THE NEW ORLEANS OFFENDER STUDY: PHASE I

VOLUME II

ESTIMATION OF COLLECTIVE INCAPACITATION EFFECTS

A REPORT TO THE NATIONAL INSTITUTE OF JUSTICE

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PREFACE

This report presents the results of the analysis of the official record databases of the New Orleans Offender Study, and is the second volume of the Phase I report. This study is an investigation of the collective incapacitative effect of the criminal justice system in New Orleans and in Louisiana.

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INTRODUCTION

The New Orleans Offender Study consists of an analysis of arrest and custody records of offenders arrested in New Orleans for burglary or armed robbery during years 1973-1986 and a survey of inmates incarcerated in the New Orleans jail in 1986. The results of the inmate survey are presented in a separate report (Miranne and Geerken,1989). This report uses official record data on the arrests and the penitentiary, jail, and juvenile custody of a sample of Study inmates to determine the collective incapacitation effect of imprisonment on serious offenses committed by burglars and armed robbers. It examines, for the first time, the extent to which the criminal justice system selectively incapacitates the high frequency offender. The results indicate that past estimates of the incapacitation effect of imprisonment were too low, both because of the quality of custody data and their assumption of a constant offense rate.

Jacqueline Cohen's review of collective incapacitation studies (Cohen, 1983) found that existing incarceration policies have only a modest incapacitative effect - less than 20% of potential crimes are prevented by incarceration. The same review evaluates studies considering the crime reduction potential of sentencing laws more severe than those currently in effect. The studies indicate that these strategies achieve only a modest effect on the crime rate and result in prison population increases of staggering proportions. Interest has thus turned to other crime-control strategies, such as selective incapacitation.[1]

Conclusions about collective incapacitation, however, are premature. First, the quality of the data available for the measurement of incapacitation, especially of adults, has been inadequate to the task. The importance of the issue deserves an investigation with reliable and complete measurements of both the offense rate and incarceration time. Second, studies of the present collective incapacitation effect need to consider the extent to which the present system is already selective. We know that offenders vary in their rates of offending, judges and parole officials include their own estimates of offending potential in their sentencing and release decisions, and offenders who are frequently arrested will probably spend more of their lives incarcerated than those who are not. We as yet have only theoretical estimates of the collective effects of these naturally selective processes. Third, it is likely that current collective effects are not uniformly effective or ineffective. It is probable that some offender types are better incapacitated than others and it is important to determine whether current incapacitation priorities make the most efficient use of existing jail and prison resources. Improvement might be achieved outside ethically questionable selective incapacitation strategies by readjustment of statutory criminal penalties and legal restrictions on pretrial release, probation, or parole. In particular, the juvenile justice system and the transition to the adult system for the young adult offender needs careful analysis. Finally, we do not yet have an accurate picture of the contribution that different forms of incarceration - pretrial jail, sentenced jail, penitentiary, juvenile institution - make to the reduction of criminal offending as the offender moves through the course of his career. This type

of data has been unavailable and is presented here for the first time.

This study uses five separate official record sets to construct a picture of the arrest and incarceration experiences of a sample of burglars and armed robbers arrested in New Orleans during the years 1973-1986. The data includes dates both of jail and penitentiary incarceration and local, state, and national arrest information. For juveniles dates of incarceration in the state's juvenile prison (training school) system are included. This report focuses on measurement of the collective incapacitative effect of the entire criminal justice system on these offenders, and investigates the contributions that different forms of incarceration make to the overall incapacitative effect over the offender's life cycle. Racial effects are also analyzed. Finally, we investigate in a preliminary way the extent to which the criminal justice system adjusts its response to the offender's level of criminal activity, and the effect of these adjustments on the collective incapacitation effect.

PREVIOUS STUDIES

Previous research on the collective incapacitation effect of imprisonment is of two types. Articles by Clarke (1974), Ehrlich (1974), Shinnar and Shinnar (1975), Greenberg (1975), and Peterson et al (1980) estimate the collective incapacitation effect of current imprisonment policies. Though the primary focus of Blumstein and Cohen (1979) is not incapacitation, estimates of incapacitation effects are used to estimate crime rates. A second type of study estimates the collective incapacitative effect of alternative imprisonment policies. Such studies are not discussed here.

The research on the effect of current policies has been reviewed in detail by Cohen (1978). These studies vary considerably in their estimate of the size of the incapacitation effect. Three (Ehrlich, Greenberg, and Blumstein and Cohen) find the size of the effect to be minimal - from 1.2 to 7.4% of total index crimes (reported and unreported).

Greenberg (1975)'s estimates are based on the average sentence length and the rate at which previously arrested offenders commit index offenses, which is assumed to be constant. He uses 1965 FBI data on criminal careers to estimate a lower bound for the index offense rate and nationally aggregated FBI Uniform Crime Reports data to estimate an upper bound. These estimates involve arbitrary assumptions about the percent of all arrests which are virgin (first time) arrests and about the crime reporting rate. His estimates of the index offense rate (lambda) are very sensitive to the values of input variables which are only poorly known. Cohen

(1978, pp 205-206, and 1983, p 15) shows that other, at least equally plausible assumptions about the values of these variables would raise the upper bound of the incapacitation effect from 7.4 to 11.9% of potential index crimes.

Ehrlich (1974) is primarily concerned with estimating the deterrent effects of imprisonment. But since the effect of imprisonment on crime rates includes both deterrent and incapacitative effects, he was required to apportion the total imprisonment effect between the two. The incapacitative effect is derived using two variables - the probability that a free offender is arrested and imprisoned and the average time actually served in prison by an offender. Both these values are estimated using data from the National Prisoner Statistics for 1960. Cohen (1978, pp 206-209) has demonstrated errors in the Ehrlich's calculations, not the least of which is an implicit assumption that the annual index offense rate = 1. Higher values for the offense rate lead to significantly higher estimates of the incapacitation effect (and correspondingly reduced estimates of the deterrent effect of imprisonment).

Blumstein and Cohen (1979) and Clarke (1974) are the only researchers to estimate incapacitation effects using individual level official data. Blumstein and Cohen use data from the FBI's computerized criminal history files on the adult arrest histories through early 1975 of all those individuals arrested for homicide, rape, robbery, aggravated assault, burglary or auto theft in Washington, D.C. during 1973. The total sample includes 5,338 individuals. The analysis is primarily concerned with estimating individual crime rates by age and cohort. Since the calculation of individual criminal intensity during a career involves estimation

of time incapacitated, the authors attempt to generate estimates of incarceration time from the arrest history data. As is common in such histories (see discussion in Geerken, 1988), information on time incarcerated is very poor. Blumstein and Cohen find that there is no information beyond the recorded arrest in 59% of the cases, and the authors assume that there were no convictions for these arrests. Data on actual time served is much less complete: only 10% of the sentences have both reception and release dates. When release date is estimated from sentence information (the minimum sentence is used), in more than one third of cases additional arrests are recorded prior to their estimated release date. Blumstein and Cohen assume that these occurrences represent error in their estimation of the length of commitment. This is not necessarily the case since an inmate may be booked for offenses against other inmates and/or for offenses committed prior to incarceration while in prison (Geerken 1988, p 12). Furthermore, a convicted offender is often given credit by the court for time served prior to trial in the calculation of his term of confinement, and this pretrial time is not available in Blumstein and Cohen's data . Their data and method yield an average sentence time per arrest of 1.9 months, which they assume is an upper bound on incarceration time. This very low incapacitation estimate justifies their regression analysis of the full sample which ignores incapacitation as a factor in the estimation of individual arrest rates.

The latter part of the Blumstein and Cohen analysis consists of an examination of selected cohorts - individuals who reached 18 in the years 1963-1966 and who had at least one arrest during ages 18-20. For these individuals they estimate time incarcerated. Though

only 5% of recorded sentences of confinement have actual time served recorded, they estimate time served by assuming minimum time on an indeterminate sentence and 1/3 time served on flat sentences. They do not consider parole revocations. They have sufficient data to estimate 74% of the lengths of confinement using these methods. They set the remainder to zero.

Using these estimation methods, Blumstein and Cohen find incapacitation effects ranging from 2.3% (aggravated assaulters) to 5.4% (burglars). Though they admit that these estimates are likely to be somewhat low (fn 46, p 580), they suggest that even doubling the estimates of time served per confinement would not significantly affect their estimates of individual arrest rates.

Clarke (1974) is the only researcher to date who focuses on the incapacitative effect of the juvenile justice system. Using the Philadelphia cohort study data, Clarke restricts his attention to the 381 juveniles institutionalized before age 18. Unfortunately he has no data on actual length of stay, which he estimates at an average of nine months per incarceration. Unlike the adult studies, he is able to disaggregate his study population by age and race. He finds that 5% of the total index arrests for whites and 15% for non-whites are averted by institutionalization. Cohen (1983, p 13) points out that Clarke's estimates are likely to be too low because he fails to consider age of onset and termination in his determination of individual arrest rates. Making such adjustments increases the incapacitation effect for whites to 15% and for blacks to 30% (Cohen, 1978, p 203).

Shinnar and Shinnar (1975) make their estimate of the incapacitation effect based on a clearly derived and specified model of the criminal career. Their estimate depends on the

expected prison stay per crime committed ("qJS" in the now commonly used career notation[2]) and the offense rate for "safety" crimes (homicide, rape, robbery, aggravated assault, and burglary). The first quantity, based on New York State data, is the average daily prison population divided by the number of crimes and the second quantity is estimated from FBI career data on offenders arrested for federal crimes in 1970. The authors calculate the incapacitation effect for the years 1940, 1960, and 1970, and estimate that the reduction from potential crime, as high as 85% in 1940, declined to 20% by 1970. Cohen (1983, pp 17-18) argues that the free crime rate estimated by the authors - ten safety crimes per year - is likely to be overestimated because of the use of an inappropriately low clearance rate and because of circular reasoning in calculating time of incarceration. She proposes reduction of the estimate by one-half, which reduces the 1970 New York State incapacitation effect to 11%.

Unlike the studies described above, the Peterson et. al. (1980) estimate is based on offender self-report data - the 1976 RAND survey of California inmates. Using data on self-reported offenses and prior incarcerations an average crime-specific offense rate is estimated for all inmates. The estimate of the total number of crimes in 1976 that would have been committed by California inmates is then compared to estimates of the total number of offenses, by type, that were committed in California during the same period. The authors estimate that 18% of potential armed robberies, 5.7% of potential burglaries, and 6.5% of potential auto thefts were averted through incarceration. The authors note, however (p 34), that the sample inmates are likely to be more serious offenders than the average prisoner, and the offense rates used might

therefore be too high and the incapacitation effect too high as well.

