A Second Chance at Schooling? Unintended Consequences of Prison Education

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Abstract

Prison education programs are rapidly expanding across the United States, but we have little causal evidence on the impact of prison education. We estimate the impact of prison education on reincarceration, future employment and education, and in-prison misconduct using detailed administrative data from Iowa. To address selection bias, we instrument for courses taken during incarceration with an opportunity metric based on course availability. We find that participating in education increases the likelihood of reincarceration within three years of release. The effect is driven by revocations rather than new crimes and is more pronounced for post-secondary education and among white prisoners. Exploring mechanisms, we find that participating in education increases the likelihood that prisoners are released with significant supervision requirements—likely contributing to increased revocations. Our findings highlight a potential unintended consequence for policymakers and practitioners to consider as prison education programs continue to proliferate.

Keywords: education, prison, incarceration, recidivism, employment

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1 Introduction

In 2024, the United States had nearly 1.8 million people in prison, giving it one of the highest incarceration rates globally—more than three times the world average (Fair and Walmsley, 2024). U.S. prisons also disproportionately house economically vulnerable individuals, including many from low-income backgrounds, with limited education, and with histories of homelessness. This population recidivates at high rates—68% of released prisoners are rearrested within three years (Durose, Cooper and Snyder, 2014)—and recidivism has substantial social costs, estimated at $151,662 per event (Steinfeld, Powers and Saltmarsh, 2018). Despite extensive efforts, reducing recidivism remains a significant public policy challenge (Alsan et al., 2025; Doleac, 2023). One potential solution garnering increased attention is prison education programs, though causal evidence on their impact remains limited. This paper estimates the effect of prison education on prisoner outcomes, including reincarceration, future employment and education, and in-prison misconduct.

The majority of U.S. public (91%) and private (88%) prisons offer educational opportunities (Harlow, 2003). In 1965, the federal government funded college courses in prisons through Pell Grants, but prisoners were made ineligible for these grants in the 1990s.[1] Higher education in prisons remained widely unavailable until the 2015 Second Chance Pell Pilot Program, which allowed 67 schools to operate in more than 100 correctional facilities throughout the U.S. Over the next six years, the pilot program provided an estimated $180 million in funding, with an average award of $4,491 to more than 40,000 incarcerated individuals (Taber and Muralidharan, 2023; Hanson, 2024). In 2023, Pell Grant eligibility was restored for all otherwise-eligible prisoners, a change expected to lead to a surge in college-in-prison programs. Despite the historical lack of availability, prisoner interest in higher education is high. A 2014 survey found that 70% of inmates want to enroll in educational programs

[1] In 1992, Pell Grant eligibility was removed for prisoners on death row and for those serving a life sentence without parole. In 1994, the Violent Crime and Law Enforcement Act banned all prisoners from receiving these grants.

while incarcerated and a "majority of incarcerated people are academically eligible for post-secondary-level courses" (Rampey et al., 2016; Oakford et al., 2019).

There are two key reasons for the lack of causal evidence in this space: limited data and selection bias. To overcome the typical data challenges, we executed a data use agreement with the Iowa Department of Corrections, the Iowa Department of Education, Iowa Workforce Development (IowaWORKS), and Grinnell College, enabling us to create a unique comprehensive, individual-level data set of people released from an Iowa prison between 2014 and 2018. Crucially, our data include every Iowa community college or Grinnell College class taken by individuals in our sample. Our data have many advantages, but two noteworthy limitations. First, we only observe records for individuals released from Iowa prisons between 2014 and 2018 across all of our datasets, which limits some of our analysis. For example, our education data only cover these individuals, so we do not have information on courses that were offered in which they did not participate or accurate counts of the number of students in each course. Second, we do not observe sentence length as determined by the judge in our data, instead our data only include actual time served. Actual time served can be affected by actions taken while incarcerated, though participating in prison education should not directly affect time served based on IDOC policy. We include robustness exercises to assuage concerns regarding prison education impacting time served. These data, even with their limitations, allow us to make progress in an important and understudied area.

To address potential selection bias from individuals opting into education, we employ an instrumental variable approach. Specifically, we calculate each individual's opportunity to participate based on the number of courses that started while they were incarcerated. We use this opportunity score to instrument for actual courses taken. Variation in opportunities to take courses arises from three factors: 1) the prison(s) in which the individual served, 2) the amount of time the individual was incarcerated, 3) the precise timing of the individual's entrance into and release from each prison. Some prisons in our sample had more course offerings than others. Furthermore, individuals who served more time in prison or who

were released later in our sample period had greater opportunity to take courses. Finally, because in-prison course offerings in our setting largely follow the academic calendar and IDOC policy typically restricts inmates from enrolling in courses once they have begun, individuals serving approximately the same amount of time can have different opportunities to take courses based on the timing of their entry and exit from each prison. Courses within each prison tend to start within the same week, meaning that individuals who enter a prison just before or after will experience different opportunities to take courses while incarcerated. To isolate random variation in time of entry, we include primary prison, release year, and time served controls in all specifications. With these controls in place, our identification relies on variation in the precise timing of when an individual enters a specific prison, either at the beginning their sentence or because of a transfer across prisons. We show that the educational opportunity instrument is predictive of the number of courses taken, but is uncorrelated with prisoner characteristics.

