the respondent's task a structured in a victim survey. The most accurate surveys only ask about incidents that have occurred in the recent past, in the case of the NCS within the past six months. This brief window in time probably cuts off a substantial amount of repetitive victimization (by some other definition) by artificially truncating the series. Only recurring interviews with a panel of respondents will reveal slow-to-unfold repeat victimization that are initiated near the end of a recall window.

Repeat victimizations also challenge the conceptual organization of victim surveys. For example, one detailed reading of a sample of series crimes revealed that "line-of-duty" crimes (repeat incidents generally attributed to a succession of strangers that the victim came into contact with while on the job) were the most common kind of repeat personal victimizations.[3] However, they frequently fell into several different NCS crime categories (some robbery, some assault, some personal thefts). Series crimes among separated or divorced marital partners included mixes of assaults, robbery, rape, theft, and burglary. In these instances the tail of repeat victimization for traditional crime categories would not appear very long. As is too often the case, the urge to produce victimization rates which match official crime categories -- a hopeless task on its face, but one which drives most surveys -- disguises the underlying processes which generate them (for an alternative, see Lynch and Biderman, 1984). Whether victimizations are "repeated" or not is a conceptual distinction even if there are multiple incident forms for a given respondent. More useful dimensions for typologizing repeat victimizations combined location (school was a big one), the relationship between the parties (thefts at home by people that

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[3] Prominent victims of series personal crimes are law enforcement personnel, security guards, social workers, nurses, teacher's aids, bartenders, and bus drivers.

victims knew were among the most common repeat crimes), and the role played by the victim at the moment (employee; dating partner; spouse) (Dodge, 1987; Dodge and Lentzner, 1978).

The "households touched by crime" concept used with great effectiveness in the NCS is an analytic tool which further increases the scope for repeat victimization. It classifies a household as a "victim" when anyone living there reports any crime incident. This ups the victimization count to encompass fully 25 percent of households, a very large figure (but down from 1975, when it was 32 percent) (Rand, 1991). Because so many and different things can happen to households classified in this fashion, multiple incident households are very common and it will be a challenge to identify useful patterns of repetitiveness in their experiences.

Various surveys do different things with their irreducible minimum of series incidents. The BCS uses the description of the most recent of them to classify them all by type, and then weights the series by its estimated frequency with an arbitrary cap of 5 (Mayhew, et al, 1989). The NCS, on the other hand, simply does not count them at all; the Census Bureau's stance is that, because they are too repetitive to account for separately, they count for nothing. As we have seen, these decisions can have a considerable impact on estimates of victimization rates, especially for high-crime areas. There are analytic procedures for coping with the problem. One approach would be to view individuals' reports of repeat victimization as "censored" at the high end by methodological fuzziness and caps on their frequency like that imposed by the BCS. Then the appropriate statistical model would be censored regression, also known as "Tobit" analysis (cf. Maddala, 1988).[4]

However, statistical patchups are never preferable to getting the data correct in the first place. Methodological research on the measurement of repeat victimization, procedural clarity and interviewer training to minimize inaccuracies in gathering reports about them, and the development of analytic categories that reveal rather than disguise their importance, clearly are called for. The BCS has always had fewer series incidents than the NCS because of the higher threshold standard of confusion it sets before interviewers can check off that box. The redesigned NCS is using new cues (such as, "were all, some or none of these incidents done by the same person(s)") to help respondents tell similar incidents apart. It also encourages the recall of repetitive events by asking if the offender had done anything else to them at another time, in every incident in which the victim has an idea who did it.

The opportunities for intervention presented by the tail of repeat victims are noted by Trickett et al.:

> ... if high crime areas are so because the proportion of residents who become victims of crime is high, the problem of crime prevention is predominately that of attempting to prevent a non-victim from becoming a victim. If high crime rates occur because of repeat victimisation, crime prevention should correspondingly focus on preventing people who have already been victimised from being victimised again. In many ways,

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[4] Reports of victimization are also censored at the high end because people fall victim before the onset of the recall window that surveys impose on them (usually six months or one year in the past), or immediately after they are interviewed. In some (perhaps many) instances classifying someone as a "nonvictim" is a distortion imposed by our survey procedures. As a result, our prediction models may be much better than they appear.

this is a less daunting prospect, notably because crime prevention effort may be focussed on a smaller proportion of people and places.

