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THE REGULATION OF ARREST RATES*

by Lawrence W. Sherman Police Foundation and University of Maryland, College Park

and

Barry D. Glick Police Foundation

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Since Durkheim, sociologists have faced a dilemma in their use of official statistics as social indicators. As analysts of social facts, we need many types of official data to test hypotheses and advance the science. But as skeptics and social critics, we know that things are not what they seem: the human systems for producing social statistics are generally flawed. An unknown level of deviance from the manifest rules of counting often subverts the validity of the data.

This dilemma has evoked at least three responses, each of which is amply illustrated in the use of social indicators of crime and justice. Some sociologists have simply taken a deep breath and proceded to analyze official data on their face value. In a much richer tradition, others have closely examined the meanings and motives (Kitsuse and Cicourel, 1963; Douglas, 1967) or social facts (Black, 1970) guiding record-keepers in their decision making and labeling processes. More recently, a third response has developed, presenting detailed comparisons of the substantive results produced by official statistics and other ways of measuring the same phenomena (Hindelang, 1978; Hindelang et al, 1979; Sherman and Langworthy, 1979). This "triangulation" response (Webb, et al. 1964) has probably provided the most useful guidance to those who would use official statistics as social indicators. For as interesting as it is to know how individual record-keepers behave at the situational level. triangulation is far more helpful in deciding whether or not to use an official statistic for a specific analytic purpose.

Triangulation is not always possible, however. Many kinds of official statistics offer no economically feasible alternative forms of measurement for comparison. Yet without some assessment of their validity, there is no way to resolve the dilemma of their use.

-1-

This paper suggests a fourth response to the dilemma of official statistics, a response that may be generally appropriate when triangulation is not possible. That response is to examine the production of an official data set as a regulatory process. The paper examines arrest statistics, showing how the validity of the data may depend upon the measured level of compliance with the rules for counting. It suggests that ambiguity and complexity in the rules themselves create pressures towards deviance, while the resources of the system for education and monitoring create pressures toward compliance. While the measures of compliance are far from complete, they can still be used to inform a judgment about the validity of arrest data for various purposes.

Sociologists have long recognized the difficulty organizations have in achieving their members' compliance with organizational norms (Etzioni, 1965). The difficulty of achieving compliance of an entire population of organizations appears to be even greater, and certainly more complex. That is exactly what is required for the production of most of our social indiciators. In health, education, justice and other areas, thousands of organizations supply data about thousands of people each month to state or regional intermediary organizations, which in turn supply the data to federal agencies for compilation into national reports.

Reiss (1982) and others have classified social control systems as either enforcement or regulatory in nature; enforcement systems devote most of their resources to the detection and punishment of rule violations, while regulatory systems devote most of their efforts to encouraging compliance through education and negotiation. National statistical systems, most of which began as voluntary

ARREST REPORTING AS A REGULATORY SYSTEM

-2-

arrangements (and which still have many of the features of voluntarism) are clearly regulatory in nature. The only sanction these systems can impose upon their members is public refusal to accept a member's reports due to doubts about their validity. But the systems do not rely on the threat of this sanction to deter deviance. Rather, they attempt to build compliance through annual meetings, training, instruction manuals, and a sense of shared purpose.

The Uniform Crime Reporting (UCR) system was founded in 1929 with an organizational design intended to encourage compliance with its rules for counting reported crimes and arrests. Despite proposals to employ the statistical expertise of the U.S. Bureau of the Census, the UCR was placed in the Federal Bureau of Investigation as a means of encouraging voluntary cooperation with the system by the nation's more than 10,000 police departments. Under the constitution, a mandatory crime reporting system would have been difficult if not impossible to legislate. (All 50 states have since passed laws requiring their local police to report crime statistics either to a state agency or directly to the FBI). But with the cooperation of the International Association of Chiefs of Police, the new system was sold to local police chiefs on the grounds that it would help them. Many at first refused to report statistics to the system, but today participation is almost universal.

Pressures for Deviance.