Cohen (1983) argues that, with the exception of Clarke (1974), all studies to date assume that offenders commit crimes at a constant rate and also share the requirement that an average individual crime rate for offenders be estimated, with the age-race disaggregation of arrest and incarceration rate by Clarke (1974) only a minor exception. This is not entirely the case, however. The Peterson et. al. incapacitation estimate does not actually require an estimate of the average offense rate for all offenders but only for those incarcerated. The offending rates of unincarcerated offenders can take any level or distribution whatsoever, since only the total crimes committed in California is relevant to the estimate: the number of offenders is unimportant. There is a problem if the sample of self-reporting offenders is not representative of the incarcerated population as a whole, but the variance of the offense rate among incarcerated offenders is also irrevalent. All prisoner self-report studies to date have found highly skewed reported offense rates. Only the average rate is important to the calculation of the incapacitation effect, however, since only it is needed to calculate the total potential crimes which would be committed by the incarcerated population. The highly skewed distribution is important as a measurement issue, however. Average offense rates are very sensitive to the extremely high outliers found in these studies. To the extent that these outliers are the product of measurement error caused by the instrument or by conscious deception (see the discussion in Miranne and Geerken, 1989) the estimate of the incapacitation effect based on them might be overestimated.

In summary, the official data aggregate studies, even those with carefully developed models such as Shinnar and Shinnar, are forced to assume a constant offense rate (Lambda). Theoretically, a probability distribution for offense rate could be estimated, but the empirical basis for such a distribution would be quite weak and assumptions about qJS would still be necessary. All studies of collective incapacitation of present policies to date using official records, even individual level studies, have been based on estimates of time served rather than on actual time served because of the very poor quality of official incarceration data.

None of the official record studies consider the incapacitative effect of jail incarceration. This is surprising, since about 1/3 of all persons incarcerated in the United States on a given day are housed in jails. About one-half of these are unconvicted, and the remainder are sentenced to a jail term or awaiting transport to the penitentiary (Report to the Nation, p 104). The Rand three-state survey found that jail inmates in California and Michigan reported offense rates that for some serious crimes were close to those of penitentiary inmates from the same state. California jail inmates reported offense rates almost identical to those of Michigan prison inmates. (Chaiken and Chaiken, 1982, pp 203-216). Pretrial jail time is unmeasured in all incapacitation studies to date: it is assumed that only convicted offenders are incapacitated.

MEASURING A VARIABLE ARREST RATE

All official record based estimates of the collective incapacitation effect to date have rested on the assumption of a constant offense rate among active offenders, though it is known that such an assumption is erroneous. It has been demonstrated that if the offense rate is independent of qJS, the assumption of a constant offense rate will always result in an underestimate of the collective incapacitation effect (see Marsh and Singer,1972, and the discussion in Cohen,1978, pp 213-214.)

Offense rates must be assumed to vary not only among offenders but during the course of any individual offender's career as well. The age/crime relationship is an important part of the currently raging debate about the usefulness of the criminal career approach (see especially the v.26 no1 issue of Criminology). There is certainly, at present, insufficient evidence for assuming a constant offense rate during an offender's career.

The assumption of a varying offense rate among active offenders and during an active offender's career poses serious methodological problems for estimating the collective incapacitation effect from official data. The criminal career literature treats the number of offenses committed by an offender as a random variable generated according to a poisson function defined by the offender's underlying offense rate. If this underlying parameter is assumed to vary over the individual's career then estimation of this parameter involves grouping like individuals in like stages of their criminal careers and averaging their observed offenses. If only arrests can be observed, as is the case with official data,

then an average of arrests can be used as an indicator of the underlying offense rate if we assume that the probability of arrest for an offense (q) is constant across groups or varies independently of the offense rate. The collective incapacitation effect can then be estimated without estimating the offense rate simply by determining the incapacitation effect of each group and averaging the group effects weighted by the groups' arrest rates and their size. Since we assume that an individual's offense and arrest rate will vary during his career, we use individual manyears (birthday to birthday) as our unit of analysis rather than individuals. After initial examination of arrest rates and incapacitation by age and race we group individual manyears on the basis of characteristics which appear to be related to the underlying offense rate. When such groups are of sufficient size and homogeneity then a reliable estimate of the underlying rate can be obtained. For each group of manyears g, with size n_g , incapacitaion effect I_g , and underlying offense rate l_g, the offenses prevented is $(n_g)(l_g)(I_g)$ and the offenses that would have occurred without incarceration is $(n_g)(l_g)$. The population incapacitation effect is then:

$$\frac{\sum_g ((n_g)(l_g)(I_g)}{\sum_g ((n_g)(l_g))}$$

If q is constant for all offenders or is independent of the offense rate, then the arrest rate (Mu) can be used as a proxy for Lambda. If this is the case, I_g can be measured without knowledge of J or S, simply by measuring the time incarcerated of the offenders in each group. If Mu is a valid proxy for Lambda, that

is, if the ratio l_g/Mu_g is the same for each group, substituting Mu_g for l_g in the formula above will yield the same population incapacitation effect since it is equivalent to dividing both the numerator and denominator by the same quantity, q.

The underlying Mu - the single parameter of the poisson function which defines the p.d.f. of the number of arrests occurring during a year - is simply the arrest rate while free. The free arrest rate for a group might be estimated in three ways: 1) the average number of arrests per year for the group's individual manyears, ignoring incapacitation, 2) the average of the free arrest rates measured in each of the group's individual manyears, or 3) the sum of all arrests for the group divided by the sum of all free time for the group. We refer to the first quantity as the individual average arrest rate, the second as the individual average free arrest rate and the third as the aggregate free arrest rate. It is widely recognized that the first quantity always yields an underestimate of the group's underlying Mu whenever there is incarceration, and it is therefore only used when data on time of incarceration for a population is poor or unavailable. We show that the second quantity yields an upwardly biased estimate of the actual free arrest rate whenever incarceration is present. The third yields an unbiased estimate. This can be demonstrated logically and through simulation.

The individual average free arrest rate is upwardly biased because incarceration is more likely to occur during manyears when arrests occur than during years when they do not occur. This is the case even when the underlying Mu is the same for all manyears, because periods of incarceration generally occur after arrests. Incarceration can, of course, take place during manyears when there

are no arrests. An incarceration period might be the continuation of a term beginning in a prior year in the individual's career, or might in some cases be unrelated to an arrest - a technical probation or parole violation, for example. But, on average, there is a higher probability an arrest will occur in manyears when incarceration occurs than when it does not. Since the observed free arrest rate for a manyear is (arrests)/(percent of days free), each arrest has a higher probability of being adjusted upward than if the occurrence of arrests and periods of incarceration were completely independent.

Simulation

Since such intuitive conclusions can sometimes be misleading or incomplete, we designed a computer simulation to verify our conclusions. The program generates a series of groups homogeneous in underlying Mu. Each group consists of 1000 manyears. The program, for each of the 365 days of each manyear, randomly decides if an arrest will occur based on the probabilities generated by the poisson p.d.f. function with Mu/365 as mean. If an arrest occurs, the program then randomly decides if the individual is incarcerated, based on a 36% probability of incarceration (J). This probability of incarceration is drawn from 1984 results for all felony offenses for the eleven states participating in the Offender-Based Transaction Statistics Program. (Source: Bureau of Justice Statistics,1988, p.2,table 1) If incarceration occurs the length of incarceration is determined randomly from an exponential p.d.f. for sentence lengths. The program was run for a range of Mu's and for a range of means, S, for the sentence length function. The program also accounts for years for which an individual is incarcerated 100% of the year (identified in the tables as "years

lost".) These years are included in the aggregate estimates but cannot be used in estimates of average free Mu. The results are summarized in Table 1 (detailed results are given in Appendix A.)

The simulation results indicate that average free Mu's are upwardly biased for Mu's .05 - 2.00 (the range of values we encountered empirically in preliminary analysis of the data), and almost certainly for all other possible values of Mu as well. This bias increases both in absolute and percentage error as average incarceration length increases over the range 1-30 months. The aggregate free Mu is unbiased over the same ranges of Mu and S. The extent of over-estimation by average free Mu appears unrelated to actual Mu.

In summary, unbiased estimates of free Mu for any group of offenders can be calculated only by separately summing arrests and days in custody for the group and combining the results. Since these aggregate values have no variance and no means based on individual level values, the bulk of the statistical tools normally used to estimate relationships between variables is unavailable. In particular, the relationship between incapacitation and Mu cannot be directly determined by the usual correlation and regression techniques. Instead, we proceed by dividing the sample of manyears into groups likely to be more homogeneous in underlying Mu and use an appropriately weighted average of the incapacitation effects estimated for each group to determine the overall criminal justice system incapacitation effect.

THE DATA

As section II of the first volume of this report describes, the official record data for the New Orleans Offender Study include information taken directly from a total of five criminal justice databases and indirectly from a sixth. These six sources are 1) the New Orleans jail management information system (STARS) covering the years 1981-1986; 2) the New Orleans Police Department's arrest history system (MOTION), complete for years 9/1973-1986; 3) the adult penitentiary and probation/parole information system (CAJUN) maintained by the Louisiana Department of Corrections (1974-1986); 4) the juvenile corrections information system (JIRMS) also maintained by the Department of Corrections (1974-1986); 5) the Louisiana computerized criminal history system (FINDEX) maintained by the Louisiana Department of Public Safety-State Police (1974-1986); 6) FBI records of arrests added to FINDEX by the State Police. These databases and the procedures followed to merge them into a single study data set are described in detail in Volume I of this Report.

All individuals arrested for burglary or armed robbery in New Orleans during the years 1973-1986 were selected for the New Orleans Offender official record study. Altogether, 22,561 offenders were initially selected. In order to insure that the manyears selected for study all fell within an offender's active career and to avoid a problematic determination of the ages of onset and termination of each offender's career we defined as active only those years between the years of each individual's first and last recorded arrest. The manyears which include the

first and last arrest are excluded from the analysis.[3] Since jail incarceration data was available only for the years 1981-1986 we limited the sample to those 15,139 individuals who had some "arrest bracketed" time intersecting the 1981-1986 period. From this group 1,972 burglars and 1,550 armed robbers were selected according to a disproportionate stratified sampling design which oversampled whites, older age cohorts, and individuals with more incarceration time. (See p38 of the first volume of this report.) Unless otherwise stated all the results reported in this study are weighted to reflect the disproportionate sampling and therefore can be generalized to the 15,139 study sample active during the years 1981-1986 (see table 2.)

