

**ICPSR**  
**Inter-university Consortium for**  
**Political and Social Research**

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**Violence and Crime in Cross-National  
Perspective, 1900-1972**

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Dane Archer and Rosemary Gartner

ICPSR 8612

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Violence & Crime in Cross-  
National Perspective,  
1900-1972

(ICPSR 8612)

Principal Investigator

Dane Archer

Rosemary Gartner

**NCJRS**

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**ACQUISITIONS**

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### Acknowledgement of Assistance

All manuscripts utilizing data made available through the Consortium should acknowledge that fact as well as identify the original collector of the data. The ICPSR Council urges all users of the ICPSR Data facilities to follow some adaptation of this statement with the parentheses indicating items to be filled in appropriately or deleted by the individual user.

The data (and tabulations) utilized in this (publication) were made available (in part) by the Inter-university Consortium for Political and Social Research. The data for VIOLENCE & CRIME IN CROSS-NATIONAL PERSPECTIVE, 1900-1972 LIST, 1985 [UNITED STATES] were collected by Dane Archer and Rosemary Gartner. Neither the collectors of the original data nor the Consortium bear any responsibility for the analyses or interpretations presented here.

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Archer, Dane and Rosemary Gartner

VIOLENCE & CRIME IN CROSS-NATIONAL PERSPECTIVE, 1900-1972 (ICPSR 8612)

SUMMARY: The purpose of this data collection was to provide comparative, cross-national, longitudinal data on rates of violent and non-violent crimes for every country in the world over a period of 72 years. The study was designed to supply both comparative breadth and historical depth. Information is included on murder, rape, robbery, assault, and thefts. CLASS IV

UNIVERSE: All countries in the world.

NOTE: The machine-readable documentation includes two FORTRAN command files which can be used in conjunction with the data files to output crime rates and raw crime figures for each country. The only documentation provided for these files is the comment statements included in the files.

RESTRICTIONS: Users are asked to send copies of articles based on the data to: Dane Archer, Stevenson College, Univ. of Calif., Santa Cruz, CA 95064 (408) 429-2555 or (408) 426-1186

EXTENT OF COLLECTION: 2 data files + machine-readable documentation  
DATA FORMAT: Card Image

PART 1: Crime File  
FILE STRUCTURE: rectangular  
CASES: 4568  
VARIABLES: 12  
RECORD LENGTH: 80  
RECORDS PER CASE: 1

PART 2: Eight Crime File  
FILE STRUCTURE: rectangular  
CASES: 3520  
VARIABLES: 11  
RECORD LENGTH: 80  
RECORDS PER CASE: 1

RELATED PUBLICATIONS:

Archer, Dane, and Rosemary Gartner. VIOLENCE AND CRIME IN A CROSS-NATIONAL PERSPECTIVE. New Haven: Yale University Press, 1984



# Comparative Crime Data File

<u>VARIABLE NAME</u>	<u>COLUMN LOCATION</u>
Country Code	1-3
Year (eg. "62" for 1962)	4-5
Constant Value "1" (unused)	6-7
Number of Murders	8-13
Number of Manslaughters	14-19
Number of Homicides	20-25
Number of Rapes	26-31
Number of Assaults	32-38
Number of Robberies	39-45
Number of Thefts	46-53
Population (in thousands)	54-59
=====	
Missing Data Code	-1

COUNTRIESCODE

Aden	56
Argentina	15
Australia	20
Austria	25
Bahrain	30
Belgium	35
Bermuda	40
Bolivia	45
Botswana	50
Brunei	55
Bulgaria	60
Burma	65
Cameroon	70
Canada	75
Central African Republic	80
Chad	90
Chile	95
China	100
Columbia	105
Congo	110
Cuba	115
Cyprus	120
Dahomey	125
Denmark	130
Dominican Republic	135
Egypt	140
El Salvador	145

COUNTRIESCODE

England and Wales	150
Ethiopia	155
Fiji	160
Finland	165
France	170
Germany	175
Gahana	180
Greece	185
Guam	195
Guyana	190
Hong Kong	200
Hungary	205
Iceland	210
India	225
Indonesia	230
Iran	215
Iraq	220
Ireland	235
Israel	240
Italy	245
Ivory Coast	250
Jamaica	255
Japan	260
Jordan	265
Kenya	270
Khmer Republic (Cambodia)	275
Korea	280

COUNTRIESCODE

Kuwait	285
Laos	290
Lebanon	295
Libya	300
Luxemburg	305
Malagasy Republic	310
Malawi	315
Malaya	320
Mauritania	325
Mauritius	330
Mexico	335
Monaco	340
Morocco	345
Nepal	350
Netherlands	355
Netherlands Antilles	360
New Zealand	365
Nigeria	370
Northern Ireland	375
Norway	380
Pakistan	385
Panama	390
Peru	395
Phillipines	400
Poland	405
Portugal	410
Puerto Rico	415

COUNTRIESCODE

Qatar	417
Rhodesia	420
Rumania	425
Scotland	430
Senegal	435
Sierra Leone	440
Singapore	445
Solomon Islands	450
South Africa	455
Spain	460
Sri Lanka	85
Sudan	465
Surinam	470
Swaziland	475
Sweden	480
Switzerland	485
Syria	490
Tangiers	495
Tanzania	500
Thailand	505
Trinidad	510
Tunisia	515
Turkey	520
Uganda	525
United States	530
Venezuela	540
Vietnam, South	545

COUNTRIESCODE

West Indies (British)	550
Yugoslavia	555
Zambia	560

CITIESCODE

Accra, Ghana	595
Amsterdam, Netherlands	600
Athens, Greece	605
Beirut, Lebanon	610
Belfast, Northern Ireland	615
Bombay, India	620
Brussels, Belgium	625
Calcutta, India	635
Caracas, Venezuela	638
Colombo City, Sri Lanka (Ceylon)	630
Doha City, Qatar	640
Dublin, Ireland	645
Freetown, Sierra	648
Georgetown, Guyana	650
Glasgow, Scotland	655
Helsinki, Finland	660
Istanbul, Turkey	665
Jerusalem, Isreal	670
Johannesburg, South Africa	675
Khartoum, Sudan	680
Kuwait City, Kuwait	683
Lagus City, Nigeria	685
Madrid, Spain	690
Manilla, Philippines	700
Mexico City, Mexico	705
Montevideo, Uruguay	695
Munich, Germany	710

CITIESCODE

Nairobi City, Kenya	715
New York City, U.S.A.	720
Oslo, Norway	725
Panama City, Panama	730
Paris, France	735
Port of Spain, Trinidad and Tobago	740
Queson City, Philippines	745
Salisbury, Rhodesia	750
Seoul, Korea	760
Stockholm, Sweden	763
Sydney, Australia	765
Tananarive, Madagascar	770
Tokyo, Japan	775
Vienna, Austria	780
Warsaw, Poland	783
Wellington, New Zealand	785
Zurich, Switzerland	790



## TWO

### The Comparative Crime Data File (CCDF): A History and Description

In assembling the Comparative Crime Data File, we sought to create an archive with both comparative breadth and historical depth. Although the CCDF eventually grew to include 110 national and 44 urban entries, with data for roughly 1900–70, we undertook the project with no idea that the archive would reach this size. Data collection occurred over approximately five years. We pursued several methods of obtaining information, particularly early in this period. While some methods proved generally more effective than others, certain approaches were appropriate for certain societies. Given the great variety of nations and cities for which we hoped to assemble data, it was perhaps inevitable that no single technique would prove adequate in all cases.

The principal sources from which the homicide data were collected were (1) correspondence with national and metropolitan government sources in virtually all nations in the world; (2) a painstaking search through annual statistical reports and other official documents of those nations which have (at least at some time) published annual crime data; and (3) secondary examination of the records kept by various national and international agencies.

Although all three methods generated entries for the CCDF, the first strategy was the most productive and also the most interesting. Our initial procedure was to contact the consulates and embassies that most nations maintain in the United States. Personnel in some of these offices were able to refer us to authorities or specific agencies in their home nations. When references of this kind were not obtainable, our next procedure was to send "blind" (i.e., not addressed to named individuals) letters to various ministries in each country. In general, we invented the names of specific government agencies (e.g., "the Department of Justice") without knowing whether they existed. Each letter explained that we were interested in studying changing levels of five specific offenses in several nations during the twentieth century. Our inquiry asked whether the recipient of the letter could provide or direct us to annual data on the offenses of homicide, assault, robbery, theft, and rape or other sex offenses between 1900 and 1970.

Our inquiry stressed that the offenses we listed might be American or Western categories and that their own records might well be organized under different headings. We emphasized that we were interested in seeing their recording categories in their original form. In addition to national data, the letter requested parallel data for a specific major city in the same country. The letter expressed our appreciation for any help the recipient could provide and offered to pay any photocopying costs incurred by our request.

In many cases, our initial inquiry produced a reply directing us to another agency; we then sent our request letter to the suggested source. In some cases, the initial letter failed to produce a response, even after several months. In these instances, we invented the name of a different agency and sent our request letter again. Because of this need for a second (and in many cases a third or fourth) letter, we sent out many successive waves of requests, totaling perhaps five hundred letters.<sup>1</sup> In most nations, the individuals we managed to contact were extremely cooperative and generous with their time, resources, and information. Many of them responded promptly with the information we had requested, and several sent more than we had asked for. Because our first contact rarely had information for both the nation and the large city, additional inquiries were usually necessary to obtain the urban data. In some cases, months passed without word, and then, unexpectedly, the information arrived. Agencies in a few nations placed us on the mailing list for their government's statistical annuals, and some of these publications continued to arrive years after our initial request for information.

Over a period of five years, the responses to our letters arrived from around the world in a seemingly limitless variety of shapes, sizes, languages, alphabets, letterheads, envelopes, and stamps. The data themselves took equally varied forms—booklets, penciled charts, entire volumes of national yearbooks, photocopies of published or unpublished lists, and massive typed or handwritten tables which unfolded like roadmaps. The variety reflected in the data was impressive, and the project quickly taught us how little we knew about political geography. We wrote to and received data from nations whose existence had been unknown to us prior to the project. In other cases, our ignorance about the situations of individual societies was underlined—when, for example, our correspondent referred to a set of data as for the "mainland" only. Without the aid of a political atlas, many of these comments would have been incomprehensible.

In some cases, the requested information arrived paired with a provocative national or political sentiment. For example, the Philippines Department of Justice letterhead read: "An orderly people make an enduring nation." Swaziland's envelope bore the legend: "Umhlaba

Uyimpilo Yetfhu—Wongel!" This was accompanied, fortunately, by the translation: "The soil is our greatest asset—help conserve it!" Many of the agencies responding to our inquiry furnished, in addition to the requested data, clarifications of their reporting procedures, informational pamphlets about their institutions and systems of justice, and various cautions about aspects of the data and their appropriate interpretation. Finally, almost all of our correspondents expressed great interest in our undertaking, and many asked to be informed of our results.

For many societies, there were indications that the information we received was not only unpublished but also untabulated prior to our request. The data we received from these societies were in the form of individually typed or even handwritten tables. Many correspondents were kind enough to construct their replies in English, and we began to appreciate just how difficult this must have been for some of them when we began receiving a flood of replies in a bewildering array of languages. In many of the letters we received, the only thing we could read was our own address at the top of the letter.

For the more frequently encountered languages, we were able to benefit from translations provided by helpful colleagues at the University of California. Less familiar languages posed greater problems; there were cases in which we could not recognize a single character in the entire correspondence. In these instances, we asked for and generally received assistance from appropriate embassy and consulate officials in the United States. Even after translation had been completed, some terminological problems remained. For example, it was often necessary to group the unique categories used by a nation under a more general rubric—for instance, a society might have as many as twelve distinct recording categories for homicides. Obviously, classifications of this kind can be difficult even when the literal meaning of a nation's recording categories has been translated.

Our primary goal, of course, was to assemble an orderly file of quantitative data on crime and violence to facilitate previously impossible comparative research. It is our expectation that much of the research which the CCDF makes possible will use the data in some form of aggregate analysis—that is, in a relatively dispassionate manner which emphasizes the data themselves more than the special characteristics of the historical period in which they were generated. This approach is, of course, an indispensable feature of most empirical research.

It seems vital to remember, however, that these comparative crime data were recorded across the moving history of changing societies. In some cases, this history spanned gradual changes in the political and

social conditions of a nation. In other cases, it encompassed transformations so acute that it seems arguable whether the same nation existed before and after. When these historical events have been extremely dramatic or abrupt, it seems appropriate to consider whether and how the relatively fragile process of producing social indicators like crime data has been perturbed.

The letters and information we received with the requested crime records were an unanticipated but fascinating dividend of the data collection process. These documents provide an intriguing window on the histories of individual nations. Our correspondents sometimes volunteered information and opinions about the ways in which various national crises and changes could have altered the data they sent us. Some of these comments about dramatic events were made in a manner which seemed, to us, curiously understated. For example, our correspondent in Brunei wrote that some gaps in that nation's data were "due to various factors including a rebellion in 1962." Our correspondent in Denmark wrote, almost as an aside, a single sentence about what must have been one of the most desperate periods in his nation's long history: "We wish to add that the Danish police statistics date back to 1921 but are missing as far as the years 1944 and 1945, 'the policeless years,' are concerned." Similarly, our correspondent from West Germany noted that although other types of crime statistics had been maintained for an extended period, the police began making their own record of crimes for the first time under the German Reich in 1936.

Correspondents in other societies also commented on long-term changes which had affected or even transformed their nations. Some of this information was indispensable to understanding the data they sent us. Our Hungarian correspondent, for example, drew our attention to the Treaty of Trianon, which in 1920 stripped his nation of two-thirds of its area and population. Officials in other nations commented on the impact of chronic political conflict on records of crime. For example, the commander of Israel's Criminal Investigation Department commented on the periodic wars and guerrilla actions that his nation had experienced: "Even if a record had been kept of all incidents of murder, manslaughter, assault, robbery, etc., during the relevant [wartime] period, it is still extremely doubtful whether it would be possible to differentiate between incidents of a political or criminal motivation." Similarly, the crime data from Belfast and Northern Ireland arrived with an "x" noted before several years in the table and the following legend: "An 'x' denotes years in which subversive elements were to the fore in the Province."

The comments of a few correspondents also revealed, perhaps un-

wittingly, the potential interplay between political changes and a nation's recorded rates of crime. In one Asian nation, for example, our correspondent commented that his nation had a "stormy history full of ups and downs" and gave the following account of its recent experience:

In our nation, violence is apt to be exercised by groups or with the back-up of some groups. From 1968 to 1970, prosecution intensified control of these villain groups and tried to reform them by organizing the national land construction corps to work on the irrigation and reclamation projects. However, these projects could not last so long because they were financed by the government. New minor gangsters sprang up like mushrooms both in the capital and in the countryside.

The case of another nation, whose civil liberties practices are a matter of current debate, provides a more chilling illustration of the ways in which crime data can sometimes be brazenly conscripted to serve political ends.<sup>2</sup> Our correspondent commented on recent trends in his nation's crime rates, asserting—without any apparent foundation in the data he himself sent us—that these rates had recently returned to the level of the 1950s after a long and steady increase. It is his analysis of this alleged change, however, that is of greatest interest:

This drastic reduction in the crime volume is due to various improvements in police service instituted by our government to improve peace and order which is one of the notable achievements in the new order in our country that have helped evolve a new concept in police work since the imposition of martial law in [date]. As a result, other syndicated crimes like smuggling, counterfeiting, and trafficking in illicit drugs have also been greatly reduced not only in [the major city] but throughout [the nation].

A European correspondent also commented on his nation's recent trends in various offenses and loyally attributed what he saw as improvements to the successes of the current regime. These three correspondents were the only ones, out of all those we contacted, who attempted to extract partisan political meanings from the data they sent us.

It is possible, of course, that many other nations also try to use crime rate fluctuations for domestic political purposes—to use "good" trends to justify the current administration or "bad" trends to provide a mandate for the next. Our work does not indicate how frequently crime data are politicized; we do know that letters from only three of our correspondents openly reflected this tendency. Naturally, to the degree that the data in a given society have direct political consequences, researchers must be concerned about possible pressures and temptations to create fraudulent records. This is true of any potentially political social indicator—housing conditions, infant mortality, life expectancy, median income, and so forth—and is not unique to crime data.

In summary, almost all the agencies and officials who responded to our request did so generously. Indeed, since the only reward for providing information was the altruistic satisfaction one might conceivably feel at assisting the halting progress of knowledge, the spirited cooperation we received was particularly impressive. Even when a correspondent reported that he did not possess the requested data, he generally expressed interest in our project and tried to suggest alternate sources.

In a project of this scale, some exceptions to this general pattern of remarkable cooperation were perhaps inevitable. In the case of a small number of countries, none of our letters was ever returned or answered, even after repeated requests. These nations, unfortunately, included the U.S.S.R. and several other Eastern European societies. Other nations usually thought of as Soviet bloc states—such as Hungary and Poland—readily provided data in response to our request. All our letters to the People's Republic of China were returned unopened, although various postmarks indicated that the letters had in fact reached Peking.

There were also a number of curious responses. In a few nations, for example, our first correspondents asserted that the data we requested had never been collected or had been lost—only to have other agencies in the same society send us these very data in response to a follow-up letter. In one industrialized society, this contradiction occurred between two agencies located in the same city. In these cases, it was only our persistence in sending additional letters to new addresses that secured the data for these nations.

We interpret these cases as additional evidence that, in some nations, historical crime records have not been the sole responsibility of any single agency. This kind of administrative diffusion poses the obvious danger that irreplaceable data can be lost—indeed, we assume that this has already happened for some of the nations missing from the CCDF. The obvious fragility of historical records of this kind seemed to us to lend added urgency to our efforts at collection and preservation.

As we have noted, direct correspondence with multiple agencies in other countries was by far the most successful of the various methods we used in assembling the CCDF. Correspondence produced data series which were longer, more complete, and more annotated than the series obtained in any other way. However, we also obtained information for some nations in two other ways.

One of these involved perusing hundreds of national statistical annuals. In general, these were of limited usefulness. Many nations do not include crime data in their statistical publications—although information on the nation's annual output of "pork bellies" or any other monetized commodity is abundantly available. This selective accounting may say something about the aspects of national life which individual societies

regard as worth recording. Statistical annuals are problematic for technical reasons as well. They tend, quite reasonably, to be printed exclusively in the nation's primary language, and they also present data without explanation or annotation. This makes it difficult to know which offenses are classified under various categories and also whether changes in law or recording practices have occurred. Despite these obstacles, we did obtain data for quite a number of societies from statistical annuals.

The third and final method we used was by far the least satisfactory. The International Criminal Police Organization (Interpol) maintains some crime records on member nations, and we examined all the annual volumes it had published through 1970. The most glaring disadvantage of this source for our purposes is that Interpol began assembling and publishing crime data only in 1953—and this makes the analysis of long-term trends impossible using these data. In addition, Interpol records begin with only 40 nations and do not report separate data for cities. Despite these problems, we included these data in the CCDF when all other methods failed to provide alternative records.

There are several other Interpol practices which further reduce the usefulness of its data, and these should be kept in mind in any analysis relying on Interpol statistics. For one thing, since Interpol depends on annual submissions of data by member nations, its records are frequently discontinuous. Interpol records are also virtually unannotated, leaving the reader completely uninformed about national changes in practice, law, reporting, or definition. Interpol also reports a summary index of the "total number of offenses." This index is apparently modeled on the kind of aggregate index favored by the U.S. Federal Bureau of Investigation's *Uniform Crime Reports*; this index sums across all offenses and is therefore analytically meaningless.

The most serious problem with Interpol data, in our view, is both simple and insurmountable. The data are collected using a standardized form of Interpol's own invention. This form includes six offense categories intended to "cover certain broad categories of ordinary law crimes which are recognized and punished in the criminal laws of almost all countries."<sup>9</sup> These are murder, sex offenses, larceny, fraud, counterfeit currency offenses, and drug offenses. However, the scope of these categories appears to have changed slightly even during the short period in which Interpol has collected data. The murder category excludes "accidental manslaughter" in 1953, but by 1969 it excludes all "manslaughter." In addition, the adjective "illicit" was added to the drug offense description sometime between 1953 and 1969.

The use of a rigid, inflexible set of recording categories seems to us highly problematic because these offense types are rather arbitrary impositions on Interpol's member nations. These allegedly universal cat-

egories seem certain to obscure each nation's actual experience of crime and violence. Interpol deliberately redefines the "native" terms, categories, and classifications used by individual nations. For example, the 1969-70 volume notes:

The General Secretariat simply reproduces the information given on [the standard Interpol] forms from each country. It is not possible to extract data from official statistics compiled by countries on the basis of criteria other than those of the I.C.P.O.-Interpol international form. The information contained in the report is unsophisticated but uniform.

The motive behind Interpol's interest in uniformity is understandable but, we feel, ill conceived. The probable goal of uniform categories was to facilitate direct cross-sectional comparisons—for example, does nation A have more of a specific crime than nation B? The problem with this approach is that direct comparisons of this kind may be justified for the offense of homicide but are almost certainly unwarranted in the case of other offenses (this issue is discussed in detail in the next chapter).

Even if these crude comparisons were Interpol's goal, its own instructions to member nations seem more likely to produce erratic changes than uniform reporting. As just one example, Interpol's many instructions to member nations include the following (emphasis added): "If a case includes several offenses which are *not directly connected with one another*, each offense should be counted separately; if the offenses are *directly related*, only the most serious one should be counted." Since these distinctions are surely a matter of some judgment, they introduce a new source of systematic error—different nations seem certain to implement this and other Interpol directives differently.

A simpler and much superior procedure, in our opinion, would be to record "native" categories as they occur. This is in general the method we have followed in assembling our own Comparative Crime Data File. Instead of insisting on its own set of invariant categories, Interpol could have collated crime records exactly as the reporting nations recorded them, along with any necessary details on each nation's definitions and changes in law and practice. This would have minimized the errors which are inevitably produced when external categories are imposed on a nation's necessarily idiosyncratic experience.

This approach also seems more appropriate scientifically since, as discussed in the next chapter, longitudinal analyses within nations are more valid than the kinds of direct cross-sectional comparisons for which Interpol data have typically been used. For longitudinal analyses, the most important quality of a data set is the consistency with which it has been generated over time—and Interpol's arbitrary modifications of each nation's data seem certain to have reduced this consistency in unknown

ways. Finally, it is also surprising that *none* of the international agencies we contacted either referred us to Interpol's publications or acknowledged their existence. This omission seems especially striking since, presumably, Interpol must interact with these same international agencies to obtain the records it publishes.

Having indicated the ways in which the data were collected, it is also appropriate to note some of the data collection strategies that proved fruitless. For one thing, some international agencies and institutions which might be expected to record and furnish crime data have not done so. For example, the United Nations publishes an impressive quantity of social indicators which enable a researcher to study a wide range of national characteristics, from the number of physicians per 100,000 population to the number of radios. The United Nations does not, however, furnish any detailed, longitudinal data on crime rates, although it once made an effort in this direction. For example, in the late 1940s, the United Nations did report an index of the number of offenses known to the police for a small number of member nations. This index did not refer to specific types of crimes and was only a summary of "major" and "minor" offenses. Various U.N. publications also reported juvenile court conviction data intermittently during the period 1946-56.

In general, however, data on crime and violence have been omitted from the United Nations' published interest in its member nations. This is somewhat surprising in view of the exhaustive detail which U.N. publications present for other indicators of far less apparent significance. There are indirect indications, however, that the U.N. does not publish crime data at least in part because it regards these data as potentially embarrassing to member nations—perhaps because the publication of comparative data would expose those nations with unusually high rates of crime and violence.

This interpretation was given additional credibility by the United Nations' curious and seemingly contradictory responses to our inquiries about whether it maintained crime data. Our first inquiry prompted a response from the assistant director of the "Crime Prevention and Criminal Justice Section," which said simply, "I am afraid that we are not in a position to provide you with the material you require." Since we assumed that this was a reference to the time and effort involved, we offered to go to New York to examine the records ourselves. We also offered to reciprocate by providing the United Nations with the considerable archive of data we had collected on our own. The reply we received informed us that "any information available here is for member states." We wrote again asking whether or not the United Nations in fact had the data we were seeking and, if so, whether these data were considered classified information. In reply, we received the following

single sentence: "The answer to your questions is that information here is not available to the public or to individual researchers." This curious and rather secretive episode lent support to our belief that the U.N. does not publish crime data because they are regarded as politically sensitive. At any rate, the United Nations' secrecy was ultimately unimportant since our own methods of data collection proved successful.

A number of scholarly collections of "world indicators" have also omitted data on crime and violence. The two best-known collections of these indicators are the works of the World Data Analysis Program at Yale University (Russett, Alker, Deutsch, and Lasswell, 1964; Taylor and Hudson, 1972). Neither of these handbooks of social indicators has any information on crime. The omission of crime data in these social indicator volumes is perhaps understandable since both works depend heavily on U.N. publications. In any case, these two handbooks of world social indicators are only cross-sectional, so that longitudinal research designs are impossible with these data. Even if it is unpreventable, the absence of data on crime and violence from social indicator collections remains somewhat ironic, as Jouvenal (1966) observed:

The indisputable pioneer of social indicators is Quetelet. A large part of his work deals with the frequency of crimes of violence which he tried to correlate with various social characteristics. It is strange that [the authors of the social indicator volumes] who pay homage to Quetelet do not give us this measurement which he considered so important.

In summary, international agencies and previous scholarly efforts have failed to provide historical records on the rates of crime for a large sample of societies. The uncharted character of international crime rates made our own program of research seem well worth the effort required.

### PREPARING THE CCDF

The finished array of data in the completed Comparative Crime Data File exhibits an orderliness which the original records obviously did not possess. Since the information arrived in a great variety of forms, it was necessary for us to make a series of procedural decisions before entering each nation's data set in the archive. In all these decisions, our general goal was to understand and, as much as possible, preserve the unique or "native" meaning the data sets carried within the society in which they were generated. We felt that this approach would minimize the risk of arbitrary interference with the data, and we wanted to avoid imposing a specific viewpoint, methodological fashion, or perspective. At the same time, we tried to be zealous about detecting any potential problems with each data set. Our approach, in short, was to try to maximize the external

intelligibility of each nation's data without compromising their original meaning.

Since many data sets arrived in need of an English translation, this was our first priority. In many cases, idiosyncratic terminology was a problem—and this was generally unaffected by translation. We ran across a great many terms which were completely mysterious to us. In these cases, we wrote to our international correspondents asking for clarification. The distinction between problems of translation and those posed by obscure terms can be illustrated with the specific cases of Hungary and India. Language, not terminology, was the problem in the Hungarian case, while terminology, not language, was the problem in the Indian data. The Hungarian information arrived in six large (11" x 16") hand-typed tables accompanied by a four-page letter. Not one word was in English, but, with the aid of a native speaker, we learned the difference between *emberoles* (murder) and *testi serles* (assault).

