Law & Social Inquiry
Journal of the American Bar Foundation

Law & Social Inquiry
Volume 43, Issue 4, 1308–1339, Fall 2018

When Frontloading Backfires: Exploring the Impact of Outsourcing Correctional Interventions on Mechanisms of Social Control

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This study demonstrates the effects of frontloading rehabilitative services to parolees through third-party residential and community-based programs. Although outsourcing treatment responsibilities to contracted reentry facilities is an increasingly common feature of postrelease supervision, the role these facilities play in reentry management and recidivism outcomes remains largely unexplored. Here, several common recidivism outcomes for parolees who attended private treatment facilities upon release are compared to those of parolees who did not. We conducted Correctional Programs Checklist assessments on each treatment site to investigate whether recidivism outcomes vary by level of programmatic quality. Our findings indicate that parolees who receive frontloaded services are significantly less likely to be rearrested or reconvicted for new crimes within eighteen months of release. These findings held across levels of programmatic quality, with larger reductions observed for programs of higher quality, and align with broader emphases in community corrections on risk assessment and organizational demands for efficiency.

INTRODUCTION

Parole is an essential and increasingly prevalent component of the US correctional system. Accompanying this visibility has been a growing recognition of parole's broader influence on multiple aspects of the reentry process. Supervision alone during the transition from prison to the community has been found to be insufficient at reducing postrelease recidivism; treatment and support services are essential during the early portions of the reentry process (Taxman 2002; Petersilia 2003). With increasing frequency, transitional and treatment service obligations are being outsourced to nongovernmental residential facilities, often run by privately

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We would like to thank our colleagues Elizabeth Griffiths and Johnna Christian for helpful comments on early drafts of this article. All errors of omission and/or commission are our own.

held, for-profit companies (Feeley 2002). Major shifts in penal policy, many of which are accelerating in light of austere fiscal conditions, have directly contributed to the growth of the privately run postrelease treatment industry (Cohen 1985; Garland 2001; Simon 2007). The residential nature of many of these postrelease treatment facilities has effectively deepened formal social control mechanisms in community corrections and, as we present throughout this paper, has more strongly aligned US corrections with the theoretical framework of the *new penology*.

The community corrections literature has largely focused on explorations of how to reduce recidivism rates effectively among former prisoners by examining the causes and correlates of postrelease crime. These findings have been synthesized in a body of applied research commonly referred to as effective correctional interventions (Lowenkamp, Latessa, and Smith 2006). When considered in conjunction with the research on recidivism that has shown that postrelease failure occurs soon after release from prison, the value of delivering evidence-based rehabilitative services during the first months after release, a process known as frontloading, becomes clear (Petersilia 2003; Solomon, Kachnowski, and Bhati 2005). In light of the convergence of the literatures on recidivism and effective correctional interventions, as well as changes in the structure of authority within the community correctional and rehabilitation arenas, the current study assesses the impact of frontloading rehabilitative services through private residential treatment facilities upon recidivism.

We employ data that reflect the postrelease recidivism patterns of a cohort of parolees, all of whom were released from the custody of a state on the East Coast between 2009 and 2011. Propensity scoring and matching routines are used to pair parolees who were frontloaded rehabilitative services through treatment centers to otherwise similarly situated parolees who did not receive these residential services. We compare the general reoffending patterns among the cohorts, as well as isolate the particular patterns of recidivism associated with program sites that strongly adhere to, as well as fail to meet, the tenets of effective correctional interventions. These classifications are determined using the treatment program's score on a Correctional Programs Checklist (CPC) assessment. The results of the current study inform two key areas of inquiry relevant to the structure and nature of community-based supervision by underscoring the complex relationship between the frontloading of services and the quality of that programming. First, the overall impact of frontloading rehabilitative services on recidivism is estimated; and, second, the distinction within those effects that can be attributed to parolees' experiences with rehabilitative services of differing levels of quality is identified. Specifically, this study highlights the different outcomes for individuals participating in privately run programs that adhere to the principles of effective correctional interventions and those that do not.

We begin by discussing how the expansion of highly custodial, though still therapeutically oriented, supervision has deepened formal social control mechanisms within the US criminal justice system. We frame these changes through the critical lens of the *new penology*, and consider the resultant impact of increased social control on both officers and offenders. We then apply this perspective to the current correctional landscape, focusing on the development of evidence-based policy frameworks and the need for effective treatment programming for the growing

parolee population. We then reflect on the intersection of these policies and how they support the hypothesis that the frontloading of rehabilitative services should reduce the reoffending rates of parolees, as well as consider how these trends may vary when the service provision is managed through modes of varying levels of programmatic quality. After presenting these empirical and theoretical landscapes, we detail the data used for the study, our analytic strategies, and our results. We conclude by contextualizing the study's findings and discussing its implications for theory, policy, and future research.

LITERATURE REVIEW

Reentry, Supervision, and Transitions in Social Control

Prison populations, despite recent reductions in the overall number of inmates, remain large enough to challenge policy makers seeking to reduce the prevalence of incarceration (Glaze and Kaeble 2015). The benefits of shifting inmates into community supervision are readily apparent: fewer individuals reside in prisons, costs are reduced, and opportunities for community integration increase. This naturally results in an amplified reliance on parole supervision and partially accounts for growth in overall parole populations (Herberman and Bonczar 2014). Parolees are also rearrested frequently, both for new crimes and for violating the terms of their release (Solomon, Kachnowski, and Bhati 2005) and, unsurprisingly, parole failures make up a significant portion of the US penal population (Petersilia 2000; Solomon, Kachnowski, and Bhati 2005; Ostermann, Salerno, and Hyatt 2015). In response to increased demands on parole systems, agencies and officers may be subjected to significant pressures to transform how they supervise and treat offenders to meet the needs of the operating reality of the community corrections system.

When agencies cannot or choose not to deliver a full range of reentry assistance or therapeutic programming, they often turn to external service providers to supply these services (Demone and Gibelman 1990). Arguments in favor of this outsourcing have included cost effectiveness, net capacity, and efficiency (Cheliotis 2006). The transition of authority for reentry management from the state to community-based residential treatment facilities is not without cost. In addition to the fiscal costs of transitional programming, which have been well debated (Petersilia 2003; Aviram 2015), the usage of frontloaded residential programming effectively deepens formal and informal mechanisms of social control. As opposed to traditional parole supervision, where contact with authorities is limited and prescribed in scope, residential programs overtly extend institutional correctional apparatuses deeper into community settings (Cohen 1979, 1985; Garland 1990, 2001; Feeley 2002; Simon 2007).

The outsourcing of residential reentry services is directly in line with the conceptual framework of the new penology (Feeley and Simon 1992, 1994; Simon and Feeley 2003), which contends that the orientation of the correctional system directly manipulates the structures of formal control by overemphasizing formalized and actuarial procedures over the discourses of individualized rehabilitation. This

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

theoretical structure directly addresses the increasingly prevalent role of private interests in criminal justice, as well as the state of the resulting "crisis of legitimacy" within community corrections (Maruna, Dabney, and Topalli 2012). Policies based on ideological reform and those responsive to fiscal pressures often have inherent tensions, a challenge not unique to corrections (Gordon and Verdun-Jones 1986). Within the context of treatment services, however, these often competing interests simply increase the number and nature of parties with authority over former prisoners, resulting in more conduct rules and opportunities to run afoul of them. The competing pressures, between treatment and control, for example, strongly influence officer conduct and, in turn, offender behavior (Klockars 1972; Skeem and Manchak 2008).

Public and privately run community supervision agencies do not, by default, subject their staff to different goals. The role for officers, as conceptualized within the *new penology*, is in many ways similar to the traditional responsibilities attributed to community supervision officers: to develop the necessary relationships to facilitate rehabilitation and enforce social order (Burton, Latessa, and Barker 1992). Though this responsibility may create ideological tensions between control and treatment (Clear and Latessa 1993), the officer's role was offender-facing; that is, focused on responding to the needs and conduct of the client. This focus shifts, under the *new penology*, with the officer becoming beholden to a set of goals focused on systematic efficiency. In this new, agency-facing role, officers become attuned to issues of contingency management. Practically, this means that high-risk or high-needs cases may be perceived as problematic *a priori*. For those individuals, an increased level of social control, most apparently through high levels of surveillance and a decreased tolerance for noncompliant actions, results (Lynch 1998; Garland 2001). This ideological shift is facilitated through actuarial risk assessment and risk-stratified supervision. These attributes of supervision are necessary to ensure that aggregate levels of performance benchmarks, few of which relate to the relative rehabilitation of offenders on a caseload, can be met.

