Job search, conditional treatment and recidivism: The employment services for ex-offenders program reconsidered

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1 Introduction

More than 600,000 people are released from prison each year in the United States. By any standards this fact raises many concerns about the likely consequences, both social and economic, of such a massive influx of ex-convicts into society.

A prominent issue nowadays is the high rates of recidivism prevalent in the country: recidivism rates measured by the Bureau of Justice Statistics show that two-third of released prisoners are re-arrested and one-half are re-incarcerated within three years after release.

The idea that having a job diminishes the chances of recidivism is well established in the criminological literature. Therefore, even though finding a job is a difficult task for ex-offenders, a policy that help these people to find a job will likely decrease the chances of recidivism.

During the 1970's and 1980's policy makers in the United States have sponsored evaluations of both in-prison job training programs, as well as post-release (community-based) employment interventions.

In this paper we evaluate a program that has as special characteristic the fact that the treatment is triggered if and only if the individual in the treatment group gets a job, although the selection of the participants in a control group and a treatment group is done randomly.

During the period of $1980 - 1985$, the National Institute of Justice (NIJ) sponsored a controlled experiment to evaluate the impact of reemployment programs for recent released prisoners. Three well established programs were chosen, in Boston, San Diego and Chicago, to participate in the Employment Services for Ex-Offenders Program, henceforth ESEO.

A total of $2,045$ prisoners who voluntarily accepted to participate were randomly assigned to either a treatment group or a control group. Those in the first group received, besides the normal services (orientation, screening, evaluation, support services, job development seminar, and job search coaching), special services which consisted of an assignment to a follow-up specialist who provided support during the job search and the 180 days following job placement. The control group received only normal services.

Previous analyses of the ESEO program data, using counts of recidivism occurrences, did not find any treatment effects

The objective of our paper is to re-evaluate the effect of the ESEO program on recidivism using duration models for job search and recidivism, with the conditional treatment incorporated in the latter duration model. Both durations are interval censored, though.

The incorporation of treatment in the conditional distribution of the recidivism duration given the covariates **and** the job search duration is inspired by (but not exactly equal to) the approach of Abbring and Van den Berg (2003). These authors propose a bivariate mixed proportional hazard model, where one duration is the timing of the treatment and the other one is the duration of interest that is affected by the treatment. In the case of the ESEO program the duration that triggers the treatment is the job search, and the duration of interest is recidivism.

2 The ESEO program

In the ESEO program, after being assigned randomly to either the control or treatment group, the clients step inside the intake unit, where they received initial orientation, screening and evaluation by an intake counsellor. While still in this first phase, to secure survival up to the job search phase, the intake counsellor offers minimal assistance services such as food, transportation, clothing and etc.

After intake, the client enters the second phase that will prepare him/her to develop job search skills: a brief job development seminar which deals with issues like appropriate dress and deportment, typical job rules, goal setting, interviewing techniques, and job hunt strategy.

The next and final phase before possible treatment is the job search assistance. The job search assistance in the ESEO program is offered equally to both control and treatment groups.

The actual treatment starts upon the first job placement. The people in the control group do not receive help after placement, whereas the people in the treatment group receive follow-up help.

The follow-up special services consist mainly of crisis intervention, counselling and, whenever necessary, re-employment assistance, during a period of six month after the first placement.

Thus, even though the split of the sample in treatment group and control group is random, the actual treatment is the follow-up special services after finding a job and is therefore not randomized.

3 The ESEO data set

The ESEO data set consist of 2045 individuals who participated in one of the three programs. However, the Inter-University Consortium for Political and Social Research of the University of Michigan made only 1074 usable observations available: 325 in Boston, 489 in Chicago and 260 in San Diego.

In order to be eligible to participate in the ESEO program an individual must have the following backgrounds:

(1) Participants voluntarily accept program services;

(2) Participants have been incarcerated at an adult Federal, State, or local correctional facility for at least 3 months and have been released within 6 months of program participation;

(3) Participants exhibit a pattern of income-producing offenses.

From the eligibility criteria it is clear that our population is a very special subset of the population of ex-offenders.

Also, since participation is voluntary and there is no information on non-participants it is not possible to assess the potential bias induced by this selection scheme.

