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Jean Bottcher and Michael E. Ezell

Journal of Research in Crime and Delinquency 2005; 42; 309
DOI: 10.1177/0022427804271918

EXAMINING THE EFFECTIVENESS OF BOOT CAMPS: A RANDOMIZED EXPERIMENT WITH A LONG-TERM FOLLOW UP

JEAN BOTTCHER
MICHAEL E. EZELL

The boot camp model became a correctional panacea for juvenile offenders during the early 1990s, promising the best of both worlds—less recidivism and lower operating costs. Although there have been numerous studies of boot camp programs since that time, most have relied on nonrandomized comparison groups. The California Youth Authority's (CYA's) experimental study of its juvenile boot camp and intensive parole program (called LEAD)—versus standard custody and parole—was an important exception, but its legislatively mandated in-house evaluation was prepared before complete outcome data were available. The present study capitalizes on full and relatively long-term follow-up arrest data for the LEAD evaluation provided by the California Department of Justice in August 2002. Using both survival models and negative binomial regression models, the results indicate that there were no significant differences between groups in terms of time to first arrest or average arrest frequency.

Keywords: *correctional boot camps; correctional program evaluation; experimental data*

Despite tragic, highly publicized consequences (Clines, 1999; Selcraig, 2000) and disappointing evaluative research results (MacKenzie et al., 2001), correctional boot camps are still supported in some areas of the country (Buckley, 2000; Walker, 2002). Documented instances of extreme abuse have led to the closure of some camps, for example, in Arizona, Georgia, and

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An earlier version of this article was presented at the annual meeting of the Western Society of Criminology in Vancouver, BC. The authors are listed in alphabetical order; they contributed equally to the preparation of this manuscript. They thank Clayton Hartjen and the anonymous reviewers for their helpful comments, and Lee Britton, Norman Coontz, and Rudy Haapanen for assistance in accessing the arrest data. Opinions in this article are those of the authors and not necessarily those of anyone else. Direct correspondence to Jean Bottcher, Social Sciences Division, Western Oregon University, Monmouth, OR 97361; bottcherj@wou.edu.

JOURNAL OF RESEARCH IN CRIME AND DELINQUENCY, Vol. 42 No. 3, August 2005 309-332
DOI: 10.1177/0022427804271918

Maryland (Schnurer and Lyons, 2000), yet camps in other states, such as Florida, Illinois, Oregon (personal survey, May 12-14, 2003), and Pennsylvania (Kempinen and Kurlychek, 2003) remain active. Most of the evaluative research is flawed by poor comparative data or program-implementation problems. For example, MacKenzie (2000) located only four evaluations based on experimentally derived comparison groups. Three of these evaluated camps (Peters, 1996a, 1996b; Thomas and Peters, 1996) apparently experienced relatively serious problems with staff turnover and an unhealthy balance between military discipline and treatment (Bourque et al., 1996). The fourth, a legislatively mandated study of a California juvenile boot camp, came due before complete outcome data were available (California Youth Authority [CYA], 1997).

The present study capitalizes on a full and relatively long-term set of arrest outcome data for that fourth experimental evaluation. Designed as an alternative placement for the CYA's least serious male offenders, the program (called LEAD for expected participant outcomes—leadership, esteem, ability, and discipline) was typical of other juvenile boot camps around the country in targeting cost savings and lower rates of recidivism as major goals and in incorporating treatment components. Although the military model was politically initiated (by Governor Pete Wilson's administration), LEAD's enabling legislation was crafted by CYA administrative staff based also on their sense of "good program elements" (Gary Maurer, personal communication, February 18, 2003).[1] The four-month institution phase opened in September 1992 and the six-month aftercare phase with the first release of graduates in January 1993; LEAD was quietly phased out during the summer of 1997.

Despite an increasing focus upon tight security and a concomitant declining focus upon correctional treatment, the CYA developed LEAD with interest, even enthusiasm. Enriched line staffing was an important element of design. Eligibility criteria included a nonserious, nonviolent juvenile court commitment; an age of at least 16 (later modified to 14); a history or risk of substance abuse; informed consent; medical clearance; and Youthful Offender Parole Board (YOPB) approval.[2] Additional criteria (established jointly by the CYA and YOPB) included ineligibility for special mental-health programs, lack of recent violent behavior, and citizenship or legal presence in the United States.[3] The selection process isolated an eligible population representing about 14 percent of the entire male juvenile court intake pool.

CYA management planned and administered the program. In response to potential problems of ward abuse, a California National Guard (CNG) consultant suggested using an officer training (or leadership) model with a

critical focus on mentoring. The two camp sites were run primarily by 12 youth counselors (rather than the standard 7), newly titled TAC officers. TAC stands for teach, advise, and counsel—key elements of the officer mentoring role. The envisioned program was not notably theoretical. Implicitly, it seemed based on assumptions that program diversity, along with a little individualized treatment, would reach more wards; that a military environment would rub off as self-discipline; and that newly developed skills and positive attitudes would "produce" less criminal behavior. An exception was the TAC mentoring role, which was explicitly and theoretically related to the manner by which LEAD might reduce recidivism. Such an effect could occur through the "referent power" of the TACs, the possibility that cadets would identify with TACs and emulate their good qualities. The program and its experimental evaluation were implemented as specified by the enabling legislation (Bottcher and Isorena, 1994; Bottcher et al., 1995; Isorena and Lara, 1995). Thus, this study with its experimental design and long-term follow-up likely represents one of the most rigorous evaluations of a correctional boot-camp program in the United States.

This article begins with a review of the literature on boot camps in corrections and a summary of the CYA's (1997) in-house evaluation findings on LEAD. It proceeds with a section on the data and methods and the results of this analysis. A concluding discussion places this study's findings in the context of contemporary corrections.

