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Evaluating the Effectiveness of Unemployment Insurance in Reducing Recidivism

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ABSTRACT

Beginning in 1972, the U.S. Department of Labor sponsored an experiment providing subsidies in the form of unemployment insurance to released prisoners in Baltimore, on the ground that such a cash cushion would reduce the incentive for otherwise destitute prisoners to return to a life of crime immediately upon release. Evaluation of the carefully designed experiment claimed that financial aid reduced recidivism, but retests in Texas and Georgia were unable to replicate the Baltimore results. This paper argues that a problem with the correct specification of the economic model of crime in the original Baltimore evaluation led to a gross overestimation of the effectiveness of unemployment insurance benefits in reducing recidivism, and estimates three alternative specifications of the recidivism function.

EVALUATING THE EFFECTIVENESS OF UNEMPLOYMENT INSURANCE IN REDUCING RECIDIVISM

In a now classic, scathing review of rehabilitation programs designed to reduce criminal recidivism, Robert Martinson concluded that "nothing works".[1] Summarizing the results of many years of research on the effectiveness of numerous correctional programs, Martinson could find little support for the belief that training, educational, or vocational programs for inmates would significantly reduce postprison recidivism. But the review was based largely on <u>in-prison</u> correctional programs. Clearly hope remained for the scattered efforts to rehabilitate exoffenders in programs existing outside prison walls. Notably, employment training, job counseling, and financial assistance programs for released prisoners were viewed as attractive alternatives to the correctional rehabilitation strategies. The road was paved for innovative experimental ventures designed to reduce crime.

One experiment in providing cash subsidies to released prisoners had a particular appeal to labor market analysts. Not only did few exoffenders qualify for that stalwart of support for many jobless, unemployment insurance, they generally failed to have enough money to make it through the first few days of postprison adjustment even before beginning to look for a job. Gate money in many states is low, the savings of released prisoners minimal, and the resources of family and friends are often sorely strained, making an additional mouth to feed the source of significant stress and conflict. So, an experiment in increasing the financial resources of recently released prisoners seemed eminently reasonable. The U.S. Department of Labor sponsored such an experiment in Baltimore beginning in 1972. The program, dubbed Living Insurance for Exoffenders (LIFE) provided what was in effect unemployment insurance of up to $60.00 per week for thirteen weeks.[2]

The logic of the experiment was straightforward. If a cash cushion were provided for released prisoners, their incentive to return to a life of crime immediately upon release from prison would be diminished. After some period of job search, individuals would find better, higher-paying jobs and in the long run would adjust better and be less likely to turn to crime than offenders without this special financial assistance.

One crucial point evidenced here is that the program was politically feasible. No major legislative labyrinth impeded the extension of normal unemployment benefit coverage to released prisoners. Nor could critics argue that exoffenders would be receiving special treatment when tens of thousands of other disadvantaged workers went without similar government subsidized support.

At first glance, the Baltimore LIFE experiment was a success. Evaluations of the carefully designed experiment revealed that the financial aid reduced recidivism. Twenty fewer arrests could be attributed to treatment effects.[3] The program was expanded and tested in Texas and Georgia. But success was not forthcoming there. Unable to replicate the Baltimore results, researchers Rossi, Berk, and Lenihan have sought to explain the subsequent failure.[4] They have four basic explanations. First, the Georgia and Texas experiments were administered differently. Whereas in Baltimore the cash subsidies were paid out by the researchers, in the retests correctional personnel or state employment agency officials were responsible for making the unemployment benefit outlays. Second, the tax rates varied from the earlier experiment. Lenihan suggests that the effective tax rate on the unemployment insurance benefit was approximately zero in the Baltimore case.[5] The Texas and Georgia retests, on the other hand, had built-in explicit tax rates varying to 75%.

Third, the sample's size and composition were enlarged in the later experiments. The Baltimore test included only male, repeat offenders with no drug history, but the Georgia and Texas samples included females and first offenders. Finally, there existed strong work disincentives in both sets of experiments. It could be argued that the zero effective tax rate in the Baltimore test merely masked some of this work reduction effect, which, as the retests discovered, overshadows the reduction in recidivism.

Each of these explanations for the inability to replicate the Baltimore experiment is equally plausible. Yet the last one is bothersome for analysts concerned with the reliability of the original program evaluation. If indeed there were work disincentive effects in the Baltimore experiment, what exclusion, omission, or oversight led the analysts to inadvertently overlook them? In this paper, I argue that a problem with the correct specification of the economic model of crime led to a gross overestimation of the effectiveness of unemployment insurance benefits in reducing recidivism. I first sketch a highly simplified economic model of optimal participation in crime. Then alternative specifications of the recidivism function, suggested by economic theory, are offered and estimated. Different estimators yield different results; in a penultimate section I compute the net reduction in recidivism arising from the experiment from various estimations and compare these computations with those obtained by earlier researchers.