Much has been made of the political pressures police chiefs are under to manipulate crime and arrest statistics to make their performance look good. These conscious manipulations are certainly a problem, although they appear to have declined somewhat in recent years. What may be far more important as a source of pressure to violate the rules for counting crime and arrest is the complexity and ambiguity of those rules themselves.

-3-

One of the major obstacles to the creation of the UCR system, for example, was the vast differences in definitions and classification of crimes across the states. A national system has to impose some uniformity (hence the name) in order to measure national crime trends and make valid comparisons across regions and cities. This was relatively easy to establish with a few of the more serious crimes, such as murder and robbery. But with many other crimes, it has been a continuing source of ambiguity and confusion for record-keepers. Another problem was the classification of incidents involving multiple crimes. This problem was solved by the creation of UCR hierarchy of offenses, with which the record keepers are instructed to classify an incident according to the specific crime highest on the hierarchy. But the hierarchy is relatively hard to remember, and time pressures make it difficult to consult each time there is a guestion.

But the greatest ambiguity lay in the definition of arrest (Sherman, 1980). Conflicting judicial opinions can be found to define arrest so broadly as to include "any restraint" on an individual's freedom to come and go as he pleases (Sweetnam V. F.W. Woolworth Co., 83 Ariz. 189 [1964]) or so narrowly as to exclude an eight-hour (involuntary) interrogation at police headquaters (U.S. v. Vita 294 F. 2d. 524 [1962]). The growing use of citations and summonses for minor offenses adds to the confusion, since these notices to appear in court at a later date are usually issued without transporting a suspect to a police station to be booked. Operationally, an arrest can be defined in five different ways: 1) detaining suspects on the street or any other location, 2) transporting suspects to a police station, 3) detaining a suspect at a police station, 4) "booking" a

-4-

suspect at a police station, or 5) filing charges against a suspect with a prosecutor. Most observational researchers have used transporting to the station house as their definition of arrest (LaFave, 1965; Black and Reiss, 1970). But in the early days of the UCR, a consultant to President Hoover's crime commission complained that police arrest statistics were often identical to prosecutor's statistics for persons charged (Warner, 1929:13). More recently, Klein and his colleagues (1975) found some agencies counting brief detentions of juveniles on the street as arrests.

This ambiguity is compounded by the inherent complexity of the arrest process. One incident can feature multiple crimes and multiple offenders, so that there is no common sense solution to deciding how many arrests to count stemming from one incident. Another problem is the bureaucratic complexity of arrests: police agencies often make arrests outside of their own jurisdictions, or within their jurisdictions on warrants obtained by other police agencies. Police agencies also receive arrestees apprehended through citizer's arrests by private citizens or paid security guards. A final pressure toward deviance in the system is turnover among the clerical personnel. Each time a new person is hired, he or she will inevitably make counting and classification mistakes until corrected (if at all) by a supervisor or colleague.

Strategies for Compliance.

The UCR system tried to cope with the pressures toward deviance from the very beginning. Its primary method has been the publication of a manual which established counting rules resolving some, but not all, of the questions that record keepers face in trying to maintain a consistent approach to compiling the statistics. Since the primary purpose of the system was to count crimes reported, the manuals devoted most of their space to that issue. Very little room was left over for confronting the problems in defining and counting arrests.

The first manual (International Association of Chiefs of Police, 1929:23) defined arrest as "the taking of a person into custody in order that he may be held to answer for a public offense". This definition did nothing to resolve the question of when someone was in fact in custody for that purpose rather than for interrogation. And by 1966, the manual had all but given up on providing a definition of arrest. The closest it came was distinguishing persons arrested ("All arrests are included even though the person is later released without being formally charged") and persons charged ("turn over for prosecution") (Federal Bureau of Investigation 1966 :2). But the manual did provide some new counting rules that helped resolve some of the ambiguities the clerks face (FBI, 1966:73), such as "Count one [arrest] for each person [no matter how many offenses he is charged with] on each separate occasion (day) he is arrested or charged."

