83-IJ-CX-04

AN EVALUATION OF THE IMPACT ON REINCARCERATION
OF CALIFORNIA SENATE BILL 224:
A FINAL REPORT TO THE NATIONAL INSTITUTE OF JUSTICE

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CONTENTS

I.EXECUTIVE SUMMARY

It is well known that a large proportion of individuals released from prison soon return with new convictions. However, cognizance of the problem is not matched with much understanding about what can be done about it. Despite a wide variety of strategies that may work in principle, there are to date no strategies that can be relied upon in practice. At best, there are some promising prospects in need of further scientific evaluation. In this final report, we focus on one such prospect: short-term financial support for ex-prisoners, conceptualized as "transitional aid."

The justification for transitional aid is simply summarized. Few would dispute that most individuals upon release from prison experience significant financial hardship. In addition, few would dispute that such hardship can easily and directly provide motivation for crime or that stress related to economic hardship may contribute to criminal activity indirectly. Transitional aid, in the form of small cash payments spread over several months, may be understood as a means to reduce the economic hardship experienced by ex-prisoners shortly after release and as a result, reduce the likelihood of new crimes.

Within the past decade, there have been several rigorous attempts to evaluate the impact of transitional aid for ex-prisoners (Lenihan, 1977; Rossi, et al., 1980; Berk and Rauma, 1983). As Glaser notes (1983: 220-234), the results are mixed. While in <u>no</u> instances has transitional aid been linked to overall

<u>increases</u> in recidivism, beneficial effects have not been consistently demonstrated.

The study described in this report builds directly upon earlier evaluations of transitional aid and especially our previous work (Berk and Rauma, 1983; Rauma and Berk, 1982). We had earlier evaluated, with a regression-discontinuity design, the impact of legislation in California making felons in state prisons eligible for unemployment compensation. Eligibility was formally earned by working at prison jobs and/or participating in prison vocational training programs for a total of at least 600 hours. Eligible prisoners, who could not find employment after release, could then receive "Former Inmate Insurance," much as other unemployed individuals obtain unemployment benefits.

Within the evaluation's regression-discontinuity design, eligible prisoners (i.e., those accumulating 600 hours or more) were treated as the experimental group, while ineligible prisoners (i.e., those accumulating less than 600 hours) were treated as the control group. Using records from the California Department of Corrections (CDC), a post-release "failure" was defined as a reincarceration in a state facility within ten months after release. Logistic regression revealed that after ten months, the proportion of eligible ex-prisoners who were reincarcerated was 13 percent lower than the proportion of ineligible ex-prisoners who were reincarcerated. This difference was statistically significant at the .10 level and easily made the program cost-effective.

One important question raised by the evaluation was whether the unemployment benefits really reduced the number of crimes, rather than merely postponing them. By law, the FI payments could not be collected for more than 26 weeks, and most ex-prisoners taking advantage of the program did so very soon after release. The research we report here exploits the data from our earlier evaluation, but through data available from the FBI, extends the followup period to 60 months. The FBI data also allows us to consider arrests outside the State of California.

The outcome data available from both the California Department of Corrections and the Federal Bureau of Investigation are not without serious flaws. For example, a large number of CDC failure are not among the FBI failures. Nevertheless, there is no reason to suspect that these and other errors are differentially distributed between the experimental and control groups, and consequently, no reason to suspect biased estimates of any treatment effects as a result of measurement error in the outcome variable.

By and large, the results from the extended followup period using the FBI data are very similar to the earlier results based on a ten month followup and CDC data. Comparisons across the two studies are complicated because both studies employed a number of different statistical procedures leading to slightly different results. However, three conclusions can be drawn.

First, the statistical power associated with the new study is stronger than the statistical power associated with the old study. This derives primarily from the much longer followup period provided by the FBI data.

Second, we are able to reject the null hypothesis of no treatment effect as least as well in the new study as the old. Depending on the analysis considered, we are able to obtain (for a one-tailed test) p-values ranging from .10 to .005.

Third, the treatment effects are apparently smaller overall for the 60 month followup than for the ten month followup. Using a discrete data approximation of a Cox proportional hazard regression, the treatment coefficient declines from around -.50 to around -.25. This means that for the 60 month followup, the odds multiplier is approximately .77, while for the ten month followup, the odds multiplier is about .60. In other words, for the longer followup, the odds of failure for the experimentals are about 77 percent the odds of failure for the controls. For the shorter followup, the odds of failure for the experimentals are about 60 percent of the odds of failure for the controls. Nevertheless, the smaller treatment effect is non-trivial and implies that the program was highly cost-effective.

We can only speculate about why the treatment effect is smaller for the longer followup period. There are surely data problems that in principle could lead to attenuated results. However, there is some evidence in our data that the effectiveness of the program declines dramatically after about

three years from the date of release. One reason may be that by then, the pool of ex-prisoners remaining on the street has been stripped of the majority of high risk individuals. In other words, ex-prisoners who have not been reincarcerated after three years are perhaps rather likely to stay out of trouble. As a result, there is very little recidivism left to prevent, and any treatment effect necessarily will be modest.

Given all of the data problems and the vulnerabilities of the regression-discontinuity design, the beneficial effects of the California FI program that we report must be treated cautiously. We believe that our results shift the weight of evidence still further in support of the transitional aid as a way to reduce recidivism.

However, it is probably premature to endorse unequivocally the California program or the transitional aid approach more generally. In particular, past research (Rossi, et al., 1980; Berk, et al., 1980) suggests that transitional aid may have two counterbalancing effects. On the one hand, the financial support may reduce some of the motivation to commit new crimes. On the other hand, the financial support may produce work disincentives, which in turn can lead to unemployment and an increase in the motivation to commit new crimes. Hence, successful transitional aid programs must provide the right amount of money under the right conditions to make the beneficial effects larger than the harmful effects. What is needed, therefore, is a package of randomized experiments in which the nature of the transitional

aid can be manipulated. For example, one could vary a) the amount of money given per week, b) the number of weeks of coverage, c) the tax rate by which earnings are deducted from the payments, and d) the time delay between the application for benefits and when the benefits are received. Only when the results of such experiments are available, will it be possible to make sensible policy recommendations.

II. INTRODUCTION

The history of criminology is littered with failed attempts to reduce recidivism. It is probably fair to say that no single strategy has proved especially successful. Rehabilitation efforts within prisons have yet to produce anything that might be routinely integrated into public policy (Sechrest et al., 1979), while post-prison programs have shown mixed results at best (Glaser, 1983). But among the more promising post-prison strategies are interventions that provide short term transitional aid to offenders shortly after release from prison.

In particular, the LIFE Experiment (Lenihan, 1977), using random assignment to experimental and control conditions, revealed that ex-prisoners provided with small weekly payments (roughly comparable to unemployment benefits) were about ten percent less likely to be arrested for new crimes than ex-prisoners given job counseling or no treatment whatsoever. In the randomized TARP experiment, Rossi, Berk and Lenihan (1980) found

no such reductions overall, but presented supplementary analyses suggesting that transitional aid could reduce recidivism if work disincentives associated with the payments could be substantially reduced. Succinctly put, the TARP payments apparently induced unemployment leading to criminal behavior <u>and</u> a counterbalancing reluctance to commit new crimes. Finally, Berk and Rauma (1983), employing a regression-discontinuity design, found recidivism reductions comparable to those found in the Life Experiment. (See also Rauma and Berk, 1982.) Reviewing these and other studies, Glaser concludes (1983: 227), "Financial aid for offenders supervised in the community achieves most if provided in a manner that is not a disincentive to work...".

The Berk and Rauma study is of special interest here. First, unlike the LIFE and TARP experiments, the intervention was real program mandated by state legislation. Hence, the program represented a strategy that was politically and practically feasible. Second, in this report we present the results of a new analysis of the Berk and Rauma data. Perhaps most important, the post-prison followup period is extended from ten months to sixty months; there is the possibility of exploring effects beyond the first year after release. In addition, FBI records are added to state records allowing for the identification of crimes missed in the original study.

In section III, we describe the legislation mandating the program. Section IV focuses briefly on the research design, while section V describes data collection. Section VI considers the

empirical results, finally, section VII contains conclusions and policy recommendations.

III. THE ENABLING LEGISLATION: CALIFORNIA SENATE BILL 224

If life is hard within prisons, it is hardly a picnic afterwards. Ex-prisoners face myriad obstacles in the transition to law-abiding behavior, with financial obstacles among the most significant (Lenihan, 1977; Silberman, 1978: 117-158). In virtually all states, gate money and earnings from prison work rarely amount to little more than loose change, while public assistance of various kinds is typically unavailable (e.g., AFDC). Moreover, few ex-prisoners have marketable skills, even if the stigma of a prison record were not sufficient to close many doors. It is not surprising, therefore, that many return to crime, perhaps as an alternative to a legitimate job (Becker, 1968; Ehrlich, 1973; Block and Heineke, 1975; Rossi, et al., 1980; Berk, et al., 1980).