The Indian data, by contrast, arrived in English. There were a number of terms, however, which were both unknown to us and unexplained. For example, all the offenses were listed in terms of the "volume of crime per one *lakh* of population," and listed next to robbery was the crime of *dacoity*. In search of an explanation, we wrote to the director of the Bureau of Police Research and Development in India, who informed us that a lakh was equal to 100,000 people. He quoted the definition of *dacoity* from the Indian Penal Code:

When five or more persons conjointly commit or attempt to commit a robbery, or where the whole number of persons conjointly committing or attempting to commit a robbery, and persons present and aiding such commission or attempt, amount to five or more, every person so committing, attempting or aiding, is said to commit "dacoity."

Translation and repeated correspondence were, therefore, the tools with which we tried to understand the internal meaning of the data sets. Although preserving the original qualities of the national data sets was our guiding principle, preparing these data for analysis made it necessary to establish and follow a series of conventions to maximize the validity and usefulness of this diverse archive. These conventions were as follows:

### 1. Offense Categories

National data were recorded in their original offense categories. For this purpose, we established three distinct categories of homicide data: murder, manslaughter, and homicide. Each nation's data were tabled under the label with which they arrived. Some nations record data in more than one of these categories, and other nations record only a single combined category such as "murder and manslaughter." A few nations

recorded one series (e.g., "murder") only until a given year, and then a different series (e.g., "homicide") after that time. In all cases, we constructed footnotes to indicate our best understanding of the exact nature of each nation's indicators.

### 2. Multiple Indicators

In some cases, more than a single indicator was identified for the same offense for the same period—for instance, homicide "offenses known" and also homicide "convictions." Parallel indicators of this kind were sometimes received from relatively independent sources, such as a Bureau of Police Statistics and a Department of Justice. The three distinct homicide categories in the CCDF made it possible for us to include more than one homicide series. We recorded these indicators in separate categories even though the same case could conceivably be present in both series—as an offense known and, later, as a conviction. The existence of these potentially redundant homicide series made it possible for us to address a classic methodological question about the reliability and validity of crime indicators, and this analysis is described in the next chapter. In cases where multiple indicators were available for the same offense, we again used footnotes to indicate the precise nature of each indicator.

### 3. Unique Indicators

In some cases, the original category labels used by individual nations were not identical to any of the general CCDF categories. In these cases, we used the closest equivalent and recorded the original offense name in a footnote. For example, Scotland's "culpable homicide" was tabled under homicide; Caracas's undifferentiated "violent crimes" was listed in place of homicide; and India's "dacoity" was included with conventional robberies.

### 4. Aggregate Indicators

In a few cases, space considerations made it necessary to collapse some of the detailed distinctions preserved in the original data. In these cases, we tabled the resulting aggregate under a single CCDF category. In the case of France, for example, we combined *meurtre*, *assassinat*, *parricide*, and *empoisonnement* into the single offense category of murder. Aggregations of this kind were, again, explained in footnotes.

### 5. Raw and Rate Data

We decided to present data on both the raw number of offenses and the offense rate per 100,000 population. For any systematic analysis, of course, data in raw form are useless, and virtually all the research re-

ported in this volume is based on rates. In some societies, however, a change as small as a single offense can produce a misleadingly dramatic change in the offense rate. In New Zealand, for example, the homicide rate doubled from 1946 to 1947—but the raw number of homicides increased from only two to four! We decided to present both raw and rate data in the CCDF to provide the kind of context and perspective that may be invisible with rate data alone.

### 6. National Populations

The data from some nations arrived in both raw and rate form. For many societies, however, we received only raw data. In these cases, it was necessary to obtain annual data on the population of these nations. We did this by consulting a variety of secondary statistical publications, including the *U.N. Demographic Yearbook*, using these population figures to generate the offense rates per 100,000 persons.

Even when a nation's data were received in both raw and rate form, we compared the same secondary population sources to the population figures the nations themselves had used in their own rate calculations. When differences were found, we preferred the secondary data and used them to recalculate the rates the nations had reported. The rationale for this preference was simple: the calculation of rates in non-census years requires the use of population estimates based only on the single most recent census, while retrospective population series are based on interpolations between two known census figures. For this reason, we believe that we have used the best population estimates available. Since the CCDF presents both raw and rate data, however, future users of the file will be able to recalculate a nation's rates using any population data the researcher prefers.

### 7. Measure Changes

For each nation and city in the CCDF, we sought to determine whether the series contained any discontinuities which could not meaningfully be crossed. This was of obvious importance, since certain types of changes would render the before-and-after data incomparable. This could present major problems for longitudinal designs.<sup>4</sup> It should be mentioned that only the data from some nations in the CCDF have problematic change points of this kind. Some of these changes, ironically, were produced when nations improved their statistical practices. The most frequent improvement of this kind involved a nation's decision to report the number of "offenses known" rather than another indicator from later in the "career" of an offense—for instance, arrests, indictments, convictions, incarcerations, or even prison populations.

The number of offenses known has been regarded as the optimal indicator for several decades although, as will be seen in the next chapter,

we have discovered that several other indicators are equally valid indices of offense trends. At different points in this century, however, a number of nations have discarded a less optimal indicator in favor of the number of offenses known. Although these changes were obviously well intended, they have sometimes done a disservice to researchers since some nations have abandoned their old indicator entirely instead of recording both the old and the new together.

The single most common indicator change in the CCDF has been from data on convictions to statistics on the number of offenses known. Finland made this change in 1927, Indonesia in 1929, and Australia as late as 1963. The effect of these measure changes on a nation's data is, not surprisingly, dramatic. Since the number of convictions is generally no more than a fraction of the number of known offenses, this change in indicators produces an enormous paper "increase" in the data—although this increase is of course an artifact. For example, after Australia changed from convictions to offenses known, in 1963, the homicide data almost tripled and the assault data increased more than tenfold. Most of our correspondents in individual nations were quick to draw our attention to these indicator changes when they had occurred.

Other measure changes were more subtle. For example, our Hungarian correspondent informed us that his nation began including cases of intent to commit murder in its murder data after 1962. We felt it was imperative to include cautions about all such measure changes, whether or not they appeared to have any discernible effect on the series. In Hungary, for example, no effect is apparent. In the case of all measure changes, then, we have tried to alert potential users of the CCDF to the danger of crossing—at least unwittingly—these change points. In the CCDF data set, all measure changes are explained in the data for each nation or city.

Finally, some data sets show interruptions because of wars, coups d'état, national emergencies, or bureaucratic lapses. When these interruptions produce a gap in the data for a particular nation, the effect is to produce more than one series of data for a given offense.

### 8. Political Changes

A few of our correspondents drew our attention to political changes which had altered the borders and populations of their nations. These changes reflect history and the vagaries of national fortunes. For example, the CCDF contains data for "Germany" from 1900 to 1930, but only data for West Germany after 1953. Similar political changes are reflected in the offense data of a few other nations: the partition of Hungary in 1920, the carving of Northern Ireland out of a formerly undivided state in the same year, and so on.

The net effect of changes of this kind is to create more than one



independent series within a single entry in the CCDF. For example, the file contains a series for an undivided Ireland from 1900 to 1912 and then, after an interruption, data for only the smaller Republic of Ireland beginning in 1923. A separate listing of data for Northern Ireland begins in 1922. Political changes of this nature are also explained in footnotes in the CCDF.

### 9. Data Quality

In any undertaking of this scale, when the original statistics are generated by hundreds of different agencies around the world, it seems necessary to assume that the quality of the data is variable. Because the issue of data quality in crime indicators is rather complex, a detailed discussion of these issues is presented in the next chapter rather than here. Since these concerns did affect the way in which we assembled the CCDF, however, their impact on our procedures deserves a brief explanation.

Some of these issues reflect differences among various offenses. As discussed in the next chapter, we restricted most of our attention to homicide because there is evidence that homicide data are more valid than data on other offenses. This difference explains our decision to record up to three different indicators of homicide offenses but only a single indicator of each other offense.

Other concerns about data quality refer to variation across different indicators of the same offense. As noted earlier, the indicator of offenses known has been preferred in the past over data on arrests, convictions, sentencings, or prison populations. It should again be emphasized that this classic concern about differences among indicators appears to be much less important for longitudinal analyses. The CCDF does contain a fairly wide range of indicator types. In the "C" section alone, Cameroon records "number of offenses reported," Canada records "convictions," and Colombia records "number arrested." Because of this diversity, the entries in the CCDF label the specific indicators which individual nations and cities have reported. This information is potentially useful for researchers who decide to limit their analyses to only those cases with the offenses known indicator, on the assumption that this restriction will maximize the validity of a given analysis.

The indicator label in the CCDF files also makes it possible for a researcher to test a hypothesis using progressive "waves" of data—that is, first using nations with the "best" indicator, then using nations with other indicators. A data quality control procedure of this kind allows a researcher to learn what effect, if any, data presumed to differ in quality have upon the conclusion one would draw from a given analysis.<sup>5</sup> This procedure is ideally suited to cross-national research with large numbers of cases. In addition, data quality control is easily incorporated into most

research designs and can greatly increase rigor and precision. The usefulness of data quality control for the CCDF is illustrated in chapter 4, on the effects of war. In summary, since indicator quality is a recurring concern in cross-national research, each entry in the CCDF carries an indicator label. Individual researchers can use this label in sampling and research designs of their choice.

### 10. National Qualifications

As noted earlier, a number of our correspondents sent us certain cautions along with their nations' data. Some of these referred to specific gaps in the crime data, unusual time periods, or even a specific year. We recorded these qualifications in footnotes in the CCDF. In addition, we received cautions of a more general nature from a few correspondents. For example, some of them appeared to be concerned, rightfully, about the appropriateness of direct international comparisons of the absolute "amount" of a given crime. Thus, our New Zealand correspondent asked whether national differences in crime definition and reporting made exact international comparisons possible. These national differences are indeed problematic in direct cross-sectional comparisons, but they are controlled for in the longitudinal designs which constitute the largest part of our work.

Similarly, our correspondent in Scotland felt confident that his nation's data for both murder and culpable homicide were solidly comparable across the entire period from 1900 to 1973, but he wondered whether less serious offenses had been affected over this long period as much by variations in recording practices as by variations in the actual incidence of crime. We had also anticipated this concern, and it is reflected in our decision to focus primarily on homicide rather than less grievous offenses. In most cases, then, we had anticipated these general concerns in our choice of research designs.

Even nonhomicide offenses can be useful in a variety of longitudinal designs. For example, one could use the data on rape rates to identify the years in which *reported* rape appears to have increased in each of a sample of nations. This increase can be of considerable interest—perhaps in a study of the ways in which increased societal concern or police cooperation can affect offense reporting, even if one flatly assumes that no actual increase in the "real" rate has occurred. One might study changes in the willingness to report rape as an index of emergent concern about women's rights, as a measure of the effectiveness of reforms in police practice and jurisprudence, or as a reflection of other social changes. In addition, changes in the reported incidence of rape might occur in an interesting temporal order in various societies.

An understanding of these and other methodological issues is ob-



vously of pivotal importance to an informed use of the data in the CCDF. For this reason, any researcher interested in using this archive to answer a question of his or her own is strongly urged to become familiar with the discussion and caveats presented in the following chapter.

### THE CCDF: AN OVERVIEW

Once the data for a nation or city had been prepared for analysis according to the ten conventions listed above, we used them to construct an entry for the CCDF. These entries were computerized and the complete file was printed in the format shown in the last part of this volume. A total of 110 nations and 44 international cities are listed in the archive. An overview of the contents of the CCDF is given in tables just prior to the entries themselves. This information can be used to select individual cases according to the needs of a given researcher. The tables indicate the total number of years for each entry in the CCDF and the approximate time period for each. Entries marked by one, two, or three asterisks contain a minimum of ten, twenty, or thirty years of continuous coverage independent of measure changes and gaps. Abbreviations are used to show which specific offenses are included for each nation and city.

### NOTES

1. Since we asked the recipients to go to considerable lengths on our behalf, we felt that an individually typed letter would be more effective than a form solicitation, despite the large number of requests involved. Our solution to this problem was to use word processing. A general form of our request letter (in English or Spanish) was programmed, and each individual request letter was generated by adding the name of an individual agency, city, and nation. The resulting letters were indistinguishable from a manually typed letter.
2. In this and the preceding example, we have not identified the nation so as to protect our correspondent from possible reprisals. In at least one case, our correspondent wrote that he had tried for over a month to obtain his Director's approval to send the letter to us—without success. Despite this, our correspondent decided on his own to send us the letter and data.
3. *International Crime Statistics* (Saint-Cloud, France: Interpol, 1969–70), p. ix.
4. Even in the case of shifts and discontinuities, approximate longitudinal analyses are in some cases possible. If two series overlap for at least some period, it is possible to test for a relationship between the two series. If a strong relationship exists, the researcher can generate estimated values for the interrupted series. Even when the change is from one homicide indicator (convictions) to another (offenses known), we have found that the two indicators are highly colinear, as discussed in the next chapter. In theory, this would allow a researcher to project estimates for the missing values in either series when one has terminated. This kind of analysis is common in econometrics but until recently has been infrequently used in the other social sciences.
5. Data quality control was pioneered by Raoul Naroll. The concept and procedure are described in his book *Data Quality Control: A New Research Technique* (New York: Free Press, 1962) and in later work (Naroll, Michik, and Naroll, 1980).

## THREE

### Problems and Prospects in Comparative Crime Research

#### INTRODUCTION TO THE ISSUES

Over the past two decades, researchers have raised important questions about possible sources of inaccuracy, incompleteness, and bias in official crime statistics. Although these critics have focused on a wide range of potential problems, the net effect of this close scrutiny has been to impugn the validity and usefulness of these records (Kitsuse and Cicourel, 1963; Wolfgang, 1963; Nettler, 1974; and Skogan, 1977). In the case of American statistics, the most frequent criticism is that these records reflect only a proportion of all crime because they fail to include unreported offenses. This problem has been given various names but is most frequently called *underreporting*.

While most research on underreporting has examined American crime data, the issue is presumably generic to official crime statistics from any society. The specter of missing or inaccurate data is of obvious significance for a data archive such as the CCDF. If massive underreporting is a factor in international crime statistics, the resulting ambiguities could be fatal to certain types of comparisons. Underreporting could hopelessly confound "real" changes in crime with "paper" changes—meaningless differences reflecting only variation or changes in reporting practices. Because this potential artifact clearly threatens some uses of international crime statistics, it is important to understand the precise nature and implications of underreporting.

A second problem concerns definitions and procedures. In the case of international statistics, it is said that cross-national comparisons are not tenable because different societies often use different *indicators* of crime—for example, offenses known, arrests, court cases, convictions, incarcerations, or even prison populations. If two societies maintain different indicators of the same offense, it is obvious that direct comparisons of the volume of the offense are problematic or impossible. While there may be other questions about crime data quality, the problems of underreporting and different indicators have caused the greatest methodological concern.

### IMPLICATIONS FOR COMPARATIVE RESEARCH

In this chapter, an effort is made to assess the implications for comparative research of these two sources of potential invalidity or incomparability in official crime statistics. Our central goal is to identify (1) what kinds of research designs are jeopardized or even invalidated by these data quality problems, and (2) what research designs are valid despite them. By means of this analysis, we hope to provide potential comparative researchers with a highly practical guide to the relative validity of a range of research designs. Our basic theoretical approach is to treat various methodological concerns as hypotheses and subject them to empirical test or, if an empirical test is not possible, to try to identify what design types are most immune to the error and invalidity which would be produced if a critical hypothesis of this kind was, in fact, correct.

In our view, a sophisticated approach requires that one eschew *both* a blanket indictment of the usefulness of crime statistics and a naïve or unthinking faith in their direct interpretability. Both of these approaches, we believe, are equally reckless and unwarranted. In addition, as this chapter makes clear, neither approach is supported by the existing evidence. Our intent is to try to identify the precise implications of several potential problems, and our approach is, wherever possible, empirical. We assume that official crime statistics may well contain sources of error but, in addition, we assume that these types of error pose a unique and variable jeopardy for different research designs.

For these reasons, it is our view that one of the best models for these methodological issues is a typology which examines the unique impact each potential problem poses for different research designs. Recent evidence on the problems of underreporting and nonstandardized indicators suggests that the degree to which they are problematic is a function of the specific comparisons one wishes to make. These two problems will be considered separately. Some recent evidence, particularly on the problem of underreporting, uses surveys of crime victims to generate unofficial estimates of crime rates. These "victimization" crime rates are then compared with official police statistics to estimate the "hidden figure" of unreported crime. Other research, particularly on the problem of different indicators, is now possible for the first time because of the CCDF. On the basis of this analysis, four different comparative research designs are compared in a summary typology which appraises the vulnerability of each design to these two data quality problems.

#### PROBLEM I: UNDERREPORTING

Few criticisms of official crime statistics have received more attention than underreporting, and few have been seen as potentially more im-

portant. The interest in this issue is scarcely surprising. It is difficult to think of any area of research on crime and violence—with the possible exception of ethnographic, descriptive, or case study approaches—in which the numerical *incidence* of crime is not of central concern. Studies of trends in crime, ecological differences, offender and victim populations, and virtually all other topics in the study of crime and violence depend at least in part on knowledge of the frequency and extent of the offense under study. The keen interest with which underreporting research has been greeted is due to the fact that measurement of the incidence of offenses is indispensable to empirical research on crime and violence.

Perhaps because empirical, quantitative research on crime statistics has been pursued with more enthusiasm in the United States than in many other nations, research on underreporting has centered upon American crime data. In addition, the crime records of other societies may be of higher quality than American data and, as a consequence, underreporting may be more of a problem in American crime statistics than in international data. This argument has been made by several scholars (e.g., Mulvihill and Tumin, 1969; Skogan, 1976) and also by a U.S. Presidential Task Force which concluded:

The United States is today, in the era of the high speed computer, trying to keep track of crime and criminals with a system that was less than adequate in the days of the horse and buggy. . . . In some respects the present system is not as good as that used in some European countries 100 years ago. (*Task Force Report: Crime and Its Impact—An Assessment*, President's Commission on Law Enforcement and the Administration of Justice, 1967: 123)

The principal reason for this criticism, at least in the United States, is that for approximately a half century this nation's official crime statistics have been drawn from police records, reflecting the number of complaints to and arrests made by individual police departments. The data from these departments are then forwarded to the Federal Bureau of Investigation which aggregates the data and publishes them as the *Uniform Crime Reports* (UCR). It may be that every system of recording crime statistics has a weakness, but the weakness of this particular system is particularly disquieting. Unless one is willing to assume that citizens are never reluctant or unmotivated to report crimes, it seems clear that these official police statistics are diminished by underreporting. As a consequence, it seems reasonable to be concerned that what appear to be characteristics of crime (e.g., an increase in rape) are in fact characteristics of underreporting (e.g., an increase in citizen willingness to report rape).

Underreporting has drawn attention to the "dark" or "hidden" figure of crime—that is, the unknown volume of offenses which never appear

in official American crime statistics. The sources and dangers of underreporting have been addressed by Wolfgang (1963) and Skogan (1976, 1977, and 1981) among other scholars. The hidden figure of crime is in many ways an empirical question, and the past fifteen years have seen a bewildering number of "victimization" surveys designed to estimate the magnitude and characteristics of the underreporting problem (Biderman, 1967; Biderman and Reiss, 1967; Ennis, 1967; Santarelli, Work, and Velde, 1974; Hindelang, 1976; Skogan, 1977 and 1981). Victimization surveys have even captured the imagination of the federal government; the United States conducts a periodic *National Crime Survey* to provide survey-based estimates of national crime rates.

The basic paradigm of victimization surveys involves the use of classical sample survey methods to produce estimates of the incidence of a variety of offenses. In a typical victimization survey, the respondent is asked whether he or she or anyone else in the household was the victim of a given crime within a specified time period, usually the previous twelve months. Depending on the sampling method used, the marginal incidence of the offense is then used to project a national rate for this particular crime. When these survey-based rates are compared to the rates based on the FBI's police statistics, the survey-based rates generally produce much higher estimates of crime rates.

The size of the hidden figure of crime appears to vary from offense to offense. In one study, for example, it was estimated that police statistics had recorded about one-third of all the burglaries and about half of the robberies, aggravated assaults, and rapes (Hindelang, 1976). Victimization surveys generally find that roughly half the respondents who mention an offense to the interviewer admit that they did not report it to the police, generally out of fear of reprisal, general cynicism, unwillingness to get the offender in trouble, or distrust of the police and judicial system. Underreporting also stems from the failure of the police to detect some crimes, to make arrests in some of the crimes they do detect, or to record some of the crimes that are reported to them—for instance, police "unfounding" procedures dismiss some reported offenses as false. Still other offenses may be lost if, in cases where an individual commits multiple offenses, only the most serious is recorded.

It has been suggested that underreporting may be an important social process in its own right, not merely a source of methodological contamination for crime statistics. Kitsuse and Cicourel (1963) have drawn attention to the role social definition plays in the reporting process, suggesting that crime statistics can be interpreted as indicators of official concern rather than as objective measures of the true volume of illegal acts. One researcher (Wheeler, 1967) has proposed that researchers examine the ways in which offenders, victims, and police interact to pro-

duce official crime statistics. A number of researchers have, in fact, approached the phenomenon of underreporting by direct observation of the differences between police records and the actual behaviors of delinquents, gang members, and young men generally (Murphy et al., 1946; McCord and McCord, 1959; Piliavin and Briar, 1964; Miller, 1967). In general, these observational or "field" studies concur with the general conclusion of victimization surveys that significant numbers of offenses are never recorded in official police statistics.

Most research on underreporting has, however, been undertaken to provide estimates of the "true" incidence of an offense and the magnitude of the underreporting proportion—the ratio of officially recorded offenses to the total number of offenses reported in victimization surveys. Direct comparisons between survey-based crime rates and official crime statistics are sometimes difficult because of differences in coverage, and a number of researchers have argued against direct comparison (e.g., Velde, Work, and Holtzman, 1975). Many surveys limit the age of their sample (e.g., to those over 12) while police statistics nominally include individuals of all ages; many surveys do not include homicide, white collar crimes, shoplifting, and other offenses included in police statistics.

It should be mentioned that victimization surveys are not without problems of their own if one's objective is an unbiased estimate of the "real" volume of crime. For example, just as police statistics may be diminished by underreporting, victimization surveys may also miss those offenses which respondents are reluctant to reveal to an interviewer. Certain offenses, such as rape by an acquaintance or a family member, are perhaps as unlikely to be reported in a survey as they are unlikely to reach police statistics (Skogan, 1976: 139). This problem has led to the suggestion that, at least for certain offenses, there is a "doubly dark" figure of crime—that is, offenses which are not reported to the police or to an interviewer in a victimization survey (Skogan, 1977b: 45).

Victimization surveys are also subject to a number of specific methodological problems. One of these has been called "telescoping"—the tendency of some respondents to report crimes that actually occurred before the time period covered in the survey. There may also be certain social class differences in the tendency to recall or report crime during an interview. This has been called the problem of "differential productivity of respondents" (Skogan, 1981).

In addition, problems intrinsic to survey research may also affect victimization studies. For example, Maltz (1977) reports that some of the National Crime Panel survey data show disquieting variance across interviewers—that is, some interviewers produce high rates of reported criminal victimization while other interviewers produce low rates. It is

difficult to know the precise reasons for this differential response, although it suggests that respondents are encouraged by or willing to confide in some interviewers more than others. Finally, there is some evidence that the structure of the victimization interview may also affect the likelihood that crimes will be recalled or remembered. In one study, respondents were more likely to recall a crime if they were first asked about their attitudes toward crime and the police; they were less likely to recall an offense if these attitude items were omitted (Maltz, 1977).

Despite these problems, it seems clear that the method of victimization research can produce useful information about crime and official statistics. This method also allows us to investigate the phenomenon of admitted underreporting—that is, the circumstances which characterize a victim's decision not to report an offense to the police. At least three variables in the offense itself appear to play a role in the decision to report the offense to the police: (1) in the case of thefts, the higher the value of the stolen items, the more likely the victim is to report the loss; (2) in the case of assaultive crimes, the victim is more likely to report the offense if a weapon is present; and (3) if the victim and offenders are strangers, the offense is more likely to be reported to police (Skogan, 1977). Existing research suggests that many victim characteristics, such as race, do not affect underreporting. Age, however, appears to be an important variable. Victimization surveys indicate that people under the age of twenty are much less likely than older people to report crimes to the police.

The seriousness of the offense is by far the most important determinant of whether or not it is reported. This has led to a tempering of what appeared to be, early in victimization research, somewhat revolutionary expectations. The finding in early victimization surveys that large numbers of offenses were missing from police statistics led some to assume that the "crime problem"—which already seemed grievous when judged from police statistics—was in fact a great deal worse than had been imagined. If only a third or a half of the crimes committed were being reported, according to this reasoning, then the actual rates of crime were in fact many times higher than the (already high) official crime rates.

New evidence suggests that this concern was exaggerated. While it is true that large numbers of crimes never reach police statistics, the importance of these unreported offenses is highly debatable. It is now recognized that most unreported crime consists of minor property offenses (Skogan, 1977b: 49). This finding has changed our understanding of the hidden figure of crime:

The reservoir of unreported crime contains a disproportionate number of less serious incidents involving small financial loss, little serious injury, and (infrequent) use of weapons. (Skogan, 1977b: 41.)

Contrary to considerable speculation about the portentous implications of unreported crime, these data indicate that the vast pool of incidents which do not come to the attention of the police does not conceal a large amount of serious crime. (Skogan, 1977b: 46)

It now appears, therefore, that while official American crime statistics do underenumerate, the degree of underenumeration varies inversely with the seriousness of the offense. With the possible exception of certain offenses such as rape, in which reporting could embarrass or stigmatize the victim, it seems reasonable to assume that the official data provide a reasonably accurate record of serious offenses. This new evidence restores to official crime statistics some important forms of usefulness.

The finding that serious offenses are relatively immune to underreporting has particular significance for the offense of homicide. At this time, there is no evidence that homicide data suffer from underreporting; in fact, there are strong reasons to believe that this particular violent crime is fully enumerated in official crime statistics. For example, a presidential crime commission concluded that, compared to other offenses, homicide was an especially valid indicator because it appeared to be invulnerable to police misclassification (Mulvihill and Tumin, 1969). Historians of crime and violence also have urged attention to this offense precisely because it appears to have been relatively invariant in definition and tabulation. Crime historians have been particularly sensitive to the risk of confusing mere definition or measurement changes with actual crime rate changes and, in general, they have concluded that this risk

is more serious for certain kinds of crimes than for others. For example, it is unlikely that there has been significant change over the last hundred years in the way in which murder has been defined or murderers apprehended. (Ferdinand, 1967: 86)

There is other evidence on this question as well. Researchers on homicide have concluded that the overwhelming majority of homicides are cleared by arrest and that, as a result, police statistics fully enumerate this offense. Other researchers have concluded that the FBI's *Uniform Crime Report* is accurate for the offenses of murder and non-negligent manslaughter (Hindelang, 1974). In most victimization surveys, including the National Crime Panel Survey, the offense of homicide is not even included because it is believed to be fully enumerated (Skogan, 1977b: 45).