The rules in the manual have been disseminated through a variety of training efforts over the years. The FBI national academy, a several month long training course for selected officers from local police agencies, served as an important means of recruiting new agencies into the UCR and teaching them how to comply with its counting rules. In recent years, as federal funds helped most states to establish their own crime counting agencies which took the responsibility of gathering police statistics to be sent to the FBI, the FBI has held biennial conferences for the state level officials. The state level officials, in turn, have taken an increased responsibility for obtaining compliance of their member police agencies with the counting rules. A variety of strategies for obtaining compliance with the manual are theoretically possible: training of police clerks, audits, and ongoing review of statistics supplied. But since the FBI has established no uniform compliance procedure, it has not been clear exactly what the states were doing.

-6-

METHODOLOGY

This paper uses a variety of methods to describe the regulation of the production of arrest statistics and to measure the level of compliance the regulatory process achieves. The research focuses exclusively on adult arrests, in order to avoid the much greater complexity of counting juvenile arrests (Klein et al, 1975).

Telephone Survey. An initial attempt to explore the procedures for counting arrests through telephone calls to police records units was abandoned after it became apparent that this method was not producing the kind of detailed responses we desired.

Site Visits. Glick visited a total of 18 police departments for one day each to interview the record keeping staff and observe arrest operations. All but two of the agencies served communities of over 100,000 people, and two were over 1,000,000. The sample was drawn from the middle Atlantic, Rocky mountain and Pacific regions of the U.S. It included one state police agency, 4 county and 13 city police agencies.

Mail survey of Police Agencies. Based on the issues that emerged in the site visits, we designed an instrument to be filled out by the heads of police crime reporting sections. The survey was mailed to all 213 city, county, and sheriffs' departments we could identify serving populations of 100,000 or more, plus a random sample of 26 of the _____ agencies serving populations of 10,000 TO 100,000. Of the 239 agencies contacted, 196 supplied usable responses for a response rate of 82%. The actual sample is dominated by agencies serving populations of 100,000 or more which comprise 175 (89%) of the 196 cases. Mail Survey of State Agencies. The one day site visits also produced a set of questions for the state agencies receiving the local police statistics. In

-7-

mailed back, for a response rate of 82% of the 50 states. to fill out an arrest report (results not reported here). police department with 900 officers.

4:00 p.m. to midnight.

order to improve the response rate and meet with state officials informally, Glick attended the three day 1981 national biennial conference of the state UCR officials at the FBI Academy. One official per state was asked to fill out the questionnaire. Thirty were returned at the conference, and 11 more were later

Case Studies. We selected four police agencies for two week site visits. The purpose of the visits (all done by Glick) was to observe the booking process in a large number of cases to see what reports were generated, audit the agency counts of arrests by offense type for a period of a month or more, and administer a questionnaire to a sample of patrol officers about when they decide

The departments were purposively selected to typify some of the many different types of police agencies and their regions. One was a large southern California urban police department with 850 officers, one was an old northeastern police department with 300 officers, one was a suburban mountain police department with 110 police officers, and one was a suburban mid-Atlantic

The observations of the booking process produced 103 cases at the California agency, 13 at the mountain agency, 29 at the northeastern agency, and 49 at the suburban agency. Only 3% were observed from 8:00 a.m. to 4:00 p.m.; 27% were observed from 12:00 midnight to 8:00 a.m. and 70% were observed from

-8-

FINDINGS

The findings show that the state UCR agencies invest relatively little effort in regulating arrest statistics and that the regulators themselves often fail to comply with UCR counting rules. This weak regulatory system allows a fairly high rate of errors on certain issues in the local police agencies. Most important, perhaps, is the error rate within agencies for certain offenses, which the audits found to be quite high. But on the key issue of the point in the arrest process at which an arrest is counted, there appears to be a fairly high level of consensus, even though it violates the UCR counting rules.

Regulation by the States

We identified three state level strategies for achieving compliance: training, report review, and "audits". Thirty three (80%) of the responding state agencies claimed that they "regularly" trained police personnel newly assigned to UCR reporting duties. Twenty-five states (63%) even claimed that police departments notify the state whenever new personnel are assigned to UCR reporting. On the other hand, 68% of the responding police agencies reported that the state agencies never provide training for UCR personnel. And 15 of the state agencies (37%) said that they do not have adequate resources for providing training to local police agencies. Since half of the agencies had 3 or less staff members for processing data from up to 1,036 police agencies (half dealt with 240 agencies or more), it is surprising that more did not report their resources to be inadequate.