In part as a response to such problems, the California legislature, in 1977, passed Senate Bill 224. Under the sponsorship of Senator Peter Behr, the bill mandated that beginning in July of 1978, individuals upon release from prison could apply for unemployment insurance. A detailed account of how the program was supposed to function can be found in a CDC Administrative Bulletin reproduced in Appendix A. To summarize briefly, eligibility was to be obtained by working at prison jobs

or by participating in prison vocational programs after January 1, 1977. For both kinds of activities, nominal earnings were accumulated at the rate of $2.50 an hour (the real rate was closer to 20 cents). If over a twelve month period earnings totaled more than $1500, an ex-prisoner who could not find work could apply for benefits at his or her local unemployment office, much like any other citizen. The amount of benefits received depended upon the hours worked in prison, with the effective range of support between $30 and $70 a week for up to 26 weeks.

All prisoners were told when released that they might be eligible for special FI (former inmate) unemployment benefits. It was very unlikely that officials providing such information knew or the prisoners themselves knew who was really eligible. Each prison kept its own eligibility records, which were not readily accessible. Indeed, when time came for the California Department of Corrections to report how the program's authorized funds were being spent, Department officials had to mount a special effort to collect eligibility figures.

When an ex-prisoner reported to his/her local unemployment office, eligibility was determined through a records search. Since there was no single place where the necessary materials were stored, requests for information had to be made to the prison(s) in which the applicant was previously incarcerated. As a result, it often took well over a month for payments to be received.

For individuals receiving benefits, the usual regulations applied. Recipients had to be ready to accept jobs, had to be seriously looking for work, and were subject to the standard agency forgiveness provisions. An upper limit of $25 a week could be earned with no reductions in payments, but, for earning in excess of $25 a week, weekly payments were reduced dollar for dollar.

By all accounts, SB 224's "Former Inmate Unemployment Insurance Program" was very popular with ex-prisoners. According to figures provided by the California Employment Development Department (Report 650, August 20, 1984), an estimated 17,200 valid claims were processed between July of 1978 and July of 1984. [1] The average number of weeks on support was a little over 13, with average benefits of $52.36 per week. The total cost of these benefits over the life of the program was approximately $12 million. No figures were ever provided on the program's administrative expenses (we doubt they were ever calculated), but they were probably not large in relative terms. Claims were handled in a routine fashion by State Unemployment Offices, and there was no evidence of large administrative costs resulting from record keeping practices within prisons. We suspect, therefore, that administrative costs increased total costs by no

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[1] Note that the period covered exceeds the duration of the program. This is because it often took several months to process FI claims. Hence, claims filed near the end of the program would not show up for a while in official statistics.

more than 25 percent. Consequently, the total bill for California taxpayers was about $15 million.

While the SB 224's FI insurance was hardly cheap, even small reductions in reincarceration attributable to the program would have made the program cost-effect. At the time, a year in prison cost the State of California about $15,000. Thus, the program would break even during its lifetime if over five years, 1000 prison-years were prevented.

There are a number of different and plausible patterns that might achieve such reductions. For example, assuming a modal term of two years, only 500 new imprisonments would have to be averted in under five years. With well over 20,000 prisoners released yearly, and postulating a baseline reincarceration rate within 12 months of about 30 percent (see our descriptive data below), 6000 individuals would ordinarily be returned to prison. If, as a result of the FI insurance, 500 of these individuals (about eight percent) managed a successful reintroduction into society, the program would pay for itself <u>in the first year alone</u>. Given five year reincarceration rates of over 50 percent, and five years to accumulate savings, it is clear that reductions of about one percent a year in reincarceration rates would easily pay for the program. And, this ignores all of the other costs associated with crimes that would be eliminated (e.g., costs to victims). Clearly, SB 224 could be a dramatic financial success with even small effects on reincarceration rates.

Few would deny that the benefits program mandated by Senate Bill 224 was at least unusual. Perhaps even more innovative was the requirement of an impact assessment. There was particular interest among legislators in whether individuals who received unemployment benefits were less likely to recidivate and, in these terms, whether the program was cost effective. In fact, Senate Bill 224 was written as "sundown legislation" with a five year lifespan (i.e., from July 1, 1978 to July 1, 1982, although claims could be paid until November 1, 1983). [2] Presumably, any efforts to reintroduce the program would have to take account of the evaluation findings.

Unfortunately, the strong legislative language requiring an impact assessment was not matched with a commitment to a randomized experiment. Yet, since eligibility was fully determined by a single threshold in nominal prison earnings, the legislation generated de facto a regression-discontinuity design. The regression-discontinuity approach is perhaps the strongest quasi-experimental design known, and it is to the properties of the design that we now turn.

IV. THE PROPERTIES OF A REGRESSION-DISCONTINUITY DESIGN

Justification for the regression-discontinuity design can be found in Rubin's concept of "ignorability" (1978). Imagine as in

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[2] The additional time was to allow for prisoners earning eligibility to be released and draw benefits.

Figure 1 that some response variable R (e.g., a felony arrest) is a function of the presence or absence of some treatment T (e.g., eligibility for unemployment benefits), a set of X covariates (e.g., prior record, age, marital status, etc), and a disturbance process e_2 meeting the usual OLS assumptions. Also imagine that assignment to the treatment and control conditions depends on X and some disturbance process e_1 meeting the usual OLS assumptions. Finally, we assume that the two disturbance processes are unrelated.

For the assignment process to be ignorable, <u>all</u> of the covariates included in X must be known, measured and used in any analysis of the impact of the treatment T on the response R. Thus, if the analysis is undertaken with multiple regression, the set of variables in X would define the full set of required regressors.

Note that in Figure 2, variables such as those represented by Z, which affect the response but <u>not</u> the assignment T, can be safely ignored. Since the Z's do not affect <u>both</u> T and R, unbiased estimates of the impact of T on R can be obtained even if the Z's are not taken into account. (However, including the Z variables in an analysis of any treatment effects will improve one's statistical power.)

For ex post facto designs, it is very difficult to demonstrate that all of the covariates in X have been included. That is, when the researcher is simply presented with a set of observational data, the specter of specification error hangs

PATH DIAGRAMS
ILLUSTRATING IGNORABILITY

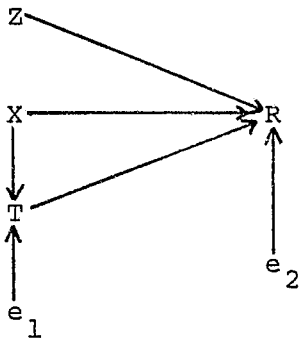### FIGURE 1
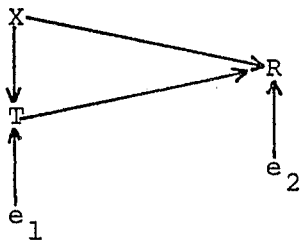
$T = f(X) + e_1$

$R = f(X, T) + e_2$

$Cov(e_1 e_2) = 0$

FIGURE 2
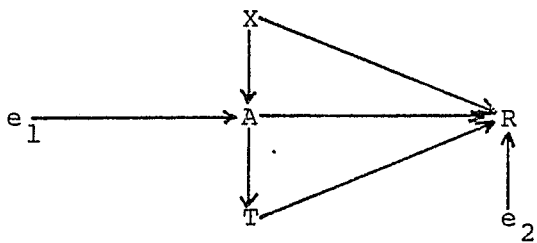

$T = f(X) + e_1$

$R = f(X, Z, T) + e_2$

$Cov(e_1 e_2) = 0$

FIGURE 3

$A = f(X) + e_1$

$T = f(A)$

$R = f(X, A, T) + e_2$

heavy over all substantive interpretations. And unfortunately, ex post facto designs dominate the criminological literature.

The best way to minimize the possibility of specification error is to employ random assignment to treatment and control conditions. Looking back at Figure 1, the link between X and T is cut; the assignment mechanism is solely a function of some chance process represented by $e1$. Consequently, the X variables are effectively transformed into the Z variables shown in Figure 2. Omitting the X's can then only reduce one's statistical power.

A weaker, but still potent alternative is to assign not by some chance process, but deterministically. Looking at Figure 3, the A represents some assignment variable (or variables) interposed between the X and T. Under SB 224, prison earnings is just such a variable. If a subject's value on A, or a scaler combination of A's, falls below (above) some threshold, the subject is given the control condition. If the subject's value falls on or above (below) the threshold, the subject is given the experimental condition. Since the impact of X on T is funneled completely through A, holding A alone constant in a multivariate analysis of the impact of T on R will in principle yield unbiased estimates (Rubin, 1977).