LITERATURE REVIEW

Correctional use of quasi-military regimentation may be traced to the "perfected" American prisons of the 1820s and 1830s (Rothman, 1995), as well as to the earliest American reform schools for juveniles (Schlossman, 1995). As described by Rothman (1995), these early prisons were designed for reform and organized toward that end around silence, discipline, and hard work. A military model fleshed out the disciplinary milieu—routines to the sound of bells, marching in lockstep, uniforms for guards and inmates, requisite deference by prisoner to guard, even the symmetry and regularity of architectural design.

The contemporaneous inventions of prison and factory and their marked similarities prompted the notion that the former was designed to support the latter. Prisons, some historians suggested, were developed to support the nascent industrial order. Rothman (1990) prefers a different interpretation—the resemblance of prisons and factories was a product of the same historically specific assumptions about how people should be controlled. Invented

at a time of enormous national growth and unregulated social change, prisons, based on Rothman's historical analyses, were envisioned as models of order in exciting but uncertain, even frightening times.

The contemporary correctional boot camp is a relatively new (but declining) phenomenon often analyzed in conjunction with other recent innovative sanctions (like drug courts and day reporting centers) called "intermediate sanctions" (Petersilia, 1998). Tonry (1998) attributes the development of intermediate sanctions to political and ideological trends regarding crime control since 1980—a declining belief in rehabilitation, an increasing commitment to the "just deserts" rationale, and a receptivity to harsher penalties. Precursors of the boot camps appeared earlier, though, as the tumultuous social changes of the 1960s and beyond were beginning to play out. Austin, Jones, and Bolyard (1993) and Flowers, Carr, and Ruback (1991) trace the concept to "shock probation" (brief incarceration that first appeared in the 1960s) and somewhat later "scared straight" programs. Over time, increasing reliance on deterrence and harsher sanctions helped produce an enormous increase in prison populations during the 1980s. New penalties were developed that toughened probation or justified less incarceration. Boot camps formed a popular but relatively less common version of these intermediate sanctions.

At one level, then, contemporary correctional boot camps appear a useful midrange sanction for selected offenders judged ready for just that amount of cost-effective deterrence or reform. Initially, though, the combination of their deliberate harshness and rigid format, popular appeal and bipartisan political support, brevity, and thin reformative veneer suggest another level of interpretation—a search for order amidst turbulent social change and an unmapped future, conditions comparable to Rothman's (1995) perceptions of pre–Civil War America. With more historical distance, we may come to see the unusual correctional boot-camp movement explained in ways comparable to Rothman's explanation of our earliest prisons.

Despite popular support, correctional boot camps elicited criticism from the beginning (Sechrest, 1989) and they remain controversial (Lutze and Brody, 1999). The primary argument surrounds the appropriateness of harsh confrontational tactics in corrections (MacKenzie et al., 2001). A vast literature (Andrews et al., 1990; Cowles, Castellano, and Gransky, 1995; Gendreau and Goggin, 2000; MacKenzie, 2000), as well as professional expertise (see, for example, Chamberlain, 1998), suggests that effective correctional treatment includes state-of-the art theoretical grounding, qualified treatment providers, prosocial modeling and reinforcement, consistent discipline, individualization, and interpersonally warm, supportive staff. Fear

tactics and verbal confrontation find no support in the literature on correctional treatment. In addition, critics contend that camps incorporate conflicting goals, may expand the correctional population (net-widening), pave the way for inmate abuse, and promote sexist attitudes (Dieterich, Boyles, and Colling, 1999; Morash and Rucker, 1990; Parent, Snyder, and Blaisdell, 1999).

Although the evaluative literature suggests that some boot camps provide more positive environments than some contemporary correctional institutions (Lutze, 1998; MacKenzie and Souryal, 1995), many of the early critics' worst fears have been realized. Criminal charges and lawsuits based on physical brutality in juvenile correctional boot camps have arisen in at least eight states in recent years; and at least six deaths have been attributed to boot-camp negligence and abuse (Schnurer and Lyons, 2000; Selcraig, 2000).

Based on an extensive search of the literature and a subsequent meta-analysis of 29 evaluations that included 44 samples with a "reasonable" comparison group and postprogram measure of recidivism, MacKenzie et al. (2001) found no overall differences in recidivism between boot camp and comparison groups. A close examination of the 9 comparison samples (from 5 studies) that yielded a statistically significant difference in favor of the boot-camp group revealed that none were randomized experiments, and furthermore, that all were based on rough comparison groups (Flowers, Carr, and Ruback, 1991; Farrington et al., 2000; Jones, 1999; MacKenzie and Souryal, 1994; Marcus-Mendoza, 1995). The MacKenzie et al. (2001) meta-analysis did not attempt to incorporate any rating of the quality of the boot-camp programs (and data to accomplish that would be rough in any event). However, as noted above, three of the most rigorously evaluated programs were not model boot camps (Bourque et al., 1996). In contrast, evaluation of New York State's highly touted and elaborately refined Youth Leadership Academy (YLA; MacKenzie et al., 1997) was based on a retrospectively (albeit very carefully) generated comparison group of youths locked up in similarly secure facilities during the same time frame (early 1993 through early 1996) but still, for some reason, not selected for the program. According to the study authors, though, the director did not consider YLA an "efficient model" until 1996 and by then—based on his descriptions and video illustrations (Cornick, 1996; Office of Juvenile Justice and Delinquency Prevention [OJJDP], 1996)—YLA had become a largely demilitarized, treatment-oriented (and ultimately unevaluated) boot camp. Granted its limitations, then, evaluative research to date provides no methodologically rigorous support for the contention that boot camps lower recidivism. Although beyond the scope of this study, there is evidence that boot camps may lower costs if

designed with that purpose in mind (MacKenzie and Piquero, 1994; Parent et al., 1999).

