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Attrition from Jail Reentry Program Increases Recidivism

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Abstract
Reentry programs represent an increasingly popular method to reduce recidivism for individuals exiting prison and jail systems throughout the United States. Most evaluations tend to focus on recidivism as the primary outcome of interest. Attrition, however, can function an important supplementary measure that complements recidivism outcomes. To demonstrate, we analyze a jail reentry program built around peer navigators serving as staff members that refer participants to necessary support services while also serving as a mentor to participants exiting jail. We use a combination of general linear models (GLMs), Mahalanobis distance matching (MDM), and panel regression to both predict attrition and compare recidivism outcomes between three attrition groups: program completers, program quitters, and matched controls. Participants that successfully completed the program did not avoid new convictions or reincarceration significantly more or less than matched controls. Participants that quit the program, however, saw significantly higher conviction and reincarceration rates compared to matched controls. The nuance added to our program evaluation by adding attrition as a differential factor is worth consideration by other reentry programs who may not be realizing the full picture of their results by presenting recidivism outcomes alone.

Keywords Reentry programs · Recidivism · Attrition · Peer navigation · Peer mentorship

The most recent data released from the U.S. Department of Justice indicate that as of 2020, an estimated 549,100 adults were incarcerated in local jails with an average length of stay equal to 27.8 days (Minton & Zeng, 2021). Data from a similar report suggest that approximately 82% of people released from state prison and reentering

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the general populace will be rearrested at least once within 10 years post-release (Antenangeli & Durose, 2021). Over half of these arrests happen soon after release with 43% of released individuals recidivating within a year of their release. Local jail recidivism rates appear similarly high based on available reports (e.g., Spjeldnes et al., 2012; Miller & Miller, 2010; Wilson et al., 2011).

Jail and Prisoner Reentry Programs

Recidivism prevention is imperative to improving public safety, mitigating the economic burden of incarceration, reducing racial disparities in jail/prison populations, improving quality of life for formerly incarcerated people, and promoting a more positive image of released individuals while also discouraging discrimination. One of the most common public service initiatives designed for this purpose are reentry programs. Either prior to release or soon after release, people currently incarcerated in either jail or prison are admitted to these programs which address various criminogenic needs that contribute to recidivism such as lack of employment/housing, negative peer and familial influences, antisocial cognition/behaviors, and substance use (Andrews et al., 1990; Andrews & Bonta, 2016). Most programs choose an area of focus within the list of empirically supported criminogenic needs while also tailoring the program to be both palatable for the participants enrolled and representing ideals of the public agencies or private organizations that sponsor them. Published recidivism outcomes of these programs vary greatly depending on the structure of the programs. Some report no significant reduction in recidivism (Visher et al., 2005; Powers et al., 2017); some rare cases find program participation was detrimental (Severson et al., 2011); some observed a short-term reduction and either did not assess long-term impact or showed diminished effect over time (Clark, 2015; Lattimore & Visher, 2013); some determined that the program benefitted only specific subpopulations within the sample (Garland & Hass, 2015; Newton et al., 2018; Cannonier et al., 2021); and a few support long-term reduction of recidivism (Duwe & King, 2013; Duwe, 2013; Higuera et al., 2021).

Attrition Within Reentry Programs

While it would be simple to attribute the variability in efficacy of reentry programs to the needs addressed and the methodology used, one key factor typically receives less attention: attrition. Many publications of reentry program results either fail to mention attrition at any point, do not address the outcomes for participants that quit the program, or limit their results to compliant and completing program participants (e.g., Haviv & Hasisi, 2019; Duwe & King, 2013; Zweig et al., 2011; McNeeley, 2018; Miller et al., 2016). Some program evaluations present supplemental analyses that find no effect and quickly move on to their primary results (e.g., Lattimore & Visher, 2013; Mowen & Boman, 2018). Others recognize the issue of attrition and attempt to introduce quantitative correctional methods to account for data missing due to withdrawal from longitudinal research (e.g., Link & Hamilton, 2017). Rarely can one find reentry outcomes based on an intention-to-treat model where *all* participants are considered (e.g., Haggård et al., 2017, Duwe & Goldman, 2009; Baggio et al., 2020). This is not to say that attrition is of no interest to researchers in the field of criminal justice; rather, the issue is that attrition is often investigated independent of recidivism outcomes (e.g., Roman et al., 2007, Listwan, 2008; Clark et al., 2020; Mitchell et al., 2022).