The catch is that the functional form of the relationship between A and R must be closely approximated. As stressed by Cook and Campbell, 1979: 137-142) a failure to properly specify the functional form between A and R may well produce "pseudo-effects." As Rubin (1977) explains, the generic problem is that

the regression of R on A must for the experimentals be extrapolated into the region of the controls, while for the controls, the regression of R on A must be extrapolated into the region of the experimentals. This is because there are no experimentals below (above) the threshold on A, and no controls above (below) the threshold on A. Thus, there are no data to test directly the appropriateness of the extrapolations.

The regression-discontinuity approach also produces less statistical power than random assignment. The inevitable correlation between the assignment variable and the treatment dummy variable will increase the variance of any estimates of the treatment effect. As a result, one typically need far larger samples and/or bigger treatment effects, then under random assignment.

To summarize (see also Berk and Rauma, 1983), the regression-discontinuity design, while weaker than a true experimental design, is far superior to usual ex post facto design in which the assignment process is typically not well understood. For the evaluation of the impact of SB 224, our assignment variable (A) is prison earnings. The covariates (X) that may be solely used to increase statistical power will include such variables as age, sex, race, prior record, and drug history. The response variable (R) will be the elapsed time between release and a return to prison for a new offense. In other words, we will analyze our regression discontinuity design

within a failure-time statistical framework. Justifications for this approach will be introduced later.

V. DATA COLLECTION

While the mandated evaluation in SB224 addressed several different outcome measures, it was clear that as a political matter, recidivism was the only outcome that really counted. That is, prisoner eligibility for unemployment benefits would have to be justified by reducing crime.

Data collection was undertaken in two phases. Working under a grant from the U.S. Department of Labor, we obtained Phase I data during 1980 and 1981. The nature and limitations of these data are discussed immediately below. We applied for funds to NIJ to improve the database constructed in Phase I, and Phase II followed. Our Phase II efforts will be discussed following our consideration of Phase I.

The Phase I data reflect an historical period when virtually all prisoners were released on parole. Recidivism in the short run would, therefore, necessarily involve a parole violation, which was in principle recorded on records maintained by the California Department of Corrections. Therefore, a "failure" was defined conceptually as any of the following;

    1. a felony offense resulting in parole revocation and/or
return to prison;
    2. a parolee at large (PAL), resulting in parole revocation
and/or return to prison;

3. technical violations resulting in parole revocation and/or return to prison;

4. misdemeanors resulting in parole revocation and/or return to prison.

All other kinds of violation were not defined as failures because they were far less serious (e.g., vehicular offenses), or because their legal status was unclear (e.g., for arrest and release). That is, we wanted to define failure in a manner that would be truly responsive to the concerns of policy makers. In short, our outcome measure for the Phase I CDC data was basically a parole revocation that would, in principle, result in <u>a return to prison</u>. 3

When the Phase I research design was developed in the fall of 1980, we were seeking to obtain a sample sufficient to find effects of the size surfacing in earlier research (i.e., reductions in recidivism of about ten percent). Thus, the target was a sample size of 1000, with no worse than an .80-.20 split between the experimentals (i.e., those eligible for the benefits) and the controls (i.e., those not eligible for the benefits). We also planned for a 12 month followup since that would almost certainly allow enough time for any short term effect to appear and would generate results <u>well before the FI program would be reconsidered by the legislature</u>.

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3 Often, time spent in jail prior to a revocation is subtracted from one's sentence. Consequently, it was possible to have a parole revoked, have the original sentence reimposed, but still not serve time in a State prison.

Working closely with the California Department of Corrections, we obtained access to data on offenders released between July of 1978 and December of 1980. Data collection was actually begun for the interval beginning in September to allow for the usual "shakedown" period. We were able to obtain a large sample of experimentals within the first seven months of data collection. However, a variety of bureaucratic obstacles made it more difficult to gather a sample of the controls and therefore, some of our controls entered the study nearly a year beyond the July 1980 start date. We eventually wound up with 920 experimentals and 255 controls. (More details can be found in Berk and Rauma, 1981.)

Unfortunately, the CDC records used to define failure were flawed in two important ways (beyond the usual kinds of errors one can expect to find in administrative files). First, since parole periods did not last more than 2 years, the followup period was limited to 24 months. At the time, we felt that since the intervention was in place for a relatively short period (up to 26 weeks for a given individual), a 12 month followup period would be sufficient. Surely, the total impact of several months of payments at about $60 a week would be felt within a year. However, if one were interested in long terms effects, such as whether crimes were being postponed rather than eliminated, the maximum followup period of 24 months was a serious constraint.

Second, the CDC records did not contain information on failures occurring outside the State of California. This meant

new crimes committed by especially mobile ex-offenders would be overlooked.

When we submitted a proposal to NIJ for a Phase II project that would extend the followup period to 24 months, a reviewer suggested that we employ FBI "rap sheet" data to address long term and out-of-state reincarcerations. NIJ concurred, and additional funds were provided to acquire and analyze the data.

We began our efforts to obtain the requisite materials in January of 1984. Names and CDC numbers from our sample of released felons (obtained in Phase I) were given to the California Department of Corrections with a request that 6, 12, and 24 month parole follow-up files be supplied. The request was fulfilled in the summer of 1984. Additional identification numbers were also supplied by CDC for this sample, allowing requests for rap sheets to the U.S. Department of Justice, Bureau of Identification.

Since these requests were being officially made by CDC, all requests and all BID data had to flow through CDC. In other words, in order to avoid the BID charge for this information, CDC acted as a go-between. Written requests, in duplicate, were sent to CDC in September, 1984, to be forwarded to the Bureau of Identification. However, because of errors and omissions in the information originally sent by CDC, the majority of the requests had to be re-processed. While the original requests were computer-generated, the re-processing was done by hand. We received the first of the BID rap sheets from CDC in February,

1985, and the remainder were received over the next seven months. The last of the data included in this report arrived in late August, 1985.

If the FBI data had proved to be simply a complement to the CDC data, all would have been well. Unfortunately, efforts to define FBI failures in the same manner as CDC failures led to troubling anomalies. Figure 4 shows the number of failures coded by the length of the followup (in months) and by the source of the failure information. The plus signs represent failures appearing only in the FBI data. The diamonds represent failures appearing only in the CDC data. Finally, the squares represent failures that appeared in both the FBI and CDC data.

One important difference between the CDC and FBI data apparent in Figure 4 is that since the CDC data do not follow a parolee for more than 24 months, the CDC-only curve is flat after 24 months. In addition, however, there are large disparities between the CDC and FBI data even in the first 24 months. For example, during the first six months after release, there are 128 failures in the FBI-only data, 247 failures in the CDC-only data and 104 failures in both datasets.

We cannot fully explain such discrepancies. Of course, the FBI data should reflect both failures in California and in other states, while the CDC data should only reflect failures in California. But this implies that the number of FBI failures must always exceed the number of CDC failures. Such is not the case.

FIGURE 4

Comparison of CDC and FBI Failure Times

Number of Failures, by Type

Six-Month Failure Periods

□ CDC-FBI Failures   + FBI Failures   ◇ CDC Failures
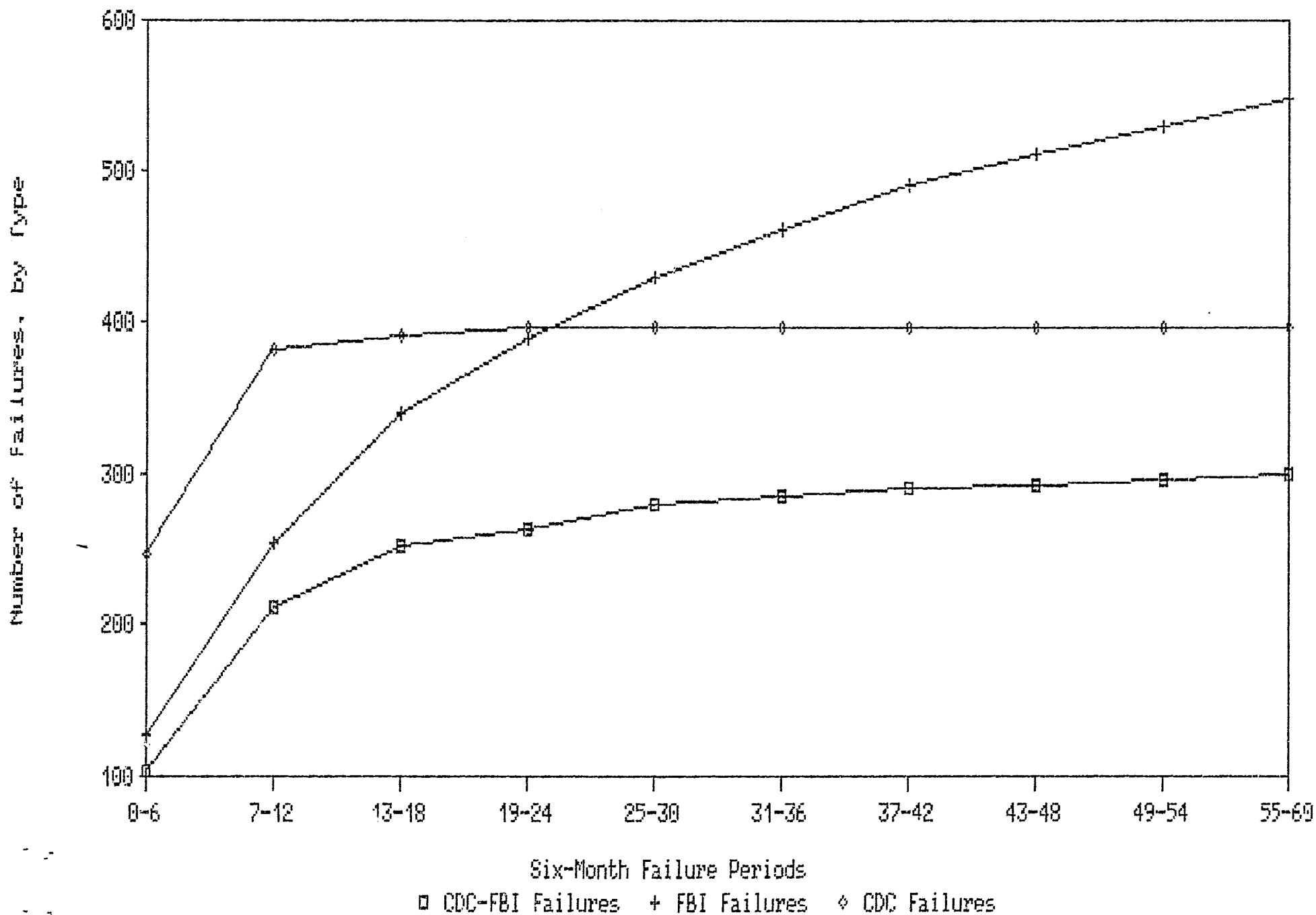
Part of the problem no doubt stems from the different paths by which reports find their way to the two agencies. The CDC data come directly from probation officers, who are required to document all officially designated parole violations. The FBI data come from police departments, and will only include violations that led to an arrest or arrests without any mention of parole violations. Moreover, not all police departments send arrest reports to the FBI.