LEAD IN-HOUSE EVALUATION[4]

Legislation called for initial process evaluations at each camp site and an impact study with a rigorous experimental design. Random selection procedures (described in the following Data and Methods section of this study) were designed around the required intake process for LEAD. In addition to observation and interview data, evaluators located or developed other measures of program delivery and performance, including documentation of aftercare services and parolee performance via monthly phone calls with parole agents.

The California Department of Justice (CDOJ) provided the primary source of outcome data, statewide arrests by law enforcement agencies. Because parole agents can arrest and detain parolees in the same manner as law enforcement officers (and potentially with the same effect), the CYA evaluators' monthly parole agent follow-up contacts provided these additional outcome data. An arrest was defined as any charge (technical or legal, by law enforcement or parole agent) that resulted in a law-enforcement citation or in any custody. Sources of data were coded so that CDOJ-verified arrests could be distinguished from arrests based only on CYA parole-agent contacts. Overall program attrition rates were 25 percent at the first site and 31 percent at the second, but all LEAD dropouts were retained in the CYA evaluation, as well as the present study.

Boot-camp sites generated lively, lengthy daily schedules of physical training, military drill and ceremony exercises, school classes, group counseling sessions, substance abuse treatment groups, and various unit maintenance routines. The program developed creatively, with varied additional elements by site, such as a bereavement-therapy group at one site in response to the many cadets who had experienced tragic losses. Both sites demonstrated similar positive characteristics, including a relatively safe and healthy environment. Comparative survey data, for example, indicated that LEAD wards, compared to control wards, felt less fear of being hurt by each other and more physically fit. LEAD wards were generally enthusiastic about the military milieu. Interview data revealed, for example, their clear awareness of the positive effects of leadership rotation, daily shifts in ward leadership roles that seemed to dampen gang conflicts considerably.[5] Wards most liked the physical training, 12-step drug treatment, and discipline of LEAD. On average, LEAD wards were incarcerated 4.6 months less than control wards. Evaluators noted that LEAD attracted many dedicated staff and provided a

location where some treatment efforts could be generated.[6] Monthly graduation ceremonies celebrated LEAD, a CYA showpiece in the mid-1990s.

Although the six-month intensive parole phase (as opposed to the standard two-month intensive parole) was initially envisioned around selected agents with caseloads of 15, the realities of ward-program recruitment forced a less auspicious design.[7] The parole phase was rather hastily developed after the first camp opened. Nonetheless, LEAD parolees, compared to control parolees, received more face-to-face contacts and more drug tests per month during their first six months on parole, clear indicators of a higher level of supervision.

Prominent among the limitations that seemed to plague LEAD were its lack of an underlying treatment philosophy that clearly explained *how* the program was expected to change its participants for the better, unresolved conflicts between cost savings and rehabilitation goals, and the need for more cohesiveness between institution and parole phases, as well as the need for continued development of the parole phase. Very few institution staff touted TAC mentoring as developed in the leadership training model, which failed to play a central role in the treatment orientation. Relatively high camp attrition rates based largely on disciplinary problems reflected staff judgments that many cadets were misplaced in an early release program. The military milieu required vigilance against abusive and demeaning confrontation tactics from line staff.[8]

When the final evaluation report was prepared (CYA, 1997), 12-month follow-up arrest and disposition data were available on only 90 percent of the LEAD group and 86 percent of the control group (because some wards had been released only a short time or were still incarcerated). Analyses showed that LEAD wards, compared to control wards, were more likely to be arrested for any offense (technical or law violation), but that neither group was more likely to be arrested for a CDOJ-verified law violation, to be arrested with a weapon, to cause injury during an arrest event, or to be arrested more times in 12 months. An analysis of disposition data revealed that LEAD wards, compared to control wards, were somewhat more likely to be returned to CYA custody following their first arrest. Furthermore, analyses by source of arrest data (CYA parole agent contact only or CDOJ verified) showed that the LEAD group, compared to the control group, received more arrests for law violations that were not verified by CDOJ rap sheets and were more likely to be arrested initially by a parole agent. The evaluation clearly indicated that on average, LEAD parolees, compared to control parolees, were more tightly supervised and subjected to more arrests (and more detention) by parole agents.[9] In sum, though, the final evaluation concluded that LEAD did not reduce recidivism.

DATA AND METHOD

This analysis relies on the experimentally derived comparison group data generated by CYA researchers for their impact evaluation of LEAD (CYA, 1997) and on a relatively long-term set of official follow-up arrest data provided by the CDOJ in August 2002. Thus, in contrast to the CYA final evaluation summarized above, the present study relies only on CDOJ arrest data (which does not provide full information on final dispositions or subsequent incarceration). In this section we describe randomization procedures, sources of data, variables, and data analysis plan.

Selection of Experimental and Control Group Members

Recall that program eligibility criteria were fairly stringent such that only 14 percent of the male juvenile court intake pool was found eligible for LEAD. Following the screening and YOPB approval process, CYA reception center staff called in the names of LEAD-approved wards to the research office during each monthly cycle. Groups of eligible wards were then stratified by ethnic categories (and parole-violator status, if possible) and selections were made using a table of random numbers. During the first year, monthly random selection procedures worked almost flawlessly except that reception center crowding sometimes forced periodic monthly selection groups, which were not always evenly numbered. Thus, the probability of being selected for LEAD was sometimes not .50. During the next year, after the second boot-camp site came online, each of the two reception centers was expected to generate enough wards to sustain one LEAD site (15 wards for each incoming platoon), as well as a control group, single-handedly. However, the reception centers were rarely able to come up with 30 eligible wards for a 50-50 split each month. Randomization was then always put off until the end of each monthly cycle and, if there were only enough wards to fill a standard platoon on a given month, random procedures were suspended and all eligible wards were sent to LEAD. However, nonrandomly placed LEAD wards were *never* included in the experimental study. Even when there were more than enough eligible wards to fill a platoon, though, the eligible wards rarely numbered 30 and the probability of being selected for LEAD was virtually never .50.