A THEORETICAL PERSPECTIVE

Suppose that there are exactly two income-earning activities, work and crime. Initially, we might assume that work is a riskless activity and crime is rewarded at a rate, G, if one is successful, -L if one is not. The probability of success is given by $(1-\alpha)$. Denote r as the random rate of return to crime. It is easy to see that the expected rate of return to crime is

$$E(r) = (1-\alpha)G-\alpha L.$$

Income Y is given by the sum of illegal and legal earnings. Let t be the fraction of time allocated to crime and (1-t) the fraction of time allocated to work. Then expected income is found to be

$$E(Y) = t[(1-t)G-\alpha L] + (1-t)w$$

where w is the wage rate. If the rational, self-interested, potential criminal acted as if he maximized his expected income, then the optimal allocation of time to crime, t*, would satisfy the following rule:

$$\text{If } \frac{E(r)}{w} > 1 \Longrightarrow t^* = 1,$$

$$\text{if } \frac{E(r)}{w} < 1 \Longrightarrow t^* = 0,$$

and

$$\text{if } \frac{E(r)}{w} = 1 \Longrightarrow 0 \leq t^* \leq 1.$$

In other words, all time would be allocated to that activity with the highest rate of return.

In the interest of realism, let us assume now that work is risky, i.e., the rate of return, w, is not a constant but rather stochastic. Then we might suppose that it takes on the value $\overline{w}$ if one is employed with probability (1-u), and it is equal to 0 otherwise. In this case the optimality conditions are essentially the same:

$$\frac{E(r)}{(1-u)\overline{w}} > 1 \Longrightarrow t^* = 1,$$

$$\frac{E(r)}{(1-u)\overline{w}} < 1 \Longrightarrow t^* = 0,$$

and

$$\frac{E(r)}{(1-u)\overline{w}} = 1 \Longrightarrow 0 \leq t^* \leq 1.$$

It is worth noting that this is the simplest economic representation of the often repeated claim "unemployment causes crime." As u, the probability of being unemployed, rises, the expected return to work (the denominator in the above expressions) falls. So the relative attractiveness of crime to work increases and the allocation of time to crime will rise, if initially one were indifferent between engaging in crime and work. Of course, as the wage received if one does work increases, so too does the expected wage, and therefore the relative attractiveness of participating in crime diminishes.

The case for unemployment insurance can be seen clearly in the context of this simple model. Rewrite the expected wage, E(w), as:

$$E(w) = (1-u)\overline{w} + u \cdot I.$$

The expected wage is equal to the wage if employed, plus the unemployment benefit, I, if unemployed. Clearly the unemployment benefit raises expected wages and thereby lowers the relative attractiveness of crime.[6]

This model is highly simplified. It does not detail the dynamics of job search in the real world, or even the demand-side effects of employers' hiring criteria. But even in this highly simplified model, it is a trivial matter to contrive an explanation for the fact that unemployment insurance may not reduce recidivism.

Suppose that the probability of being unemployed is functionally dependent upon the level of unemployment benefits.[7] We could write the expected wage then as:

$$E(w) = [(1-u(I))]\overline{w} + u(I)I.$$

A little computation reveals now that increased unemployment insurance does not unambiguously increase expected wages and thereby reduce the relative attractiveness of crime.[8] In fact, to the extent that increased unemployment benefits may increase unemployment, and increased unemployment may lower expected wages, it is possible for higher unemployment benefits to result in <u>higher</u> crime rates. It all depends on the extent to which unemployment rates are raised by the benefits and upon the wage rate and the probability of unemployment. Paradoxically, the work disincentive effect would be smallest in this simple model when the wage rates are very low or the unemployment rate is very high.[9] The more disadvantaged the population, the better this intervention strategy can be in reducing recidivism.

In summary, from a theoretical perspective, the recidivism function should depend upon the expected returns to work and crime; these depend in turn upon the gains, losses and the probability of success in crime, and the wage, unemployment and unemployment benefit rates.

SPECIFICATION AND ESTIMATION OF THE RECIDIVISM FUNCTION

In the economics literature there exist numerous examples of attempts to estimate what are essentially offense supply functions.[10] Without exception, inclusion of variables like the certainty and severity of punishment along with measures of legitimate opportunities is regarded as central for a "correct" specification of what has come to be called the "economic model of crime." Mallar and Thornton, in their excellent evaluation of the LIFE experiment, omit both kinds of measures in these specifications of the rearrest function.[11] In particular, one could argue, the omission of expected wages--or some component of wage rates--would bias upward the coefficient associated with receipt of unemployment insurance. Both variables should ideally be included.

Using the same data set as Mallar and Thornton, I have constructed a number of proxies for the desired variables in the economic model of crime.[12] The certainty of punishment is measured by the ratio of previous convictions to previous arrests. This could be regarded as the individual's subjective probability of getting punished again. The severity of punishment is measured by time served on the last offense; it is the difference between the year of arrest for the current conviction and the year of release--an admittedly crude proxy, but the best available measure given the limitation of the data set. Although there are no measures of the gains to crime, variables like age and race could be correlated with criminal returns.