Another part of the problem is that the CDC records do not explicitly distinguish between parole violations that result in a return to prison from parole violations that do not result in a return to prison. Recall that violators may be given credit for time in jail already served. The best we could do was _infer_ when reincarceration occurred.

In particular, the FBI data were coded so that a failure required clear evidence of doing some time. That is, a failure was recorded when there was a conviction and evidence that at least three months were spent confined. [4] A similar determination could not be made from the CDC data.

The disparities between the two data sources are very troubling, and we know of no fully satisfactory way to construct an overall outcome measure. However, since it is only in the FBI data that long term and out-of-state outcomes can be pursued, the FBI data will of necessity serve as the source of information on

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[4] Since a person could easily spend time in jail awaiting adjudication, we decided that some minimum threshold had to be exceeded before reincarceration was assumed.

failures. In other words, failures that are reported only in the CDC data will be ignored.

Our reliance solely on the FBI data no doubt means that some unknown number of failures are not counted. However, such underreporting should not matter <u>unless the underreporting is associated with the treatment after proper statistical controls are introduced.</u> That is, underreporting will not bias the results unless after conditioning on the assignment variable (i.e., hours worked in prison), there remains an association between the treatment received and the degree of underreporting.

VI. FINDINGS

Describing the Sample

Table 1 shows some selected descriptive statistics on our final sample. By and large, the statistics reveal just about what one would expect and what earlier experimental studies have shown. Most of the sample are male (98%), a minority are white (44%), the mean IQ is well within the average range (mean=102), the mean educational grade placement score is well under 12 years of age (mean=7.6), a substantial minority had experienced commitments as juveniles (35%), and most were released from substantial sentences (mean=29 months).

There is also some evidence that our sample compares favorably with the population from which it was drawn. Table 2 shows that the sample and the population are nearly identical

with respect to race, age, number of prior prison sentences, the number of parole terms (including the current one), and the proportion of opiate users. In short, there are some grounds for generalizing our later findings to the population of California inmates and to prison populations in general.

From Table 1, it is also possible to learn a bit about the treatment. On the average, applicants filed for FI benefits about two weeks after release , and benefits arrived on the average about five weeks later. Given the legislature's intent of delivering transitional aid quickly to newly released ex-prisoners, the seven-week delay is important; the program was not being implemented as planned. The delay also makes the delivery of benefits under SB224 rather different from the delivery of benefits in the earlier experimental studies. The experimental studies essentially had a check waiting for experimental subjects immediately upon release or upon going to the local unemployment office. Clearly, therefore, there are ample grounds for concern about external validity.

Table 1 also indicates that for the experimental group, the mean maximum payments for which individuals were eligible was $45.00. [5] The drafters of SB224 intended for benefits to be about

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[5] Our experimental and control groups were carefully screened so that individuals who had other sources of unemployment eligibility were excluded. Individuals who did not accumulate sufficient hours in prison to qualify for the benefits could combine these hours with employment after release in order to receive benefits. These "combined" FI claims were excluded from the analysis, as were disability insurance claims. Together these other types of claims only accounted for about 6% of all FI claims paid from the beginning of the program through January 1,

TABLE 1

Selected Descriptive Statistics

(N = 1053)

Variable	Mean	Standard Deviation	Minimum	Maximum
Parole Failure (dummy)	0.32	0.47	0	1
Got Benefits (dummy)	0.82	0.38	0	1
Treatment Months (dummy)	0.88	0.32	0	1
Treatment Months * Hours Worked (hours)	1180.27	689.32	0	3641
Maximum Weekly Benefits (dollars)[a]	44.80	10.53	30.00	100.00
Time Between Release and Application (weeks)	1.86	2.63	0	8.69
Time Between Application and Benefits (weeks)[a]	5.08	2.04	4.34	21.73
Age at Release (years)	32.56	8.37	19	63
Male (dummy)	0.98	0.14	0	1
White (dummy)	0.44	0.50	0	1
Opiate Addict	0.39	0.49	0	1
Has Escape History (dummy)	0.18	0.38	0	1
Has Juvenile Commitments (dummy)	0.34	0.47	0	1
Prior Prison Terms (integers)	0.61	0.99	0	4
Grade Placement Score (integers)	7.58	2.89	0	12
IQ Score (integers)	102.93	11.40	78	150
Length of Last Prison Term (months)[b]	28.65	12.57	11	60

TABLE 1 (Cont'd)

Length of Last Camp Term (months)[c]	1.73	4.07	0	27
Was on Work Furlough (dummy)	0.05	0.21	0	1
Released to L. A. County (dummy)	0.28	0.45	0	1
Released to San Diego County (dummy)	0.06	0.25	0	1
Released to San Francisco County (dummy)	0.07	0.26	0	1
Released to Alemeda County (dummy)	0.08	0.27	0	1
Parole Region I (dummy)	0.23	0.42	0	1
Parole Region II (dummy)	0.28	0.45	0	1
Parole Region III (dummy)	0.24	0.43	0	1
Parole Region IV (dummy)	0.25	0.44	0	1

a: For ex-offenders who get benefits (N = 863)

b: Outliers recoded to equal 60

c: Most people had no camp time

TABLE 2

Selected Characteristics of the California Parole

Population, By Year, 1977-1979, Compared

to the Final Sample

Characteristics	1977	1978	1979	Program Sample
Population/Sample Size	13,258	9,102	9,382	1,175
White(%)	45.4	44.8	42.9	44.4
Median Age (years)	31.7	30.8	30.3	30.0
0 Prior Prison Terms (%)	66.0	66.1	67.0	64.7
1 Prior Prison Term (%)	19.7	20.4	20.7	20.8
2 Prior Prison Terms (%)	8.2	7.8	7.1	7.7
3 or more Prior Prison Terms (%)	6.2	5.6	5.1	6.8
1st Parole (%)	76.2	78.2	82.1	87.5
2nd Parole (%)	14.7	14.5	12.7	8.0
3rd Parole (%)	5.3	4.2	3.2	4.5*
4th (or more) Parole (%)	3.9	3.2	2.0	----
Opiate Addicts (%)	43.4	47.1**	48.0**	39.6

*includes 4 or more paroles

**includes addicts and users

$10 more. The disparity derives from the difference between what an ex-prisoner needed to qualify for the benefits and how the benefits were actually calculated. Recall that eligibility was earned by working, over the course of a base year, 653 hours at a nominal rate $2.30 an hour. The base year was determined through the usual rules, which meant that in practice, the base period included an earlier four-quarter interval ending about six months before the application date for the FI program. However, once this threshold was passed, payments were determined by the highest quarterly earnings within the base period. For example, a prisoner who earned the absolute minimum of $1500 (653 hours times $2.30 an hour), evenly spread over four quarters would be eligible for only $30 a week. In short, the payments for which the experimental group was eligible were modest and about $25 less per week than provided by earlier experiments. Once again, questions of external validity can be raised.