The final CYA evaluation study file was formed of all eligible wards randomly placed during the first two years of LEAD operation. Based on data presented in their final report (CYA, 1997), study group attrition following random assignment was impressively small. Overall, 10 (or 2 percent) of the randomly selected wards were lost, 9 from the experimental (or LEAD)

group (representing a 3 percent loss), leaving a total of 632 wards (348 in the LEAD group and 284 in the control group).

Source of Arrest Data

Follow-up arrest data were retrieved from the CDOJ. These data are known as the California Information and Identification (or CII rap sheet) information. When an individual is committed to the CYA, he or she is assigned a CII identification number and a computerized CII rap-sheet file is initiated and maintained by the CDOJ. When an adult is arrested in California, the arrest is reported by the arresting law-enforcement agency to the CDOJ. Thus, any time one of the wards in our samples was arrested as an adult, the arrest record, including the date of arrest and information on the arrest charges was forwarded to CDOJ. If wards are released by the CYA while still minors (under age 18), the CYA reports any subsequent criminal arrests to the CDOJ until they become adults.

The files of the CDOJ were searched in late August 2002. We permitted eight months of "lag time" for any arrests to be entered by CDOJ into the case's "rap-sheet" file. Thus, the arrests were "censored" as of December 31, 2001, and any arrests occurring between that date and August 2002 were not included in the analyses for this study. Postrelease follow-up (or exposure) periods for the sample averaged just over 7.5 years. The minimum follow-up time was just over 2 years and the maximum just over 9 years. Of the 632 wards in the CYA study file, 11 (4 LEAD and 7 control) cases could not be located through CDOJ. This left us with 621 (or 344 LEAD and 277 control) subjects for the analyses presented in this article.

From the arrest data, we extracted the three most serious criminal charges per arrest event using the procedural algorithm described in Ezell and Cohen (2005), who analyzed similar arrest data with three samples of CYA releasees. Briefly, the algorithm considers violent offenses the most serious charges, then serious property offenses (e.g., burglary, auto theft), followed by major drug offenses (e.g., sales and trafficking), and finally, the least serious miscellaneous charges (e.g., petty theft, drunk in public, trespassing). Allowing multiple arrest charges per arrest event is a more accurate way of cataloging an individual's arrest record than using a simple count of the number of times arrested (see Geerken, 1994). Due to limitations in the available data, the arrest charges did not include charges for probation violations, parole violations, or traffic offenses. These types of charges are not reliably reported to the CDOJ. Rather, the arrest charges variable only counts charges regarding the more "garden variety" street-crime offenses (e.g., robbery, theft, possession of drugs).

Data Analysis Plan and Variables

Our analyses of outcome data begin with a summary description of the breadth and quantity of arrest charges that occurred after release to parole. We then turn our attention to survival models that focus on a statistical comparison of the "arrest survival times" (i.e., time until first arrest) of boot camp and control wards. We begin the analysis of the arrest survival data with a simple graphical and statistical comparison of the survival curves of the two groups. We then move to more advanced Cox proportional hazards models that allow us to investigate whether the boot-camp participants had significantly different "hazards" (or "risk") of first arrest in comparison with control group members, while simultaneously controlling for the effects of other available variables of interest. The survival/hazard analyses rely on the use of the two variables required for such models: (1) a "survival time" dependent variable that measures the length of time (in days) an individual "survived" between date of parole release and either the date of first arrest or December 31, 2001; and (2) a "censoring indicator" that points out whether an arrest occurred at that time or not (1 = arrested, 0 = not arrested). Thus, for individuals not eventually arrested for a criminal offense, the survival time equals the elapsed number of days between their parole date and December 31, 2001, and the censoring indicator equals zero.

Our final set of analyses examines whether, on average, boot-camp participants, compared to control wards, accumulated a different mean number of arrest charges during several periods of follow-up. These analyses employ the negative binomial regression model that accounts for the fact that the number of arrest charges is a nonnegative count variable (Land, McCall and Nagin, 1996). If we were to apply a standard OLS linear regression model that assumes a continuous, normally distributed dependent variable as opposed to the skewed count dependent variable in our data, it would produce biased, inefficient, and inconsistent estimates of the covariates included in the model specification, as well as possibly predict a negative number of events (King, 1988; Long, 1997). For these reasons, we have chosen to use the negative binomial model based on a probability distribution that explicitly takes into account the discrete nature of count variables.

Data on subject characteristics, such as ethnicity and initial CYA commitment offense, were collected from various computer files within the CYA and the CYA's Offender Based Institutional Tracking System (OBITS). We use these subject characteristics as independent variables in the Cox proportional hazards models and negative binomial models presented below.

TABLE 1: Characteristics by Experimental Group

Characteristics	*Experimental Group*		
	LEAD (n = 344)	*Control (n = 277)*	*Chi-square/ t Test p Value*
Ethnicity (%)			
White	24.4	25.6	.903
Latino	43.9	42.2	
African American	24.1	25.6	
Other	7.6	6.5	
Prior local confinements			
Mean	1.6	1.7	.328
SD	1.4	1.4	
Age at initial CYA commitment			
Mean	17.1	17.0	.631
SD	0.9	0.9	
County of initial commitment (%)			
San Francisco Bay area	16.3	14.4	.922
Other Northern California	44.5	46.6	
Los Angeles	27.9	27.8	
Other Southern California	11.3	11.2	
Initial commitment offense (%)			
Drug; minor	5.2	8.7	.125
Property	72.7	66.3	
Person	22.1	25.0	
Age at program admission			
Mean	17.5	17.5	.697
SD	1.3	1.3	
Program admission status (%)			
First commitment	83.4	83.0	.895
Parole violator	16.6	17.0	

NOTE: All subjects in this study were males. The modal response was substituted for one subject with missing information on initial commitment offense and the mean number was substituted for 10 subjects with missing information on prior local confinements.