Since 30 states (73%) reported that most police agencies supply arrest data on the standard FBI monthly "persons arrested" form, it is difficult for them to learn much from reviewing the reports. Only inconsistencies in police presentations of the summary statistics could tip the state officials off to a problem in report preparation. Unlike some other systems for reporting social statistics, such as the causes of death (for which the death certificates are forwarded to the state agencies), the UCR does not require that raw incident data be forwarded to the states. The volume of crime would make such a procedure prohibitively costly.

Even with this limited information, however, the respondents identified several areas of poor compliance by police record-keepers. The respondents were almost all highly confident about the accuracy of the police classification of multiple offense arrests according to the UCR hierarchy of offenses. But 25 of them (61%) claimed that police have problems reporting correct racial classifications for non-black minority groups. Twenty-five (61%) also said that some police agencies take credit for arrests made by other agencies, leading to double counting. Thirteen (32%) said police agencies were probably not reporting arson arrests made by fire officials.

Only 26 of the states (63%) claimed to have any procedures at all for reviewing the accuracy of arrest statistics. Only 19 (46%) claim that they try to insure that adult arrests are counted using the standard FBI definition of adults as 18 years old or older. The only certain way to verify the arrest totals police report in each category is to compare them to a review of each separate arrest report. But as far as we can tell, none of the state agencies conduct this kind of audit, with one possible exception. A few states may audit reports for one specific offense if there appears to be a large month to month change. But no state we know of conducts a comprehensive tally check for arrests in all offense categories. Only 6 (15%) of the states claimed to conduct routine audits of any sort, and most respondents defined an audit merely as a "spot check" of selected cases. Fourteen states (34%) defined an audit as a "systematic counting of arrest reports," but all of those stated that they had insufficient funds to

-9-

conduct such an audit. Twelve (29%) said they conduct an audit only when a problem is identified. Almost half (46%) said they had insufficient resources to conduct audits. Not one state reported ever making an unannounced visit to a police agency in order to audit its records.

Even if the state agencies could devote more resources to obtaining police compliance with counting rules, it is not clear that they could communicate the proper instructions. On most of a variety of guestions we included to test the state agency respondents' knowledge of the UCR procedures, a majority of them answered incorrectly. This is despite the fact that 93% of the respondents were the heads of the agencies (admittedly away from their offices) and 73% of the respondents had held their UCR positions for two years or more.

Most of the test questions covered key issues affecting the validity of reported arrest rates. Twenty (49%) said that if a police agency does not approve an arrest and the suspect is released, the arrest should not be counted; 56% gave the same wrong answer to the guestion phrased in a slightly different form. Twenty-seven (66%) said that arrests should not be counted unless on arrest report is generated, again a violation of UCR rules. Only 17 (41%) of the states require that arrests made by summons or citation include data on the offender's demographic characteristics, required for the "Age, Sex, Race, and Ethinic Origin For Persons Arrested" form. Only 15 (37%) respondents said (correctly) that citations other than traffic should be counted as arrests, and only 20 (49%) said that summonses should be counted. Six (15%) did not know whether to count field interrogations, and one even said that a separate arrest should be counted for each charge preferred, rather than for each person arrested. Their responses were much more accurate (63-78% right), however, on

-11-

jurisdiction. Compliance by Local Police

counting.

The one piece of good news in these results for arrest analysts is that there appears to be a fairly high level of uniformity in the basic definition of arrests for purposes of counting. As Table 1 shows, every department responding said that an arrest report is filled out (and presumably counted) whenever a police officer charges and books a suspect. The substantial responses saying arrest reports are always filled out in certain other circumstances as well should probably be interpreted as statements of ideals about how police officers should behave rather than empirical reports on their actual behavior. None of Glick's 20 site visits revealed any departments which counted arrests for UCR purposes without a formal booking process in a police facility (although several did count arrests for internal purposes). It seems clear that regardless of how the courts might define arrests, what police agencies count under the label of arrest is booking in a police station.