This research, in which Ken Pease has played a prominent role, is conceptually linked to a demonstration project in Kirkholt, near Manchester, which illustrates the leverage that repeat victims offer for crime prevention. The community had an extremely high crime rate: in 1985 about one-quarter of all homes were burglarized. The Kirkholt demonstration project began with the observation that the chances of one of those households being revictimized was more than four times that of a non-victim household being victimized. Among a sample of households burglarized in December, 1986, one half had already been victimized earlier in 1986 (Pease, 1991).

In brief, the Kirkholt program involved a series of opportunity reduction, target hardening, and surveillance programs targeted at recent burglary victims. Home security was upgraded, gas and electricity prepayment meters were removed, the formation of small "Cocoon Neighbourhood Watches" was encouraged among neighbors immediately adjacent to victimized units, and other standard services were offered to victims. The result: the Kirkholt burglary rate fell to 40 percent of its baseline level, and repeat victimizations virtually ceased (Forrester, et al, 1988). A two-year followup indicated that this decline in rates continued, perhaps in part due to the extension of the program to school and offender-based efforts. Over the duration of the evaluation burglary of dwellings in Kirkholt was off 75 percent and the program showed a net benefit-over-cost of £1.2 million (Forrester, et al, 1990).

There are many practical advantages of the Kirkholt approach. As Pease (1991) points out, because once-victims have a high chance of being targeted again in the near future (in Canada it's within six weeks; see Polvi, et al, 1990), offenders come face-to-face with the program with greater certitude than most prevention strategies. Politically, the program serves the most legitimate clients, for whom even fairly expensive services can be justified. In practice it also targets the most socially and economically vulnerable sectors of society, and (as documented by Trickett, et al) high crime areas. As with most crime prevention programs, the issue of whether crime was reduced or displaced loomed over the Kirkholt success story. In general I think this is an over-stressed concern, for the bulk of carefully-evaluated prevention projects have had such woefully-small effects that it not clear that we are distressing offenders enough for them to even notice, much less go elsewhere. Pease (1991) quipped that at least displacement "shares the agony around," a view which he defends more seriously in a major review of displacement issues (Barr and Pease, 1990).

BRINGING SUBSTANCE BACK IN

There are few good reasons for conducting large-scale surveys just to estimate the victimization rate. They are expensive and consume resources that probably could be better spent on more focused studies. There is little interest in the raw numbers. Because of start-up errors and software bugs there are four different NCS estimates of US victimization rates for 1973; three were published with appropriate *mea culpas*, but it is was embarrassingly obvious that there was no interest in the fact that numbers differing by hundreds of thousands of crimes were appearing here and there. These numbers only come to life as part of a trend line like that established by the NCS, the BCS, and the Dutch national surveys. On their face they are boring, and have little policy utility.

Short of lengthening a trend or evaluating a program, victimization surveys make their impact by the force of their content. This is driven by the conceptual focus taken by the survey, and successful surveys have strong foci. Perhaps the first in any jurisdiction can get away with a

"gee-whiz, look at that" approach to familiar descriptive topics like rates for demographic groups, reporting to the police, and levels of injury and loss, but those cannot for long sustain the interest of relevant constituencies.