As Blumstein and Cohen (1979, p565) point out, there is no reasonable way of generating a random sample from the population of active offenders. First, only offenders who are arrested are available for study through official criminal justice records. Offenders who are more vulnerable to arrest because of a high frequency of offending or because of their own ineptness at eluding police are overrepresented in any arrestee sample. The following analysis, therefore, is in fact based on estimates of the incapacitation effect of the criminal justice system on detected offenders. While this is a limitation imposed by the nature of the study data, other approaches which estimate undetected offense behavior have their own set of special problems (see pp 2-3 of the first volume of this report). In the long run, only a combination of methods will yield a complete picture of the effectiveness of the criminal justice system's incapacitative efforts on crime. It is first necessary, however, to obtain the best possible estimates from official data.

It is difficult to determine the extent to which our initial sample of manyears is representative of the population of burglars and armed robbers active during the period 1981-86. The higher the offense rate for the criterion offenses of burglary and armed robbery the higher the probability of inclusion in the sample, if q is independent of the offense rate. This effect should lead to an overrepresentation of high frequency criterion offenders in the sample.

On the other hand, incarceration reduces the probability of inclusion by the limiting the number of manyears during which an arrest could occur. This applies not only to the criterion arrest but to the bracketing arrests as well. To the extent that high frequency offenders are more often incarcerated, the sample is biased toward the low frequency offender. It is not possible to determine the net effect of these two tendencies on the representativeness of the frequency distribution of sample charge and arrest rates.

The effects of the sample selection procedures on measurement of the incapacitation effect, however, is to bias it downward. No incarceration term in excess of 13 years which begins within the study period can be counted since and end bracketing arrest cannot occur. Terms of 13 years or more are common in Louisiana. The penalty for armed robbery, for example, is five to ninety-nine years without benefit of parole, probation, or suspension of sentence. A number of crimes carry this "no benefit" restriction, including murder, rape, aggravated arson, and possession of schedule 1 narcotics (opiates, hallucinogens, and PCP.) The penalty for simple burglary is up to twelve years, but multi-bill statutes can be (and in New Orleans, always are) used to double these

penalties. The following results, then, are sure to underestimate the incapacitation effect in Louisiana.

RESULTS

The types of and time in custody and the free charge and arrest rates for burglar and armed robber manyears are reported in Tables 3 and 4. (In the following analyses, burglary and armed robbery dependent variables are charges and the index offense dependent variable is in the form of arrests.) Manyears 1973-1980 and 1981-1986 are reported separately because there are no parish jail incarceration records prior to 1981. (Parish incarceration time prior to 1981 was available and counted for terms which extended into 1981, however.) In addition, the sample selection criteria - active at some time during 1981-1986 - means that the 1973-1980 manyears are drawn only from the population of offender still active 2-12 years later. For example, juveniles (age < 16 category) reported in the 1973-1980 category were all, by definition, still active as adults and, as such, are not representative of all juvenile offenders, some of whom terminate their careers before adulthood. Therefore the results for 1973-80 should be interpreted with caution, and are presented here primarily to compare juvenile with adults from the same period.

Custody is calculated in three different ways. The first ("unadjusted"), is simply the total penitentiary time, parish jail time, and juvenile institution time expressed as a percentage of the year. Penitentiary time ("DOC") includes time held in the state penitentiary and time held in a parish jail under sentence to the state penitentiary. Also included is pretrial and presentence time awaiting sentence to the penitentiary, since it is common

practice to grant credit for pretrial/presentence time served on the penitentiary sentence. Parish jail time includes time spent prior to trial and sentence (unless it is a penitentiary sentence) and time spent sentenced to the parish jail, as well as any other special custody type (hold for extradition, federal court, and probation or parole violation). Juvenile institution time ("JUV") refers to incarceration in a state juvenile facility. Local detention in juvenile facilities is not included.

The second total custody figure is adjusted for the lack of data about jail incarceration in other parishes. We estimated time spent in other parish jails on the basis of the percentage of Louisiana arrests which occur outside New Orleans. This additional estimated jail time is added to the second custody figure. (It is not, however, included in the "Parish Jail" column.)

The third total custody figure is calculated by adjusting for incarceration in other states based on the ratio of arrests outside Louisiana to Louisiana arrests. We assume that average incarceration time per arrest is 74% of the Louisiana time per arrest, because of Louisiana's high incarceration rate.[4] The resulting custody figure is almost certainly too low. The number of out-of-state arrests is calculated from the FINDEX database: the Louisiana State Police record of arrests. Out-of-state arrests are added to this database by State Police data entry operators using FBI rapsheets returned to the State Police after a felony arrest fingerprint card is sent to the FBI. As volume I, section I of this report describes, many arrests are missing from an offender's FBI rap sheet. In addition, it appears that the State Police did not always add the FBI arrests to their database.

Total incarceration time is also underestimated because we have

no data on and make no adjustments for federal incarceration time (about 5% of U.S. incarcerated male population in 1986).

This underestimation of out-of-state incarceration time is probably not uniform across age and racial categories. As Table 5 shows, whites are much more likely to have both out-of-parish and out-of-state arrests than blacks, and are much more likely to be born outside the state. Out-of-parish criminal justice contacts are also related to age, with the 20-29 age category showing the greatest mobility.

Incapacitation Results

Collective incapacitation effects range from 12% to 27%. In general, blacks are much better incapacitated than are whites (83% better for burglars, 42% for armed robbers), though this may in part be a function of the greater underestimation of out-of-parish incarceration time for whites. Though blacks' free burglary and armed robbery rates are about the same as those of whites', their free index arrest rates are about 30% higher, at least a partial explanation of their differences in time incarcerated. (See Table 6.)

Armed robbers are better incapacitated than burglars in the case of both racial groups. Armed robbers' free index charge rates are higher than those of burglars, again, providing a partial expanation of difference in incapacitation.

Incapacitation effects are very low for juveniles (ages 16 and under): 2-3% for burglars and 3-4% for armed robbers. Both time in custody and the armed robbery rate for black armed robbers (1981-1986) is negatively related to age. For both black burglars and white armed robbers incarceration time peaks in their 20's but

criterion free arrest rates peak in their late teens. The criminal justice system, especially for burglary, seems to spend too little of its resources on teenage offenders (both juvenile and adult), and might significantly improve its overall incapacitative effect by shifting resources from older (50+) offenders to holding the young, much more active offender.

Note that these incarceration and arrest rate patterns have a particular relevance to arrest rate studies that do not measure or account for incarceration time. The age pattern of incarceration time might mask a negative relationship between offense rate and age. Race effects could also be masked.

It is clear that prior studies, which have uniformly ignored jail time as a component of incapacitation, have as a result considerably underestimated the overall effect of the criminal justice system. Jail and juvenile institution time (which even this study underestimates) are particularly important in the young adult years, constituting one-half of total time incarcerated for adult teen armed robbers. Juvenile institutions in Louisiana make a significant contribution to <u>adult</u> incapacitation until age 21.

Table 7 provides a detailed picture of the custody commitments over the offender's life cycle. Young adult blacks have many more custody commitments that do not end in a sentence of incarceration ("state pretrial") than do young whites and are more likely to be sentenced to jail ("sentenced jail"). Young whites, however, are much more likely to be sentenced to the penitentiary. In general, commitments for serious offenses tend either to decline or to reach a plateau as the still active offender ages. Commitments for minor offenses (violations of municipal ordinances such as public drunkeness and petty theft) increase with age, dramatically so for

whites.

Charge rates while free for each index offense and for selected non-index offenses are presented in tables 8-11. There is little or no specialization by these offenders. Except for the rates of their criterion offenses, burglars and armed robbers are indistinguishable on the basis of arrest frequency of any other crime type. Armed robbers are arrested for burglary and theft at about the same rate as they are arrested for armed robbery. The characterization of offenders as burglars or armed robbers in this study, therefore, is meaningful only as a method of measuring the incapacitation effect for burglary and armed robbery offenses, not as a classification of offender type.

DISAGGREGATION AND RECOMPUTATION OF THE INCAPACITATION EFFECT

The incapacitation effects we have calculated for burglars and armed robbers are too low if the average term of incarceration per arrest (JS) is not negatively correlated with the free arrest rate. If groups of offenders characterized by a high free arrest rate are better incapacitated than those with fewer arrests, the overall incapacitation effect on crime is greater than that calculated under the assumption of a constant arrest rate for all offenders.

It is important, also, to distinguish between the free arrest rate for all crimes and charge rates for specific offenses, such as burglary and armed robbery. We expect time incarcerated to be a function of the number and seriousness of all arrests. The incapacitation effect for a specific crime may be very different from the effect on all crimes even for the same population of active offenders. This occurs if the relationship of JS to the free arrest rate for one type of crime differs from its relationship to the total free arrest rate. In addition, since we know that most offenders are arrested for more than one crime type during their careers, the incarceration resulting from an arrest for one crime type has an impact on the offender's ability to commit and be arrested for other types.

Thus, a model of the incapacitative effect of the criminal justice system on, for example, burglary, has to include the likelihood and length of incarceration following burglary arrests, the probability and frequency of a burglar's arrest for other crimes and the likelihood and length of incarceration for each of those crimes. In addition, the model has to deal with the influence of prior arrests and of prior convictions and

incarcerations for each crime type on the subsequent probabilites and lengths of incarceration of each crime. Finally, the model would have to consider the "dense pack" phenomenon for arrests: the tendency for multiple arrests during a brief period to be treated as a cluster by the criminal justice system, dismissing many charges after a conviction on one or sentencing the offender to concurrent terms of incarceration for multiple convictions. A complete model would also include probabilities of probation and parole revocation and the probability and length of pretrial detention.