The bad news is that this nearly universal practice violates the UCR manual (FBI, 1966: 2), which asks agencies to count arrests even though the suspect is released without being charged. The good news is that it may not be a major source of invalidity in arrest data, since in not one of the 194 incidents Glick observed in which suspects were brought into a police facility was a suspect released without being charged. The bad news is that the modal charge was drunkenness, and that most of the arrests were for minor offenses (only 16% were

three questions about rules for counting arrests involving more than one

Given the weak capacity of the regulatory system, it is not surprising that we found a low level of actual compliance with the major rules of arrest

[Table 1 About Here]

for violent crimes of robbery, burglary, rape, assault with a deadly weapon, and arson), which means that the observation results may not apply to arrests for the rarer, more serious offenses.

It is still quite possible that some police agencies routinely arrest people for serious offenses, take them to a police facility, interrogate them, and release them without creating a formal record. The survey found that 76% of the agencies would not report an arrest to UCR if the suspect was released after being brought to the station without being charged. Ninety-two percent said they would not report an arrest to UCR unless an arrest report had been filled out.

<u>Review.</u> A greater source of variation in arrest counting among the police agencies may be the policies regarding supervisory review. In 34% of the departments, arrests are reported (as UCR requires) even if a supervisor disapproves an arrest and the suspect is released. But in 61% of the agencies, the responding UCR officials said that those arrests would not be counted for UCR. Comparisons of arrest rates across police agencies may therefore reveal differences in the intensity of supervisory review or counting practices rather than differences in rates of taking people into custody. Unfortunately, we were unable to pursue this issue in the four observation studies to see how many arrest reports were later disapproved; it is impossible to estimate the influence of this policy difference on reported arrest rates.

<u>Multiple Charges.</u> A clearly major source of variation is the compliance with the rules on counting persons arrested rather than charges brought. Eleven percent of the police respondents said that they counted each charge brought as one arrest. In a later question giving an example of one person charged with three offenses in one incident, 10% of the respondents said their agency would count three arrests--even though the UCR manual clearly says count only one

-13-

arrest for each person on each day he or she is arrested. It is possible that these responses merely reflect the UCR section head's micunderstanding (82% of respondents were unit heads, and 80% had two or more years in the position), not the actual practices of the clerks who actually do the work. Since the median number of clerks was only two, however, it seems likely that the section heads do help perform the work, and that their counting definitions are actually employed. Agencies counting all charges could show at least twice as many arrests as an agency charging the same number of people, depending on how many multiple charge incidents each of them processes.

<u>Summonses</u>. Another major distortion in arrest counting is police practices with summonses and citations. The UCR manual requires that both of these substitutes for booking be counted in the arrest statistics. But 29% of the respondents

said they do not include adult citation, and 57% do not include adult sommonses, in their arrest statistics for UCR. For analytic purposes, it would be better if the deviance from this rule was more widespread. But the splits at one third and one half raise substanital doubts about the comparibility across police agencies of arrest statistics for the wide array of minor offenses for which police can issue summonses and citations.

<u>Jurisdiction</u>. About half (49%) of the responding departments share jurisdiction with other local public police agencies, and that creates another source of regulatory failure. Contrary to the UCR manual, 15% of the respondents say they do not report arrests their officers make in another department's jurisdiction. Ten percent improperly take credit for arrests other agencies make in their own jurisdiction. And 44% report arrests their officers make based on warrants from other jurisdictions. The magnitude of variation these differences introduce into arrest rates depends on the proportion of cross-jurisdictional arrests,

-14-

which we did not estimate. But given the high level of overlap, especially in suburban agencies, it is at least plausible that this deviance from counting rules creates substantial error.