The dominant explanatory paradigm for large-scale victimization surveys has been the routine activity/lifestyle model that underlies the BCS and (in sketchy fashion) the International Crime Survey (Van Dijk, Mayhew and Killias, 1990). It marks a sharp departure from offender-based models of crime, and has been the focus of innumerable research reports. The NCS lacked any explanatory variables before the introduction of a redesigned questionnaire in 1989 (and the new data have yet to be released), so during the 1970s its individual-and-household orientation of victim surveys led to a series of books and articles that perhaps involved the largest existential leaps of faith in modern social science. Researchers were forced to guess the implications of standard survey demographics in order to make sense of the findings. Observation: blacks are twice as likely as whites to be robbed. Explanation: because they are black, and because in the US neighborhoods tend to be racially segregated, blacks are very likely to live near other black people, and that's risky because blacks offend at a higher rate than do whites. Observation: burglary rates are highest at the top and bottom of the income distribution. Explanation: poor people tend to live near poor people, and etc; wealthy victims are victimized, on the other hand, because they are attractive targets with plenty to steal. Observation: burglary rates are higher in households with more people living in them. Explanation: routine activity theory posits that the "absence of capable guardians" facilitates victimization, so this does not fit the theory so well.

Victimization surveys also gain substantive import through add-on modules and rotating emphases on specific issues. For example, the 1988 BCS included a series of new questions elucidating the important issue of crime in the workplace (finding, for example, that 70 percent of personal thefts from people in the labor force took place at work). A fifty percent subsample of 1988 BCS respondents were also administered a 26-page add-on questionnaire which gathered an immense amount of detailed information concerning contacts between the police and the public. The 1989 NCS included an add-on set of questions on school crime that was administered to students in sample households; an earlier supplement was used to provide estimates of the seriousness ratings of a long list of types of crime. In 1992, a special supplement was administered to a large add-on sample of 12-19 year olds for the BCS; they previously were excluded from the survey. The 1992 BCS also includes a self-report questionnaire asking about offending and drug use; it was handed over to be filled out during the interview session, and was carried away in a sealed envelope to protect the anonymity of the respondent. That survey also includes an expanded section of obscene telephone calls, which research by Pease (1991) indicates is rated just as seriously as burglary by its largely female victims.

GOING WHERE THE CRIME IS

My final recommendation is to think seriously about fielding alternatives to traditional household-based victim surveys. Empirical and theoretical contributions to victimization research would be easier to make in studies of crime against commercial establishments, and there are several reasons to think that commercial victimization grants us greater leverage on the over-all crime rate.

Commercial crime constitutes a large percentage of the total in several important categories. In the United States, the FBI reports that "nonresidential" incidents currently constitute about one-third of all reported burglaries. As this implies, commercial establishments are more at risk than individuals or households, for there are far fewer targets for commercial

than residential burglary. In 1976 (the last year for which national commercial victimization data were collected by the US Census Bureau), commercial burglary rates were almost twice as high as that for residences, and commercial robbery rates were six times those for individuals (National Criminal Justice Information and Statistics Service, 1976: Table 2). Victimization surveys in New York City found that more than one-third of all robbery and more than one-half of all burglary there in 1975 was directed at commercial organizations (Skogan, 1976).

Commercial crime also has features that should make it amenable to intervention. Its targets (for the kinds of crime considered here) are fixed in space, and are open or empty at predictable intervals. They can be watched and wired, and redesigned to reduce their vulnerability to intrusion. As noted above, they are also relatively few in number relative to the number of incidents in which they are involved. In some categories, the "tail" of repeat commercial victimization is quite long, suggesting that attention to repeat victims could have considerable leverage on the over-all crime count. The potential targets of commercial crime would appear to have both the motivation and the resources to do something to reduce their risks, if appropriate measures can be identified. Research on commercial-crime offenders suggests that they are more likely to be repeat, relatively professional criminals, strangers to the victim, and (in the case of robbery) they overwhelmingly employ guns; they may be more likely to qualify for the close attention of the criminal justice system, if they are apprehended.

Differential vulnerability linked to their business operations may underlie the large differences in victimization rates that has been observed among commercial establishments. Hindelang (1976) found that retail stores were by far the most likely victims of commercial robbery, and that among them gas stations, grocery and liquor stores, and drug stores stood out. (A study of reported commercial robberies in Oakland found that gas stations, liquor stores, and small grocery stores alone accounted for 50 percent of all commercial robbery; see Wilcox, 1973.) Victimization by commercial burglary was also most common among retail establishments, perhaps because they so obviously house objects of value. Several studies have noted apparently high rates of crime against small convenience grocery stores. Ciale and Leroux (1983) think this is due to their long hours of operation and small number of employees typically on duty. Entering and exiting targets of this sort is also easy, and does not attract attention. Because of the opportunities they present, commercial establishments may be even _more_ at risk than other inhabitants of the same area; in Britain, small inner-city shopkeepers face twice the risk of burglary for residents of such areas, and are far more likely to report being assaulted (Ekblom and Simon, 1988).