RESULTS

Bivariate Comparisons of Experimental and Control Groups

We begin the presentation of results (in Table 1) with a comparison of ward characteristics by group. Although probabilistically speaking, the randomization procedures should by themselves ensure comparability, these analyses are particularly important because the randomization procedures were not always 50/50. The chi-square statistic (from a two-way tabular

TABLE 2: Summary Arrest Charge Information by Experimental Group Status, Offense Type, and Length of Follow-up

	Offense Type							
	All-Offense Charges		*Serious-Offense Charges*[a]		*Violent-Offense Charges*		*Property-Offense Charges*	
Length of Follow-up	*LEAD*	*Control*	*LEAD*	*Control*	*LEAD*	*Control*	*LEAD*	*Control*
One year								
Mean number	0.97	1.04	0.54	0.60	0.32	0.31	0.30	0.26
% with any	43.6	50.18	30.23	37.18	19.77	19.86	18.6	18.77
Two years								
Mean number	1.63	2.06	0.85	1.09	0.55	0.69	0.46	0.51
% with any	59.59	68.59	44.77	53.07	29.65	35.02	26.45	29.96
Three years								
Mean number	2.59	3.02	1.31	1.52	0.79	1.04	0.72	0.69
% with any	75.29	77.26	55.81	60.65	39.24	45.85	34.30	36.82
All available data								
Mean number	6.68	7.25	3.17	3.18	2.15	2.48	1.61	1.49
% with any	91.57	91.70	82.27	80.87	68.31	71.12	54.65	53.07

NOTE: Analyses of total arrest events produced comparable findings. Mean numbers of arrest events for one year were 0.66 (LEAD) and 0.68 (Control), for two years, 1.10 (LEAD) and 1.33 (Control), for three years, 1.71 (LEAD) and 2.00 (Control), and for all years, 4.28 (LEAD) and 4.59 (Control).

a. Serious offense charges included homicide, forcible rape, robbery, aggravated assault, kidnap/extortion, child molestation, sodomy/forced oral copulation, weapon discharge, burglary, auto theft, arson, drug sales/trafficking, and drug possession/possession for sale.

analysis) was used for the categorical variables, and the *t*-statistic (from an independent samples *t* test) was employed for the continuous variables.

These analyses indicate that the two groups were composed of comparable youths. For example, upon admission for the current stay, roughly 83 percent of each group were "first commitments" (that is, this was the first time they had been committed to the CYA) and roughly 17 percent were parole violators. The bulk of each group (about 73 percent of the LEAD group and 66 percent of the control group) were initially committed for property offenses and, at program admission, wards in each group averaged 17.5 years of age. None of the variations in subject characteristics was statistically significant.

Descriptive Comparisons of Arrest Charge Outcomes

Table 2 presents arrest-charge outcomes for the two groups at various follow-up interval lengths and disaggregated by different offense-type categories. We include results here for four different follow-up time periods and for four

different offense categorizations, but focus our discussion and subsequent statistical analyses (using the negative binomial regression model) on the total number of arrest charges. It is important to keep in mind that because we did not have access to incarceration data in the postrelease period, we do not have a measure of "street time" for the members of each group. Thus, if any differences in this description (or in the analyses that follow) are due to differences in amount of street time, we will not be able to verify this. Recall, however, that on average, the LEAD group was released to a longer period of intensive parole than the control group, and according to the CYA's final evaluation, LEAD members were significantly more likely to be taken off the street for parole technicalities than control wards (CYA, 1997).

In the first year after release, 44 percent of the boot-camp wards and 50 percent of the control wards were arrested for new criminal offenses. The average ward in each group accumulated about 1 new arrest charge during that year. Looking at serious offenses only, we find that 30 percent of the LEAD group had been arrested for at least 1 serious charge, whereas 37 percent of the control group had been so arrested. The LEAD group averaged 0.54 serious arrest charges, whereas the control group averaged 0.60. Based on the accumulated data on total offenses and serious offenses for the two- and three-year periods, the small differences between the two groups seem to widen a bit. For example, after two years, about 60 percent of the LEAD group, compared to about 69 percent of the control group, had been arrested for at least one criminal offense. However, using all of the follow-up data, we find a considerable degree of similarity between the two groups. About 92 percent of each group had been arrested at least once, and roughly 80 percent of each group had been arrested at least once for a serious offense.

Analyses of Time to First Arrest

We now turn our focus to the lengths of time that on average, wards from each of the two groups managed to "survive" without being arrested. Figure 1 presents the survival curves for time to first criminal arrest. The survival curves represent the fraction of each group still arrest free at given time points (represented by days in the figure). As seen in the curves of both groups, the survival curves drop quite steadily during the first one thousand days after release. For example, at the 200th day, 30 percent of the boot camp wards and 33 percent of the control group wards had already been arrested for a new criminal offense. At the end of the first year, only 50 percent of the boot camp wards and 46 percent of the control wards remained free from a new criminal offense arrest. At the end of this study (using all available data), the estimated survivor function indicates that just 8 percent of each group survived arrest free. A graph of the survival curves for time to first serious criminal arrest

