



## ABSTRACT

This study looks at residential burglary as measured by the three countries' national crime surveys in the early 1980s. Attention is paid to differences in survey design affecting measurement. For five of twelve major differences identified, 'hard' adjustments were made through data re-analysis to improve comparability; for the remainder 'soft' adjustments were made, using methodological literature. Levels of burglary offences recorded by the police were set against adjusted survey results.

Adjusted survey data somewhat altered the picture of burglary observable from unadjusted data, and the lessons of this for comparisons of survey data are discussed. Nonetheless, US householders remained more vulnerable to burglary than those in Canada, and England. Police figures showed early 1980 residential risks to be similar, reflecting differential levels of and trends in recording and citizen reporting. (Lower US reporting was confirmed by survey data).

Other findings were: levels of attempted burglary in the three countries are more similar than completed offences; the US has more 'walk-in' burglaries than Canada; burglary seems more 'suburbanized' in the US than in England; and, on police figures, non-residential burglary is a lesser problem in the US. These findings are seen mainly to reflect differences in the structure of opportunities available to burglars.

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## SECTION I

### INTRODUCTION

This report looks at residential burglary in the United States, Canada and England and Wales (England hereafter). It draws principally on data derived from the US National Crime Survey, the Canadian Urban Victimization Survey, and the British Crime Survey. Measurement issues are a main focus, and here the report does three things. Firstly, it documents the processes of comparing existing crime survey information. Secondly, it adds to the very small number of comparative studies which have used crime (or victim) survey data adjusted to take account of differences in survey design. Thirdly, it contrasts adjusted survey figures on residential burglary with police statistics to show how the picture of burglary levels and trends differs according to which index is used.<sup>1</sup> The report illustrates again the problems of making sound comparisons of police data. More important, it shows that, when turning to the alternative of surveys, differences in their design also jeopardizes straightforward comparisons. The main thrust of the study is on measurement, but substantive findings emerge, which are taken up in Section IV.

#### MEASURING CRIME IN DIFFERENT COUNTRIES

Documenting levels and trends in crime in different jurisdictions has been seen to promise 'performance indicators' for criminal justice practitioners, and to offer a basis for testing theories about the causes and prevention of crime. Some case studies and historical analyses apart, most comparative exercises have relied on police statistics, even though the limitations of these are well-known. They cannot enumerate crimes which do not get into police records, while comparability can be threatened by differences in culture, law, police practice, and the classification, definition and counting of offences. The consensus among many criminologists about police figures is that 'league tables' of which countries have most crime will be inaccurate, though more sound may be comparisons of trends in crime, and analyses of broad crime levels in terms of major

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1. In legal terminology, burglary refers to entering a building as a trespasser with the intention of committing a crime, not necessarily theft. The survey definitions reflect this, but cover break-ins to residential properties only. Police figures for the three countries distinguish between break-ins to residential properties, and those involving non-residential targets.

social and economic variables (eg, Krohn and Wellford, 1977; Newman, 1981; Stack, 1984).

### CRIME SURVEYS

Crime surveys offer alternative information about crime. They ask representative samples of the population to recount offences of which they have been victim over a given period, and grossed up their results can give national estimates of the extent of these offences. Surveys repeated over time can also help show crime trends. The first major crime surveys were carried out for the United States President's Commission on Crime in the mid-1960s, leading to a program of national and city-level surveys in the United States in 1972. There have been several changes to this program since then, but a very large household survey - the National Crime Survey (NCS) - remains. This is the survey front-runner in terms of size, complexity and the methodological work it has generated. Towards the end of the 1970s, other countries began to introduce surveys, which by now have been conducted at national level in over a dozen countries, and at a more local level in many others. Canada conducted its first major survey in Greater Vancouver in 1979, followed in 1982 by a more extensive survey of seven major urban areas - the Canadian Urban Victimization Survey (CUVS). In Britain, the first survey - the British Crime Survey (BCS) - was conducted in 1982, with a second sweep following in 1984.

As an alternative to police figures, crime surveys have various limitations which have been thoroughly aired (see, Skogan, 1981; Sparks, 1982; Block and Block, 1984). They are best suited to uncovering crimes which have clearly identifiable people as victims: they cannot easily count, for instance, crimes against organisations, or 'victimless crimes'. Respondents also fail to recount - deliberately or through forgetfulness - all crimes they have experienced within the so-called 'recall' or 'reference' period; and they are prone to draw in incidents which occurred before, or after this period. There are various response biases, too: for instance, those not successfully contacted may well be the most heavily victimised, while among those who *are* interviewed, some - eg, better-educated respondents - may remember more incidents, or choose to define more incidents as victimizations. Based on a population sample, too, survey findings are subject to sampling error, particularly large for relatively rare crimes, and for smaller surveys. These problems have been assessed mainly in terms of individual surveys, especially when survey results have been put alongside police figures. For comparing different surveys,

however, these - and other - measurement issues gain more significance insofar as they can be *differentially* influenced by the way a survey is designed and administered. Many national surveys have drawn on the NCS, replicating much of its questionnaire and many of its basic features. Even so, there remain differences in sampling, field procedures, coverage, offence definitions, and so on, all of which jeopardize simple comparisons of the amount, and type of victimization that a survey has captured.

Nonetheless, the apparent potential of survey data for offering an alternative image of crime in different countries is inescapable. Most important, surveys cover - at least for the crimes they measure - offences whether or not reported to, or recorded by the police. They also document rates of reporting which may explain different levels of recorded crime, and they typically measure the cost, impact and characteristics of criminal incidents. Moreover, in providing information at the level of individual offences, survey data allow more scope for standardising coverage of different types of incidents in a way which aggregated police statistics precludes. Finally, as surveys collect demographic, and sometimes 'lifestyle', information, they allow comparisons across country of whether victimization risks are concentrated on the same, or different groups.

Comparative work using survey data has not been extensive. Surveys designed to be similar have attracted some attention. Mayhew and Smith (1985), for instance, looked at results from the first BCS which was conducted in Scotland as well as England and Wales. Comparisons have also been done of surveys (sometimes small ones with restricted coverage) carried out since the early 1970s in the Scandinavian countries (eg, Hauge and Wolf, 1974; Wolf, 1976; Sveri, 1982; and Arooma, 1986). More local surveys, again designed with comparability in mind, have received some attention too. A first was carried out by Clinard in Zurich, using an early NCS questionnaire (for results, see Clinard, 1977: 1978). Similar questionnaires were used in surveys in Stuttgart and Gottingen in 1973/74 (see Kirchhoff and Kirchhoff, 1984). Companion mail surveys were conducted in Baden-Wurttemberg (Federal Republic of Germany) and Texas (see Teske and Arnold, 1982). Some other comparative work has focused on particular aspects of victimization. Notably, Skogan (1984) drew on several surveys to examine reporting of crime to the police. And Van Dijk and Steinmetz (1983) have considered the relationship between 'lifestyle' factors and crime on the basis of the Greater Vancouver and Dutch surveys.

Using the independently organized, mainly national surveys as an international index of crime has been more problematic. Differences in design mean that sound comparisons need survey data to be directly manipulated to improve consistency. Access to these can be one problem, but a greater one is that the work is difficult and time-consuming. Richard Block has done some of the most complex work by reanalyzing 1976 NCS data to match better the 1977 Dutch survey (Block, 1984), and he has more work in hand, drawing on the NCS, the BCS and the Dutch surveys (Block, 1986). His 1984 study showed that street robbery rates were similar in the US and Holland, burglary rates some five times higher in the US, while street assaults and wallet/purse theft were actually higher in the Netherlands.

Some other authors have compared surveys without any or much attempt at standardization. Braithwaite and Biles (1980) compared rates from the 1975 Australian survey with those from the 1975 NCS. Robbery, theft and burglary were shown to be higher in the US (burglary twice as high), though assault and rape were on a par, and motor vehicle theft rates were lower in the US. Looking at offences against city residents in 1977 as measured by the Greater Vancouver Survey, the NCS and the Dutch survey, Van Dijk and Steinmetz (1983) found burglary rates to be again five times higher in the US than in Holland, though there was less difference for Canada; US car theft was lower than in Canada, though higher than in Holland; rates of wallet/purse theft were much higher in the US than in Canada, and - going against Block - than in Holland. Hough (1986) provides some tentative international comparisons of violent crime. He found robbery to be only slightly higher in the US than in Canada, Holland, or England, though not assault, which Hough saw as particularly difficult to compare. Finally, Breen and Rottman (1985) made some comparisons between the first BCS, the 1982/83 Irish crime survey, the NCS and the Dutch surveys. Rates of burglary and vehicle theft were shown to be higher in Ireland than in England or Scotland; Irish burglary rates, though, were well below those in the US, whereas vehicle theft rates were said to be higher than in the US, Holland, and Great Britain.

#### **THE PRESENT STUDY**

This study looks at one offence - residential burglary - as measured in three important national surveys. More attention than has been the case in other comparative work - even Block's - is paid to survey differences which are likely to affect the measurement of



levels and patterns of burglary. Survey data are reanalyzed to take account of five differences to present an 'adjusted' picture of burglary, while the likely effect of some remaining differences - which cannot be adjusted for - are also taken into account.

Survey data on burglary for the three countries are not equally extensive. Annual national burglary estimates are currently available from the NCS for the period 1973-1985. For Canada, the best data are from the 1982 CUVS which measured victimization in seven major urban centers in 1981 (see, eg, Ministry of the Solicitor General, 1983). For England, national estimates are available from two sweeps of the BCS - pertaining to 1981 (Hough and Mayhew, 1983) and 1983 (Hough and Mayhew, 1985). To these can be added some earlier estimates of burglaries with loss derived from the General Household Survey (Home Office, 1982). The size of the NCS is considerably greater than the other two surveys (and than every other survey). For the majority of the period since 1973, approximately 132,000 members of about 65,000 households have been interviewed every six months. The sample in the 1982 CUVS was second largest, with one person interviewed in each of some 61,000 households. In comparison, the BCS is small: in England and Wales some 11,000 people (again one respondent per household) were interviewed in each of the two surveys.

The study also draws on police statistics, to see how the three countries vary according to which crime index is used. The problems of comparing police statistics should be less serious here than in wider exercises. Only one offence is compared, for three countries with more rather than less socio-economic similarity, and with reasonably comparable criminal justice systems. It was also possible, through liaison with personnel involved in the compilation of the statistics, to check for differences in definition and classification. Nonetheless, to anticipate one conclusion of the study, police figures for each country provide a less comprehensive measure of burglary than survey figures, tapping as they do offences outside the police count. Moreover, for an analysis of trends in burglary, survey figures, when they are available over time, may again be more telling than police figures, which can be influenced by changes in reporting and recording practices. For comparative purposes, survey figures lose some of their advantage in that design differences undermine tight comparability. Nevertheless, if some standardization is done, they can provide a corrective to police statistics.

A comparison of burglary starts with some advantages. First, it is a relatively common, and clearly defined crime, probably rather easier to measure than some other offences: for instance, sexual crimes and assaultive violence (eg, Sparks, 1982). Secondly, in all three countries, burglary has a similar lay meaning; and - more to the point - in survey and police terms, offence definitions and 'hierarchy' rules are largely comparable, with the differences *on the face of it* being reasonably identifiable. Thirdly, for comparing survey and police figures, the problem is avoided of having different counts for crimes where more than one person is victimized: in both sources, burglary is treated as having one victim.

The focus on the broad problem of 'residential burglary' here reflects the fact that it is a distinct entity from the point of view of those who experience it, fear it, or want to prevent it. However, in reality burglary takes many forms, involving different targets, methods used, and purposes. This study offers few hard conclusions about why burglars operate the way they do, and how they choose their targets, though some speculations are made in Section IV. One point made is that considering what 'best' opportunities that each country offers to potential burglars may help understand levels and patterns of risk better.

#### **PLAN OF THE REPORT**

Section II starts with design differences between the three surveys, and then compares recent levels - and some patterns - of burglary as they appear from unadjusted results on the one hand and data which takes account of survey differences on the other. Section III turns to a comparison of burglaries recorded by the police in the three countries over the period 1972-1984. For the US and England, it also compares trends in burglary as evidenced by survey results. Section IV highlights the lessons of the study for comparative work using police data and - in particular - survey results. It also draws the results together to try and say something substantive about how each country is faring on the burglary front, and what some major differences in their burglary problems are.

## SECTION II

### BURGLARY ASSESSED BY THE CRIME SURVEYS

The comparisons below of residential burglary in the US, England and Canada draw on the following: (i) for the US, National Crime Survey (NCS) data for 1982; (ii) for England, British Crime Survey (BCS) results for 1981 and 1983 (the data pooled to increase stability); and (iii) for Canada, results from the CUVS for to 1981. A distinction is made between *completed* burglary offences (ie, involving entry into the home), and *attempted* ones. The CUVS and NCS make a further distinction between completed offences involving force, and those which do not; there is some discussion of differences in patterns in this regard.<sup>1</sup> Levels of reporting to the police in the three countries are also considered. As will be seen, firm conclusions about 'real' differences between the three countries in levels and patterns of burglary are complicated by design 'noise'.

#### SURVEY DIFFERENCES

The major survey differences which impinge on the measurement of burglary fall into two groups. The first comprises five differences for which 'hard' adjustments have been made through the manipulation of data from the three surveys. Each difference is explained below, with more detail in Appendix A on the approach taken in each survey, and in Appendix B on their independent effect on unadjusted rates. For the second set of seven differences, direct adjustment to survey data was not feasible, either because the structure of the surveys precluded it, or because the necessary information could only have been obtained with totally disproportionate effort. These differences are set out later, with some conclusions about their possible effect on survey measures.

#### DIFFERENCES ACCOUNTED FOR

##### i. *Hierarchical classification procedures*

Procedures differ somewhat as regards incidents which can be considered as 'essentially' burglaries, but which - because they involve a further type of offence - are reclassified

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1. The BCS does not distinguish between 'forcible' and 'no-force' entries. Nor is it possible, because of questionnaire differences, to classify BCS break-ins into those with and without force sufficiently well to match the NCS and CUVS

under another heading. Briefly here, more burglaries involving an assault on a victim are reclassified in the NCS and CUVS than in the BCS, depressing American and Canadian rates relative to BCS ones, albeit not markedly. Reclassified burglaries in all three surveys were re-included as burglaries (for a similar analysis of NCS burglaries, see Rand, 1985).

ii. *Series counting*

'Series' offences are repeated offences of a very similar nature which have consistently caused problems because (i) they are often non-discrete events and therefore different from usual offences; (ii) they cannot always be dated accurately; and (iii) victims may not remember accurately how many times they occurred (cf. Paez and Dodge, 1982). In the BCS, series incidents are accommodated in published rates estimates. In published NCS rates, series incidents are excluded entirely, thus depressing NCS rates relative to BCS ones. In the CUVS, the most stringent conditions for series incidents apply (they had to comprise at least five similar offences) but they are counted as one for rate purposes. This again depresses CUVS rates relative to BCS ones, though to a lesser extent than with the NCS.

In the adjustments made, BCS series counting procedures were applied: ie, in the NCS, series incidents were counted at face value between three and five, while series containing more than five incidents were counted as five. In the CUVS, series incidents were all counted as five. BCS procedures were adopted mainly because data from the other two surveys could be recomputed to match these best, though they also show most markedly the effect of a decision to take fuller account of series. The effect of some alternative treatments of series offences is shown in Appendix C.

iii. *Types of property covered ('outbuilding' burglaries)*

Each survey includes burglaries to the household's primary residence, second homes, and garages attached to these premises. However, 'outbuildings' burglaries - ie, break-ins to detached structures on the housing plot - are included in the NCS and CUVS, whereas they are not in the BCS. This suppresses BCS rates relative to NCS and CUVS ones, quite substantially. To adjust for this, incidents in the BCS classified as outbuilding burglaries were included with dwelling burglaries.

iv. *Respondent eligibility for reporting household crimes*

In the NCS, all age-eligible members of the household are interviewed, as against only one respondent per household in the BCS and CUVS. Each survey designates one respondent (the household respondent) to report on household crimes such as burglary. Even so, the NCS design allows some additional burglaries to be picked up from 'secondary' (ie, non-household) respondents, inflating NCS counts. To account for this, burglaries which emanated from secondary respondents were excluded from the NCS.

v. *The treatment of no-force attempts*

The surveys differ in whether no-force attempts are counted as burglaries. Briefly, the NCS discounts no-force attempts, whereas the CUVS includes them. The relevant questions in the BCS differ from those in the NCS and CUVS, and classification procedures are correspondingly different. Nonetheless, the way the BCS handles no-force attempts is closer to NCS procedures than to CUVS ones. To adjust for this difference, no-force attempts have been excluded from Canadian figures.