To our knowledge, all of the data necessary to estimate the parameters of such a complex model is not available at this time. Our approach to estimating the incapacitation effect of incarceration on burglary and armed robbery proceeds instead by measuring charges and incarceration directly and ignoring the process relating the two. It is, however, necessary to separate offender manyears into groups relatively homogeneous in free charge rate by identifying characteristics of offenders and of their associated manyears related to the free arrest rate and use these variables to divide the sample. The number of groups, of course, is limited by the necessity of a sufficient number in each group to make a reliable estimate of the free arrest rate and percentage of time incarcerated. As we have demonstrated, the aggregate free arrest rate is an unbiased estimator of the true arrest rate, and the incapacitation effect is simply the average percentage of the manyear incarcerated. To estimate a total incapacitative effect on, for example, burglary, we calculate the burglary charges prevented in each group (charges prevented = (incapacitation percentage)(free burglary charge rate)(number of manyears making up

the group)) , sum these over all groups and divide this total by the sum of all group potential charges (charges potential for any group = free burglary charges rate X number of manyears making up the group.)

Selection of Predictor Variables

The levels of the incapacitation effect we found in the age and race analysis (Tables 3,4, and 6) lead us to differ with the Blumstein and Cohen argument (1979 p 566) that incarceration can be safely ignored in the estimation of arrest rates. The error introduced by substituting the observed arrest rate for the arrest rate while free makes the results of any exploratory regression analyses misleading. On the other hand, our analysis of the upward bias introduced by the tendency of arrests and incarceration to occur in the same manyear implies that free arrest rates cannot be calculated for individual manyears. We therefore cannot use arrest rate while free as a dependent variable in a regression analysis. We proceed instead by performing stepwise regression analysis on the subset of manyears for which there is less than 5% incarceration.[5]

Stepwise regression analysis of this no-custody group indicates that for burglary, prior history of index property offenses is the best single predictor, prior history of violent index offenses is the best predictor of armed robbery arrests, and for both burglars and armed robbers prior history of index arrests is the best predictor of index arrests.

Various formulations of these independent variables showed that average charges per active year is the best predictor. The

variable, prior charges, is adjusted by dividing it by the number of prior career manyears (years since first recorded arrest in the 1973-1986 study period.) We refer to these variables as "prior year Mu's".

Blumstein and Cohen (1979) define an offender manyear as a manyear for the calculation of crime-specific arrest rates only if the offender had a prior arrest for that crime. It seemed to us, however, that there is no a priori reason for considering a prior year arrest as a better indicator of current year criminal activity than an arrest in a subsequent year. Though an offender may not yet have begun committing the types of crime he will later be arrested for, he may have desisted commiting the type of crime for which he was previously arrested. (Indeed, Blumstein and Cohen suggest that offenders commit less variety of crime types as they age.) Our initial approach, therefore, was to define all manyears both prior and subsequent to a burglary or armed robbery arrest as active burglar or armed robber manyears.

When using the prior Mu variables in category form, however, we ob consistent U shaped pattern in the aggregate charge and arrest rates b Mu: the lowest and highest prior Mu groups yield the highest charge an arrest rates. The reason for this pattern lies in sample selection. sample of burglars was drawn from the population of offenders who had one recorded burglary arrest in New Orleans during the 13 year study p For manyears for which prior property Mu is zero, by definition the cu a subsequent manyear must include at least one burglary arrest. In ge the current manyear is likely to include a burglary arrest if no burgl has yet occurred in his career.

To remove the influence of this sample selection effect, we analyze manyears for which there is a prior burglary or armed

robbery arrest separately from manyears for which the first criterion arrest has not yet occurred.[6] The results are presented in tables 12 through 16. It is clear that prior property arrests is a good predictor of the free burglary rate. The burglary rate for the highest prior property Mu category is about five times the rate for the lowest category when we limit the manyears to those preceded by a burglary arrest. Incapacitation shows a monotonic pattern in the same direction, ranging from 12 to 37% as one moves from low to high free burglary rate groups. In fact, incapacitation is more strongly related to prior property Mu than it is to the free burglary rate, especially for blacks. This suggests that the criminal justice system responds to prior arrests as well as current ones, not a surprising finding given the dependence of decisions on pretrial detention, probation, parole, and sentencing on rap sheet information.

For burglary, though the incapacitation effect is higher for blacks than for whites at all burglary arrest rate levels, the effect is about equally elastic with respect to the burglary rate. The elasticity of incapacitation (calculated from lowest to highest prior Mu category) with respect to the burglary arrest rate is .61 for blacks and .58 for whites. Finally, the pattern of burglary Mu for the full sample demonstrates the selection effect when the prior burglary arrest restriction is lifted.

Just as the burglary charge rate can be predicted by prior property arrests, prior index Mu is a good predictor of the free index arrest rate, both for blacks and whites. Here also the incapacitation effect increases as the group free arrest rate rises and in fact, the elasticity of incapacitation with respect to arrest rate is greater than for burglary alone (blacks = .89,

whites = .92).

Turning to the armed robbers, it is clear that though prior violent Mu is a good predictor of incapacitation, it is a weak predictor of the free armed robbery rate. As with burglary, prior index Mu is a good predictor of the free index arrest rate for armed robbers and the incapacitation effect is positively related to the free index arrest rate.

Table 16 gives the improved measures of incapacitation effect derived from the disaggregation procedure. The procedure always increases the size of the effect, as high as 38% for white burglars. In general, burglary incapacitation effects are much better improved by the procedure than are the effects for armed robbery. For armed robbers, incarceration tracks their overall crime rate - as measured by the free index arrest rate. No conclusion can be drawn about the relationship of incapacitation to the free armed robbery rate because the attempt to create groups relatively homogeneous in armed robbery Mu was not successful.

Our calculation of incapacitation effects before disaggregation indicates that earlier empirically based estimates of the adult crime incapacitation effect were too low. Even though limited to one predictor and a control variable because of sample size, the disaggregation analysis confirms that assessment, since it demonstrates that the assumption of a constant arrest or offense rate leads to underestimates of the incapacitation effect. The criminal justice system does appear to better incapacitate the high frequency offender.

SUMMARY AND CONCLUSION: PHASE I

Methodological Issues

During the development of the official record database on which this study is based, we found it necessary to investigate carefully the process by which official records are produced. It is clear that the methods by which official histories are built and maintained produce a predominance of false negative over false positive error, and the problem of missing data grows much worse as one moves away from the stage of the initial arrest. Our arrest database is a combination of three official databases, merged only after a long and complex series of matching procedures on a variety of personal identifiers. Even with this effort, we are certain that the record of arrests is incomplete, and the evidence is strong that the missing data is not randomly distributed across race and age categories. If this is true of our database, it is certainly the case with others that have formed the basis of previous research. This is true especially if the "rap sheet" is from a single source, as most are. It is our experience that the majority of criminal justice officials are unaware of the limitations of the data they use on a daily basis, and criminal justice researchers generally depend on these practitioners to interpert the records. It is impossible to properly evaluate the effects of past or proposed law enforcement, sentencing, or treatment strategies as long as the criminal justice information systems in the United States remain in such disarray. It is in research professionals' own best interests to encourage and support efforts to improve the state of these information systems.

Rap sheets are deficient not only in their record of arrests but also in the dispositions of those arrests. We find that the best measures of incarceration are developed not from rap sheets but from the databases maintained by jail and correctional institutions. For this reason we measure actual jail and penitentiary time served, including readmissions for probation and parole violations, directly from the records of adult and juvenile institutions. As a result, this study describes the relative contributions of penitentiary, jail and juvenile institution to incapacitation of the offender for the first time. Some surprising findings emerge. Juvenile institutions, for example, make a significant contribution to young adult incapacitation because of the overlap of age jurisdiction for ages 17-20.

The levels of incarceration found in this study, and the significant differences in incapacitation by age and race, call into serious question any study which draws conclusions about the relationship of these variables to arrest rates without adjusting those rates for incarceration time. The differences we document in out-of-parish and out-of-state arrests by age and race casts doubt on any recidivism studies which rely on single-state rap sheet data or institutional readmissions as a dependent variable.

In general, this study should alert researchers to be more sensitive to measurement issues when using official criminal justice data.

Findings

Our results indicate that collective incapacitation effects exceed those reported in other empirically based estimates using official statistics. Under the assumption of a constant arrest rate, our most conservative estimates range from 12% for white

burglars to 27% for black armed robbers. These estimates, even under this limiting assumption, are higher than previous official record studies in part because we are able to count jail incarceration time and in part because of the more complete custody data developed in this project.

We find that incapacitation through incarceration is most effective for those aged 20-40 most ineffective in the juvenile and early adult years, when arrest rates indicate a high frequency of criminal activity. Armed robbers are better incapacitated than burglars and blacks better than whites.

We find no evidence whatsoever of crime type specialization. It is clear that the incapacitative effect of the criminal justice system on any one offense type is a complex function of the web of penalties and probabilities of incarceration of all other crime types. Modifications in the penalties - actual penalties imposed - for theft will have an impact on armed robbery and burglary rates, for example.

Our analysis of the effects of disaggregating the offender population into group's similar in offense rate frequency indicate that the criminal justice system already selectively incarcerates offenders. Even with the relatively limited disaggregation possible with the available predictor variables, incapacitation effects exceeding 40% were measured for moderately high frequency offenders. As a result, the true incapacitation effects for burglary and armed robbery are significantly higher than those measured under the assumption of a constant offense rate.

Phase II of the New Orleans Offender Study will make use of a much larger sample of offenders. Not only will much more detailed disaggregations be possible, but offender groups such as females

will be available for study.

Policy Implications and Future Research

The call for "selective incapacitation" as a means to a more efficient use of expensive prison resources implies that the present system of "collective incapacitation" indiscriminately incarcerates both the frequent and infrequent offender. There is little doubt that some resources are wasted on individuals who would be no risk if free, but it is clear from our analysis that the system does discriminate among offenders by better incapacitating groups with high offense rates.

The system's major weakness appears in it's treatment of age groups. Juvenile and young adult offenders are the highest rate offenders but the most poorly incapacitated. The reason lies, in part, in the more lenient standards applied to the juvenile offender by the separate system of juvenile justice. But another reason lies in the dependence of the system on prior criminal behavior as an indicator of offending potential, documented in our disaggregation analysis. This dependence creates a built-in lag between the onset of high frequency offending behavior and the resulting incapacitating effect. Thus, the system's method of identifying the high rate offender is ineffective for the young offender.

This finding leads us to recommend further investigation of the extent to which "just desserts" incarceration strategies would effect the crime rate by increasing the probability of incarceration for young, high frequency offenders and lowering it for much older, generally low frequency offenders. In general, future research needs to be done on the relationship of statutory penalties and criminal procedure to the actual incarceration times

of offenders, and of those incarceration times on the rates of all serious crimes. Only in this way can we determine the most effective policy initiatives for an impact on particular offenses.