In the victimization survey by Ennis (1967), however, respondents were asked to note any homicides of which they were aware. In this study, the data indicated that official crime statistics on this offense were accurate and valid. Thus, evidence on this question appears to be consistent and persuasive: homicide is the most valid of offense indicators in that official statistics on this offense are immune to underreporting.<sup>1</sup>

Since the majority of victimization surveys have been done in the United States, the effect of underreporting on international crime data is less well understood. As indicated earlier, there is a widespread belief that the official crime statistics of the United States are in many ways inferior to those of other industrial nations. At the same time, the few cross-national victimization surveys that have been done suggest that underreporting also occurs in other societies and that the pattern of decreasing underreporting with the increasing severity of an offense is similar to that in the United States (e.g., Sparks, 1976; Sparks, Genn, and Dodd, 1977).

In cross-national records on homicide, it appears to be the case—as in the United States—that underreporting is simply not an issue. Existing evidence suggests that homicides are fully enumerated (Phillipson, 1974; Verkko, 1953 and 1956). These findings indicate, again, that data on homicide are superior to other offense data in terms of their resistance to underreporting. As a result, as will be discussed below, homicide data can be presumed to be valid for a wide range of research designs—only some of which are appropriate for data on less serious offenses.

#### Underreporting and Levels of Crime

These findings that not all offenses are reflected in official crime statistics and, at the same time, that the most serious offenses—particularly homicides—are fully enumerated have nontrivial implications for the validity of research on crime and violence. These implications, clearly, vary depending upon the specific offense examined and the uses to which official crime data are applied.

The research designs most vulnerable to the problem of underreporting are those which address the absolute volume or *level* of an offense. The basic problem with this sort of analysis is that one cannot assume a priori that the “underreporting proportion”—the ratio of reported offenses to total offenses—is invariant. For example, a study using official crime statistics to compare the levels of burglary in two cities (or two nations) will be jeopardized or even invalidated if the underreporting proportions in these two units differ. Similarly, a study comparing the level of burglary in a city (or nation) in 1960 and 1970 will produce spurious results if the underreporting proportion has changed during this decade. This problem is not limited to comparisons: it also affects one-sample studies—for example, a simple estimate of the probability that a home in a given city or nation will be burglarized in a given year will be inaccurate if the underreporting proportion for this offense is low.

The generic problem in such research designs is that the “real” crime rate and the underreporting proportion are hopelessly confounded. If

official statistics show that city A has a higher rate of burglary than city B, we can conclude *either* that city A really has a higher burglary rate or that city A does not have a higher burglary rate but merely a more complete enumeration of this offense. This uncertainty is irreducible for certain uses of official crime statistics. In some cases, of course, one could do victimization surveys to answer these questions—but, quite apart from the different set of problems which characterize victimization surveys, the expense and time required would severely restrict the number and range of possible investigations.

There are a number of possible solutions to this problem of the confounding of offense levels and underreporting proportions. Each of these solutions carries a different form of threat to the validity of the inferences a researcher might hope to draw. The most conservative solution is to renounce all comparisons of the levels of various offenses. One might argue that the confounding effects of underreporting proportions make the “real” level or volume of an offense fatally ambiguous and, therefore, useless to the investigator.

While we are not unsympathetic with this conclusion, our own inclinations are to take a somewhat less conservative approach. This purist argument is highly persuasive, in our view, for a particular combination of indicators and designs which we believe may well be generally invalidated by underreporting: *analyses of the absolute level of nonhomicide offenses*. Instances of this research design abound in both American and cross-national research—for example, contrasting the robbery rate in California and New York, estimating the frequency of assault in Chicago, calculating the “clearance rate” (arrests/number of offenses) for burglary in the United States, and comparing rape rates in the United States and Britain. Such studies require us to believe that official statistics provide valid estimate of the real level of these offenses—and the evidence from underreporting research makes this belief generally untenable.

In some cases, these questions may have empirical answers. For example, before making the comparisons of crime levels mentioned above, one could examine victimization survey data from the affected jurisdictions to determine whether the underreporting proportions differ in the jurisdictions to be compared. If the underreporting proportions do not differ, the evidence from the victimization research could be reported as empirical justification for using official crime statistics in a direct comparison of crime levels.

On the other hand, if the underreporting proportions *do* differ in the jurisdictions one wishes to compare, a weighting procedure might be appropriate. This procedure can be illustrated using one of the hypothetical comparisons listed above—a design which contrasts the robbery rate in California and New York. Suppose that official statistics show the

robbery rates of these two states to be, respectively, 100 and 65 offenses per 100,000 population. Suppose further that victimization surveys have shown the respective underreporting proportions to be .80 to .50—i.e., that 80% of all robberies are reported to the police in California but only 50% of all robberies are reported to the police in New York.

One can use this information to produce new estimates of the robbery rates in the two jurisdictions. In this example, the "corrected" or "adjusted" robbery rates (i.e., the official rate divided by the underreporting proportion) are 125 and 130. Contrary to the impression produced by official statistics, therefore, New York would have a *higher* robbery rate than California when the official statistics are corrected for underreporting.

In summary, research on the *levels* of one or more crimes requires that certain assumptions be made about the underreporting proportions which characterize the offenses under study. Two reporting units (cities, states, or nations) can be compared on the level of an offense only if one or more of the following conditions are met: (1) underreporting is not a problem for this offense; (2) underreporting proportions for this crime are known and are essentially equal for both reporting units; or (3) underreporting proportions for this crime are unequal but can be used as weights to correct the official rates for this offense.

These conditions are clearly stringent. At present, there is only a single offense for which underreporting can be assumed to be no problem: homicide.<sup>2</sup> For this reason, comparisons of the *level* of homicide in different jurisdictions can reasonably be assumed to be immune to underreporting and therefore valid. There are, of course, other considerations which affect cross-national comparisons—for instance, statutory differences between nations in the way this offense is defined, an example being the distinction between homicide and manslaughter.

In general, however, homicide is an offense which appears valid for virtually all comparative research designs because all existing evidence suggests that this offense is fully enumerated. Research designs on the levels of nonhomicide offenses must therefore meet at least one of the remaining two conditions, both of which require estimates of the underreporting proportions for the jurisdictional units in the research design. For cross-national comparisons of the *levels* of an offense, these two conditions cannot be met at present since little or no empirical research has been done on underreporting proportions in other societies. Although the prevailing belief among many social scientists is that underreporting is not a problem in many other nations, this assumption has not been widely documented with victimization research in a large sample of nations.

With the exception of the fully reported offense of homicide, there-

fore, simple cross-national comparisons of the levels of a crime are at present imprudent or even unwarranted. In the absence of empirical estimates of underreporting proportions in the societies under study, such comparisons would require the seemingly indefensible assumption that the two unknown proportions are equal.

The prospects for the comparisons of crime levels *within* the United States may be somewhat brighter, if only because more victimization surveys have been done. If one examines a number of victimization surveys, it appears that—even though underreporting certainly exists—the underreporting proportions are fairly stable across different jurisdictions. A number of such surveys are listed in table 3.1, and the underreporting proportions uncovered in these surveys are indicated. For the offense of robbery, for example, table 3.1 indicates that the mean

Table 3.1. Comparison of Independent Estimates of Underreporting Proportions for Five Offenses

Offense	Five-city study <sup>a</sup>		Eight-city study <sup>b</sup>		Thirteen-city study <sup>c</sup>		UCR/Ennis <sup>d</sup>
	Mean proportion	Range	Mean proportion	Range	Mean proportion	Range	
Robbery	.51	(.47-.60)	.52	(.44-.57)	.53	(.44-.65)	.65
Simple Assault	.33	(.28-.37)	.33	(.27-.39)	.33	(.29-.45)	— <sup>e</sup>
Aggravated Assault	.53	(.51-.57)	.51	(.46-.60)	.48	(.41-.55)	.49
Aggravated Assault with Injury	.66	(.57-.73)	.58	(.52-.63)	.58	(.46-.77)	— <sup>e</sup>
Rape	.54	(.46-.61)	.50	(.35-.58)	.49	(.34-.65)	.27
Burglary	.54	(.52-.57)	.54	(.50-.57)	.53	(.46-.58)	.31
Household Larceny	.24	(.22-.26)	.27	(.20-.32)	.25	(.19-.32)	.44

NOTES: The underreporting proportions in the five-, eight- and thirteen-city studies are the ratio of two self-report figures: (1) the number of crimes respondents said they reported to police and (2) the total number of crimes respondents said they experienced. The UCR/Ennis proportion, however, compares self-report data to the actual police statistics compiled in the FBI's *Uniform Crime Report*.

<sup>a</sup> Means calculated from Table 8 of Santarelli et al. (1974b).

<sup>b</sup> Means calculated from Table 8 of Santarelli et al. (1974a).

<sup>c</sup> Means calculated from the thirteen different Table 6's of Velde et al. (1975).

<sup>d</sup> Underreporting proportion calculated from Table 1 of Ennis (1967), using the UCR rates for individuals and residencies.

<sup>e</sup> The Ennis survey did not report rates for these assault categories.

underreporting proportion was .51 in a five-city survey, .52 in an eight-city survey, and .53 in a thirteen-city survey. Table 3.1 also presents the range of underreporting proportions—for example, in the five-city survey, the robbery underreporting proportion varied from .47 to .60.<sup>3</sup>

It is apparent from table 3.1 that underreporting varies from offense to offense—for instance, household larcenies appear to be reported relatively infrequently while robbery and aggravated assault with injury appear to be enumerated much more fully. Despite the variation across offenses, the range of the underreporting proportions within each offense category appears to be narrow, at least for some offenses. Since sampling errors occur in any survey, it may not be unreasonable to assume that ranges of this magnitude could be accounted for merely by the standard error of the mean. At any rate, it does not appear to be the case—at least for some offenses—that the underreporting proportions vary widely from city to city. This stability is perhaps particularly impressive since the twenty-six cities included in this comparison vary greatly in size and other characteristics.

For these twenty-six cities, then, valid comparisons of the absolute level of certain crimes, particularly the more serious offenses of aggravated assault and robbery, may be possible. The jurisdictional stability of underreporting in this analysis supports the conclusion that underreporting may be due to the nature of the offense rather than to a wide range of variables which vary unpredictably from city to city. This information could be used in diverse ways. If underreporting proportions appear highly stable across jurisdictions—as they do for the offense of robbery—then one would feel encouraged to compare directly official statistics on the robbery rates in these cities.

For other offenses, one could even compute how different the underreporting proportions would have to be to produce spurious offense rate differences of an observed size. For example, suppose city A reports an aggravated assault rate of 200 (per 100,000 persons), while city B reports an aggravated assault rate of 400. For this difference to be spurious (i.e., due to inter-city variation in underreporting), the underreporting proportions in the two cities would have to differ by a factor of at least 2:1. For example, the underreporting proportions would have to be something like .40 in city A and .80 in city B. An examination of table 3.1 shows that, across all twenty-six cities in the comparison, the offense of aggravated assault varied only from .41 to .60. This suggests that a 2:1 ratio between the underreporting proportions of city A and city B is extremely unlikely and that the observed higher assault rate in city B is real rather than spurious.

This analysis can be stated in more general form. The relative stability of the underreporting proportion can be used to determine how likely

it is that an observed difference in the official offense rates of two jurisdictions is only an artifact of differential reporting. The general form of this informal "spuriousness test" might be stated as follows:

If two jurisdictions have rates of the same offense which have a ratio of  $x/y$ , this difference in offense rates is (other things being equal) genuine unless one is willing to assume that the underreporting proportions in the two jurisdictions have a ratio which is as large as or larger than  $y/x$ .

This spuriousness test has the obvious implication that large differences are affected less by underreporting artifacts than are small differences. For example, if two cities have burglary rates of 3000 and 1000 (per 100,000 population), these rates make a ratio of 3:1. The underreporting proportion in the city with the apparently higher burglary rate would have to be *three* times higher (e.g., .31 vs .93) for this crime rate difference to be an artifact due to underreporting. As the data for burglary in table 3.1 indicate, differences of this magnitude in underreporting proportions seem extremely unlikely.

A smaller difference in observed offense rates would, of course, be more vulnerable to underreporting. If two jurisdictions had reported rape rates of 20 and 30 (per 100,000 population), for example, this difference could be an artifact if the underreporting proportions in the two jurisdictions had a ratio of 3:2—e.g., .60 and .40. As can be seen from table 3.1, this difference is—unlike previous examples—within the apparent range of underreporting for the offense of rape. As a result, it might be judicious in this instance to conclude that the comparison is indeterminate. The difference between the two jurisdictions could be genuine, but it could as easily be an artifact of differential underreporting in the two jurisdictions.

In summary, studies of the *level* of an offense are vulnerable to underreporting. In particular, the following conclusions appear warranted: (1) in the case of homicide, underreporting appears to be negligible or nonexistent, and studies of the levels of this offense are therefore not problematic; (2) for other offenses, it appears that the most serious offenses are well enumerated—that is, most are reported; (3) less serious offenses are more often underreported; (4) in cases where the magnitude of the underreporting of an offense can be estimated, this estimate can be used to produce weighted or corrected estimates of the offense rate; (5) even for the less serious offenses, underreporting proportions appear to be fairly stable, at least in the United States; and (6) in comparisons of crime rates in two jurisdictions, one can determine whether observed differences in crime rates are likely to be genuine or an artifact of differential underreporting in the two jurisdictions. Some of these conclusions, while warranted in terms of available evidence from U.S. crime



statistics, have not been examined using cross-national data. For this reason, some caution seems appropriate before assuming that these conclusions also apply to the data of other nations.

From the above analysis, it appears that underreporting proportions are reasonably stable across different jurisdictions. This stability provides indirect encouragement for comparisons of offense levels in the same jurisdiction at two or more points in time. Longitudinal research designs examine changes in an offense rate over time. This type of design seems warranted on theoretical grounds, since most of the factors which produce underreporting seem likely to be relatively enduring in nature.<sup>4</sup> This assumption about the temporal stability of underreporting proportions is addressed in the next section of this chapter.

### Underreporting and Trends of Crime

In the previous section, we examined the implications of underreporting for studies of the level of an offense. For reasons which will be indicated in this section, studies of *trends* of an offense are almost certainly less vulnerable to underreporting. This is because many of the idiosyncratic ways in which a nation generates offense rates—definition, record keeping, social or cultural attitudes about the seriousness of different offenses, etc.—are relatively durable over time. Studies of offense trends therefore hold these idiosyncrasies—and the underreporting proportions they produce—constant.

A comparison of how German and American assault rates changed after World War I would, for example, be unaffected by national differences in underreporting proportions as long as the proportions remained consistent. This comparison would not require that the underreporting proportions be the same in both countries, only that they be stable within each country. As an extreme example, one could make this comparison even if 80% of all assaults were reported to the police in Germany (i.e., an underreporting proportion of .80) but only 20% of the American assaults were reported. This very large difference would have no effect on the study of assault rate changes after World War I—as long as these proportions were stable within both societies. Similarly, a comparison of the relationship between unemployment fluctuations and trends in robbery in the United States and England would be unaffected by different underreporting proportions in these nations, as long as the proportions remained internally consistent.

While studies of offense levels are strongly affected by underreporting, studies of offense trends are relatively unaffected because trend research does not require the assumption that underreporting proportions are the same in the jurisdictions to be compared. The importance of this distinction between studies of offense levels and studies of offense trends

has been recognized for some time, although its implications for the problem of underreporting have not been generally recognized. More than forty years ago, however, the difference between offense levels and offense trends was noted by the celebrated historian of crime Leon Radzinowicz:

The impossibility of determining numerically the static aspect of criminality need not be any bar to a determination of its dynamic aspect—i.e., the changes taking place in the course of time. (1939: 275)

Trend designs are relatively invulnerable to underreporting because, unlike studies of offense levels, they require only a single condition or assumption: that reported offenses are related to the real number of offenses by some constant, which can be known or unknown. For example, if roughly 50% of a nation's robberies are consistently reported to the police, then official statistics on this offense are a perfectly valid index of *trends* in robbery—even though the official statistics greatly underestimate the actual incidence of this offense.<sup>5</sup>

In trend designs, it is unnecessary to use weighting to correct for underreporting proportions or even to know what these proportions are—as long as a researcher is willing to assume that these proportions are stable over time. Trend designs are even valid if some types of variation occur in the underreporting proportion over time. If this proportion fluctuates randomly around some mean value, then trend designs would suffer only from random (or "benign") error and not biased (or "malignant") error (Naroll, 1962). Random error has been called "benign" because it generally affects a research design only in a conservative direction—that is, it can decrease a researcher's chance of discovering a relationship in a data set but is unlikely to produce a spurious finding. In the case of crime trend research, this is because random errors inflate the nonmeaningful variance or "noise" in the offense rate. The effect of this "noise" upon research is called "attenuation," a reduction in the researcher's chance of identifying an important change or a relationship between two variables.

Spurious findings, by contrast, occur in cases of biased or malignant error—e.g., cases in which the error in the underreporting proportion over time is correlated, directly or indirectly, with an independent variable in a research design. For example, an urban police department's announced "crackdown" on street robberies could have the paradoxical effect of increasing the *official* robbery rate if the announcement somehow increases the likelihood that victims will report robberies, thus changing the underreporting proportion. The research designs in greatest jeopardy of spurious findings of this kind are, of course, studies designed to evaluate some intervention or policy change.



In general, trend designs require only the minimal assumption that underreporting proportions are stable over time, or at least stable with only random fluctuations. In theory, at least, this is an eminently testable assumption. In the case of American offense data, for example, one could test the temporal stability of underreporting proportions by means of consecutive replications of victimization surveys in the same jurisdictions—for example, by replicating in serial fashion the surveys of Santarelli et al. (1974a and 1974b) and Velde et al. (1975). Longitudinal victimization surveys of this kind could indicate whether underreporting proportions are stable over time. At the present time, however, definitive evidence on this question does not exist. For example, Skogan (1977b: 50) concluded that there were no data upon which to estimate the temporal relationship between reported and unreported offenses. At the same time, the existence of a National Crime Panel presupposes serial victimization surveys and, from a purely methodological perspective, this would be one of the most important contributions victimization research could make.

Lacking victimization surveys which are comparable over long periods of time, a number of less satisfactory approximations are available. For example, one of the reports of the National Crime Panel survey (Velde, McQuade, Wormeli, Bratt, and Renshaw, 1976) compared underreporting proportions in the 1973 and 1974 surveys; another (Velde, Wormeli, Bratt, and Renshaw, 1977) compared underreporting in the 1974 and 1975 victimization surveys. These comparisons do not provide ideal tests because they compare data from consecutive years rather than from longer intervals. But we have examined these data to see whether there are any changes in reporting over time and, if there are, whether these changes are any greater than the random fluctuations one would expect from sampling error alone in any survey.

In the study comparing the 1973 and 1974 data, the median percent change in reporting (offenses victims say they reported to police/all offenses victims say occurred) was +2.8% over 33 different offense categories. This figure was roughly half the median standard error (5.7%) for these 33 offense categories—that is, this 2.8% change in reporting appears to be due to sampling differences alone. In the comparison of the 1974 and 1975 data, the median change in reporting was +1.9% over 33 offense categories. This figure was, again, much smaller than the median standard error in these surveys (5.3%). In these two studies, therefore, the reporting proportions appear to be impressively stable, with no evidence of overall changes in underreporting.<sup>6</sup>

There is also some indirect evidence which offers grounds for considerable optimism on this point. The underreporting proportions for the twenty-six cities in table 3.1, for example, show generally impressive

consistency within a given crime category. This cross-sectional evidence suggests that the magnitude of underreporting may be firmly linked to the nature of specific offenses rather than to the (presumably changing) conditions of individual cities.

In summary, the study of offense trends is notably less problematic than research on offense levels. Although much research remains to be done, existing evidence suggests that underreporting proportions are reasonably stable over time, at least for the more serious offenses. This relative stability may be because underreporting is a function primarily of the seriousness of an offense rather than of other, more transitory factors. As discussed earlier, data on homicide are immune to the effects of underreporting and therefore are a valid basis for comparative analyses of both levels of and trends in this offense.

## PROBLEM 2: DIFFERENT INDICATORS

A persistent concern of researchers on crime and violence has been the relative merits of different indicators of the "real" level or rate of various offenses. There is a variety of potential indicators inherent in any criminal justice system: (1) the number of criminal acts known to the police ("offenses known"); (2) the number of suspected offenders detained ("arrests"); (3) the number of persons brought to trial ("indictments"); (4) the number of individuals found guilty of an offense ("convictions"); (5) the number of people sent to prison or some other institution ("incarcerations"); and even (6) the total number of persons incarcerated at any one time ("prison population").

Each of these indicators can provide a reflection of how much of a given offense is occurring in a society although, obviously, not all these reflections would be of equal size or accuracy. Various indicators occur at different distances from the offense itself, so that one would expect to observe roughly declining sums at successive points in the justice system: fewer arrests than offenses, fewer indictments than arrests, fewer convictions than indictments, and so on. This pattern of diminishing numbers has been called "criminal case mortality" (Van Vechten, 1942). One of the most famous dicta in the study of crime concerns this issue: "The value of a crime for index purposes decreases as the distance from the crime itself in terms of procedure increases" (Sellin, 1931: 346).

It is for this reason that the number of offenses known is considered to be the most accurate official measure of actual criminal behavior. It can be argued, of course, that victimization surveys are even closer to the offense than any of these official indicators (Maltz, 1977: 35). As a result, some researchers have concluded that, except for the fully enumerated offense of homicide, no official statistic will correspond directly to the number of criminal acts in society (Nettler, 1974: 44). At the same

time, it is certainly the case that not all official statistics are equally accurate, and the number of known offenses is considered highly preferable to other indicators (Clinard and Abbott, 1973: 22).

Although less desirable as measures of the volume of crime, other indicators still have potential uses. For example, one could use the indicator of the prison population to see whether conviction and sentencing patterns are affected by crowding or vacancies in a society's prisons. It might be that judicial discretion in sentencing (e.g., suspended vs. served sentences) is highly sensitive to the prison space available. If so, one would expect to find that the severity of judicial sentencing would correlate with, but lag behind, fluctuations in indicators of the prison population.

For research designs in which the incidence of an offense is theoretically important, however, many researchers have assumed that valid analyses are impossible with measures other than the number of offenses known. This is a methodologically rather "purist" position and, although it appears to be widely held, an analysis presented later in this chapter suggests that this position greatly understates the validity and usefulness of other types of indicators. Whether or not the offenses-known indicator is available, however, it seems clear that many types of direct comparisons are not meaningful unless one has at least the *same* indicator in the jurisdictions one wishes to compare.

#### Different Indicators and Levels of Crimes

The problems posed by different indicators are particularly important for comparisons of the absolute level of an offense in a number of jurisdictions. These difficulties become acute in cross-national research because it cannot be assumed that the magnitudes of "case mortality" (e.g., attrition from the number of known offenses to the number of convictions) are similar across nations. Direct comparison using different indicators is vulnerable to potentially gross differences in the efficiency of national systems of criminal justice. It might be the case that 80% of all robberies are cleared by arrest in one society but only 25% in another nation. In this instance, national differences in the incidence of the offense would be hopelessly confounded with national differences in the clearance rate for this offense.

The hazards posed by this indicator problem are easy to illustrate concretely. Suppose, for example, that one wished to make a comparison of the absolute level of the homicide rates in the United States and Canada in 1970. This comparison would be impossible if only convictions data were available for the United States and only offenses known data for Canada. In some cases, the availability of additional data can solve

this problem. If *both* indicators can be obtained for a sample jurisdiction or sample time period in one of these societies, one can determine the ratio of the two indicators and, on this basis, generate an estimate of the value of the missing indicator.

For example, if sample data show that homicide convictions in the United States are consistently .8 (4/5) times the number of known offenses, the convictions data could be weighted by 1.25 (5/4) to estimate the number of known offenses. This estimated number of known offenses in the United States in 1970 can then be compared with the reported number of known offenses in Canada in 1970. In the absence of a sample of simultaneous indicators for at least one society, however, a simple comparison across two different indicators would produce a spurious elevation in the estimate of the homicide rate in Canada.

The problem of different indicators has implications for research with the CCDF since our data file includes a variety of indicators. For most nations in the file, the preferred indicator of known offenses is available. For a few nations in the file, only a less desirable indicator, such as convictions, is available. For some of the nations in the CCDF, two indicators are available for the same years. Nations with multiple indicators can be used to assess the value of even poor indicators for certain research designs, and this analysis is presented below.

#### Different Indicators and Trends of Crime

As discussed above, different types of indicators are of variable quality for estimating the "true" incidence of an offense.<sup>7</sup> However, even imperfect indicators of the volume of an offense can still be useful indices of the *trends* in an offense if they can be assumed or shown to be related to the number of known offenses over time by some constant function. If this can be demonstrated, then fluctuations in these imperfect indicators (arrests, convictions, etc.) can still be used as indices of fluctuations in the incidence of known offenses. If indicators of different quality were found to bear a linear relationship to the number of known offenses, in short, it would be possible to use imperfect indicators to estimate trends in a good indicator which is not available.

Thus the critical empirical question concerns the relationship between good and imperfect indicators. Using data from the CCDF, we were able to assess the degree to which several different indicators manifest the same trends over time. The results of this analysis, using sixteen cases in the CCDF for which two indicators are available, are presented in table 3.2.

The evidence in table 3.2 strongly suggests that even poor indicators can serve as valid indicators of offense trends. This finding has nontrivial

**Table 3.2. Correlations between Good and Imperfect Indicators of the Same Offense**

Nation	Years	Indicators	Correlation between two indicators for each offense	
Australia	1964-1972	(1) Crimes known	Murder and manslaughter	1.00 <sup>a</sup>
		(2) Crimes cleared	Assault	1.00 <sup>a</sup>
			Rape	.98
			Robbery	.96
Canada	1919-1943	(1) Offenses known	Offenses against the person; murder, manslaughter and assault	.90
		(2) Convictions	Offenses against property with violence; robbery and burglary	.93
			Offenses against property without violence; theft	.94
Canada	1952-1967	(1) Charges	Homicide	.82
		(2) Convictions	Assault	.96
			Robbery	1.00 <sup>a</sup>
			Theft	.93
			Offenses against women	.98
Denmark	1933-1947	(1) Crimes known	Homicide	.95
		(2) Crimes cleared	Assault	.96
			Rape	.90
			Robbery	.91
			Theft	.97
Denmark	1948-1959	(1) Crimes known	Homicide	.92
		(2) Crimes cleared	Assault	.92
			Robbery	.91
			Theft	.87
			Rape	.81
Finland	1913-1924	(1) Offenses reported	Homicide	.84
		(2) Prosecutions		
		(1) Offenses reported	Homicide	.74
		(2) Convictions		

**Table 3.2. (continued)**

Nation	Years	Indicators	Correlation between two indicators for each offense	
Ireland	1961-1964	(1) Crimes known	Murder	.85
		(2) Court cases	Manslaughter	1.00 <sup>a</sup>
			Assault	1.00 <sup>a</sup>
			Indecent assaults against females	.99
			Robbery	1.00 <sup>a</sup>
Kenya	1964-1968	(1) Crimes known	Burglary and housebreaking	.99
		(2) Arrests	Offenses against property without violence	.41
Mexico	1966-1972	(1) Crimes known	Homicide	.79
		(2) Convictions	Robbery	.11
			Theft	.93
Netherlands	1949-1972	(1) Crimes known	Homicide	.94
		(2) Convictions	Robbery	.59
			Theft	.85
New Zealand	1920-1954	(1) Cases presented	Homicide	.73
		(2) Convictions	Assault	.81
			Rape	.65
			Robbery	.89
Norway	1957-1970	(1) Final sentences	Murder and manslaughter; crimes against life	.77
		(2) Crimes known	Assault	.79
			Robbery, theft and housebreaking	.41
Norway	1957-1970	(1) Offenses known	All offenses	.96
		(2) Offenses cleared	All offenses	.98
Norway	1957-1970	(1) Offenses known	Homicide	.42
		(2) Persons proceeded against	Assault	.92
			Robbery-theft	.97
			Rape	.42

Table 3.2. (continued)

Nation	Years	Indicators	Correlation between two indicators for each offense	
Sweden	1959-1966	(1) Offenses reported	Homicide	.89
			Robbery	.86
		(2) Offenses cleared	Burglary	.48
			Rape	.82
Tanzania	1962-1972	(1) Crimes known	Murder and manslaughter	-.60
		(2) Convictions	Assault	-.52
			Robbery	.74
			Theft	-.39
Thailand	1945-1962	(1) Crimes known	All offenses	1.00*
U.S.	1933-1971	(2) Convictions		
		(1) UCR murder and non-negligent manslaughter	Homicide	.85
		(2) Rates of death caused by homicides—vital statistics of U.S. <sup>b</sup>		
		Median correlation across all cases:		
		Homicide, murder and manslaughter		.85
		Rape		.86
		Assault		.92
		Robbery, property offenses with violence		.90
		Theft		.90
		All offenses (3 cases)		.98

\* Correlation is .995 or greater

<sup>b</sup> Vital statistics section of U.S. Statistical Abstract

implications for cross-national research because of the inevitable heterogeneity in the criminal justice systems of different nations. The analyses summarized in table 3.2 substantially expand the number of comparative research designs which appear warranted. The general conclusion which emerges is that offense indicators from various stages of criminal justice processing "covary" over time—that is, they appear to trace similar trends over time. This finding suggests that while the notion of "criminal case mortality" is well founded, this "mortality" appears to

be crudely linear—e.g., the proportionate reduction from crimes reported to police to convictions in a given society is approximately constant.