Individually, the adjustments to account for these five design differences differ in their effect on burglary rates; and they have *differential* effects on completed and attempted offences (see also Appendix B). The single most important adjustment was to BCS data, to take account of the exclusion of outbuilding burglaries: this increased the completed burglary rate by 40%, though attempts were much less altered (perhaps because outbuildings break-ins are usually successful because the premises are more likely to be unlocked.) The second most independently important adjustment was the exclusion of no-force attempts from the CUVS data, which reduced the rate of attempts by 22%. Of third importance was applying BCS series counting principles to the NCS which raised the NCS total burglary rate, for instance, by 14%. The exclusion of secondary respondents from the NCS was also of some significance, depressing the unadjusted NCS total burglary rate by 12%. Using the expanded definition of burglary to count all 'essential' burglaries made a comparatively small difference, increasing the NCS total burglary rate by 5% for instance, and the CUVS rate by 7%. Simultaneously applying the adjustments to each survey's data produces, as will be seen, a generally weaker effect on overall rates, since the adjustments counterbalance each other.

## FIRST ADJUSTMENT RESULTS

### *The US and England and Wales*

Because of the urban focus of the CUVS, its burglary rate sits uncomfortably with the national estimates from the NCS and BCS. Presented first, then, are unadjusted and adjusted national rates of burglaries per 10,000 households for the US and England (Table 1).

### TABLE 1 ABOUT HERE

Unadjusted rates show householders in the US to be much more at risk of burglary. The difference in rates stems in particular from much higher US rates of completed offences. The level of attempted burglary was not dissimilar, reflecting the relatively greater proportion of attempts in England (40% of all burglaries) than in the US (24%). The net effect of the adjustments made is to narrow the gap between the US and England. This is particularly so for completed offences because of the considerable number of completed outbuilding burglaries added into the BCS. For the NCS, the effect of BCS series counting and adding in 'essential' burglaries (which together would have increased the unadjusted overall rate by 20%) is much offset by excluding burglaries reported by secondary respondents.

### *The US, England and Wales and Canada*

The main difficulty with a comparison with Canada is deciding on the appropriate urban elements for the NCS and BCS, given that the population covered in the CUVS is not clearly defined.<sup>2</sup> The best option seemed to be to draw on respondents who lived in the following areas:

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2. CUVS sampling was meant to cover one city in each region (an eighth city was dropped), the catchment area being defined by police regions. The areas covered were: Greater Vancouver, Edmonton, Winnipeg, Toronto, Montreal, Halifax-Dartmouth, and St John's. Large cities were over-represented though the sample also covered outlying areas. St John's, Newfoundland, was the least urban city covered, though with weighting procedures the contribution of St John's to the overall burglary rate was small. The number of households in the seven cities was 2,424,900, comprising 36% of the 6,743,000 households in urban areas in 1981, and 30% of the total number of households in Canada as a whole.

Table 1

NATIONAL BURGLARY RATES IN THE US AND ENGLAND AND WALES:  
UNADJUSTED AND ADJUSTED DATA FROM THE NCS AND BCS

Rate per 10,000 households						
	UNADJUSTED DATA			ADJUSTED DATA		
	Completed	Attempts	Total	Completed	Attempts	Total
<i>US (1982)</i>	591	191	782	614	198	812
<i>England (1981/3)</i>	271	178	450	380	186	566
<i>US versus England</i>	+118%	+ 7%	+74%	+62%	+ 6%	+43%

Notes:

1. The adjustments made are as follows:

- BCS: i. 'Expanded' definition of burglary.  
ii. Outbuildings burglaries added into domestic burglaries.

- NCS: i. 'Expanded' definition of burglary.  
ii. Series incidents counted as in the BCS (ie, counted at face value between three and five; series of more than five counted as five.)  
iii. Victimization reports from 'secondary' respondents excluded.

2. Unadjusted rates for the US are derived from *Criminal Victimization in the United States, 1982* (Bureau of Justice Statistics, 1984). Unadjusted data for England comes from the pooled samples of the 1981 and 1983 BCS.

- i. In the US, central cities in Standard Metropolitan Statistical Areas (SMSAs);<sup>3</sup>
- ii. In England, inner cities and metropolitan areas.<sup>4</sup>

Table 2 shows adjusted and unadjusted rates of burglary for the CUVS and for city areas, as defined, covered by the NCS and BCS. On unadjusted figures, completed burglaries in US cities were 20% higher than in Canada; they were 76% higher than in cities in England. Compared to cities in England, Canadian completed burglary rates were nearly half as high again. For attempted crimes, on the other hand, unadjusted figures suggest that householders in the US faced lower risks of attempted burglary than those in both Canada and England. The rate of attempts in Canada was a little higher than in England.

TABLE 2 ABOUT HERE

The adjustments alter the picture. While the overall gap between the US and Canada remains the same, there is a change in the figures for completed and attempted offences. The US attempts rate is now higher (because of the exclusion of CUVS no-force attempts), though the rate for completed offences falls. The US lead over England again falls substantially for completed offences. The Canadian lead over England for completed offences also lessens. The risk of attempts in England is now higher than in Canada.

*Forcible and no-force entries*

The effect of the adjustments on CUVS and NCS rates of forcible and no-force entries into the home is shown in Table 3. On unadjusted figures, the US clearly has a greater problem of no-force entries than Canada; rates of forcible entries are not dissimilar. Adjusted figures reduce the difference in no-force entries, though far from eliminate it. For the

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3. An SMSA is a county or group of contiguous counties that contains at least one city of 50,000 inhabitants or more, or a grouping of cities with this population. In addition, contiguous counties are included in an SMSA if they are socially and economically integrated with the central city. Definitions for the New England States differ slightly (see Bureau of Justice Statistics, 1984:99). It is possible to distinguish in the NCS four metropolitan areas with cities of varying sizes. However, burglary rates in 1982 did not differ significantly within these four classes (Bureau of Justice Statistics, 1984:9). Households in central cities comprised 30% of all NCS households in 1982.

4. Inner cities are defined according to the Centre for Environmental Studies' PRAG Classification, based on the 1971 Census. Metropolitan counties are large city areas (as defined in the 1972 Local Government Act), excluding inner cities. The pooled BCS data show households in inner cities and metropolitan areas to comprise 38% of all households in the sample.



Table 2

CITY BURGLARY RATES IN THE US, ENGLAND AND WALES AND CANADA:  
UNADJUSTED AND ADJUSTED DATA FROM THE NCS, BCS AND CUVS

Rate per 10,000 households						
	UNADJUSTED DATA			ADJUSTED DATA		
	Completed	Attempts	Total	Completed	Attempts	Total
<i>US cities (1982)</i>	753	274	1027	768	299	1066
<i>England cities (1981/83)</i>	428	281	709	545	289	834
<i>Canada (1981)</i>	630	307	937	696	269	965
<i>US versus</i>						
<i>England</i>	+76%	- 3%	+45%	+41%	+ 3%	+28%
<i>Canada</i>	+20%	-11%	+10%	+10%	+11%	+10%

Notes:

1. For adjustments made to the BCS and NCS, see note (i) to Table 1. The adjustments to the CUVS were (i) 'expanded' definition of burglary; (ii) series counting as in the BCS; and (iii) exclusion of no-force attempts.
2. Unadjusted city rates for the US are derived from *Criminal Victimization in the United States, 1982* (Bureau of Justice Statistics, 1984: 36). Unadjusted data for England comes from the pooled samples of the 1981 and 1983 BCS. Unadjusted data for Canada is calculated from *Canadian Urban Victimization Survey: Reported and Unreported Crimes* (Bulletin No. 2, Ministry of the Solicitor General, Canada, 1984:7).
3. For the NCS, city areas are central cities in Standard Metropolitan Statistical Areas (see footnote 3, page 11). For the BCS, city areas are inner cities and metropolitan areas (see footnote 4, page 11).

NCS, series counting has roughly the same effect on both types of break-in, though the expanded definition of burglary raises no-force entries more than it does forcible ones. This, however, is offset by the exclusion of secondary respondents who, compared to household respondents, are more likely to report no-force entries (and thefts from second homes and hotels). In the CUVS, again, series counting has roughly the same effect on no-force and forcible entries, while using the expanded definition of burglary increases the former rather more than the latter.

TABLE 3 ABOUT HERE

*Reporting to the police*

On unadjusted figures, US reporting rates were consistently lower than in England and Canada (see Table 4). With the adjustments made, US reporting rates rise slightly mainly due to the exclusion of secondary respondents who are less likely than household respondents to say that the police became involved. Excluding no-force attempts in the CUVS slightly increases the reporting rate for attempts. For the BCS, including outbuilding burglaries reduces the reporting rate for completed burglaries. Overall, the pattern of lower US reporting still stands with the adjustments made. For instance, there was an 18 percentage point difference between the US and England in the reporting of completed burglaries (city data); for Canada and the US the difference was 13 points.

TABLE 4 ABOUT HERE

There is no marked area effect on reporting rates: national US and English rates are much the same as city ones (cf. Laub, 1981; Hough, 1984). This is consistent with the view that the seriousness of an offence is a much more important determinant of whether the police become involved than are demographic variables, the place where victims live, or attitudes towards law enforcement (eg. Skogan, 1984; Bureau of Justice Statistics, 1985a).

The lower US reporting rates for both attempted and completed offences, and for forcible and no-force completed offences which can be compared in the US and Canada, is some indication that US householders do not report their burglaries less often simply because they are less serious. However, Table 5 presents a better control for seriousness, by looking at reporting rates for burglaries involving different levels of (net) theft losses. For all value bands, the police are brought in more in England and Canada.

*Table 3*

**THE EFFECTS OF ADJUSTMENTS ON CITY RATES  
OF FORCIBLE AND NO-FORCE ENTRIES:  
UNADJUSTED AND ADJUSTED DATA FROM THE NCS AND CUVS**

	<i>UNADJUSTED DATA</i>		<i>ADJUSTED DATA</i>	
	No-force entries	Forcible entries	No-force entries	Forcible entries
<i>US</i>	380	373	394	373
<i>Canada</i>	271	359	307	389
<i>US versus Canada</i>	+40%	+ 4%	+28%	- 4%

Note: For adjustments to NCS and CUVS data, see notes to Table 2.

Table 4

REPORTING TO THE POLICE:  
UNADJUSTED AND ADJUSTED DATA FROM THE NCS, BCS AND CUVS

% reported to police					
	US National	England national	US cities	England cities	Canada cities
<b>UNADJUSTED DATA</b>					
All completed	55	82	55	84	74
All attempts	33	44	33	44	42
-----					
<b>ADJUSTED DATA</b>					
No-force entries			43		58
Forcible entries			77		83
All completed	56	72	59	77	72
All attempts	34	44	35	44	46

Notes:

1. Unadjusted national figures for % reported to the police for the US come from *Criminal Victimization in the United States, 1982* (Bureau of Justice Statistics, 1984). Unadjusted city figures are calculated by Rand. Unadjusted and adjusted rates for England come from the pooled samples of the 1982 and 1984 BCS. Unadjusted data for Canada was supplied by the Ministry of the Solicitor General. It differs very slightly from that in the *Canadian Urban Victimization Survey: Reported and Unreported Crimes*, (Bulletin No. 2, Ministry of the Solicitor General, Canada, 1984), in that the % reported is based on totals which include a "DK" coding (see note ii. below).
2. In each survey, % reported to the police" signifies the percentage of cases which respondents said became known to the police. For the NCS and the CUVS, % reported is based on a total which includes "DK" answers. The BCS did not have a "DK" coding. In the NCS, 1.3% of burglary victims said they did not know whether the police were aware of the offence; the figure for the CUVS was 1.4%.

Table 5

REPORTING TO THE POLICE, BY VALUE OF THEFT LOSS:  
ADJUSTED DATA FROM THE NCS, BCS AND CUVS

	% reported to police		
	NCS	BCS	CUVS
Attempts	34	44	46
Completed			
No/small loss	39	52	51
Medium loss	43	72	70
High loss	81	96	93

Notes:

1. For the adjustments to these figures, see note (i) to Table 1.
2. "No/small loss" burglaries were those for which there was no theft loss, or for which there were losses as follows:
  - in the NCS, under \$(US)50; in the BCS, under £25; and in the CUVS, under \$(Can)60."Medium loss" burglaries covered losses of:
  - in the NCS, \$(US)50.00 to under \$(US)200; in the BCS, £25 to under £100; and in the CUVS, \$(Can)60 to under \$(Can)250."High loss" burglaries covered losses of:
  - in the NCS, over \$(US)200; in the BCS, over £100; and in the CUVS, over \$(Can)250.

These figures take account of 1982 exchange rates, and the fact that purchasing power in North America was (and is) higher than in England.

## TABLE 5 ABOUT HERE

**REMAINING SURVEY DIFFERENCES**

These adjusted survey data represent an improvement on unadjusted data, but their reliability is still potentially threatened by remaining differences between the surveys which have not been accounted for. The differences do not work in any consistent direction, and it is possible only to make informed guesses as to their importance. Each remaining difference is discussed below, with the particularly problematic issues of bounding and reference period length taken together first.

i. and ii. *Reference period and bounding*

Compared to the NCS, the CUVS and BCS both allow respondents a longer 'reference' period (ie, the timespan over which they are asked to recall whether or not they were victimized). In the NCS, respondents are asked about crimes over the previous six months, with annual rates taking this into account. In the CUVS, respondents interviewed in January and February of 1982 reported on victimizations in 1981. In the BCS, interviewing took place between January and March and respondents reported for the period up until the interview; annual rates were derived from incidents said to have fallen within the calendar year ending 31 December (see also Appendix A)

The NCS also has a singular rotating panel design in which each household is interviewed every six months on up to seven occasions. The BCS and the CUVS employ more common cross-sectional sampling in which respondents are interviewed only once. In the NCS, rate estimates are based on data from all rotating panels, with the exception of reports from the first interview conducted. First interview data are discounted - in a process known as 'bounding' (see also Appendix A) - on the grounds that respondents will bring forward in time ('telescope in') victimizations which actually happened prior to the reference period; reports in subsequent interviews are deemed to be more reliable because of the natural 'bounding' that successive interviews provide. In the event of respondents moving from the sample address, the incoming household is treated as a replacement, with any victimizations in their first interview *counted* in rates. Present estimates put the number of such unbounded interviews in the NCS at about 20% (Biderman and Cantor, 1984). Independently, the shorter reference period of the NCS and the use of bounding will have

large effects on NCS rates relative to the two other surveys. However, they operate in different directions and with interactive effects.