TABLE 1

SIMULATION SUMMARY

S (months per incarceration)	Average Error		Average Percent Error		Average Years Lost/1000
	Avg mu	Agg mu	Avg mu	Agg mu	
3	.1777	.0051	19.2	1.0	2
6	.4940	.0046	42.3	0.4	20
9	.7493	.0069	75.4	1.2	56
12	.9117	.0044	88.8	1.4	97
15	1.0577	.0023	95.4	0.1	134
18	1.2437	.0103	122.3	0.5	171
21	1.2874	.0041	130.3	1.2	202
24	1.2643	.0063	128.0	0.1	236
27	1.3592	.0085	136.5	1.1	266
30	1.2936	.0079	126.0	1.0	294

TABLE 2

SAMPLE AND POPULATION - MANYEARS

Blacks			Whites		
1973-1980 Armed Robbers					
Age	Sample	Population	Age	Sample	Population
<16	542	2700	<16	93	96
17-19	878	4501	17-19	283	308
20-29	2838	9937	20-29	1176	1311
30-39	1602	2032	30-39	349	372
40-49	354	358	40-49	52	57
50+	59	59	50+	28	30
1981-1986 Armed Robbers					
Age	Sample	Population	Age	Sample	Population
18-19	128	594	18-19	37	38
20-29	1731	8422	20-29	696	759
30-39	1365	2969	30-39	390	427
40-49	474	500	40-49	69	73
50+	102	103	50+	26	28
1973-1980 Burglars					
Age	Sample	Population	Age	Sample	Population
<16	624	5364	<16	252	942
17-19	936	8395	17-19	441	1744
20-29	2977	18831	20-29	1590	4388
30-39	2000	4572	30-39	998	1195
40-49	445	969	40-49	276	295
50+	90	206	50+	147	155
1981-1986 Burglars					
Age	Sample	Population	Age	Sample	Population
18-19	123	1209	18-19	59	203
20-29	1856	17145	20-29	916	3585
30-39	1494	5948	30-39	727	1327
40-49	610	1196	40-49	314	329
50+	155	347	50+	138	147

TABLE 3

CUSTODY AND FREE ARREST RATES

BY AGE AND RACE

ARMED ROBBER MANYEARS 1973-1986

Age	DOC	PARISH JAIL	JUV	CUSTODY UNADJ.	CUSTODY PAR[1]	CUSTODY U.S.[2]	Index (free)	Arm.Rob. (free)
Blacks (1973-1980)								
<16	.000	.000	.036	.036	.036	.036	1.037	0.156
17-19	.110	.009	.013	.132	.133	.134	0.808	0.242
20-29	.245	.010	.000	.255	.256	.272	0.604	0.206
30-39	.219	.007	.000	.226	.227	.238	0.394	0.157
40-49	.181	.006	.000	.187	.187	.206	0.390	0.088
50+	.049	.006	.000	.055	.055	.056	0.201	0.074
Whites (1973-1980)								
<16	.000	.000	.030	.030	.030	.030	0.670	0.082
17-19	.081	.002	.007	.090	.091	.101	0.679	0.234
20-29	.170	.003	.000	.173	.174	.198	0.499	0.200
30-39	.122	.002	.000	.124	.124	.133	0.358	0.138
40-49	.064	.002	.000	.066	.066	.076	0.400	0.357
52+	.000	.000	.000	.000	.000	.000	0.370	0.140
Blacks (1981-1986)								
18-19	.161	.075	.050	.286	.289	.289	0.759	0.225
20-29	.219	.050	.001	.270	.275	.281	0.501	0.153
30-39	.175	.051	.000	.226	.233	.238	0.407	0.092
40-49	.120	.037	.000	.157	.162	.166	0.300	0.060
50+	.064	.017	.000	.081	.084	.084	0.251	0.076
Whites (1981-1986)								
18-19	.081	.019	.000	.100	.105	.112	0.428	0.180
20-29	.143	.034	.000	.177	.191	.215	0.382	0.115
30-39	.112	.030	.000	.142	.151	.160	0.440	0.143
40-49	.095	.032	.000	.127	.134	.136	0.266	0.116
50+	.045	.016	.000	.061	.061	.061	0.043	0.000

1. Adjusted for estimated jail incarceration time in other parishes. (see text.)
2. Adjusted for other parish jail time and for estimated out-of-state incareceration time. (See text.)

TABLE 4

CUSTODY AND FREE ARREST RATES

BY AGE AND RACE

BURGLAR MANYEARS 1973-1986

BURGLARS

Age	DOC	PARISH JAIL	JUV	CUSTODY UNADJ.	CUSTODY PAR[1]	CUSTODY U.S.[2]	Index (free)	Burglary (free)
Blacks (1973-1980)								
<16	.000	.000	.021	.021	.021	.021	0.797	0.439
17-19	.066	.012	.012	.090	.091	.093	0.650	0.298
20-29	.167	.010	.000	.177	.178	.184	0.490	0.221
30-39	.184	.008	.000	.192	.193	.203	0.402	0.176
40-49	.142	.013	.000	.155	.156	.167	0.468	0.204
50+	.217	.013	.000	.230	.231	.235	0.288	0.078
Whites (1973-1980)								
<16	.000	.000	.029	.029	.029	.029	0.814	0.598
17-19	.045	.003	.008	.056	.057	.060	0.553	0.426
20-29	.076	.004	.000	.080	.081	.092	0.374	0.330
30-39	.098	.003	.000	.101	.102	.117	0.396	0.170
40-49	.100	.001	.000	.101	.101	.113	0.428	0.192
50+	.105	.011	.000	.116	.117	.120	0.398	0.136
Blacks (1981-1986)								
18-19	.084	.067	.017	.168	.170	.178	0.596	0.292
20-29	.183	.048	.001	.232	.237	.240	0.408	0.171
30-39	.149	.036	.000	.185	.191	.194	0.347	0.112
40-49	.137	.042	.000	.179	.186	.190	0.309	0.198
50+	.089	.045	.000	.134	.139	.140	0.407	0.105
Whites (1981-1986)								
18-19	.120	.025	.002	.147	.158	.158	0.439	0.463
20-29	.077	.022	.000	.099	.110	.115	0.282	0.136
30-39	.090	.017	.000	.107	.113	.122	0.353	0.168
40-49	.100	.024	.000	.124	.129	.135	0.347	0.194
50+	.062	.015	.000	.077	.081	.082	0.349	0.087

1. Adjusted for estimated jail incarceration time in other parishes. (see text.)
2. Adjusted for other parish jail time and for estimated out-of-state incareceration time. (See text.)

TABLE 5

ARRESTS OUTSIDE NEW ORLEANS AND OUT-OF-STATE
BY RACE AND AGE

	PROPORTIONS					
	Arrests Outside N.O.	Arrests Outside LA	Born in LA		Arrests Outside N.O.	Arrests Outside LA	Born in LA
Black Armed Robbers	(1973-1980)				(1981-1986)		
<16	0.007	0.000	.96				
17-19	0.069	0.008	.94	18-19	0.040	0.000	.91
20-29	0.137	0.063	.86	20-29	0.128	0.023	.90
30-39	0.131	0.048	.86	30-39	0.161	0.023	.84
40-49	0.156	0.100	.89	40-49	0.148	0.023	.85
50+	0.059	0.012	.88	50+	0.167	0.000	.87
White Armed Robbers	(1973-1980)				(1981-1986)		
<16	0.033	0.000	.85				
17-19	0.471	0.107	.70	18-19	0.326	0.063	.56
20-29	0.406	0.139	.60	20-29	0.488	0.125	.60
30-39	0.219	0.076	.45	30-39	0.349	0.057	.54
40-49	0.315	0.148	.27	40-49	0.226	0.016	.31
52+	0.096	0.000	.44	50+	0.000	0.000	.36
Black Burglars	(1973-1980)				(1981-1986)		
<16	0.008	0.000	.96				
17-19	0.062	0.018	.95	18-19	0.068	0.045	.93
20-29	0.140	0.035	.87	20-29	0.123	0.012	.90
30-39	0.128	0.051	.89	30-39	0.173	0.013	.87
40-49	0.156	0.073	.87	40-49	0.179	0.024	.86
50+	0.061	0.017	.77	50+	0.120	0.010	.75
White Burglars	(1973-1980)				(1981-1986)		
<16	0.046	0.038	.81				
17-19	0.365	0.061	.81	18-19	0.457	0.000	.76
20-29	0.453	0.137	.67	20-29	0.519	0.049	.74
30-39	0.318	0.144	.43	30-39	0.400	0.076	.54
40-49	0.226	0.116	.40	40-49	0.233	0.048	.43
50+	0.083	0.030	.45	50+	0.249	0.017	.39

TABLE 6

INCAPACITATION EFFECTS BY RACE
ADULT MANYEARS

Manyears 1981-1986

	Blacks	Whites
Burglars		
Arrest Rates While Free: burglary	.16	.16
index	.40	.31
Incapacitation	.22	.12
N	4238	2154
Weighted to Pop.	25848	5593
Armed Robbers		
Arrest Rates While Free: armed robbery	.14	.12
index	.48	.38
Incapacitation	.27	.19
N	3800	1218
Weighted to Pop.	12589	1328

TABLE 7

CUSTODY COMMITMENTS
1981-1986

	state pretrial	sentenced jail	sentenced doc	municipal pretrial	municipal sentenced
Black Armed Robbers					
18-19	0.40	0.07	0.06	0.26	0.03
20-29	0.30	0.06	0.06	0.25	0.05
30-39	0.26	0.03	0.05	0.26	0.09
40-49	0.20	0.04	0.01	0.31	0.13
50+	0.16	0.01	0.01	0.50	0.23
White Armed Robbers					
18-19	0.16	0.03	0.13	0.11	0.03
20-29	0.15	0.02	0.06	0.12	0.03
30-39	0.21	0.02	0.03	0.31	0.09
40-49	0.20	0.01	0.03	0.64	0.66
50+	0.04	0.04	0.04	1.01	1.10
Black Burglars					
18-19	0.37	0.10	0.05	0.17	0.02
20-29	0.25	0.06	0.07	0.24	0.07
30-39	0.23	0.04	0.05	0.26	0.07
40-49	0.18	0.05	0.05	0.29	0.14
50+	0.20	0.06	0.03	0.38	0.18
White Burglars					
18-19	0.12	0.01	0.10	0.20	0.00
20-29	0.12	0.02	0.04	0.17	0.02
30-39	0.19	0.01	0.04	0.24	0.12
40-49	0.17	0.02	0.04	0.45	0.39
50+	0.15	0.01	0.04	0.59	1.08