To summarize, while the sample of ex-prisoners look "typical," the treatment seems somewhat different from the treatments in earlier experimental studies. It is not clear, therefore, how much our evaluation can be seen as a replication of past work and how much our evaluation can be seen as a unique effort. We will return to such questions later.

1981. For all practical purposes, therefore, FI benefits were delivered solely on the basis of prison earnings.

Program Impact

Figure 5 shows a graph of the cumulative proportion of ex-prisoners recincarcerated, broken down by months at risk and by membership in the experimental or control group. Three conclusions are easily drawn. First, when one compares the failure proportions for the experimentals and controls, without doing any partialing, it is clear that members of the control group are substantially more likely to fail. The gap between the experimentals and controls ranges from about five percent to about twelve percent, and even the smallest disparities are statistically significant at the .05 level.

Second, the arithmetic difference between the experimentals and control increases month by month up to about 48 months at risk. That is, the gross difference between the experimentals and controls is magnified as the followup period increases in length. However, one must also keep in mind that there could well be floor effects early in the followup period; with a modest failure rate at six months among the controls (i.e., 16 percent), reductions in reincarceration may be difficult to generate.

Finally, both the experimental and control curves in Figure 5 increase over time, but roughly at a decreasing rate. In other words, the likelihood of failure, given that one is still at risk, seems to decline with time.

To summarize, the temporal patterns shown in Figure 5 seem rather consistent with past research, including earlier randomized experiments. However, one must not forget that Figure

FIGURE 5

GROSS TREATMENT EFFECTS BROKEN DOWN
BY THE LENGTH OF THE FOLLOWUP PERIOD

PROPORTION FAILING
BY MONTHS AT RISK
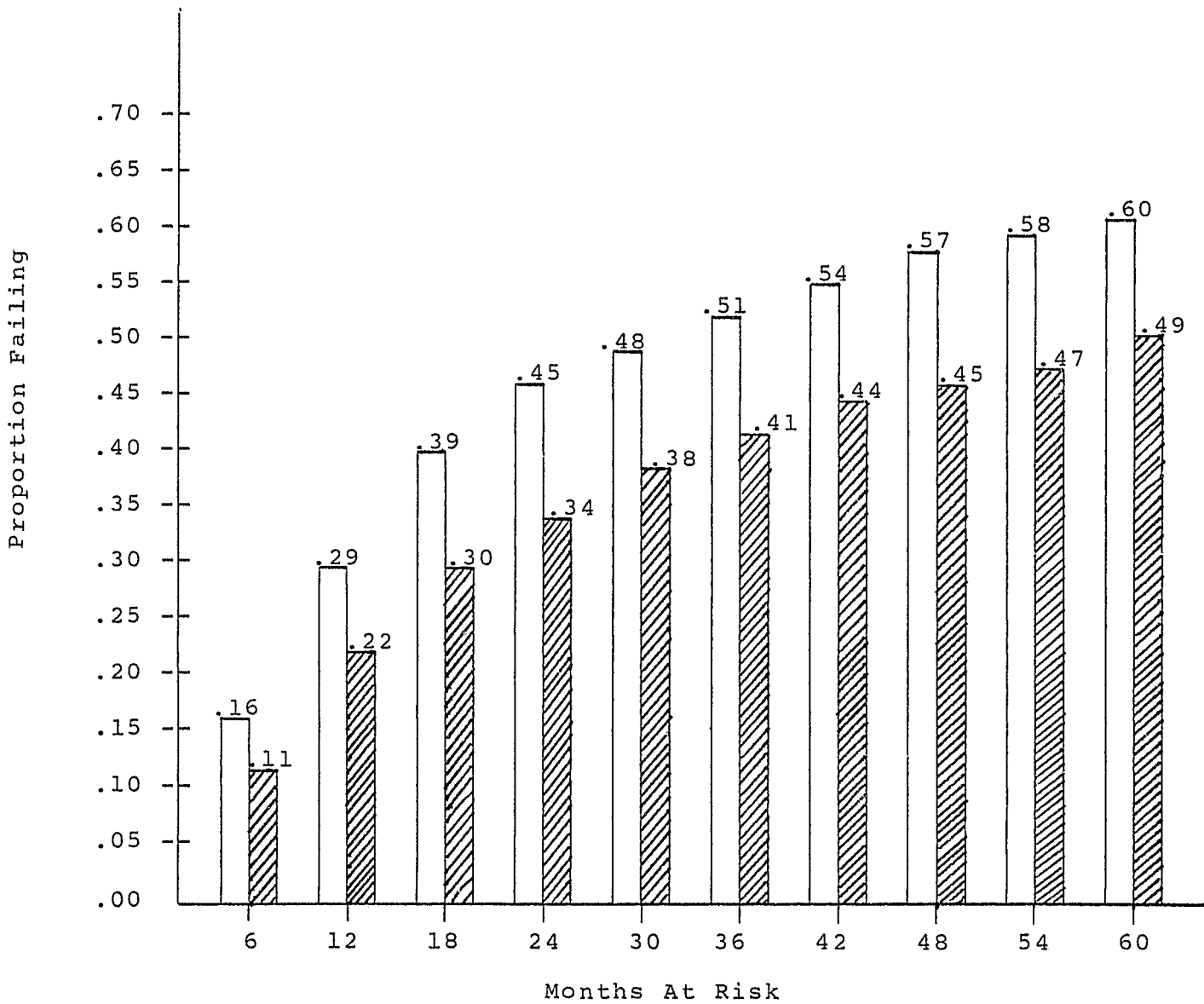
= Experimental Group (N=842)

= Control Group (N=230)

TABLE 3

A TABULAR REPRESENTATION OF
THE GROSS TREATMENT EFFECTS

6 Months at Risk

	Fail	Not Fail	
Experimentals	.11 (91)	.89 (751)	.79 (842)
Controls	.16 (37)	.84 (193)	.21 (230)
	.12 (128)	.88 (944)	1072

failure odds (experimentals)=.12
failure odds (controls)=.19
odds ratio
    (experimentals/controls)=.63
logarithm of odds ratio
            (base e)=-.46

60 Months at Risk

	Fail	Not Fail	
Experimentals	.49 (410)	.51 (432)	.79 (842)
Controls	.60 (138)	.40 (92)	.21 (230)
	.51 (548)	.49 (524)	1072

failure odds (experimentals)=.96
failure odds (controls)=1.50
odds ratio
    (experimentals/controls)=.64
logarithm of odds ratio
            (base e)=-.44

5 presents only gross effects. We have yet to show the results when controls are introduced for the assignment process.

As an introduction to the multivariate statistical results to follow, consider Table 3. The cross-tabulation at the top of the table builds directly on Figure 5; it shows the treatment effect at six months. The statistics to the right of the cross-tabulation, particularly the odds ratio and the logarithm of the odds ratio, will have direct analogs in the multivariate tables. The odds ratio is .63 while the log of the odds ratio is -.46. Both imply that the odds of failure for the experimentals is about two-thirds the odds of failure for the controls.

The cross-tabulation at the bottom of the table shows the treatment effect at 60 months. And to the right we have provided a set of statistics parallel to those provided for first cross-tabulation. At this point, perhaps the major message is that while in simple difference terms the treatment effect looks bigger at 60 months than at six months, the odds ratios are very similar. That is, once one takes the base failure rate into account in each period, the gross treatment effects are comparable; the odds of failure for the experimentals is about two-thirds the odds of failure for the controls.

In our earlier published evaluation of SB224, based on Phase I data, we emphasized an analysis of the regression-discontinuity data relying primarily on logistic regression (Rauma and Berk, 1982; Berk and Rauma, 1983). However, one can improve on statistical efficiency by using techniques that take as the

dependent variable not a simple binary outcome (i.e., fail or not), but the elapsed time between exposure to the experimental or control condition and either a failure or the end of the followup period. Such time-to-failure models have a long history in biostatistics and have recently been introduced into the criminal justice literature (e.g., Berk and Rauma, 1983; Schmidt and Witte, 1984).

In this report, time-to-failure results will be emphasized (although logistic regressions were also estimated). For computational convenience, we will employ time-to-failure data grouped into ten equal classes (as in Figure 1). This amounts to estimating a logistic regression (using a binary outcome) for each of the ten time periods, with all but the intercepts constrained to be the same across time periods. Each logistic regression includes only those individuals still at risk to failure (i.e., those who have not yet failed) during the time period in question.