There is considerable evidence that the six-month NCS reference period should *ceteris parabis* produce better recall and therefore relatively higher rates than those from the BCS or CUVS (cf. Cantor, 1985). One NCS experiment showed burglary rates using a 12-month reference period to be 14% lower than annual estimates based on a 6-month one (Bushery, 1981).<sup>5</sup> A follow-up study put the figure at 15% (Kobilarchik *et al.*, 1983). On the question of whether respondents will be most likely to forget less salient crimes with a 12-month reference period the evidence is equivocal.<sup>6</sup>

In contrast, the use of bounding in the NCS is likely to produce *fewer* victimization reports. For instance, in a comparison of bounded and unbounded interviews, both with a 6-month reference period, Murphy and Cowan (1984) showed a rate of completed burglary which was 32% higher with unbounded interviews, though the increase in rates with a 12-month reference period may be less if 'telescoping' is reduced with a longer reference period.<sup>7</sup> Having an unbounded reference period is also assumed to 'pull in' more completed than attempted offences (see, eg, Biderman and Lynch, 1981; Biderman *et al.*, 1986). Yet again, though, there is contrary evidence from the Murphy and Cowan (1984)

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5. This figure is only of limited use in considering NCS rates *versus* those of the BCS and CUVS. Firstly, most interviews were bounded in both reference period conditions, though because of replacements there were more unbounded respondents in the 12-month group, inflating the 12-month rates. If *all* interviews had been bounded, the 12-month shortfall may have been greater. Similarly, if *all* interviews had been *unbounded*, the 12-month shortfall may also have been greater to the extent that one would expect less 'telescoping in' with the longer reference period. Secondly, series incidents were not counted in the experiment: series would play a bigger part with a longer reference period, and if these were accounted for this might have *decreased* the 12-month shortfall. It is difficult to say whether the effects would have cancelled each other out had all the interviews in both reference period conditions had equivalent (or unbounded) status, and had series incidents been counted.

6. The Washington DC records check indicated that incidents involving smaller financial losses were *not* more likely to be forgotten (Turner, 1970 quoted in Skogan, 1981:18). As against this, work done for the Kobilarchik *et al.* (1983) study shows that a 12-month rather than a 6-month reference period resulted in lower counts for low-loss larcenies and simple assaults, whereas this was not the case for higher-loss larcenies and aggravated assaults (Alexander, personal communication). Unfortunately, no similar data are available for attempted and completed burglaries.

7. Neither again, though, did Murphy and Cowan's (1984) figures account for series incidents. These will play a bigger part with a longer reference period, so that if series had been counted even higher rates would be expected with unbounded *versus* bounded interviews

study which showed that unbounded interviews inflated the rate of attempted burglary *more* (by 66%) than the rate of completed burglaries (32%). The explanation they offer is that less serious incidents may be more prone to be forward telescoped since the time of occurrence is less accurately recalled.

One check on the interactive effect of bounding and reference period length is possible from comparing 'conventional' NCS rates with those from one set of the City Surveys conducted in the early 1970s, using a 12-month reference period and unbounded interviews. Specifically, the comparison is between (i) the average of 1972 and 1974 rates for four of the five cities covered in the Five Largest Cities Survey (US Department of Justice, 1976), and (ii) the average of 1972 and 1974 NCS rates for cities of 1 million or more population.<sup>8</sup> This comparison is a rough one since there were several differences between the national NCS and the City Surveys (eg, Penick and Owens, 1976; Sparks, 1982). This said, the City Survey completed burglary rate is 22% higher than that from the NCS, and 50% for attempts (the higher figure for attempts being in line with Murphy and Cowan, 1984). These figures are only suggestive of the interactive effect of bounding and reference period length, but they should not overestimate the difference in rates.<sup>9</sup>

In sum, then, the potentially strong effects of the shorter NCS reference period and bounded interviews to an extent cancel out. It is conjectured here, though, that the unbounded nature of interviews in the CUVS and BCS will, even though their reference periods were longer, led to inflated levels of burglary relative to the NCS. Whether the

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8. The NCS cities of 1 million or more population are the same as the five largest cities covered in this set of City Surveys. New York has been excluded from the surveys in the five largest cities as rates of burglary there seemed aberrantly low - 21% lower than the rate in the other four cities combined. Including New York put the City Survey burglary rate only marginally higher than that from the NCS, though the rate of attempts was 25% higher.

9. The proportion of incidents identified as series in the 1972 and 1974 Five Main Cities Surveys appears to be greater than in the 1973 NCS (author's computation). This may reflect the longer reference period in the City Surveys, though it is also believed that interview controls were weaker in the City Surveys than in the NCS which may also have affected the handling of series incidents. Had series been counted (or dealt with more similarly) in the City Surveys and the NCS, the City rates may have been even higher. Secondly, interviewing for the 1972 and 1974 City Surveys took place, respectively, in the first quarter of 1973 and 1975. (Respondents were asked about crimes over the previous twelve months, ending with the month prior to the interview.) The fact that more recent victimizations will be remembered best, and that the seasonal pattern of burglary shows a peak in the summer months (Rand, 1985), may again depress the City Survey rates relative to the NCS ones as the summer months will have been further away from the date of interview for proportionately more respondents in the City Surveys.



effect here differs by type of burglary is difficult to say from the available evidence.

ii. *Panel versus cross-sectional designs*

It is possible that the NCS panel design affects its rates relative to the other two surveys insofar as fewer victimizations are reported as 'time-in-panel' increases. This may reflect (and the evidence is unclear) either 'panel fatigue' (respondents tiring of the task of having to recall and report on victimizations), or 'panel experience' (respondents fulfilling the interview task more correctly having gained experience of it). Tests show generally declining rates of burglary victimization as time-in-panel increases, though the differences do not reach statistical significance.<sup>10</sup> Nonetheless, there is some suggestion that there less salient incidents fall off most - at least insofar as those with most panel experience are more likely to say they called the police (Murphy and Cowan, 1984). This may be because being a respondent in a crime survey increases the probability of the police being informed in the event of a victimization. But perhaps equally probable is that there *is* some fall-off in the recounting of less serious incidents about which the police are unlikely to informed.

The NCS panel design also means, secondly, that some respondents do not participate throughout the full 'life' of their panel either because they move or because they later refuse interviews (Bushery, 1984). The loss of these respondents is important as they tend have higher than average victimization rates (see, eg, Lehen and Reiss, 1978). For rate estimation, however, any effect of panel attrition is probably more than counterbalanced by the higher rates of victimization that come from unbounded interviews with incoming households who replace movers.

In sum, then, it is assumed here that neither panel fatigue nor the loss of movers appreciably depresses NCS rates relative to the BCS and CUVS. Panel fatigue may slightly depress rates for attempts and no-force entries insofar as respondents who have been in the panel for some time may avoid admitting an incident about which there is relatively

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10. The number of victimizations reported at the very end of time-in-panel have also been shown to sometimes rise (eg, Woltman and Bushery, 1984). The greatest drop in rates is, in fact, between the first and second interview, though 'telescoping' of incidents into the first interview will play a major part here.

little say. However, equivocal evidence here makes it best to ignore this possibility.

iv. *Method of interview*

In the CUVS, all interviews are conducted by telephone, while in the BCS they all are face-to-face. Both methods are used in the NCS, with approximately half of all interviews being done by telephone in 1982. Even though victimizations reported in the first interview are not generally counted, this interview is meant to be in person because of what is seen as an unacceptably high no-response rates with telephone 'cold contacts'.

A test in Canada prior to the Greater Vancouver Victimization Survey showed no difference in victimization productivity between respondents interviewed personally and those interviewed by telephone (Catlin and Murray, 1979). Victims here, however, were identified from police records and it is possible that less memorable unreported offences will be less likely to be recalled in a telephone interview. In the alternating telephone/personal visit procedures used in the NCS, interview mode did not greatly affect victimization rates (Roman and Silva, 1982). However, the evidence is not as favorable to *full* telephone interviewing, or to making a first contact by telephone. Victimization rates for persons entering the NCS sample to replace movers have been shown to be low when they were interviewed by telephone rather than in person (Woltman and Bushery, 1984). Moreover, in tests involving respondents who have mostly been interviewed at least once before in person, lower victimization rates have been observed when full telephone interviewing was subsequently used (eg, Woltman and Bushery, 1984). Most differences in victimization rates for sub-categories of offences have not been statistically robust, though in Turner's (1984) study, the difference of 13% for burglary in maximum personal interviewing compared with maximum telephone interviewing is likely to be so (author's computation).

In sum, it is assumed here that the level of telephone interviewing in the NCS in 1982 might somewhat depress the NCS completed burglary rate relative to BCS ones. By the same token, Canadian rates should be even more depressed. Telephone interviewing in the NCS and CUVS may lead to more of a relative undercount of less salient offences. Again, the effect here will be more pronounced for the CUVS relative to the BCS.

v. *Response rates*

In all three surveys, non-response will vary across different demographic subgroups (see, for the NCS, Biderman *et al.*, 1986), and in ways likely to undercount particularly victimized households. However, the bias due to this will be smallest in the NCS since it had the highest response rate - about 96.5% for households (see Love and Turner, 1984, for some reasons) - and some account of non-response bias is taken in weighting by matching non-responders with responders on various socio-demographic variables. The CUVS response rate was 81%, and again some account is taken of non-response in weighting, though only on an area basis. The response rates in the 1982 and 1984 BCS were 80% and 77% respectively; no account was taken of non-response in weighting (see also Appendix A).

One can suppose that, relative to the NCS, BCS rates are depressed most because of an undercount of heavily victimized households. CUVS rates are also likely to be depressed relative to NCS ones, though perhaps not to such an extent. There may not be much of a differential response rate effect for different types of burglary. However, higher non-response levels in the BCS and CUVS could possibly inflate their levels of reporting to the police insofar as groups less likely to report - eg, younger people and the less residentially stable - will be disproportionately undercounted in the surveys.

vi. *Classification filters*

Both the CUVS and the NCS classify offences on the basis on Incident Form information according to computer algorithms. In the BCS, classification is done manually by selected coders. BCS and NCS classification criteria seem tighter than those of the CUVS. The BCS allows scope for marginally criminal incidents to be deleted (even though they elicit responses on the Incident Form), and this is not uncommon. Although the NCS and the CUVS have virtually identical Incident Forms, CUVS classification filters for completed burglaries seem wider, letting through a greater number of 'marginal' incidents (ie, for which evidence of entry was sparse, or where in which it was unclear whether the entry was illegal). Comparison of the numbers of no-loss burglaries supports this: these comprise

26% of all CUVS completed offences, as against 19% of NCS and 16% of BCS ones. <sup>11</sup>

In sum, it is assumed here that there is some additional count of 'marginal' completed burglaries in the CUVS that would be excluded in the NCS and BCS. These are most likely to be no-force entries. Insofar as these are relatively unlikely to be reported to the police, this may be depressing the CUVS reporting rate - albeit slightly.

vii. *Differences in screener question options*

All three surveys (and indeed most other crime surveys) use so-called 'screening' questions early on in the questionnaire to tap whether or not a victimization has occurred; collection of detailed information about the incident is delayed in case respondents realize that saying "yes" to a question about victimization leads to a lengthy set of questions. The number of 'screeners' in a questionnaire can influence victimization counts since the additional prompts can lead to recollection of incidents which would not otherwise have been mentioned. In both the CUVS and the BCS, screening questions were included to cover vandalism (ie, incidents of damage to property); these are not included in the NCS (see Appendix A). The indications from the 1982 BCS are that these screeners gave rise to only a small number of burglary classifications, most of which were attempts. CUVS figures, however, were higher: some 2.5% of completed burglaries emanated from vandalism screeners, and a full 10% of attempts (attempts with force only). (Most of the relevant attempted burglaries emanated from the property vandalism screener at the end of the screener list.)

The view here is that the effect of vandalism screeners on *completed* burglary counts in the CUVS and BCS is small. However, while BCS attempts seems not much affected by the vandalism screeners, the level of CUVS attempts appears to be somewhat inflated by them.

### **OVERALL EFFECT OF REMAINING SURVEY DIFFERENCES**

How much, then, are these remaining design and procedural differences between the surveys

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11. These calculations are based on city data, using the expanded definition of burglary, and taking outbuilding burglaries into account in the BCS. Secondary respondents are excluded from the NCS. Cases where the amount of theft loss is not known are excluded.

likely to modify the picture from the adjusted rates already presented? Table 6 presents some estimates of the effects on adjusted rates of completed and attempted offences. These are presented as 'inflationary' effects, with each "\*" sign signifying an inflation of the rate for the relevant survey of about 5%. The inflationary effects cannot be seen as hard and fast. Although the direction of the effects is reasonably incontestable, their magnitude is a matter of judgement.<sup>12</sup> It should be emphasized, too, that the effects have been considered only from a relative viewpoint, and that in relation to what features *inflate* one survey's rates relative to the others. Inflationary effects are not, of course, necessarily 'good' - in the sense of higher rates signifying more complete counts. This might be so for full personal interviewing and good control for non-response, but inflations that come from loose classification and unbounded interviews indicate less accurate counting. Of the three surveys, the NCS will overall produce the most accurate victimization measure. But accuracy of measurement has been taken as less at issue here than comparability.

#### TABLE 6 ABOUT HERE

Table 6 shows no panel effects for the NCS relative to the other surveys. Panel effects may mean some overcount of less salient incidents, but it is difficult to be sure. One difference - on classification - is seen to affect only completed CUVS offences, while the vandalism screener questions in the CUVS are seen to have an inflationary effect only on attempted burglaries. Full personal interviewing in the BCS is assumed to produce slightly more of an overcount of attempts in that survey than with completed offences: more so in comparison with the CUVS than the NCS. (Partial personal interviewing in the NCS may also mean more of a relative overcount of attempts than completed entries compared to the CUVS, but the difference is probably small enough to be ignored.) On balance, it the effect of the 'soft' adjustments made to account for remaining survey differences, to crudely equalize measurement, will be to pull down BCS and CUVS rates relative to those of the NCS. This will make the gaps between English and Canadian rates and those of the US wider than those seen from the first (or 'hard') adjustment figures.

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12. It should be said, too, that the inflationary effects suggested have been considered in terms of the respective designs of the three surveys as they have been applied in the field and data analysis offices. The first set of 'harder' adjustments have already changed the 'population' of burglaries as they are measured by unadjusted figures. It is possible that the inflationary effects suggested may operate rather differently on these new classes of burglary figures. There is little option but to ignore this.

Table 6

POSSIBLE 'INFLATIONARY' EFFECTS OF UNACCOUNTED FOR DIFFERENCES BETWEEN THE NCS, BCS, AND CUVS

	NCS	BCS	NCS	CUVS	BCS	CUVS
<i>Interview mode</i>						
Completed offences		*	*		**	
Attempts		**	*		***	
<i>Differential response rates</i>						
Completed offences	**		*			*
Attempts	**		*			*
<i>Bounding/reference period</i>						
Completed offences		****		****		
Attempts		****		****		
<i>Classification</i>						
Completed offences				*		*
<i>Vandalism screeners</i>						
Attempts				**		*
<b>TOTAL RELATIVE INFLATIONARY EFFECTS</b>						
Completed offences	**	*****	**	*****	**	**
Attempts	**	*****	**	*****	***	**

Note: Each \* may signify an inflation of the rate for the relevant survey of about 5%.

Table 7 shows the relative positions of the three countries on the burglary front as measured by (i) unadjusted survey figures; (ii) figures with 'hard' adjustments made; and (iii) figures (final) with incorporate both 'hard' and 'soft' adjustments. Only percentage differences are given, rounded to the most appropriate nearest 5%: more precise figures are obviously best avoided <sup>13</sup>.

#### TABLE 7 ABOUT HERE

##### *Completed offences*

As regards the England and the US, the risk of completed offences on national figures seem about 85% higher in the US; this is higher than the first adjustment figure, but much lower than the unadjusted one. City rates indicate a narrower US lead over England - in the region of 55%; again, this is higher than the first adjustment figure, but lower than the unadjusted one. This points to a difference in the geographical distribution of burglary - with the US having a less steep decrease in burglary outside central cities than is the case in England. It is difficult to match areas precisely across surveys, but the rate of burglary in areas in the US outside central cities as defined here was only 28% lower than the central city rate, whereas in England it was 49% lower.

US city rates for completed offences may be nearer to 25% higher than in Canada; this is more similar to the unadjusted figure than to the 'hard' adjustments one of 10%. (If anything, classification differences between the NCS and the CUVS will be inflating the Canadian rate of no-force entries more than the rate of forcible ones.) Final figures put completed offences about 25% higher in Canada than in England; this is slightly lower than the 'hard' adjustment figures, and lower still than unadjusted ones.

##### *Attempts*

The picture with attempts is rather different. Final national figures put the risk of attempts in the US about 25% higher than in England, substantially less than with completed offences; unadjusted and first adjustment figures show more similar rates.