The correlational analysis in table 3.2 indicates that good and imperfect indicators of an offense provide a consistent picture of the trends in the incidence of this offense. For the offense of homicide, for example, the median correlation between good and poor indicators for the sixteen cases is .85. This suggests the existence of an extremely strong relationship, over time, between a good indicator—known offenses—and less optimal indicators such as offenses cleared, prosecutions, convictions, etc. Although there is some variation across various societies, in general, the pattern of the relationships is both clear and strong. If one is interested in the *trend* of an offense, it appears that many different indicators—not merely the "best" indicator of known offenses—provide a valid index.

The pattern appears to be equally encouraging for offenses other than homicide, to judge by the median correlations between good and poor indicators in table 3.2: rape (.86), assault (.92), robbery (.90), theft (.87), and "all offenses" (.98). By the standards of measurement reliability in the social sciences, these values are extremely high. For example, intelligence tests and other measures in wide use in psychology tend to have reliabilities in the range of .60-.75. Attitude scales commonly have reliabilities which are lower still. Purely from the perspective of social science measurement, therefore, the fact that imperfect indicators are correlated with the best measure in the range of .85-.95 is impressive.<sup>8</sup>

This analysis suggests that, at least for some types of research designs, the widespread belief that indicators other than offenses known are unusable is unwarranted. Criminal case mortality has clear implications for studies of offense *levels*—some indicators are suitable for research on offense levels while others are not. But for *trend* designs, in which one seeks to characterize the direction and degree of change in an offense, it seems clear that a variety of indicators can provide a valid measure. In addition, trend estimates from imperfect indicators appear to be equally valid for homicide and less serious offenses.

On the basis of this analysis, we have concluded that data which cannot support certain research designs are entirely valid for other designs. If one has available only data on homicide convictions for Sri Lanka, for example, it is not possible to compare the absolute number of homicide offenses in Sri Lanka and another nation with only records on the number of known offenses. But the convictions indicator is valid if one wishes, for example, to (1) compare long-term trends in homicide in Sri Lanka and some other nation, (2) determine the effect of employment changes on homicide rates in Sri Lanka, or (3) test for the possible changes in the homicide rate if Sri Lanka abolishes capital punishment.

In our view, this analysis argues for a *design-specific methodology* which reconceives the potential validity of data on crime and violence in terms of the research design to which these data are to be applied. This *design specificity* is important since many of the research designs in greatest demand in the social sciences are, explicitly or not, trend designs in which an investigator is concerned with whether an offense rate goes up, remains stable, or goes down. In a great many cases, we believe it is possible to answer these questions about trends and changes with a high level of confidence even if the indicator available does not allow us to know the precise level of this offense at any one time.<sup>9</sup>

In summary, the problems of underreporting and different indicators clearly have *selective* effects rather than general effects. These problems jeopardize certain research designs while affecting other designs not at all. The analysis presented thus far can be summarized in typological form in table 3.3. This typology presents four generic comparative designs created from the combinations of two dichotomies: (1) offense levels vs. offense trends, and (2) homicide vs. other offenses. This typology subsumes a substantial proportion of all comparative research on crime and violence.

Because these four generic research designs are *differentially* susceptible to the problems of underreporting and different indicators, the typology provides a useful summary of the methodological issues addressed in this chapter. Because it provides a succinct summary of the relative vulnerability of these different designs, this typology is analogous to the comparisons which Campbell and Stanley (1966) published for nonexperimental research in general. Since table 3.3 addresses some of the unique problems inherent in comparative designs using crime data, it is intended to be both more concrete and directly applicable to this particular research domain.

It is clear from the table that not all designs are equally immune to methodological problems. Type IV designs—which involve comparisons of changes or trends in homicide rates—are unaffected by either underreporting or the problem of different indicators. For these reasons, they are preferable in terms of validity to all other research designs on crime and violence. At the other end of the validity spectrum, Type I designs—which seek to compare the levels of lesser offenses in different jurisdictions—are likely to be invalidated by *both* these serious methodological problems. For these reasons, it is difficult to imagine circumstances under which Type I research designs can be justified on the basis of official statistics alone.<sup>10</sup>

The other two research designs have methodological weaknesses which are complementary. Type II designs, which examine the levels of hom-

icide in different jurisdictions, are invulnerable to underreporting but are problematic if different indicators (arrests vs. convictions, etc.) are used in these jurisdictions.<sup>11</sup> Type III designs, by contrast, are vulnerable to the possibility of different underreporting proportions in different jurisdictions. But if one is willing to make the assumption that underreporting proportions are comparable in two jurisdictions (or can use victimization surveys to produce corrected offense rates), then these designs are probably valid even if different indicators are reported for these jurisdictions.

As indicated by this typology, the four generic research designs vary greatly in terms of the degree to which they are methodologically vulnerable. For this reason, it does not appear to be meaningful to discuss, in general terms, whether "research on crime statistics" is valid. Instead, this discussion needs to be design-specific rather than global. While certain generic research designs are almost certainly insupportable (at least on the basis of official statistics alone), others are both valid and unproblematic.

**Table 3.3.** Comparative Research Design Typology: Implications of Two Data Problems in Each of Four Designs

Type of Comparison	Offense Type	
	Nonhomicide offenses	Homicide offenses
	Type I (—, —)	Type II (+, —)
Levels	<i>Underreporting</i> (—): Comparisons of levels of lesser offenses cannot be made until the ratio (offenses reported/total offenses) has been estimated for each jurisdiction to be compared.	<i>Underreporting</i> (+): Not a problem in U.S. homicide data; probably not a problem in comparative data.
	<i>Different indicators</i> (—): Comparisons of levels of lesser offenses are not possible if different indicators are present unless (1) underreporting proportions are known (above) and (2) the relationship between different indicators can be estimated.	<i>Different indicators</i> (—): Comparisons of homicide rate levels in different jurisdictions cannot be made if different indicators are reported unless an indicator ratio (e.g., convictions/all homicides) can be estimated for one jurisdiction to produce a common indicator for comparison across jurisdictions.

Table 3.3. (continued)

Type of Comparison	Offense Type	
	Nonhomicide offenses	Homicide offenses
	Type III (-, +)	Type IV (+, +)
Trends	<i>Underreporting</i> (-): Comparisons of trends of lesser offenses can only be made if one can assume or show that the ratio (offenses reported/total offenses) is a constant over time (with no more than random fluctuations) in each jurisdiction compared.	<i>Underreporting</i> (+): Not a problem in U.S. homicide data; probably not a problem in comparative data.
	<i>Different indicators</i> (+): Comparisons of trends in lesser offenses are possible if one assumes a constant underreporting proportion (above) since different indicators of the same offense are strongly correlated over time.	<i>Different indicators</i> (+): Not a problem in U.S. and comparative homicide data; different indicators of homicide are strongly correlated over time.

### CONTROLLING DATA QUALITY

The evidence reviewed above suggests that considerable variation exists (1) between indicators of different crimes, with indicators of homicide having the greatest validity, and (2) between different indicators of the same crime, with the indicator of offenses known the best index of the actual volume of crime. If one's research design calls for a comparison of the levels of violent crime in several jurisdictions, therefore, one might try to include these known differences in data quality in the research design. This might be done, for example, by limiting comparisons to those cases with data on the number of homicide offenses known.

It has been recognized for some time that it should be possible to include in a research design information about the *variable* quality of different data. The idea of "data quality control" was first suggested by Naroll (1962; Naroll and Cohen, 1970) as a method for dealing with large data archives in which some of the data can be assumed to be of

higher quality than others. The basic principle of data quality control is a simple one: one should include in comparative research known data quality differences as a *variable* in the analysis.

There are a number of ways in which this might be done, but perhaps the simplest involves analyzing the data in sequential waves—the first wave would include only the data sets of highest quality, the second wave cases of average quality, and the third wave cases of the most doubtful quality. In this way, an investigator can estimate a result or test a hypothesis *within* each level of data quality. This procedure eliminates the possibility that spurious findings could be produced by comparing indicators of different quality.

For some uses of the data in the CCDF, data quality control may be appropriate and useful. In chapter 4, we present a study of the effects of wars upon violent crime rates in postwar societies. Using the CCDF, it was possible to identify for this analysis a large number of "nation-wars" (one nation in one war) and a number of control societies uninvolved in war. As a test of the effect of wars upon violent crime, the homicide rates of combatant and noncombatant nations were compared before and after the war years. As a part of the analysis, a data quality control procedure was used. In the first analysis, *all* the cases for which any homicide indicator could be obtained were included in the analysis. The second analysis, however, included *only* those cases for which the best homicide indicator (offenses known) was available. In this instance, both analyses pointed to the same conclusion about the effect of wars.

By making variations in data quality an explicit part of the analysis, data quality control excludes the possibility of spurious results due to complex interactions between the antecedent or causal variable and the type of indicator used in the analysis. In our study of the effect of wars, for example, data quality control eliminates the methodological danger that wars could produce increases in the actual level of homicide (offenses known) but decreases in the willingness of judges and juries to sentence offenders to prison (convictions and incarcerations). If this were the case, one might observe homicide increases in nations with the indicator of known offenses but apparent "decreases" in nations with the indicator of convictions.<sup>12</sup> In a large data set, complex interactions like this simple example would be invisible to the researcher without a data quality control procedure.

Whether or not a data quality control procedure should be used in a given study is largely a matter of the researcher's judgment. Most of the CCDF entries have a good indicator for homicide, for example, and studies of this offense may therefore not require data quality controls. The general attraction of a data quality control procedure, however, is that this simple method allows the researcher to examine a relationship

or test a hypothesis while holding constant known or presumed variation in the validity of various data sets.

### SUMMARY AND CONCLUSIONS

The premise guiding the methodological discussions in this chapter has been that the validity of research on crime and violence is *design-specific*. As a result of a number of specific methodological problems, some research designs are not defensible on the basis of official statistics alone, while others are demonstrably unaffected by these same problems. As a result of this analysis, we believe that an undifferentiated approach to crime statistics is methodologically unjustifiable. It cannot be argued persuasively that these data are either suitable or unsuitable for all purposes. It should be emphasized that we are not arguing that the validity of research with crime statistics is in any way indeterminate. Instead, the analysis presented in this chapter demonstrates that great variation exists across different research designs. Some research designs are perhaps fatally flawed, while others can be presumed to be valid.

In addition to the general finding that these methodological problems are design-specific in their implications, a number of specific conclusions appear warranted on the basis of existing research and additional analyses of data from the CCDF:

1. *Homicide Data in All Designs.* There is no evidence that official statistics on homicides underenumerate the actual incidence of this offense. As a result, homicide data constitute a valid basis for comparative research on both the levels and trends of this violent crime.

2. *Nonhomicide Offenses in Level Designs.* Research on the level of nonhomicide offenses involves some threats to validity because official statistics on these crimes are subject to underreporting. While the magnitude and consistency of the underreporting proportions for these offenses are not known for all jurisdictions and time periods, it appears that offense seriousness and underreporting are inversely related—that is, most serious offenses are reported. In addition, one can use information presented in this chapter to test for the likelihood that an observed difference in offense levels between two jurisdictions could be an artifact of differential underreporting.

3. *Nonhomicide Offenses in Trend Designs.* Research on trends in nonhomicide offenses is somewhat less problematic than research on offense levels. In these designs, it is not necessary to know the real level of an offense. The only methodological assumption necessary is that the underreporting proportion remains constant with no more than random fluctuations. In addition, one can use information presented in this chap-

ter to test for the likelihood that an apparent change in an offense rate could be an artifact of differential reporting.

4. *Different Indicators in Level Designs.* In attempting direct comparisons of offense levels in two jurisdictions, it is obviously essential that both jurisdictions report the same indicator. If each jurisdiction reports a different indicator, comparisons of levels of an offense are possible only if one has a sample of two overlapping indicators for the same period for at least one of the two jurisdictions.

5. *Different Indicators in Trend Designs.* While it is true that the indicator of known offenses is preferable to other indicators as a measure of the level of a given offense, a wide variety of indicators appear to be equally valid in research on trends or changes in an offense. As shown in this chapter, good and imperfect indicators are strongly correlated over time and may be considered substitutive in research on offense trends. This finding expands considerably the number of comparisons of offense rate trends and changes which are possible.

6. *Data Quality Control.* In a data set like the CCDF, considerable variation exists across individual cases. This variation includes differences in the indicator used, in the type of offense reported (e.g., "violent crimes" or different violent crimes listed separately), and almost certainly in the quality of the data themselves. These differences can be included in a research design by using a data quality control procedure described in this chapter.

One of the conclusions of the design-specific model is that some research designs are more conservative than others. Since the offense of homicide is fully enumerated, the most conservative comparative research design is to limit comparisons to levels and trends in this offense. In addition, the indicator of offenses known is widely regarded as the most valid offense indicator, so an even more conservative design would be to limit comparisons only to those nations with this particular homicide indicator.

While this research design is certainly unimpeachable, it is our view that nonhomicide data and other indicators are still useful and valid in certain types of research designs. In general, our approach in this chapter has been to examine the strengths and limits of these designs. In many cases, there appears to be strong empirical support for certain less conservative research designs. For example, evidence indicates that a large number of indicators provide an index of crime trends as valid as that provided by the offenses-known indicator. This finding provides strong support for the essentially substitutive nature of different indicators in trend designs.

In other cases, the appropriateness of the less conservative research designs is more a matter of indirect evidence. For example, comparisons of nonhomicide offenses can include the likelihood that differences in underreporting could explain an observed difference in offense rates. In this instance, one may want to approach less conservative research designs in a probabilistic manner. Using information like that presented in this chapter, it is possible in some cases to assess the relative likelihood that an observed difference could be spuriously produced by artifacts like reporting differences or, alternately, that the observed difference is genuine.

In the final analysis, of course, the researcher is best served by all possible efforts to assess the validity of the data in any given analysis. As indicated in this chapter, however, a great many research designs are viable. Certain designs are immune to serious methodological problems while others are less conservative but still valid for specific comparative purposes. While this chapter has attempted to indicate the nature of major methodological pitfalls, the prospect for many forms of comparative research is clearly auspicious.

### NOTES

1. While this conclusion appears to be true, it should not be taken to mean that there are no *unknown* homicides. There are cases, for example, in which a homicide is discovered only long after the fact—for example, when the bodies of long-dead victims are unearthed. Prior to discovery, these victims may have been classified as “missing persons,” “runaways,” etc. One suspects that hidden homicides of this kind are more common in societies like the United States in which homicides are unusually common.
2. Traditional lore among crime researchers has held that the offense of auto theft is also fully enumerated because most insurance policies require that a loss be reported as a prerequisite to compensation. Some victimization surveys have found that this offense is fully enumerated (e.g., Ennis, 1967), while other surveys suggest that the underreporting proportion is at least .90, though less than 1.00 (Velde, Work, and Holtzman, 1975: 61; Skogan, 1977).
3. The underreporting proportion can be estimated in different ways. In the city surveys summarized in table 3.1, the underreporting proportions are the ratio of two *self-report* figures: (1) the number of crimes respondents said they reported to police, and (2) the total number of crimes respondents said they experienced. The UCR/Ennis proportion, however, compares self-report data to the actual police statistics compiled in the FBI's *Uniform Crime Report*.
4. There are some circumstances, however, which can produce a “paper” crime wave merely by sudden increases in the underreporting proportion. An example might be a wave of prostitution or gambling arrests just prior to an election. A celebrated case of changing underreporting proportions involved New York City crime statistics in 1950 (Leonard, 1952; Wolfgang, 1963: 715; President's Commission on Law Enforcement and the Administration of Justice, 1967: 22–23). Before 1950, New York's statistics for several offenses were suspiciously low—for example, the city reported an absolute number of robberies smaller than that for the city of Chicago, yet Chicago's population was half the size of New York's. Under pressure from the FBI's *Uniform*

*Crime Reports*, New York initiated a centralized reporting system. This innovation radically changed the city's underreporting proportion and produced a “paper” crime wave—reported robberies rose about 400% and burglaries increased 1300%.

5. The importance of this issue was first recognized by researchers in the early nineteenth century (Wolfgang, 1963: 713; President's Commission on Law Enforcement and the Administration of Justice, 1967: 21). The assumption that reported offenses bear a constant relation to actual offenses has been an expressed part of the rationale for the interpretability of the records on serious offenses maintained in the FBI's *Uniform Crime Reports* (Wolfgang, 1963: 709). Most researchers interested in comparative research also have concluded that underreporting proportions in other nations are stable over time (Verkko, 1953; Wolf, 1971).
6. Naturally, these comparisons assume that similar questions, question sequences, and interview formats are used. Surveys that used different descriptions of what constitutes “assault” would, of course, produce different levels of self-reported victimization. For this reason, a standardization of victimization survey methods is of obvious importance.
7. It should be emphasized that the “true” incidence of an offense, by definition, can never be known. As discussed earlier, even victimization surveys may omit offenses, particularly those which are either trivial or highly sensitive. In addition, victimization surveys are merely another *estimate* of the offense rate, and the absence of a palpable, perfect record of offense incidence makes it impossible to know precisely how accurate this estimate is. As a result, except in the case of the fully enumerated offense of homicide, all research on crime deals in estimates rather than the *real* incidence of an offense.
8. The obtained correlations in the range of .85–.95 can be seen either as measures of *reliability* (in the test-retest sense where the true volume of an offense is unknown) or as measures of *validity* (if one assumes that the good measure is a reasonably error-free index of the true volume of an offense).
9. The essential argument in this analysis is that one indicator can serve as a proxy for another indicator which happens to be unavailable. In this case, the evidence suggests that an imperfect indicator like convictions can be used as a proxy for the missing indicator of offenses known in a trend design. As will be discussed in a later chapter, other types of proxy indicators are possible—e.g., using trends in urban homicide data to estimate national trends in cases where the national data are unavailable. The use of proxy variables is not recommended, of course, unless one has empirical evidence that the proxy variables are likely to bear a strong relationship to the missing variable. In addition, although most social science research is concerned with trends, some studies involve an interest in residuals, short-term fluctuations which remain when time-series data have been “detrended.” It is our view that this issue is less central to research on crime and violence, and we have not attempted it here. This question could be addressed using CCDF data, of course, simply by “detrending” a good and a poor indicator and then correlating the residuals (Kendall, 1973; Johnston, 1972).
10. Type I designs are possible, however, if one has available certain types of additional information. One would need estimates of the underreporting proportions for each jurisdiction to be compared and, in cases where different indicators are also involved, one would also need to be able to estimate the relationships among these different indicators (e.g., convictions/offenses known). While this additional information is not impossible to obtain, it cannot be estimated from official statistics alone.
11. This comparison is possible, of course, if one can estimate the relationship, within one of the societies, between the given indicator and the missing indicator. In the case of a society which reports only homicide convictions, for example, analysis of official archives might make it possible to learn for a sample of years how many homicide cases had produced the number of convictions reported. This would allow one to



estimate the convictions/known offenses ratio, and this ratio could be used to transform convictions data into estimates of the number of known offenses.

12. It should be noted that the mathematical possibility of interactions of this nature varies inversely with the degree to which different indicators of the same offense are correlated over time. The evidence reviewed earlier in this chapter suggests that different indicators (offenses known, convictions, etc.) are in fact *highly* correlated over time. As a result, it is improbable that homicide offenses and homicide convictions would show contradictory trends. For this reason, the risk of "indicator interactions" in trend designs may not be great in most cases.

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## II.

### VIOLENCE IN CROSS-NATIONAL PERSPECTIVE



```
//EIGH: JOB 21048,ARCHER,CLASS=D,
***@B UCSCVM VS1 21048 A 00000000 UCSCC ARCHER
// MSGCLASS=Q
```

```
LOG >>>> EIGHT - 21048,ARCHER -D- - 14:50 ARCHER
LOG IEF233A M 4C1,SC5820,EIGHT,PTPCH,NEW.EIGHT.PLUS
LOG Job EIGHT,PTPCH(IEBPTPCH) #001 Complete 0000
LOG IEF280E K 4C1,SC5820,EIGHT,PTPCH
// EXEC PTPCH,SYSOUT=Q
XXPTPCH PROC SYSOUT='A' 00000010
XXPTPCH EXEC PGM=IEBPTPCH,REGION=50K 00000020
//SYSPRINT DD SYSOUT=Q
X/SYSPRINT DD SYSOUT=8SYSOUT 00000030
IEF653I SUBSTITUTION JCL - SYSOUT=Q
```

```
//SYSIN DD *
//SYSUT1 DD DSN=NEW.EIGHT.PLUS,
// UNIT=TAPE,
// VOL=(,RETAIN,SER=SC5820),
// DISP=OLD,
// LABEL=(2,SL)
//SYSUT2 DD SYSOUT=Q,DCB=(RECFM=FBA,LRECL=81)
//
```

```
IEF236I ALLOC. FOR EIGHT PTPCH
IEF237I 4C1 ALLOCATED TO SYSUT1
IEF142I - STEP WAS EXECUTED - COND CODE 0000
IEF285I NEW.EIGHT.PLUS KEPT
IEF285I VOL SER NOS= SC5820.
IEF373I STEP /PTPCH / START 86245.1450
IEF374I STEP /PTPCH / STOP 86245.1452 CPU OMIN 02.30SEC STDR VIRT 122K
```

Device type	Address	I/O count	Sysout DD name	Lines	Class	Copies
Tape	4C1	353	SYSPRINT	5	Q	
			SYSUT2	3,520	Q	

----- Total sysin count 1. Partition size 1,152K; Memory used 122K.

```
IEF280E K 4C1,SC5820,EIGHT,PTPCH
IEF298I EIGHT SYSOUT=Q.
IEF375I JOB /EIGHT / START 86245.1450
IEF376I JOB /EIGHT / STOP 86245.1452 CPU OMIN 02.30SEC
```

Job started Sep 02,1986 Tue 14:50:13 CPU 1d-014561

Estimated job cost (does not include tape mounts or JCL printing):

CPU time	\$ .28	00:00:02.30	at \$450. per hour
I/O requests	\$ .42	353	at \$1.20 per 1000
Lines printed	\$4.05	3,525	at \$1.15 per 1000

Total \$4.75 (\$ .35 minimum).