The shift to the risk-based supervision strategies of the *new penology* may be in both public and privately managed supervision agencies. However, the profit structure of the organizations operating residential treatment facilities operates under starkly different sets of goals and incentives than within the public sector. The effect of outsourcing treatment services on social control goes beyond the traditional net-widening critiques that are associated with community corrections (e.g., McMahon 1990). Setting aside concerns about the proliferation of beds in private residential treatment facilities and the resultant need to fill them driven by private ownership (see Lundahl et al. 2009; Ackerman, Sacks, and Furman 2014), the primary influence of subcontracting these responsibilities is in the nature, not the scope, of correctional social control. As Stanley Cohen (1985) notes, privatization overtly inserts market forces into the discourse surrounding correctional programming and may supplant the traditional aims and modes of social control.

The removal of governmental actors (and their unique system of public accountability) from the treatment-oriented aspects of reentry creates tensions within the basic theoretical justifications for correctional supervision. Under this framework, traditional goals and structures of penal management are replaced.

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

What remains is a system that focuses on actuarially derived assessments over clinical judgment, evaluations of impacts in the aggregate instead of for each individual, and the prioritization of risk management over treatment (Feeley and Simon 1994; Garland 1997). These changes set in motion a series of "loose[ly] coordinat[ed] . . . transitions to penal discourse, objectives and techniques, resulting in a reentry process where the prevention of recidivism is decoupled from social implications [and] treated as a pointer to systemic success in imposing long-term custodial control over chronically troublesome aggregates of offenders" (Cheliotis 2006, 316). As a result, the goal of reentry trends toward systematic efficiency and away from reducing individual-level recidivism.

The disjunction between privatized supervision and rehabilitative ideals manifests within the hierarchical structure of key organizational relationships, a component of social control, and in how successes are defined. In a standard treatment model, and as often replicated by agency-run rehabilitation programs, the appropriate direct service provider determines individualized needs by working in conjunction with the person receiving care. Under a privatized model, parolees receiving the services are no longer the primary customers of the criminal justice system; they are provided only those services that are offered through the providers and as determined by their classification. Individual-level reductions in recidivism or behavioral change, which are notoriously difficult to measure consistently, are replaced with market-based factors (such as price, location, capacity, and pseudo-outcomes like discharges and escapes), as the primary outcomes of interest.

While shifting the focus away from traditional clinical metrics, this strategy continues the progression toward a narrower focus on processes related exclusively to management. This may include an emphasis on administrative tasks, including data entry, paperwork, and file management, and away from face-to-face meetings and individualized rehabilitative planning (Lynch 1998). These two task sets result in starkly different data becoming available to officers when a revocation decision must be made. Additionally, changes in the structure of social control have the capacity to impact the character of key relationships, especially informal control exerted within community-based treatment. The therapeutic communities of halfway houses and postrelease residential treatment facilities, for example, are less able to transmit and maintain traditional norms and standards of behavior encouraged by the older, treatment-centric approaches to supervision (Sampson and Groves 1989).

Effective Correctional Interventions

Increased reliance on community supervision also creates a need for reentry programming that is both effective and efficient. It is important that the definition of effective programming includes both reductions in recidivism and the limiting of negative behaviors that may result in parole violations and returns to incarceration. As empirical efforts have shaped our collective understanding of which services and programmatic components are effective at reducing recidivism, policy makers and practitioners have faced mounting pressures to develop and integrate these strategies into postrelease treatment regimens.

Over the past two decades, scholars have explored the particular treatments and services that are offered to the reintegrating population in efforts to develop strategies to decrease postrelease recidivism. This focus on the identification—and replication—of effective interventions has risen to the forefront of correctional research. Findings in this area have demonstrated that there are several characteristics that are common to successful treatment regimens, including employing actuarial assessments, enhancing intrinsic motivations to spur behavioral change, engaging in prosocial and community support for offenders, using directed practice to reinforce new skills, and deploying empirical assessments (see Taxman 2002; Lowenkamp and Latessa 2004, 2005; Lowenkamp, Latessa, and Holsinger 2006; Taxman and Marlowe 2006). Additionally, research on the risk-needs framework underscores the need to target higher-risk offenders, address criminogenic needs, and match clients with both interventions and staff that have strengths in responding to their individual learning style (Andrews, Bonta, and Hogue 1990; Lowenkamp and Latessa 2005). The broad strategies that encompass the characteristics and traits of programs empirically proven as effective have been collectively labeled as the Principles of Effective Intervention (PEI) (Gendreau 1996; Taxman 2002; Lowenkamp, Latessa, and Smith 2006).

Because of this strong empirical base, correctional programs that successfully employ components of the PEI are referred to as evidence-based practices (EBPs). However, because of the necessarily broad nature of the PEI, the universe of EBPs encompasses several different types of interventional strategies (Hurtig and Lenart 2011; Taxman and Belenko 2012). This diversity makes interprogram comparisons, even those with similar aims and mechanisms, challenging. Metrics of this nature, however, are essential for practitioners seeking new EBPs to implement, as well as for evaluation research seeking to identify why a program may have failed (Gendreau, Goggin, and Smith 1999; Lowenkamp, Latessa, and Smith 2006; Lowenkamp et al. 2010b). EBPs foster an environment in which the key dimensions of supervision are those that focus on the quality, not exclusively the quantity, of the oversight. A comparison of the requirements of evidence-based programming and the characteristics of an intervention can serve as a meaningful proxy for program integrity, as areas in which a program as implemented and the intended services diverged are those that require reorganization.

The CPC was developed specifically to assess the extent to which correctional programs adhere to the PEI (Latessa 2013). It is an empirically based instrument that measures the capacity of the program to deliver effective services and whether the content of the program was developed within a framework of EBPs. Studies using the CPC and analogous assessment instruments (e.g., the Correctional Program Assessment Inventory [CPAI]) have found that higher scoring programs are associated with more substantial decreases in recidivism. For example, Lowenkamp, Latessa, and Holsinger (2006) demonstrated that residential community-based programs in Ohio that scored in the "low" category of the CPAI decreased returns to custody within two years of release from prison by 1.7 percent when compared to matched controls who were not transitioned through residential programs. Programs with "fair" scores decreased recidivism by 8.1 percent, and those with "high" scores decreased recidivism by 22 percent within the same follow-up period. A similar

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

study conducted in Ohio found that programs that scored "low" on the CPC increased reconvictions within two years of release from prison by approximately 3 percent, with "moderate" scoring programs recidivism rates decreasing by 6 percent. Those programs that scored "high" on the CPC were associated with an 18 percent decrease in reconvictions (Latessa, Lovins, and Smith 2010; Ostermann 2013).

Considerations of the potential impact for EBPs often overlook some areas of implementation that may significantly impact the fidelity of the individual program to the model intervention and/or the potential to realize the benefits measured in similar programs. The exercise of officer discretion is of particular importance to the current study. During the course of supervision, officers have a significant amount of flexibility in determining who they will enact revocation proceedings against when parolees run afoul of the rules and regulations of parole. Some of the commonly relied on factors are either offender- (i.e., prior criminal history), case- (e.g., offense seriousness), or supervision-specific (e.g., nature of the violation) factors and are largely uncontroversial (Steen et al. 2013; Grattet and Lin 2016). Others, however, are individualized and may be subjective, including mental health (Skeem et al. 2014) and substance abuse (Dowden and Brown 2002). The interplay of these factors in determining the result of situations when a revocation may take place is directly informed by the supervision regime in place, a categorization of cultural prioritization of deviance detection, and sanctioning that may characterize an agency (Grattet, Lin, and Petersilia 2011). As the foci of the *new penology* are integrated into those regimes, officers may be increasingly likely to employ revocations as tools to manage their caseloads—and particularly in challenging or difficult cases—regardless of the evidence base supporting the services being provided at the time a revocation decision takes place.

Within the EBP literature, the timing of treatment delivery is often overlooked in favor of a focus on substantive matters. Research has consistently demonstrated that the first months after release from prison are the most trying for parolees and, as a result, are a period of increased risk of rearrest (Grattet et al. 2009; Durose, Cooper, and Snyder 2014) and addiction relapse (Cropsey et al. 2012; Clark et al. 2014). A policy of frontloading encourages the delivery of high doses of rehabilitative services immediately after release, and a decreasing rate over time. Frontloading, as a component of an evidence-based crime reduction regimen, is also based on a solid empirical foundation (Petersilia 2003; Ostermann and Hyatt 2014). When combined with EBPs, frontloading allows parolees to receive effective services and support during a particularly vulnerable point in the reentry process. Accordingly, the timing of the services being delivered should be considered in conjunction with the substantive content of the programming and the organizational goals when assessing the potential impact of these services.