Given the initial sample, the individuals were randomly assigned to either the treatment or control group. Controls received the standard services, and the people in the treatment group received, in addition to that, emotional support and advocacy during the follow-up period of 180 days after placement.

The endogenous variables are:

- A: Indicator for attrition. $A = 1$ means the individual is either a “no show” or a “drop-out”, $A = 0$ otherwise;
- T_s $(s = search)$ is the duration of the job search, i.e., the time between the date of release and the date of placement in the first job after release;
- T_c $(c = crime)$ is the recidivism time, i.e., the time between the date of release and the date of the first arrest after release.

The two duration variables are interval censored: we only observe them in the form of intervals: $T_s \in (a, b]$, $T_c \in (p, q]$, $b \leq p$, where $(a, b], (p, q] \in \{(0, 1], (1, 6], (6, 12], (12, \infty)\}$. The unit of measurement for these durations is months.

These events are only observed if there is no attrition: $A = 0$.

The following set of covariates has been singled out, next to the group assignment indicator:

- $G = 1$ if selected in the treatment group, $= 0$ if selected in the control group
- $DRUGS$: Indicator for drugs use during the last 5 years;
- $WHITE$: Race indicator;
- $MALE$: Gender indicator;
- $EDUC\_L = 1$ if years of schooling ≤ 8, $= 0$ otherwise;
- $EDUC\_H = 1$ if years of schooling > 12, $= 0$ otherwise;
- AGE: Age in years;
- $AGE1ARR$: Age of first arrest, in years;
- $SANDIEGO$: Indicator for San Diego;
- $CHICAGO$: Indicator for Chicago;

4 Attrition

There is a substantial number of individuals in the sample who do not show up for the common part of the program, i.e., the assistance with the job search.

With a few exceptions, attrition occurs straight away after release. It is therefore reasonable to assume that, conditional on the covariates (stacked in a vector X), A is independent of T_s and T_c, because in most cases the attrition decision $A = 1$ is made before T_s or T_c are realized.

The conditional attrition probability has been modeled as a Logit model. It is conceivable that attrition is also affected by the group assignment: The prospect of receiving treatment ($G = 1$) may lead to a lower attrition probability. Therefore, next to the original covariates we have included the group assignment G and the products of G with each of the covariates in the Logit model for A.

The initial Logit model has been subjected to a sequence of Wald and likelihood ratio tests to clean the model of insignificant covariates. Only the final results are presented in Table 1:

Table 1. Logit results for attrition

Covariates	Estimates	t-val.
$MALE \times (1 - G)$	0.7835337	3.93
$CHICAGO \times (1 - G)$	1.7985664	7.94
$CHICAGO \times G$	0.5250147	2.71
$SANDIEGO \times (1 - G)$	1.0452205	3.55
$SANDIEGO \times G$	0.4278509	2.05
1	-0.7661767	-5.26
Log-likelihood:	-654.831	
Sample size:	1074	

It follows from Table 1 that males have a higher attrition rate than females, but only if selected in the control group. Moreover, the attrition rates in Chicago are significantly higher for the control group than for the treatment group, and the same applies to San Diego.

To compare the attrition rates in San Diego and Chicago with the attrition rate in Boston, we have to compare the coefficients of the location dummies with the intercept, which leads to the conclusion that the attrition rates in Chicago and San Diego are much higher than in Boston, in particular for the control group, and within the control group the attrition rate in Chicago is higher than in San Diego.

Finally, note that the dependence of attrition on the group assignment does not conflict with our assumption that conditional on the covariates T_s and T_c are independent of A, because G is determined randomly.

5 Preliminary data analysis

As said before, the durations T_c and T_s are only observed in the form of interval indicators, for the intervals $(0, 1], (1, 6], (6, 12]$ and $(12, \infty)$. Table 2 below presents the number of observations in each interval and combination of intervals, for both groups as well as separately for the treatment group $(G = 1)$ and the control group $(G = 0)$.