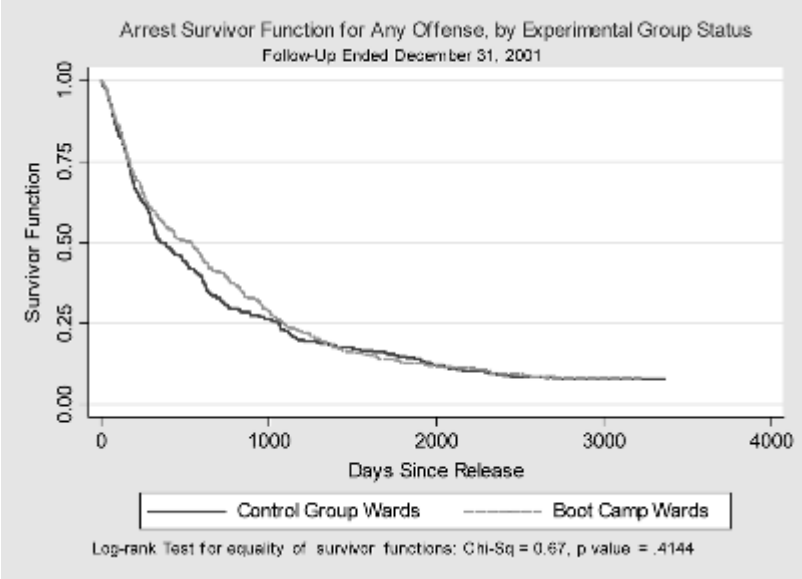

Figure 1: Survivor Function for Any Offense

generated survival curves for the groups that were substantively comparable to those presented in Figure 1, except that the curves dropped somewhat more slowly and ended at 0.18 (Control) and 0.19 (LEAD). (This graph is available upon request.)

Our next set of analyses (using Cox proportional hazards models) examines the possible effect of LEAD on the time until a first arrest occurs. Table 3 contains the results: Model 1 with just the boot-camp variable and model 2 with the boot-camp variable and other available independent variables. The hazard ratios (which are exponentiated parameter estimates) substantively indicate how the hazard rates (or instantaneous rate of event occurrence) either varies between two groups (categorical variables) or changes with increasing values of a variable (continuous variables; Allison, 1995). By the nature of the Cox model specification, the hazard ratios are calculated independent of time and assumed to be proportional over the entire follow-up period.

Model 1 indicates that the LEAD wards had hazard rates that were 7 percent [(1 – 0.933)*100] lower than control wards. However, this estimate was not statistically significant (z value = –0.81; p value = .418). Model 2 estimates the LEAD effect while holding constant the effects of other variables

TABLE 3: Estimates from Cox Proportional Hazards Model: Time to First Criminal Arrest

	Model 1			*Model 2*		
Variable	*Hazard Ratio*	*Robust* SE	p Value	*Hazard* Ratio	*Robust* SE	p *Value*
LEAD	0.933	.079	.418	1.010	0.089	.909
Ethnicity						
White				0.934	0.105	.544
African American				1.272	0.143	.033
Other				0.701	0.119	.036
Prior local confinements				1.107	0.029	.000
County of initial commitment						
Los Angeles				1.133	0.126	.262
Initial commitment offense						
Drug; minor				1.382	0.272	.101
Property				0.983	0.111	.883
Admission status						
Parole violator				0.779	0.113	.085
Age at release				1.092	0.039	.013

NOTE: The modal response was substituted for one subject with missing information on initial commitment offense and the mean number was substituted for 10 subjects with missing information on prior local confinements.

(which is important in this study because perfect 50/50 randomization was not always possible). Examining these results, we still find that the hazard rates of the boot camp and control groups were not significantly different from one another (z value = 0.11; p value = .909).

Hazard ratios for the remaining variables in model 2 indicate that several predicted time to first arrest. Although white wards had hazard rates that were not significantly different from those of Latino wards (the reference group), African American wards had rates that were significantly higher (by 27 percent) and wards in the "Other" ethnic group had rates that were significantly lower (by 30 percent). Wards from Los Angeles County, an area previously associated with more subsequent ward arrests than other areas (CYA, 1997), had hazard rates that were no different from the wards committed from other California counties combined (the reference group). Compared to wards committed for person offenses (the reference group), wards committed for drug or minor offenses had elevated hazard rates that were marginally significant (p value = .101). Because only 5 percent of the sample were committed for drug or minor offenses, statistical power may be responsible for the lack of significance (given the size of the estimated effect). Wards admitted for parole violations had hazard rates that were lower (based only on marginal significance) compared to those committed to the CYA for the first time (the reference group). Surprisingly, older age at release was significantly related

to shorter time to first arrest, with each additional year increasing the hazard rate by about 9 percent. Although we cannot interpret this finding definitively, it is likely that this variable is merely picking up unobserved heterogeneity in the criminal propensity of these wards such that those who were older at release (usually because of behavioral problems that delayed their release) were likely to reoffend faster. Finally, and typically, the number of prior local confinements (the best available measure of prior record) was significantly related to higher hazard rates, with each additional local confinement leading to roughly an 11 percent increase in the hazard of a first criminal offense arrest.

Analyses of Counts of Arrest Charges

The final statistical analyses examine differences in average numbers of all follow-up arrest charges between the two groups during four time periods. Recall that these data were presented descriptively in Table 2. Table 4 presents the results of eight negative binomial models: four with just the experimental group variable and four full-specification models.

The results of model 1 reveal no significant difference in average arrest charges between LEAD and control-group wards during the first year of release. The parameter estimate indicates that boot-camp wards, compared to control group wards, had a 7 percent reduction in the expected number of arrest charges [i.e., (100*((exp(–.071))– 1) = –.07], but the associated *p* value (.543) indicates that this difference was not statistically significant. Adding the other available independent variables into the model specification only confirms the findings from model 1. Substantively, the parameter estimate in model 2 indicates only about a 2 percent difference in expected arrest counts by group and, again, this estimate is not significantly different from zero (*p* value = .831).