Hierarchy. Another unregulated source of deviance is the classification of multiple charge arrests. In three different test questions concerning the UCR hierarchy (which should determine the classification according to the most serious charge), a substantial portion of the respondents answered incorrectly. Two test questions for the more serious "Part I" offenses yielded correct response rates of 84% and 72%. A third question on less "Part II" offenses (for which the UCR manual allows each police agency to construct its own hierarchy) yielded a much lower agreement score of only 52%. This hierarchy variance could cause great error in counting arrests for crimes that are frequently combined, like robbery and assault.

Several other violations of counting rules were reported in the survey, although it is not clear that the situations they cover occur very frequently. Forty-three percent of the respondents said they do not include citizens' arrests in their UCR statistics. Thirteen percent said that they would count two arrests if additional charges were placed against a suspect in custody stemming from the same incident for which he or she had been initially arrested. Thirty-one percent said they do not include arson arrests made by fire officials (although not all jurisdictions empower fire officials to make arrests). One agency even said it included police field interrogations in the arrest count. which would greatly multiply its arrest figures relative to other agencies.

The site visits also revealed various paperwork problems. There is substantial variation, for example, in month to month "updating" of arrest data. Some agencies cut off reports received by the 28th of the month, and add all new reports (as they straggle in) to the next month's statistics. Others update the monthly data.

in this fashion. Errors Within Agencies

Looking just at the more serious "Part I" offenses, the Table shows more than ten percent error in almost every category in all four cities. The total error for all Part I offenses is not as large as it is for specific crime categories, suggesting that some of the error is due to misclassifications. The size of the percentage differences do seem to decline for the offense categories

-15-

figures for each month regardless of when the arrest reports arrive. These variations could create serious problems for time series analyses employing

The computer has created some additional problems. In one Virginia police agency, for example, if an initial charge is later changed, it is entered in the computer as a recode. But since the system does not allow the original charge to be deleted, the effect is to count two arrests.

Adding to the confusion is the fact that most police agencies routinely keep two sets of arrest statistics, one for UCR reports and one for administrative purposes. The administrative reports are often compiled from officers' activity logs, so that in departments using two-officer cars many arrests would be counted twice. Researchers who use police agency annual reports as their source of arrest data may well be receiving statistics inflated

Whatever the impact of differences in compliance with counting rules across departments, it is clear that violations of counting rules within departments also create substantial error. Table 2 presents the results of what may be one of the first comprehensive audits of police arrest statistics. The difference between the arrest totals the agencies reported to UCR for those months and the number of arrest reports Glick counted are striking.

[Table 2 About Here]

-16-

with larger Ns of arrests, but that pattern does not hold across agencies. Both the California and the suburban agency have comparable N sizes, yet the magnitude of error is almost three times greater for most Part I offenses in the suburban agency.

These data tend to refute a theory that police inflate arrest statistics to make their performance look better. In at least in two of the four agencies the reported arrest statistics for Part I offenses understate rather than overstate their arrest activity. At least three understate their arrest activity for as many offense categories as they overstate. Only the old northeast police department's reports are consistent with intentional inflation of arrest totals in Part I, but its understatement for Part II arrests and of total arrests discount that theory.

Conclusion

The findings suggest that the regulation of arrest statistics is extremely weak, and that the pressures for deviance from counting rules overwhelm the available strategies for obtaining compliance. Although there is much apparent agreement on the basic definition of arrest as booking and charging (in violation of UCR rules), there is so much variance in compliance with other counting rules that major questions about the uses of arrest statistics must be addressed. There is little evidence that police agencies are intentionally misrepresenting their arrest data. Nonetheless, the substantial internal tally errors revealed by the four audits suggest that no one cares enough about these data to confirm them. An analogy might be a bank that did not have supervisors verify each teller's cash count at the end of each day.

These conclusions suggest three kinds of implications: substantive implications for arrest analyses, policy implications, and general implications for social indicators.