Legitimate opportunities are captured in a variety of ways. First, education can be viewed as a form of investment in future earnings. Second, higher earnings may be associated with greater experience. A measure of experience is computed as the length of time on the longest job held prior to incarceration, appropriately discounted by the length of time since that job was held.[13] Third, expected wages are computed as the average weekly wage for each month. Annually, this measure takes account of the weeks unemployed during the year. On a monthly basis, this measure incorporates the weeks unemployed during the entire month. Receipt of unemployment insurance is entered as a separate variable rather than appended to the expected wage variable as is done in the expected wage equation in the previous section. This is done both because the actual amounts received are not available in this version of the Baltimore LIFE tape and because of a desire to estimate the separate effects of the unemployment benefit. Each of these measures of legitimate opportunities is expected to be inversely related to recidivism.

Because unemployment probabilities are significantly affected for exoffenders by the job arrangements prior to release from prison, the variable job arrangement was included. To ward off the possible bias associated with selective screening by correctional personnel, a last control for type of prior release was made.

The results of maximum likelihood estimates of logistic functions for the probability of being rearrested in the t^{th} month are presented in Table 1. In the last column are the results of estimates of the probability of being rearrested during the year. Note that the dependent variable takes on the value of 0 if "successful," but only becomes 1, denoting rearrest, in at most one month. Thus, the sum of the monthly probabilities equals the annual rearrest rate. This is somewhat of an anomaly. If the experiment works best to reduce crimes among those who would have committed only one crime during the year, then the estimated treatment effect using this dichotomous measure would seriously overstate the crime reduction benefits.[14]

Nonetheless, the results are revealing. In the annual equation, increases in the average weekly wage have a strong negative effect on the rearrest rate. While receiving unemployment insurance reduces recidivism the estimated coefficient is only significant at the 10% level. This one-tailed statistical test is notably weaker than the 1% level met by

Table 1

MAXIMUM LIKELIHOOD ESTIMATES OF LOGIT MODEL OF POST PRISON ARRESTS
(t-statistics in parentheses)

Independent Variable	Annual Equation	Monthly Equations											
		A_1	A_2	A_3	A_4	A_5	A_6	A_7	A_8	A_9	A_{10}	A_{11}	A_{12}
Constant	2.529	-4.495	-1.338	-6.868	-3.464	-4.895	-1.978	-1.866	.025	.093	1.666	-15.393	.507
	(3.352)	(-1.902)	(-.863)	(-3.641)	(-1.838)	(-2.583)	(-1.398)	(-.950)	(.001)	(.054)	(.872)	(-.015)	(.034)
Wage	-.015	-.013	-.010	-.018	-.019	-.004	-.001	-.006	-.002	-.000	-.004	-.008	.002
	(-5.943)	(-1.393)	(-2.035)	(-3.215)	(-2.784)	(-.880)	(-.238)	(-1.187)	(-.453)	(-.116)	(-.974)	(-2.107)	(.506)
Treatment Group	-.291	-1.370	-.319	.146	.779	.108	-.912	-.382	.088	1.11	-.833	-.094	-.330
	(-1.377)	(-1.650)	(-.720)	(.311)	(1.479)	(.225)	(-2.180)	(-.700)	(.156)	(2.263)	(1.636)	(-.207)	(-.712)
Education	-.058	.073	-.435	.191	.149	.121	-.055	-.037	-.015	-.250	.011	-.221	-.216
	(-1.078)	(.405)	(-.384)	(1.654)	(1.155)	(.979)	(-.556)	(.273)	(-.094)	(-1.930)	(.086)	(-1.700)	(-1.809)
Experience	-.001	-.025	.922	-.016	-.034	-.039	.013	-.011	-.016	.002	.031	-.014	.021
	(-.107)	(-.890)	(.531)	(-.891)	(-1.371)	(-1.66)	(.902)	(-.464)	(-.599)	(.127)	(1.756)	(-.784)	(1.238)
Race	.513	.202	.028	1.182	.053	.927	.313	-.103	.678	-.955	-.426	15.747	.430
	(1.599)	(.184)	(.044)	(1.117)	(.065)	(.886)	(.493)	(-.132)	(.638)	(-1.660)	(-.707)	(.015)	(.560)
Age	-.035	.095	-.016	.070	-.026	.004	-.022	-.001	-.136	-.027	-.133	-.084	-.052
	(-1.622)	(1.61)	(.326)	(1.660)	(-.396)	(.073)	(-.503)	(-.010)	(-1.286)	(-.529)	(-1.875)	(-1.202)	(-.987)
Time Served	.054	-.103	.699	-.024	.179	-.001	.041	-.168	-.239	-.238	-.310	-.244	.009
	(-.915)	(-.482)	(.591)	(-.252)	(1.629)	(-.009)	(.427)	(-.900)	(-.917)	(-1.379)	(-1.250)	(-1.363)	(.067)
Paroled	.087	.187	-.110	.0411	-1.503	.732	.319	-.472	.441	.975	-.140	1.268	-.381
	(.321)	(.235)	(-.213)	(-.727)	(-2.654)	(1.082)	(.579)	(-.755)	(.623)	(1.575)	(-.245)	(1.959)	(-.681)
Job Arranged	-.239	-.761	-.463	1.318	.559	-.324	.105	.575	-.371	-1.409	.021	-.399	-.048
	(-1.025)	(-.951)	(-.953)	(2.326)	(1.027)	(-.636)	(.234)	(.950)	(-.630)	(-2.732)	(.042)	(-.818)	(-.092)
Convictions/ Arrests	-.136	-1.17	-.571	-.393	.213	.049	-.364	.425	.371	1.088	-.159	1.437	-.553
	(-.597)	(-1.62)	(-1.27)	(-.798)	.388	(.095)	(-.896)	(.703)	(.603)	(1.853)	(-.338)	(2.305)	(-1.192)
X^2	70.352	12.128	8.810	21.515	30.601	9.309	7.790	5.650	10.599	27.247	18.155	24.480	8.077