The findings for the analyses of the cumulated two-year arrest charges were slightly different. The parameter estimate in model 3 indicates that LEAD wards had an expected arrest-charge count that was about 21 percent less than the control wards, and the associated *p* value (.021) shows that this difference was statistically significant. In the full model (model 4), the difference still seems notable in size (about 15 percent) but it is only marginally significant (*p* value = .094). Note, too, that this difference seems visually confirmed in Figure 1. Because there is no reason to expect such a delayed but positive effect on subsequent criminal activity from the boot camp experience, these (albeit modest) findings are virtually impossible to interpret definitively. Recall, however, that LEAD graduates (who comprised about 72 percent of the boot-camp group) were referred to a lengthier period of intensive parole and were subjected to more arrests (using a more encompassing

TABLE 4: Estimates from Negative Binomial Model: Number of Criminal Arrest Charges Within the First Year, the First Two Years, the First Three Years, and All Available Years of Release

	Arrest Charges in First Year						*Arrest Charges in First Two Years*					
	Model 1			*Model 2*			*Model 3*			*Model 4*		
Variable	Coefficient	*Robust* SE	p *Value*	*Coefficient*	Robust SE	p *Value*	*Coefficient*	Robust SE	p *Value*	*Coefficient*	*Robust* SE	p *Value*
LEAD	–0.071	.118	.543	–0.024	.113	.831	–0.234	.101	.021	–0.159	.095	.094
Ethnicity												
White				0.101	.148	.496				0.081	.119	.496
African American				0.425	.140	.002				0.329	.118	.005
Other				–0.254	.267	.342				–0.219	.257	.395
Prior local confinements				0.090	.036	.013				0.050	.034	.143
County of initial commitment												
Los Angeles				0.026	.123	.833				–0.120	.106	.258
Initial commitment offense												
Drug; minor				0.948	.211	.000				0.662	.167	.000
Property				0.429	.151	.004				0.280	.122	.022
Admission status												
Parole violator				–0.057	.181	.753				–0.109	.152	.473
Age at release				0.214	.045	.000				0.249	.041	.000
Constant	0.039	.080	.625	–4.671	.876	.000	0.723	.073	.000	–4.379	.760	.000

(continued)

TABLE 4 (continued)

	Arrest Charges in First Three Years						*Arrest Charges in All Available Years*					
	Model 5			*Model 6*			*Model 7*			*Model 8*		
Variable	Coefficient	*Robust* SE	p *Value*	*Coefficient*	Robust SE	p *Value*	*Coefficient*	*Robust* SE	p *Value*	*Coefficient*	Robust SE	p *Value*
LEAD	–0.153	.088	.084	–0.081	.083	.332	–0.082	.069	.239	–0.070	.069	.309
Ethnicity												
White				0.206	.106	.052				0.048	.085	.570
African American				0.265	.103	.010				0.109	.086	.204
Other				–0.318	.209	.130				–0.460	.160	.004
Prior local confinements				0.057	.028	.040				0.066	.023	.005
County of initial commitment												
Los Angeles				–0.033	.094	.728				–0.092	.075	.222
Initial commitment offense												
Drug; minor				0.587	.144	.000				0.403	.124	.001
Property				0.246	.107	.021				0.283	.081	.000
Admission status												
Parole violator				–0.034	.124	.783				0.131	.100	.190
Age at release				0.229	.036	.000				–0.017	.028	.546
Constant	1.105	.064	.000	–3.642	.657	.000	1.980	.053	.000	1.930	.519	.000

NOTE: The modal response was substituted for one subject with missing information on initial commitment offense and the mean number was substituted for 10 subjects with missing information on prior local confinements. The same analyses as those presented in this table using only serious arrest charges (defined in the note on Table 2) and using number of arrests as dependent variables produced substantively similar findings. These additional analyses are available upon request.

definition than the present study, which included technical violations) and to more detention by parole agents than other study wards (CYA, 1997). Thus, the most likely possibility is that those higher rates of custody among boot-camp wards, compared to control wards, slightly dampened the cumulative arrest charge totals during the first couple of years following release.

Expanding the length of follow-up to three years of data, models 5 and 6 of Table 4 present the results of the bivariate and multivariate specifications of the negative binomial regression model. In model 5, the parameter estimate for the boot-camp variable is marginally significant (with a *p* value of .084) and substantively indicates that the boot-camp wards had expected arrest charges that were about 14 percent lower than the control-group wards. However, after we control for the effects of the other variables, the boot-camp coefficient is no longer significant. Considering all available data (in models 7 and 8), differences between the experimental groups are not statistically significant. Furthermore, in the full specification model 8, age at release is no longer a significant predictor and, except for the relatively small "Other" category, ethnicity is no longer a significant variable. Prior record (measured by prior local county confinements) still remains a predictor of arrest charge counts, and wards committed for drug and minor offenses and for property offenses still had expected arrest event counts that were significantly greater than wards committed for person offenses.

The substantive conclusions of the models discussed above were replicated when we used (1) the count number of serious arrest charges and (2) the count number of total arrest events ("number of arrests") as the dependent variables.

DISCUSSION AND CONCLUSION

This study capitalized on a long-term set of outcome arrest data for a previously incomplete but rigorously designed experimental evaluation of a relatively well-developed and implemented juvenile boot camp and intensive aftercare program (called LEAD). In sum, it found no significant differences between boot camp and control youths in average time to first arrest or in average overall arrest charges during the first year, during the first three years, and during all available years following release to parole. An anomalous difference in the two-year follow-up period, which favored the LEAD group and held up with marginal significance controlling for available independent variables, cannot be explained but was likely due to the dampening effects of tighter parole supervision, including more time in custody, for LEAD wards versus control wards during the first year following release to parole. We conclude that the LEAD boot camp (which incorporated a shorter

period of incarceration that averaged 4.6 months) and its intensive aftercare program neither reduced crime nor placed the public at any greater risk of crime.