Opportunity factors like the "ease of access" to retail commercial establishments may enhance their vulnerability to crime. Both Lynch (1987) and Collins, Cox and Langan (1987) found that working in an establishment that was open to the public was linked to higher levels of personal victimization at the workplace. Nasar (1981) compared sites of commercial burglaries with a random sample of business establishments and found that victims were more often located at the corners of major arterial streets; he hypothesized that this was due to the ease of approach and escape these settings afforded. Baumer and Carrington (1986) compared bank robbery sites with a random sample of remaining bank offices in Indiana, to explore the factors that predicted victimization. They also found that being situated on a major street was a risk factor, along with being located in a commercial strip or shopping mall, and being a small branch rather than a main office. They interpreted all of these as ease-of-access factors. Their interviews with incarcerated bank robbers indicated that another important factor in target selection was parking -- like other consumers, bank robbers do not want to walk very far to their car.

In the United States, one great impediment to conducting national commercial victimization surveys is the absence of a sampling frame comparable to that available for

residential addresses. Commercial surveys were originally conducted as part of the National Crime Survey program, but were discontinued after 1976 in order to save money and because errors in estimates made from the survey were very large due to sampling problems. In other nations commercial sampling frames may be readily available, and -- for the same reasons that they are prone to victimization -- easy to survey.

At a minimum, household-based surveys could be designed to capture data about commercial offenses (or at least offenses at commercial locations) in which individuals are involved as victims. The archetypal instance is commercial robbery, which always leaves an (often shaky) individual victim behind. Sadly, the NCS deliberately <u>excludes</u> victims of such offenses from consideration entirely, based upon its fruitless quest to mimic the Uniform Crime Report. However, both Lynch (1987) and Collins, Cox and Langan (1987) found that having a job requiring frequent contact with the public is linked to higher risk of victimization, and Lynch reports that having a job that requires handling money puts people at higher risk of on-the-job victimization. As noted above, the 1988 BCS found that 70 percent of personal thefts from people in the labor force took place at work, indicating the importance of understanding more about crime in the workplace.

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Table 1: Fear of Crime in Neighborhood Context

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                    Neighborhood Safety After Dark

Multiple R                .42960
R Square                  .18455
Adjusted R Square         .18272
Standard Error            .97678

Analysis of Variance
                DF     Sum of Squares      Mean Square
Regression      17        1633.53080         96.09005
Residual      7565        7217.68851          .95409
F=100.71385  Signif F =   .0000
```

Variable	B	SE B	T	Sig T
NONWHITE	**-.754592**	**.053716**	**-14.048**	**.0000**
VICTIMIZATION	**-.047337**	**.022704**	**-2.085**	**.0371**
HISPANIC	.026670	.049726	.536	.5917
BLACK	.109645	.040426	2.712	.0067
ELDERLY	-.211538	.047330	-4.469	.0000
FEMALE	-.520280	.023783	-21.876	.0000
AGE	-.005968	.001217	-4.906	.0000
ROBBED	-.351659	.067395	-5.218	.0000
BURGLED	-.228638	.032392	-7.059	.0000
UNEMPLOYED	.112682	.036703	3.070	.0021
LIVE ALONE	.082083	.025650	3.200	.0014
POOR ENGLISH	-.105714	.027607	-3.829	.0001
SINGL FAML HOME	.062137	.030031	2.069	.0386
HS GRADUATE	.114160	.026701	4.275	.0000
HAVE CHILDREN	-.050418	.026064	-1.934	.0531
RESIDENCE	-2.88956E-04	1.13618E-04	-2.543	.0110
RENT HOME	-.070989	.031453	-2.257	.0240
(Constant)	3.501022	.075988	46.073	.0000