the wage variable. The only other variables significant even at the 10% level are race and age.

Turning to the monthly equations the results are even more striking. Whereas in the first four months average weekly wages are strongly related to lower recidivism, the effects of the financial aid are mixed. In only the first, sixth, and ninth months are the estimated coefficients of the treatment effect significant at the 5% level. Then, in the ninth month the effect is positive! Part of this arises because of the odd way of measuring monthly rearrest rates, a point that can easily be addressed by redefining success.

An alternative specification, detailed in Table 2, is estimated to capture a more intuitive notion of postprison success. Here the dependent variable is defined as the probability that the individual was not rearrested in month t, given that up until that point he was not rearrested. This conditional probability denotes in essence the survival rate. The independent variables are the same and the results are no less surprising. In every month, save the first, the average weekly wage is positively related to success and significant at the 1% level. In the first month the level of significance drops to 10%, but the effect is still positive. The effects of the financial aid on survival, though, are less clear cut. In the first month there is a large effect on postprison survival, although it is not strongly significant, not quite reaching the 5% level. In the second month there is a slight positive effect. In no other month can we ascertain an effect significantly different from zero. Importantly, in the first and second months where the treatment efforts appear operative, the overall explanatory power of the estimated equations is low. Performing a likelihood

TABLE 2

Maximum Likelihood Estimates of Coefficients in Logistic Model of Monthly Survival Probabilities
(t-statistics in parentheses)