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13. The effects discussed may operate differently on national rather than city estimates. For most of them, it is unlikely that there will be a large differential effects across area, though it could be that for the BCS (in which non-contacts were rather higher in city areas), the effect of non-response is more marked for city estimates than national ones. A small allowance has been made for this in Table 7.

Table 7

**LEVELS OF RESIDENTIAL BURGLARY IN THE US, ENGLAND AND CANADA  
UNADJUSTED AND ADJUSTED DATA FROM THE NCS, BCS, AND CUVS**

	COMPLETED			ATTEMPTS			TOTAL		
	None	1st	Final	ADJUSTMENTS			None	1st	Final
	None	1st	Final	None	1st	Final	None	1st	Final
<b>US versus ENGLAND</b>									
National	+120%	+60%	+85%	+ 5%	+ 5%	+25%	+75%	+45%	+65%
Cities	+ 75%	+40%	+55%	- 5%	+ 5%	+20%	+45%	+30%	+45%
<b>US versus CANADA</b>									
Cities	+ 20%	+10%	+25%	-10%	+10%	+30%	+10%	+10%	+25%
<b>CANADA versus ENGLAND</b>									
Cities	+ 45%	+30%	+25%	+10%	- 5%	- 5%	+30%	+15%	+15%

Note:

1. 1st adjustments for the NCS and BCS are those described in note (i) to Table 1; those for the CUVS are those described in note (ii) to Table 2.
2. For the NCS, city areas are central cities in Standard Metropolitan Statistical areas (see footnote 3, page 11). For the BCS, city areas are inner cities and metropolitan areas (see footnote 4, page 11).



Final city estimates for attempts are in the same region as national ones (there is slightly less of a difference): in other words, one does not see quite such a reduction in the risk of attempts in non-city areas in England compared as was the case with completed offences.<sup>14</sup>

Looking at Canada and the US, final figures show that the risks of attempts in the US was higher by the same rough order of magnitude as with completed offences - in this case 30% higher; unadjusted figures showed there to be a higher rate of attempts in Canada, while first adjustments put the US 15% higher. Looking at Canada and England, on final figures the risk of attempts was some 5% lower in Canada than in England, the same of on first adjustment figures; unadjusted data had shown Canadian rates to be rather higher.

#### *Reporting to the police*

On balance it is probably not the case that unadjusted-for differences between the surveys are at issue in explaining why American householders seem less likely to bring in the police after a burglary. If the panel design of the NCS leads to experienced respondents filtering out less serious crimes, NCS reporting figures could be inflated slightly. And, again, if less serious crimes are disproportionately lost in telephone interviews, this may be artificially inflating US reporting rates too. But if NCS reporting levels are not inflated, are perhaps those from the CUVS and BCS? On the one hand, any interview mode effect would of course operate more on the CUVS than on the NCS. On the other hand, the capture of some generally less serious burglaries through the vandalism screeners in the CUVS may actually be depressing Canadian reporting rates slightly, though this is unlikely to be operating much in the BCS. The looser crime classification in the CUVS - 'letting through' more relatively trivial 'no-force' entries - might also appear to work in the direction of depressing CUVS reporting rates, though in fact the more marked gap between reporting rates in the NCS and CUVS for no-force entries than for forcible ones (Table 4) leaves some doubt about this. The equivocal evidence about how unbounded interviews asking for recall over 12-months affect the capture of less salient events makes it best

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14. In England, attempts are particularly high in rural areas, probably reflecting the fact that houses are more often left unlocked (cf. Hough, 1984). However, while attempts are relatively high in inner cities, they are relatively low in metropolitan areas. Taking these areas together to match CUVS cities gives a proportion of attempts to completed offences which is not dissimilar to rural areas. In the NCS, attempts are rather higher in central cities than in other SMSA areas, or in non-metropolitan areas.

to disregard any effect here. Higher non-response levels in the CUVS and BCS could have some inflationary effect on reporting insofar as groups less likely to report will be disproportionately undercounted; but this is unlikely to account for much of the differences observed.

### CAVEATS

Several qualifications need to be made about this final 'league table'. In comparing burglary in the three countries, the device has been adopted of pairing two sets of surveys, and looking at what levels of burglary might have been measured had their designs been more comparable. For one thing, the year for which burglary was measured differs slightly: 1981 for Canada, the average of 1981 and 1983 for England, and 1982 for the US NCS (rates in 1981 were slightly, but significantly higher, those in 1982 slightly, but significantly lower.) Much more important though is that this 'pairing' strategy is more useful for assessing relative differences than for saying what the 'true' level of burglary in each country might be. Other comparative strategies may could be considered. For instance, each survey could have been taken in turn as standard, and estimates made (through both direct data manipulation and informed guesswork as here) as to what levels of burglary would have been measured in the two other countries had the procedures of the 'standard' survey applied. This would give a broader comparative base, albeit at the cost of more complicated results.

Neither is it necessarily the case that account has been taken of all design variations between the surveys. Some identified in this exercise (eg, the slightly different reference period cut-off points in the CUVS and BCS (see Appendix A) may have had more effect than has been allowed for. And there may well be other design differences which remain hidden within the pages of survey manuals; or which would defy quantification in any case. One potentially important difference in this last regard could be 'house' and interviewer effects, arising from such things as differences in field supervision, quality control, and the training and performance of interviewers themselves. In one field test done in the context of the NCS Redesign, the Survey Research Centre at the University of Michigan issued a questionnaire using random-digit dialling under a computer-assisted telephone interview (CATI) system, but otherwise following NCS procedures as closely as possible. Unbounded SRC victimization rates were over twice as high as regular NCS

unbounded rates, a difference that was largely put down to a combination of interviewer and 'house' effects (Biderman *et al.*, 1986).

A further caveat relates to the reliance here on *incidence* rates (the number of victimizations set over the number of households interviewed), where series counting rules can play a big part in influencing the size of the numerator. An approach which minimizes the series effect is to compare *prevalence* rates (measuring households victimized once or more), albeit at the cost of concealing possible differences in the level of multiple victimization. In this study, technical difficulties precluded the calculation of detailed adjusted prevalence figures for the NCS. Nonetheless, setting some imperfectly adjusted prevalence figures for the NCS against tighter ones for the BCS and CUVS gives a picture of relative rates which is strikingly in line with that based on incidence rates. The nature of the NCS figures preclude firm conclusions being drawn, but on the face of it this suggests that, at given levels of general risk, levels of multiple victimisation in the three countries are similar.

Finally, there is the question of sampling error. Are the differences observed in this study statistically robust? The calculation of sampling error for statistics based on adjusted data from the three surveys poses considerable difficulty, as the conventional error estimates for unadjusted data are not readily applicable after the considerable adjustments made. By and large, most of the differences pointed to on the basis of adjusted data appear large to withstand such statistical testing that could be devised: differences of - let us say - 10% may be suspect. At the same time, though, it cannot be emphasised enough that sampling variance itself will be the most trivial error associated with the estimates made in this study. In particular, the 'soft' adjustments made to account for unmanipulable survey design differences are based only on informed guesswork, and the degree of possible error associated with them may be far in excess of sampling variance for statistics with known or unknown parameters. Moreover, as pointed out above, there could be yet further differences in how the surveys are administered and their results processed, which would undermine comparability even more. Given these uncertainties, attention to sampling error may serve only to create a spurious illusion of accuracy about figures subject to considerably more important measurement 'noise'.

## **SECTION III**

### **LEVELS OF BURGLARY RECORDED BY THE POLICE**

An obvious question is whether the level of burglary in the US, Canada and England as evidenced from their surveys is reflected in offences recorded by the police. This section takes this up. Appendix D addresses some points about the comparability of police figures, and some other technical issues. The data below takes account of one known classification difference between the US and the other two countries. This pertains to how break-ins to structures not directly attached to a house are dealt with by the police.

#### **POLICE AND SURVEY FIGURES CONTRASTED**

To approximate the time period over which survey data were compared, Table 8 presents the average of 1981-1983 rates of residential burglaries as measured by police figures. Alongside these are survey rates, taking account of the 'hard' and 'soft' adjustments described in Section II; rates are per 10,000 households. Police figures show relatively small differences between the three countries in levels of burglary - in each case smaller than those suggested by survey data. For instance, offences of residential burglary recorded by the police were only some 16% higher in the US over the 1981-1983 period than in England, whereas survey data indicate US national burglary rates were some 65% higher. Survey data also show a bigger US lead over Canada (about 25%) than police figures (5%). Comparing England and Canada, the differences between the two indices are smaller.

#### **TABLE 8 ABOUT HERE**

The figures are only suggestive of what 'real' differences between the three countries on the basis of the two indicators might be. The adjusted survey figures are inevitably somewhat crude, given the problems of estimating the effect of design differences which cannot be accounted for through data reanalysis. The time period over which levels of burglary are compared with the two indicators is also slightly different. Neither can uniformity of classification and counting procedures be guaranteed for police burglary statistics, though it is unlikely that US rates of residential burglary are deflated by some incidents being dealt with as non-residential which in England or Canada would be

Table 8

**RATES OF RESIDENTIAL BURGLARY IN THE US, CANADA AND ENGLAND AS MEASURED BY POLICE FIGURES (1981-1983), AND ADJUSTED SURVEY DATA**

Rate per 10,000 households			
	POLICE FIGURES average 1981-1983	SURVEY DATA adjusted	
	<i>National</i>	<i>National</i>	<i>Cities</i>
<b>US</b> <b>ENGLAND</b>	270 233		
<i>US higher</i>	+16%	+65%	+45%
<b>US</b> <b>CANADA</b>	270 256		
<i>US higher</i>	+ 5%	NA	+25%
<b>CANADA</b> <b>ENGLAND</b>	256 233		
<i>Canada higher</i>	+10%	NA	+15%

Note: The survey data relates to 1982 for the US; 1981 for Canada; and the average of 1981 and 1983 for England. Thus, police figures pertain to a period slightly broader than the surveys' coverage.

dealt with as residential offences. Non-residential offences in the US formed a smaller proportion of all burglaries over 1981-83 (34%) than in England (45%) or Canada (42%). Reflecting this, the rate of non-residential offences (calculated per capita) was a third lower in the US over 1981-83 than in England, and a fifth lower than in Canada.

The comparability of police measures, however, is more seriously challenged by three other factors: (i) differences in householders' reporting habits; (ii) police recording practices; and (iii) differences in recent burglary trends. Potential inconsistencies here mean that police figures cannot be taken at face value. Indeed, their comparability remains sufficiently uncertain as to suggest that survey results - notwithstanding their own difficulties - provide the sounder picture.

## REPORTING

US police burglary rates could be depressed because the police have fewer potentially recordable burglaries known to them on account of lower levels of citizen reporting. This is consistent with survey evidence presented in Section II, and with Skogan's (1984) conclusions about NCS reporting rates in comparison with those of other countries.

Why are US reporting rates lower? It should be *easier* to report to the police in the US, at least insofar as telephoning is concerned - by far the main means of notification: at the beginning of the 1980s, for instance, about 90% of North American households had telephones (Euromonitor, 1985) as against 70% in Great Britain (Social Trends, 1986). On the other hand, insurance may play a part since crime surveys have consistently shown that being insured provides a strong incentive to report property offences. In the US, although there are more owner-occupiers (more inclined to bring in the police), who have slightly higher levels of insurance coverage, cover among renters seems rather lower: and renters face higher risks.<sup>1</sup> Attitudes to the police may also be affecting the inclination to report. These are seen as having a much weaker influence on reporting than the seriousness of the incident experienced, although they may operate to a small extent

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1. In 1984, 76% of all households spent money to insure the contents of their dwelling (personal communication, Association of British Insurers). Among owner-occupiers, 87% had theft cover; 59% of renters. Figures for the US show that 93% of owner-occupiers had cover in 1984, though a much lower figure of 28% of renters (eg, Insurance Information Institute, 1985).

with regard to whether or not to report more trivial offences (see Skogan, 1984; Hough and Mayhew, 1985). Survey questions on why the police were not called have different response options, making comparison difficult. Other polls in the three countries show a consistent majority of citizens holding favorable opinions about the police, though fine comparisons of their results is ruled out by differences in question wording, sample compositions, etc. Conventional wisdom may have it that US householders will think the police will be less responsive if called. This is shaky ground on which to conclude that such views keep US reporting rates down, though it is consistent with the lower rates.

## RECORDING

It is unclear whether equivalent proportions of reported offences get into police books as 'crimes known', in the three countries, and under the same headings. Cases of attempted and no-loss break-ins may be particularly suspect: Burrows (1986), for example, showed wide variation between English police forces in how many break-ins were recorded as vandalism.<sup>2</sup> Many factors influence recording, and several studies have shown wide variations in crime rates between different local police areas attributable to such things as styles of enforcement and organizational decisions on recording strategy (eg, in England, Farrington and Dowds, 1985, and Burrows, 1986; in the US, Reynolds and Blyth, 1975, and McCleary *et al.*, 1982). Such local variations probably do not 'come out in the wash' at national level.

Survey data for the US and England allow a broad estimate of recording levels by setting the number of burglaries reported to the police against the numbers recorded by them. (Appropriate data for Canada are unavailable because of the restricted coverage of the CUVS.) Such a calculation, using survey figures adjusted along the lines described in Section II, shows a virtually identical figure for both countries of two-thirds of the estimated 'reported' burglaries being recorded under a residential heading in police statistics in 1982. However, as matching reported and recorded offences in this way is

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2. The available statistics do not allow a comparison of the three countries in terms of the number of recorded burglaries which were attempts - one possible indicator of recording policies. (In England and Wales in 1984, for instance, a fifth of dwelling entries involved no loss.) Clear-up rates are not strikingly different: in 1984, 32% of all burglaries were cleared in Canada, 28% in England, and 25% in the US. But given the effect of procedural and management rules on clearance figures (eg, Burrows, 1986), it is doubtful whether much weight should be attached to them in any case.

somewhat problematic (Appendix D). the similarity of figures here may not signify much.

## TRENDS

A third challenge to police figures as a reliable index of burglary levels for a given period is the degree to which they reflect differences between the countries simply in terms of *differential* trends in public reporting and police recording practices. On the face of it, the police trend in residential burglaries has been markedly different in England than in North America. Figure 1 shows rates (per 10,000 households) over the period 1972-1985 in the three countries. English rates have risen steadily (more than doubling over the period), while in the US and Canada an increase in burglaries was stemmed at the beginning of this decade.<sup>3</sup> In 1972, residential break-ins in the US were over 70% higher than in England and nearly 40% higher than in Canada. By 1985, English rates were higher than those in the US (by 14%), and in Canada (10%). The respective trends are roughly mirrored for non-residential offences.

### FIGURE 1 ABOUT HERE

However, burglary trends according to police figures in the US and England are not entirely in line with trends evidenced by surveys in these two countries.<sup>4</sup> Comparing trends is not without problems (see Appendix D), though survey data can still provide a pointer as to whether factors other than 'real' changes in burglary might be affecting police figures.