At a basic level, attrition's impact on ex-participants seems simple: participants that quit reentry programs consistently have worse outcomes than those that complete the programs (Jewell & Wormith, 2010; Mitchell et al., 2022; Olver et al., 2011; Lockwood & Harris, 2013; Friedman et al., 2022). To assume a direct relationship where attrition *causes* negative outcomes, however, could be a fallacious conclusion. As an example, this observation could be due to selection bias. Certain factors predict attrition similar to recidivism, and thus program participants that attrite may represent a higher risk population to begin with (Wormith & Olver, 2002; Jewell & Wormith, 2010; Mitchell et al., 2022). Others label this theory insufficient and claim that failure to complete the program procures negative consequences such as longer sentence time, negative authoritative interactions, and withdrawal of financial support that increase risk of recidivism (Olver et al., 2011). The previous stance may still lack detail as it clusters together people that quit the program voluntarily and those that are removed for some sort of violation of program rules; such a distinction may have profound implications on recidivism outcomes (Lockwood & Harris, 2013). None of these theories are mutually exclusive and they likely layer together to form a complex model of the relationship between attrition and recidivism outcomes. Analyzing attrition outcomes alongside recidivism would go a long way in identifying the functional nuance behind attrition in reentry programs.

The Role of Peer Support in Reentry Programs

By adding attrition as an additional metric by which reentry programs are evaluated, common subtopics in reentry literature can be given new perspective. Take for example the role of peer support in reentry programs. Implementation of peer support is based both on the theories of criminogenic needs (Andrews & Bonta, 1990; Andrews & Bonta, 2016) and differential association theory (Cressey, 1954), which both highlight the heavy motivational influence peers have on an individual's criminal tendencies. In short, a peer defined by antisocial, anti-authority, and pro-crime values, cognition, and behaviors is likely to pass those same traits to other peers. While both theories tend to focus on the negative influence peers pose, one must consider the premise that peers who support reentry and reform can have an equally impactful, positive influence on the individual leaving and not returning to jail (Mowen & Boman, 2018).

Many reentry programs of late have integrated peer support into their service plan, to moderate success as gauged by reduced recidivism (Bellamy et al., 2019; Woods et al., 2013; Sells et al., 2020), decreased drug-seeking and relapse behaviors (Ray et al., 2021; Marlow et al., 2015), and positive survey responses/feedback (Ray et al., 2021; Johnson et al., 2015; Kirkwood, 2021). Explanations as to why peer supports function effectively in reentry programs include firsthand knowledge of the reentry experience (Reingle Gonzalez et al., 2019), personality and motivational differences from non-peer staff (Lebel et al., 2014), the ability to form trusting relationships that would not be possible without firsthand experience (Perrin et al., 2017), and the sense of community and belonging that peer supports build with their clients as they reenter society (Martinez et al., 2022).

The one measure conspicuously missing from the previous description of peer support is attrition. Even in scenarios where peer support had clear positive outcomes, no papers attribute this to reduced attrition—the point is not even discussed. Thus, a clear gap in the literature makes peer support an ideal reentry subtopic to test the value of analyzing attrition alongside recidivism. Is there some as-of-yet unobserved interplay between peer support, attrition, and recidivism outcomes waiting to be identified, or does attrition add no meaningful information to the final program evaluation?

Current Study

If criminal justice advocates wish to fully comprehend the mechanisms behind how reentry programs benefit participants, expanding analyses to include attrition is a logical next step. We present the results of a jail reentry program that addressed the need for employment, housing, and healthcare while utilizing a peer navigation system to bolster program/service engagement and mediate reentry with the ultimate goal of preventing recidivism. In analyzing the success of this program, the following research questions were proposed:

1. Do any factors associated with demographics, criminal history, or program services predict attrition?
2. Do participants that complete the reentry program have fewer recidivism events than participants that did not complete the program?
3. Do program completers and non-completers have fewer recidivism events than matched controls that were never in the reentry program to begin with?
4. If program completion or non-completion affects recidivism, how does the effect change over time (e.g., 1 year post-intake, 2 years post-intake)?
5. Over the course of 5 years after the reentry program started, how does the number of recidivism events differ between program completers, program non-completers, and matched controls?