Frontloading services to parolees through a residential program encourages individuals to begin several parallel forms of intensive treatment when they are more likely to be effective: immediately upon release from full custody housing, but before the expiration of supervision authority (see, e.g., Petersilia 2007). As a common component of postrelease plans, residential treatment facilities focus on changing the behavioral patterns of parolees at a key period of reentry. Preventative treatments, as well as increased levels of supervision, should be delivered

contemporaneous to the periods of increased risk of failure (Petersilia 2003; Rosenfeld, Wallman, and Fornango 2005; Ostermann, Salerno, and Hyatt 2015). This temporal nexus is crucial for an evidence-based treatment model. The usage of an intensive residential program, especially when compared to the limited opportunities for treatment facilitated by standard community supervision (Latessa and Travis 1991), can increase the impact of the substantive content of the intervention, as well as provide the opportunity for delivery with greater fidelity to the PEI.

The Net Effects of Outsourcing Community-Based Rehabilitation and Supervision

The core body of research on EBPs in community corrections supports the redoubling of interventions that integrate the theoretical and practical traits common to effective programming. These types of programs, however, are often more resource intensive and require offenders to engage in their supervision and treatment protocols beyond what is expected under standard supervision. These efforts have the potential to tax supervision agencies, many of which operate at or near their capacity. This is especially significant in the period immediately after a parolee's release from prison. Concordantly, the nexus of residential programming and frontloaded services has reshaped the systems of social control that enforce behavioral norms and encourage rehabilitation among the parolee population.

In this environment, officers may be pressured to implement supervision policies in a manner that may undermine otherwise effective treatment programs. Shifting much of the responsibility of rehabilitation to nongovernmental actors, even if the effect on recidivism is positive or neutral, also changes the role of parole supervision as both a public responsibility and the primary component of formalized correctional control in the community. The public may be less likely to view treatment services, in this context, as legitimate, given governmental divestment of those responsibilities.

We examine this complex dynamic here by using the EBP movement as a framework. This inquiry explores the effect of frontloading parolees into privately run residential treatment facilities of variable quality. This, in turn, sets the stage for important queries regarding the broader influence of trends in how, and by whom, supervision is delivered. Of particular importance is the extent to which the expansion of social control ushered in by shifts in the public-private divide within corrections could have a ripple effect, increasing punitiveness and decreasing legitimacy, within the reentry process.

DATA AND METHODS

Participants

A state parole board (SPB) in a highly populated jurisdiction on the East Coast of the United States provided data for this study. The SPB supplied us with a

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

customized dataset that included individual-level demographic, instant offense, date of sentencing, prerelease actuarial risk score (Level of Service Inventory-Revised [LSI-R]), and county of conviction information for cohorts of parolees released from state-run prisons in 2009 ($n = 6{,}264$), 2010 ($n = 6{,}065$), and 2011 ($n = 5{,}627$). A total of 17,956 parolees were released from state institutions during this period and were eligible for inclusion in this study.

At the time of release, some parolees were transitioned through residential, community-based services. In these semicustodial facilities, parolees were front-loaded rehabilitative services by contracted, community-based programs run by third-party (i.e., private, nongovernmental) agencies. Services that are offered to parolees at each of these facilities are intensive, follow standardized curricula, and often include cognitive behavioral therapies intended to reduce criminal thinking patterns, relapse avoidance classes, anger and aggression reduction courses, mental health and substance abuse treatments, academic assistance, family intervention and remediation, parenting skills courses, and life skills and employment readiness training. All parolees, regardless of their transition and release plan, were to receive individual counseling from program staff. The exact mix of services, however, could have varied between sites.

Parolees who were not targeted for residential programming were returned directly to the community and they resided at an approved residence. These parolees received a standard level of community supervision. Under this "business as usual" level of parole supervision, which included monthly home and office visits and random urine monitoring, parolees did not receive the curriculum-based targeted rehabilitative services offered within the residential facilities (e.g., drug treatment). Parole officers generally provide one-on-one guidance such as employment counseling, referrals, and job placement, assistance with transportation, development of job readiness skills, and assistance with finding and securing housing resources. In both cases, however, all parolees received one-on-one counseling and supervision from their parole officers until the expiration of their sentence (or return to custody). This counseling was, for those parolees receiving frontloaded services, in addition to the treatment they received inside the residential facility. Approximately 15 percent of the parolees released from 2009 to 2011 ($n = 2{,}615$) were frontloaded rehabilitative services through placement into residential community programs at five sites during the course of our study.

Data Collection

Criminal history and recidivism data were obtained from two data abstracting systems. The first database contains information on all formally recognized rearrests and reconvictions that occur within the state. The second contains information about parole revocations. Throughout our analyses, the parole revocation outcomes include conduct that stems solely from technical violations that resulted in the parolee being sent back to the custody of the state's Department of Corrections. In all cases, revocations for technical parole violations are for noncriminal breaches of the terms and conditions of parole supervision (e.g., moving without alerting one's parole officer, curfew infractions, failing to attend meetings with a parole officer).

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

Measures

Dependent Variables

The total time at risk for this study is eighteen months postrelease from prison. In addition to the total follow-up period, we provide recidivism analyses for two traditional follow-up times within the recidivism literature: six and twelve months after release from prison. These timeframes, though of limited duration, capture the months considered to be the most essential for frontloading services. We define recidivism in five different ways across the various follow-up times, including: (1) a rearrest for a new crime; (2) a reconviction that stems from a new arrest; (3) a parole revocation that stems from technical violations and results in a return to custody; (4) a combined measure of rearrest or parole revocation, whichever occurs soonest after release from prison; and (5) a combined measure of reconviction or parole revocation, whichever occurs soonest after release from prison. This range of variables is intended to cover the universe of outcomes common in the correctional literature and to account for variation in court processing and technical violations underrepresented in analyses limited to a single outcome (Maltz 1984; Ostermann, Salerno, and Hyatt 2015).

Independent Variables

Our analyses use macro- and micro-level independent variables that are germane to the measurement of frontloading rehabilitative services to parolees. At the macro level, we explore the general effects of frontloading services to parolees through the use of contracted residential, community-based service providers. The SPB uses five sites to transition parolees from prison into the community as a part of a step-down approach. The Board Panel that makes the parolees' discretionary release decision issues a set time, usually ranging from 90 to 180 days, during which the parolee must reside at the facility. The program uses scores from the LSI-R, an actuarial risk and needs assessment tool, to inform its treatment protocols and to provide the parolee with various transitional services such as employment readiness, anger and aggression reduction courses, mental health and substance abuse counseling, life skills training, and family intervention. Importantly, the security levels within residential facilities approximate incarceration in a state prison. However, parolees can leave the facility to seek employment, attend classes, and participate in specialized treatment groups not offered at the facility. Therefore, individuals residing at these facilities, despite spending the majority of their time in a custodial-like environment, can engage in criminal activity while on the street. Parolees who are not frontloaded services through the residential, community-based sites receive standard, general guidance and supervision from their parole officers.

At the micro level, we explore the specific effects of frontloading services to parolees across the five residential, community-based service providers in accordance with their level of quality as measured by the results of a CPC evaluation. As discussed above, CPC assessments measure fidelity to the PEI (Lowenkamp et al. 2010a). Our research team was trained and certified on the use of the CPC by

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

certified trainers in March 2012.[1] Assessments of the five treatment sites included in this study were conducted between June 2012 and September 2013. At the conclusion of the CPC evaluation, one program (Program A) was scored "highly effective," one program was considered "effective" (Program B), Program C fell within the "needs improvement" range, and the final two programs (D and E) were both scored as "ineffective." Taken together, the five programs considered as a part of this study encompass the full spectrum of levels of fidelity to the PEI in correctional programming.[2]

Control and Matching Variables

We employ many covariates that have previously been shown to predict program referral to construct our propensity scores. Our covariates include demographic, instant offense, extent and severity of prior offending, prerelease actuarial risk, and area deprivation information for each of our cases. Demographic variables include age at release, age at first arrest, gender, and race (black, Hispanic, or white, with other serving as the reference category). Instant offense variables consist of the number of offenses for which the offender was incarcerated (one, two, or three, with four or more serving as the reference category); the type of instant offense for which the offender was incarcerated (public order, property, drug, or sex crimes, with violent crimes serving as the reference category); and time served (measured as the number of days between the date of sentence to state incarceration and the date of release).