Aus. ha

PRINT/PUNCH DATA SET UTILITY

PAGE 0001

PUNCH

EOF ON SYSIN

END OF DATA FOR SDS OR MEMBER

20 3 1	-1	-1	51	90	244	-1	-1	3850	19
20 4 1	-1	-1	58	97	225	-1	-1	3926	20
20 5 1	-1	-1	46	96	256	-1	-1	4002	21
20 6 1	-1	-1	56	70	239	-1	-1	4078	22
20 7 1	-1	-1	45	90	255	-1	-1	4154	23
20 8 1	-1	-1	46	60	278	-1	-1	4230	24
20 9 1	-1	-1	45	59	260	-1	-1	4306	25
20 10 1	-1	-1	46	77	250	-1	-1	4382	26
20 11 1	-1	-1	33	87	235	-1	-1	4455	27
20 12 1	-1	-1	49	88	221	-1	-1	4556	28
20 13 1	-1	-1	52	71	298	-1	-1	4657	29
20 14 1	-1	-1	57	67	300	-1	-1	4758	30
20 15 1	-1	-1	40	14	226	-1	-1	4859	31
20 16 1	-1	-1	35	15	193	-1	-1	4960	32
20 17 1	-1	-1	30	15	239	-1	-1	5061	33
20 18 1	-1	-1	21	11	155	-1	-1	5162	34
20 19 1	-1	-1	48	3	220	-1	-1	5263	35
20 20 1	-1	-1	38	7	223	-1	-1	5360	36
20 21 1	-1	-1	46	8	235	-1	-1	5476	37
20 22 1	-1	-1	46	5	240	-1	-1	5592	38
20 23 1	-1	-1	30	8	246	-1	-1	5708	39
20 24 1	-1	-1	34	5	217	-1	-1	5824	40
20 25 1	-1	-1	41	8	253	-1	-1	5939	41
20 26 1	-1	-1	37	15	235	-1	-1	6044	42
20 27 1	-1	-1	45	14	224	-1	-1	6149	43
20 28 1	-1	-1	42	11	222	-1	-1	6252	44
20 29 1	-1	-1	48	15	244	-1	-1	6359	45
20 30 1	-1	-1	29	14	224	-1	-1	6463	46
20 31 1	-1	-1	51	9	211	-1	-1	6516	47
20 32 1	-1	-1	32	14	217	-1	-1	6569	48
20 33 1	-1	-1	36	16	216	-1	-1	6622	49
20 34 1	-1	-1	32	6	191	-1	-1	6675	50
20 35 1	-1	-1	40	10	188	-1	-1	6726	51
20 36 1	-1	-1	34	14	182	-1	-1	6789	52
20 37 1	-1	-1	38	20	227	-1	-1	6852	53
20 38 1	-1	-1	37	9	60	85	754	6915	54
20 39 1	-1	-1	34	10	64	117	988	6978	55
20 40 1	-1	-1	-1	-1	-1	-1	-1	-1	56
20 41 1	-1	-1	33	28	46	100	697	7110	57
20 42 1	-1	-1	41	14	74	134	748	7181	58
20 43 1	-1	-1	37	24	62	148	868	7235	59
20 44 1	-1	-1	-1	-1	-1	-1	-1	-1	60
20 45 1	-1	-1	-1	-1	-1	-1	-1	-1	61
20 46 1	-1	-1	37	25	130	110	1033	7465	62
20 47 1	-1	-1	51	22	170	128	1027	7579	63
20 48 1	-1	-1	59	13	173	104	1072	7709	64
20 49 1	-1	-1	60	10	175	138	1073	7908	65
20 50 1	-1	-1	67	17	215	170	1103	8179	66
20 51 1	-1	-1	67	19	203	176	1158	8422	67
20 52 1	-1	-1	61	21	207	165	1457	8636	68
20 53 1	-1	-1	83	22	318	137	1405	8818	69
20 54 1	-1	-1	76	26	270	197	1333	8987	70
20 55 1	-1	-1	83	17	271	158	1373	9201	71
20 56 1	-1	-1	60	43	268	162	1805	9426	72
20 57 1	-1	-1	79	13	241	184	2286	9640	73
20 58 1	-1	-1	80	25	245	158	2533	9842	74
20 59 1	-1	-1	93	29	212	188	2404	10056	75
20 60 1	-1	-1	87	39	222	229	2727	10275	76
20 61 1	-1	-1	90	70	254	223	3235	10508	77

Austria

Bahrain

2062	1	-1	-1	114	48	231	298	3026	10705	78
2063	1	-1	-1	95	83	185	135	3095	10916	79
2064	1	143	34	272	262	1924	592	39695	11136	80
2065	1	141	29	271	257	1898	7730	46826	11380	81
2066	1	163	29	321	251	2227	992	56841	11600	82
2067	1	137	36	300	311	2158	960	19072	11800	83
2068	1	143	43	300	363	2508	1280	23562	12000	84
2069	1	157	33	280	364	2483	1599	25597	12260	85
2070	1	172	22	339	416	3282	1999	30591	12510	86
2071	1	185	39	343	578	3862	2818	33925	12760	87
2072	1	207	46	422	544	4285	3045	36976	12960	88
2553	1	60	23	-1	698	2830	250	20720	6954	99
2554	1	68	23	-1	609	2753	272	20930	6969	100
2555	1	66	30	-1	730	2888	311	22085	6974	101
2556	1	88	20	-1	659	2584	292	24627	6983	102
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23546	1	8	7	-1	12	1514	31	10900 2957	1193
23547	1	3	14	-1	12	1138	24	10886 2974	1194
23548	1	17	5	-1	18	1330	33	10221 2985	1195
23549	1	6	9	-1	16	1369	28	8388 2981	1196
23550	1	9	5	-1	14	1258	32	8414 2969	1197

23552	1	10	3	-1	17	1412	23	10561	2949	1199
23553	1	12	7	-1	9	1497	36	11229	2945	1200
23554	1	4	1	-1	12	1466	24	8243	2933	1201
23555	1	5	3	-1	12	1388	41	8141	2909	1202
23556	1	4	3	-1	6	1563	27	8900	2898	1203
23557	1	12	2	-1	9	1402	33	9842	2885	1204
23558	1	10	2	-1	10	1500	61	11766	2853	1205
23559	1	9	2	-1	5	1545	59	12655	2846	1206
23560	1	4	2	-1	12	1592	59	10709	2834	1207
23561	1	11	2	-1	7	1779	42	9861	2818	1208
23562	1	6	6	-1	12	1824	39	9817	2824	1209
23563	1	4	1	-1	13	2236	42	10002	2841	1210
23564	1	9	5	-1	19	2363	62	11150	2849	1211
23565	1	7	5	-1	22	2692	77	10140	2880	1212
23566	1	8	7	-1	13	2876	73	11791	2880	1213
23567	1	9	4	-1	15	3047	86	12481	2900	1214
23568	1	12	3	-1	16	3364	101	13893	2910	1215
23569	1	8	5	-1	10	3680	147	15477	2930	1216
23570	1	12	7	-1	15	3645	215	18570	2950	1217
23571	1	10	4	-1	24	4365	314	23242	2970	1218
23572	1	22	6	-1	26	4840	618	24280	3010	1219
23573	1	23	6	-1	42	6258	619	22206	3033	1220
24049	1	47	-1	-1	-1	1334	196	7257	1066	1229
24050	1	50	-1	-1	-1	1982	201	9142	1258	1230
24051	1	61	-1	-1	-1	2618	165	12774	1516	1231
24052	1	61	-1	-1	-1	3035	157	16840	1607	1232
24053	1	66	-1	-1	-1	2584	125	14571	1650	1233
24054	1	85	-1	-1	-1	3051	85	15291	1688	1234



SEP 19 1986 86/2

```
*****
** VM/SP SERVICE LEVEL 0310 *** CPU SERIAL 014561 CPU MODEL 4341 *****
** PRINT STARTED 09/03/86 16:23:31 *****
```

RSCS	RSCS	USERID	ORIGIN	*****	*****	*****
ARCHER	UCSCVM	DISTCODE	SYSTEM	*****	*****	*****
		FILENAME	FILETYPE	*****	*****	*****
09/03/86	16:16:26	FILE CREATION DATE		*****	*****	*****
5824	00001001	SPOOLID	COUNT	*****	*****	*****
09/03/86	16:23:31	FILE PRINT DATE		*****	*****	*****
0	04A	CLASS	DEVICE			
STANDARD		FORMS				

	VV	VV	MM	MM
	VV	VV	MM	MM
	VV	VV	MMM	MMM
	VV	VV	MMM	MMM
	VV	VV	MM MM	MM MM
3333333333		7777777777	MMMM	C0000000
33333333333		7777777777	MM	0000000000
33	VV33	77VV	77	00MM 00
	V33	VV	77M	00MM 00
	33	VV	77MM	00MM 00
	3333VV	VV	77	MM 00MM 00
	3333	VVVV	77	MM 00MM 00
	33	VV	77	MM 00MM 00
	33		77	MM 00 00
33	33		77	MM 00 00
33333333333				0000000000
3333333333				C0000000

TAG DATA: FILE (2620) ORIGIN UCSCC daemon 9/03/86 16:19:49 P.D.T.

**DANE ARCHER**  
**STEVENSON COLLEGE**  
**UNIVERSITY OF CALIFORNIA**  
**SANTA CRUZ, CALIF. 95064**

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```

"eight" (sample)  
(raw data set)

\*\*\*\*\*

```
//EIGH: JOB 21048,ARCHER,CLASS=D,
***@B UCSCVM VSI 21048 A 00000000 UCSCC ARCHER
// MSGCLASS=Q
```

```
LOG >>>> EIGHT - 21048,ARCHER -D- - 14:50 ARCHER
LOG IEF233A M 4C1,SC5820,EIGHT,PTPCH,NEW,EIGHT,PLUS
LOG Job EIGHT,PTPCH(IEBPTPCH) #001 Complete 0000
LOG IEF280E K 4C1,SC5820,EIGHT,PTPCH
// EXEC PTPCH,SYSOUT=Q
XXPTPCH PROC SYSOUT='A' 00000010
XXPTPCH EXEC PGM=IEBPTPCH,REGION=50K 00000020
//SYSPRINT DD SYSOUT=Q
X/SYSPRINT DD SYSOUT=SYSOUT 00000030
IEF653I SUBSTITUTION JCL - SYSOUT=Q
```

```
//SYSIN DD *
//SYSUT1 DD DSN=NEW.EIGHT.PLUS,
// UNIT=TAPE,
// VOL=(,RETAIN,SER=SC5820),
// DISP=OLD,
// LABEL=(2,SL)
//SYSUT2 DD SYSOUT=Q,DCB=(RECFM=FBA,LRECL=81)
//
```

```
IEF236I ALLOC FOR EIGHT PTPCH
IEF237I 4C1 ALLOCATED TO SYSUT1
IEF142I - STEP WAS EXECUTED - COND CODE 0000
IEF285I NEW.EIGHT.PLUS KEPT
IEF285I VOL SER NOS= SC5820.
IEF373I STEP /PTPCH / START 86245.1450
IEF374I STEP /PTPCH / STOP 86245.1452 CPU OMIN 02.30SEC STOR VIRT 122K
```

Device type	Address	I/O count	Sysout DD name	Lines	Class	Copies
Tape	4C1	353	SYSPRINT	5	Q	
			SYSUT2	3,520	Q	

```
----- Total sysin count 1 Partition size 1,152K; Memory used 122K.
IEF280E K 4C1,SC5820,EIGHT,PTPCH
IEF298I EIGHT SYSOUT=Q.
IEF375I JOB /EIGHT / START 86245.1450
IEF376I JOB /EIGHT / STOP 86245.1452 CPU OMIN 02.30SEC
```

Job started Sep 02, 1986 Tue 14:50:13 CPU 1d-014561

Estimated job cost (does not include tape mounts or JCL printing):

CPU time	\$ .28	00:00:02.30	at \$450. per hour
I/O requests	\$ .42	353	at \$1.20 per 1000
Lines printed	\$4.05	3,525	at \$1.15 per 1000

Total \$4.75 (\$ .35 minimum).

PUNCH

EOF ON SYSIN

END OF DATA FOR SDS OR MEMBER

20 3 1	-1	-1	51	90	244	-1	-1	3850	19
20 4 1	-1	-1	58	97	225	-1	-1	3926	20
20 5 1	-1	-1	46	96	256	-1	-1	4002	21
20 6 1	-1	-1	58	70	239	-1	-1	4078	22
20 7 1	-1	-1	45	80	255	-1	-1	4154	23
20 8 1	-1	-1	46	60	278	-1	-1	4230	24
20 9 1	-1	-1	45	59	260	-1	-1	4306	25
2010 1	-1	-1	46	77	250	-1	-1	4382	26
2011 1	-1	-1	33	97	235	-1	-1	4455	27
2012 1	-1	-1	48	88	221	-1	-1	4556	28
2013 1	-1	-1	52	71	298	-1	-1	4657	29
2014 1	-1	-1	57	67	300	-1	-1	4758	30
2015 1	-1	-1	40	14	226	-1	-1	4859	31
2016 1	-1	-1	35	15	193	-1	-1	4960	32
2017 1	-1	-1	30	15	239	-1	-1	8061	33
2018 1	-1	-1	21	11	155	-1	-1	9162	34
2019 1	-1	-1	48	3	220	-1	-1	8263	35
2020 1	-1	-1	38	7	223	-1	-1	5360	36
2021 1	-1	-1	46	8	235	-1	-1	5476	37
2022 1	-1	-1	46	5	240	-1	-1	5592	38
2023 1	-1	-1	30	8	246	-1	-1	8708	39
2024 1	-1	-1	34	5	217	-1	-1	9824	40
2025 1	-1	-1	41	8	283	-1	-1	8939	41
2026 1	-1	-1	37	15	235	-1	-1	6044	42
2027 1	-1	-1	45	14	224	-1	-1	6149	43
2028 1	-1	-1	42	11	222	-1	-1	6252	44
2029 1	-1	-1	48	15	244	-1	-1	8359	45
2030 1	-1	-1	28	14	224	-1	-1	6463	46
2031 1	-1	-1	51	9	211	-1	-1	6816	47
2032 1	-1	-1	32	14	217	-1	-1	6569	48
2033 1	-1	-1	36	16	216	-1	-1	6622	49
2034 1	-1	-1	32	6	191	-1	-1	6675	50
2035 1	-1	-1	40	10	186	-1	-1	6726	51
2036 1	-1	-1	34	14	182	-1	-1	6789	52
2037 1	-1	-1	38	20	227	-1	-1	6852	53
2038 1	-1	-1	37	9	60	85	754	6915	54
2039 1	-1	-1	34	10	64	117	988	6978	55
2040 1	-1	-1	-1	-1	-1	-1	-1	-1	56
2041 1	-1	-1	33	28	46	100	597	7110	57
2042 1	-1	-1	41	14	74	134	748	7181	58
2043 1	-1	-1	37	24	62	148	868	7235	59
2044 1	-1	-1	-1	-1	-1	-1	-1	-1	60
2045 1	-1	-1	-1	-1	-1	-1	-1	-1	61
2046 1	-1	-1	37	25	130	110	1033	7465	62
2047 1	-1	-1	51	22	170	128	1027	7579	63
2048 1	-1	-1	59	13	173	104	1072	7709	64
2049 1	-1	-1	60	10	175	138	1073	7808	65
2050 1	-1	-1	67	17	215	170	1103	8179	66
2051 1	-1	-1	67	19	203	176	1158	8422	67
2052 1	-1	-1	61	21	207	165	1457	8636	68
2053 1	-1	-1	83	22	318	137	1405	8818	69
2054 1	-1	-1	76	26	270	197	1333	8987	70
2055 1	-1	-1	83	17	271	158	1373	9201	71
2056 1	-1	-1	60	43	268	162	1805	9426	72
2057 1	-1	-1	79	13	241	184	2286	9640	73
2058 1	-1	-1	80	25	245	158	2533	9842	74
2059 1	-1	-1	93	29	212	188	2404	10056	75
2060 1	-1	-1	87	39	222	229	2727	10275	76
2061 1	-1	-1	90	70	254	223	3235	10508	77

2062	1	-1	-1	114	48	231	298	3026	10705	78
2063	1	-1	-1	95	83	185	135	3095	10916	79
2064	1	143	34	272	262	1924	592	39695	11136	80
2065	1	141	29	271	287	1898	7730	46828	11390	81
2066	1	163	29	321	251	2227	892	56841	11800	82
2067	1	137	36	300	311	2158	960	19072	11800	83
2068	1	143	43	300	363	2508	1280	23562	12000	84
2069	1	157	33	280	364	2483	1599	25597	12260	85
2070	1	172	22	339	416	3282	1999	30591	12510	86
2071	1	185	39	343	578	3862	2818	33825	12760	87
2072	1	207	46	422	644	4285	3048	38976	12960	88
2553	1	60	23	-1	688	2830	250	20720	8954	99
2554	1	68	23	-1	608	2753	272	20930	6969	100
2555	1	66	30	-1	730	2888	311	22085	6974	101
2556	1	88	20	-1	659	2584	292	24627	6983	102
2557	1	59	33	-1	540	2907	338	28287	6997	103
2558	1	53	14	-1	640	2914	412	28568	7021	104
2559	1	62	30	-1	683	2855	353	29554	7048	105
2560	1	55	25	-1	671	2856	362	30569	7048	106
2561	1	45	25	-1	641	2424	383	32261	7087	107
2562	1	54	20	-1	709	2566	367	36618	7130	108
2563	1	47	20	-1	600	2781	378	38618	7172	109
2564	1	44	22	-1	689	2688	405	40180	7220	110
2565	1	45	29	-1	604	2648	433	45089	7250	111
2566	1	49	14	-1	582	2667	461	50667	7290	112
2567	1	53	24	-1	583	2954	597	55841	7320	113
2568	1	45	10	-1	600	2845	520	62873	7350	114
2569	1	82	30	-1	658	2923	550	65486	7376	115
2570	1	70	35	-1	673	3178	548	67676	7390	116
2571	1	69	36	-1	625	2879	684	67649	7460	117
2572	1	63	40	-1	679	3136	761	84127	7490	118
2573	1	70	34	-1	630	3132	825	81687	7522	119
3063	1	-1	-1	2	11	10	20	0	170	124
3064	1	-1	-1	4	8	6	13	0	177	125
3065	1	-1	-1	3	5	8	22	0	185	126
3066	1	-1	-1	2	5	7	16	2	193	127
3067	1	-1	-1	1	6	4	14	1	190	128
3068	1	-1	-1	5	1	3	7	1	200	129
3069	1	-1	-1	2	4	4	15	2	210	130
3070	1	-1	-1	2	1	3	14	3	220	131
3071	1	-1	-1	2	3	4	16	4	220	132
3072	1	-1	-1	1	0	8	16	2	220	133
3073	1	-1	-1	2	5	4	28	0	227	134
3541	1	-1	-1	11	294	-1	-1	15689	8274	149
3542	1	-1	-1	15	311	-1	-1	22440	8246	150
3543	1	-1	-1	28	308	-1	-1	24018	8241	151
3544	1	-1	-1	5	254	-1	-1	12851	8291	152
3545	1	-1	-1	33	259	-1	-1	5844	8339	153
3546	1	-1	-1	37	399	-1	-1	7520	8367	154
3547	1	-1	-1	45	451	-1	-1	6981	8450	155
3548	1	-1	-1	49	562	-1	-1	7942	8557	156
3549	1	-1	-1	41	783	-1	-1	6884	8614	157
3550	1	-1	-1	29	775	-1	-1	4866	8638	158
3551	1	-1	-1	22	661	-1	-1	5653	8678	159
3552	1	-1	-1	27	747	-1	-1	6932	8730	160
3553	1	-1	-1	20	749	-1	-1	5300	8778	161
3554	1	-1	-1	11	705	-1	-1	4438	8819	162
3555	1	-1	-1	18	713	-1	-1	4266	8868	163
3556	1	-1	-1	14	806	-1	-1	4860	8924	164
3557	1	-1	-1	15	857	-1	-1	4986	8989	165
3558	1	-1	-1	17	825	-1	-1	4974	9053	166
3559	1	-1	-1	22	1009	-1	-1	4977	9104	167
3560	1	-1	-1	21	1130	-1	-1	5558	9153	168
3561	1	-1	-1	11	1114	-1	-1	5667	9184	169
3562	1	-1	-1	18	1141	-1	-1	5714	9221	170
3563	1	-1	-1	28	1051	-1	-1	5847	9222	

3564	1	-1	-1	16	993	-1	-1	5980	9378	172
3565	1	-1	-1	24	990	-1	-1	5577	9460	173
3566	1	-1	-1	26	866	-1	-1	6220	9530	174
3567	1	-1	-1	27	1049	-1	-1	7158	9580	175
3568	1	-1	-1	27	1006	-1	-1	7087	9620	176
3569	1	-1	-1	34	808	-1	-1	6274	9680	177
4041	1	1	1	-1	4	106	4	393	32	182
4042	1	1	0	-1	1	133	2	415	33	183
4043	1	1	1	-1	0	126	5	335	33	184
4044	1	0	1	-1	0	138	3	385	34	185
4045	1	2	1	-1	3	98	8	408	35	186
4046	1	0	1	-1	2	113	2	419	35	187
4047	1	1	1	-1	1	98	9	581	35	188
4048	1	0	0	-1	13	85	4	598	36	189
4049	1	2	0	-1	7	91	3	515	36	190
4050	1	1	2	-1	4	82	2	503	37	191
4051	1	0	1	-1	7	101	5	552	38	192
4052	1	-1	-1	-1	-1	-1	-1	-1	-1	193
4053	1	1	1	-1	4	60	13	752	39	194
4054	1	3	2	-1	0	71	12	796	40	195
4055	1	0	0	-1	3	78	13	832	40	196
4056	1	2	1	-1	5	89	15	1109	41	197
4057	1	0	1	-1	1	113	8	896	42	198
4058	1	0	2	-1	5	96	11	1910	43	199
4059	1	7	1	-1	4	134	11	1757	43	200
4060	1	2	1	-1	3	100	14	2558	44	201
4061	1	1	2	-1	2	97	15	2518	45	202
4062	1	1	0	-1	2	127	21	2909	46	203
4063	1	0	0	-1	7	124	22	3059	47	204
4064	1	2	1	-1	5	132	22	2800	48	205
4065	1	0	0	-1	3	121	18	3416	50	206
4066	1	1	3	-1	3	147	12	3305	50	207
4067	1	2	0	-1	5	176	16	3759	50	208
4068	1	2	4	-1	2	85	11	2509	50	209
4069	1	1	1	-1	4	98	11	3092	50	210
4070	1	-1	-1	-1	-1	-1	-1	-1	-1	211
4071	1	3	0	-1	5	156	26	2120	50	212
4072	1	2	0	-1	3	155	17	1769	51	213
4073	1	7	1	-1	5	100	33	1837	52	214
4074	1	5	1	-1	5	112	39	2594	52	215
5564	1	0	-1	0	1	68	0	122	100	229
5565	1	0	-1	0	2	56	0	113	100	230
5566	1	1	-1	0	3	62	0	185	110	231
5567	1	1	-1	0	0	13	0	189	110	232
5568	1	0	-1	0	1	31	0	166	120	233
5569	1	1	-1	0	1	46	2	245	120	234
5570	1	2	-1	0	0	250	4	284	130	235
5571	1	0	-1	0	3	263	2	328	140	236
5572	1	0	-1	0	0	316	4	335	140	237
5573	1	2	-1	0	4	336	4	297	143	238
5574	1	2	-1	0	3	175	9	254	146	239
6553	1	11495	-1	-1	2207	-1	38573	46164	19710	272
6554	1	9072	-1	-1	1944	-1	31482	37613	20040	273
6555	1	7246	-1	-1	1737	-1	28378	40999	20390	274
6556	1	12034	-1	-1	2504	-1	36154	45860	20730	275
6557	1	2302	-1	-1	528	-1	6320	9180	21130	276
6558	1	10256	-1	-1	2463	-1	30133	43388	21530	277
6559	1	7091	-1	-1	3761	-1	16661	34033	21940	278
6560	1	7879	-1	-1	3427	-1	19566	33480	22360	279
6561	1	8232	-1	-1	3549	-1	20307	34713	22780	280
6562	1	7400	-1	-1	3704	-1	15992	37052	23253	281
6563	1	6774	-1	-1	3717	-1	14968	41030	23735	282
6564	1	6792	-1	-1	3720	-1	17712	45948	24229	283
6565	1	9120	-1	-1	5040	-1	25368	64656	24730	284
6566	1	9975	-1	-1	4200	-1	30600	68725	25250	285
6567	1	9698	-1	-1	3562	-1	29432	88764	25810	286



6568	1	8505	-1	-1	6831	-1	20061	75141	26390	287
6569	1	7755	-1	-1	3273	-1	15098	60115	26980	288
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19572	1	-1	29	-1	24	103	66	1147	90	968
1957301	1	-1	28	-1	22	85	79	1246	90	968
1957401	1	-1	33	-1	20	95	86	1422	90	970
19061	1	25	8	-1	54	146	107	647	583	975
19062	1	31	15	-1	66	154	169	1158	598	976
19063	1	46	15	-1	56	174	264	1022	610	977
19064	1	166	8	-1	84	218	267	678	628	978
19065	1	43	6	-1	96	251	338	794	643	979
19066	1	40	1	-1	99	241	308	863	658	980
19067	1	30	3	-1	94	224	252	1285	673	981
19068	1	52	3	-1	118	178	299	1011	688	982
19069	1	44	2	-1	122	219	459	859	703	983
19070	1	47	3	-1	148	259	434	966	720	984
19071	1	49	6	-1	128	280	571	855	740	985
19072	1	63	2	-1	138	246	452	943	750	986
20053	1	-1	-1	28	3	194	102	11780	2250	993
20054	1	-1	-1	38	4	336	109	13477	2360	994
20055	1	-1	-1	28	1	318	141	13261	2490	995
20056	1	-1	-1	39	3	414	127	14534	2615	996
20057	1	-1	-1	17	3	341	122	12413	2736	997
20058	1	-1	-1	30	4	432	104	9665	2854	998
20059	1	-1	-1	24	1	404	82	9241	2967	999
20060	1	-1	-1	30	2	539	89	8578	3075	1000
20061	1	-1	-1	25	1	638	114	9821	3178	1001
20062	1	-1	-1	20	4	675	130	8902	3310	1002
20063	1	-1	-1	25	3	633	153	8274	3420	1003
20064	1	-1	-1	42	2	941	230	8779	3500	1004
20065	1	-1	-1	33	5	905	380	11329	3600	1005
20066	1	-1	-1	40	12	1054	520	11664	3630	1006
20067	1	-1	-1	73	13	1068	1224	10854	3720	1007
20068	1	-1	-1	50	10	1060	1801	10610	3800	1008
20069	1	-1	-1	55	31	1266	2327	10452	3860	1009
20070	1	-1	-1	71	28	1337	3006	11579	3960	1010
20071	1	-1	-1	98	45	1598	5146	11937	4050	1011
20072	1	-1	-1	115	62	1726	7404	10728	4080	1012
20073	1	-1	-1	110	64	2237	8717	13210	4160	1013
205 0	1	231	347	-1	91	3258	82	15331	17748	1017

205 3 1	353	551	-1	253	6581	124	22296	18118	1020
205 4 1	422	512	-1	281	25584	144	20225	18336	1021
205 5 1	439	463	-1	212	28246	213	25822	18486	1022
205 6 1	419	487	-1	239	30268	197	24424	18837	1023
205 7 1	429	520	-1	288	27753	142	23306	18790	1024
205 8 1	471	480	-1	311	27993	158	24505	18944	1025
205 9 1	511	521	-1	355	24929	182	21105	19098	1026
20510 1	478	520	-1	369	25934	214	19769	19255	1027
20511 1	504	587	-1	428	25177	197	20093	19412	1028
20512 1	460	616	-1	418	25571	161	14208	19569	1029
20513 1	489	637	-1	374	23293	189	21165	19727	1030
20514 1	391	609	-1	366	18115	143	18409	19887	1031
20515 1	316	593	-1	228	10894	137	22198	20048	1032
20516 1	260	530	-1	170	8893	123	20523	20211	1033
20517 1	215	474	-1	113	8250	107	23867	20374	1034
20521 1	194	100	-1	50	8171	118	18269	8049	1038
20522 1	222	161	-1	88	8347	140	22644	8111	1039
20523 1	251	195	-1	96	8121	107	27647	8173	1040
20524 1	269	242	-1	148	8882	127	26268	8235	1041
20525 1	266	331	-1	240	8447	103	18276	8299	1042
20526 1	238	284	-1	228	8374	99	12841	8368	1043
20527 1	238	309	-1	214	8136	80	12466	8439	1044
20528 1	238	375	-1	234	8897	73	12268	8809	1045
20529 1	265	417	-1	325	8805	70	11534	8579	1046
20530 1	338	429	-1	386	9864	70	11905	8649	1047
20531 1	400	526	-1	355	11423	93	12746	8715	1048
20532 1	423	490	-1	347	12749	131	16422	8781	1049
20533 1	335	484	-1	351	13573	111	18735	8847	1050
20534 1	330	397	-1	314	12130	104	17506	8913	1051
20535 1	301	377	-1	260	11797	108	19240	8980	1052
20536 1	354	449	-1	312	12074	129	20859	9040	1053
20537 1	372	476	-1	306	11684	95	19143	9100	1054
20538 1	413	467	-1	287	11647	141	17929	9160	1055
20541 1	347	308	-1	-1	-1	107	18856	9419	1058
20552 1	288	222	-1	-1	-1	118	27281	9498	1059
20553 1	253	166	-1	-1	-1	178	26791	9585	1060
20554 1	269	86	-1	-1	7105	156	23647	9691	1061
20555 1	515	90	-1	-1	8243	188	34622	9808	1062
20556 1	338	74	-1	-1	7180	107	17774	9811	1063
20557 1	447	62	-1	-1	7856	104	9617	9840	1064
20558 1	458	96	-1	290	8456	109	10770	9882	1065
20559 1	409	118	-1	413	12749	136	11009	9937	1066
20560 1	340	82	-1	382	8785	159	8867	9984	1067
20561 1	301	94	-1	371	8940	113	10474	10028	1068
20562 1	383	56	-1	475	9049	130	11133	10061	1069
20563 1	386	46	-1	370	8426	165	7788	10088	1070
20564 1	480	49	-1	468	7560	173	10163	10100	1071
20565 1	485	59	-1	469	7778	189	12534	10130	1072
20566 1	466	62	-1	439	7735	199	13953	10170	1073
20567 1	424	63	-1	528	8715	279	13671	10210	1074
20568 1	372	32	-1	429	7988	294	10934	10250	1075
20569 1	406	46	-1	521	7144	261	8095	10290	1076
20570 1	430	27	-1	680	6343	376	5775	10320	1077
20571 1	370	52	-1	604	6917	495	8592	10350	1078
20572 1	422	41	-1	560	6739	482	9515	10380	1079
22553 1	9802	-1	-1	-1	-1	13986	256567372000		1089
22554 1	9765	-1	-1	-1	-1	12995	223866377000		1090
22555 1	9700	-1	-1	-1	-1	11489	21202838821		1091
22556 1	10025	-1	-1	-1	-1	13015	236214384217		1092
22557 1	10419	-1	-1	-1	-1	12968	233239402225		1093
22558 1	10661	-1	-1	-1	-1	11778	236103410686		1094
22559 1	10712	-1	-1	-1	-1	10141	233052419613		1095
22560 1	10910	-1	-1	-1	-1	10244	228842429027		1096
22561 1	11188	-1	-1	-1	-1	10641	232868439000		1097
22562 1	11586	-1	-1	-1	-1	12441	252453449641		1098