By designing the statistical assessment of this program around these research questions, the role of attrition in the success or failure of reentry programs can be quantified and investigated.

Method

Participants

Participants were recruited to the jail reentry program while incarcerated at the Kent County Correctional Facility. Program eligibility criteria included 18–26 years of age, male, medium to high risk (as measured by the Proxy Score; Bogue et al., 2006), and 30 days or less until their release from the jail. Priority recruitment was also given for black and Hispanic men. Analyses that did not require matched controls included a total of $N=137$ program participants. This overall sample includes $n=57$ participants that completed the program ("completers") and $n=79$ participants that quit the program ("quitters"). The quitters group includes both participants that directly requested to discontinue the program and those that could not be contacted for 3 months.

For analyses that used matched data, the $n=57$ completers were matched against $n=57$ quitters and $n=57$ control cases. Control cases were selected from a larger dataset of general recidivism data ($n=437$) that never participated in the program via Mahalanobis distance matching (MDM; King & Nielsen, 2019) with optimal pair matching. Collectively, all cases can be labelled as belonging to one of three "completion groups": completers, quitters, or controls. All cases were matched according to their age at the time of first arrest, race/ethnicity, attainment of a high school diploma or equivalent, length of stay in jail, time since release from jail, number of past arrests, number of past violent felonies, number of past nonviolent felonies, and risk of recidivism (i.e., proxy scores). The exact demographics and criminal history of both the unmatched and matched samples are given in Table 1.

Table 1 Demographics and criminal history by group, pre- and post-match

	Completers	Quitters		Controls	
	Both	Original	Matched	Original	Matched
	***n*=57**	*n*=79	***n*=57**	*n*=437	***n*=57**
Age	**21.9±0.6**	21.2±0.6	**21.0±0.6**	33.8±1.1	**24.9±1.3**
Race					
Black	**86.0%**	78.5%	**87.7%**	55.1%	**86.0%**
Hispanic	**3.5%**	6.3%	**3.5%**	6.9%	**3.5%**
White	**10.5%**	15.2%	**8.8%**	37.8%	**10.5%**
High School Education	**66.7%**	54.4%	**63.2%**	67.5%	**66.7%**
Risk Score					
2	**19.3%**	7.6%	**8.8%**	38.1%	**19.3%**
3	**80.7%**	92.4%	**91.2%**	60.3%	**80.7%**
# Past Arrests	**4.1±0.8**	4.2±0.7	**3.7±0.7**	4.6±0.4	**4.3±0.6**
# Felonies, Nonviolent	**1.6±0.4**	2.6±0.6	**2.2±0.6**	2.6±0.3	**1.3±0.4**
# Felonies, Violent	**1.1±0.4**	1.1±0.3	**1.0±0.4**	0.9±0.1	**1.1±0.3**
Days in Jail	**124±23**	131±16	**132±17**	182±10	**148±21**
Years since Release	**3.10±0.19**	3.41±0.18	**3.45±0.19**	2.71±0.12	**2.94±0.29**

Note Error values represent 5% margin of error

Bolded values are descriptive statistica for the finalpost matched data sets used in later analyses. Non-bolded columns represent pre-matched data

Procedure

All procedures for data collection fell under the purview of secondary data analysis and are exempt from IRB approval. The dataset used did not include any identifying information. Program staff identified eligible participants upon their entry into jail and arranged face-to-face meetings to describe the program in detail and obtained informed consent from all prospective participants that elected to join the program. Each participant's program start date was defined as the day of their release. Upon release from the local jail, participants were assigned a "peer navigator"—a case manager with similar experiences to program participants with regards to past criminal involvement, navigation through the justice system, and the challenges faced upon release.

Peer navigators first worked with participants to develop an individual reentry plan that set goals for housing, employment, and healthcare navigation. Participants would then receive counseling from a licensed provider and meet with their peer navigator on a weekly basis. The peer navigator guided participants on healthcare navigation via linkages to a collaborative Health and Social Service Network (HSSN), which included several workforce development partners within the community, partners for health navigation, and partners for housing assistance programs. Additionally, peer navigators encouraged engagement and participation using evidenced based practice guidelines for motivational interviewing techniques tailored towards offenders to increase change behaviors, engage in available services, and reduce recidivism (Walters et al., 2007).