The number of arrests the parolee experienced prior to release measures the extent of prior offending, and the severity of prior offending is gauged by attaching an estimate of the costs of the parolee's prior arrests (measured in *willingness to pay* [WTP], and in 2007 dollars). We constructed an estimated cost of the crimes committed by each individual over the course of his or her criminal history by investigating the most serious charge within each prerelease arrest event and attaching a WTP indicator that was adapted from Cohen, Piquero, and Jennings (2010). The strategy of attaching costs to the most serious charge within an arrest event is consistent with prior costs of crime research (McCollister, French, and Fang 2010; Piquero 2011; Ostermann and Caplan 2013).

1. Practically, the assessment takes approximately one week to conduct and consists of interviews with the executive director of the program, clinical director(s), clinical, assessment, and intake staff, security officers and clients; staff surveys; reviews of clinical curriculums and assessment instruments; observations of treatment groups and counseling sessions; and review of both open and closed files. Programs can score up to eighty-three points across two domains: capacity and content. Three subcategories make up the capacity area: leadership and development, staff characteristics, and quality assurance; two domains comprise the content area: offender assessment and treatment characteristics. Each program is scored across all categories, and a final score is tallied. Standardized national scoring rates are used to compare programs. Those scoring within the 65–100 percent range are ranked "highly effective"; programs scoring between 55 and 64 percent are considered "effective"; scores from 46 to 54 percent indicate that a program "needs improvement"; and, last, programs ranked at the 45th percentile or less are deemed "ineffective." The CPC, though proprietary, has been widely used to evaluate correctional programming (see Latessa et al. 2009; Latessa, Lowenkamp, and Bechtel 2009; Ostermann 2013; Duwe and Clark 2015).

2. The five programs assessed for this study include the complete population of residential programs in the jurisdiction. The state does not operate any directly comparable, public facilities.

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

Estimates of the costs of criminal histories were constructed in order to represent the gravity of, and harm caused during, each individual's prior criminal career. This strategy allows us to estimate a uniform measurement that serves as a meaningful proxy for the relative seriousness of the parolees' previous arrests. Within the logistic regression models that were constructed to estimate the propensity scores, we use the log transformation of the costs of an individual's criminal history because the data were right-skewed due to the large estimated costs of some crimes (e.g., a murder was estimated to cost $11.8 M).

The parolees' LSI-R composite scores are employed as the relevant prerelease actuarial risk scores, as these are the assessments that were used to inform the Board Panel's discretionary release decision. These assessments were conducted approximately six months prior to the parolee's release and, in addition to contributing to the panel's release decision, inform the decision to place the parolee into a residential program. Finally, we constructed a relative socioeconomic deprivation measure by attaching information from the 2000 decennial census to each parolee's self-identified county in which he or she would reside after release. This information reflects county-level measures for the proportion of the population that is black, the unemployment rate, the proportion of female-headed households, and the proportion of individuals whose income is below the poverty level. These measures were combined using factor analysis ($\alpha = .763$).

Analytic Approach

We performed a series of six propensity scoring and matching routines. First, parolees who were frontloaded rehabilitative services were matched to those who were not, then parolees frontloaded services at each of the five program sites were matched to similar parolees who did not receive those services. Our analytic strategy culminated in the pairing of parolees frontloaded services across all of the five programs to controls, as well as pairs of parolees transitioned through each of the five programs to parolees who were not frontloaded services. This strategy of performing separate analyses (rather than performing the matching process between the larger pools of parolees who are and are not frontloaded services and then merely stratifying by program) ensures that the analyses conclude with the best possible match for each treatment group member across the various programs being obtained from the comparison group pool in accordance with their propensity scores.

All the routines use a logistic regression modeling strategy where the control variables are regressed upon group membership to produce propensity scores that communicate the estimated likelihood that both treatment and comparison group members would be a part of the treatment group given the unique mix of their predictor variable profiles. Throughout the analyses, we use a one-to-one nearest neighbor strategy without replacement, with matches between treatment and comparison group members occurring within a caliper distance of +/– .01 of the propensity score.[3] This means that only one treatment group member is matched to a

3. Different specifications of caliper distances, matching routines, and one-to-many matching led to substantively similar findings.

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

potential comparison group member, that once a match is obtained, the matched comparison group member cannot be used to establish other matches, and that matched pairs will have roughly the same likelihood, based on the predictor variables entered into the logistic regression, of receiving the treatment. By constructing matched groups, we are able to employ between-group comparisons to estimate the average treatment effects on the treated (ATT). These estimates communicate the recidivism rates of parolees frontloaded rehabilitative services as compared to parolees who were not frontloaded services, as well as those same differences across each of the five specific rehabilitative programs compared.

FINDINGS

Between-Group Differences Prior to Matching

Table 1 presents descriptive statistics and bivariate comparisons between parolees receiving frontloaded services and those who did not, both prior to and after matching on their propensity scores. Comparisons show that the two groups of parolees differed across several of the covariates that were entered into the logistic regression model used to build the propensity scores. Parolees who were frontloaded services were significantly older at the time of their release from prison ($t = 6.03$, $p \leq .001$) and experienced their first arrest at a significantly younger age ($t = -7.59$, $p \leq .001$) than parolees who were not frontloaded rehabilitative services. A significantly greater proportion of parolees who were frontloaded services were male ($\mu = 94.7\%$, $t = 4.74$, $p \leq .001$), white ($\mu = 23.5\%$, $t = 3.49$, $p \leq .001$), and returned to more socially deprived counties upon their release from prison ($t = 3.44$, $p \leq .001$).

Parolees frontloaded rehabilitative services also had about 2.5 more prior arrests ($t = 18.28$, $p \leq .001$) and significantly more costly criminal histories ($t = 6.47$, $p \leq .001$). A significantly lower proportion of these parolees were serving instant offenses that were either public order ($t = -4.24$, $p \leq .001$) or sexual ($t = -8.74$, $p \leq .001$) in nature than those not frontloaded services. A significantly greater proportion of frontloaded parolees were also serving instant offenses that were property ($t = 10.54$, $p \leq .001$) or drug crimes ($t = 4.40$, $p \leq .001$). Finally, those frontloaded services had significantly higher average prerelease LSI-R scores ($t = 23.98$, $p \leq .001$) by a magnitude of approximately 3 points than those who did not receive frontloaded services. In sum, the Board Panel members who are charged with deciding whether parolees should be transitioned through frontloaded residential treatment services appear to be, on average, targeting higher-risk parolees. Parolees frontloaded services have more prior criminal justice contacts, these contacts are more serious, their first contact occurred at a younger age, they hail from more socially disorganized areas, and they are higher risk on an established actuarial assessment instrument. This allocation strategy aligns with the relevant PEI.

Propensity Scoring and Matching Balancing Statistics and Sensitivity Analyses

The propensity scoring and matching procedure was effective at minimizing the differences in covariates between the treatment and comparison groups. Prior to

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

TABLE 1.
Pre- and Postmatched Covariate Differences Between Parolees Frontloaded Services and Parolees Not Frontloaded Services

	Prematching				Postmatching				
	Frontloaded Services ($n = 2,615$)	Not Frontloaded Services ($n = 15,341$)	% Bias	t Test	Frontloaded Services ($n = 2,573$)	Not Frontloaded Services ($n = 2,573$)	% Bias	% Bias Reduction	t Test
Age	36.2	34.9	13.2	6.03***	36.1	36.0	1.4	89.6	0.49
Age first arrested	20.3	21.4	−17.9	−7.59***	20.3	20.2	2.5	85.9	1.05
Male	94.7	92.0	10.9	4.74***	94.7	94.5	0.8	92.8	0.31
Black	60.5	58.7	3.8	1.75	60.6	61.4	−1.7	54.4	−0.63
White	23.5	20.4	7.5	3.49***	23.4	23.6	−0.7	91.2	−0.82
Hispanic	15.7	20.4	−12.4	−5.55***	23.4	23.6	−0.7	91.2	−0.23
County deprivation	49.6	48.9	7.5	3.44***	49.6	49.7	−0.6	91.9	−0.22
Time served (days)	813.6	872.6	−4.6	−2.35*	815.2	833.0	−1.4	69.2	−0.53
Prior arrests	10.2	7.7	37.6	18.28***	10.1	10.1	−0.8	97.8	−0.28
Log costs of prior arrests	11.2	10.9	14.8	6.47***	11.1	11.2	−2.3	84.4	−0.89
Two instant offenses	30.0	27.8	2.6	1.18	29.5	29.8	−1.9	26.0	−0.67
Three instant offenses	13.7	14.0	−0.9	−0.41	13.7	13.8	−0.3	62.3	−0.12
Four or more instant offenses	14.7	14.3	1.0	0.47	14.8	14.2	1.7	−62.9	0.59
Public order offense	4.0	6.1	−9.8	−4.24***	4.0	4.4	−1.6	83.6	−0.63
Property offense	19.4	11.7	21.4	10.54***	19.0	18.0	2.7	87.4	0.90
Drug offense	48.3	43.6	9.5	4.40***	48.6	48.5	0.1	99.2	0.03
Sex offense	1.6	5.7	−22.0	−8.74***	1.6	2.3	−3.5	84.0	−1.71
LSI-R score	26.8	23.5	54.1	23.98***	26.8	26.9	−1.8	96.7	−0.68