Independent Variable	Month 1	Month 2	Month 3	Month 4	Month 5	Month 6	Month 7	Month 8	Month 9	Month 10	Month 11	Month 12
Constant	4.849	1.826	2.906	2.619	2.555	1.480	1.041	0.431	-0.421	-1.398	-1.702	-2.541
	(2.012)	(1.356)	(2.640)	(2.608)	(1.796)	(1.801)	(1.289)	(0.551)	(-0.562)	(-1.924)	(2.259)	(-3.350)
Treatment Group	1.297	0.576	0.358	-0.009	-0.071	0.256	0.255	0.250	0.024	0.184	0.194	0.250
	(1.572)	(1.482)	(1.163)	(-0.032)	(-0.280)	(1.144)	(1.151)	(1.153)	(0.116)	(0.887)	(0.933)	(1.214)
Nonwhite	-.100	-0.085	-0.353	-0.361	-0.561	-0.508	-0.509	-0.680	-0.350	-0.210	-0.444	-0.410
	(-.092)	(-0.150)	(-0.688)	(-0.784)	(-1.325)	(-1.370)	(-1.431)	(-1.954)	(-1.077)	(-0.662)	(-1.3898)	(-1.300)
Paroled	-.207	0.010	0.137	0.752	0.506	0.291	0.388	0.297	0.044	0.091	-0.049	0.024
	(-.262)	(0.022)	(0.368)	(2.272)	(-1.675)	(1.040)	(1.410)	(1.101)	(0.167)	(0.351)	(-0.187)	(0.091)
Job Arranged	.684	0.533	-0.264	-0.388	-0.147	-0.148	-0.178	-0.102	0.362	0.301	0.284	0.332
	(.855)	(1.272)	(-0.769)	(-1.267)	(-0.532)	(-0.586)	(-0.726)	(-0.428)	(1.579)	(1.343)	(1.242)	(1.452)
Experience	.026	0.004	0.008	0.013	0.022	0.015	0.011	0.014	0.009	0.0000004	0.009	0.005
	(.893)	(0.026)	(0.700)	(1.119)	(2.110)	(1.695)	(1.304)	(1.608)	(1.121)	(0.998)	(1.082)	(0.626)
Convictions/Arrests	-.197	0.234	0.133	-0.028	0.204	0.817	0.449	0.368	-0.077	0.060	-0.204	-0.116
	(-.155)	(0.344)	(0.237)	(-0.055)	(0.450)	(1.983)	(1.126)	(0.943)	(-0.204)	(0.162)	(-0.553)	(-0.315)
Age	-.786	-0.016	-0.040	-0.029	-0.036	-0.031	-0.018	0.0004	0.012	0.033	0.030	0.043
	(-1.356)	(-0.414)	(-1.326)	(-0.977)	(-1.364)	(-1.314)	(-0.792)	(0.019)	(0.569)	(1.710)	(1.382)	(2.023)
Time Served	.146	0.002	0.022	-0.046	-0.025	-0.032	-0.023	-0.032	-0.006	0.010	0.071	0.060
	(.663)	(0.022)	(0.304)	(-0.700)	(-0.421)	(-0.577)	(-0.429)	(-0.596)	(-0.106)	(0.204)	(1.288)	(1.082)
Education	-.089	0.004	-0.082	-0.122	-0.121	-0.077	-0.066	-0.040	-0.003	0.004	0.042	0.082
	(-.518)	(0.046)	(-1.074)	(-1.712)	(-1.898)	(-1.334)	(-1.183)	(-0.745)	(-0.057)	(0.085)	(0.786)	(1.582)
Average Weekly Wage in in Month t	.012	0.010	0.016	0.019	0.013	0.010	0.011	0.011	0.010	0.010	0.009	0.007
	(1.282)	(2.444)	(4.519)	(5.927)	(5.170)	(4.814)	(5.584)	(5.577)	(5.707)	(5.848)	(5.337)	(4.337)
[Mean Weekly Wage in Month t]	[$49.75]	[$57.09]	[$60.19]	[$65.70]	[$63.71]	[$63.34]	[$62.24]	[$61.24]	[$59.00]	[$60.08]	[$58.56]	[$51.26]
Mean Survival Rate	97.92%	92.59%	87.73%	83.33%	78.94%	72.22%	68.75%	65.71%	60.19%	55.32%	51.62%	46.76%
X^2	9.515	13.388	29.848	61.946	49.716	40.784	50.284	51.734	52.822	53.914	58.224	50.524

ratio test suggests that one should reject the hypothesis, on the basis of the low chi-squared value, that the logistic function with its included independent variables would predict survival rates better than the mean survival rate for the sample.

Until now, we have argued that exclusion of other variables like expected wages biases upwards the coefficient of the financial aid variable. A further complaint, though, arises when we include expected wages--and their implied component of unemployment--without taking into account the inherent simultaneity of participation in crime with participation in work. A third specification is implied here.

Recidivism depends upon expected wage. The expected wage, though, depends upon hours worked (i.e., unemployment). The greater the average weekly hours worked, the higher will be the average weekly wage earnings. But hours worked depend upon time spent in crime. To the extent that people combine work and crime, this is no constraint. But what about the people who get caught and go to jail? Being incarcerated reduces the hours available to work and thus, ceteris paribus, lowers the expected wage. Now, to complete this model, a final equation is needed to determine days spent in jail per week. Those who get rearrested are more likely to spend days in jail than the survivors. Thus there is a simultaneous equation system from which it is possible to estimate separately the recidivism and work disincentive effects. These results are displayed in Table 3.[15]

As we hypothesized, higher wages reduce rearrests; longer hours worked increase weekly wages; days in jail restrict hours worked; and higher rearrest rates increase days in jail. The separate effects of the financial assistance are everywhere of the same sign as the right-hand-side endogenous variable.

Table 3

Instrument Variable Estimates of Postprison Outcomes
(t-statistics in parentheses)