Table 2. Observations per interval (A=0)

$T_s \diagdown T_c$	$(0,1]$	$(1,6]$	$(6,12]$	$(12,\infty)$	$Total$
$(0,1]$	12	56	43	152	263
$(1,6]$	6	44	39	112	201
$(6,12]$	1	7	6	20	34
$(12,\infty)$	0	1	1	3	5
$Total$	19	108	89	287	503

Treatment group only:

$T_s \diagdown T_c$	$(0,1]$	$(1,6]$	$(6,12]$	$(12,\infty)$	$Total$
$(0,1]$	9	39	32	105	185
$(1,6]$	5	35	30	81	151
$(6,12]$	1	4	5	18	28
$(12,\infty)$	0	1	1	3	5
$Total$	15	79	68	207	369

Control group only:

$T_s \diagdown T_c$	$(0,1]$	$(1,6]$	$(6,12]$	$(12,\infty)$	$Total$
$(0,1]$	3	17	11	47	78
$(1,6]$	1	9	9	31	50
$(6,12]$	0	3	1	2	6
$(12,\infty)$	0	0	0	0	0
$Total$	4	29	21	80	134

By dividing the entries in rows 1-4 in Table 2 by the corresponding row totals we get estimates of the conditional probabilities

$$P\left[T_c \in (p, q] | T_s \in (a, b]\right],$$

and in the last rows the unconditional probabilities $P\left[T_c \in (p, q]\right]$. These probabilities, times 100%, are presented in Table 3.

Table 3. Estimated conditional probabilities $P\left[T_c \in (p,q] | T_s \in (a,b]\right] \times 100\%$

$T_s \diagdown T_c$	$(0,1]$	$(1,6]$	$(6,12]$	$(12,\infty)$
$(0,1]$	5	21	16	58
$(1,6]$	3	22	19	56
$(6,12]$	3	20	18	59
$(12,\infty)$	0	20	20	60
$(0,\infty)$	4	21	18	57

Treatment group only:

$T_s \diagdown T_c$	$(0,1]$	$(1,6]$	$(6,12]$	$(12,\infty)$
$(0,1]$	5	21	17	57
$(1,6]$	3	23	20	54
$(6,12]$	4	14	18	64
$(12,\infty)$	0	20	20	60
$(0,\infty)$	4	21.5	18.5	56

Control group only (? = undefined):

$T_s \diagdown T_c$	$(0,1]$	$(1,6]$	$(6,12]$	$(12,\infty)$
$(0,1]$	4	22	14	60
$(1,6]$	2	18	18	62
$(6,12]$	0	50	17	33
$(12,\infty)$	?	?	?	?
$(0,\infty)$	3	21.6	15.7	59.7

Comparing the entries in rows 1-4 of Table 3 with the corresponding entries in row 5, it appears that for both groups separately and together in all but one case,

$$P\left[T_c \in (p,q] | T_s \in (a,b]\right] \approx P\left[T_c \in (p,q]\right]$$

The exception is the estimate of

$$P\left[T_c > 12 | T_s \in (6,12]\right]$$

for the control group, but this estimate is based on only two observations.

We have tested the null hypothesis that

$$P\left[T_c \in (p,q] | T_s \in (a,b]\right] = P\left[T_c \in (p,q]\right]$$

for $p \geq b$. In all cases the null hypothesis is not rejected at any conventional significance level!

Therefore, it seems that the events $T_c \in (p,q]$ and $T_s \in (a,b]$ for $b \leq p$ are independent.

However, if there is a treatment effect one would expect that in the case $G = 1$ these events are dependent:

$$P\left[T_c \in (p,q] | T_s \in (a,b], G = 1\right] \neq P\left[T_c \in (p,q] | G = 1\right], \ p \geq b$$

These results are compatible with independence of T_c and T_s conditional on the covariates, provided that the vector X of covariates can be partitioned as

$$X = (X_s', X_c')',$$

where X_s and X_c are **independent**, and

$$\begin{aligned} P\left[T_s \leq t|X\right] &= P\left[T_s \leq t|X_s\right], \\ P\left[T_c \leq t|X\right] &= P\left[T_c \leq t|X_c\right]. \end{aligned}$$

Therefore, at least for the control group, we will assume that these conditions hold.