#### *Survey and police trends in England and Wales*

Trend information on burglaries *with loss* is available by combining BCS estimates for 1981 and 1983 with a measure of burglary risks from the General Household Survey (GHS) for

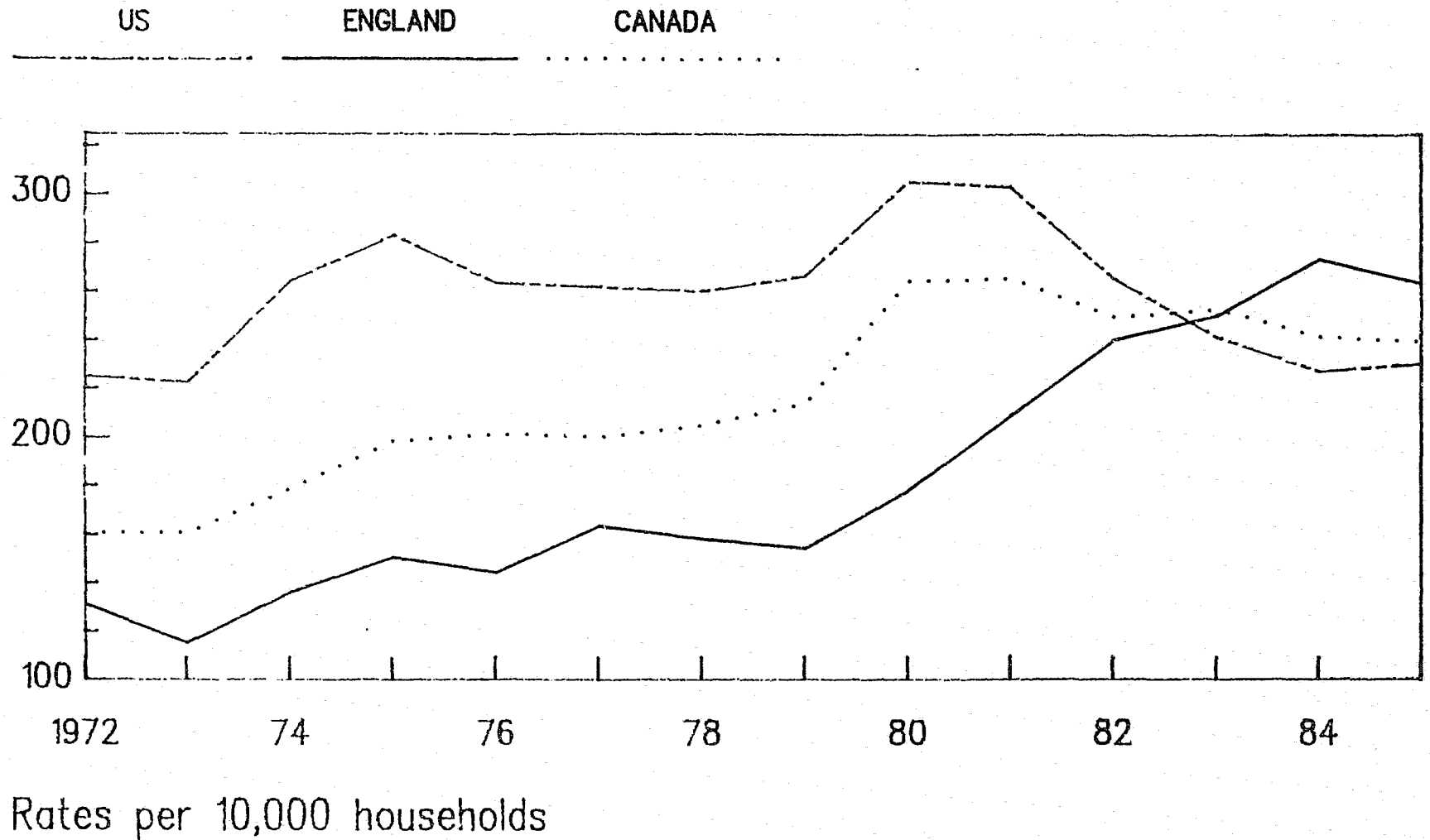
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3. In all three countries, household size has fallen since 1972 increasing the number of households relative to population. Rates expressed on a household base, therefore, show a less steep increase than rates expressed on a population base.

4. There is no equivalent survey trend data from Canada to examine whether the downfall in Canadian police rates - less marked in any case than US ones - would have been mirrored in victimization reports. However, survey data from the CUVS in Edmonton has recently been compared to data from a later crime survey in the same city. This showed a downturn in burglary rates between 1981 and 1984, matching the overall police trend in Canada (Stripinis, personal communication).



*FIGURE 1*  
RESIDENTIAL BURGLARY OFFENCES RECORDED  
BY THE POLICE, 1972-1985



1972, 1973, 1979 and 1980.<sup>5</sup> Figure 2 shows survey and police rates of loss-burglaries over the period 1972-1984. 1972 values are indexed at 100, to remove the difference in the levels in the two series. The years for which there was no survey figure are indicated. The rate (on a household base) nearly doubled over the period according to police figures, whereas survey figures indicate an increase of less than 10% (see Hough and Mayhew, 1983). This divergence leaves little doubt that police figures exaggerate the burglary increase. And they may have done so more recently: the police increase was proportionately higher between 1979 and 1983 than earlier. Higher levels of reporting appear to play some part in the rise in police figures: the upward trend in reporting apparent from the GHS results was not statistically very robust (Home Office, 1982), but it is consistent with significantly increased reporting between 1981 and 1983 as measured by the BCS ( $p < 0.05$ ). Higher reporting, however, cannot explain all the divergence between the police and survey measures: fuller recording by the police of those crimes which they came to know about is clearly operating as well, a point returned to.

FIGURE 2 ABOUT HERE

#### *Survey and police trends in the US*

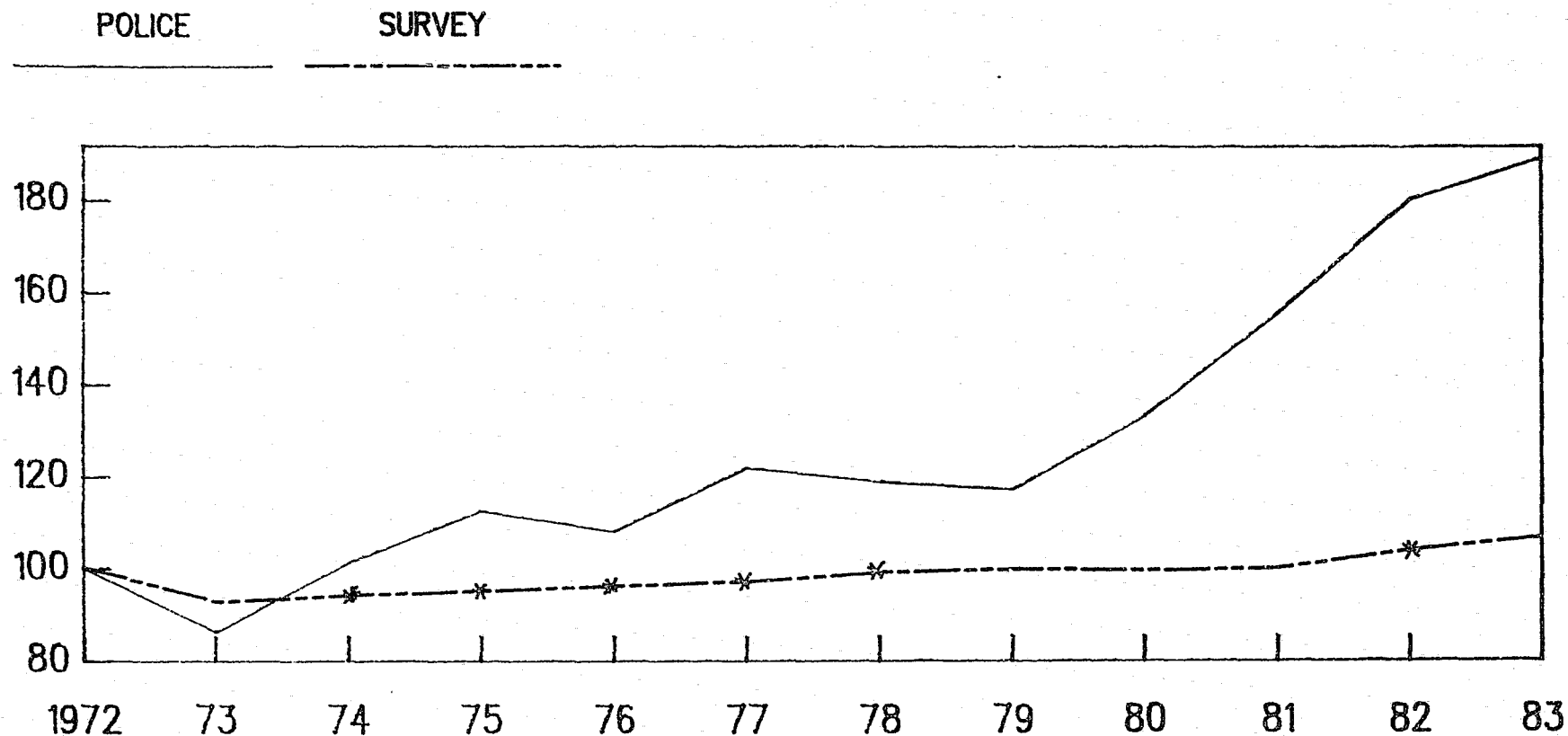
Figure 3 shows UCR and NCS rates of all residential burglaries over the period 1973-1985, with 1973 values indexed at 100. On UCR figures, there was an increase of 37% in burglaries between 1973 and 1980. Although some of the peak in rates at the end of the decade has been said to be artificial (reflecting an undercount of the population, Biderman *et al*, 1983), NCS rates - falling by 8% - are not in line. Taking the full period, the NCS rate of burglary in 1985 was 32% lower than in 1973, while, despite the fall since 1980, the UCR level was still marginally higher than in 1973.

FIGURE 3 ABOUT HERE

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5. The GHS is an annual survey conducted by the Office of Population Censuses and Surveys of about 10,000 households in England and Wales, in which the household head, or spouse, is interviewed about household matters. Various 'add-on' components are periodically included, as was the case with the burglary questions. There were several differences in the approach to victimization estimation in the GHS and BCS, and though a number of adjustments were made to BCS data to improve comparability, these will not account for all the differences. The data in Figure 2 are based on reports from householders who had lived in their home for more than one year. Because movers have higher burglary rates than others, the survey figures are therefore underestimates. The best estimate of the increase in the rate of burglaries between 1972 and 1983 is 7%. Allowing for sampling error, it is 95% certain that the increase did not exceed 33% ( $p < 0.05$ ; one-tailed test).

**FIGURE 2**  
RESIDENTIAL BURGLARIES INVOLVING LOSS,  
ENGLAND: SURVEY AND POLICE RATES

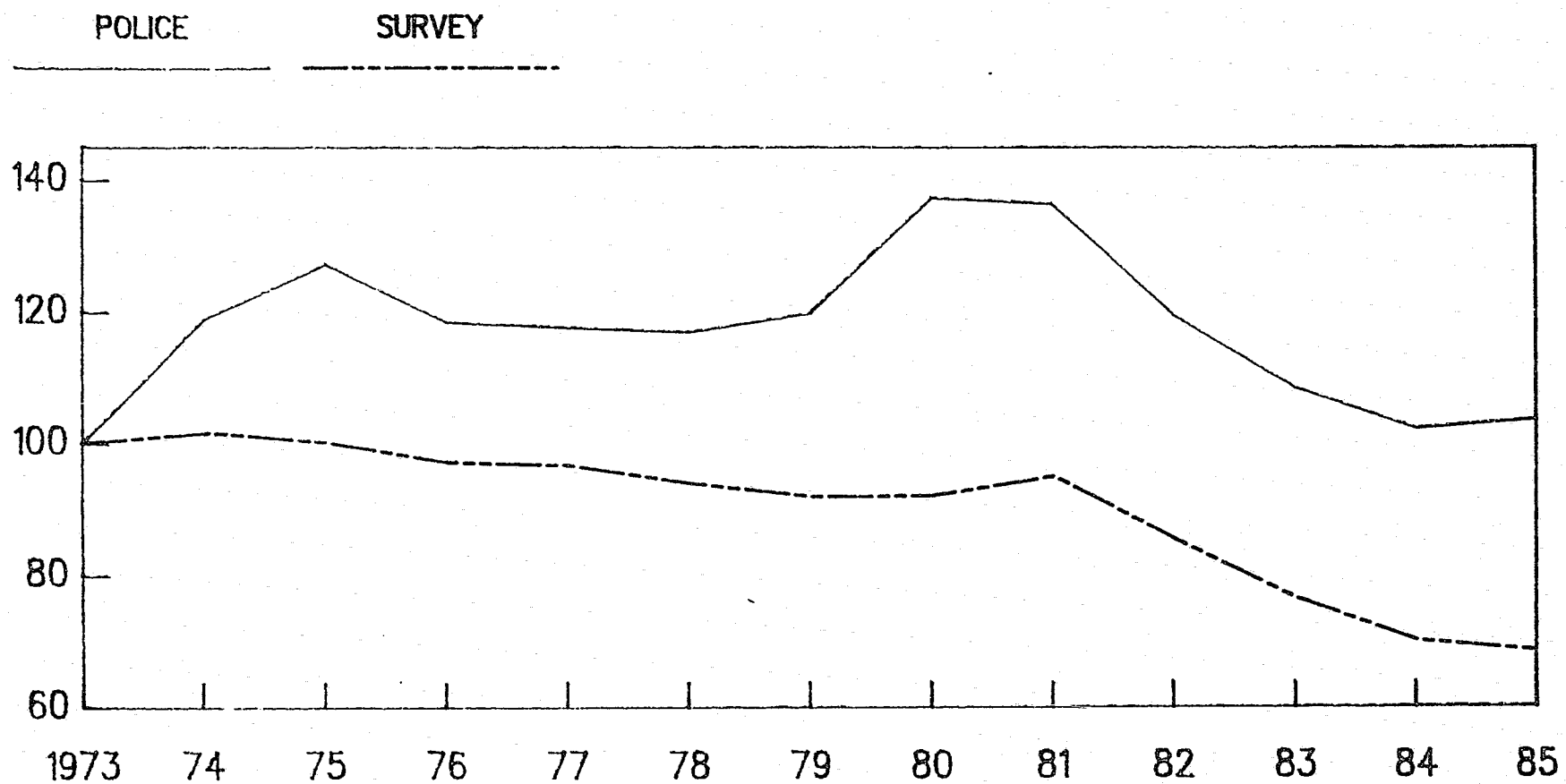


Rates per 10,000 households

Base: 1972=100

\* Unknown survey values

**FIGURE 3**  
RESIDENTIAL BURGLARIES IN THE US:  
SURVEY AND POLICE RATES



Rates per 10,000 households  
Base: 1973=100

The less favorable UCR figures almost certainly reflect changes in citizen reporting and police recording. The proportion of NCS burglaries said to have become known to the police has increased somewhat over the period, perhaps accounting for about 10% of the 1980 increase in UCR figures over 1973 (author's computations). The increase in survey reporting levels may partly reflect a decline in the young population covered by the NCS, who are least likely to report to the police (Biderman *et al.*, 1983), though this does not rule out some 'real' increase in reporting. This has perhaps been influenced by improving attitudes to the police (US Department of Justice, 1985:211) in turn possibly due to the fact that police departments are seen have become more ethnically reflective of their communities.

The divergence between NCS and UCR burglary trends - over 1973-1980 at least - may also signify more recording by the police of reported crime. In 1973, for example, about 50% of NCS residential burglaries said to have been known to the police were recorded, whereas by 1980 the figure was just over 70%; the 1985 figure was only slightly higher. The precise percentages here demand less attention than the general upward swing until 1980 in the proportion of 'reported' crime which were recorded.

In sum, then, survey evidence does much to endorse the point that, from a comparative viewpoint, police figures on residential burglaries at the beginning of the 1980s should not be taken at face value. Indeed, their comparability remains sufficiently uncertain as to suggest that survey results - notwithstanding their own difficulties - provide the sounder picture.

Principally, the indications are that English police figures in the early 1980s are inflated *relative* to North American ones. Most can be said about the reasons for this in comparison with the US - though many points may also apply to Canada. Survey evidence indicates that reporting of burglaries has risen in both the US and England. Technical complications make it difficult to use their survey series to compare reporting trends precisely, though the indications are that the increase in England was steeper over the 1973-1985 period. This is plausible. Telephone ownership went up by 80% between 1973 and 1985 (albeit more steeply in the 1970s than the 1980s), while such an increase was

precluded in the US because of consistently higher ownership rates. Moreover, there was a greater increase in owner-occupation in England (over 20% between 1972 and 1984, for instance) than in the US (3%). One can only guess whether owner-occupation *per se* affects reporting, but it means that more households have insurance cover for theft. In England, there was nearly a 10% increase between 1972 and 1985 in the number of homes with household contents insurance, though comparable US data are hard to come by.