	Rearrest Equations		Wage Equations		Hours Worked Equations		Days in Jail Equations	
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constraint	.982 (5.608)	.864 (13.356)	-76.347 (-2.807)	-44.398 (-2.554)	23.440 (6.017)	25.540 (9.437)	.427 (.509)	.332 (.998)
Treatment	-.057 (-1.225)	—	6.208 (1.305)	—	-1.798 (-2.168)	-1.877 (-2.277)	.007 (.056)	—
Wage	-.005 (-2.750)	-.006 (-5.502)	—	—	—	—	—	—
Hours Worked	—	—	3.682 (2.941)	3.796 (5.100)	—	—	—	—
Days in Jail	—	—	—	—	-2.893 (-1.923)	-4.221 (-4.119)	—	—
Rearrest	—	—	—	—	—	—	1.114 (1.723)	1.354 (2.991)
Education	-.014 (-1.168)	—	.132 (.110)	—	.382 (1.736)	.366 (1.700)	-.001 (-.030)	—
Convictions/Arrests	.018 (.227)	—	2.653 (.358)	—	-2.699 (-1.649)	-3.250 (-2.145)	-.456 (-1.933)	-.395 (-1.728)
Time Served	-.010 (-.877)	—	.572 (.542)	—	.124 (.592)	—	.011 (.336)	—
Paroled	.020 (.334)	—	6.299 (.117)	—	-.822 (-.743)	—	-.221 (-1.302)	—
Job Arranged	-.021 (-.361)	—	-.123 (-.017)	—	4.048 (4.275)	3.658 (4.157)	.087 (.533)	—
Age	-.005 (-1.184)	—	.564 (1.313)	—	.037 (.425)	—	-.007 (-.488)	—
Race	.113 (1.540)	—	9.625 (1.491)	—	-.582 (-.423)	—	.298 (1.455)	—
Experience	.001 (.679)	—	.339 (1.809)	.471 (2.922)	.036 (1.002)	—	-.008 (-1.728)	-.009 (-2.079)
Skilled Blue Collar	—	—	9.479 (1.168)	—	—	—	—	—
Living with Family	—	—	—	—	-1.390 (-1.583)	—	—	—
Unskilled	.077 (1.469)	—	—	—	—	—	—	—
Family Members in Prison	—	—	—	—	—	—	.159 (1.147)	—

Thus the unemployment insurance lowers rearrest, raises wages, reduces hours worked, and increases days in jail. This is problematical because the net effect of the treatment is no longer unambiguous. And here is where the work disincentive effect is seen most clearly. One would need to work more hours to raise wages and thereby reduce recidivism. But the unemployment insurance tends to reduce hours worked. To assure that the insurance benefit actually results in reduced crime, then we must show that on balance the positive wage effects offset the negative work reduction effects.[16]

With a little effort one can solve the odd-numbered equations in Table 3 simultaneously for the rearrest rate and then differentiate the resulting value with respect to the treatment variable. One discovers then that

$$\frac{\partial t^*}{\partial I} = \frac{1}{1-\gamma_1\gamma_2\gamma_3\gamma_4} \left(\alpha_1 + \alpha_2\gamma_1 + \alpha_3\gamma_1\gamma_2 + \alpha_4\gamma_1\gamma_2\gamma_3\right)$$

where t^* is the rearrest rate, I is the unemployment insurance variable, and

α_1 = unemployment (treatment) coefficient in rearrest equation
α_2 = unemployment (treatment) coefficient in wage equation
α_3 = unemployment (treatment) coefficient in hours equation
α_4 = unemployment (treatment) coefficient in jail equation
γ_1 = wage coefficient in rearrest equation
γ_2 = hours coefficient in wage equation
γ_3 = jail coefficient in hours equation
γ_4 = rearrest coefficient in jail equation.

A little arithmetic reveals that the treatment effect is about -0.06, in the same order of magnitude estimated in the rearrest equation and displayed in column 1 of Table 3.[17]

Immediately we realize, though, that many of the coefficients used to arrive at this figure are insignificant. In particular, the coefficient of the treatment effect in the rearrest equation is insignificant at the 5% level. The model, therefore, was reestimated omitting all variables with coefficients insignificant at the 5% level.[18] The same computation was performed to arrive at the net treatment effect. Now the effect of unemployment insurance is to <u>increase</u> rearrest rates! There is an intuitive way to see this. From the even-numbered columns of Table 3 it is seen that the only direct effect of the treatment is on hours worked. Here, unemployment reduces the average weekly hours. By lowering hours worked we depress the wage, which in turn increases rearrest, thereby raising days in jail and further reducing hours worked. Thus a multiplier effect is operative here, with insufficient offsetting effect to keep from increasing crime. This is clearly the most extreme case of work disincentive. Briefly, we inspect the total reduction in arrests in the previous cases and this one.

ESTIMATED REDUCTIONS IN ARRESTS

It is convenient to know not only the direction of the treatment effect but also its magnitude. In following the analysis of Mallar and Thornton, it is possible to derive the change in rearrests attributable to the financial aid experiments by multiplying the change in the probability of rearrest due to the experiment by the number of subjects receiving the cash subsidy, in this case 216. For nonlinear models the probability change in question, or the partial derivative, is not a constant. I have chosen, for computational convenience, to evaluate the derivatives at the mean of the dependent variable, a procedure equivalent to evaluating the derivatives of the means of the independent variables when the estimated error approaches zero.[19] In linear models, of course, this evaluation procedure is not necessary.

Table 4 summarizes the rearrest computations for each of the three specifications suggested in the previous section. First, in column 1, however, the bench mark value derived by Mallar and Thornton is given.[20] More than 20 rearrests are diverted by the experiment, according to their calculations.