If there is a treatment effect, then for $t > T_s$,

$$P\left[T_c \leq t|T_s, X_c, G = 1\right] \neq P\left[T_c \leq t|X_c, G = 1\right].$$

However, if the dependence of

$$P\left[T_c \leq t|T_s, X_c, G = 1\right]$$

on T_s is substantially reduced after X_c is integrated out, then the inequality

$$\begin{aligned} &P\left[T_c \in (p, q]|T_s \in (a, b], G = 1\right] \\ &\quad \neq P\left[T_c \in (p, q]|G = 1\right], \ p \geq b \end{aligned}$$

may no longer be detectable. Therefore, a treatment effect may still be possible, but if so it will likely work via the covariates X.

6 The Abbring-Van den Berg model

Abbring and Van den Berg (2003) consider the problem of identification of treatment effects in a bivariate mixed proportional hazard model, where one duration, S, is the timing of an intervention on another duration Y.

They specify the hazard functions of these duration as

$$\theta_S(t|X,V) = \lambda_S(t)\varphi_S(X)V_S$$

for the duration S and

$$\theta_Y(t|S,X,V) = \begin{cases} \lambda_Y(t)\varphi_Y(X)V_Y & \text{if } t \leq S \\ \lambda_Y(t)\varphi_Y(X)\delta\left(t|S,X\right)V_Y & \text{if } t > S \end{cases}$$

for the duration Y, where X is a vector of covariates, $V_S > 0$ and $V_Y > 0$ are unobserved heterogeneity variables that are independent of X, the $\lambda_i(t)$ and $\varphi_i(X)$, $i = S, Y$, are the baseline and systematic hazards, respectively, and $\delta\left(t|S,X\right)$ represents the conditional treatment effect.

In the case of the ESEO program, S is the job search duration T_s and Y is the recidivism duration T_c. Since T_s and T_c are interval censored, and the support of the covariates is countable, we cannot take unobserved heterogeneity into account, because otherwise the model would not be identified.

Besides, the effective sample size is too small for semi-nonparametric estimation of the unobserved heterogeneity distribution.

Thus, in our notation, the conditional hazard functions of the Abbring-Van den Berg model become

$$\theta_s(t|X) = \exp\left(\beta_s' X_s\right)\lambda_s(t)$$

for the job search duration T_s, with X_s a subvector of covariates relevant for job search, and

$$\theta_c(t|T_s, X, G) = \begin{cases} \exp\left(\beta_c' X_c\right)\lambda_c(t) & \text{if } t \leq T_s \\ \exp\left(\beta_c' X_c\right)\delta\left(t|T_s, X, G\right)\lambda_c(t) & \text{if } t > T_s \end{cases}$$

for the recidivism duration T_c, with X_c a subvector of covariates relevant for recidivism, where the treatment effect factor $\delta\left(t|T_s, X, G\right) = 1$ for the control group $G = 0$, and $\delta\left(t|T_s, X, G\right)$ is to be determined for the treatment group $G = 1$.

Given the proportional hazard structure of the model, the subvectors X_s and X_c of the covariates and the baseline hazards $\lambda_s(t)$ and $\lambda_c(t)$ will be determined empirically.

7 Job search

The conditional survival function of the job search duration T_s is

$$S_s(t|X) = \exp\left(-\exp\left(\beta_s' X_s\right)\Lambda_s(t)\right),$$

where

$$\Lambda_s(t) = \int_0^t \lambda_s\left(\tau\right) d\tau$$

is the integrated hazard.

Since we can only estimate $\Lambda_s(t)$ for $t \in \{1, 6, 12\}$, we may without loss of generality assume that $\Lambda_s(t)$ is piecewise linear:

$$\begin{aligned}
\Lambda_s(t|\alpha_s) &= \sum_{k=1}^{i-1} \alpha_k\left(b_k - b_{k-1}\right) + \alpha_i\left(t - b_{i-1}\right) \\
&\quad \text{for } t \in (b_{i-1}, b_i], \\
b_0 &= 0, b_1 = 1, b_2 = 6, b_3 = 12 \\
\alpha_i &> 0 \text{ for } i = 1, ..., 3,\ \alpha_s = (\alpha_1, \alpha_2, \alpha_3)'
\end{aligned}$$

In first instance we have included all available covariates in the job search model. Then we have conducted a series of Wald and likelihood ratio tests to determine the subvector X_s of covariates that are relevant for the job search duration. Moreover, on the basis of the estimation results for the piecewise linear integrated baseline hazard we have deducted the functional form of the underlying smooth baseline hazard $\lambda_s(t)$.