*: $p \leq .05$; **: $p \leq .01$; ***: $p \leq .001$.

matching, parolees frontloaded rehabilitative services significantly differed from parolees not frontloaded services across thirteen of the seventeen covariates that were used to construct the propensity scores. Four of the covariate comparisons had percentage biases above |20|. After matching, none of the differences that met a statistically significant threshold remained, and none of the percentage biases exceeded |20|. A total of 2,573 of the 2,615 treatment group parolees were matched to appropriate comparison group members for a match rate of approximately 98.4 percent.

Similar results were found within each of the five program-specific comparisons.[4] In short, like the broader comparisons surrounding the frontloading of rehabilitative services, parolees transitioned through particular programs differed significantly from parolees not frontloaded services across several of the covariates entered into the logistic regression models used to construct the propensity scores. After matching, all the previously statistically significant covariate differences were nonsignificant and none of the standardized biases exceeded |20|. Successful matches were obtained for 277 of the 288 parolees transitioned through Program A (the "highly effective" program); 43 of the 44 parolees transitioned through Program B (the "effective" program); 761 of the 769 parolees transitioned through Program C (the "needs improvement" program); 473 of the 478 parolees transitioned through program D (one of two "ineffective" programs); and 1,034 of the 1,044 parolees transitioned through Program E (the second "ineffective" program). These correspond to match rates of 96.2, 97.7, 98.9, 98.9, and 99.0 percent, respectively.

Despite our matching routines achieving balance on the observed covariates that were entered into the logistic regression analyses used to construct the propensity scores, there is still a possibility that unmeasured confounding variables may have introduced residual selection bias into the analyses. This bias may, in turn, cause the conditional independence assumption to fail (Rosenbaum 2005). While the assumption of unconfoundedness is not testable, conducting *sensitivity analyses* can ascertain the magnitude of hidden bias that would have to be present to reverse the conclusions of a study that used a propensity scoring and matching approach (Rosenbaum 2002; Loughran et al. 2015).[5]

To present our results economically, we limit our description of our sensitivity findings to the eighteen-month follow-up effects for the macro-level analysis: parolees frontloaded residential community-based treatment services compared to those not receiving those services.[6] The critical Γ for the upper bound (communicating

4. In the interest of brevity, we do not present the pre- and postmatching covariate differences for each of the five programs that we explored. These results are, however, available upon request.

5. To explore the sensitivity of our analyses we employed the user-written *mhbounds* Stata routine (Becker and Caliendo 2007). The sensitivity analyses within this routine are based on Mantel and Haenszel's (1959) test statistic Q for binary outcomes, and can be used to communicate the critical Gamma (Γ) at which unmeasured confounds would reverse the ATT findings (Apel et al. 2010). The Γ statistic can be interpreted as an odds ratio, and the Q statistics calculated through the *mhbounds* routine allow for the exploration of critical Γ levels for both positive and negative self-selection (in our case, the odds of hidden bias increasing or decreasing the likelihood of being frontloaded residential, community-based services for a treated individual vs. an untreated individual).

6. Sensitivity analyses conducted on the program-specific propensity scoring and matching routines led to substantively similar findings. The full set of findings is available upon request.

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

positive self-selection) was between 1.35 and 1.4, while the critical Γ for the lower bound (communicating negative self-selection) was between 1.2 and 1.25. These Γ values represent scenarios assuming that hidden bias would be required to increase the odds of frontloaded community-based treatment by an additional 35 to 40 percent above the estimated propensity score, and that hidden bias would be required to decrease the odds of treatment selection by 20 to 25 percent in order to reverse the conclusions reported here. Given that Becker and Caleiendo (2007) have demonstrated that treatment effect estimates in applied research are often sensitive to Γ as small as 1.15, we consider the results of our propensity scoring and matching routines to be robust (Apel et al. 2010).

Average Treatment Effects of Frontloading Rehabilitative Services upon Recidivism

Differences in recidivism between parolees frontloaded rehabilitative services and those who were not, both prior to and after matching upon the propensity score, are presented in Table 2. Results show that both prior to and after matching, parolees frontloaded rehabilitative services differ from parolees not frontloaded services across many of the different definitions of recidivism that were explored, as well as across the different follow-up times that were analyzed. With the exception of rearrests ($t = -2.24$, $p \leq .01$) and reconvictions (nonsignificant) within six months, prior to matching, parolees frontloaded services exhibited significantly worse outcomes across all the definitions of recidivism and follow-up times than those who did not receive those services. For example, within eighteen months of release from prison, 38 percent of parolees frontloaded services were rearrested, 20 percent were reconvicted, 26 percent had their parole terms revoked for technical parole violations, over half were either rearrested or revoked, and almost 40 percent were either reconvicted or revoked. Among the parolees not frontloaded services, approximately 30 percent were rearrested and 16 percent were reconvicted; this group exhibited revocation rates and combined revocation and rearrest/reconviction rates of about 15 percentage points less than parolees frontloaded services.

After matching, significant differences in recidivism rates remain between the two groups. These differences are evident across both different definitions and follow-up times. Although parolees frontloaded services exhibit significantly better rearrest and reconviction rates, they fare significantly worse than their matched counterparts when considering parole revocations and combined measures of revocations and rearrests or reconvictions as the definition of recidivism. For example, after matching, within eighteen months of release the rearrest and reconviction rates of parolees not frontloaded services grow to 41 and 23 percent, respectively, compared to 38 ($t = -2.44$, $p \leq .001$) and 20 ($t = -2.66$, $p \leq .01$) percent for those frontloaded services. However, within the same follow-up period, the revocation rate of parolees not frontloaded services is 14.6 percent compared to 26.1 percent for those frontloaded services ($t = 11.61$, $p \leq .001$). These large differences in revocation rates between the groups translate into a significantly greater proportion of parolees who were frontloaded services experiencing rearrests or revocations (50.6%

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

TABLE 2.
Pre- and Postmatched Differences in Recidivism Between Parolees Frontloaded Correctional Services and Parolees Not Frontloaded Correctional Services

	Prematching				Postmatching			
	Frontloaded Services ($n = 2,615$)	Not Frontloaded Services ($n = 15,341$)	Difference (S.E.)	t Test	Frontloaded Services ($n = 2,573$)	Not Frontloaded Services ($n = 2,573$)	Difference (S.E.)	t Test
Rearrested								
6 months	10.6	12.1	−1.5 (0.6)	−2.24**	10.6	16.4	−5.79 (0.8)	−7.34***
12 months	26.0	21.8	4.2 (0.9)	4.71***	25.9	30.0	−4.12 (1.1)	−3.90***
18 months	38.0	29.7	8.3 (0.3)	8.49***	37.9	40.7	−2.80 (1.1)	−2.44*
Reconvicted								
6 months	4.2	3.9	0.2 (0.4)	0.64	4.2	6.2	−2.0 (0.5)	−4.06***
12 months	11.2	9.6	1.5 (0.6)	2.44*	11.0	13.6	−2.6 (0.8)	−3.32***
18 months	20.2	16.3	3.9 (0.8)	5.00***	20.0	22.6	−2.6 (1.0)	−2.66**
Revoked								
6 months	13.8	4.9	8.9 (0.5)	17.68***	13.8	7.7	6.2 (0.7)	7.95***
12 months	23.2	9.3	13.8 (0.6)	20.88***	23.2	13.3	9.9 (0.9)	10.38***
18 months	26.1	10.6	15.4 (0.7)	22.09***	26.1	14.6	11.5 (0.9)	11.61***
Rearrested or revoked								
6 months	20.3	15.2	5.1 (0.8)	6.61***	20.3	21.1	−0.8 (1.0)	−0.84
12 months	39.8	26.6	13.2 (1.0)	13.91***	39.7	36.7	3.0 (1.2)	2.63**
18 months	50.7	34.3	16.5 (1.0)	16.25***	50.6	46.4	4.2 (1.2)	3.57***
Reconvicted or revoked								
6 months	16.9	8.3	8.6 (0.6)	13.98***	16.9	12.9	4.0 (0.9)	4.57***
12 months	29.8	16.8	13.0 (0.8)	15.92***	29.7	23.6	6.0 (1.1)	5.66***
18 months	38.4	23.3	15.2 (0.9)	16.56***	38.2	32.1	6.2 (1.1)	5.43***

*: $p \leq .05$; **: $p \leq .01$; ***: $p \leq .001$.

vs. 46.4%, $t = 3.57$, $p \leq .001$) as well as reconvictions or revocations (38.2% vs. 32.1%, $t = 5.43$, $p \leq .001$) than parolees with very similar background characteristics who were not frontloaded services. Taken together, these results suggest that the frontloading of treatment services has a preventative effect on subsequent criminal recidivism, but an aggravating effect on subsequent parole revocations. We now turn our attention to isolating the effects of programmatic integrity, considered in light of the mandates of the PEI and as quantified by the CPC assessments conducted.