The analytic cost of the discrete approach is small. As the number of time periods increases and as the duration of each time period decreases, the discrete model approaches the Cox proportional hazard regression formulation in continuous time (Lawless, 1982: 372-377). Our experience is that the approach occurs very quickly and that with ten time periods representing followup durations from six to 60 months, results in continuous

TABLE 4

MAXIMUM LIKELIHOOD ESTIMATES FOR
TIME-TO-FAILURE MODEL

Variable	Coeff	T-value	P-Value
Period 1 constant	-1.09	-1.85	.063
Period 2 constant	-1.06	-1.78	.074
Period 3 constant	-1.26	-2.10	.035
Period 4 constant	-1.65	-2.81	.005
Period 5 constant	-1.67	-2.76	.006
Period 6 constant	-1.89	-3.09	.002
Period 7 constant	-2.05	-3.29	.001
Period 8 constant	-2.49	-3.88	.000+
Period 9 constant	-2.07	-3.33	.000+
Period 10 Constant	-2.51	-3.33	.000+
Hours worked for periods 1-6	0.17	0.47	.065
Hours worked for periods 7-10	-0.01	-0.30	.976
Hours worked squared periods 1-6	-0.12	-1.02	.309
Treatment (binary, 1=eligible)	-0.25	-1.32	.095*
Addict (binary)	0.50	5.40	.000+
Age (in years)	-0.04	-4.72	.000+
Months served in CDC camp During prior commitment	-0.02	-1.20	.228
History of escapes (binary)	0.49	4.28	.000+
Achievement test score (grade equivalent)	0.01	0.52	.599
IQ score	-0.00+	-0.57	.564
History juvenile offenses (binary)	0.27	2.71	.007
Released in Los Angeles (binary)	-0.09	-0.58	.558
Male (binary)	0.46	1.42	.153
Released in Oakland (binary)	0.32	1.63	.102
Number of prior prison terms	0.09	1.44	.150
Paroled to Region 2 (binary)	0.13	0.92	.359
Paroled to Region 3 (binary)	-0.08	-0.39	.694
Paroled to Region 4 (binary)	0.20	1.26	.207
Released to San Diego binary)	-0.08	-0.35	.714
Released to San Francisco (binary)	0.03	0.17	.868
Race is white (binary)	-0.20	-1.93	.054
Released on work Furlough(binary)	-0.18	-0.75	.452

* one-tailed test

time would not be substantively different. 6

Table 4 shows the maximum likelihood estimates and significance tests for the time-to-failure model. First, the ten period constants allow for different intercepts for each logistic regression estimated. Perhaps the major conclusion is that for people still at risk to fail (i.e., those who have not yet failed), the probability of failure by and large declines as the followup period increases. Perhaps, the poor parole risks simply fail early, leaving behind a pool of better parole risks. Alternatively, the process of staying out of trouble itself produces benefits that over time improve an ex-prisoner's chances. The former can be conceptualized as a type of individual heterogeneity while the latter can be conceptualized as a type of state dependence (Hsiao, 1985: 124-126).

Second, the key to a credible analysis of regression-discontinuity data lies in properly modeling the relationship between the assignment variable (hours worked in prison) and the outcome variable (time-to-failure). We began with a single linear variable of hours worked in prison. Then, as the literature suggests (e.g., Trochim, 1984), we experimented with polynomial functions of hours worked in prison, hoping to catch important non-linearities. We also estimated fully separate logistic regressions for each of the ten time periods (i.e., constraining none of the estimates across equations), trying polynomial

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6 Note that by using for each logistic regression only those individuals still at risk, one is taking righthand censoring formally into account.

functions of hours. Applying significance tests to these efforts, we arrived at the three variables for hours worked in prison shown in Table 4: a linear function of hours worked for time periods one through six, another linear function of hours worked for periods seven through ten, and a quadratic function of hours worked for periods one through six. Nevertheless, it is apparent from the coefficients and P-values that the assignment variable is not strongly related to the outcome. This will have implications to which we will return.

Third, we have included on Table 4 a number of covariates beyond functions of hours worked in prison. Were we estimating a linear model, the sole purpose of these covariates would be to improve statistical power. Insofar as the error sum of squares is reduced, smaller standard errors result. However, for non-linear models, the covariates play an additional and important role.

In brief, even with a randomized experiment, heterogeneity among the assigned units in the outcome will, for a wide variety of non-linear models, lead to a particular kind of biased estimates of any treatment effects (Gail, et al., 1984). For linear models, such as linear regression or analysis of variance, (properly implemented) randomization will suffice to produce unbiased estimates of treatment effects, which as a consequence of random assignment and a linear functional form, are not dependent on the distribution of covariates in the population (from which the sample data are in principle drawn). However, for many non-linear functional forms, such as the logistic,

randomization will <u>not</u> yield the same result. Rather, the treatment estimate compares the response within some population of a <u>randomly selected</u> unit exposed to the treatment with another <u>randomly selected</u> unit that is not exposed to the treatment. What we want, in contrast, is an estimate of the treatment effect for each <u>given</u> unit. (Gail, et al., 1984: 432). The former is necessarily population specific, while the latter is not. Only if <u>all</u> sources of heterogeneity are partialed out via covariates, does one obtain the desired treatment estimates that are not dependent on the particular distribution of the covariates in the population.

Our regression-discontinuity design is meant to closely approximate the level of internal validity one may obtain with random assignment. However, even if we have managed to produce that close approximation, we remain vulnerable to the biasing effects of individual heterogeneity. Hence, the covariates in Table 4; we are trying to soak up important sources of individual heterogeneity. Note that a number of large and statistically significant coefficients are reported, all of which have sensible signs. For example, ex-prisoners who have been addicts, who are younger, who have a history of escape attempts, and who have a history of juvenile offenses are at much greater risk for reincarceration. Nevertheless, important heterogeneity may well remain, a point to which we will return.

Fourth, and perhaps most important, the point estimate for the impact of FI eligibility is -.25. This translates into a odds

ratio of .78; the odds of failing for the experimentals is .78 the odds of failing for the controls. In other words, the estimated treatment effect, partialing on functions of hours worked in prison and a number of other covariates, is a bit smaller than the gross treatment effects reported in Table 3 and Figure 5. Using a null hypothesis of no effect and a one-tailed test, the treatment effect is statistically significant at the .10 level, but not the .05 .level. That is, if the treatment effect were really zero, a negative coefficient as large as the one obtained could have occurred about one time in ten by chance alone.

There are a number of possible analytical responses to the estimated treatment effect. One option is to adopt (in advance) the .10 level of statistical significance and then reject the null hypothesis that the intervention did not work. If one has strong prior beliefs that the program reduced reincarceration rates, and/or a loss function in which false negatives are especially troubling, this is a reasonable response.

Another option is to adopt (in advance) the more conventional .05 level of statistical significance, and then fail to reject the null hypothesis of no effect. If one has few a priori preconceptions about the program's effects, or strong prior beliefs that the program is ineffective (or harmful), and/or a loss function in which false positives are especially troubling, this is reasonable response.

Yet another option is to adopt .05 level of statistical significance but use a treatment effect as the null hypothesis that would allow the program financially to break even (rather than a null hypothesis of zero). We earlier suggested that reduction in reincarceration rates of five percent overall would make the program cost effective. At the mean failure rate for both groups, the logistic coefficient of -.25 translates into a difference between the experimental and controls of about five percent. That is, the proportion the experimentals who fail is about five percent less than the proportion of the controls who fail. Using the five percent figure as the basis of the null hypotheses (translated in a logistic regression coefficient), the null hypothesis cannot be rejected. One would, therefore, tentatively accept the null hypothesis that the program worked at a cost-effective level.

A fourth option would be to examine the data more closely to see if there are treatment effects obscured by a single, overall estimate of effect. To begin, we did not find important interaction effects for different kinds of offenders although there was a suggestion that the program worked better for individuals who would have been better risks. For example, we did not find interaction effects by race. However, first offenders seemed to benefit more from the FI eligibility (but not by greater than chance amounts) than offenders with a prior conviction record.

Far more promising are the implications of fully separate logistic regression run for each of the time periods. The treatment effect coefficient was -.29 (t=-0.68) at six months grew to -.45 (t=-1.49) at 24 months and then declined to -.22 (t=-.50) at 60 months. In other words, the effect was in the predicted direction in each time period, but peaked at about two years after release.

If one takes this temporal pattern seriously, it suggests that any beneficial impact of FI eligibility is attenuated early when the high risk cases are likely to recidivate and late when most of the individuals who remain in the pool are low risk. In other words, the treatment effect may be weak early because the trouble-makers are very likely get into trouble despite FI eligibility. Likewise, the treatment effect may be weak late because the survivors are very unlikely to get into trouble, even if FI eligibility were not available. In contrast, the treatment effect is greatest when the pool at risk to reincarceration no longer has many sure losers but still is not constituted almost exclusively of sure winners.

The temporal pattern of treatment effects also has implications for why the overall impact of FI eligibility is smaller in the Phase II analysis than the Phase I analysis published earlier (Berk and Rauma, 1983; Rauma and Berk, 1982). In brief, if the treatment's impact declines dramatically after about 24 months, the average effect aggregated over the 60 months

followup period likely will be attenuated compared to the average effect aggregated over the ten month followup period.