22563	1	10754	-1	-1	-1	-1	12691	242487460490	1099
22564	1	11748	-1	-1	-1	-1	13623	273676471627	1100
22565	1	12310	-1	-1	-1	-1	13022	273702483410	1101
22566	1	12691	-1	-1	-1	-1	13402	298701494110	1102
22567	1	13398	-1	-1	-1	-1	16582	339861508080	1103
22568	1	13841	-1	-1	-1	-1	16551	318813516380	1104
22569	1	14732	-1	-1	-1	-1	15971	300140527950	1105
22570	1	15708	-1	-1	-1	-1	26795	337211539860	1106
22571	1	16180	-1	-1	-1	-1	29595	335204551830	1107
23052	1	4015	-1	-1	-1	-1	48460	187034 78700	1122
23053	1	3823	-1	-1	-1	-1	14893	180465 79900	1123
23054	1	3464	-1	-1	-1	-1	12569	163357 81100	1124
23055	1	2979	-1	-1	-1	-1	13108	193537 83858	1125
23056	1	3216	-1	-1	-1	-1	16357	199556 85654	1126
23057	1	3056	-1	-1	-1	-1	15595	190780 87514	1127
23058	1	2615	-1	-1	-1	-1	16006	235934 89441	1128
23059	1	2720	-1	-1	-1	-1	13818	235617 91438	1129
23060	1	2310	-1	-1	-1	-1	11618	225372 83506	1130
23061	1	2117	-1	-1	-1	-1	9033	197882 95655	1131
23062	1	1301	-1	-1	-1	-1	6198	246192 97765	1132
23063	1	1270	-1	-1	-1	-1	4831	213760100045	1133
23064	1	1228	-1	-1	-1	-1	5412	237031102070	1134
23065	1	3497	-1	-1	-1	-1	6229	182387104340	1135
23066	1	1634	-1	-1	-1	-1	4434	144886106610	1136
235 0	1	38	54	-1	88	20199	61	782 4466	1155
235 1	1	25	37	-1	97	20199	94	807 4459	1156
235 2	1	39	36	-1	78	20199	108	678 4452	1157
235 3	1	26	47	-1	77	16260	90	616 4446	1158
235 4	1	29	43	-1	87	16260	81	644 4439	1159
235 5	1	36	45	-1	81	16260	88	588 4432	1160
235 6	1	19	44	-1	60	16260	68	550 4426	1161
235 7	1	23	41	-1	62	16260	82	539 4419	1162
235 8	1	28	29	-1	59	13704	102	534 4413	1163
235 9	1	29	40	-1	60	13704	38	383 4406	1164
23510	1	33	88	-1	76	13704	23	408 4399	1165
23511	1	25	95	-1	75	13704	48	445 4391	1166
23512	1	25	55	-1	65	13151	43	387 4387	1167
23523	1	83	-1	-1	-1	-1	-1	-1 3031	1170
23524	1	48	-1	-1	-1	-1	-1	-1 3007	1171
23525	1	33	83	-1	102	813	308	3428 2985	1172
23526	1	50	88	-1	134	890	207	4301 2973	1173
23527	1	28	80	-1	130	88	88	3943 2961	1174
23528	1	22	23	-1	152	88	59	3292 2949	1175
23529	1	21	32	-1	16	1118	40	3462 2937	1176
23530	1	16	38	-1	13	967	33	3521 2927	1177
23531	1	17	35	-1	23	963	38	3527 2936	1178
23532	1	12	42	-1	8	897	55	3621 2945	1179
23533	1	19	30	-1	138	893	56	3933 2954	1180
23534	1	7	45	-1	154	1353	86	3793 2963	1181
2353501	1	17	51	-1	14	1314	34	3423 2971	1182
2353601	1	16	50	-1	18	1257	32	3712 2968	1183
23537	1	12	58	-1	15	896	15	3840 2965	1184
23538	1	14	8	-1	13	1064	27	4360 2962	1185
23539	1	10	12	-1	4	137	32	5707 2959	1186
23540	1	12	12	-1	6	904	41	6293 2958	1187
23541	1	9	15	-1	9	971	56	9760 2961	1188
23542	1	8	11	-1	14	1238	42	12790 2963	1189
23543	1	13	19	-1	10	1314	75	12630 2946	1190
23544	1	8	13	-1	11	1248	33	11532 2944	1191
23545	1	6	14	-1	16	1612	32	12116 2952	1192
23546	1	8	7	-1	12	1514	31	10900 2957	1193
23547	1	3	14	-1	12	1138	24	10886 2974	1194
23548	1	17	5	-1	18	1330	33	10221 2985	1195
23549	1	6	9	-1	16	1369	28	8388 2981	1196
23550	1	9	5	-1	14	1258	32	8414 2969	1197
23551	1	5	3	-1	9	1395	94	8787 2964	1198

23553	1	12	7	-1	9	1497	36	11229	2945	1200
23554	1	4	1	-1	12	1466	24	8243	2933	1201
23555	1	5	3	-1	12	1388	41	8141	2809	1202
23556	1	4	3	-1	6	1563	27	8800	2898	1203
23557	1	12	2	-1	9	1402	33	8842	2888	1204
23558	1	10	2	-1	10	1500	61	11766	2853	1205
23559	1	9	2	-1	5	1545	59	12655	2846	1206
23560	1	4	2	-1	12	1592	59	10709	2834	1207
23561	1	11	2	-1	7	1779	42	9861	2818	1208
23562	1	6	6	-1	12	1824	38	9817	2824	1209
23563	1	4	1	-1	13	2236	42	10002	2841	1210
23564	1	9	5	-1	19	2363	62	11150	2849	1211
23565	1	7	5	-1	22	2692	77	10140	2880	1212
23566	1	8	7	-1	13	2876	73	11791	2880	1213
23567	1	9	4	-1	15	3047	86	12481	2900	1214
23568	1	12	3	-1	18	3364	101	13893	2910	1215
23569	1	8	5	-1	10	3680	147	15477	2930	1216
23570	1	12	7	-1	15	3645	215	18570	2950	1217
23571	1	10	4	-1	24	4365	314	23242	2970	1218
23572	1	22	6	-1	26	4840	618	24280	3010	1219
23573	1	23	6	-1	42	6258	619	22206	3033	1220
24048	1	47	-1	-1	-1	1334	196	7257	1066	1229
24050	1	50	-1	-1	-1	1982	201	8142	1258	1230
24051	1	61	-1	-1	-1	2618	165	12774	1516	1231
24052	1	61	-1	-1	-1	3035	157	16840	1607	1232
24053	1	66	-1	-1	-1	2584	125	14571	1650	1233
24054	1	65	-1	-1	-1	3051	85	15291	1688	1234

\*\* PRINT FINISHED 09/03/86 16:23:44 \*\*

[illegible]

SEP 19 1986 8612

\*\*\*\*\*  
 \*\* VM/SP SERVICE LEVEL 0310 \*\*\*\* CPU SERIAL 014561 CPU MODEL 4341 \*\*\*\*\*  
 \*\* PRINT STARTED 09/03/86 16:20:50 \*\*\*\*\*

RSCS	RSCS	USERID	ORIGIN	*****	*****	*	VV	VV	MM	MM
ARCHER	UCSCVM	DISTCODE	SYSTEM	*****	*****	**	VV	VV	MMM	MMM
		FILENAME	FILETYPE	**	**	***	VV	VV	MMM	MMM
				**	*****	**	VV	VV	MM MM	MM MM
09/03/86	16:06:41	FILE CREATION DATE		*****	*****	**	3333333333	7777777777	MMMM	00000000
5797	00001100	SPOOLID COUNT		*****	*****	**	3333333333	7777777777	MM	0000000000
09/03/86	16:20:50	FILE PRINT DATE		**	**	**	33	VV33	77	00MM 00
0	04A	CLASS DEVICE		**	**	**	V33	VV	77M	00MM 00
STANDARD		FORMS		*****	*****	*****	33	VV	77MM	00MM 00
							3333VV	VV	77 MM	00MM 00
							3333	VVVV	77 MM	00MM 00
							33	VV	77 MM	00MM 00
							33		77	00 00
							33		77	00 00
							3333333333		77	0000000000
							3333333333		77	00000000

DANE ARCHER  
 STEVENSON COLLEGE  
 UNIVERSITY OF CALIFORNIA  
 SANTA CRUZ, CALIF. 95064

TAG DATA: FILE (2379) ORIGIN UCSCC daemon 9/03/86 16:11:12 P.D.T.

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```

"Skip" (sample)  
 formatted data and  
 the Fortran program  
 used to format the  
 data in the raw  
 ("Eight") file.

\*\*\*\*\*  
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From VS1@UCSCVM.BITNET Tue Sep 2 15:32:04 1986

Received: by ucsc (5.9/4.7)

Id AA11442; Tue, 2 Sep 86 15:28:41 PDT

From: VS1@UCSCVM.BITNET

Message-Id: <8609022228.AA11442@ucsc>

To: archer@UCSCVM.BITNET

Date: Tue Sep 2 15:14:13 1986

Subject: TPEPRINT

Status: R

//TPEPRINT JOB 21048,ARCHER,CLASS=D,MSGCLASS=Q  
 LOG >>>> TPEPRINT - 21048,ARCHER -D- - 15:12 ARCHER  
 LOG IEF233A M 4CO,SC5820,,TPEPRINT,GENER,NEW.SKIP

LOG Job TPEPRINT,GENER(IEBGENER) #001 complete 0000

LOG IEF280E K 4CO,SC5820,TPEPRINT,GENER

\*\*\*EB UCSCYM VS1 21048 A 00000000 UCSCC ARCHER

//STEP1 EXEC GENER,SYSOUT=Q

XXGENER PROC SYSOUT='A' 00000010

XXGENER EXEC PGM=IEBGENER,REGION=50K 00000020

XXSYSPRINT DD SYSOUT=ASYSOUT 00000030

IEF6531 SUBSTITUTION JCL - SYSOUT=Q

//SYSIN DD DUMMY

X/SYSIN DD DUMMY 00000040

//SYSUT1 DD DSN=NEW.SKIP,

// UNIT=TAPE.

// VOL=(,RETAIN,SER=SC5820).

// DISP=OLD,

// LABEL=(4,SL)

//SYSUT2 DD SYSOUT=Q

//

IEF236I ALLOC. FOR TPEPRINT GENER STEP1

IEF237I 4CO ALLOCATED TO SYSUT1

IEF142I - STEP WAS EXECUTED - COND CODE 0000

IEF285I NEW.SKIP KEPT

IEF285I VOL SER NOS= SC5820.

IEF373I STEP /GENER / START 86245.1512

IEF374I STEP /GENER / STOP 86245.1514 CPU OMIN 03.93SEC STOR VIRT 162K

Device type	Address	I/O count	Sysout DD name	Lines	Class	Copies
Dummy	000	0	SYSPRINT	4	Q	
Tape	400	106	SYSUT2	6,279	Q	

----- Total sysin count 0. Partition size 640K; Memory used 162K.

IEF280E K 4CO,SC5820,TPEPRINT,GENER

IEF298I TPEPRINT SYSOUT=Q

IEF375I JOB /TPEPRINT/ START 86245.1512

IEF376I JOB /TPEPRINT/ STOP 86245.1514 CPU OMIN 03.93SEC

Job started Sep 02,1986 Tue 15:12:48

CPU id-014561

Estimated job cost (does not include tape mounts or JCL printing):

CPU time	\$ .48	00:00:03.93 at \$450. per hour
I/O requests	\$ .12	106 at \$1.20 per 1000
Lines printed	\$7.22	6,283 at \$1.15 per 1000

-----  
 Total \$7.83 (\$ .35 minimum).