Program Quality and Recidivism Outcomes

The differences in recidivism between parolees frontloaded rehabilitative programs and matched controls for each of the five programs can be found in Table 3. A significantly lower proportion of participants of Program A, the "highly effective" program, were rearrested for new crimes within six ($t = -4.96$, $p \leq .001$) and twelve months ($t = -2.40$, $p \leq .05$) of release from prison than their matched counterparts who were not transitioned through a program. Parolees transitioned through this program were subsequently rearrested at rates of about 8 and 25 percent after six and twelve months compared to rates of 16 and 31 percent for otherwise similar parolees who were not transitioned through a program. However, revocation rates for parolees transitioned through this program were significantly higher within the twelve-month (24.2% vs. 15.5%; $t = 3.26$, $p \leq .001$) and eighteen-month (26.0% vs. 17.7%; $t = 3.05$, $p \leq .01$) follow-up periods than their matched comparisons. Parolees who were transitioned through Program B, the "effective" program, did not significantly differ from their matched pairs across the various definitions of recidivism and follow-up times.

Parolees transitioned through Program C, the "needs improvement" program, exhibited significantly lower rates of rearrest over six (11.3% vs. 20.2%; $t = -6.80$, $p \leq .001$) and twelve months (28.3% vs. 33.8%; $t = -3.07$, $p \leq .01$), but these differences tapered off at the eighteen-month mark. Parolees transitioned through this program were revoked at a rate of 22.2 percent compared to 17.2 percent for their matched pairs within eithteen months of release from prison ($t = 3.04$, $p \leq .01$), which likely translated into a significantly higher rate of the combined measure of reconvictions or revocations for this group within an identical follow-up time ($t = 1.99$, $p \leq .05$). Likewise, the significantly lower rearrest rate for Program C parolees within six months of release likely contributed to their significantly lower rate of rearrest or revocation within six months of release from prison than their matched pairs ($t = -4.17$, $p \leq .001$).

Lastly, parolees transitioned through Programs D and E, both scoring in the "ineffective" range on the CPC, had their parole terms revoked for technical infractions and experienced either a reconviction or a revocation at significantly higher rates across all of the follow-up times under study than their matched counterparts. Across both programs, revocation rates differed between the treatment and comparison groups by approximately 6, 10, and 13 percentage points across the six-, twelve-, and eighteen-month follow-up times. Reconviction or revocation rates

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

TABLE 3.
Postmatched Differences in Recidivism Between Parolees Frontloaded Correctional Services and Parolees Not Frontloaded Correctional Services According to Program Quality

	"Highly Effective" Program Comparison				"Effective" Program Comparison			
	Program A ($n = 277$)	Program A Matched Comparison ($n = 277$)	Difference (S.E.)	t Test	Program B ($n = 43$)	Program B Matched Comparison ($n = 43$)	Difference (S.E.)	t Test
Rearrested								
6 months	7.6	16.2	−8.7 (1.7)	−4.96***	16.3	25.6	−9.3 (8.8)	1.05
12 months	24.9	31.4	−6.5 (2.7)	−2.40*	23.3	37.2	−13.9 (9.9)	1.41
18 months	38.6	44.0	−5.4 (3.0)	−1.78	46.5	58.4	−11.6 (10.8)	1.07
Reconvicted								
6 months	5.8	5.4	0.4 (1.5)	0.24	7.0	7.0	0 (5.6)	0
12 months	10.5	14.8	−4.3 (2.0)	−2.17*	20.9	16.3	4.7 (8.5)	−0.55
18 months	19.9	24.5	−4.7 (2.5)	−1.86	20.9	27.9	−7.0 (9.3)	0.75
Revoked								
6 months	15.5	11.9	3.6 (2.3)	1.60	25.6	14.0	11.6 (8.6)	−1.35
12 months	24.2	15.5	8.7 (2.7)	3.26***	34.9	32.6	2.4 (10.3)	−0.22
18 months	26.0	17.7	8.3 (2.7)	3.05**	34.9	34.9	0 (10.4)	0
Rearrested or revoked								
6 months	20.9	24.9	−4.0 (2.6)	−1.54	30.2	32.6	−2.3 (10.1)	0.23
12 months	40.8	39.4	1.4 (3.1)	0.47	44.2	53.5	−9.3 (10.9)	0.86
18 months	51.6	50.5	1.1 (3.1)	0.35	58.1	69.8	−11.6 (10.4)	1.12
Reconvicted or revoked								
6 months	20.6	16.6	4.0 (2.5)	1.57	27.9	20.9	7.0 (9.3)	−0.75
12 months	32.5	27.8	4.7 (2.9)	1.60	41.9	41.9	0 (10.8)	0
18 months	39.4	36.1	3.2 (3.1)	1.07	48.8	41.9	6.9 (10.8)	0.64

TABLE 3 *(Continued)*

	"Needs Improvement" Program Comparison				"Ineffective" Program Comparison			
	Program C ($n = 761$)	Program C Matched Comparison ($n = 761$)	Difference (S.E.)	*t* Test	Program D ($n = 473$)	Program D Matched Comparison ($n = 473$)	Difference (S.E.)	*t* Test
Rearrested								
6 months	11.3	20.2	−8.9 (1.3)	−6.80***	11.6	14.4	−2.7 (1.6)	−1.74
12 months	28.3	33.8	−5.5 (1.8)	−3.07**	24.7	28.8	−4.0 (2.1)	−1.90
18 months	39.8	41.8	−2.0 (1.9)	−1.01	35.7	37.8	−2.1 (2.4)	−0.90
Reconvicted								
6 months	4.3	5.7	−1.3 (0.8)	−1.60	3.4	3.8	−0.4 (0.9)	−0.47
12 months	10.2	15.1	−4.9 (1.2)	−3.94***	12.9	11.4	1.5 (1.6)	0.92
18 months	20.6	22.9	−2.2 (1.6)	−1.40	20.5	22.0	−1.5 (2.0)	−0.75
Revoked								
6 months	10.4	10.2	0.2 (1.2)	0.11	14.2	8.0	6.1 (1.7)	3.65***
12 months	18.8	16.3	2.6 (1.6)	1.69	24.5	14.4	10.1 (2.1)	4.85***
18 months	22.2	17.2	5.0 (1.6)	3.04**	28.8	16.3	12.5 (2.2)	5.69***
Rearrested or revoked								
6 months	19.4	26.1	−6.7 (1.6)	−4.17***	19.5	19.9	−0.4 (1.9)	−0.22
12 months	41.1	41.0	0.1 (2.0)	0.07	37.0	36.2	0.8 (2.4)	0.36
18 months	52.3	48.8	3.5 (2.0)	1.79	48.2	44.2	4.2 (2.4)	1.64
Reconvicted or revoked								
6 months	13.9	14.5	−0.5 (1.4)	−0.38	16.9	11.4	5.5 (1.8)	3.01**
12 months	26.1	26.7	−0.5 (1.8)	−0.30	30.0	22.2	7.8 (2.2)	3.50***
18 months	37.7	33.9	3.8 (1.9)	1.99*	38.5	31.9	6.6 (2.4)	2.76**