We find that only the location dummy variables matter for job search, so that

$$X_s = (CHICAGO, SANDIEGO)' .$$

Moreover, we cannot reject the null hypothesis that the baseline hazard is of the Weibull type.

Thus, the survival function of the job search duration T_s now takes the form

$$S_s(t|X) = \exp\left(-\exp\left(\beta_s' X_s\right) \alpha_{1,s} t^{\alpha_{2,s}}\right).$$

The estimation results for this model are presented in Table 4.

Table 4. Job search

Covariates	Estimates	t-val.
$CHICAGO$	-1.228695	-7.988
$SANDIEGO$	-0.379488	-2.745
Parameters		
$\alpha_{1,s}$	1.197083	10.880
$\alpha_{2,s}$	0.884122	15.957
Log-likelihood:	-422.063	
Sample size:	503	

The results in Table 4 are only final with respect to the model specification.

The coefficients involved will be re-estimated jointly with those of the recidivism model specified below. At that point we will interpret the results.

8 Recidivism of the control group

In view of the results of the preliminary data analysis, we will assume that conditional on the covariates the recidivism duration T_c for the **control group** is independent of the job search duration T_s. Moreover, similar to the job search case, the conditional survival function of the recidivism duration T_c for the control group will be modeled as a proportional hazard model:

$$S_c(t|X) = \exp\left(-\exp\left(\beta_c' X\right)\Lambda_c(t)\right),$$

where $\Lambda_c(t)$ is the integrated hazard.

Again, we may without loss of generality assume that $\Lambda_c(t)$ is piecewise linear:

$$\begin{aligned}
\Lambda_c(t|\alpha_c) &= \sum_{k=1}^{i-1} \alpha_k \left(b_k - b_{k-1}\right) + \alpha_i \left(t - b_{i-1}\right) \\
&\quad \text{for } t \in (b_{i-1}, b_i], \\
b_0 &= 0, b_1 = 1, b_2 = 6, b_3 = 12 \\
\alpha_i &> 0 \text{ for } i = 1, ..., 3,\ \alpha_c = (\alpha_1, \alpha_2, \alpha_3)'
\end{aligned}$$

We have followed the same specification strategy as for job search. Surprisingly, we find that none of the covariates matter for recidivism, and that the baseline hazard is constant. Thus, the distribution of T_c for the control group is exponential, so that the survival function involved takes the form

$$S_c(t|X) = \exp\left(-\alpha_c.t\right).$$

The maximum likelihood estimation result for α_c is presented in Table 5:

Table 5. Recidivism $(G = 0)$

Parameter	Estimate	t-val.
α_c	0.043681	7.396
Log-likelihood:	-139.357	
Sample size:	134	

Note that this result implies that

$$E\left[T_c|G = 0\right] = 1/\alpha_c \approx 23$$

months.

9 Incorporating conditional treatment

For the treatment group ($G = 1$), treatment is only received if $T_c > T_s$. Therefore we will assume that if $T_c \leq T_s$ the distribution of T_c is the same as for the control group:

$$P\left[T_c \leq t | T_s, X, G = 1\right] = 1 - \exp\left(-\alpha_c . t\right) \quad \text{if } t \leq T_s.$$

This is the ”no anticipation” condition in Abbring and Van den Berg (2003, Assumption 1).

Recall from the results of the preliminary data analysis that if there is a treatment effect then it will likely work via the covariates. Therefore, let

$$P\left[T_c \leq t | T_s, X_c, G = 1\right] = 1 - \exp\left(-\alpha_c . T_s\right) \times \exp\left(-\alpha_c . \exp\left(\beta_c' X_c\right) . \left(t - T_s\right)\right) \text{ if } t > T_s,$$

where X_c is the vector of relevant covariates, which now also includes 1 for the constant term.

Thus, the conditional survival function of the recidivism duration T_c given T_s, X_c, and G is specified as

$$S_c(t|T_s, X_c, G) = I\left(t \leq T_s\right)\exp\left(-\alpha_c.t\right)$$
$$+I\left(t > T_s\right)\exp\left(-\alpha_c.T_s\right)$$
$$\times \exp\left(-\alpha_c.\exp\left(G.\beta_c' X_c\right).\left(t - T_s\right)\right),$$

where $I(.)$ is the indicator function.