IEB352I WARNING : OUTPUT RECFM/LRECL/BLKSIZE COPIED FROM INPUT

PROCESSING ENDED AT EOD

C\*\*\*\*\*  
C THIS PROGRAM IS FOR OUTPUT OF CRIME RATES AND RAW CRIME FIGURES, SKIPPING A  
C LINE AT THE END OF EACH DECADE. THE FOLLOWING IS A KEY TO SOME OF THE  
C VARIABLES IN THE PROGRAM:  
C  
C "IBLANK" IS A MATRIX THAT CONTAINS PRINT FORMATTING INFORMATION FOR BLANK  
C SPACES WHEN CRIME DATA IS NON-EXISTENT.  
C "IFORMT" IS A MATRIX THAT CONTAINS PRINT FORMATTING INFORMATION FOR  
C EXISTING CRIME DATA.  
C "IFRMT" IS A MATRIX THAT CONTAINS PRINT FORMATTING INFORMATION FOR EACH DATA  
C YEAR. IT CONSISTS OF PIECES FROM "IFORMT" AND "IBLANK" AND CHANGES FOR  
C EACH YEAR PRINTED.  
C "JCHECK" IS A VARIABLE THAT INDICATES WHETHER THE DATA IS BEING PRINTED ON  
C THE FIRST OR SECOND PAGE FOR EACH COUNTRY.  
C "JCNTY" IS THE COUNTRY CODE NUMBER.  
C "JCOUNT" IS A VARIABLE THAT COUNTS THE NUMBER OF LINES PRINTED ON EACH PAGE.  
C "JCRIME(I)" IS A SEVEN-DIMENSIONED VARIABLE FOR EACH RAW CRIME FIGURE.  
C "JKL" IS A BLANK VARIABLE FED INTO EACH "JCRIME(I)" FOR WHICH NO DATA IS  
C AVAILABLE.  
C "JPOP" IS EACH YEAR'S POPULATION.  
C "JYEAR" IS EACH YEAR.  
C "K" IS THE NUMBER OF FOOTNOTES FOR EACH COUNTRY.  
C "L" INDICATES THE TYPE OF CRIME STATISTICS.  
C "LASTY" IS THE LAST YEAR OF DATA FOR EACH COUNTRY.  
C "N(I)" IS A SEVEN-DIMENSIONED VARIABLE THAT CONTAINS THE FOOTNOTE NUMBERS  
C FOR EACH CRIME AS THEY APPEAR ON THE PRINTOUT.  
C "NCON(I,J)" IS A MATRIX CONTAINING THE TABLE OF CONTENTS.  
C "RCRIME(I)" IS A SEVEN-DIMENSIONED VARIABLE FOR EACH CRIME RATE.  
C "RKL" IS A BLANK VARIABLE FED INTO EACH "RCRIME(I)" FOR WHICH NO DATA IS  
C AVAILABLE.  
C\*\*\*\*\*

```

1      DIMENSION IFORMT (4,16),IFRMT(4,16),IBLANK(4,16)
2      DIMENSION N(7),JCRIME(7),RCRIME(7)
3      DIMENSION NCON(168,8)
4      DATA JKL/' ',' '
5      DATA RKL/' ',' '
6      READ (5,600) IFRMT
7      600      FORMAT (4A4)
8      READ(5,610) IBLANK
9      610      FORMAT (4A4)
10     DO 620 I=1,4
11         IFORMT (I,1)=IFORMT (I,1)
12         IFORMT (I,16)=IFORMT(I,16)
13     620      CONTINUE
14     WRITE (6,700)
15     700      FORMAT (1H1)
16     DO 300 I=1,12
17         WRITE (6,299)
18     299      FORMAT (2H0 )
19     300      CONTINUE
20     WRITE (6,305)
21     305      FORMAT (52X,27HCOMPARATIVE CRIME DATA FILE)
22     WRITE (6,310)
23     310      FORMAT (1H0,60X,9HJUNE 1976)
24     DO 320 I=1,5
25         WRITE (6,315)
26     315      FORMAT (2H0 )
27     320      CONTINUE
28     WRITE (6,325)
29     325      FORMAT(50X,32HDANE ARCHER AND ROSEMARY GARTNER)

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30      WRITE (6,330)
31 330   FORMAT (1H0,54X,23HDEPARTMENT OF SOCIOLOGY)
32      WRITE (6,335)
33 335   FORMAT(1H0,54X,24HUNIVERSITY OF CALIFORNIA)
34      WRITE (6,340)
35 340   FORMAT (1H0,55X,22HSANTA CRUZ, CALIFORNIA)
36      WRITE(6,345)
37 345   FORMAT(1H0,64X,5H95064)
38      READ (5,2) LCNTRY
39 2      FORMAT (I3)
C *****
C THIS PART OF THE PROGRAM (BETWEEN ASTERISKS) IS FOR TABLE OF CONTENTS.
40      DO 1090 I = 1,168
41      READ (5,1089) (NCON(I,J),J=1,8)
42 1089   FORMAT (8A4)
43 1090   CONTINUE
44      WRITE (6,1012)
45 1012   FORMAT ('H1')
46      WRITE (6,9099)
47 9099   FORMAT (1H1)
48      WRITE (6,1025)
49 1025   FORMAT (1H,50X,'TABLE OF CONTENTS')
50      WRITE (6,1015)
51 1015   FORMAT (1H0,'COUNTRIES',70X,'CITIES')
52      WRITE (6,999)
53 999    FORMAT (1H+,'_____',79X,'____')
54      WRITE (6,1016)
55 1016   FORMAT (1H)
56      DO 1014 KCON = 1,56
57      WRITE (6,1013) (NCON(KCON,J),J=1,8),(NCON(KCON+56,J),J=1,8),
1 (NCON(KCON+112,J),J=1,8)
58 1013   FORMAT (1H,8A4,12X,8A4,12X,8A4)
59 1014   CONTINUE
C *****
60 3      WRITE (6,12345)
61 12345  FORMAT (1H1)
62      WRITE(6,4)
63 4      FORMAT (1H0,130(1H-))
64      JCHECK = 5
65      READ (9,5)
66 5      FORMAT (1H0,40H
COUNTRY)
67 6      WRITE (6,5)
68      IF (JCHECK-1) 8,10,8
69 8      READ (9,9) L,(N(I), I=1,7),LASTY,K
70 9      FORMAT (I1,7A1,2I2)
71      LASTY=LASTY+1900
72 10     IF (L) 1000,50,11
73 11     IF (L-1) 1000,20,12
74 12     IF (L-2) 1000,30,15
75 15     IF (L-3) 1000,40,1000
76 20     WRITE(6,25)
77 25     FORMAT (1H+,40X,43HNUMBER OF CONVICTIONS -- (RATE PER 100,000))
78      GOTO 60
79 30     WRITE (6,35)
80 35     FORMAT(1H+,37X,
1 49HNUMBER OF OFFENSES REPORTED -- (RATE PER 100,000))
81      GOTO 60
82 40     WRITE (6,45)
83 45     FORMAT(1H+,37X,
1 49HNUMBER OF OFFENSES REPORTED -- (RATE PER 100,000))
84      GOTO 60

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85 50 WRITE (6,55)
86 55 FORMAT (1H+.41X,38HCRIME STATISTICS -- (RATE PER 100,000))
87 60 IF (JCHECK-1) 88,65,88
88 65 WRITE (6,70)
89 70 FORMAT (1H+.95X,9HCONTINUED)
90 88 WRITE (6,90) (N(I);I = 1,7)
91 90 FORMAT (26H0 YEAR MURDER (.A1,13H) MANSLTR (,
1 A1,13H) HOMICIDE (.A1,14H) RAPE (.A1,16H) ASSAULT (,
2 A1,16H) ROBBERY (.A1,17H) THEFT (.A1,
3 9H) POP)
92 JCOUNT = 0
93 WRITE (6,95)
94 95 FORMAT (1H )
95 IF (JCHECK-1) 98,132,98
96 98 READ (9,100) JCNTRY,JYEAR,(JCRIME(I),I=1,7),POP
97 100 FORMAT (13,12,2X,4I6,2I7,18,F6.0)
98 12839 IF (JCHECK-5) 105,102,1000
99 102 JYEAR=JYEAR+1900
100 JCHECK=0
101 105 JCOUNT=JCOUNT+1
102 JX=JYEAR/10
103 IF (JYEAR-(10*JX).NE.0) GO TO 107
104 WRITE (6,201)
105 201 FORMAT(1H )
106 JCOUNT=JCOUNT+1
107 107 IF (JCOUNT-51) 132,131,131
108 131 JCHECK = 1
109 WRITE (6,12355)
110 12355 FORMAT (1H1)
111 WRITE (6,12365)
112 12365 FORMAT (1H0)
113 GOTO 6
114 132 IF (JYEAR) 133,135,135
115 133 WRITE (6,134)
116 134 FORMAT (8H GAP)
117 GOTO 98
118 135 JYEAR= JYEAR +1900
119 202 IF(POP+. ) 1010,138,140
120 138 WRITE (6,139) JYEAR
121 139 FORMAT (1H .4X,14)
122 GOTO 98
123 140 DO 200 I=1,7
124 ICOL=2*I
125 IF (JCRIME(I)+1) 1010,160,150
126 150 RCRIME(1)=FLOAT(JCRIME(I))/(POP/100.)
127 DO 155 J=1,4
128 IFORMT(J,ICOL)=IFORMT(J,ICOL)
129 IFORMT(J,ICOL+1)=IFORMT(J,ICOL+1)
130 155 CONTINUE
131 GOTO 200
132 160 DO 170 J=1,4
133 IFORMT(J,ICOL)=IBLANK(J,ICOL)
134 IFORMT(J,ICOL+1)= IBLANK (J,ICOL+1)
135 170 CONTINUE
136 JCRIME (1)=JKL
137 RCRIME(1)=RKL
138 200 CONTINUE
139 JPOP=IFIX(POP)
140 WRITE(6,IFORMT) JYEAR,(JCRIME(I),RCRIME(I),I=1,7),JPOP
141 218 IF (LASTY-JYEAR) 1000,220,98
142 220 IF (K) 1000,270,223

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143 223 JCOUNT = JCOUNT + 2
144     IF (JCOUNT-51) 230,3001,3001
145 3001 WRITE (6,3002)
146 3002 FORMAT (1H1)
147     WRITE (6,3003)
148 3003 FORMAT (1H0)
149     JCOUNT = 0
150 230  WRITE (6,240)
151 240  FORMAT (11HOF00TNOTES:)
152     DD 260 1*1,K
153     READ (9,250)
154 250  FORMAT(10X,65H
1      NOTE)
155     JCOUNT = JCOUNT + 1
156     IF (JCOUNT-51) 260,2001,2001
157 2001  WRITE (6,2002)
158 2002  FORMAT (1H1)
159     WRITE (6,2003)
160 2003  FORMAT (1H0)
161     WRITE (6,2004)
162 2004  FORMAT (1H )
163     JCOUNT = 0
164 260  WRITE (6,250)
165 270  IF (JCNTY-LCNTY) 3,1000,1000
166 1000  WRITE (6,1005)
167 1005  FORMAT (1H1)
168 1010  STOP
169     END

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\$ENTRY

COMPARATIVE CRIME DATA FILE

JUNE 1976

DANE ARCHER AND ROSEMARY GARTNER

DEPARTMENT OF SOCIOLOGY

UNIVERSITY OF CALIFORNIA

SANTA CRUZ, CALIFORNIA

95064

## TABLE OF CONTENTS

COUNTRIES		CITIES
ADEN	LEBANON	ACCRA, GHANA
ARGENTINA	LIBYA	AMSTERDAM, NETHERLANDS
AUSTRALIA	LUXEMBOURG	ATHENS, GREECE
AUSTRIA	MADAGASCAR	BEIRUT, LEBANON
BAHRAIN	MALAWI (NYASALAND)	BELFAST, NORTHERN IRELAND
BELGIUM	MALAYA	BOMBAY, INDIA
BERMUDA	MAURITANIA	BRUSSELS, BELGIUM
BOLIVIA	MAURITIUS	CALCUTTA, INDIA
BOTSWANA	MEXICO	CARACAS, VENEZUELA
BRUNEI	MONACO	COLOMBO CITY, SRI LANKA
BULGARIA	MOROCCO	DAHO CITY, QATAR
BURMA	NEPAL	DUBLIN, IRELAND
CAMEROON	NETHERLANDS	FREETOWN, SIERRA LEONE
CANADA	NETHERLANDS ANTILLES	GEORGETOWN, GUYANA
CENTRAL AFRICAN REPUBLIC	NEW ZEALAND	GLASGOW, SCOTLAND
CHAD	NIGERIA	HELSINKI, FINLAND
CHILE	NORTHERN IRELAND	ISTANBUL, TURKEY
CHINA, REPUBLIC OF	NORWAY	JERUSALEM, ISRAEL
COLOMBIA	PAKISTAN	JOHANNESBURG, SOUTH AFRICA
CONGO (BRAZZAVILLE)	PANAMA	KHARTOUM, SUDAN
CUBA	PERU	KUWAIT CITY, KUWAIT
CYPRUS	PHILIPPINES	LAGOS CITY, NIGERIA
DAHOMEY	POLAND	MADRID, SPAIN
DENMARK	PORTUGAL	MANILA, PHILIPPINES
DOMINICAN REPUBLIC	PUERTO RICO	MEXICO CITY, MEXICO
EGYPT	QATAR	MONTEVIDEO, URUGUAY
EL SALVADOR	RHODESIA	MUNICH, GERMANY
ENGLAND (AND WALES)	RUMANIA	NAIROBI CITY, KENYA
ETHIOPIA	SCOTLAND	NEW YORK CITY, UNITED STATES
FIJI	SENEGAL	OSLO, NORWAY
FINLAND	SIERRA LEONE	PANAMA CITY, PANAMA
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GREECE	SPAIN	SALISBURY, RHODESIA
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HONG KONG	SURINAM	SYDNEY, AUSTRALIA
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ICELAND	SWEDEN	TOKYO, JAPAN
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IRAN	TANGIERS	WELLINGTON, NEW ZEALAND
IRAQ	TANZANIA	ZURICH, SWITZERLAND
IRELAND (EIRE)	THAILAND	
ISRAEL	TRINIDAD AND TOBAGO	
ITALY	TUNISIA	
IVORY COAST	TURKEY	
JAMAICA	UGANDA	
JAPAN	UNITED STATES	
JORDAN	VENEZUELA	
KENYA	VIETNAM, SOUTH	
KHMER REPUBLIC (CAMBODIA)	WEST INDIES (BRITISH)	
KOREA	YUGOSLAVIA	
KUWAIT	ZAMBIA	
LAOS		

ADEN: \*

## NUMBER OF OFFENSES REPORTED \*\* (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1960	8 ( 3.88)			67 ( 32.52)		925 ( 449.03)		206
1961	13 ( 6.19)			77 ( 36.67)		275 ( 130.95)	782 ( 372.38)	210
1962								
1963	18 ( 8.00)			117 ( 52.00)		295 ( 131.11)	616 ( 273.78)	225
1964	46 ( 20.00)			90 ( 39.13)		344 ( 149.57)	975 ( 423.91)	230
1965	153 ( 64.56)			78 ( 32.91)		411 ( 173.42)	797 ( 336.29)	237
1966	354 (141.60)			67 ( 26.80)		376 ( 150.40)	730 ( 292.00)	250

## FOOTNOTES:

\* BRITISH COLONY IN SOUTH YEMEN UNTIL 1967; RAW CRIME DATA  
FROM INTERPOL, RATES COMPUTED FROM POPULATION FIGURES ABOVE  
1. SEX OFFENCES



ARGENTINA *		NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)						
YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE (1)	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1965	828 ( 3.85)			1087 ( 5.06)		35622 ( 165.76)	75785 ( 352.65)	21490
1966	675 ( 3.09)			1208 ( 5.54)		30013 ( 137.55)	68103 ( 312.11)	21820
FOOTNOTES:								
* RAW CRIME DATA FROM INTERPOL; RATES COMPUTED FROM POPULATION FIGURES ABOVE								
1. SEX OFFENSES								

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AUSTRALIA *		NUMBER OF CONVICTIONS ** (RATE PER 100,000)						POP
YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE (1)	RAPE (2)	ASSAULT (3)	ROBBERY (4)	THEFT (5)	
1903			51 ( 1.32)	90 ( 2.34)	244 ( 6.34)			3850
1904			58 ( 1.48)	97 ( 2.47)	225 ( 5.73)			3926
1905			46 ( 1.15)	96 ( 2.40)	256 ( 6.40)			4002
1906			56 ( 1.37)	70 ( 1.72)	239 ( 5.86)			4078
1907			45 ( 1.08)	90 ( 2.17)	255 ( 6.14)			4154
1908			46 ( 1.09)	60 ( 1.42)	278 ( 6.57)			4230
1909			45 ( 1.05)	59 ( 1.37)	260 ( 6.04)			4306
1910			46 ( 1.05)	77 ( 1.76)	250 ( 5.71)			4382
1911			33 ( 0.74)	97 ( 2.18)	235 ( 5.27)			4455
1912			49 ( 1.08)	88 ( 1.93)	221 ( 4.85)			4556
1913			52 ( 1.12)	71 ( 1.52)	298 ( 6.40)			4657
1914			57 ( 1.20)	67 ( 1.41)	300 ( 6.31)			4758
1915			40 ( 0.62)	14 ( 0.29)	226 ( 4.65)			4859
1916			35 ( 0.71)	15 ( 0.30)	193 ( 3.89)			4960
1917			30 ( 0.59)	15 ( 0.30)	239 ( 4.72)			5061
1918			21 ( 0.41)	11 ( 0.21)	155 ( 3.00)			5162
1919			48 ( 0.91)	3 ( 0.06)	220 ( 4.18)			5263
1920			38 ( 0.71)	7 ( 0.13)	223 ( 4.16)			5360
1921			46 ( 0.84)	8 ( 0.15)	235 ( 4.29)			5476
1922			46 ( 0.82)	5 ( 0.09)	240 ( 4.29)			5592
1923			30 ( 0.53)	9 ( 0.16)	246 ( 4.31)			5708
1924			34 ( 0.58)	5 ( 0.09)	217 ( 3.73)			5824
1925			41 ( 0.69)	8 ( 0.13)	253 ( 4.26)			5939
1926			37 ( 0.61)	15 ( 0.25)	235 ( 3.89)			6044
1927			45 ( 0.73)	14 ( 0.23)	224 ( 3.64)			6149
1928			42 ( 0.67)	11 ( 0.18)	222 ( 3.58)			6252
1929			48 ( 0.75)	15 ( 0.24)	244 ( 3.84)			6359
1930			29 ( 0.45)	14 ( 0.22)	224 ( 3.47)			6463
1931			51 ( 0.78)	9 ( 0.14)	211 ( 3.24)			6516
1932			32 ( 0.49)	14 ( 0.21)	217 ( 3.30)			6569
1933			36 ( 0.54)	16 ( 0.24)	216 ( 3.26)			6622
1934			32 ( 0.48)	8 ( 0.09)	191 ( 2.86)			6675
1935			40 ( 0.59)	10 ( 0.15)	188 ( 2.80)			6726
1936			34 ( 0.50)	14 ( 0.21)	182 ( 2.68)			6789
1937			38 ( 0.55)	20 ( 0.29)	227 ( 3.31)			6852
1938			37 ( 0.54)	9 ( 0.13)	60 ( 0.87)	85 ( 1.23)	754 ( 10.90)	6915
1939			34 ( 0.49)	10 ( 0.14)	64 ( 0.92)	117 ( 1.68)	988 ( 14.16)	6978
1940								
1941			33 ( 0.46)	28 ( 0.39)	46 ( 0.65)	100 ( 1.41)	697 ( 9.80)	7110
1942			41 ( 0.57)	14 ( 0.19)	74 ( 1.03)	134 ( 1.87)	748 ( 10.42)	7181
1943			37 ( 0.51)	24 ( 0.33)	62 ( 0.86)	148 ( 2.05)	858 ( 12.06)	7235
1944								
1945								
1946			37 ( 0.50)	25 ( 0.33)	130 ( 1.74)	110 ( 1.47)	1033 ( 13.84)	7465
1947			51 ( 0.67)	22 ( 0.29)	170 ( 2.24)	128 ( 1.69)	1027 ( 13.55)	7579
1948			59 ( 0.77)	13 ( 0.17)	173 ( 2.24)	104 ( 1.35)	1072 ( 13.91)	7709

AUSTRALIA *		NUMBER OF CONVICTIONS -- (RATE PER 100,000)					CONTINUED	
YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE (1)	RAPE (2)	ASSAULT (3)	ROBBERY (4)	THEFT (5)	POP
1949			60 ( 0.76)	10 ( 0.13)	175 ( 2.21)	138 ( 1.75)	1073 ( 13.57)	7908
1950			67 ( 0.82)	17 ( 0.21)	215 ( 2.63)	170 ( 2.08)	1103 ( 13.49)	8179
1951			67 ( 0.80)	19 ( 0.23)	203 ( 2.41)	176 ( 2.09)	1158 ( 13.75)	8422
1952			61 ( 0.71)	21 ( 0.24)	207 ( 2.40)	165 ( 1.91)	1457 ( 16.87)	8636
1953			83 ( 0.94)	22 ( 0.25)	318 ( 3.61)	137 ( 1.55)	1405 ( 15.94)	8815
1954			76 ( 0.85)	26 ( 0.29)	270 ( 3.00)	197 ( 2.19)	1333 ( 14.83)	8987
1955			83 ( 0.90)	17 ( 0.18)	271 ( 2.95)	158 ( 1.72)	1373 ( 14.92)	9201
1956			60 ( 0.64)	43 ( 0.46)	268 ( 2.84)	162 ( 1.72)	1805 ( 19.15)	9426
1957			79 ( 0.82)	13 ( 0.13)	241 ( 2.50)	184 ( 1.91)	2286 ( 23.71)	9640
1958			80 ( 0.81)	25 ( 0.25)	245 ( 2.49)	158 ( 1.61)	2533 ( 25.74)	9842
1959			93 ( 0.92)	29 ( 0.29)	212 ( 2.11)	188 ( 1.87)	2404 ( 23.91)	10056
1960			87 ( 0.85)	39 ( 0.38)	222 ( 2.16)	229 ( 2.23)	2727 ( 26.54)	10275
1961			90 ( 0.86)	70 ( 0.67)	254 ( 2.42)	223 ( 2.12)	3235 ( 30.79)	10508
1962			114 ( 1.06)	48 ( 0.45)	231 ( 2.16)	298 ( 2.78)	3026 ( 28.27)	10705
1963			95 ( 0.87)	83 ( 0.76)	185 ( 1.69)	135 ( 1.24)	3085 ( 28.35)	10916
1964	143 ( 1.28)	34 ( 0.31)	272 ( 2.44)	262 ( 2.35)	1924 ( 17.28)	592 ( 5.32)	39695 ( 356.46)	11136
1965	141 ( 1.24)	29 ( 0.25)	271 ( 2.38)	257 ( 2.26)	1895 ( 16.64)	730 ( 6.41)	46626 ( 409.36)	11390
1966	163 ( 1.41)	29 ( 0.25)	321 ( 2.77)	251 ( 2.16)	2227 ( 19.20)	992 ( 8.55)	56841 ( 490.01)	11600
1967	137 ( 1.16)	36 ( 0.31)	300 ( 2.54)	311 ( 2.64)	2158 ( 18.29)	960 ( 8.14)	19072 ( 161.63)	11800
1968	143 ( 1.19)	43 ( 0.36)	300 ( 2.50)	363 ( 3.02)	2508 ( 20.90)	1280 ( 10.67)	23562 ( 196.35)	12000
1969	157 ( 1.28)	33 ( 0.27)	280 ( 2.28)	364 ( 2.97)	2483 ( 20.25)	1599 ( 13.04)	25597 ( 208.78)	12260
1970	172 ( 1.37)	22 ( 0.18)	339 ( 2.71)	416 ( 3.33)	3282 ( 26.24)	1999 ( 15.98)	30591 ( 244.53)	12510
1971	185 ( 1.45)	39 ( 0.31)	343 ( 2.69)	578 ( 4.53)	3862 ( 30.27)	2818 ( 22.08)	33925 ( 265.87)	12760
1972	207 ( 1.60)	46 ( 0.35)	422 ( 3.26)	544 ( 4.20)	4285 ( 33.06)	3045 ( 23.50)	36976 ( 285.31)	12860

# FOOTNOTES:

\* CASES KNOWN AFTER 1963

1. MURDER, MANSLAUGHTER & ATTEMPTED MURDER
2. INCLUDES CRIMES OF LUST BEFORE 1916
3. OFFENSES AGAINST THE PERSON, 1903-1937;  
AGGRAVATED AND COMMON ASSAULT, 1938-1963;  
SERIOUS ASSAULT, 1964-1972
4. ROBBERY AND STEALING FROM THE PERSON
5. BREAKING AND ENTERING: 1967-1972 - EXCLUDES OFFENSES  
INVOLVING PROPERTY VALUED AT \$100 OR LESS; 1971 - EXCLUDES  
8,175 ATTEMPTED BREAKING AND ENTERING OFFENSES WHICH WERE  
REPORTED OR BECAME KNOWN; 1972 - EXCLUDES 8,282 ATTEMPTED  
BREAKING AND ENTERING OFFENSES WHICH WERE REPORTED OR  
BECAME KNOWN

## AUSTRIA

## CRIME STATISTICS -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT (1)	ROBBERY ( )	THEFT (2)	POP
1953	60 ( 0.86)	23 ( 0.33)		698 ( 10.04)	2830 ( 40.70)	250 ( 3.60)	20720 ( 297.96)	6954
1954	68 ( 0.98)	23 ( 0.33)		609 ( 8.74)	2753 ( 39.50)	272 ( 3.90)	20930 ( 300.33)	6969
1955	66 ( 0.95)	30 ( 0.43)		730 ( 10.47)	2888 ( 41.41)	311 ( 4.46)	22085 ( 316.68)	6974
1956	88 ( 1.28)	20 ( 0.28)		659 ( 9.44)	2584 ( 37.00)	292 ( 4.18)	24627 ( 352.67)	6983
1957	58 ( 0.84)	33 ( 0.47)		540 ( 7.72)	2907 ( 41.55)	338 ( 4.83)	28287 ( 404.27)	6997
1958	53 ( 0.75)	14 ( 0.20)		640 ( 9.12)	2914 ( 41.50)	412 ( 5.87)	28569 ( 406.91)	7021
1959	62 ( 0.88)	30 ( 0.43)		683 ( 9.69)	2855 ( 40.50)	353 ( 5.01)	29554 ( 419.27)	7049
1960	55 ( 0.78)	28 ( 0.35)		671 ( 9.52)	2856 ( 40.52)	362 ( 5.14)	30569 ( 433.73)	7048
1961	45 ( 0.63)	25 ( 0.35)		641 ( 9.04)	2424 ( 34.20)	383 ( 5.40)	32261 ( 455.21)	7087
1962	54 ( 0.76)	20 ( 0.28)		709 ( 9.94)	2566 ( 35.99)	367 ( 5.15)	36618 ( 513.58)	7130
1963	47 ( 0.66)	20 ( 0.28)		600 ( 8.37)	2761 ( 38.50)	378 ( 5.27)	38618 ( 538.46)	7172
1964	44 ( 0.61)	22 ( 0.30)		669 ( 9.27)	2695 ( 37.33)	405 ( 5.61)	40180 ( 556.51)	7220
1965	45 ( 0.62)	29 ( 0.40)		604 ( 8.33)	2648 ( 36.52)	433 ( 5.97)	45059 ( 621.50)	7250
1966	49 ( 0.67)	14 ( 0.19)		582 ( 7.98)	2667 ( 36.58)	461 ( 6.32)	50667 ( 695.02)	7290
1967	53 ( 0.72)	24 ( 0.33)		583 ( 7.96)	2954 ( 40.36)	597 ( 8.16)	55841 ( 762.85)	7320
1968	48 ( 0.61)	10 ( 0.14)		600 ( 8.16)	2845 ( 38.71)	520 ( 7.07)	62873 ( 855.41)	7350
1969	52 ( 0.71)	30 ( 0.41)		659 ( 8.94)	2923 ( 39.66)	550 ( 7.46)	65496 ( 888.68)	7370
1970	70 ( 0.95)	35 ( 0.47)		673 ( 9.11)	3178 ( 43.00)	549 ( 7.43)	67676 ( 915.78)	7390
1971	69 ( 0.92)	36 ( 0.48)		625 ( 8.38)	2875 ( 38.54)	684 ( 9.17)	67649 ( 906.82)	7460
1972	63 ( 0.84)	40 ( 0.53)		679 ( 9.07)	3136 ( 41.87)	761 ( 10.16)	84127 ( 1123.19)	7490
1973	70 ( 0.93)	34 ( 0.46)		630 ( 8.38)	3132 ( 41.64)	825 ( 10.97)	81687 ( 1085.97)	7522

## FOOTNOTES:

1. SERIOUS BODILY INJURY
2. AGGRAVATED THEFT

## BAHRAIN

NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE (1)	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1963			2 ( 1.18)	11 ( 6.47)	10 ( 5.88)	20 ( 11.76)	0 ( 0.00)	170
1964			4 ( 2.26)	8 ( 4.52)	6 ( 3.39)	13 ( 7.34)	0 ( 0.00)	177
1965			3 ( 1.62)	5 ( 2.70)	8 ( 4.32)	22 ( 11.89)	0 ( 0.00)	185
1966			2 ( 1.04)	5 ( 2.69)	7 ( 3.63)	16 ( 8.29)	2 ( 1.04)	193
1967			1 ( 0.53)	6 ( 3.16)	4 ( 2.11)	14 ( 7.37)	1 ( 0.53)	190
1968			5 ( 2.50)	1 ( 0.50)	3 ( 1.50)	7 ( 3.50)	1 ( 0.50)	200
1969			2 ( 0.95)	4 ( 1.90)	4 ( 1.90)	15 ( 7.14)	2 ( 0.95)	210
1970			2 ( 0.91)	1 ( 0.45)	3 ( 1.36)	14 ( 6.36)	3 ( 1.36)	220
1971			2 ( 0.91)	3 ( 1.36)	4 ( 1.82)	16 ( 7.27)	4 ( 1.82)	220
1972			1 ( 0.45)	0 ( 0.00)	8 ( 3.64)	16 ( 7.27)	2 ( 0.91)	220
1973			2 ( 0.88)	5 ( 2.20)	4 ( 1.76)	28 ( 12.33)	0 ( 0.00)	227

## FOOTNOTES:

1. MURDER AND MANSLAUGHTER

BELGIUM								
CRIME STATISTICS ** (RATE PER 100,000)								
YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE (1)	ASSAULT ( )	ROBBERY ( )	THEFT (2)	POP
1909	57 ( 0.77)				25725 ( 347.17)		11937 ( 161.09)	7410
1910	40 ( 0.54)				27796 ( 374.46)		11534 ( 155.38)	7423
1911	29 ( 0.39)				26583 ( 357.49)		10928 ( 146.96)	7436
1912	55 ( 0.74)				27696 ( 371.81)		12239 ( 164.30)	7449
1913	44 ( 0.59)				27134 ( 363.63)		12234 ( 163.95)	7462
1914								
GAP								
1919	110 ( 1.48)				5173 ( 69.81)		19700 ( 265.86)	7410
1920	93 ( 1.26)				9427 ( 127.31)		15899 ( 214.71)	7405
1921								
GAP								
1941			11 ( 0.13)	294 ( 3.55)			15689 ( 189.62)	8274
1942			15 ( 0.18)	311 ( 3.77)			22440 ( 272.13)	8246
1943			28 ( 0.34)	308 ( 3.74)			24018 ( 291.45)	8241
1944			5 ( 0.06)	254 ( 3.06)			12851 ( 155.00)	8291
1945			33 ( 0.40)	259 ( 3.11)			5844 ( 70.08)	8339
1946			37 ( 0.44)	399 ( 4.77)			7520 ( 89.88)	8367
1947			45 ( 0.53)	451 ( 5.34)			6981 ( 82.62)	8450
1948			49 ( 0.57)	562 ( 6.57)			7942 ( 92.81)	8557
1949			41 ( 0.48)	783 ( 9.09)			6884 ( 79.92)	8614
1950			29 ( 0.34)	775 ( 8.97)			4966 ( 57.48)	8639
1951			22 ( 0.25)	661 ( 7.62)			5653 ( 65.14)	8678
1952			27 ( 0.31)	747 ( 8.56)			6932 ( 79.40)	8730
1953			20 ( 0.23)	749 ( 8.53)			5300 ( 60.38)	8778
1954			11 ( 0.12)	705 ( 7.99)			4438 ( 50.32)	8819
1955			18 ( 0.20)	713 ( 8.04)			4266 ( 48.11)	8868
1956			14 ( 0.16)	806 ( 9.03)			4860 ( 54.46)	8924
1957			15 ( 0.17)	857 ( 9.53)			4986 ( 55.47)	8989
1958			17 ( 0.19)	825 ( 9.11)			4974 ( 54.94)	9053
1959			22 ( 0.24)	1009 ( 11.08)			4977 ( 54.67)	9104
1960			21 ( 0.23)	1130 ( 12.35)			5558 ( 60.72)	9159
1961			11 ( 0.12)	1114 ( 12.13)			5667 ( 61.71)	9184
1962			18 ( 0.20)	1141 ( 12.37)			5714 ( 61.97)	9221
1963			28 ( 0.30)	1051 ( 11.31)			5847 ( 62.94)	9290
1964			16 ( 0.17)	993 ( 10.59)			5980 ( 63.77)	9378
1965			24 ( 0.25)	990 ( 10.47)			5577 ( 58.95)	9460
1966			26 ( 0.27)	866 ( 9.09)			6220 ( 65.27)	9530
1967			27 ( 0.28)	1049 ( 10.95)			7158 ( 74.72)	9580
1968			27 ( 0.28)	1006 ( 10.46)			7087 ( 73.67)	9620