Various factors could explain why recording has increased, both in the US and, perhaps even more so, in England. In the US, alleged changes in the responsiveness of the US police to victims and ethnic minority groups may have played a part, if not organizational changes. Lynch (1983), for instance, examining UCR figures between 1973-1979, points to more detailed record keeping systems, high technology information processing, and the increasing use of civilian staff which has probably reduced the scope for discretion with respect to recording. Similar trends may well have operated in England - and in ways that may help explain the continuing increase in police burglary figures. For example, England may have had more ground to make up - particularly from the late 1970s onwards - with respect to increasing computerization. And insofar as more police may mean more recorded crime, it is notable that the increase in police establishment in England has been greater than in the US: a survey of police employment trends in 88 US cities showed no increase in sworn officers and civilians between 1973 and 1982 (Bureau of Justice Statistics, 1986); in England there numbers rose by a fifth, with the steepest increase at the turn of the decade (Council of Europe, 1985).

Any greater reporting and recording 'push' in England over the period would explain its much steeper increase in residential burglary. It should be said, too, that the rise in police figures has not been confined to burglary. Burglary has shown one of the steepest increases, but there was a steeper upward trend in other offences in England than in the US over the 1970s, before the downturn in UCR figures for burglary and many other crimes began. As for the recent downturn in American - and Canadian - rates, it could reflect a 'real' contrast to England, notwithstanding the weaknesses of police trends. BCS estimates in England were significantly higher (21%,  $p < 0.05$ ) in 1983 than in 1981. The recent North American figures might of course be just a felicitous 'blip' - and US police rates increased slightly in 1985 over 1984. Nonetheless, the downward trend is a strong

one across most crime categories, and - in the US - is reflected in NCS rates.

Parenthetically, what might explain the respite from increasing crime in North America around 1980? Certainly sentences of imprisonment have increased recently, in particular in the US, though political rhetoric about harsher sentencing has to contend with weak empirical evidence that offenders are very responsive to changes in the penalties awaiting them. In any case, the chance of imprisonment for arrested burglars in the early 1980s was not dissimilar in England, the US and Canada (Lynch, 1987). Sentences of imprisonment have increased recently in England, too, though the effect on prison population figures have been mitigated by increasing use of parole (see Worrall and Pease, 1986). Another possibility is that declining North American rates reflect more energetic crime prevention programs, with community crime prevention schemes, for instance, at last beginning to bite into the number of easy opportunities for break-ins. Again, this is a palatable conclusion, but not one which is very empirically solid. Titus' (1984) review of community crime prevention, for instance, is more favorable than most (eg, Kelling, 1986; Rosenbaum, 1986), but even the effects Titus claims would not suggest that better residential security can account for the recent decline in rates. What about the higher unemployment rate in England? Establishing the association between crime and unemployment has generally proved difficult (eg, Council of Europe, 1985; Cantor and Land, 1985), though the notion has popular plausibility, and some of the more direct studies are in its favor (eg, Farrington *et al.*, 1986; Box, 1987). One last factor which is thought to bear on North American patterns is the decline in the size of high-offending age groups. The evidence for age effects is still under debate, though they have far from been ruled out (Sheley and Smith, 1985). Demographic trends in England have not exactly mirrored those in North America, where the drop in the highest-risk age group started rather earlier. There is possibly a clue here.

## SECTION IV

### CONCLUSIONS AND DISCUSSION

This section takes up, first, some issues about using police figures and crime survey data for comparative purposes, as illustrated in this study. Most points about police figures are not new. Those about crime survey data are newer, and have implications for future comparisons. The main thrust of the study was to document the processes of comparing crime survey information - and there have been few exercises of this sort. Nonetheless, some substantive findings have emerged, and the section ends with these.

#### POLICE STATISTICS FOR COMPARATIVE PURPOSES

Even for a relatively unproblematic offence measured in three reasonably similar countries, this exercise endorses the ritual warnings about comparing police figures. Without needing to labor the points, one threat to sound comparison comes from definitional and narrower classification differences. Here, one clear definitional difference was identified (pertaining to 'outbuilding' burglaries), while other uncertainties about the similarity of counting processes were underlined. A second problem is ascertaining whether similar proportions of victimizations are reported to the police. Some light could be thrown on this here, but this is less likely to be so in wider comparisons. A third difficulty is not knowing whether similar proportions of reported offences get *recorded* in police tallies - crucial to relative rates.

Definitional and classification issues bear on recording, but other procedural differences are also important: eg, enforcement and recording 'styles', computerization, workloads, staffing levels, and whether ancillary staff are used to filter reports from the public.

Research confirms recording differences within and between police forces at local level: at national level the differences may be greater.

The findings do not support the conventional argument that police figures are more useful for comparative *trend* analysis than for measuring how much crime a particular country has at any point in time. *Differential* changes in reporting and police recording will usually be imponderable - presenting a severe drawback for trend analysis. Contrasting



crime survey information from the US and England with police figures here showed that constancy of reporting and recording cannot be assumed. Even so, the precision with which reporting changes can be documented using surveys is not great, and to date the number of countries for which it would be applicable is small, for few have continuing survey programs. The position may improve with time, and if account can be taken of design differences, survey comparisons of reporting levels may be helpful. The difficulty of matching survey levels of reported crimes with police figures means that estimates of the proportion of reported offences which are recorded will be very loose - perhaps too loose to be of much help. Nonetheless, there are few other measures of recording changes.

Measuring crime through surveys has been attended by much methodological work on the direction and magnitude of measurement error. Police figures - though around considerably longer - have lacked comparable scrutiny. Even so, they will undoubtedly continue to be used in much comparative work. In the future, survey information may help plot their contours better, though more for individual countries than from a comparative viewpoint. Collecting police statistics directly from those who deal with them is undoubtedly advisable, though in wider comparisons this is less easy than using compilations such as those by Interpol and the United Nations. These would be less problematic if there was better documentation of the sources of the figures, of counting principles, and of the precise components of crime categories. Broad offence categories are often used in the belief that this can paper over the cracks, avoiding the problem of not knowing what goes where. It is a moot point whether this reduces errors or compounds them.

### **CRIME SURVEY DATA FOR COMPARATIVE PURPOSES**

When available, survey data offer an obvious alternative measure of crime to police statistics. And when the messages conflict, there will be more reason to calibrate from survey information which bypasses differences in reporting and police recording practices. One example of where survey results contradict police figures is Mayhew and Smith's (1985) comparison of England and Scotland: according to BCS data, levels of most offences were similar, in contrast to the picture from police figures of higher Scottish risks.

However, this comparison was facilitated by the use of the same survey instrument: an uncommon situation. Using published results from different, independently organized surveys to unravel how a wider number of countries are faring with crime is another

matter. They may give a misleading picture.

This is firmly endorsed in this study which examined data from only three surveys, and in relation to one offence: differences in survey design and administration were of clear importance. The differences fall into two groups: those for which data manipulation could take 'hard' account; and those which could be expected to affect measurement, but for which only 'soft' adjustments could be made as data manipulation was not feasible.

Of the first five differences, four compromised comparisons most:

- i. The exclusion in the BCS of a class of 'outbuilding' burglaries included in the other two surveys. Including these raised BCS rates substantially.
- ii. The inclusion in the CUVS of some no-force attempts. Excluding these reduced CUVS rates by some margin.
- iii. Different procedures for counting series offences in the surveys. Standardizing for series counting affected NCS rates most.
- iv. The inclusion in the NCS of victimizations reported by 'secondary' respondents. Omitting burglary reports by these respondents reduced NCS rates by some margin.

Of the other differences, the most important seemed to be:

- i. The effect of not having interviews bounded in the CUVS and BCS in the way NCS ones are. This is likely to produce inflated levels of victimization even with a longer 12-month reference period.
- ii. The way in which respondents are interviewed (by telephone, face-to-face, or both). Face-to-face interviewing will be more productive of victimization reports than telephone interviews.
- iii. Survey response levels, and the weighting procedures that are adopted to account for people who do not participate. Inadequate, or no weighting will depress victimization levels.
- iv. The use of screener questions to capture vandalism incidents. These can generate additional counts of offences eventually coded as burglary.

The two differences of overriding importance were seen to be: (i) definitional and classification differences; and (ii) whether and how interviews are bounded.

Among those who have considered comparing surveys, the need to standardize for *definitional* differences has received more attention than crime *classification* differences. In this case, *definitional* standardization added into the BCS a class of break-ins not conventionally included, while *classification* standardization excluded

from the CUVS a category of attempted crimes. There is a clear lesson from this for comparisons of other offence. in other set of surveys. Though precise information is required to do it, standardization on both fronts is feasible. Definitional and classification differences warn against relying on broad offence categories. This adds to the danger of 'populations' of offences being different, especially - but not only - as regards coverage of attempts and marginally criminal incidents.

The second major difference was the use of bounding, unique to NCS interviews. Other features of the NCS are more manageable (eg. secondary respondents) or less important (eg. mixed mode interviewing), but bounding is not. This makes comparisons with the NCS particularly difficult, even though its size and complexity aids data manipulation. This is unfortunate given that the US is of special interest for benchmark purposes, and that NCS questions have often been directly adopted with comparisons in mind. (The CUVS questionnaire, for instance, is virtually identical to the NCS.)

Other survey comparisons have generally shown NCS victimization rates higher than other countries, though the gaps may have been smaller if design differences had been accommodated. Thus, the configurations of other surveys will tend to an *overcount* of victimization relative to the NCS - insofar, at least, as they have, eg, unbounded interviews, full personal interviewing, and series counting. The effects here are likely to outweigh those of features which *inflate* NCS rates relative to other surveys (eg, secondary respondents, the short reference period, and non-response weighting). Nonetheless, this is a generalization: the change to rates after standardization will depend on the offences and the surveys considered.

The counting of series incidents, which has struck most alarm so far, was fairly important in this comparison (as unadjusted NCS figures exclude series incidents), but may be less so in comparisons of surveys where series are counted. An alternative approach to minimizing the series effect is to compare prevalence figures (households or persons victimized once or more). This may conceal differences between countries in the degree of multiple victimization (more likely as reference period length increases) - perhaps a small cost. One difficulty is the computation of prevalence rates for the NCS, which need

data linked from different interviews. Only a crude prevalence comparison was done in this study (showing relative positions much the same as on the basis of incident rates).

Even if survey analysts are aware of all the design differences identified in this study (heaven forbid that they think of others), they would be ill-advised to assume they 'wash out'. Here, for instance, unadjusted figures showed higher attempted burglary risks in Canada than America, not confirmed after standardization. 'Hard' adjustments altered the picture of relative risks, minimizing differences for completed burglaries at least. 'Soft' adjustments did not necessarily take one back to where one started: they sometimes gave a rate estimate closer to an unadjusted figure, but this depended on which type of burglary was being considered, and which pair of countries were being compared.

For different offences and survey configurations, methodology may affect results in ways and degrees other than was the case here. There seems little option but to get down to brass tacks with each survey, and each offence. This is likely to be so even comparing surveys more similar to each other than to the NCS. This study offers some guidelines as to the importance of design features such as bounding, reference period length and series counting, but these will not replace direct analysis. Whether it is possible to identify all relevant differences between surveys is questionable: and unknown differences may be as important as known ones. In this regard, 'house' or interviewer effects might be extremely important, at least in the light of evidence from the NCS Redesign (see p. 24).

#### *Implications for the future*

In view of this catalogue of difficulties, what are the implications for further work survey data? For one thing, it is necessary to confront the issue of pursuing rates comparisons, on which this study has focussed. Accommodating design differences is clearly complicated, and remaining unknowns are likely to leave rates estimates in the air. This may suggest abandoning rates comparisons in favor of comparative analysis of crime *patterns* (cf. Gottfredson, 1986). Nevertheless, league tables based on survey results have obvious appeal, especially given the limitations of police comparisons. Criminal justice personnel, and the public, are unlikely to cease to want confirmation of how they stand relative to other countries.

As important, though, is that the work involved in assessing how design differences affect rates is necessary to other analysis. In this regard, finer categorization of broad offence types - to unravel definitional and classification differences - can show how risks of different types of incidents vary. Here, for instance, respective levels of attempted and completed burglaries in the three countries were far from equivalent. Design differences may also permeate - and restrict comparison between - other variables conventionally brought into crime analysis. Comparison of losses from burglary, for instance, will be misleading if the 'population' of burglaries which are measured differ, as will analysis of time of occurrence or mode of entry. Design could also influence the picture of how far the correlates of victimization differ between countries, notwithstanding differences in absolute levels of risk.

This last point should not be overstated. Analysis done within different countries (there is little so far across them) indicate that both personal and household victimization is related in similar ways to major variables such as age and area of residence - a point returned to in relation to burglary. This is important, but does not undermine the need to be sure that any differences found are not the product of niceties such as who is asked what (and how), and what offences are filtered out (or elsewhere). The NCS procedure of including victimizations reported by secondary respondents, for instance, might confound comparisons of the relationship between risk and those variables (eg. income or housing type) themselves related to family size. The effect of losing movers in the NCS, similarly, might upset mobility and risk associations. Research in relation to personal victimization, too, shows that different reference periods change the relative productivity of respondents of different ages. With longer reference periods those most likely to be victimized (eg, the young and minority groups) *under-report* relative to others (Cantor, 1985; Kobilarchik *et al.*, 1983). This will alter the strength of the relationship between age and victimization in surveys with different reference periods. (Surveys of the commoner BCS/CUVS design, incidentally, should show *less* of a difference between age and risk than might actually be the case.)

If criminal justice personnel are interested in international comparisons, however, survey data should not be ruled out, for one because of the advantage over police figures of knowing the constituents of measurement, and the rough magnitude of various errors. However, more comparable surveys would mean coordinated action to ensure consistency in sampling, questionnaire construction and the definition and coverage of crime. There is a model of sorts for this from Scandinavia and Great Britain, but it is a lot to hope for. Countries without surveys will have varying funds available, and may find it easier to add victimization questions onto existing surveys - whose sampling, size, and so on, will often be fixed. They may also be reluctant to forego new methodology in the interests of using what has been tried and tested. Countries which already have surveys are probably even worse off. They will be hidebound by the need to maintain series consistency - which (as current NCS redesigners are only too aware) even minor changes can threaten.

The idea of major countries funding smaller, complementary surveys, perhaps concentrating on a limited range of offences, is worth considering. These could run alongside major national programs and be centrally overseen. Costs are a major problem. Some international criminological organization may be willing to make a financial contribution, though they may be more inclined to focus on less-developed countries which lack even police and court statistics. Another difficulty is that the new data would inevitably produce different results from regular series. This would keep survey analysts busy in conference papers, but would hardly help administrators and the public.

Another possibility is for different countries to finance an international polling agency (such as Gallup or Harris) to add victimization questions into ongoing polls. Indeed, some have already covered victimization, albeit with substantial comparability problems (eg, Gallup International, 1984). Criminologists may balk at this as producing information lacking in technical sophistication and detail; sample sizes, too, may be a problem. Nevertheless, there are some advantages: basic social-demographic data are already collected, and international fieldforces and data analysis capabilities are to hand, geared to producing quick results. Some input from criminologists would clearly be essential, though the job would no doubt attract some takers.

## DIFFERENCES IN BURGLARY

Apart these methodological lessons, this study can say something about differences in (i) levels of burglary in the US, Canada and England, and (ii) the pattern of their burglary problems. A discussion of these follows, incorporating a summary of main findings.

### *Levels of burglary*

Burglary risks from survey data relate to 1982 for the US; 1981 for Canada; and the average of 1981 and 1983 for England. Comparisons between the US and England are at national and city level; those incorporating Canada are for city areas only.

Standardizing for differences in survey design have been shown to be problematic, and firm figures on 'real' differences in risks are elusive. The best estimates from this study are that overall US burglary rates were appreciably higher than in England nationally (about 65% higher); US city rates were also higher, though not by as much (about 45%). Compared to Canada, US city rates are about 25% higher. Canadian city rates are about 15% higher than English ones.