A final response is to take seriously the concept of ignorability. Recall that for the assignment of a treatment to be ignorable, all covariates associated with <u>both</u> the treatment and the outcome must be included in the analysis. This clearly means that one need not include any covariates that are <u>not</u> related to <u>both</u> the treatment and the outcome. In our case, despite a variety of efforts to find statistically significant and large effects for various functions of hours worked in prison, we were unable to do so. Moreover, we paid a high price for including functions of hours; such functions were inevitably and highly associated with the treatment dummy variable for which estimates of treatment effects were obtained.

Therefore, it is reasonable to drop hours worked in prison from the multivariate analysis. One can anticipate that although the estimate of the treatment effect will change little, the standard error for the effect will drop dramatically. As a result, the t-value will be well in excess of conventional levels of statistical significance. Any estimated treatment effect will be essentially unaltered because hours worked in prison is effectively unrelated to reincarceration. The associated standard error will decline substantially because of large reductions in multicollinearity. In Table 4, for example, the estimate of the treatment effect and the estimate of the impact of hours in the earlier time periods is correlated -.70. Similarly, the estimate

of the treatment effect and the estimate of the impact of hours squared in the earlier time period is correlated -.47. Clearly, our standard errors are being affected.

Just as anticipated, dropping hours worked in prison from the analysis had a very small impact on the estimated treatment effect, but dramatically altered its associated t-value. The treatment effect increased slightly from -.25 to -.30, while for the null hypothesis of no effect the t-value increased substantially (in absolute value) from -1.31 to -2.66 (P-value < .005). Clearly, our estimated treatment effect is now statistically significant by any conventional standard. A combination of a small change in the estimated treatment effect and a large change in the standard error apparently comfirms our suspicion that hours worked in prison was not an important covariate. 7

VII. CONCLUSIONS AND POLICY IMPLICATIONS

The conclusions one draws from our analysis of the impact of California Senate Bill 224 depend in part on where one places the burden of proof. If one's reading of past research on similar programs makes one skeptical of such interventions, and if one's

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7 We could have employed the same strategy for each of the other covariates in Table 4 that did not have statistically significant effects on reincarceration. However, the largest correlation between the estimated treatment effect and estimates of the impact of any of the other covariates was -.18. The typical correlation was about .10 in absolute value. Clearly, there was little to be gained in pruning the model further.

loss function places very heavy weight on avoiding false positives, our findings will not be seen as a compelling demonstration that eligibility for unemployment benefits reduces crime in a cost-effective fashion; there are simply too many uncertainties.

Perhaps most important, our measure of failure taken from FBI records is clearly imperfect. No doubt many failures were overlooked and probably some individuals were falsely classified as failures. The key issue, however, is whether such errors occurred <u>differentially</u> for the experimentals and controls. Undercounts or overcounts unrelated to the treatment will not bias estimated treatment effects. We cannot think of any plausible scenarios by which differential undercounting or overcounting could have occurred.

Another major vulnerability lies in the degree to which the assignment process is really ignorable. By all accounts (see, for example, Appendix A), the number of hours worked in prison was the sole determinant of FI eligibility. Moreover, we experimented with a wide variety of functional forms for the impact of hours on reincarceration. However, unlike a randomized experiment, there is no way to determine for certain whether ignorability is achieved.

Still another problem is individual heterogeniety. While our use of covariates no doubt reduced individual heterogeniety, it surely did not eliminate it. Still, all may not be lost. Perhaps most important, our sample seems to be rather representative of

the policy relevant population. That is, our sample seems very much like any random cross-section of California prisoners in the early 1980's. Consequently, the degree to which our results are population specific many not be a critical matter.

There are also grounds for being uneasy with our use of significance tests to determine whether the treatment effect was statistically significant. First, under the model which maximized the impact of hours worked in prison, and which assumed a null hypothesis of no effect, the treatment P-value was just a bit under .10 (for a one-tailed test). Many readers may prefer a more demanding threshold for statistical significance.

Second, many readers may find small comfort in our use, alternatively, of a null hypothesis of a cost-effective treatment effect. We were, in effect, assuming the program worked unless the data showed otherwise. Some readers may prefer to begin with the more conventional assumption that the program did not work at all.

Third, the far smaller treatment P-value of less than .005 (for a null hypothesis of no effect and a one-tailed test) was produced after dropping from the equation hours worked in prison. While such a decision can be justified by significance tests and the definition of ignorability, we risk falsely accepting the null hypothesis for the impact of hours.

To summarize, while it is clear that reasonable people may differ about what our findings imply, we feel that the weight of the evidence strongly supports California's FI program. However,

we do not believe that enough evidence exists in our reserch and past studies to justify a wholesale adoption of FI programs across the county.

Rather, we would favor a series of randomized experiments field testing programs based on the FI concept. The key problem would be to find a way to capitalize on the positive incentives that the unemployment benefits may well provide, without producing harmful work disincentives leading to crime (Rossi, et al., 1980; Berk, et al., 1980). We suspect that the California program had more success than the TARP demonstration project, for example, because the lower levels of payments in California struck a better balance between positive and negative effects. This implies that the series of experiments we propose should at least vary the level of benefits offered. In addition, however, it would be important to vary the tax rate by which earnings were deducted from benefits, the number of weeks for which benefits could be obtained, and the delay between application for benefits and receiving benefits.[8] Short of such experiments, it is impossible to know the degree to which the positive effects we claim for California's FI insurance will generalize to new settings.

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[8] The TARP project tried to build in some of these concerns and did not find any effects. However, it is doubtful that the experimental subjects understood much about the treatment, except that they would be receiving payments if they were not employed. New experiments would have to make a much greater effort to insure that treatment content was understood. In addition, the experimental design would have provide sufficient statistical power to properly address interaction effects among the different treatment dimensions.

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APPENDIX A

California Department of Corrections	Number: A.B. No. 79/42
ADMINISTRATIVE BULLETIN	Date: July 26, 1979
Subject: Unemployment Compensation for Former Inmates	Cancellation Date:

This bulletin cancels Administrative Bulletin No. 78/43 relating to unemployment compensation for former inmates. The procedures set forth in that bulletin are included in this bulletin. In addition, procedures have been added for responding to an inmate's appeal before an Administrative Law Judge (Section 3.4).

Chapter 1149 of the Statutes of 1977 provides that certain inmates may qualify for up to 26 weeks of unemployment compensation or disability benefits upon release from prison. Releasees from the California Rehabilitation Center are also covered by the bill; therefore, any reference to inmates, parolees or former inmates includes "N" numbers released to outpatient status.

Eligibility and weekly benefits depend on the number of hours an inmate worked or was engaged in training during a 12-month base period to be determined by the Employment Development Department (EDD) in each individual inmate's case.

I. ELIGIBILITY

A. Unemployment Compensation

For the purpose of determining eligibility, an inmate must have worked a total of 652 hours on qualifying assignments during the 12-month base period. Without regard for pay or non-pay status or actual rate of pay on such assignments, the inmate will be considered to have been paid at the rate of $2.30 per hour. Therefore, a hypothetical total earnings of $1,500 during the Base period is required to qualify for unemployment compensation ($2.30 x 652 hours = $1,500).

Once basic eligibility is established, the hours involved in qualifying assignments during each quarter of the base period will determine the amount of unemployment compensation to be awarded.

B. Disability Benefits

For the purpose of determining eligibility for disability benefits (as opposed to unemployment compensation), an inmate must have worked a total of 131 hours on qualifying assignments during the base period, based upon the same hypothetical rate of pay. A total earnings of $300 is required to qualify for disability benefits ($2.30 x 131 hours = $300).

C. Retroactivity

Eligibility is retroactive to January 1, 1977, but does not become effective until July 1, 1978. All inmates released on or after January 1, 1978 will be given the CDC Form 807 (Notice of Unemployment and Disability Payments for Former Prison Inmates), however, a claim cannot be filed with EDD before July 1, 1978.

II. WORK AND TRAINING ASSIGNMENTS

A. Qualifying Assignments

1. Vocational training is accepted as a qualifying assignment for unemployment compensation and disability benefits.

2. Any other assignment which produces a product or provides a service to the state or its employees is also accepted as a qualifying assignment.

B. Non-Qualifying Assignments

Participation or assignment in the following activities or housing does not qualify inmates for eligibility unless assigned as clerks or other full-time workers.

1. Academic education.

2. Non-vocational training.

3. Inmate activity or self-help groups.

4. Handicraft and other leisure time activities.

5. Orientation periods.

6. Pre-release programs.

7. Unassigned for any reason, including medical.

8. Hospitalized.

9. Assignment to specialized housing units (Management Control, Security, Psychiatric Management and Protective Units), unless assigned to training, productive work or service assignments within or outside the units.

10. Work furlough.

III. TIMEKEEPING AND RECORDS

A uniform method of recording and reporting is required, with one central area in the institution specifically designated to receive and compile such information. This function will be within the accounting office. An Account Clerk II position has been budgeted for each institution for this purpose.