Substantive Implications

The most immediate implication of these findings is that cross-sectional analyses of arrest rates may be invalid. The cross-sectional attempts to estimate the deterrent effects of arrests on crime rates (e.g., Wilson and Boland, 1978) may be measuring differences in counting practices rather than differences in the volume of apprehensions of suspects. The studies of differences in police "styles" (Wilson, 1968; Swanson, 1978), and in the quantity of social control across communities (Gardiner, 1969) may be explaining the behavior of clerks rather than the behavior of patrol officers. Longitudinal analysis of changes in arrest rates over time (e.g., Logan, 1975 and Greenberg, et al, 1979) may not suffer from the threats to reliability these findings show for cross-sectional analyses. But they do suffer from the high error rate detected in the audits. Year to year changes of 10 to 20 percent in arrest totals by crime category seem easily attributable to tallying errors. Moreover, changes over time in the pressures to deviate from the counting rules may also affect longitudinal analyses. For example, Blumstein and Cohen (1972) have supported a homeostatic theory of the stability of punishment by showing how reported rates of arrests for victimless crimes (except for drugs) decreased in the late 1960s as arrests for more serious offenses increased. The evidence in the present paper about the underreporting of arrests made through summonses and citations suggests an alternate explanation. As the use of summonses and citations for victimless crimes increased throughout the 1960s, the reporting of arrests for those offenses declined even though the arrests themselves did not.

The use of arrests to measure criminality by racial population groups (Wolfgang, et al, 1972; Hindelang, 1978; Hindelang, et al, 1979) may also be questioned, although not challenged, with these findings. The fact that what

-17-

-18-

gets counted as an arrest may vary does not suggest that it varies systematically by race. But since other aspects of police decision making have been found to have racial correlates (see Sherman, 1980, for a review), it is not implausible speculation that charging decisions (as distinct from taking into custody) may be influenced by race. Police in some cities may, for example, be fearful that black suspects will be more likely than whites to file complaints against the officer, in which case the filing of formal charges would improve the officer's defense to a complaint.

Regardless of racial correlates, the haphazard practices on charging may threaten the validity of studies of recidivism (e.g., Lipton, et al, 1975). The measured rate of recidivism (defined as re-arrest) for each subject may depend as much on the police agency clerks the subjects are exposed to as it does on their own behavior and on police detection practices. Any study tracking released offenders across several communities would be well advised to examine and compare different police agencies' practices for recording arrests. Policy Implications

The clearest policy implication of these findings is that UCR arrest statistics should not be used to evaluate police agency performance in relation to other police agencies. Even year-to-year evaluations of arrest trends may be suspect due to the error rate. For any agency that wants to, the violations of the counting rules are so easy to implement and so difficult to detect that evaluations of this nature may simply increase intentional misreporting, as opposed to the current haphazard misreporting.

A second policy implication presumes some variance across individuals in recording the fact that they have been arrested. This paper does not directly demonstrate that variance, but the findings do suggest it is possible or even likely. If it is true, then many criminal justice decisions about people based recording creates a problem of fairness. General Implications for Social Indicators

these four conditions:

- organizations.
- certificates).
- and monitoring strategies.

The more of these factors that influence the production of any social indicator, the less likely it becomes that comparisons across the social units or organizations supplying the data will be valid. This proposition may not

-19-

on their past records are being made unfairly. A sentencing judge, for example, may punish one convict more severely than another based on a difference in the length of their arrest records. If that difference is due to the different recording practices of different police agencies, the variation in arrest

A third policy implication seems almost futile to mention in an era of federal government cutbacks in spending on the production of social statistics. Nonetheless, it seems reasonable to conclude that many of the violations in counting rules could be corrected by supplying more resources to the regulatory system. If the state UCR agencies had more personnel, they could conduct more training and audits, yielding greater compliance in counting practices.

The regulation of arrest rates may not be unique. As a general proposition, one may suggest that compliance with counting rules will be reduced by any of

1. Quasi-voluntary statistical reporting by large numbers of

2. Reports produced in summary form (like arrest statistics) rather than raw incident reports on each item comprising the count (like death

3. Strong pressures toward deviance created by the ambiguity of event definitions and the complexity of counting rules.

4. Weak regulatory systems to counteract those pressures with educational

resolve the dilemma of whether to analyze data produced under these conditions, but it might help to make the decision more informed.