1969			34 ( 0.35)	908 ( 9.41)			6274 ( 65.02)	9650

FOOTNOTES:

1. RAPE AND CRIMES AGAINST MODESTY
2. ROBBERY AND THEFT



BERMUDA								
NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)								
YEAR	MURDER (1)	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT (2)	ROBBERY ( )	THEFT ( )	POP
1941	1 ( 3.13)	1 ( 3.13)		4 ( 12.50)	106 ( 331.25)	4 ( 12.50)	393 (1228.13)	32
1942	1 ( 3.03)	0 ( 0.00)		1 ( 3.03)	133 ( 403.03)	2 ( 6.06)	415 (1257.58)	33
1943	1 ( 3.03)	1 ( 3.03)		0 ( 0.00)	126 ( 381.82)	5 ( 15.15)	335 (1015.15)	33
1944	0 ( 0.00)	1 ( 2.94)		0 ( 0.00)	138 ( 405.88)	3 ( 8.82)	365 (1073.53)	34
1945	2 ( 5.71)	1 ( 2.86)		3 ( 8.57)	98 ( 280.00)	6 ( 17.14)	409 (1168.57)	35
1946	0 ( 0.00)	1 ( 2.86)		2 ( 5.71)	113 ( 322.86)	2 ( 5.71)	419 (1197.14)	35
1947	1 ( 2.86)	1 ( 2.86)		1 ( 2.86)	98 ( 280.00)	9 ( 25.71)	581 (1660.00)	35
1948	0 ( 0.00)	0 ( 0.00)		13 ( 36.11)	85 ( 236.11)	4 ( 11.11)	598 (1661.11)	36
1949	2 ( 5.56)	0 ( 0.00)		7 ( 19.44)	91 ( 252.78)	3 ( 8.33)	515 (1430.56)	36
1950	1 ( 2.70)	2 ( 5.41)		4 ( 10.81)	82 ( 221.62)	2 ( 5.41)	503 (1359.46)	37
1951	0 ( 0.00)	1 ( 2.63)		7 ( 18.42)	101 ( 265.79)	5 ( 13.16)	552 (1452.63)	38
1952								
1953	1 ( 2.56)	1 ( 2.56)		4 ( 10.26)	60 ( 153.85)	13 ( 33.33)	752 (1928.21)	39
1954	3 ( 7.50)	2 ( 5.00)		0 ( 0.00)	71 ( 177.50)	12 ( 30.00)	736 (1950.00)	40
1955	0 ( 0.00)	0 ( 0.00)		3 ( 7.50)	78 ( 195.00)	13 ( 32.50)	832 (2080.00)	40
1956	2 ( 4.88)	1 ( 2.44)		5 ( 12.20)	89 ( 217.07)	15 ( 36.59)	1109 (2704.88)	41
1957	0 ( 0.00)	1 ( 2.38)		1 ( 2.38)	113 ( 269.05)	9 ( 21.43)	896 (2133.33)	42
1958	0 ( 0.00)	2 ( 4.65)		5 ( 11.63)	96 ( 223.26)	11 ( 25.58)	1910 (4441.86)	43
1959	7 ( 16.28)	1 ( 2.33)		4 ( 9.30)	134 ( 311.63)	11 ( 25.58)	1757 (4086.05)	43
1960	2 ( 4.55)	1 ( 2.27)		3 ( 6.82)	100 ( 227.27)	14 ( 31.82)	2558 (5813.63)	44
1961	1 ( 2.22)	2 ( 4.44)		2 ( 4.44)	97 ( 215.56)	15 ( 33.33)	2518 (5595.56)	45
1962	1 ( 2.17)	0 ( 0.00)		2 ( 4.35)	127 ( 276.09)	21 ( 45.65)	2909 (6323.91)	46
1963	0 ( 0.00)	0 ( 0.00)		7 ( 14.89)	124 ( 263.83)	22 ( 46.81)	3059 (6508.51)	47
1964	2 ( 4.17)	1 ( 2.08)		5 ( 10.42)	132 ( 275.00)	22 ( 45.83)	2800 (5833.33)	48
1965	0 ( 0.00)	0 ( 0.00)		3 ( 6.00)	121 ( 242.00)	18 ( 36.00)	3416 (6832.00)	50
1966	1 ( 2.00)	3 ( 6.00)		3 ( 6.00)	147 ( 294.00)	12 ( 24.00)	3305 (6610.00)	50
1967	2 ( 4.00)	0 ( 0.00)		5 ( 10.00)	176 ( 352.00)	16 ( 32.00)	3759 (7518.00)	50
1968	2 ( 4.00)	4 ( 8.00)		2 ( 4.00)	55 ( 110.00)	11 ( 22.00)	2509 (5018.00)	50
1969	1 ( 2.00)	1 ( 2.00)		4 ( 8.00)	98 ( 196.00)	11 ( 22.00)	3092 (6184.00)	50
1970								
1971	3 ( 6.00)	0 ( 0.00)		5 ( 10.00)	156 ( 312.00)	26 ( 52.00)	2120 (4240.00)	50
1972	2 ( 3.92)	0 ( 0.00)		3 ( 5.88)	155 ( 303.92)	17 ( 33.33)	1769 (3468.63)	51
1973	7 ( 13.46)	1 ( 1.92)		5 ( 9.62)	100 ( 192.31)	33 ( 63.46)	1837 (3532.69)	52
1974	5 ( 9.62)	1 ( 1.92)		5 ( 9.62)	112 ( 215.38)	39 ( 75.00)	2594 (4988.46)	52

FOOTNOTES:

1. MURDER AND ATTEMPTED MURDER AFTER 1953
2. INCLUDES ASSAULT AND SERIOUS ASSAULT, 1953-1961

BOLIVIA \*

CRIME STATISTICS -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1961	553 ( 15.80)			364 ( 10.40)			1868 ( 53.37)	3500

FOOTNOTES:

\* COEFFICIENT OF OFFENDERS; RAW CRIME DATA FROM INTERPOL.  
RATES COMPUTED FROM POPULATION FIGURES ABOVE



**BOTSWANA**

**NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)**

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT (1)	ROBBERY ( )	THEFT (2)	POP
1971	29 ( 4.33)	25 ( 3.73)		138 ( 20.60)	2600 ( 388.06)	38 ( 5.67)	4352 ( 649.55)	670

**FOOTNOTES:**

1. SERIOUS AND COMMON ASSAULT
2. BURGLARY, STEALING STOCK, AND OTHER THEFT

BRUNEI \*

## CRIME STATISTICS -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE (1)	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1964	0 ( 0.00)		0 ( 0.00)	1 ( 1.00)	68 ( 68.00)	0 ( 0.00)	122 ( 122.00)	100
1965	0 ( 0.00)		0 ( 0.00)	2 ( 2.00)	56 ( 56.00)	0 ( 0.00)	113 ( 113.00)	100
1966	1 ( 0.91)		0 ( 0.00)	3 ( 2.73)	62 ( 56.36)	0 ( 0.00)	185 ( 168.18)	110
1967	1 ( 0.91)		0 ( 0.00)	0 ( 0.00)	13 ( 11.82)	0 ( 0.00)	189 ( 171.82)	110
1968	0 ( 0.00)		0 ( 0.00)	1 ( 0.83)	31 ( 25.83)	0 ( 0.00)	166 ( 138.33)	120
1969	1 ( 0.83)		0 ( 0.00)	1 ( 0.83)	46 ( 38.33)	2 ( 1.67)	245 ( 204.17)	120
1970	2 ( 1.54)		0 ( 0.00)	0 ( 0.00)	250 ( 192.31)	4 ( 3.08)	284 ( 218.46)	130
1971	0 ( 0.00)		0 ( 0.00)	3 ( 2.14)	363 ( 259.29)	2 ( 1.43)	329 ( 235.00)	140
1972	0 ( 0.00)		0 ( 0.00)	0 ( 0.00)	316 ( 225.71)	4 ( 2.86)	335 ( 239.29)	140
1973	2 ( 1.40)		0 ( 0.00)	4 ( 2.80)	336 ( 234.97)	4 ( 2.80)	297 ( 207.69)	143
1974	2 ( 1.37)		0 ( 0.00)	3 ( 2.05)	175 ( 119.86)	9 ( 6.16)	254 ( 173.97)	146

## FOOTNOTES:

\* RAW CRIME DATA FROM INTERPOL: RATES COMPUTED FROM  
POPULATION FIGURES ABOVE  
1. CULPABLE HOMICIDE

BRUNEI \*

## CRIME STATISTICS -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE (1)	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1964	0 ( 0.00)		0 ( 0.00)	1 ( 1.00)	68 ( 68.00)	0 ( 0.00)	122 ( 122.00)	100
1965	0 ( 0.00)		0 ( 0.00)	2 ( 2.00)	56 ( 56.00)	0 ( 0.00)	113 ( 113.00)	100
1966	1 ( 0.91)		0 ( 0.00)	3 ( 2.73)	62 ( 56.36)	0 ( 0.00)	185 ( 168.18)	110
1967	1 ( 0.91)		0 ( 0.00)	0 ( 0.00)	13 ( 11.82)	0 ( 0.00)	189 ( 171.82)	110
1968	0 ( 0.00)		0 ( 0.00)	1 ( 0.83)	31 ( 25.83)	0 ( 0.00)	166 ( 138.33)	120
1969	1 ( 0.83)		0 ( 0.00)	1 ( 0.83)	46 ( 38.33)	2 ( 1.67)	245 ( 204.17)	120
1970	2 ( 1.54)		0 ( 0.00)	0 ( 0.00)	250 ( 192.31)	4 ( 3.08)	284 ( 218.46)	130
1971	0 ( 0.00)		0 ( 0.00)	3 ( 2.14)	363 ( 259.29)	2 ( 1.43)	329 ( 235.00)	140
1972	0 ( 0.00)		0 ( 0.00)	0 ( 0.00)	316 ( 225.71)	4 ( 2.86)	335 ( 239.29)	140
1973	2 ( 1.40)		0 ( 0.00)	4 ( 2.80)	336 ( 234.97)	4 ( 2.80)	297 ( 207.69)	143
1974	2 ( 1.37)		0 ( 0.00)	3 ( 2.05)	175 ( 119.86)	9 ( 6.16)	254 ( 173.97)	146

## FOOTNOTES:

\* RAW CRIME DATA FROM INTERPOL; RATES COMPUTED FROM  
POPULATION FIGURES ABOVE  
1. CULPABLE HOMICIDE

## BURMA \*

## NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE (1)	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1953	11495 ( 58.32)			2207 ( 11.20)		38573 ( 195.70)	46164 ( 234.22)	19710
1954	9072 ( 45.27)			1944 ( 9.70)		31482 ( 157.10)	37613 ( 187.69)	20040
1955	7246 ( 35.54)			1737 ( 8.52)		25379 ( 124.47)	40899 ( 201.07)	20390
1956	12034 ( 58.05)			2504 ( 12.08)		36154 ( 174.40)	45860 ( 221.23)	20730
1957	2302 ( 10.89)			528 ( 2.50)		6320 ( 29.91)	9180 ( 43.45)	21130
1958	10256 ( 47.64)			2463 ( 11.44)		30133 ( 139.96)	43388 ( 201.52)	21530
1959	7031 ( 32.05)			3761 ( 17.14)		16661 ( 75.94)	34033 ( 155.12)	21940
1960	7879 ( 35.24)			3427 ( 15.33)		19566 ( 87.50)	33480 ( 149.73)	22360
1961	8232 ( 36.14)			3549 ( 15.58)		20307 ( 89.14)	34713 ( 152.38)	22780
1962	7400 ( 31.82)			3704 ( 15.93)		15992 ( 68.77)	37052 ( 159.34)	23253
1963	6774 ( 28.54)			3717 ( 15.66)		14968 ( 63.06)	41030 ( 172.87)	23735
1964	6792 ( 28.03)			3720 ( 15.35)		17712 ( 73.10)	45948 ( 189.64)	24229
1965	9120 ( 36.88)			5040 ( 20.38)		25368 ( 102.58)	64656 ( 261.45)	24730
1966	9975 ( 39.50)			4200 ( 16.63)		30600 ( 121.19)	68725 ( 272.18)	25250
1967	9698 ( 37.57)			3562 ( 13.80)		29432 ( 114.03)	88764 ( 343.91)	25810
1968	8505 ( 32.23)			6831 ( 25.88)		20061 ( 76.02)	75141 ( 284.73)	26390
1969	7755 ( 28.74)			3273 ( 12.13)		15098 ( 55.96)	60115 ( 222.81)	26980
1970	7252 ( 26.29)			3416 ( 12.39)		62804 ( 227.72)	11172 ( 40.51)	27580

## FOOTNOTES:

\* RAW CRIME DATA FROM INTERPOL; RATES COMPUTED FROM  
POPULATION FIGURES ABOVE  
1. SEX OFFENSES

## CAMEROON \*

## NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1965	256 ( 4.82)			49 ( 0.92)		183 ( 3.45)	355 ( 6.69)	5310
1966	146 ( 2.69)			50 ( 0.92)		217 ( 4.00)	267 ( 4.93)	5420
1967								
1968	30 ( 0.53)			35 ( 0.62)		160 ( 2.84)	255 ( 4.63)	5630
1969	119 ( 2.07)			138 ( 2.40)		512 ( 8.82)	2326 ( 40.52)	5740
1970	102 ( 1.75)			149 ( 2.55)		627 ( 10.74)	5624 ( 96.30)	5840

## FOOTNOTES:

\* RAW CRIME DATA FROM INTERPOL; RATES COMPUTED FROM  
POPULATION FIGURES ABOVE  
1. SEX OFFENSES

CANADA								
NUMBER OF CONVICTIONS -- (RATE PER 100,000)								
YEAR	MURDER (1)	MANSLTR (2)	HOMICIDE (3)	RAPE ( )	ASSAULT ( )	ROBBERY (4)	THEFT (5)	POP
1901	7 ( 0.13)	12 ( 0.22)			346 ( 6.44)	52 ( 0.97)	316 ( 5.88)	5371
1902	11 ( 0.20)	17 ( 0.31)			522 ( 9.40)	29 ( 0.52)	287 ( 5.17)	5555
1903	8 ( 0.14)	14 ( 0.24)			680 ( 11.85)	81 ( 1.41)	346 ( 6.03)	5739
1904	14 ( 0.24)	19 ( 0.32)			699 ( 11.80)	92 ( 1.55)	366 ( 6.18)	5923
1905	12 ( 0.20)	15 ( 0.25)			651 ( 10.66)	84 ( 1.38)	477 ( 7.81)	6107
1906	3 ( 0.05)	15 ( 0.24)			592 ( 9.41)	62 ( 0.99)	392 ( 6.23)	6291
1907	8 ( 0.12)	24 ( 0.37)			728 ( 11.24)	99 ( 1.53)	318 ( 4.91)	6475
1908	14 ( 0.21)	22 ( 0.33)			1179 ( 17.71)	101 ( 1.52)	507 ( 7.61)	6659
1909	18 ( 0.26)	24 ( 0.35)			1264 ( 18.47)	92 ( 1.34)	313 ( 4.57)	6843
1910								
GAP								
1919		60 ( 0.69)			1323 ( 15.31)	2300 ( 26.62)	8605 ( 99.61)	8639
1920		69 ( 0.78)			1389 ( 15.75)	1868 ( 21.18)	6559 ( 74.37)	8820
1921		56 ( 0.62)			1474 ( 16.44)	1966 ( 21.92)	5938 ( 66.21)	8968
1922		64 ( 0.70)			1451 ( 15.92)	1207 ( 13.24)	5865 ( 64.34)	9116
1923		53 ( 0.57)			1302 ( 14.05)	1704 ( 18.39)	6164 ( 66.54)	9264
1924		47 ( 0.50)			1440 ( 15.30)	1905 ( 20.24)	6610 ( 70.23)	9412
1925		55 ( 0.58)			1400 ( 14.65)	1624 ( 16.99)	6651 ( 69.58)	9559
1926		60 ( 0.62)	126 ( 1.29)		1743 ( 17.89)	1896 ( 19.46)	7108 ( 72.95)	9744
1927		51 ( 0.51)	124 ( 1.25)		1470 ( 14.81)	2167 ( 21.82)	7870 ( 79.26)	9928
1928		54 ( 0.53)	150 ( 1.48)		1923 ( 19.01)	2553 ( 25.24)	8777 ( 86.78)	10114
1929		85 ( 0.83)	182 ( 1.77)		2202 ( 21.38)	3696 ( 35.89)	10540 ( 102.34)	10299
1930		68 ( 0.65)	214 ( 2.04)		2268 ( 21.63)	4327 ( 41.27)	11610 ( 110.74)	10484
1931		77 ( 0.73)	172 ( 1.62)		2336 ( 22.01)	4267 ( 40.20)	11144 ( 104.99)	10614
1932		68 ( 0.63)	158 ( 1.47)		2144 ( 19.96)	4347 ( 40.46)	11257 ( 104.77)	10744
1933		63 ( 0.58)	147 ( 1.35)		2167 ( 19.93)	4238 ( 38.97)	10719 ( 98.57)	10874
1934		58 ( 0.53)	142 ( 1.29)		2471 ( 22.46)	4147 ( 37.69)	10603 ( 96.36)	11004
1935		56 ( 0.50)	153 ( 1.37)		2667 ( 23.95)	4841 ( 43.48)	11026 ( 99.02)	11135
1936		81 ( 0.72)	137 ( 1.22)		2553 ( 22.71)	4604 ( 40.95)	11905 ( 105.83)	11244
1937		57 ( 0.50)	138 ( 1.22)		2440 ( 21.49)	5509 ( 48.52)	14048 ( 123.74)	11353
1938		68 ( 0.59)	127 ( 1.11)		2639 ( 23.02)	6147 ( 53.63)	14763 ( 128.80)	11462
1939		70 ( 0.60)	124 ( 1.07)		2896 ( 25.89)	6418 ( 46.81)	13464 ( 116.36)	11571
1940		65 ( 0.56)	148 ( 1.27)		3632 ( 31.09)	4217 ( 36.10)	11632 ( 99.57)	11682
1941		60 ( 0.51)	130 ( 1.10)		3914 ( 33.11)	3920 ( 33.16)	11056 ( 93.52)	11822
1942		68 ( 0.57)	113 ( 0.94)		4301 ( 35.96)	4223 ( 35.80)	12158 ( 101.64)	11962
1943		44 ( 0.36)	125 ( 1.03)		4088 ( 33.76)	5291 ( 43.70)	12565 ( 103.77)	12108
1944		30 ( 0.24)	106 ( 0.86)		4183 ( 34.11)	5297 ( 43.20)	12280 ( 100.15)	12262
1945		59 ( 0.48)	152 ( 1.23)		4814 ( 38.84)	5783 ( 46.66)	12522 ( 101.03)	12394
1946		87 ( 0.69)	146 ( 1.16)		5994 ( 47.49)	5304 ( 42.02)	12172 ( 96.43)	12622
1947		73 ( 0.57)	146 ( 1.13)		5462 ( 42.38)	5541 ( 42.99)	11719 ( 90.93)	12888
1948		67 ( 0.51)	155 ( 1.18)		4929 ( 37.43)	5076 ( 38.55)	11217 ( 85.19)	13167
1949		77 ( 0.57)	172 ( 1.28)		4194 ( 31.19)	3952 ( 29.39)	9457 ( 70.33)	13447
1950		94 ( 0.69)	112 ( 0.82)		4153 ( 30.29)	4292 ( 31.30)	9162 ( 66.82)	13712
1951		107 ( 0.76)	137 ( 0.98)		3491 ( 24.92)	3883 ( 27.72)	9870 ( 70.45)	14009
1952		95 ( 0.66)	135 ( 0.94)		3874 ( 26.85)	4040 ( 28.00)	9777 ( 67.75)	14430

CANADA		NUMBER OF CONVICTIONS -- (RATE PER 100,000)				CONTINUED		
YEAR	MURDER (1)	MANSLTR (2)	HOMICIDE (3)	RAPE ( )	ASSAULT ( )	ROBBERY (4)	THEFT (5)	POP
1953		89 ( 0.60)	149 ( 1.01)		3864 ( 26.14)	4174 ( 28.24)	9001 ( 60.90)	14781
1954	125 ( 0.82)	96 ( 0.63)	157 ( 1.03)		3765 ( 24.78)	4743 ( 31.21)	8746 ( 57.56)	15195
1955	118 ( 0.76)	59 ( 0.38)	157 ( 1.01)		3483 ( 22.33)	4606 ( 29.52)	8229 ( 52.75)	15601
1956	131 ( 0.81)	83 ( 0.58)			3536 ( 21.83)	4580 ( 28.41)	9192 ( 57.01)	16123
1957	129 ( 0.77)	118 ( 0.71)			3638 ( 21.81)	5588 ( 33.51)	11497 ( 68.94)	16677
1958	153 ( 0.89)	98 ( 0.57)			3457 ( 20.19)	6896 ( 40.28)	12847 ( 75.04)	17120
1959	141 ( 0.80)	115 ( 0.66)			3180 ( 18.15)	6435 ( 36.73)	12660 ( 72.25)	17522
1960	190 ( 1.06)	112 ( 0.63)			3550 ( 19.82)	7642 ( 42.67)	14435 ( 80.60)	17909
1961	185 ( 1.01)	114 ( 0.62)			3772 ( 20.65)	7863 ( 43.04)	15456 ( 84.60)	18269
1962	217 ( 1.17)	164 ( 0.88)			3875 ( 21.37)	7776 ( 41.81)	15001 ( 80.65)	18600
1963	215 ( 1.14)	151 ( 0.80)			4077 ( 21.54)	8701 ( 45.97)	16765 ( 88.57)	18928
1964	218 ( 1.13)	137 ( 0.71)			4394 ( 22.84)	8399 ( 43.66)	16649 ( 86.55)	19237
1965	243 ( 1.23)	141 ( 0.72)			4579 ( 23.27)	7927 ( 40.28)	17972 ( 91.32)	19680
1966	221 ( 1.10)	142 ( 0.71)			5097 ( 25.42)	7965 ( 39.73)	20480 ( 102.14)	20050
1967	282 ( 1.38)	168 ( 0.82)			4587 ( 22.47)	8179 ( 40.07)	20264 ( 99.28)	20410
1968	315 ( 1.52)							20730
1969	346 ( 1.65)							21030
1970	433 ( 2.03)							21320
1971	427 ( 1.98)							21600
1972	479 ( 2.19)							21850
1973	474 ( 2.14)							22130

FOOTNOTES:

1. OFFENSES KNOWN, 1954-1973
2. MURDER AND MANSLAUGHTER FROM 1919
3. VITAL STATISTICS
4. ROBBERY AND BURGLARY AFTER 1909
5. BREAKING AND ENTERING BEFORE 1919



## CENTRAL AFRICAN REPUBLIC \*

NUMBER OF OFFENSES REPORTED -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE (1)	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1967	72 ( 4.80)			4 ( 0.27)		196 ( 13.07)	303 ( 20.20)	1500
1968	19 ( 1.23)			23 ( 1.49)		106 ( 6.88)	971 ( 63.05)	1540
1969	66 ( 4.18)			48 ( 3.04)		99 ( 6.27)	459 ( 29.05)	1580
1970	39 ( 2.42)			27 ( 1.68)		126 ( 7.83)	420 ( 26.09)	1610

## FOOTNOTES:

\* COEFFICIENT OF OFFENDERS IN 1967; RAW CRIME DATA FROM  
INTERPOL. RATES COMPUTED FROM POPULATION FIGURES ABOVE  
1. SEX OFFENSES



CHAD \*

NUMBER OF OFFENSES REPORTED \*\* (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE (1)	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1967	416 ( 12.13)			12 ( 0.35)		2104 ( 61.34)		3430
1968	452 ( 12.91)			8 ( 0.23)		1908 ( 54.51)		3500

## FOOTNOTES:

\* RAW CRIME DATA FROM INTERPOL; RATES COMPUTED FROM  
POPULATION FIGURES ABOVE  
1. SEX OFFENSES

CHILE CRIME STATISTICS -- (RATE PER 100,000)								
YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1900			855 (29.27)		290 ( 9.93)	2481 ( 84.94)		2921
1901			1002 (33.78)		310 ( 10.45)	2328 ( 78.52)		2966
1902			1032 (34.27)		362 ( 12.02)	2475 ( 82.20)		3011
1903			1068 (34.95)		331 ( 10.83)	2516 ( 82.33)		3056
1904			1040 (33.54)		331 ( 10.67)	2943 ( 94.90)		3101
1905			1040 (33.06)		317 ( 10.08)	715 ( 22.73)		3146
1906			1063 (33.31)		306 ( 9.59)	2308 ( 72.33)		3191
1907			941 (29.12)		216 ( 6.69)	1798 ( 55.65)		3231
1908			1177 (38.00)		365 ( 11.79)	1830 ( 59.09)		3097
1909			1252 (42.25)		260 ( 8.77)	2005 ( 67.67)		2963
1910			1252 (44.26)		366 ( 12.94)	1576 ( 55.71)		2829
1911			1246 (46.22)		284 ( 10.53)	1649 ( 61.16)		2696
1912			1275 (45.26)		298 ( 10.58)	1707 ( 60.60)		2817
1913			437 (14.87)		302 ( 10.28)	10 ( 0.34)		2938
1914			668 (21.84)		274 ( 8.96)	2106 ( 68.85)	4172 ( 136.38)	3059
1915			458 (14.40)		179 ( 5.63)	2743 ( 86.26)	6278 ( 197.42)	3180
1916			617 (18.69)		208 ( 6.30)	2226 ( 67.43)	5752 ( 174.25)	3301
1917			398 (11.63)		206 ( 6.02)	2501 ( 73.09)	7248 ( 211.81)	3422
1918			473 (13.95)		267 ( 7.54)	2865 ( 80.86)	7535 ( 212.67)	3543
1919			1486 (39.74)		350 ( 9.55)	58 ( 1.58)	8737 ( 238.46)	3664
1920			1272 (33.61)		278 ( 7.34)	2089 ( 55.19)	8207 ( 216.83)	3785
1921			1354 (35.23)		269 ( 7.00)	2533 ( 65.91)	8787 ( 228.65)	3843
1922			1421 (36.43)		325 ( 8.33)	2012 ( 51.58)	9420 ( 241.48)	3901
1923			1473 (37.21)		304 ( 7.68)	1616 ( 40.82)	8278 ( 209.09)	3859
1924			1324 (32.96)		333 ( 8.28)	1657 ( 41.25)	8067 ( 200.82)	4017
1925			1204 (29.56)		326 ( 8.00)	1647 ( 40.44)	7639 ( 187.55)	4073
1926			2251 (54.49)		399 ( 9.66)	1440 ( 34.86)		4131
1927			2245 (53.59)		549 ( 13.11)	1668 ( 39.82)		4189
1928			851 (20.04)		562 ( 13.23)	3723 ( 87.66)	7481 ( 176.15)	4247
1929			809 (18.79)		598 ( 13.89)	3338 ( 77.54)	7530 ( 174.91)	4305
1930			143 ( 3.28)		21 ( 0.48)	123 ( 2.82)	9229 ( 211.43)	4385
1931			22 ( 0.50)		986 ( 22.25)	39 ( 0.88)	10885 ( 245.60)	4432
1932			2656 (59.04)		762 ( 16.94)	6088 ( 135.32)	15785 ( 350.86)	4499
1933			2726 (59.70)		832 ( 18.22)	6335 ( 138.74)		4566
1934			2707 (58.43)		837 ( 18.07)	4887 ( 105.48)		4633
1935			1047 (22.28)		682 ( 14.51)	9716 ( 206.72)	20551 ( 437.26)	4700
1936			1134 (23.76)		640 ( 13.41)	9229 ( 193.36)	18298 ( 404.32)	4773
1937			1109 (22.88)		615 ( 12.69)	9079 ( 187.35)	19268 ( 397.61)	4846
1938			951 (19.33)		503 ( 10.23)	9312 ( 189.31)	17064 ( 346.90)	4919
1939			988 (19.79)		527 ( 10.56)	8466 ( 169.59)	17791 ( 356.39)	4962
1940			1024 (20.23)		562 ( 11.10)	7907 ( 156.17)	17386 ( 343.39)	5063
1941			1006 (19.52)		566 ( 10.98)	6996 ( 135.74)	17321 ( 336.07)	5154
1942			1152 (21.97)		578 ( 11.02)	7753 ( 147.85)	20318 ( 387.45)	5244
1943			987 (18.48)		671 ( 12.56)	8067 ( 151.04)	19701 ( 368.86)	5341
1944			1069 (19.65)		577 ( 10.61)	7155 ( 131.53)	17123 ( 314.76)	5440

CHILE	CRIME STATISTICS -- (RATE PER 100,000)					CONTINUED		
YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT ( )	ROBBERY ( )	THEFT ( )	POP
1945			747 (13.48)		526 ( 9.49)	5679 ( 102.49)	16630 ( 300.13)	5541
1946			848 (15.03)		501 ( 8.88)	5318 ( 94.24)	17548 ( 310.97)	5643
1947			811 (14.11)		561 ( 9.76)	5572 ( 96.84)	19867 ( 345.63)	5748
1948			943 (16.11)		601 ( 10.27)	5448 ( 93.06)	19768 ( 337.68)	5854
1949			878 (16.40)		615 ( 10.32)	5000 ( 83.86)	29859 ( 500.82)	5962
1950			720 (11.86)		895 ( 14.74)	5170 ( 85.13)	19121 ( 314.85)	6073
1951			710 (11.48)		779 ( 12.59)	4747 ( 76.75)	20680 ( 334.36)	6185
1952			2361 (37.48)		799 ( 12.68)	3445 ( 54.69)	19935 ( 306.95)	6299
1953			696 (10.77)		877 ( 13.58)	4483 ( 69.40)	17952 ( 277.89)	6460
1954			628 ( 9.50)		1018 ( 15.38)	4890 ( 73.87)	19718 ( 297.85)	6620
1955			661 ( 9.73)		995 ( 14.65)	5548 ( 81.71)	20510 ( 302.06)	6790
1956			2087 (29.98)		902 ( 12.96)	3388 ( 48.66)	20810 ( 298.91)	6962
1957			633 ( 8.87)		385 ( 5.39)	4147 ( 58.11)		7137
1958			642 ( 8.78)		1036 ( 14.16)	5407 ( 73.91)		7316
1959			312 ( 4.16)		518 ( 6.81)	2414 ( 32.19)		7500
1960			554 ( 7.21)			5016 ( 65.24)		7689
1961			756 ( 9.62)		926 ( 11.78)	6080 ( 77.37)	15129 ( 192.53)	7858
1962			657 ( 8.18)		920 ( 11.46)	5016 ( 62.47)	14690 ( 182.96)	8029
1963			651 ( 7.82)		963 ( 11.72)	5828 ( 70.93)		8217
1964			489 ( 5.76)		772 ( 9.09)	5421 ( 63.84)		8492
1965			603 ( 6.92)		1011 ( 11.61)	6453 ( 74.09)		8710
1966			541 ( 6.07)		893 ( 10.01)	4021 ( 45.08)		8920
1967			640 ( 7.00)		842 ( 9.21)	4140 ( 45.30)		9140
1968			711 ( 7.60)		684 ( 7.32)	4105 ( 43.90)		9350
1969			652 ( 6.81)		542 ( 5.66)	4450 ( 46.50)		9570
1970			587 ( 6.04)		418 ( 4.30)	4737 ( 48.73)		9720
1971			763 ( 7.72)		474 ( 4.80)	4857 ( 49.16)		9880
1972			823 ( 8.20)		542 ( 5.40)		6313 ( 62.88)	10040
1973			710 ( 6.94)		583 ( 5.70)		5958 ( 58.24)	10230
1974			426 ( 4.08)		723 ( 6.93)		5606 ( 53.75)	10430

## CHINA, REPUBLIC OF (TAIWAN)

## CRIME STATISTICS -- (RATE PER 100,000)

YEAR	MURDER ( )	MANSLTR ( )	HOMICIDE ( )	RAPE ( )	ASSAULT (1)	ROBBERY (2)	THEFT (3)	PDP
1952	553 ( 6.91)				2534 ( 31.67)	126 ( 1.57)	6657 ( 83.21)	8000
1953	491 ( 5.94)				3449 ( 41.75)	182 ( 2.20)	8688 ( 105.17)	8261
1954	637 ( 7.39)				5760 ( 66.84)	250 ( 2.90)	10845 ( 125.86)	8617
1955	842 ( 10.58)				5865 ( 65.85)	192 ( 2.16)	12708 ( 142.67)	8907
1956	947 ( 10.25)				6886 ( 74.52)	317 ( 3.43)	12782 ( 138.33)	9240
1957	921 ( 9.69)				5719 ( 60.16)	209 ( 2.20)	12098 ( 127.27)	9506
1958	1022 ( 10.37)				6545 ( 66.44)	226 ( 2.29)	12516 ( 127.05)	9851
1959	1199 ( 11.72)				6616 ( 64.66)	303 ( 2.96)	13180 ( 128.81)	10232
1960	1440 ( 13.57)				7734 ( 72.88)	260 ( 2.45)	13685 ( 128.96)	10612
1961	1511 ( 13.77)				8086 ( 73.70)	337 ( 3.07)	15549 ( 141.73)	10971
1962	1556 ( 13.71)				7827 ( 68.97)	406 ( 3.58)	16410 ( 144.59)	11349
1963	1702 ( 14.55)				8074 ( 69.03)	465 ( 3.98)	18274 ( 156.24)	11696
1964	1753 ( 14.52)				8435 ( 69.88)	319 ( 2.64)	18659 ( 154.59)	12070
1965	2042 ( 16.43)				8927 ( 71.84)	277 ( 2.23)	18262 ( 146.95)	12427
1966	2345 ( 18.34)				9210 ( 72.04)	318 ( 2.49)	17161 ( 134.24)	12784
1967	2419 ( 18.41)				9204 ( 70.04)	313 ( 2.38)	14101 ( 107.31)	13141
1968	2469 ( 18.29)				8763 ( 64.92)	466 ( 3.45)	13840 ( 102.53)	13498
1969	3027 ( 21.85)				9768 ( 70.50)	375 ( 2.71)	15589 ( 112.52)	13855

## FOOTNOTES:

1. INJURY
2. PILLAGE AND ROBBERY
3. BURGLARY

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