Next, rewrite the survival function of the job search duration T_s as

$$S_s(t|X) = \exp\left(-\exp\left(\beta_s' X_s\right) t^{\alpha_s}\right),$$
$$X_s = \left(CHICAGO, SANDIEGO, 1\right)'$$

Then it follows that for $0 \leq a < b \leq p < q$,

$$P\left[T_c \in (p, q], T_s \in (a, b] | X, G\right]$$
$$= \int_{S_s(b|X_s)}^{S_s(a|X_s)} \exp\left[-\alpha_c.\exp\left(-\alpha_s^{-1}\beta_s' X_s\right)\right.$$
$$\left.\times \left(1 - \exp\left(G.\beta_c' X_c\right)\right)\left(\ln\left(1/u\right)\right)^{1/\alpha_s}\right] du$$
$$\times \left[\exp\left(-\alpha_c.\exp\left(G.\beta_c' X_c\right).p\right)\right.$$
$$\left. - \exp\left(-\alpha_c.\exp\left(G.\beta_c' X_c\right).q\right)\right]$$

The parameters involved can now be (re-)estimated by maximum likelihood.

10 Joint maximum likelihood results

Again, we have conducted a series of Wald and likelihood ration test to remove insignificant covariates in the recidivism part of the model.

Table 6. Job search, recidivism and treatment effects

Job search	Estimates	t-val.
α_s	0.875049	12.320
$CHICAGO$	-1.225155	-7.440
$SANDIEGO$	-0.324369	-2.141
1	0.184070	1.868
Recidivism		
α_c	0.041905	7.664
$BOSTON$	0.425046	2.304
AGE	-0.045503	-2.720
1	1.222241	2.536
Log-Likelihood:	-847.358	
Sample size:	503	

The result is that only two covariates matter for treatment: Age and the location dummy Boston.

The estimation results for job search are very close to the previous results, as expected.

The significant negative signs of the location dummies in the left-side panel of Table 6 indicate that the job search takes longer in Chicago and San Diego than in Boston, and in Chicago longer than in San Diego. Thus, its seems that the ex-convicts in Boston receive more or better help with the job search than in the other two cities.

Recall that for the control group the recidivism duration has an exponential distribution which does not depend on covariates. The two covariates in the right-hand side panel of Table 6 are therefore related to the effect of treatment for the treatment group only. How to interpret these results will be discussed next.

11 Treatment effects

Our model implies that

$$\begin{aligned} &E[T_c|T_c > T_s, T_s, X_c, G = 1] \\ &\quad - E[T_c|T_c > T_s, T_s, X_c, G = 0] \\ &\quad = \alpha_c^{-1} \left(\exp\left(-\beta_c' X_c\right) - 1\right). \end{aligned}$$

This expression may be interpreted as (a version of) the conditional treatment effect. Thus, the treatment has a positive effect, in the sense that treatment increases the expected time between release and rearrest, if $\beta_c' X_c < 0$, regardless the job search duration.

Our estimation results yield

$$\begin{aligned} \widehat{\beta}_c' X_c = &\ 1.222241 + 0.425046.BOSTON \\ &-0.045503.AGE. \end{aligned}$$

As to the "Boston" effect, $\widehat{\beta}_c' X_c$ is larger for Boston than for the other two locations, so that ceteris paribus the conditional treatment effect on recidivism in Boston is less than in Chicago and San Diego. This difference increases with age.

Moreover, the treatment reduces the risk of recidivism in Chicago and San Diego if

$$AGE > 27,$$

and in Boston if

$$AGE > 36.$$

Thus, in general, treatment only reduces the risk of recidivism for older ex-inmates, and increases the risk of recidivism for younger ex-inmates! With how much is illustrated in Figure 1.