This was the picture at the beginning of the 1980s, since when burglary levels in Canada and the US as recorded by the police have generally fallen, confirmed in the US by the NCS. (By NCS reckoning, for instance, US householders were at no greater risk of burglary in 1984 than in 1973). In England, the longer-term survey estimates indicate the steep increase in police figures is exaggerated, though 1983 *versus* 1981 estimates are consistent with a 'real' continuing upward trend. Though it is difficult to be certain, then, by the mid-1980s there may be a smaller gap in burglary risks between the three countries. Reasons for the singular downfall in North American rates are still much debated. It was suggested here that differences in sanctions or crime prevention initiatives perhaps explain the contrasting English picture less well than relative unemployment patterns and the effects of demographic changes. Critical, though, is whether the downfall in North American rates will be sustained.

For the beginning of the 1980s, survey estimates are at odds police figures - which are less plausible. Police figures shows early 1980 risks only a little higher in the US than in England, and virtually the same as in Canada. These 'point' estimates reflect

*differential* trends in burglary recorded by the police, which it has been argued could have been driven by differential changes in recording and reporting as much as by actual burglary levels. Survey results for both the US and England (particularly England) present a more heartening picture of what is happening to burglary trends than do police figures. This should give criminal justice administrators pause for thought, and those who mount, or want to mount, national surveys some useful ammunition.

English police figures may have risen particularly steeply because of more scope for both a reporting and recording 'push'. Several factors could be implicated here, among them increased telephone ownership, owner occupation and insurance coverage; a higher increase in police establishment may also play a part. Differential changes in reporting and recording apart, survey evidence nonetheless suggests that one reason for the relatively low US police figures is that reporting of burglary is lower than in England and Canada. This finding is consistent with Skogan's (1984) comparison reporting rates in the NCS and other surveys. Again it is difficult to explain, though lower insurance levels among high-risk households in rented property is one clue. Less confidence in the police among Americans is another possibility, though this is not able to be empirically well-backed. Attitudes to the police appear to have improved, though lower reporting is consistent with their still being less favorable than in England or Canada.

#### PATTERNS OF BURGLARY

There has been limited analysis in this study correlates of burglary risk in the three countries. Suffice it to say here that results from unadjusted survey data show generally similar associations between burglary victimization and the major variables brought into its analysis (2). This testifies to their explanatory strength, an important point for victimization theory. How far finer differences in patterns that are observable - or would be with more detailed analysis - can be explained in terms of design format is difficult to say; quite possibly some of them could be.

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2. Differential rates of burglary victimization have been dealt with by, eg, Hough (1984) and Gottfredson (1984) for the BCS; Ministry of the Solicitor General (forthcoming) for the CUVS; and Rand (1985) and Garofalo and Clarke (1986) for the NCS. For results from Holland, see Van Dijk and Steinmetz (1983) and Block (1984). Breen and Rottman (1985) deal with Ireland.



There is a broadly similar relationship between risk and household income. Those near the bottom and top ends of the income ladder seem most at risk, though in the CUVS, the very lowest income group was an exception. There seems no reason not to accept the conventional explanation for this: that low-income families are targets by virtue of their proximity to likely offenders, while high-income ones are the target of more professional offenders after better pickings (3). As regards tenure (related to income of course), a fairly stable finding is that renters are at greatest risk, partly no doubt because they live in the most criminogenic parts of urban areas. Risks also seem consistently lower for households headed by older people. Apartments and duplexes seem rather more likely targets than houses, though again partly explained by locality; the tallest apartments are not necessarily vulnerable, perhaps because when they house higher-income families they have better access security. In each country, a large proportion of burglaries happen in the daytime when houses are empty, and direct measures of daytime occupancy in the BCS and CUVS show householders who leave their homes empty the longest as most at risk; this is in line with American work using surrogate measures of occupancy to explain burglary levels and trends (eg, Cohen and Cantor, 1981; Block, 1984). It may be that slightly more English burglaries are committed at night (author's computations), though the differences are not large and may be a function of different offence coverage (4). It may also be (though it is difficult to match the data well) that rather more burglaries in the US take place while householders are at home, with more of them involving some sort of direct confrontation with a household member.

This study itself has revealed some *differences* in burglary patterns.

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3. The NCS shows a slightly different risk/income relationship for whites and non-whites. For black households, middle-income families faced some of the highest risks: this may be because they are less likely, or able to distance themselves from higher crime areas, and remaining within these might become the most attractive targets. Household income was only measured in the 1984 BCS (in 1982 personal income was measured). There is much missing data (as the British are somewhat reluctant to state income levels) weakening analysis possibilities further. In the 1984 BCS, those living in 'high status non-family areas' and those in the 'poorest council estates' were shown to face the highest risks of burglary (Hough and Mayhew, 1985). This roughly matches the curvilinear association with income found in the CUVs and NCS.

4. Excluding respondents who could not say when their burglaries occurred, the figures show 58% of BCS burglaries to be committed at 'night' (ie. between 6.00pm and 6.00am), as opposed to 55% in the CUVS and 52% in the NCS.

- \* Dissimilarity in what the police in the three countries count under a commercial (or non-residential) heading cannot be ruled out, but the commercial burglary rate, and its size relative to the residential problem, seems lowest in the US.
- \* Within the category of completed residential offences, US homes seem more susceptible to 'walk-in' (no-force entries) than in Canada. (The comparison was not possible for England). Survey data indicated that 'walk-in' burglaries were at least 30% higher in the US, though forcible entries were on a par. 'Soft' adjustments would if anything increase this figure.
- \* US householders, particularly compared to English ones, face a greater risk of burglars getting into the home. Adjusted survey figures show a US completed burglary rate 85% higher than England nationally; completed burglary is also more common than in Canada though not by as much (25%). Risks of burglars trying to break in (attempts) were only 25% higher nationally in the US than England; there were about 30% higher than in Canada.
- \* There appears to be a different geographical distribution of burglary in the US and England, for which both national and city rates could be compared. City areas in the US are certainly the most vulnerable to burglary, but the fall-off in risks outside cities was less steep than in England.

Various explanations can be offered for these differences, which cannot always be empirically well-backed. Moreover, the game is the easy one of post-hoc theorizing, explaining differences after they have been observed. Burglars' habits and target preferences is not well-understood within any one country despite a significant amount of research: explaining across-country differences is inevitably speculative. Nonetheless, underpinning the suggestions made below is the notion that, whatever levels of relative risk, they are not necessarily arising from the same *types* of burglars, using the same *tactics*. Nor will they necessarily be after the same *rewards*: pre-payment gas and electricity meters, for instance, offer English burglars a singular source of cash particularly in public housing and rented accommodation. Patterns of burglary in each country, in other words, may reflect burglars' responses to different opportunities. The

stronger differences between the US and England often illustrate these points best.

### *Commercial burglary*

Given the smaller numbers of commercial targets, the *risk* of commercial burglary will be higher than for dwellings in each country, perhaps also reflecting better rewards. (Rand *et al.* (1983) show commercial burglary risks to be five times higher than residential ones in the US). Commercial burglary nonetheless seems a relatively less important problem in the US. Why might this be so? The ratio of commercial establishments to population in the countries could differ, though for now one would assume not. The security of commercial properties may - relative to residential targets - differ too, though again the assumption would be not. Reporting of commercial burglary may be a difference, but since reporting of commercial crimes is generally high, there might be little scope for a reporting difference to be operating.

Instead, the relatively smaller problem of commercial burglary in the US may reflect differences in the best opportunities burglars have. Environmental differences may offer US burglars easier residential targets than in England, in terms, for instance, of lower housing density, better access to dwellings etc (see below). In these terms, Canada is more similar to the US than to England, though there might be vulnerability differences even so as regards the likelihood of findings homes with doors and windows unlocked (see again below). Another explanation may be that commercial premises in the US are protected more by the increased 'guardianship' provided by shops and business premises being open longer hours - an explanation which has featured in explaining higher commercial burglary rates in Holland than in the US (Block, 1984).

### *Non-force entries in Canada and the US*

There are various explanations for the higher rate of non-force entries in the US. For instance, the lowest and highest US income groups are most at risk of no-force burglaries (Rand, 1985); less income disparity in Canadian cities would mean fewer targets that are susceptible (for whatever reason) to walk-in entries. In the US, rented property and housing structures with about 4-9 units are particularly vulnerable to no-force entries; different relative numbers of such targets in US and Canadian cities might also be

implicated. The number of houses likely to be unoccupied during the day does not seem greatly different, though for what it is worth the number of working women is fractionally smaller in Canada generally, and the proportion of large households higher (5).

Canadians might prefer to explain their lower risks of no-force entries by their city householders being more security-conscious, leaving fewer homes unlocked. Good comparative evidence on security behavior is hard to come by, though it seems unlikely that more secure Canadian homes is the answer. In both countries, there are high levels of awareness of, and even participation in Neighborhood Watch and property marking schemes, which denote a tendency to keep property reasonably well secured. Moreover, there may well be more of a security mentality in American cities, the product of widespread belief in the pervasive risks of both property and violent crime.

Climate may be the best clue to explaining the higher US rate of no-force entries - and indeed higher levels of other types of burglary, given that Canadian housing patterns may not differ much from American ones. Daytime burglaries constitute a major problem in each country, and these rise in summer months. But, specifically, rates of no-force entries in the US are highest between April and November, showing more seasonal fluctuation than forcible or attempted entries (Rand, 1985). This will reflect the greater tendency to leave windows and doors open during warm months, notwithstanding the fact that US householders are more likely to have central air conditioning. The same general seasonal pattern is apparent in Canada (Ministry of the Solicitor General, forthcoming), but the shorter period of very hot weather in Canada may well mean Canadians hold open house for less time.

*Who gets in, how often, and where*

What about the findings about the broader equivalence of attempted than completed burglary in the three countries, and what looks like a less strong association with urbanization and burglary victimization in the US than in England? Social and environmental

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5. Females as a percentage of the total labor force in 1982 were 43% in the US, 42% in Canada, and 40% (see later) in England (US Department of Commerce, 1985). The proportion of households with four or more people was 28% in the US in 1982, 33% in Canada, and 27% in England (various sources).

differences may be at work in keeping English burglary rates (especially completed burglaries) lower than in North America, where - to put it broadly - burglars may have an easier time of it. For one, there will be rather more daytime occupancy in England. The proportion of large households is no higher in England than in the US, and is smaller than in Canada. However, there is a larger elderly, housebound population (6). And employment patterns differ too: the proportion of women who work is slightly smaller, while the higher unemployment rate may have the single advantage of keeping someone in during the day. Perhaps even more crucial are environmental differences. Complex street patterns, for instance, are more typical of England, and these have been shown to decrease residential risks (Beavon, 1985; Poyner *et al.*, 1985). More important is that England is a much more densely populated country than the two North American ones, even in urban areas. This will be reflected in smaller gardens, more terraced housing, houses generally nearer together, and fewer potential entry points, all factors which are related to lower burglary risks no doubt because decrease the amount of cover, and increase the chance of neighbors being able to spot suspicious behaviour (Waller and Okihiro, 1978; Winchester and Jackson, 1982; Mayhew, 1984; Poyner *et al.*, 1985). Overall, then, environmental constraints in England may be more of deterrence to potential burglars - consistent with lower completed burglary rates. Or, if burglars start to try and get in, they may make them more likely to have to give up - consistent with the relatively higher proportion of attempts. It would be much better to measure all these environmental and housing differences at national and city level, and with effort it could be done. The idea that the relatively greater proportion of attempts in England denotes greater home security cannot be ruled out, though it is difficult to square with the greater general concern about crime in N. America, and with the greater ability to purchase security hardware.

The more equivalent risks in city and suburban areas in the US in contrast to England are in line with Block's (1984) comparison of US and Dutch burglary patterns. This too may denote differential opportunities. The 'suburbanization' of American crime has received attention recently, with several explanations drawn upon. Sampson (1986), for instance, argues that what is at issue, apart from urbanization, is 'structural density' (ie. the proportion of multiple dwelling units); this may be having greater effect in rural and

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6. In 1982, 15% of the population was aged 65 or more in England, as against 11% in the US and 9% in Canada (World Bank, 1983).

suburban areas in the US insofar as new apartments in these localities are more attractive targets. A similar notion is that 'transitional' areas, previously characteristic of inner cities, have diffused into suburbia (Stahura and Huff, 1986). Changes in criminal mobility patterns have also been brought in, both in terms of offenders themselves leaving cities, or if staying put, exploiting outlying areas more (eg. Lenz, 1986). In brief, then, the suburbanization of burglary may be another example of America getting there first; data on respective patterns of residential mobility would help substantiate this.

Such differences are hardly likely to be the whole story in explaining why burglary hits out-of-town areas in America more, or indeed other differences in burglary patterns: the proportions of various types of burglars who are operating will be important too. Differential propensity to commit burglary, and other crimes, has featured large in other country's judgments about American crime rates, though whether more Americans are inclined to commit crime, or a similar proportion inclined to commit it more often, remains in the air. Nonetheless, if higher rates of completed burglary in the US do not just indicate a greater overall criminal propensity (or more unlocked homes in the summer), they may at least signify more single-minded offenders. Possibly the influence of drugs, or the need to finance a drug habit, may spur more people in America (or the same proportion more often) to break into homes, and be less deterred - if they have to - from tackling locked windows and doors. Higher US rates may also reflect a higher proportion of more professional offenders. Age profiles and arrest chances are problematic indices here, but for what they are worth they show burglars in America to be rather older compared to English ones, and less likely to be arrested: clearance by arrest rates in 1982 were lower in the US (15%) than in England (29%) or Canada (22%); arrested burglars were also rather older in the US (39% were 21 or over, as against 32% in England for instance). The higher out-of-city burglary rates in the US may be particularly sustained by older, more professional offenders, who may indeed reside themselves in the out-of-town localities. Even if not, they are more likely to be prepared, and able to stray to further afield because more of them will have cars (6). English burglars, who let us suppose reside predominantly in cities, will tend to choose targets closer to home to the extent that they are younger, more inexperienced and less likely to have access to 'wheels'.

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6. The legal driving age in England is higher - at 18 - than in the US, and per capita registration very considerably lower. In 1982, there were more than twice as many cars registered per head in the US than in England. The difference in the availability of cars to young men will be even greater.

## APPENDIX A

### DIFFERENCES BETWEEN THE SURVEYS

#### HIERARCHICAL CLASSIFICATION PROCEDURES

In all three surveys, incidents involving sexual attacks and the use or threat of force with theft (ie. robbery) are reclassified, but different procedures operate with regard to burglary involving assaults on or threats to the victim. In the NCS and CUVS, the threshold of classification is lower, with burglaries involving even simple assaults being reclassified. In the BCS, only burglary with an element of serious wounding is reclassified.

#### SERIES OFFENCES

##### (i) NCS

In the NCS, series offences have usually been excluded from the number of crimes on which rates are calculated. Incidents were counted as series if three conditions were met:

- i. there were at least three incidents in the series;
- ii. all incidents were very similar in key details, so that they would classify as the same type of offence; and
- iii. the respondent was unable to report details about the individual incidents separately, even after extensive probing.

For 1982, respondents' accounts of several incidents treated as one series comprised about 3% of all burglary, counting each series as a single crime.

##### (ii) CUVS

Series have been counted as one incident for rate purposes. The second and third NCS condition for the definition of series incidents holds, but there had to be at least five incidents in the series. Reflecting this, series incidents comprised about only 0.9% of all 1981 burglaries, counting each series as a single crime.

##### (iii) BCS

Interviewers could treat two or more incidents as series provided that they were all very similar in type, were done under the same circumstances and probably committed by the same person. Series were given a score equal to the number of incidents in the series occurring in the calendar year for which annual rates were calculated, with an arbitrary top limit of five. About 9% of burglaries from the combined 1981/83 data were of a series type, counting each series as a single crime; the figure is much lower - similar to the NCS 3% - for series comprising three or more incidents.

#### TYPES OF PROPERTY COVERED

Apart from the treatment of break-ins to outbuildings, there is a further less important, difference. The NCS and BCS in this case specifically include as burglaries thefts occurring while any of the household respondents were in hotels. The CUVS questionnaire

did not emphasize this, though the effect is unlikely to be large. In the NCS, for instance, only some 5% of burglaries overall were targeted at second homes and hotels.