In column 2, the rearrest reductions computed from Table 1--the monthly and annual specifications of the conventional economic model of crime--are displayed. Noting that in some months the treatment effect is positive and in other months it is negative, we obtain the sum for the year. This total, denoting a reduction in rearrests by 14.5, is contrasted with the reduction computed from the annual equation. When the average rearrests for the year are estimated, the reduction in rearrests due to the experiment is calculated to be 15.6. Thus the annual derivation overestimates the total of the monthly tallies by more than one rearrest among 216 participants.[21]

In column 3 the increased number of survivors for each month is displayed. The numbers are premultiplied by -1 to reflect the fact that an added survivor is really a diverted rearrestee. The value computed for the twelfth month is essentially the annual estimated reduction. It is lower than the Mallar and Thornton value and both the total monthly and annual reductions obtained in the conventional economic model.

To compute the total monthly reductions in the survival model, it is necessary to find the change in survivals from month to month. This is done in column 4. The sum of these changes, -7.73, represents the number of fewer rearrests among those exoffenders receiving financial aid. This value,

Table 4

Estimated Reductions in Rearrests

	Mallar & Thornton Model (1)	Conventional Economic Model (2)	Survival Model (3)	Change in Survivals (4)	Simultaneous Equation Model (5)	Simultaneous Equation Model (6)
Month 1	--	-6.2	-5.71			
				-2.83		
Month 2	--	-3.5	-8.54		--	--
				0.22		
Month 3		1.15	-8.32		--	
				8.35		
Month 4	--	7.1	0.03		--	--
				2.52		
Month 5	--	1.0	2.55		--	--
				-13.64		
Month 6	--	-12.3	-11.09		--	--
				-0.74		
Month 7	--	-2.8	-11.83		--	--
				-0.34		
Month 8	--	0.6	-12.17		--	--
				10.93		
Month 9	--	12.5	-1.24		--	--
				-8.58		
Month 10	--	-8.4	-9.82		--	--
				-0.65		
Month 11	--	-0.7	-10.47		--	--
				-2.97		
Month 12	--	-3.3	-13.44		--	--
Total	--	-14.5	--	-7.73	--	--
Annual	-20.5	-15.6	-13.44	--	-12.55	+10.56

Notes: a. From Mallar and Thornton, "Transitional Aid for Released Prisoners," Table 5.
b. From Table 1.
c. From Table 2.
d. From Table 3, all variables in.
e. From Table 4, only significant variables in.

it is easily seen, is more than 60% lower than the value estimated originally by Mallar and Thornton.

From Table 3, the instrumental variable estimates of the simultaneous equation model of rearrest, there are two computations of the effect of financial aid. In column 5 the net reduction in arrests attributable to the experiment when all of the variables in the model are included is shown to be -12.55. In column 6, in contrast, rather than displaying a reduction in rearrests, there is shown a net increase in rearrests of 10.56 due to the experiment. This value comes about as a result of dropping the insignificant coefficients and reestimation of the simultaneous equation model detailed in Table 3. Because the direct effects of the treatment are eliminated in all of the equations except the hours worked equation, the work disincentive effect dominates, to create an estimated net increase in rearrests.

If one were to crudely average these alternative calculations of the effect of the experiment on rearrest, one would find that the actual reduction is more than one-half that reported by earlier analysts. Given that our estimates of the experimental effects range from a high of -15.6 to a low of +10.56 the evidence is clear that a mere respecification leads to significant reductions in the anticipated recidivism changes that can be attributed to the financial aid experiment.

CONCLUSION

The main point of this paper is that specification error led the evaluation of the Baltimore LIFE project grossly to overestimate the recidivism reduction arising from treatment effects. Relatedly, there are found to be problems in the measurement of the outcomes and the appropriate means of equation estimation. The rearrest measures are dichotomous for the year and are not adjusted for frequency, or even seriousness. The interaction of work decisions with crime decisions requires a simultaneous equation framework which requires special estimation techniques. But these problems, along with others I have not mentioned like selectivity bias, are standard ones in the evaluation of any program, project or experiment. What evidence is there, one might ask, if all of the standard evaluation problems had been solved in the original study, that one would have predicted the failure of the retest?

Here, I think, economic theory plays a role. I stated earlier that it was paradoxical that the unemployment insurance would probably work better to reduce recidivism among the very disadvantaged exoffenders rather than among their less disadvantaged peers. At least this is true within the very simple model I have sketched. If the Baltimore sample was more disadvantaged than the Georgia or Texas samples, it is no wonder that the financial aid experiment seemed to work initially.

If something is to be learned from this particular exercise in reevaluating an evaluation of an experiment that is now nearly doomed to join Martinson's troop of nonworking rehabilitation strategies, it is that labor market interventions can work. To make them work, though, the populations need to be appropriately targeted--here very disadvantaged workers--and countervailing effects like work disincentives need to be accounted for.

FOOTNOTES

[1]Robert Martinson, "What Works? Questions and Answers About Prison Reform," The Public Interest, No. 35 (Spring, 1974).