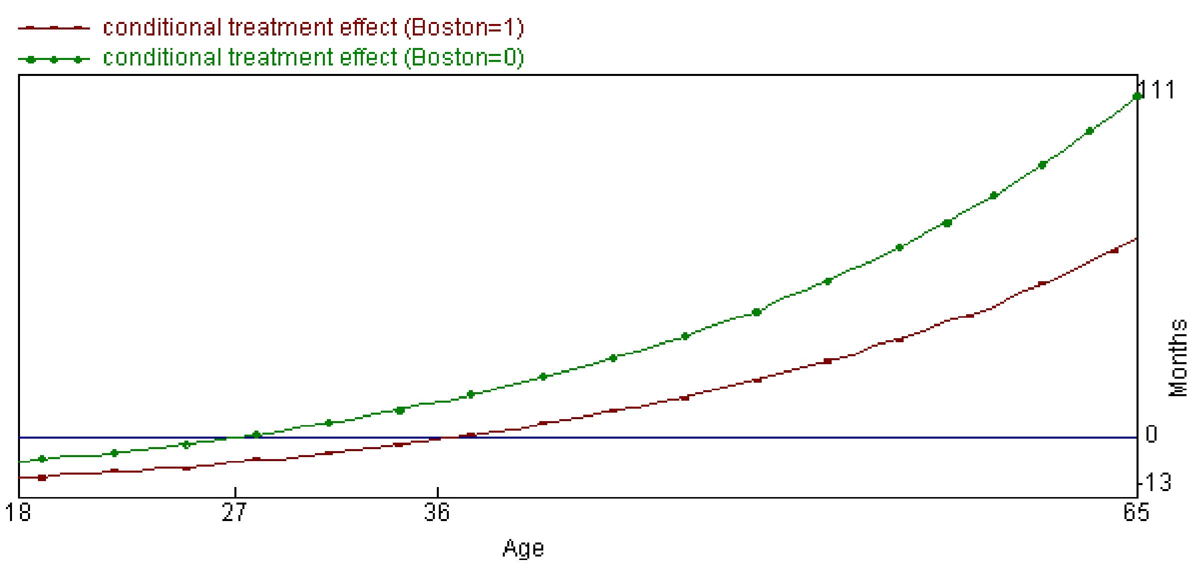

Figure 1: Conditional treatment effect on recidivism.

12 Comparison with the preliminary data analysis

In the preliminary data analysis we have argued that if the dependence of $P\left[T_c \leq t | T_s, X_c\right]$ on T_s is substantially reduced after X_c is integrated out, then the inequality

$$P\left[T_c \in (p,q] | T_s \in (a,b], G=1\right] \neq P\left[T_c \in (p,q] | G=1\right], \ p \geq b$$

may no longer be detectable. To verify this conjecture, we have computed

$$P\left[T_c \in (p,q] | T_s = t_s, X_c, G=1\right], \ t_s < p$$

on the basis of our estimation results, and then averaged these estimates over the treatment group.

Indeed, we find that the dependence of these conditional probabilities on $T_s = t_s < p$ is weak, which explains why we could not find any dependence.

Table 7. Estimated $P\left[T_c \in (p,q] | T_s = t_s, G = 1\right]$

p	q	Range of t_s	Mean	Minimum	Maximum
1	6	$0 \longrightarrow 1$	0.20697	0.20567	0.20829
6	12	$0 \longrightarrow 1$	0.18507	0.18405	0.18610
6	12	$1 \longrightarrow 6$	0.19164	0.18610	0.19753
12	∞	$0 \longrightarrow 1$	0.56324	0.56194	0.56457
12	∞	$1 \longrightarrow 6$	0.57202	0.56457	0.58015
12	∞	$6 \longrightarrow 12$	0.59190	0.58015	0.60480

13 Conclusions

In contrast with previous studies we find that the ESEO program has an effect on recidivism, but this effect depends on age and location: the ESEO program reduces the risk of recidivism only for ex-inmates over the age of 27 in San Diego and Chicago, and over the age of 36 in Boston, but increases the risk of recidivism for the other ex-inmates in the treatment group.

In view of Figure 1 it seems that the positive effect of the treatment for the older ex-convicts outweighs the negative effect for the younger ex-convicts, in terms of the expected number of months with which the rearrest will be postponed.

Our results provide evidence that employment programs for ex-offenders can reduce recidivism, provided that these programs take the heterogeneity of the population of ex-offenders into account. A program that is uniform for all ex-offenders may not yield the expected results.