### **RESPONDENT ELIGIBILITY FOR REPORTING HOUSEHOLD CRIMES**

In the BCS and CUVS, one person aged 16 and over was interviewed per household. This person reported on household crimes and those personal ones which involved him/herself. In the NCS, personal victimization reports are elicited from all members of the household; the age-limit for inclusion in the survey is 12, though victimizations for 12-13 year olds are reported by proxy. A selected respondent (in 1982, one aged 18 or over) is designated to report on household crimes, though any crime emanating from individual screen questions which is subsequently classified as a burglary is counted (cf. Dodge, 1985).

### **THE TREATMENT OF NO-FORCE ATTEMPTS**

'No-force' attempts are deliberately excluded in the NCS; indeed attempts are labelled 'Attempted Forcible Entry'. No-force incidents, however, are counted (and in some cases separately labelled) in the CUVS; they comprise 24% of attempted burglaries.

In the BCS, fuller information was collected in the first sweep of the survey on the use of force in burglaries. Even so, the questions asked about mode of entry (and attempted entry) differs from those in the NCS and CUVS. This makes it difficult to replicate the NCS category of forcible attempts, and the CUVS ones of forcible and no-force attempts. However, it is known that a large number - 24% - of potential attempted burglary Incident Forms were discounted on grounds of insufficient evidence that an attempt had taken place. The figure is identical to the CUVS count of non-force attempts, making it reasonable to suppose that the BCS does not in effect count them. The population of attempted offences covered by the BCS is similar, that is, to that in the NCS.

### **REFERENCE PERIOD**

In both the BCS and CUVS, respondents were interviewed roughly in the first two months of the year. However, whereas in the BCS respondents reported on victimizations up until the point of the interview, in the CUVS they were asked only about incidents that happened up until the 31 December. No account has been taken of this difference, as its effect is difficult to quantify, and may not be very great. 'Recency bias' might suggest that CUVS respondents 'pulled out' some 1981 offences into 1982, though it might equally be argued that the perceived need to satisfy interviewers with a victimization report of some kind counterbalanced this. On this view, some 1982 offences would have been 'backward telescoped' into the reference period.

The recall curve for burglaries in the BCS shows a larger number of incidents reported in the first quarter of the main calendar year than would be expected taking seasonal patterns into account. This will reflect 'forward telescoping'. The period after the end of the calendar year produces slightly higher numbers of incidents than would be expected on a pro-rata basis, no doubt because of the 'recency bias'. Since these incidents are not included in annual rates, they might to an extent balance out the inflation at the beginning of the recall period due to 'telescoping in'. Although the recall curve does evidence telescoping and recency bias, the effects are not as marked as might be imagined given the relatively long reference period. The patterns for different types of burglaries, moreover, are not dissimilar; in particular, the patterns for attempted crimes mirrors that of higher-loss burglaries.

### **BOUNDING**

Only the NCS uses 'properly' bounded interviews for rate estimations. Bounding is achieved through: omitting victimization reports obtained in the first interview from



rate calculations: and by checking in the field that offences reported in the present interview were not reported in the previous one. However, since the NCS uses a probability sample of addresses rather than designated households or persons, not all the interviews conducted in returning rotation groups are subject to bounding: interviews in replacement households and in households that were 'noninterview' or not in sample in the previous period are unbounded.

## RESPONSE RATES

Of the 'household' non-responders in the NCS (3.5%), rather less than a third refused, while for the remainder a suitable respondent was not identified (Penick and Owens, 1976). There is an additional 1% or so 'person' non-response within responding households. In the CUVS, response rates are based on the proportion of all randomly-digit-dialled numbers reaching a residence in which an interview was obtained (Hofmann and Catlin, 1985). Most householders not interviewed refused. In the BCS, about half of the non-interviewed households refused interviews, and half were not contacted despite repeated personal visits. In the BCS, persons not interviewed were reasonably similar in standard demographic terms to those interviewed, though this is not to say that they will be as similar in terms of victimization experience. There was some undercount of 16-19 years olds (see, eg, NOP, 1985), though this would produce less bias for burglary estimates than for personal offences.

Some estimate of the effect of non-response can be made from an analysis of differences between NCS respondents who dropped out of the sample after one interview. Biderman and Cantor (1984) shows these to have a victimization rate 1.7 times higher than those interviewed at least three times; about 16% of the NCS sample at any one time is with persons that will only have one interview. If one assumed that this 16% are the same in terms of victimization-proneness as those who do not enter the BCS or CUVS samples, then it can be estimated that the NCS victimization rate would be about 35% higher on this account. This, however, almost is an overstatement. For instance, the figure of 1.7 refers to all victimizations, not burglary specifically, and it is based on unbounded rates for a six month recall period; it is also doubtful whether all non-responders in the two other surveys are as highly victimised (Cantor, personal communication).

## CLASSIFICATION FILTERS

While all three surveys delete a number of cases which are 'out-of-scope' (eg, not in the reference period or have commercial establishments as their targets), it is difficult to get comparative figures. After this stage, both the NCS and the CUVS rely on computer coding to decide a 'type of crime' classification for each Incident Form. The NCS edits out about 5% of Incident Forms as not meeting the criteria laid down for counting an incident as burglary; this figure does not include 'out-of-scope' cases (Tinari, personal communication). In the CUVS, there is no available figure on the number of discounted forms, but they are thought to be virtually non-existent (Johnson, personal communication).

The manual procedures adopted in the BCS were decided on in large part because one aim of the survey was to try and match police statistics as closely as possible. Thus, crime classification coding was done by a very small number of specially trained coders, working to a classification document which showed how the major sequences of responses were to be handled in terms of type of offence. Incidents which were ambiguous, only marginally criminal, or problematic in some other way, were referred to police statistical officers and the crime survey personnel in the Home Office. Judgement was then made as to whether the offence was in-scope, and likely to be counted a crime if it came to the attention of the police. Of all burglary Incident Report Forms, some 4% were discounted.

## DIFFERENCES IN SCREENER QUESTION OPTIONS

In the CUVS, the first vandalism screener, close to the beginning of the list related to damage to vehicles. The second screener - on damage to household property - came at the end of the list. Both screeners gave rise to some burglary codes, the latter more than the former. They were more likely to lead to a classification of attempted burglary than completed. One might suppose that incidents which were mentioned in relation to the second, very late screener would not otherwise have been picked up: 1% and 8% respectively of completed and attempted burglaries emanated from this second screener.

In the BCS, the vandalism screener question did not have such an impact, even though there were more of them; on the basis of 1982 BCS evidence about 2-3% of attempted burglaries emanated from these screeners; the figures are negligible for completed offences. The difference in figures between the CUVS and BCS may reflect looser crime classification controls in the CUVS insofar as the incidents involved provide evidence of burglary only through some damage committed. Most the relevant incidents classified as burglary were in fact damage-only incidents.

## APPENDIX B

## THE EFFECTS OF ADJUSTMENTS

Table B:1 shows the independent effect of each of the adjustments made to the three surveys' data to improve comparability on the unadjusted rate of burglary per 10,000 households. For the NCS and the BCS, the effects relate to national-level data. The effects of adjustments were broadly the same for city-level data, though the inclusion of outbuilding burglaries into the BCS had less effect on city rates. This may reflect the fact that residential properties have fewer outbuildings in urban areas.

Table B:1

THE EFFECTS OF ADJUSTMENTS ON  
ATTEMPTED (A), COMPLETED (C), AND TOTAL (T) BURGLARY RATES:  
US, ENGLAND AND CANADA

	US (NCS) (national)			ENGLAND (BCS) (national)			CANADA (CUVS) (city)		
	A	C	T	A	C	T	A	C	T
Unadjusted rate per 10,000 households	191	591	782	178	271	450	307	630	937
1 Expanded definition of burglary	+3%	+5%	+5%	+<1%	+<1%	+<1%	+9%	+6%	+7%
2 Series counted as in BCS	+11%	+15%	+14%	-	-	-	+1%	+4%	+3%
3 Outbuilding burg- laries added	-	-	-	+4%	+40%	+26%	-	-	-
4 Second'y respondents excluded	-9%	-13%	-12%	-	-	-	-	-	-
5 No-force attempts excluded	-	-	-	-	-	-	-22%	-	-7%
1 + 2	+15%	+21%	+20%	-	-	-	+11%	+10%	+11%
1 + 2 + 4	+4%	+4%	+4%	-	-	-	-	-	-
1 + 2 + 5	-	-	-	-	-	-	-14%	+11%	+3%

Note: For the second adjustment, series incidents are counted at face value between three and five; series of more than five are counted as five.

## APPENDIX C

### ALTERNATIVE TREATMENT OF SERIES OFFENCES

Adjusted incidence rates presented in Section II are influenced by procedures adopted for counting series incidents. Using BCS methods, independent of other adjustments, raised published NCS (national) and CUVS (cities) total burglary rates by 14% and 3% respectively.

The three surveys have different series thresholds, so that a simple exclusion of series cases for the CUVS and the BCS does not achieve strict comparability with the NCS. Nevertheless, for illustrative purposes, Table C:1 shows the effect, in contrast to full BCS series counting (column 1) of (i) counting series as one in all three surveys (column 2), and (ii) excluding series incidents from the CUVS and the BCS altogether (column 3). For all these three series counting options, other design factors have been standardized: ie, (i) reports from secondary respondents in the NCS have been excluded; (ii) an expanded definition of completed burglary is used in all three surveys; and (iii) no-force attempts are excluded from the CUVS. The rates for the NCS and BCS apply to city areas as defined in Section II.

It can be seen that, after other standardizations, not counting series (the current NCS approach) had most effect on BCS rates, reducing them by 19%; this is consistent with the lower series threshold employed in the BCS. Reflecting the higher threshold in the CUVS, its rates were least affected (they were reduced by only 4%). For the NCS, not counting series at all gave rates 10% lower than if BCS procedures were followed.

Table C:1

**CITY BURGLARY RATES IN THE US, ENGLAND AND CANADA:  
THE EFFECT OF ALTERNATIVE TREATMENT OF 'SERIES' INCIDENTS**

	STANDARDIZED RATES		
	1 Series counted as in BCS	2 Series counted as one	3 Series not counted
		<u>% decrease on column 1</u>	
<i>US (NCS)</i>			
Attempts	299	- 8%	-10%
Completed	768	- 8%	-10%
Total	1066	- 8%	-10%
<i>ENGLAND (BCS)</i>			
Attempts	289	-14%	-16%
Completed	545	-10%	-21%
Total	834	-11%	-19%
<i>CANADA (CUVS)</i>			
Attempts	269	- 2%	- 3%
Completed	696	- 4%	- 5%
Total	965	- 4%	- 4%

Note: \* Standardized rates: to maximize comparability, these rates are based on the 'expanded' definition of burglary (with BCS burglaries including those to outbuildings); with reports of burglary victimization from secondary respondents in the NCS excluded; and CUVS no-force attempts excluded.

## APPENDIX D

### POLICE FIGURES

Definitions of burglary in the US, Canada and England are similar, covering the act of entering a building as a trespasser with the intention of committing a crime. 'Hierarchical' counting principles also appear similar (for instance with regard to break-ins accompanied by assault on the householder). For the US police figures recorded in the *Uniform Crime Reports*, a so-called 'hotel rule' applies for counting as one incident separate burglaries to multi-unit dwellings reported by one person. This does not seem to apply in Canada or England. This could affect relative burglary counts, though it is unlikely to be much as it is unclear how strictly the hotel rule is applied (see Poggio *et al.* 1985).

#### *Residential and non-residential offences*

In the US, a breakdown between residential and non-residential burglary is available from a large subset of the police agencies who report to the UCR program. In Figures 1 and 3, the percentages of residential and non-residential burglaries from this subset has been applied to the national total of all burglaries.

In England, a distinction is made between 'burglaries in a dwelling' and those in premises other than a dwelling. A small number of 'aggravated burglaries' are included in each.

In Canada, a three-fold distinction is drawn between 'residential' burglaries, 'commercial' burglaries, and 'other' burglaries. In Figure 1, commercial and other burglaries have been combined to produce the 'non-residential' figures, with the adjustment described below.

#### *Classification differences*

The data in Section III takes account of one known classification difference between the US and the two other countries. This is that UCR residential burglaries (and attempts) encompass break-ins to all buildings (sheds, attached and unattached garages, etc) on the housing plot. In contrast, the part of the Englishman's and Canadian's castle which burglary violates comprises only the housing unit, and any building directly attached to the house; thefts from unattached buildings around the house go in 'burglaries other than in a dwelling' in the English case, and in 'other burglaries' in the Canadian. No information is available for Canada as to the number of 'outbuilding' burglaries which go into the residual category. In England, the indications are that, in 1983, about 8% of burglaries which were classified as 'non-residential' may have had domestic structures unattached to the house - outbuildings - as their target (estimated from Home Office (1984), unpublished). No data are available for earlier years, but the assumption has been made that the 8% figure has been constant since 1972. A more adventurous assumption is made that the same 8% of 'other' burglaries in Canada were to outbuildings, again a constant figure over the period. This adjustment, of course, only affects the level of non-residential and residential burglaries in the two countries, not the trend.

#### *Comparing police and survey burglary levels*

Comparing offences levels through police and survey figures is difficult both for point-in-time comparisons and for contrasting trends. Sampling error on survey statistics is one problem, while non-sampling errors figures can jeopardize survey trends to the extent that the errors change over time. Neither do survey and police statistics simply

capture different quantities of burglary events (cf. Block and Block, 1984; O'Brien, 1985). There will be some overlap between them, with survey figures much boosted by crimes which go unreported. Even so, surveys will not reveal all offences known to the police. Nor will police figures match offences defined which respondents say became known to the police. Both the NCS and BCS show evidence of a shortfall between 'reported' burglaries and the numbers of burglaries recorded in police statistics. Some respondents, for instance, may feel embarrassed to admit they have not reported an offence and will say they have done so; or they may think the offence was notified to the police if they informed other authorities. On the police side, case reports can get lost, survey 'burglaries' get put into other police categories, or are 'unfounded' through lack of proof, doubts about the veracity of the report, or reluctance to enter into the police count offences which have little promise of being 'cleared up'.

#### *Comparing UCR and NCS figures*

The discrepancy between UCR and NCS figures for all offences which can be matched in the two series has already received attention (Poggio *et al.*, 1985; Biderman *et al.*, 1983; Biderman and Lynch, 1984). Biderman *et al.* have shown that the discrepancies can be reduced by adjusting for differences in the scope of crime each measure covers, for procedural differences in the construction of crimes, and for differences in the population counts that each source uses for rate denominators. With regard to a UCR/NCS burglary comparison, some of the discrepancy problem are not relevant. As far as level estimates are concerned, the effect of remaining inconsistencies partly cancel each other out, though NCS rates may be artificially inflated a little relative to UCR ones. The principal reason for this is that the UCR requires the police to make a judgement of intent to steal or commit a felony on the part of the offender. In practice, the police may have difficulty in deciding intent, but nonetheless some crimes classified as burglary in the NCS would be treated as vandalism or trespassing in UCR counts. This will *inflate* NCS burglaries relative to UCR ones, perhaps in the region of 7%-10% (Poggio *et al.*, 1985).

#### *Comparing UCR and NCS trends*

The recent downfall in NCS rates may be exaggerated by a fall in the proportion of interviews with 'replacement' households, for which conventional 'bounding' procedures do not apply (see Section II). These 'unbounded' interviews have been fewer since the turn of the decade on account of lower mobility associated with a period of relative economic recession; given that they are more productive of victimizations than bounded ones, this will have artificially reduced very recent NCS rates (see Biderman and Lynch, 1984). Conversely, part of the increase in UCR rates during the later part of the 1970s may reflect the fact that the UCR uses intercensal estimates that increasingly undercount the population as the end of the decade progresses. This artificially increases the crime rate at the end of the decade (see Biderman *et al.*, 1983). NCS weighting procedures mean that NCS rates were little affected by these intercensal underestimates.

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