[2]See Charles Mallar and Craig Thornton, "Transitional Aid for Released Prisoners: Evidence from the LIFE Experiment," Journal of Human Resources, XIII, No. 2 (Spring, 1978).

[3]Ibid. The sample size was 413.

[4]Peter Rossi, Richard Berk, and Kenneth Lenihan, Money, Work and Crime (Academic Press, 1980).

[5]Kenneth Lenihan, When Money Counts: An Experimental Study of Providing Financial Aid and Job Placement Services to Released Prisoners (Washington: Bureau of Social Science Research, 1976).

[6]Also, when we introduce unemployment insurance, the effect of unemployment on crime is no longer unambiguous. At least in the context of this simple model, as benefits grow relative to the wage, if employed, crime may fall as people opt for unemployment rather than work or crime.

[7]This could be the single-period analog of the multi-period phenomenon by which the duration of unemployment is a function of the "cost" of further search. Unemployment insurance, of course, reduces this cost and thus leads to longer job search.

[8]Specifically, we differentiate E(w) with respect to I to obtain:

$$\frac{\partial\, E(w)}{\partial\, I} = -u'\,\overline{w} + u + u'I,$$

which is of ambiguous sign.

[9]From fn. 8, we find that

$$\frac{\partial E(w)}{\partial I} \gtrless 0 \text{ as } \frac{u + u'I}{u'} \gtrless \bar{w}.$$

Clearly the larger $\bar{w}$ or the smaller u, the less likely it will be that $\partial E(w)/\partial I > 0$, the necessary condition for unemployment insurance to reduce participation in crime.

[10]A classic of sorts is Isaac Ehrlich's, "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation," Journal of Political Economy, 81 (1973). The first attempt to estimate the offense supply function from individual data was made by Ann Witte, "Estimating the Economic Model of Crime with Individual Data," Quarterly Journal of Economics, XCIV, No. 1 (February, 1980).

[11]Mallar and Thornton, "Transitional Aid for Released Prisoners." Specifically, the variables they include are age, $(\text{age})^2$, education, race, and dummy variables for control and experimental groups.

[12]I am grateful to Kenneth Lenihan and Louis Geneive for making the Baltimore LIFE tape available to me.

[13]Specifically, experience = $x \cdot \exp(-.0042 \cdot z)$ where x is the length in months of the longest job held, and z is the length in months since that job. The rate of discount, .0042, is approximately 5% per year.

[14]So, one would prefer to have not only the month of rearrest but also the numbers of rearrests each month. In this discussion it is apparent that I am equating rearrests with crimes committed. This certainly is not a valid comparison. Some individuals commit many crimes for a given arrest,

and some get rearrested when in fact they are guilty of nothing. Aside from the fact that we have no other measure of participation in crime--official or self-reported--rearrest seems to capture something called failure, making it an applicable measure of performance.

[15]The reader can verify that the system is exactly identified. There are four equations, four unknowns, three exogenous variables excluded for each equation and not in each other equation, and two endogenous variables in each equation. The method of estimation is instrumental variables.

The choice of a linear model was based on the desire for tractability in the computations below. However, a reduced form estimate of the logistic rearrest equation was derived and is available from the author.

[16]Along, of course, with the effects of the treatment on days in jail and directly on rearrest.

[17]Note that

$$\alpha_1 = -0.057 \qquad \gamma_1 = -0.005$$
$$\alpha_2 = 6.200 \qquad \gamma_2 = 3.680$$
$$\alpha_3 = -1.790 \qquad \gamma_3 = -2.890$$
$$\alpha_4 = 0.007 \qquad \gamma_4 = 1.110$$

so
$$\frac{\partial\ t^*}{\partial\ I} = \left[\frac{1}{1-(-.005)(3.68)(-2.89)(1.11)}\right] \times$$
$$\left[(-.057) + (6.20)(-.005) + (-1.79)(3.68)(-.005) + (.007)(-.005)(3.68)(-2.89)\right] = -.0581.$$

[18] Dropping the zero coefficient yields, from the even-numbered columns of Table 3:

$$\frac{\partial\ t^*}{\partial\ I} = \left[\frac{1}{1-(-.006)(3.79)(-4.22)(1.35)}\right] \quad x$$

$$(-1.87)(-.006)(3.79)$$

$$= .0489$$

[19] Alternatives would be to evaluate the derivatives at the mean of the independent variables for the control group, or at the mean of the predicted probabilities.

[20] Mallar and Thornton obtain this value from probit estimates for the annual equation. I have estimated another specification for the annual equation omitting the wage variable and obtained a value of -19.8 for the change in rearrests.

[21] This is not so surprising, because of the many months in which there are positive estimated coefficients for the effect of the treatment on rearrest. But a complaint emerges that many of these monthly values are insignificant even at the weak 10% level. Recomputation, dropping the insignificant coefficient, leads to an estimated reduction in rearrest of 7.3.