TABLE 8

INDEX CHARGE RATES WHILE FREE
1973-1980

	murder	mansl	rape	unarm rob	armed rob	agg ass	burg	theft	auto theft
Black Armed Robbers									
<16	0.01	0.00	0.00	0.09	0.16	0.08	0.44	0.32	0.10
17-19	0.03	0.00	0.01	0.10	0.24	0.08	0.18	0.36	0.02
20-29	0.04	0.00	0.01	0.04	0.21	0.07	0.16	0.25	0.01
30-39	0.03	0.00	0.01	0.01	0.16	0.05	0.08	0.14	0.01
40-49	0.03	0.00	0.01	0.04	0.09	0.05	0.06	0.19	0.01
50+	0.00	0.00	0.00	0.00	0.07	0.05	0.00	0.15	0.02
White Armed Robbers									
<16	0.00	0.00	0.00	0.02	0.08	0.01	0.43	0.13	0.14
17-19	0.02	0.00	0.00	0.03	0.23	0.04	0.44	0.24	0.01
20-29	0.02	0.00	0.01	0.04	0.20	0.07	0.20	0.16	0.01
30-39	0.02	0.00	0.00	0.02	0.14	0.06	0.07	0.15	0.01
40-49	0.00	0.00	0.00	0.02	0.36	0.13	0.15	0.02	0.00
50+	0.00	0.00	0.00	0.00	0.14	0.07	0.03	0.23	0.00
Black Burglars									
<16	0.00	0.00	0.00	0.05	0.06	0.08	0.44	0.22	0.07
17-19	0.01	0.00	0.01	0.06	0.08	0.03	0.30	0.30	0.02
20-29	0.02	0.00	0.01	0.02	0.06	0.05	0.22	0.22	0.01
30-39	0.03	0.00	0.00	0.01	0.04	0.03	0.18	0.23	0.01
40-49	0.01	0.00	0.01	0.01	0.01	0.01	0.20	0.24	0.01
50+	0.00	0.00	0.03	0.00	0.01	0.01	0.08	0.22	0.00
White Burglars									
<16	0.00	0.00	0.00	0.02	0.00	0.03	0.60	0.16	0.15
17-19	0.01	0.00	0.01	0.01	0.02	0.04	0.43	0.18	0.01
20-29	0.01	0.00	0.01	0.01	0.02	0.04	0.33	0.13	0.01
30-39	0.01	0.00	0.00	0.03	0.02	0.03	0.17	0.20	0.01
40-49	0.02	0.00	0.00	0.01	0.02	0.01	0.19	0.21	0.02
50+	0.00	0.00	0.00	0.00	0.01	0.01	0.14	0.26	0.01

TABLE 9

INDEX CHARGE RATES WHILE FREE
1981-1986

	murder	mansl	rape	unarm rob	armed rob	agg ass	burg	theft	auto theft
Black Armed Robbers									
18-19	0.04	0.00	0.04	0.08	0.23	0.07	0.24	0.27	0.00
20-29	0.01	0.00	0.01	0.04	0.15	0.07	0.11	0.26	0.01
30-39	0.03	0.00	0.01	0.01	0.09	0.07	0.08	0.20	0.00
40-49	0.00	0.00	0.01	0.00	0.06	0.04	0.04	0.19	0.00
50+	0.01	0.00	0.01	0.01	0.08	0.02	0.00	0.17	0.00
White Armed Robbers									
18-19	0.00	0.00	0.00	0.00	0.18	0.06	0.16	0.15	0.00
20-29	0.01	0.00	0.01	0.01	0.11	0.06	0.11	0.22	0.01
30-39	0.02	0.00	0.02	0.05	0.14	0.06	0.08	0.21	0.01
40-49	0.02	0.00	0.00	0.00	0.12	0.03	0.05	0.10	0.00
50+	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.00	0.00
Black Burglars									
18-19	0.01	0.00	0.01	0.10	0.06	0.09	0.29	0.17	0.00
20-29	0.01	0.00	0.00	0.01	0.03	0.05	0.17	0.20	0.01
30-39	0.01	0.00	0.01	0.01	0.01	0.05	0.11	0.20	0.01
40-49	0.01	0.00	0.01	0.00	0.01	0.02	0.20	0.20	0.00
50+	0.01	0.00	0.01	0.02	0.00	0.05	0.10	0.27	0.01
White Burglars									
18-19	0.00	0.00	0.00	0.00	0.05	0.01	0.46	0.13	0.00
20-29	0.01	0.00	0.00	0.00	0.01	0.07	0.14	0.12	0.01
30-39	0.00	0.00	0.00	0.01	0.02	0.05	0.17	0.17	0.01
40-49	0.01	0.00	0.00	0.00	0.01	0.03	0.20	0.16	0.00
50+	0.01	0.00	0.00	0.00	0.00	0.02	0.09	0.24	0.01

TABLE 10

NON-INDEX CHARGE RATES WHILE FREE
1973-1980

	arson	stolen prop	weapon	drugs	marij	kid-nap	tres-pass	other
Black Armed Robbers								
<16	0.01	0.06	0.05	0.03	0.03	0.00	0.00	0.52
17-19	0.00	0.28	0.08	0.08	0.02	0.02	0.01	1.00
20-29	0.00	0.22	0.12	0.12	0.03	0.01	0.00	0.93
30-39	0.00	0.12	0.12	0.12	0.01	0.01	0.00	0.80
40-49	0.00	0.11	0.10	0.13	0.01	0.01	0.01	0.84
50+	0.00	0.05	0.05	0.00	0.00	0.00	0.00	2.04
White Armed Robbers								
<16	0.00	0.08	0.05	0.05	0.05	0.00	0.00	0.51
17-19	0.00	0.26	0.08	0.28	0.08	0.01	0.00	0.86
20-29	0.00	0.11	0.07	0.17	0.05	0.01	0.01	1.01
30-39	0.00	0.12	0.09	0.09	0.03	0.02	0.00	1.06
40-49	0.00	0.10	0.08	0.02	0.02	0.00	0.02	0.82
50+	0.00	0.10	0.03	0.17	0.03	0.00	0.00	0.71
Black Burglars								
<16	0.01	0.06	0.03	0.03	0.03	0.00	0.00	0.48
17-19	0.00	0.28	0.06	0.06	0.02	0.01	0.02	0.78
20-29	0.00	0.21	0.07	0.09	0.01	0.00	0.01	0.78
30-39	0.00	0.16	0.09	0.09	0.01	0.00	0.01	0.74
40-49	0.00	0.22	0.08	0.11	0.01	0.00	0.01	0.70
50+	0.00	0.14	0.05	0.04	0.00	0.00	0.01	1.31
White Burglars								
<16	0.00	0.08	0.05	0.02	0.05	0.00	0.00	0.49
17-19	0.01	0.26	0.06	0.20	0.05	0.00	0.01	1.01
20-29	0.01	0.12	0.07	0.14	0.03	0.00	0.02	1.00
30-39	0.00	0.11	0.08	0.10	0.01	0.00	0.00	1.39
40-49	0.01	0.18	0.05	0.10	0.00	0.00	0.00	1.51
50+	0.00	0.22	0.05	0.06	0.01	0.00	0.01	1.75

TABLE 11

NON-INDEX CHARGE RATES WHILE FREE
1981-1986

	arson	stolen prop	weapon	drugs	marij	kid-nap	tres-pass	other
Black Armed Robbers								
18-19	0.00	0.18	0.06	0.07	0.08	0.00	0.04	0.87
20-29	0.00	0.22	0.10	0.11	0.08	0.00	0.01	0.89
30-39	0.00	0.14	0.08	0.10	0.05	0.01	0.01	0.94
40-49	0.00	0.10	0.11	0.07	0.04	0.01	0.00	0.92
50+	0.00	0.08	0.07	0.03	0.02	0.00	0.00	1.30
White Armed Robbers								
18-19	0.00	0.12	0.03	0.07	0.12	0.02	0.00	1.04
20-29	0.00	0.11	0.09	0.08	0.06	0.01	0.01	0.94
30-39	0.01	0.10	0.07	0.07	0.05	0.01	0.00	1.17
40-49	0.00	0.09	0.13	0.08	0.01	0.00	0.00	2.34
50+	0.00	0.07	0.00	0.22	0.00	0.00	0.00	2.82
Black Burglars								
18-19	0.00	0.19	0.10	0.04	0.09	0.00	0.02	0.62
20-29	0.00	0.20	0.05	0.07	0.05	0.01	0.01	0.93
30-39	0.00	0.14	0.05	0.09	0.04	0.00	0.01	0.89
40-49	0.00	0.15	0.10	0.06	0.02	0.00	0.00	0.91
50+	0.00	0.16	0.05	0.02	0.01	0.00	0.03	1.09
White Burglars								
18-19	0.00	0.38	0.01	0.01	0.04	0.01	0.01	0.77
20-29	0.00	0.10	0.08	0.06	0.05	0.00	0.02	1.07
30-39	0.00	0.11	0.07	0.07	0.02	0.00	0.02	1.17
40-49	0.00	0.14	0.07	0.03	0.02	0.00	0.00	1.78
50+	0.01	0.13	0.01	0.13	0.01	0.00	0.03	2.55