Successful program completion was defined as participating in program services and maintaining contact with program staff for a minimum of 12 months after release. A participant was only considered to have quit the program if program staff could not make any contact with the participant for at least 3 months. Alternatively, the participant could request to quit the program of their own volition. Participants were not removed from the program in the event of a new arrest, conviction, or jail sentence. This was because there were frequently cases where the length of the reincarceration period did not exceed the 12 months of designated participation time—therefore participants were permitted to resume services upon release provided they either maintained contact or made arrangements with their peer navigator or other program staff. If the reincarceration period occurred before the 9-month mark and lasted for more than 3 months (i.e., after 12 months), the participant was removed from the program and was not included as a "Completer" as defined in the group assignments.

Data, Measures, and Outcomes

For program participants (both completers and quitters), demographics, risk assessment outcomes, contact data, and completion status were all taken from administrative records completed by program staff. Contact data included number of contact hours and contact events both within the first 3 months of the program and the entire 12-month duration of the program. Past felony bookings and recidivism of both program participants and matched controls were pulled from JailView by the Kent County Criminal Justice Planner. Demographics for nonparticipants were also pulled from JailView. Recidivism data for nonparticipants was limited in scope to the same timeframe that the reentry program was active (07/2016–07/2021).

Except for data used in a panel model analysis, all recidivism events were quantified with reference to the date of program intake for participants and initial arrest for nonparticipants. After program intake or the initial arrest, recidivism events for a given individual are counted for a one-year and two-year period. This includes the total number of new convictions, total number of incarceration events, a binary yes/no measure for if the individual received any new convictions, and a binary yes/no measure for if the individual was sentenced to jail time. For panel model data, recidivism events are quantified as running totals with reference to the program start date (07/2016) rather than the intake/booking date. No binary recidivism measures were defined for the panel model.

Statistical Analysis

All analyses were carried out in R (R Core Team, 2022; Version 4.2.1). Recidivism-based outcomes were analyzed through a combination of general linear models (GLMs) and panel regression. Attrition analyses used unmatched data; all other models used matched data. The MDM matching methods described in the Participants section used the MatchIt (Ho et al., 2011) and optmatch (Hansen & Klopfer, 2006) packages. Model creation for GLM analyses made use of the MASS package (Venables & Ripley, 2002) for stepwise selection based on AIC optimization. Panel data regression was carried out using the plm package (Croissant & Millo, 2008), and marginal effects for the final panel models were calculated using the marginaleffects package (Arel-Bundock, 2022). All other analyses made use of base R functions and packages.

Logistic regression tested for the effects of measured demographic variables and criminal history on attrition outcomes among program participants. The initial model included the following input variables: age at the time of first arrest, race/ethnicity, attainment of a high school diploma or equivalent, length of stay in jail, time since release from jail, number of past arrests, number of past violent felonies, number of past nonviolent felonies, risk of recidivism, and number of contact events within the first three months after intake. This full model was reduced down via bidirectional stepwise selection according to the lowest achievable AIC criterion.

Another set of GLM models utilized the matched dataset and focused on the effects of program participation and program completion on recidivism outcomes. These models utilized a $k=3$ factor for completion status (hereafter called *Group*), which could equal either “Completer”, “Quitter”, or “Control”. Control was set as the reference level with separate contrasts being run for completers vs. controls and quitters vs. controls. No other factors were included in the model due to all relevant factors being controlled via the matching criteria. Binary recidivism outcomes used logistic regression models, while a Poisson regression model was used for number of new convictions within one year and two years. No control variables were included as likelihood ratio tests testing their inclusion showed no significant impact on model fit by including them.

As a final means of evaluating recidivism outcomes, a panel dataset was created from five years of recidivism data beginning with program commencement (07/2016–07/2021). This dataset was extrapolated from the matched dataset, where each of the $N=171$ cases were expanded into $N=10{,}260$ rows of data ($n=60$, each row represents one month). The following time-dependent factors were created specifically for the panel analysis: (1) "*Intake*", which for a given case changes from 0 to 1 after they either enter into the program (participants) or are arrested for the first time (non-participants), (2) "*In_Jail*", which equals 1 whenever the subject is currently incarcerated for a given month, and (3) "*COVID*", which changes from 0 to 1 starting on 04/2020 to represent the onset of the COVID-19 pandemic, which disrupted program activities in a major way—especially with regards to contacting program participants. The *Intake* variable was necessary to account the fact that participants joined the program at different times, while the *In_Jail* and *COVID* variables were included as control variables to account for times when the participant either could not recidivate (*In_Jail*) or where conviction/incarceration procedures were different than normal (*COVID*). Two Hausman-Taylor panel models (Hausman & Taylor, 1981) were created for two recidivism measures: number of convictions and number of incarceration events. The input for both models was the same:

$$Recidivism \sim Group^{*}Intake + In\_Jail + COVID \tag{1}$$

The *Group* factor represents the same $k=3$ factor from the matched GLM models with Control participants again set as the reference level. By crossing this term with *Intake*, two distinct interaction terms are created: one where completers are compared against controls for all months after the intake date, and one where quitters are compared against controls for all months after the intake date. Significance testing of these interaction terms represents the main result of interest from the panel models. Marginal means for each group, split by *Intake*, are reported alongside 5% margin of error.

Results

Attrition Analyses

Before investigating whether or not participants that quit the program fare worse than those that completed it, we first sought to understand what factors predicted attrition in the first place. Table 1 shows the pre-match demographic information and criminal history for both completers and quitters, while Table 2 shows the results of the attrition analysis. When the GLM for attrition includes all possible inputs specified in the matching criteria, only two factors significantly predicted successful program completion: fewer past nonviolent felonies, b = -0.212, z = -2.00, p=.046, and increased number of program staff encounters within the first three months after intake, b=0.071, z=2.10, p=.036. Two other factors approached but did not reach significance: higher age at the time of booking, b=0.148, z=1.68, p=.092, and identifying as black rather than a non-Hispanic white man, b = -1.189, z = -1.85, p=.064.

Table 2 Attrition analysis, unmatched GLM

Factor	*b*	*z*	*p*
Full Model (AIC=182.04)			
Age	0.148	1.68	0.092
Race			
Black vs. Hispanic	-0.941	-0.93	0.353
Black vs. White	-1.189	-1.85	0.064
High School Education	0.583	1.43	0.152
Risk Score	-0.954	-1.53	0.127
# of Past Arrests	0.075	1.03	0.304
# of Past Nonviolent Felonies	-0.212	-2.00	0.046*
# of Past Violent Felonies	-0.004	-0.02	0.981
Days in Jail	-0.002	-0.74	0.461
Years since Release	-0.292	-1.05	0.295
Contact in 3 Months	0.071	2.10	0.036
AIC-Optimized Model (AIC=176.26)			
Age	0.14	1.85	0.064
# of Past Nonviolent Felonies	-0.21	-2.26	0.024*
Contact in 3 Months	0.08	2.55	0.011*

*$p<.05$, **$p<.01$, ***$p<.001$

Table 3 Recidivism Outcomes, Matched Raw Counts

Measure	Group	Year 1	Year 2
# of New Convictions	Controls	8	21
	Completers	11	37
	Quitters	29	47
New Conviction, Y/N	Controls	8 (14.0%)	17 (29.8%)
	Completers	9 (15.8%)	23 (40.4%)
	Quitters	20 (35.1%)	25 (43.9%)
Reincarcerated, Y/N	Controls	4 (7.0%)	6 (10.5%)
	Completers	1 (1.8%)	6 (10.5%)
	Quitters	4 (7.0%)	9 (15.8%)

Note Percentages in parentheses represent the value above it as a percentage of the group sample size. One person can have more than one conviction in # of new convictions

When this full model is reduced using AIC optimization, only three factors remain: age, number of nonviolent felonies, and number of staff encounters in the first three months of the program. Coefficients for these three factors minimally changed from the full model, and no changes in significance were observed.

Recidivism Analysis

The recidivism analysis sought to compare recidivism events between groups once all the previously described group differences were accounted for with statistical matching (i.e., MDM). Descriptive statistics for all recidivism measures within the matched dataset are given in Table 3. Recidivism outcomes from the matched GLM models are given in Table 4. Within the first year of program intake or control group admission, the completer group did not differ from the control group in number of new convictions, earning at least one new conviction, or serving jail time. In comparison, the quitter group was convicted at a significantly higher frequency than controls, $b=1.06$, $z=2.90$, $p=.004$, and a significantly higher fraction of the quitter group had been convicted at least once within a year of intake, $b=1.20$, $z=2.54$, $p=.011$. With reference to completers, significantly more quitters earned new convictions than completers, $b = -1.06$, $z = -2.32$, $p=.021$, and the total number of new convictions from quitters significantly exceeded those from completers, $b = -0.97$, $z = -2.74$, $p=.006$. With regards to serving actual jail time, an equal number of controls and quitters were reincarcerated within a year. Only one completer had been reincarcerated compared to four quitters and four controls, but this difference was not significant for either contrast.