	<u>"When is</u>
	Circumstanc
a.	When a police o imposes any res freedom?
b.	When a police o a citizen in a to a police fac
с.	When a police o a citizen he is
d.	When a police o citizen at a po for more than f
е.	When a police o and books a sus
*	Ns do not total

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Table 1

Police Agency Responses to Question of an arrest report filled out in your jurisdiction?"

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nces

Per Cent Responses (N)*

		Don't				
	Always	Sometimes	Never	Know	Total	
fficer traint of	16% (32)	40% (78)	23% (46)	6% (11)	(167)	
officer places car and drives ility?	12% (24)	47% (92)	20% (39)	5% (10)	(165)	
officer advises under arrest?	60% (117)	16% (31)	8% (16)	3% (5)	(169)	
officer detains a lice facility our hours?	32% (62)	31% (60)	17% (33)	6% (11)	(166)	
officer charges pect?	97 % (191)	0	0	0	(191)	

1 196 due to missing responses.

		Table	2
Audit	of	Arrest	Statistics
in Fo	ur f	Police	Departments

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Offense	California*		Rocky Mountain**		01d Northeast**		Suburban*	
	% under/over	audit	% under/over	audit	% under/over	audit	% under/over	audit
Part I	reported by	count	reported by	count	reported by	count	reported by	count
Arrests	department		department		department		department	
Homicide	-	10	-	2	+100%	0	-	5
Rape	-11%	9	-	0	+25%	4	-60%	5
Robberv	-12%	33	-20%	5	+27%	15	-40%	30
Aggravated								
Assault	-12%	60	+167%	3	+83%	23	+143%	7
Burglary	+8%	71	-46%	28	+36%	25	-30%	79
Larcenv	-15%	168	+7%	27	-34%	59	+13%	202
Auto Theft	+6%	32	-17%	- 6	+10%	19	+17%	6
Arson	-			Ő -		2		6
Total of								[
Part I Arreste	-8%	383	+24%	71	+11%	147	-12	340
Part II	5		· L. 1/3			<u> </u>	A	
Arrests								
ni i esta								
Athon Accault	-27%	62	+43%	14	-21%	14	-10%	50
Forgery	-27,0	02	1450	T.	-610	T .	-10%	50
(counterfait)	-6%	32	-23%	. 3	-54%	. 22 -	+71%	7
Fraud	-0%		-100%	2	+100%	0	-129	25
Embozzlomont	. –		-100%	<u> </u>	-100%		-1C%	25
Stolon						U	100%	U
Dronont v					+150%	6	+1400%	1
Vandaliem	-150%		-	-	+10%	· 0	T1400%	17
Vallua I I Sill	+100%	10	+200%	1	740%	S A	-416	L/
Weapons Decentitution/	703%	19	+200%	<u>ل</u>		4	-100%	52
Vice		10			+100%	·	1000	2
VICE		40	-				-100%	10
Deve Abuse	+1.0%	160		1	-100%	- 4	100	
Drug Abuse	T10%	108	+0UU%	1	742%	20	-12%	CO
Gamb i Trig	TOA	12	· · · · · · · · · · · · · · · · · · ·		-	_	.	4
uttenses						1	. · · · ·	
Against	100%						0.0%	
	-100%	2	- . * * * *		- 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 1999 - 199	-	-80%	5
Uriving Under	00		1 🗸	1.00	11~		100%	
the influence	2 +8%	3/1	+1%	102	-11%	4/	+100%	U
Liquor Laws	-38%	26	-	-	-100%	4		0
Drunk	+1%	035	+100%	U	+200%	2	-	U
Disorderly	-24%	46	-20%	5	-10%	59	+11%	46
Vagrancy	-	0	-	-	-100%	2	-	0
All Others	-19%	230	+12%	143	-61%	65	+122%	54
Suspicion		-	-	-	• •	-	-	
	· · · · · · · · · · · · · · · · · · ·			1				
Total Part								
II Arrests	+1%	1676	+9%	336	-15%	271	+28%	339
Total Part I								
& II Arrests	-1%	2059	+12%	407	-6%	418	+14%	679
						1		

* - 1 month total
** - 3 month total

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