	"Ineffective" Program Comparison			
	Program E ($n = 1{,}034$)	Program E Matched Comparison ($n = 1{,}034$)	Difference (S.E.)	t Test
Rearrested				
6 months	10.2	16.5	−6.4 (1.1)	−5.95***
12 months	25.0	27.7	−2.6 (1.5)	−1.76
18 months	36.8	38.2	−1.4 (1.6)	−0.82
Reconvicted				
6 months	4.0	5.4	−1.5 (0.7)	−2.05*
12 months	10.6	13.6	−3.0 (1.1)	−2.74**
18 months	19.3	20.3	−1.0 (1.4)	−0.71
Revoked				
6 months	15.2	7.8	7.4 (1.2)	6.19***
12 months	24.9	13.5	11.3 (1.4)	7.94***
18 months	27.2	14.6	12.6 (1.5)	8.55***
Rearrested or revoked				
6 months	20.7	21.1	−3.9 (1.4)	−0.28
12 months	39.3	34.3	4.9 (1.7)	2.98**
18 months	49.6	44.0	5.6 (1.7)	3.31***
Reconvicted or revoked				
6 months	17.7	12.0	5.7 (1.3)	4.44***
12 months	30.8	24.0	6.8 (1.6)	4.35***
18 months	37.9	30.5	7.4 (1.6)	4.55***

*: $p \leq .05$; **: $p \leq .01$; ***: $p \leq .001$.

significantly differed by about 6, 8, and 7 percentage points across the follow-up times. Program E parolees exhibited significantly lower rearrest and reconviction rates over six months of follow-up time ($t = -5.95$, $p \leq .001$ and $t = -2.05$, $p \leq .05$, respectively). However, a significantly greater proportion of parolees transitioned through this program were rearrested or revoked at both the twelve-month ($t = 2.98$, $p \leq .01$) and eighteen-month ($t = 3.31$, $p \leq .001$) marks.

DISCUSSION

Our findings indicate that the process of frontloading rehabilitative services to parolees significantly lowers the likelihood of rearrest and reconviction within eighteen months of postrelease follow-up time, but at the cost of substantially increasing the probability of parole revocation. In line with the fundamentals of the evidence-based corrections literature, the effect sizes of our findings consistently align across the varying levels of programmatic quality. A higher score on the CPC, an aggregate measure of program quality, is generally associated with a more substantial reduction in criminal recidivism. Additionally, although treatment group parolees were more likely to have their parole terms revoked for technical infractions than their matched counterparts across all the programs, the size of these effects were generally smaller within higher-quality programs. Thus, these results suggest that adherence to the PEI can encourage a relative reduction in technical violations. However, our findings allude to the fact that parolees who were frontloaded services may appear more successful when considering criminal recidivism outcomes such as rearrests and reconvictions because they are likely not at risk for experiencing these types of outcomes for much of the follow-up period due to their increased likelihood of having their term of community supervision revoked for technical infractions.

In short, reductions in rearrest and reconviction that are associated with the frontloading of rehabilitative services are likely driven by the mechanism of increasing risks of parole revocation. To shed additional light on this phenomenon, we conducted a series of post hoc analyses on those who were "not rearrested" and "not reconvicted" to ascertain whether they were revoked during the follow-up period. Approximately 20 percent of parolees who were frontloaded services who were not rearrested had their parole revoked, while 10 percent of the matched comparison group who were not rearrested were revoked. Likewise, 23 percent of parolees who were frontloaded services and who were not reconvicted experienced a revocation, while 12 percent of their matched counterparts were not reconvicted, but revoked. Our post hoc analyses demonstrate that twice as many parolees who are frontloaded services and who are not rearrested/reconvicted experience parole revocations that prevent them from experiencing such outcomes than matched parolees who are not frontloaded services. These results highlight the complex relationship between both the nature and timing of rehabilitative programming and successful community reintegration.

It is counterintuitive that frontloading rehabilitative programming increases the likelihood that parolees will experience revocations of parole stemming from technical infractions, especially when the practices of the programs strongly align

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

with the tenets of effective correctional interventions (Petersilia 2003; Grattet et al. 2009). We interpret these findings by drawing from the broader theoretical, policy, and contextual backdrops in which these services are delivered. First, regardless of the treatment component, residents of these facilities live under a stricter set of rules than individuals in the community. These additional rules govern a wider range of behaviors, largely undetectable in the community, for which the offender can be sanctioned. The residential nature of the programs also increases the raw number of hours the parolees are being monitored. This likely has a one-way ratcheting effect on levels of formalized control, and increases the number of technical violations both detected and prosecuted. However, within this context, the increased monitoring experienced by parolees who were frontloaded services is likely not a key driver of our results due to high successful completion rates across all of the programs (with rates of approximately 75 percent across the five sites).[7] Further, follow-up analyses indicate that 91 percent of parolees who were both frontloaded services and had their parole revoked experienced the revocation after successfully completing the frontloaded program.

The proclivity of parole officers to revoke parolees who have previously completed transitional services likely relates to the ways in which their own job performance is measured. Revocation and recidivism rates are rarely used to communicate parole officer performance (Caplan 2006; Kinnevy and Caplan 2008; Miller 2015). Instead, officer performance is measured by surrogates for these goals, such as the number of face-to-face contacts they have with parolees, the number of home visits conducted, the number of community contacts initiated, or the number of urine samples collected. Relatedly, core parole goals including "increasing public safety" or "ensuring pro-social transitions from prison to the community" are relatively ambiguous constructs. The more easily quantifiable metrics used for performance evaluation allow for agencies and managers to exercise control over officers and reduce the parameters of effective supervision to whether or not they have met minimum contact thresholds dictated through agency policy. Officers are, therefore, tasked with applying the levers of social control in accordance with aggregate-level classifications and policies, not what they consider to be effective for the individual parolee.

The substitution of agency-focused measures of efficiency over offender-level outcomes, for example, the percentage of an officer's caseload that successfully returns to the community, changes the incentive structure that may drive decision making. The goals of community reintegration and delivery of evidence-based treatment become secondary. In light of the high revocation rates observed in this study, this approach undercuts the impact of potentially beneficial programming. The impact is particularly notable for successful interventions following the PEI, as parolees become more likely to be diverted away from these resources and back into custody if these services have previously been geared toward the parolee. The net effect of this conceptualization of the supervision climate redefines the roles of both the officer and agency, as well as formalizing and operationalizing the ideological

7. Follow-up analyses limited to successful program completers and their matched controls led to substantively similar findings.

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

shift toward a *new penological* orientation within community corrections (Malcolm and Feeley 1992, 1994).

The tensions in this element of parole officer decision making are informed by their responsibilities as street-level bureaucrats within the correctional system (see Lipsky 1980; Gilbert 1997). In this role, officers are generally granted significant power in how they respond to individual offenders and infractions. Conflicts occur when their discretion is limited, either through the exercise of managerial oversight (Whitehead and Lindquist 1986), or when the information they are systematically provided to make decisions with is drawn from generalizations perceived to be uninformative (e.g., aggregated risk scores) (Harcourt 2007). In keeping with the *new penology*, these constraints are increasingly commonplace. From this perspective, officers should make decisions based on the systematic need to assess and manage a large number of offenders and must rely largely on group-level actuarial data. The goal of correctional supervision, therefore, shifts to a system of management and control. Officers must manage their offenders with this in mind and accomplish these goals by increasing the intensity of control.

High rates of returns to custody, like the ones reported here, offer direct evidence of "the efficiency and effectiveness of parole as a control apparatus" (Feeley and Simon 1992, 455). Parole officers must continuously evaluate what opportunities they have to punish—and to incentivize—parolees in light of the pressures to prevent crime and encourage rehabilitation. With pressures toward efficiency and the minimization of individual-level characteristics, officers may see revocation as an appealing—and primary—management strategy.

This shift in the structure of social control has strong downstream implications. As the metrics that are used to guide officers toward achieving the agency's goals change, they, in turn, impact officer behavior through incentivizing the use of easy to measure contact-counting behaviors (and de-emphasizing individualized and time-consuming therapeutic activities). This may result in parole officers supplanting EBPs with easily countable, surrogate behaviors. For example, motivational interviewing (MI) is a common EBP used with parolee populations. Officer-parolee interactions that follow this approach have a demonstrated effectiveness, but they are more time and labor intensive than standard supervision practices (Miller and Moyers 2006; Taxman and Marlowe 2006). If agencies routinely measured recidivism (including parole revocations) as a performance measure, and held individual officers directly accountable for the failure rates of those under their supervisory purview, it is likely that officer behaviors and decisions to enter parolees into the revocation stream would substantially shift. EBPs like MI could be more readily employed. Until this occurs systematically, short-term efficiency and process management, the problematic *new penological* supervision paradigms, will continue to take precedence over long-term behavioral change.