Within two years of intake, differences between the three completion groups mostly disappeared. Quitters earned significantly more convictions than controls, $b=0.67$, $z=2.68$, $p=.007$; no significant differences were found between completers and controls. Expressing new convictions as a binary outcome eliminates statistical significance entirely, where neither the completers nor the quitters had significantly higher odds of being convicted at least once compared against the controls. Six completers, six controls, and nine quitters had been reincarcerated within two years. This difference was nonsignificant for all contrasts.

Inputting recidivism data into panel models covering five years of program activity amplified the observed differences from the GLMs that covered only the first two years after program intake. Over a full five-year analysis period, significantly more completers and quitters alike had been convicted than controls after intake (Table 5). This effect was much stronger for quitters, $b=0.537$, $z=17.05$, $p<.001$,

Table 4 Recidivism Outcomes, Matched GLM

Measure	Contrast	Year	*b*	*z*	*p*
# of New Convictions	Quit vs. Compl	1	-0.97	-2.74	**0.006****
		2	-0.24	-1.09	0.276
	Ctrl vs. Compl	1	0.10	0.22	0.827
		2	0.43	1.65	0.099
	Ctrl vs. Quit	1	1.06	2.90	**0.004****
		2	0.67	2.68	**0.007****
New Conviction, Y/N	Quit vs. Compl	1	-1.06	-2.32	**0.021***
		2	-0.14	-0.38	0.704
	Ctrl vs. Compl	1	0.14	0.26	0.793
		2	0.46	1.17	0.240
	Ctrl vs. Quit	1	1.20	2.54	**0.011***
		2	0.61	1.55	0.122
Reincarcerated, Y/N	Quit vs. Compl	1	-1.44	-1.27	0.204
		2	-0.47	-0.83	0.409
	Ctrl vs. Compl	1	-1.44	-1.27	0.204
		2	0.00	0.00	1.000
	Ctrl vs. Quit	1	0.00	0.00	1.000
		2	0.47	0.83	0.409

*$p<.05$, **$p<.01$, ***$p<.001$

Note Compl=program completers, Quit=program quitters, Ctrl=controls. Poisson regression used for # of New Sentences, binomial regression used for Y/N measures

The bolded values are the actual significance values. Bold values additionally denote an asterisk to thereferenced critical values that are mentioned above

Table 5 Recidivism Outcomes, Matched Panel Regression

Factor	b	z	p
New Convictions (R^2_{adj}=0.6178)			
Group			
Control vs. Completers	0.004	0.04	0.579
Control vs. Quitters	0.051	0.53	0.863
Intake	1.126	48.12	<0.001***
In Jail	-0.150	-7.59	<0.001***
COVID	0.475	35.30	<0.001***
Group * Intake			
Control vs. Completers	0.140	4.64	<0.001***
Control vs. Quitters	0.537	17.05	<0.001***
Reincarceration (R^2_{adj}=0.8800)			
Group			
Control vs. Completers	<0.001	-0.01	0.990
Control vs. Quitters	0.044	1.29	0.198
Intake	1.022	136.93	<0.001***
In Jail	0.061	9.72	<0.001***
COVID	0.077	17.90	<0.001***
Group * Intake			
Control vs. Completers	0.004	0.45	0.649
Control vs. Quitters	0.072	7.20	<0.001***

*$p<.05$, **$p<.01$, ***$p<.001$

Note Coefficients reported as <0.001 can be positive or negative; the number represents magnitude in this scenario

than completers, $b=0.140$, $z=4.64$, $p<.001$. Significantly more quitters had been reincarcerated than controls after program intake, $b=0.072$, $z=7.20$, $p<.001$. In contrast, no significant difference in reincarceration rates were observed between completers and controls, $b=0.004$, $z=0.45$, $p=.649$. When results of the conviction model are expressed as marginal means, both completers ($M=0.47\pm0.14$) and controls ($M=0.31\pm0.14$) share overlapping 95% confidence intervals post-intake, but quitters ($M=0.92\pm0.14$) surpass both of the former groups. Marginal means for the reincarceration model show the same pattern, where the confidence interval for quitters ($M=0.21\pm0.06$) again rises above both completers ($M=0.10\pm0.06$) and controls ($M=0.09\pm0.06$) after program intake.