The bulk of the evidence from previous studies supports the conclusion that boot camps are ineffective as correctional treatment (MacKenzie et al., 2001). In contrast to most prior studies though, this study comprised three notably strong features—the experimental design, virtually complete long-term follow-up data, and a relatively impressive focal boot camp. It was also limited, though, in significant ways—most notably by the lack of consistent 50/50 randomization procedures, as well as the lack of subsequent incarceration data (for street-time information) and the reliance on only official arrest outcome data. Nonetheless, this study's strengths markedly increase our confidence in previous research findings.

Why did LEAD fail to reduce recidivism? Recall the elements of effective correctional treatment (discussed in the literature review above): theoretical grounding in state-of-the-art treatment modalities, trained treatment staff, prosocial role modeling and reinforcement, avoidance of confrontational tactics, consistent discipline, individualization, and interpersonally warm, supportive staff. Although many LEAD staff were good role models and clearly cared about their cadets, the program itself was not *specifically* designed to incorporate any of these important dimensions of effective treatment. In particular, LEAD was not theoretically grounded in the best contemporary treatment methods; and CYA youth counselors were not trained in state-of-the-art treatment techniques. Furthermore, the officer-mentoring model did not take hold in the program, confrontational tactics were commonly employed, and most program activities were focused on group performance.

Once noted for its progressive, experimental treatment programs, the CYA had become, by the 1990s, a politically driven, less professional, and increasingly punitive agency (Broder, 2004; Palmer and Petrosino, 2003). Faced with political pressure from the governor, the CYA administration was unable to sustain its initial decision not to pursue a boot-camp program. Furthermore, having largely abandoned its mission of rehabilitation, the agency did not have many professionally trained treatment staff to develop the program. Continuously refined in an ad hoc but often creative manner, LEAD's boot-camp phase was still fundamentally a militarized quick fix and its aftercare a hastily designed and unevenly implemented, albeit longer term and overall somewhat more diversified supervision service. As many staff repeatedly complained, as well, the two major goals of the program really did conflict. In short, although the boot-camp's regimentation, impressive array of daily activities, and enriched staffing generally improved the institution environment and its intensive aftercare clearly provided more surveillance,

LEAD did not focus much on individual needs or provide much by way of treatment services. Thus, in our opinion, the program was, at the outset, unlikely to reduce rates of recidivism among its participants.

NOTES

1. Before LEAD was established, national experts at a state-sponsored Boot Camp Forum in Sacramento advised against using the boot camp model. As his agency's representative at the Forum, Mr. Maurer brought this advice back to the California Youth Authority (CYA) Director who agreed not to pursue a camp program. Eventually, though, the governor determined that the CYA and the Department of Corrections would develop boot camps to reduce the spiraling costs of incarceration. Mr. Maurer also felt that the governor and his advisors believed in boot camps as treatment and found them politically appealing.

2. The Youthful Offender Parole Board (YOPB) is a separate state department composed of members appointed by the governor and charged with various decision-making responsibilities such as parole release.

3. The first two additional criteria reflected the YOPB's unwillingness to place violent or mentally unhealthy youths in an early release program and the third reflected the Wilson administration's crackdown on illegal immigrants (see, for example, Martínez, 2001).

4. This section is based on information presented in various CYA publications: Bottcher and Isorena (1994); Bottcher et al. (1995); Bottcher, Isorena and Belnas (1996); CYA (1997); and Isorena and Lara (1995).

5. Street-gang-affiliated wards said they put their allegiances on hold, so to speak, so that their platoons would respond favorably when they were in charge. Evaluators were surprised to observe the effectiveness of this technique for keeping gang conflicts in check. If leadership roles were not randomly rotated on a daily basis (as happened at the second site), however, the technique was not effective.

6. For example, in response to common problems on parole, a pilot aftercare project with group homes and work slots through the California Conservation Corps was developed and federally supported during the second year. This program was expanded and federally funded in the following two years, as well, and it appeared particularly promising. However, it was not developed soon enough to benefit wards in the experimental study group.

7. LEAD aftercare was defined by a "case count credit" of 3.5 for six months per LEAD parolee and by higher levels of service. The case count credit was the "equivalent" of a 15 to 1 caseload (that is, LEAD parolees were to receive the service intensity of a parole agent who had a caseload of only 15 parolees). Information from field parole agents indicated that the 15 to 1 parolee to agent ratio was unevenly implemented and that "sheer numbers" made the 3.5 credit mathematically correct but not practically meaningful in all cases (CYA, 1997).

8. The author who was the LEAD principal investigator during the first four years of the evaluation observed occasional instances of verbal abuse at both sites. Based on conversations with a consultant from the California National Guard (CNG), she was also aware of his concerns regarding the use of inappropriate confrontational tactics by some LEAD TAC officers. These concerns became a focal point for some CNG-training sessions. In response to an open-ended question regarding negative program features, 60 percent of the LEAD interviewees, compared to 40 percent of the control wards, mentioned unfair, vindictive or harsh staff (CYA, 1997).

9. Subsequent analyses by Ezell, Land and Cohen (2003) using the same data set and multivariate proportional hazards models indicated that LEAD wards, compared to control wards, had elevated hazards of a first arrest (p value = .056) but not of second or third arrests.

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Jean Bottcher is a retired California Youth Authority research specialist and an adjunct professor in the Department of Criminal Justice at Western Oregon University. She was principal investigator of the LEAD program from 1992 to 1996. Her research interests include gender and delinquency and justice-system handling of juveniles as adults.

Michael E. Ezell is an assistant professor in the Department of Sociology at Vanderbilt University with research interests in criminology, juvenile delinquency, and quantitative methods. His primary area of research focuses on the chronic offender population with an emphasis on their longitudinal offending patterns.