The following procedure will be instituted at each institution:

A CDC 191-B Time Card titled "Unemployment/Disability Insurance" will be processed every month for each inmate that qualifies by work assignment for the coverage. These will be processed in the same manner that is presently used for inmate pay card processing. It is expected that each work supervisor will accurately fill in the qualifying time worked on a daily basis, with the exception that for inmates assigned to Correctional Industries the CDC 191-B will be processed with only a monthly total. The reason for this exception is because Correctional Industries' inmates use time clocks, and this record will be available for audit purposes.

At the end of each month, the hours worked will be totaled and the card signed by the work supervisor and the inmate (except those assigned to Correctional Industries), and approved in the same manner that is presently used for pay cards. The cards will then be forwarded to the accounting office for posting.

All non-pay assignments must have a position number assigned for recording on the Inmate Employment Record.

A. Recording of inmates' time will reflect only the actual hours present and productively utilized at place of assignment, except for excused absences. Excused

absences are limited to answering calls or passes or otherwise detained by staff. Lockdown of an institution or assignment area which prevents inmates from reporting to their assignments is not an excused absence.

Absences to engage in visits, canteen, laundry, non-staff interviews, sick call, medical lay-ins and other routine functions are not excused and credit will not be given. Inmates will not be allowed access to timekeeping records. Recording of time shall be done by staff only.

B. 1. Information from the timekeeping forms of the various areas will be transferred monthly to an Inmate Employment Record. This form is the same in overall dimensions and column size as the "Inmate Trust Ledger"; therefore, it can be used with the same machine settings. Different column headings and posting references are used and hours will be posted instead of monetary figures. The form is a different color to distinguish it from the Inmate Trust Ledger.

2. Records for transfers will be expedited. Ample notice will be given to the Account Clerk II so that information can be requested from the supervisor for up-to-date compilation of time prior to transfer.

3. The Employment Record will be posted in duplicate. When an inmate transfers, the original goes with him and the copy is filed locally. The original shall be sent to the accounting office of the receiving institution and not packaged in the C-file. Releases to parole and discharge will result in both copies being retained at the institution of release.

IV. PROCESSING CLAIMS

A. 807 Notice

A CDC 807 (Notice of Unemployment and Disability Payments) will be given each inmate at the time of his release from prison. A record of such issue will be typed on the CDC 1515 form on the line that notifies the inmate regarding a Certificate of Rehabilitation. For the benefit of EDD, and to expedite processing, the inmate's

social security account number (SSA), his year of birth and the name of the institution from which released will be typed on the 807 form.

Inmates released to parole or discharge from a work furlough center will also be notified of possible eligibility dependent upon qualifying employment or training experienced in prison. Time worked for a private employer while on work furlough is separately reported by the employer, just as would be the case for any private citizen employee. The releasing institution will retain the qualifying work record.

B. Application and Verification

1. Any parolee or dischargee who thinks he qualifies for benefits should take the 807 form to an EDD office and file a claim.

2. To establish eligibility, EDD staff will initiate a form with the former inmate's name, CDC number, social security number and year of birth. The form will also identify the base period dates. The form will be sent to the releasing institution for verification of total qualifying hours the former inmate worked, or was enrolled in training during the base period.

3. When the form is received by the institution, the former inmate's employment record will be checked and the total hours worked per quarter will be entered on the form. If at all possible, this will be done the same day of receipt and the form returned immediately to EDD so they can stay within their deadline for claim payment. The date it was received and returned will be recorded on the employment record to avoid duplicate claims being honored. If information has been previously supplied for that inmate, this will be called to EDD's attention, giving the dates of the base period involved.

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4. EDD identifies their clients by social security number. CDC will also verify by this method, even though we use the CDC number and date of birth for additional identifying data. Because all inmates do not have social security account numbers, they shall be urged to apply for one early in their incarceration so they can be identified by this method.

Appeals by former inmates regarding disagreement over information provided by CDC should be directed to EDD. EDD will contact the Account Clerk II at the releasing institution for further information or clarification.

Experience has shown that inmates whose appeal claims are not verified by the department will normally pursue the appeal, which in the normal course of events will result in a hearing before an Administrative Law Judge, whose decision is final. Normal procedures require both the claimant and the former employee (department) to be present and available for questioning, following standard adversary legal procedures, but it is clearly economically impractical for institution staff to make the long trip and spend the time defending the state's position at the hearing. The resultant lack of defense has, in almost every case, led to the claimant's appeal being granted; in many cases under what would appear to be somewhat questionable equity.

To avoid improper judgments against the state because of a lack of personal appearance by an "employer" representative, all institutions will, when notified of claimant appeal, use the attached form supplied by EDD (each institution to reproduce as needed) to provide official legal response to the appeal. When used, copy should be made and forwarded to the Central Office Former Inmate Unemployment Claims Coordinator for information and inclusion in the Central Office Inmate Record Files.

California Department of Corrections ADMINISTRATIVE BULLETIN Subject: Unemployment Compensation for Former Inmates	Number: A.B. No. 79/42
	Date: July 26, 1979
	Cancellation Date:

5. The name, address, and phone number of the Account Clerk II and a back-up person, responsible for timekeeping and processing claims at each institution, will be provided to EDD. The name and phone number of the Central Office Coordinator for this program will also be provided. The above positions will be the persons for EDD to contact in resolving problems that may develop. Information regarding EDD's contact person will be provided to the above persons when available.

V. RETROACTIVITY

A. Claims filed by former inmates released prior to issue of this Administrative Bulletin will be referred to the releasing institution, but no Inmate Employment Record will be on file. In such cases, CDC 151's, 190's, 191's, Inmate Trust Ledgers, bed cards, etc., will be researched for information. If the releasing institution lacks sufficient information to file an accurate report, contact will be made with the proper Parole Region (for active cases) or CMF Archives (for discharged cases). The Regional Administrators and Associate Superintendet, CMF-RCN, will designate a staff person responsible to review 103-B's, 128-C's, 128-E's or other pertinent records and indicate other institutions where the inmate was housed during the base period. This information will be supplied to the Account Clerk II at the releasing institution who has the responsibility to coordinate and compile the information and send it to EDD.

The most expedient method will be utilized in requesting and providing the above information.

B. For retroactive purposes, the record of pay positions should be sufficiently accurate, but time worked on non-pay positions will have to be estimated according to various positions held.

To accomplish this, each institution must determine what positions qualify and set the number of hours for each assignment. For instance, an inmate assigned all day to the culinary dining room may only work two hours during each meal, yet be assigned or on call from 6:00 a.m. to

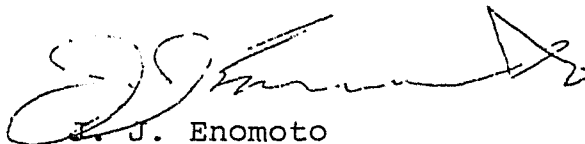6:00 p.m. Credit for such assignment would be six hours, not the full twelve assigned.

When the list has been compiled for each assignment, each institution will send copies to all other institutions so that total hours worked by each inmate released can be compiled by the releasing institution.

C. 1. To avoid extending the above procedure unduly, an additional process will be necessary for approximately 18-20 months until complete records for each inmate are on file in each institution. This process is to be instituted immediately.

   2. Just prior to an inmate's release, his case manager will review the central file and other pertinent records to determine total hours worked during the past 20 months. This information will be recorded by month and routed to the accounting office. No specific format is necessary for this report, but it should bear the recorder's signature and be able to withstand audit. The Account Clerk II will do the summary posting of this information and will be able to assist the Case Managers in their compilations by indicating the specific months for which information is lacking on each inmate.

This bulletin must be shared with all employees, especially those who supervise inmates and parolees. Formal in-service training classes need to be held for managers/supervisors in the institutions so they can properly explain and administer this program. Where possible, inmate supervisors should also have IST, but due to the number of staff involved, the most logical process for their training is through their individual supervisors.

The need for proper work ethics, adequate standards and proper timekeeping cannot be over-emphasized. This will require a high degree of responsibility and accountability of all staff and inmates.

This bulletin will remain in effect until incorporated into the appropriate manual(s).

J. J. Enomoto
Director of Corrections

Attachment

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STATE OF CALIFORNIA

EMPLOYMENT DEVELOPMENT DEPARTMENT

AFFIDAVIT OF QUALIFYING HOURS FOR FORMER INMATE UNEMPLOYMENT INSURANCE/
DISABILITY INSURANCE CLAIMS

TO:

Adminisrative Law Judge _____

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_____

Case No.:_____

Claimant:_____

SSA No.:_____

Affidavit of_____, Title_____

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I declare under penality of perjury that the foregoing, to the best of my know-
ledge and belief, is true and correct,

executed on_____19____, at_____O'Clock___M.

at_____

_____
Signature of Affiant

STATE OF CALIFORNIA
}ss
COUNTY OF_____}

Page No._____of a_____
page affidavit.