Discussion

Three main conclusions can be drawn from the analysis of this jail reentry program: (1) completing the program was not significantly beneficial to participants, (2) quitting the program was significantly detrimental to participants, and (3) increased peer support significantly mitigated the odds of dropping the program. The implications for the field as a whole are worth consideration. Specifically, the priority of increasing the benefits attained from successfully completing reentry programs needs to be balanced with risk management related to preventing attrition. By designing the program evaluation to consider both recidivism and attrition, findings related to both can be used to establish and refine evidence-based reentry program protocols that benefit a wider scope of prospective participants.

The two different types of recidivism analyses used in this evaluation represent similar yet unique interpretations of program outcomes. The GLM results primarily represent participant-centered recidivism outcomes, where the models reference the first two years after a participant joins the program. The panel models more evenly balance participant-centered and program-centered perspectives, where the first five years of program activity are the frame of reference. This is not to say the panel model does not consider participant-level factors; the inclusion of *Intake* and *Group* factors create contrasts that answer similar questions as the GLMs while allowing for flexibility for including time-variant control factors such as active incarceration status (*In_Jail*) and the COVID-19 pandemic (*COVID*).

The outcomes of this study can roughly be divided into three time periods: one year post-intake, two years post-intake, and five years after the opening of the program. The one-year time period represents the time when completers finished their tenure in the program. At this time, it could be said that the program saw some success in preventing completers from serving jail time, but otherwise our data indicates that the program was ineffective—especially with regards to reducing the number of convictions earned after release. When this is compared against outcomes for similar programs, the results are not surprising. While the program fostered a peer mentorship system that included peer support for reentering participants, there was an equal emphasis on peer navigation—in other words, provisions and referrals of employment, housing, and healthcare. Programs focused on housing and healthcare are limited in abundance, but most have shown to be ineffective in reducing recidivism (Jacobs & Gottlieb, 2020; Leclair et al., 2019; Mallik-Kane & Visher, 2008; Aslim et al., 2021; Hammett et al., 2001). Employment has seen more modest successes in reducing recidivism (Andrews & Bonta, 2016; Duwe, 2015a, b; Graffam et al., 2014; Duwe, 2012), but these effects come with caveats where only specific subgroups may benefit (Zweig et al., 2011) and fade with time (Tripodi et al., 2009). Dissenting conclusions on the benefits of employment exist where reduction of recidivism is either minimal or nonexistent (Cook et al., 2015; Newton et al., 2018). Our results support the notion of nuanced benefits similar to Tripodi et al. (2009), where the second-year GLMs and the marginal means from the panel models suggest that any benefits observed by program completers faded after a year or so. This conclusion should be taken cautiously, however, since the Year 1 comparison between completers and controls approached, but did not achieve, statistical significance.

The key finding that should be emphasized from this evaluation is that participants that quit the reentry program unambiguously faced worse outcomes than both matched controls and program completers. This effect was especially pronounced in the first year, where a significantly higher proportion of program quitters were reconvicted after release than both completers and controls. The reconviction gap between the three groups narrowed in the second year, but within that same timeframe, a higher proportion of quitters found themselves serving more jail time than controls and completers both. By the end of the five-year program period, this difference was significant. These results represent a novel discovery in the literature—most studies have sufficiently shown that program quitters fare worse than completers (e.g., Jewell & Wormith, 2010; Mitchell et al., 2021 ; Olver et al., 2011; Lockwood & Harris, 2013), but faring worse than matched controls is an outcome not readily found in the current literature. This backs the theory proposed by Olver et al. (2011), where quitting the program has consequences beyond just selection bias.