This ideological shift is greatly facilitated, and in some cases encouraged, by the process through which treatment services are subcontracted out to nongovernmental providers. Claims of the value added through the management of corrections by outside parties highlight these same priorities: efficiency and cost effectiveness (see, e.g., Hart, Shliefer, and Vishny 1997). At the same time, outsourcing, due to the contractual nature of the relationship, encourages the use of

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metrics for success that can be quantified in the short term. This is necessary for the service providers to demonstrate that they are delivering services as promised. Long-term recidivism of program participants, a much more complex and difficult to measure outcome, is challenging to link to the substance of supervision. This further reinforces the changes in how control is applied within the parole environment. At the same time, governmental divestment reduces the immediate oversight that state actors have over the substance of rehabilitative programming. While we are not suggesting that the state has no interest in, or ability to, oversee contracted programs, there is a stark difference between agency micromanagement at the daily level and the periodic reviews that are more likely once a service has been outsourced.

The results of the CPC presented here underscore this difference; all the programs, even those that failed to reach minimum standards for effective, evidence-based programming, received government contracts. The degree to which each of these programs adhered to the CPC was not available at the time the contracts were issued, or when the capacity of each site was assessed. It is unclear whether the selection of evidence-based programming or simply bed capacity was a higher priority when these contracts were administered.

The results of our analysis have shown that programs that scored higher on the CPC reduced recidivism (here, rearrests and reconvictions) at greater rates than less rigorous programs. However, the responsibilities for reducing recidivism (and the attendant goals of protecting public safety) are borne largely by the state. The service providers may measure success simply by treating the individuals under their supervision for the duration of the contracted services. For programs focused on profit through efficiency, this outcome is paramount. The less evidence-based programs may simply have been able to enroll more offenders over the same period of time; the delivery of true EBPs can be expensive and time consuming. We are, once again, presented with an institutional structure that values process over effectiveness, here with regard to back-end impact.

Benchmark data on contacts are not without value, as they do show that services are being delivered in accordance with the recommended frontloaded timeline. The effects of this process may impact supervision beyond the initial postrelease placement of the parolee. The Board's decision to employ those services may signal that those parolees are higher risk or borderline cases who would not have been released to parole in the absence of these sorts of programmatic resources. Subsequently, parole officers may hold differential attitudes about, and behaviors toward, frontloaded parolees, causing them to label the case as higher risk than other parolees on their caseload. This one-way "ratcheting" process results in the reifying of existing risk profiles, and allows risk profiles to alter agency decision-making processes (Harcourt 2007). Even in the absence of an actuarial risk score, officers likely recognize that the assignment of a parolee to frontloaded services can serve as a proxy for the Parole Board's assessment of dangerousness and/or high criminogenic needs. The fact that the frontloaded programs are delivered in a semicustodial environment and under higher levels of social control likely reinforces this perception.

Though intended to facilitate (and even mandate) treatment, the net effect of frontloading services may be unofficially and informally to label certain offenders as

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difficult or challenging cases (Cullen and Jonson 2013). This process could make the officer more mindful and less tolerant of negative behaviors, even when they are of a similar nature to other nonidentified parolees on the officer's caseload, and result in increased technical violations. Resultantly, parolees receiving frontloaded services would be revoked for conduct that would, under standard supervision conditions, not have resulted in a revocation. This is an interesting mechanism, especially when considering that the results of our matching routines were able to identify characteristically similar parolees on officer caseloads, but for the experience of frontloaded services. These effects also underscore the complex and often subjective influences, both intentional and inadvertent, which impact the decisions to revoke a given parolee.

It is essential to note that these findings should not be viewed as an attack on contracted community programs. Indeed, the administrators of programs that do not strongly align with effective correctional interventions should strive to provide more theoretically sound services—but, in turn, effective service provision should matter. Studies have consistently demonstrated that the practices of correctional programs that more closely align with risk, need, and responsivity principles provide for better recidivism results than lower-quality programs (Lowenkamp, Latessa, and Smith 2006). Although they are contracted service providers, there must be a level of reciprocal responsibility for the well-being of clients between the state and the provider. It is antithetical to the goals of community corrections to support a policy of community-based programming that increases the aggregate likelihood of being revoked during the course of supervision.

Similar studies should be conducted in other jurisdictions to investigate the external validity of our findings. These explorations can help to specify whether similar phenomena are taking place in other correctional contexts, or whether the parole policies particular to the instant context are an anomaly. Additionally, future scholarship should strive to study the contextual mechanisms underlying parole officer decision making more comprehensively, especially the decision to enter a parolee into the revocation stream, and how these decisions relate to performance measures and agency goals. Finally, the shifts in social control evidenced in this scenario, and as predicted by the scholars of the *new penology*, should be investigated more deeply and with a focus on the complex interaction between organizational structure, ideology, and the economics of community corrections. These types of research endeavors may provide additional insights into the reasoning behind why frontloading rehabilitative services can increase the likelihood of subsequent parole revocation.

LIMITATIONS

This study, as is the case for most secondary analyses, is limited by the administrative data and measures collected by our partner agency and made available for this research. In this case, some key data were not gathered or retained, including the specific reason(s) an individual was enrolled in the facilities being studied. Examples include, but are not limited to, institutional infraction history, prior

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press

participation in in-prison treatment programming, custodial disciplinary records, prior performance on parole, levels of drug and/or alcohol dependence, the viability of the offender's postrelease plan, and the presence of a significant mental health history. Additionally, both the matching variables and outcome data are limited to conduct within a single jurisdiction. Though there is some research suggesting that paroled offenders tend to remain—and offend—within a single state (e.g., Rengert, Piquero, and Jones 1999; Spivak and Damphousse 2006), we cannot exclude the potential for undercounting offending that took place outside of the state.

There is also a degree of temporal variation between the various measures employed in this analysis. It should be noted that our study period consists of parolees released from prison from January 2009 to the end of December 2011, and that recidivism outcomes are gathered for eighteen months postrelease, a period that does not directly align with the period during which we conducted the CPC assessments. Thus, it is possible that some of the characteristics of the program may have changed between the time that our recidivism data were gathered and the time that our CPC assessments were conducted. However, based on the information gathered during the CPC assessment, as well as subsequent follow-up and anecdotal data, this is exceedingly unlikely. Further, items within the CPC instrument explicitly ask whether any shifts in programming or programming resources have occurred within the previous two years; all the programs responded in the negative. Thus, while readers should qualify these results, it is likely that the retrospective nature of these data does not substantively impact our findings.

CONCLUSION

The monetary and social costs associated with sustained high rates of correctional supervision have reinvigorated interest in the development of effective rehabilitative programming. These results show that the resultant guidelines developed for community-based supervision can be effective in encouraging treatments that reduce rearrest and reconviction rates when frontloaded as part of the reentry process. Additionally, although enrollment in the programming options studied here was associated with an increase in the likelihood of revocation, those interventions that more closely followed the principles of evidence-based programming were able to reduce the relative rate of technical violations.

However, the expansion of EBPs, even those known to be effective interventions, is not without cost. As the demand for evidence-based programming has grown, private, often for-profit entities have sought the opportunity to provide these essential services. This has created another interested party in the correctional process, and has further expanded the administrative reach of the carceral state (Gottschalk 2008; Beckett and Murakawa 2012). Attendant reliance on concrete performance metrics and actuarial assessments, which are not necessarily problematic on their own, has encouraged a control- and law-enforcement-oriented paradigm to become the dominant mode of supervision. The net result is an increase in the constraints of formal social control within the community correctional system

and the solidification of the framework of the *new penology* as a powerful, and defining, archetype for parole treatment and supervision.

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APPENDIX 1: ESTIMATED WILLINGNESS TO PAY COSTS OF CRIMES (2007 DOLLARS)

	Willingness to Pay
Murder	$11.8 million
Rape	$290,000
Armed robbery	$280,000
Robbery	$39,000
Aggravated assault	$85,000
Simple assault	$19,000
Burglary	$35,000
Motor vehicle theft	$17,000
Larceny	$4,000
Drunk driving crash	$60,000
Arson	$115,000
Vandalism	$2,000
Fraud	$5,500
Other crimes	$1,000

[1]*Notes*: This table was adapted from "Studying the Costs of Crime Across Offender Trajectories" by M. A. Cohen, A. R. Piquero, and W. G. Jennings. 2010. *Criminology & Public Policy* 9:286. Copyright 2010 by the American Society of Criminology.

https://doi.org/10.1111/lsi.12300 Published online by Cambridge University Press