TABLE 12

DISAGGREGATION BY PRIOR PROPERTY CAREER CHARGES AND RACE

BURGLARY CHARGES FOR BURGLAR MANYEARS 1981-86

Property Prior Mu	Prior Burglary Arrests Only N popn.	sam.	Incap.	Burglary Free Mu	All Burglar Manyears N popn.	sam.	Incap.	Burglary Free Mu
				TOTAL				
.00-.20	818	232	0.12	0.07	1639	388	0.08	0.19
.20-.40	1549	329	0.16	0.06	1704	772	0.16	0.10
.40-.60	1100	236	0.21	0.08	1239	266	0.21	0.11
.60-1.0	1095	248	0.30	0.23	1132	264	0.30	0.24
1.0+	1260	296	0.37	0.33	1319	312	0.37	0.32
				BLACKS				
.00-.20	668	148	0.13	0.07	1375	257	0.08	0.19
.20-.40	1250	206	0.18	0.05	1355	226	0.18	0.10
.40-.60	932	161	0.21	0.06	1056	184	0.21	0.09
.60-1.0	896	161	0.32	0.22	920	170	0.32	0.24
1.0+	1027	188	0.38	0.29	1061	196	0.38	0.29
				WHITES				
.00-.20	150	84	0.09	0.09	264	131	0.09	0.27
.20-.40	299	123	0.09	0.08	349	146	0.08	0.09
.40-.60	168	75	0.17	0.13	182	82	0.17	0.18
.60-1.0	199	87	0.15	0.24	212	94	0.15	0.23
1.0	232	108	0.31	0.49	257	116	0.29	0.46

TABLE 13

DISAGGREGATION BY PRIOR INDEX CAREER ARRESTS AND RACE

INDEX ARRESTS FOR BURGLAR MANYEARS 1981-86

Index Prior Mu	Prior Burglary Arrests Only N popn.	sam.	Incap.	Index Free Mu	All Burglar Manyears N popn.	sam.	Incap.	Index Free Mu
				TOTAL				
.00-.20	528	134	0.10	0.23	1100	248	0.07	0.32
.20-.40	1255	269	0.13	0.22	1553	326	0.12	0.27
.40-.60	961	208	0.21	0.28	1112	241	0.20	0.29
.60-1.0	1442	330	0.24	0.39	1491	348	0.24	0.42
1.0+	1635	400	0.38	0.89	1775	439	0.37	0.90
				BLACKS				
.00-.20	417	69	0.11	0.24	881	141	0.07	0.33
.20-.40	982	158	0.13	0.23	1233	193	0.12	0.29
.40-.60	790	139	0.23	0.30	921	162	0.22	0.30
.60-1.0	1212	223	0.25	0.39	1250	237	0.25	0.42
1.0+	1372	275	0.39	0.93	1481	300	0.38	0.95
				WHITES				
.00-.20	111	65	0.06	0.12	219	107	0.06	0.26
.20-.40	273	111	0.09	0.19	321	133	0.08	0.19
.40-.60	171	69	0.15	0.21	190	79	0.14	0.28
.60-1.0	230	107	0.16	0.42	241	111	0.16	0.43
1.0+	263	125	0.29	0.62	294	139	0.27	0.57

TABLE 14

DISAGGREGATION BY PRIOR VIOLENT CAREER CHARGES AND RACE

ARMED ROBBERY CHARGES FOR ARMED ROBBER MANYEARS 1981-86

Violent Prior Mu	Prior Arm. Robb. Arrests Only N popn.	sam.	Incap.	Arm. Robb. Free Mu	All Arm. Robb. Manyears N popn.	sam.	Incap.	Arm. Robb. Free Mu
				TOTAL				
.00-.20	484	208	0.22	0.05	997	406	0.18	0.39
.20-.40	753	307	0.28	0.03	914	355	0.28	0.06
.40-.60	487	178	0.36	0.02	532	193	0.35	0.05
.60+	508	193	0.42	0.07	556	205	0.39	0.10
				BLACKS				
.00-.20	432	161	0.23	0.05	871	290	0.18	0.41
.20-.40	685	244	0.28	0.03	836	282	0.28	0.06
.40-.60	443	137	0.36	0.02	486	150	0.35	0.05
.60+	458	147	0.43	0.06	506	159	0.40	0.10
				WHITES				
.00-.40	120	110	0.25	0.01	205	189	0.18	0.10
.40+	95	87	0.31	0.04	97	89	0.31	0.04

TABLE 15

DISAGGREGATION BY PRIOR INDEX CAREER ARRESTS AND RACE

INDEX ARRESTS FOR ARMED ROBBER MANYEARS 1981-86

Prior Arm. Robb. Arrests Only					All Arm. Robb. Manyears			
Index Prior Mu	N popn.	N sam.	Incap.	Index Free Mu	N popn.	N sam.	Incap.	Index Free Mu
				TOTAL				
.00-.40	439	208	0.21	0.23	759	343	0.15	0.45
.40-.60	449	180	0.27	0.23	549	216	0.24	0.32
.60-1.0	587	218	0.32	0.41	701	256	0.32	0.48
1.0+	759	280	0.40	0.75	989	342	0.37	0.81
				BLACKS				
.00-.40	378	154	0.21	0.18	642	238	0.15	0.47
.40-.60	412	146	0.27	0.25	502	172	0.24	0.33
.60-1.0	534	169	0.31	0.43	640	199	0.31	0.51
1.0+	694	220	0.41	0.78	912	270	0.37	0.83
				WHITES				
.00-.60	98	88	0.22	0.14	164	149	0.15	0.21
.60+	118	109	0.36	0.34	139	129	0.32	0.39

TABLE 16

DISAGGREGATION BY PRIOR CRIMINAL HISTORY AND RACE

SUMMARY OF RESULTS

INCAPACITATION EFFECTS

Manyears with Prior Criterion Arrests Only

	Aggregated	Dissaggregated
BURGLARY		
History Only (5 groups)	.24	.30
Blacks (5 groups)	.25	.31
Whites (5 groups)	.16	.22
History & Race (10 groups)	.23	.29
ARMED ROBBERY		
History Only (4 groups)	.32	.33
Blacks (4 groups)	.32	.33
Whites (2 groups)	.28	.30
History & Race (6 groups)	.32	.33
INDEX OFFENSES (BURGLARS)		
History Only (5 groups)	.24	.30
Blacks (5 groups)	.25	.31
Whites (5 groups)	.16	.20
History & Race (10 groups)	.23	.29
INDEX OFFENSES (ARMED ROBBERS)		
History Only (4 groups)	.32	.35
Blacks (4 groups)	.32	.36
Whites (2 groups)	.30	.32
History & Race		

(10 groups)	.32	.35

NOTES

1. The term "collective incapacitation" may be defined as the percentage of potential crime which is prevented by incarceration of offenders under current sentencing policies. The term "selective incapacitation" refers to crime reduction resulting from sentencing policies based on some formal predictive model which includes offender characteristics (such as age or drug addiction) and prior offender criminal behavior. Of course, justice system officials already attempt to be selective in their use of incarceration and this study demonstrates that they succeed to some extent in their attempt.

2. q = probability of arrest for an offense, J = probability of a sentence to incarceration given arrest, and S = expected length of sentence given incarceration.

3. To some extent our approach is similar to that of Blumstein and Cohen (1979) in their study of Washington, D.C. arrestees. Their criterion for the selection of their study sample was at least one arrest for an Index offense (excluding larceny) in D.C. in 1973. For a portion of their analysis they assumed that all offenders had been criminally active since age 18, but later resticted the sample to offenders who were actually arrested at ages 18, 19, or 20. This approach was intended to insure that all offenders were criminally active during the manyears included in the analysis.

4. The estimate of the adjustment factor for out of state

incarceration was developed as follows:

a) The incarceration rate of states where arrests most frequently occurred for sample offenders (Geerken, 1988, Table 8, p52) was compared to that of Louisiana. (See "Persons under Jurisdiction, State and Federal, on Dec 1, 1986" in Bureau of Justice Statistics, 1986, table 5A.) The states, their incarceration rates, and frequency of arrest occurrence in the study population are as follows:

	rate	frequency
California	218	716
Florida	273	549
New York	216	354
Texas	228	228
Georgia	282	188
All Other		1230

The overall United States rate is used for all other. (225)

b) This procedure yields a weighted out-of-state incarceration rate of 234. The ratio of this rate to the Louisiana rate (316) is .74, the adjustment rate for out-of-state incarceration. 5.We used 5% custody rather than 0% custody since we include pretrial time in our custody measure and some brief period of custody (if only one day) is always associated with an arrest. Therefore restricting the group to 0% custody would eliminate the possibility of arrests during the manyear.

6. The manyears included in the dissaggregation analysis are only a subset of the sample of manyears used to generate the incapacitation effects reported to this point. The reason lies in the construction of the prior Mu predictor variable. Many offenders had arrests recorded prior to 1973, though the arrest history databases (MOTION and FINDEX) could only be considered complete after 1973. For the earlier analyses, an arrest prior to 1973 could be used as a bracketing arrest and all manyears could be

counted from 1973 until the last recorded arrest. For the prior Mu variable, however, the bracketing arrest used is the first arrest on or after 1973, so that only manyears where recorded arrests were complete would be used. Prior Mu could only be calculated for years with prior bracketed manyears, so the sample of manyears was reduced accordingly.

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APPENDIX A

SIMULATION RESULTS DETAIL

SIMULATION RESULTS

S = 3 MONTHS

mu	yrs lost per 1000	OBSERVED VALUES average free mu	OBSERVED VALUES aggregate free mu	PROPORTION ERROR average free mu	PROPORTION ERROR aggregate free mu	Incapac-itation
0.05	0	0.0647	0.0482	0.294	-.036	0.005
0.10	0	0.1549	0.1092	0.549	0.092	0.011
0.15	0	0.1922	0.1665	0.281	0.110	0.016
0.20	0	0.2179	0.2029	0.089	0.014	0.014
0.25	1	0.3175	0.2757	0.270	0.103	0.033
0.30	1	0.4263	0.2898	0.421	-.034	0.025
0.35	0	0.3901	0.3375	0.115	-.036	0.030
0.40	0	0.4711	0.4235	0.178	0.059	0.038
0.45	2	0.5316	0.4568	0.181	0.015	0.041
0.50	0	0.5424	0.4879	0.085	-.024	0.038
0.55	0	0.5938	0.5319	0.080	-.033	0.041
0.60	0	0.7354	0.6341	0.226	0.057	0.049
0.65	0	0.6563	0.6102	0.010	-.061	0.047
0.70	2	0.8380	0.7374	0.197	0.053	0.066
0.75	1	0.8419	0.7636	0.123	0.018	0.058
0.80	2	0.9109	0.8150	0.139	0.019	0.071
0.85	1	1.0100	0.8267	0.188	-.027	0.060
0.90	0	1.2387	0.9137	0.376	0.015	0.071
0.95	1	1.1499	0.9275	0.210	-.024	0.074
1.00	1	1.1952	1.0400	0.195	0.040	0.085
1.05	1	1.5922	1.0606	0.516	0.010	0.085
1.10	1	1.2550	1.1084	0.141	0.008	0.093
1.15	3	1.3609	1.1357	0.183	-.012	0.099
1.20	1	1.4089	1.2355	0.174	0.030	0.101
1.25	0	1.4053	1.2260	0.124	-.019	0.093
1.30	0	1.6874	1.3655	0.298	0.050	0.104
1.35	1	1.4929	1.3116	0.106	-.028	0.104
1.40	4	1.5181	1.3399	0.084	-.043	0.118
1.45	3	1.6735	1.4726	0.154	0.016	0.115
1.50	2	1.7450	1.5104	0.163	0.007	0.121
1.55	4	1.7128	1.5487	0.105	-.001	0.118
1.60	4	1.9639	1.6613	0.227	0.038	0.132
1.65	2	1.8960	1.6341	0.149	-.010	0.137
1.70	2	1.9822	1.6853	0.166	-.009	0.145
1.75	3	1.9290	1.7039	0.102	-.026	0.129
1.80	3	2.1215	1.8544	0.179	0.030	0.142
1.85	6	2.0969	1.8591	0.133	0.005	0.148
1.90	3	2.2291	1.8953	0.173	-.002	0.151
1.95	6	2.2237	1.9816	0.140	0.016	0.156
2.00	5	2.3337	2.0141	0.167	0.007	0.164