As for how this finding should be interpreted in the context of the current study, any feasible explanation likely ties back to how program quitters were defined in the first place. The most common cause for someone to be removed from the program was loss of contact for at least 3 months; participants explicitly asking to be removed from the program or being jailed for a sentence longer than 3 months at the end of their program participation were much rarer occurrences. Reasons for dropping contact could be related to feelings of shame, where the participant may have felt they were failing at societal reintegration and withdrew from contacting their peer navigator, who they potentially perceived as more successful. This could explain why quitters fared worse than matched controls, where the emotional consequences of failure may have been more poignant than never trying in the first place. Alternatively, contact may have been dropped due to a perceived lack of utility. If a participant felt they were not receiving the benefits of program participation they originally expected, they may have let their participation die off (i.e., no contact) due to them not seeing the program as worth their effort. These same feelings may have also been directed at the peer navigator directly, where participants may have been more likely to blame their direct contact for perceived lack of benefit rather than the program as a whole. This kind of attitude could be more common when the participant and the peer navigator lacked a common background, goals, or interests beyond having a criminal past. These interpretations are difficult to verify given the nature of no-contact attrition, but they are at least worth consideration when trying to identify and ultimately prevent withdrawal from reentry programs—especially within the context of future research studies that may explicitly test these assumptions.

Application of the data yielded by the attrition analyses without overcomplicating the matter would best be accomplished by shifting focus from program effectiveness to program retention. The exact factors that predict attrition may vary depending upon program structure, but in the case of this program, peer support is the one factor within the control of program staff that can prevent attrition. Increased contacts between program staff and participants within the first three months (i.e., before quitters began to drop out) predicted lower odds of attrition. This lines up with the priority of importance for criminogenic needs proposed by Andrews and Bonta (2016), where participants may find more value in social support than financial support. By expanding the program to include both formal and informal protocols and policies directly focused on increasing peer contact hours, future program evaluations may be able to introduce causal research designs that explicitly manipulate peer support as a treatment condition to determine if peer support functions as a protective factor.

The conclusions drawn from this data should not be separated from the context behind it, nor should its limitations be ignored. First and foremost, the program was primarily focused on referrals to financial support services and basic needs. At no point was any sort of behavioral or cognitive therapy provided, as seen in other reentry programs (e.g., Miller & Miller, 2015; Visher et al., 2017). Such a fundamentally different approach to reentry may change the entire dynamic as to what predicts attrition and the effect it has on participants that quit the program. Second, the study may be underpowered given the scarcity of recidivism outcomes like reincarceration, where a higher number of program completers may have been needed to draw more accurate conclusions about the effect of the program or lack thereof. Third, the matching of participants with non-participants was done *post hoc*. While we took care in controlling for a wide variety of factors that could differentiate the two groups, the rigor of such a procedure does not approach that of a randomized-control trial (RCT) with designated controls from the start. Fourth, the COVID-19 pandemic began to peak approximately one or two years before the program ended. While we attempted to account for this event in the panel model, the reality is that capturing all the intricate effects the pandemic had on recidivism outcomes would be an immense project in and of itself. At minimum, the reduced activity of the justice and legal systems during this time could translate to a wider discrepancy between frequency of criminal behavior and formal records of recidivism. Finally, the quasi-experimental design of the study cannot explain causality is the same way a randomized control trial could have. While the matching procedure for the program data against control data was thorough, it's still the case where controls were not explicitly assigned to a non-treatment condition. The conclusions about completers vs. controls and quitters vs. controls may be impacted as a result. All of these limitations should be considered alongside the data presented when determining the generalizability of the reported outcomes.

Conclusion

With this program, we have both identified attrition as a critical issue relevant to any reentry program. Quitting the reentry program predicted worse recidivism outcomes than program completers and controls alike. While it is entirely possible that the effect attrition has may be unique to this program, the reality is that more focus needs to be devoted to measuring attrition to make that conclusion. Compared with recidivism, attrition data is not readily available in a concerning portion of reentry literature. If it were simply the case where quitters found themselves no worse off than before they entered the program, then this would not be such a problem. Our data refutes this idea—quitting the program represents a negative impact worse than having never been in the program to begin with. Until the field can reliably investigate the problem, solutions cannot be reliably assessed. In our case, we found that increased peer navigator contact significantly lowered program attrition, but as stated before, this was not a finding borne of explicit, rigorous experimental design. If an RCT for prisoner or jail reentry focused on preventing attrition and implementing peer support as a main factor of interest, then the benefit to reentry program structures moving forward could be immense. It is our hope that our analysis renews interest in the analysis of program attrition and prompts further investigation so that *all* reentry program participants can be given a fair chance at reform and fulfillment after serving their time in jail, not just the ones that finish the program.

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Declarations

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