SIMULATION RESULTS
S = 6 MONTHS

mu	yrs lost per 1000	OBSERVED VALUES average free mu	OBSERVED VALUES aggregate free mu	PROPORTION ERROR average free mu	PROPORTION ERROR aggregate free mu	Incapac-itation
0.05	0	0.0553	0.0491	0.106	-.018	0.003
0.10	0	0.1431	0.1060	0.431	0.060	0.019
0.15	3	0.2266	0.1532	0.511	0.021	0.027
0.20	5	0.2632	0.2098	0.316	0.049	0.033
0.25	4	0.3179	0.2312	0.272	-.075	0.041
0.30	9	0.4596	0.3162	0.532	0.054	0.058
0.35	13	0.4684	0.3382	0.338	-.034	0.063
0.40	11	0.4914	0.3803	0.228	-.049	0.074
0.45	12	0.5423	0.4418	0.205	-.018	0.068
0.50	7	0.6552	0.5064	0.310	0.013	0.079
0.55	17	0.7306	0.5489	0.328	-.002	0.099
0.60	12	0.7694	0.6102	0.282	0.017	0.104
0.65	21	0.8007	0.6305	0.232	-.030	0.121
0.70	11	0.9208	0.7118	0.315	0.017	0.107
0.75	19	1.1124	0.7602	0.483	0.014	0.134
0.80	10	1.0394	0.7670	0.299	-.041	0.115
0.85	22	1.4123	0.9052	0.662	0.065	0.145
0.90	25	1.5619	0.9213	0.735	0.024	0.156
0.95	16	1.4183	0.9690	0.493	0.020	0.159
1.00	24	1.2402	1.0028	0.240	0.003	0.153
1.05	22	1.3380	1.0536	0.274	0.003	0.156
1.10	15	1.7989	1.0675	0.635	-.030	0.148
1.15	23	1.5571	1.1760	0.354	0.023	0.173
1.20	19	1.7338	1.1533	0.445	-.039	0.159
1.25	30	1.7811	1.3130	0.425	0.050	0.195
1.30	32	1.9231	1.2814	0.479	-.014	0.208
1.35	20	2.1719	1.3158	0.609	-.025	0.173
1.40	23	1.9700	1.3905	0.407	-.007	0.197
1.45	28	2.1393	1.5086	0.475	0.040	0.195
1.50	37	2.1024	1.5209	0.402	0.014	0.216
1.55	30	2.0823	1.5598	0.343	0.006	0.208
1.60	30	3.0096	1.5794	0.881	-.013	0.227
1.65	33	3.2478	1.6666	0.968	0.010	0.238
1.70	22	2.4179	1.6565	0.422	-.026	0.214
1.75	34	3.0465	1.7057	0.741	-.025	0.252
1.80	35	2.5854	1.8230	0.436	0.013	0.241
1.85	25	2.5744	1.9170	0.392	0.036	0.249
1.90	32	2.5345	1.9338	0.334	0.018	0.258
1.95	46	2.4625	1.9769	0.263	0.014	0.271
2.00	25	2.6549	2.0238	0.327	0.012	0.247

SIMULATION RESULTS
S = 12 MONTHS

mu	yrs lost per 1000	OBSERVED VALUES average free mu	OBSERVED VALUES aggregate free mu	PROPORTION ERROR average free mu	PROPORTION ERROR aggregate free mu	Incapac-itation
0.05	9	0.1114	0.0587	1.228	0.174	0.030
0.10	9	0.2337	0.1173	1.337	0.173	0.036
0.15	15	0.2006	0.1404	0.337	-.064	0.047
0.20	21	0.2878	0.1861	0.439	-.069	0.066
0.25	29	0.5637	0.2714	1.255	0.086	0.082
0.30	31	0.4221	0.3162	0.407	0.054	0.085
0.35	42	0.6596	0.3618	0.885	0.034	0.121
0.40	28	1.0555	0.3804	1.639	-.049	0.096
0.45	58	0.7577	0.4726	0.684	0.050	0.148
0.50	62	0.9104	0.5152	0.821	0.030	0.164
0.55	65	1.3196	0.5414	1.399	-.016	0.181
0.60	70	1.3269	0.6295	1.212	0.049	0.197
0.65	60	1.1983	0.6627	0.844	0.020	0.178
0.70	88	1.2586	0.6831	0.798	-.024	0.219
0.75	77	1.0548	0.7540	0.406	0.005	0.205
0.80	82	1.2905	0.8126	0.613	0.016	0.222
0.85	89	1.4625	0.8741	0.721	0.028	0.230
0.90	102	1.3976	0.9318	0.553	0.035	0.260
0.95	96	1.9719	0.9584	1.076	0.009	0.266
1.00	121	1.4864	0.9743	0.486	-.026	0.277
1.05	111	1.7056	1.0379	0.624	-.012	0.288
1.10	100	2.0794	1.1446	0.890	0.041	0.285
1.15	112	2.2022	1.1411	0.915	-.008	0.290
1.20	91	2.7653	1.2286	1.304	0.024	0.285
1.25	141	2.3195	1.2472	0.856	-.002	0.323
1.30	142	3.0440	1.3302	1.342	0.023	0.340
1.35	135	2.4619	1.3455	0.824	-.003	0.356
1.40	138	2.8785	1.3974	1.056	-.002	0.326
1.45	138	2.4534	1.3918	0.692	-.040	0.351
1.50	137	2.3398	1.4826	0.560	-.012	0.348
1.55	125	2.8886	1.5136	0.864	-.023	0.348
1.60	141	3.3176	1.6850	1.073	0.053	0.367
1.65	146	2.9220	1.6719	0.771	0.013	0.381
1.70	148	3.7576	1.7160	1.210	0.009	0.400
1.75	143	3.0749	1.8301	0.757	0.046	0.384
1.80	189	4.0551	1.7947	1.253	-.003	0.430
1.85	169	3.3567	1.8014	0.814	-.026	0.416
1.90	137	2.6826	1.8436	0.412	-.030	0.386
1.95	147	3.8846	1.9717	0.992	0.011	0.405
2.00	145	4.3077	1.9572	1.154	-.021	0.400

SIMULATION RESULTS
S = 30 MONTHS

mu	yrs lost per 1000	OBSERVED VALUES average free mu	OBSERVED VALUES aggregate free mu	PROPORTION ERROR average free mu	PROPORTION ERROR aggregate free mu	Incapac-itation
0.05	51	0.1050	0.0557	1.100	0.114	0.068
0.10	45	0.2402	0.0904	1.402	-.096	0.071
0.15	61	0.3049	0.1424	1.033	-.051	0.101
0.20	100	0.3652	0.2078	0.826	0.039	0.153
0.25	134	0.4012	0.2658	0.605	0.063	0.203
0.30	135	0.7031	0.2899	1.344	-.034	0.216
0.35	158	1.0876	0.3440	2.107	-.017	0.230
0.40	207	0.9607	0.4373	1.402	0.093	0.293
0.45	210	0.8284	0.4560	0.841	0.013	0.290
0.50	196	1.4296	0.5436	1.859	0.087	0.296
0.55	249	1.1423	0.5441	1.077	-.011	0.362
0.60	192	0.9110	0.5694	0.518	-.051	0.296
0.65	247	2.0305	0.6774	2.124	0.042	0.359
0.70	256	1.6530	0.7038	1.361	0.005	0.389
0.75	257	1.3099	0.7828	0.747	0.044	0.381
0.80	261	2.2419	0.8060	1.802	0.007	0.403
0.85	287	2.4104	0.8577	1.836	0.009	0.438
0.90	291	1.6453	0.9393	0.828	0.044	0.441
0.95	311	1.7645	0.9451	0.857	-.005	0.458
1.00	329	2.4971	0.9898	1.497	-.010	0.490
1.05	366	2.9846	1.0831	1.842	0.032	0.523
1.10	348	2.5796	1.0741	1.345	-.024	0.504
1.15	338	2.3525	1.1813	1.046	0.027	0.515
1.20	351	3.5537	1.2290	1.961	0.024	0.534
1.25	337	3.2334	1.2549	1.587	0.004	0.510
1.30	370	2.4657	1.2983	0.897	-.001	0.540
1.35	370	2.5780	1.4488	0.910	0.073	0.564
1.40	352	2.9584	1.4357	1.113	0.026	0.548
1.45	383	3.5088	1.4835	1.420	0.023	0.581
1.50	357	2.7279	1.4280	0.819	-.048	0.540
1.55	435	3.0949	1.5211	0.997	-.019	0.619
1.60	395	3.1082	1.5654	0.943	-.022	0.592
1.65	388	3.4813	1.6226	1.110	-.017	0.586
1.70	451	4.1674	1.7449	1.451	0.026	0.644
1.75	412	3.5458	1.7485	1.026	-.001	0.619
1.80	426	4.7976	1.8418	1.665	0.023	0.627
1.85	413	4.5921	1.8409	1.482	-.005	0.622
1.90	410	3.7963	1.9675	0.998	0.036	0.633
1.95	470	3.5272	1.8834	0.809	-.034	0.655
2.00	411	5.6574	2.0142	1.829	0.007	0.633