Our results suggest that participating in prison education increases the likelihood of reincarceration within three years of release, though the result is driven by revocations (i.e., violating the terms of release) rather than by new crimes. We show that our results primarily hold for post-secondary courses and among white individuals. Examining other outcomes, we find that prison education significantly increases the likelihood of employment, but does not affect average quarterly earnings, the number of quarters worked, or participation in community college courses within three years of release.

Additional analysis suggests that it is unlikely that the increase in reincarceration is a direct effect of participation education. Instead, we find that participating in education affects how individuals are released, decreasing the likelihood of being released free and clear and increasing the likelihood of being assigned to work release. This represents a shift towards more intensive post-release supervision, and work release in Iowa dramatically increases the probability of reincarceration, mostly due to an increased probability of revocation (Lee, 2023). Mediation analysis, similar to (Finkelstein et al., 2024), using a reduced-form model,

shows that education has no impact on reincarceration when we control for release type. Thus, in our context, participation in education programs has an unintended consequence—causing individuals to be released into higher levels of post-release supervision, which likely mediates our effects on reincarceration.

Because the way individuals are released appears to be driving our results, we briefly explore why participation in education might affect release type. We hypothesize that this largely operates through the release recommendations of case managers, which, in Iowa, are almost always followed by the Parole Board (Lee, 2023). Two factors that might shed light on why case managers may increasingly recommend work release for participants in education are that education, particularly post-secondary education, is generally viewed favorably, and that work release is not generally viewed disfavorably and can be thought of as providing stable housing to an individual who would not otherwise have it following release. While we do not have ideal data to study the elements that determine assignment to different levels of post-release supervision, we can explore whether education affects in-prison misconduct. Misconduct is an important consideration when case managers make these release recommendations.

Using our preferred 2SLS model, we find that individuals participating in education have significantly fewer violent in-prison misconducts, but this results masks important temporal variation. Using an event-study design, we show that after starting their first course, individuals commit more in-prison misconduct, particularly drug and property misconduct. This may be partially due to increased opportunity (i.e., more movement around the prison for courses). In addition, anecdotal evidence suggests that correctional officers, who typically do not have a college education, are opposed to incarcerated people being given opportunities that the officers do not feel they have been afforded. In some cases, this may lead correctional officers to target students, and particularly students in post-secondary classes, with increased misconduct citations. This increase in misconduct may be contributing to being assigned to work release, as misconduct may be interpreted as indicating a need for addi-

tional programming (e.g., drug or alcohol treatment programs), which may be most easily facilitated in a work release facility, or that the individual remains a danger to society.

Our central contribution is to the literature examining the effect of prison education on prisoner outcomes. While studies show that improved education reduces criminal activity (Lochner and Moretti, 2004), the impact of prison education on future offending remains an open question. Few causal studies address this question, instead previous work often directly compares prisoners who did and did not take courses, failing to properly account for selection bias (Linden and Perry, 1983).

More recent causal studies suggest that educational programming, paired with other intensive services and cultural shifts toward rehabilitation, can reduce recidivism (Arbour, Lacroix and Marchand, 2024; Alsan et al., 2025; Totarelli, 2024; Bhuller, Khoury and Løken, 2025). Specifically, Alsan et al. (2025) show that the"IGNITE" education program in the Genesee county jail reduced misconduct and 3-month recidivism. Similarly, Arbour, Lacroix and Marchand (2024) show that targeted educational programs in Canadian prisons decreased recidivism. While these results are encouraging, the programs considered in these studies include targeted educational programming paired with a variety of interventions. In practice, these additional interventions are not typically present in U.S. prisons. Instead, most prison education programs are designed to mirror education outside of prison (e.g., a course transplanted from a local community college), with little in the way of additional support. As such, our paper is one of the first to offer a causal analysis of the impact of education as it is typically offered in U.S. prisons. Our rich administrative data also give us a unique ability to examine not only recidivism, but also other critical outcomes that are usually challenging for researchers to assess, including in-prison misconduct and key post-release labor market outcomes such as education, employment, and wages.

In addition to contributing to the literature on prison education, we build on a substantial body of research highlighting how unintended consequences may significantly alter the social impact of incarceration- or education-related policies (Agan and Starr, 2018; Johnson and

Raphael, 2009; Charles and Luoh, 2010; Doleac and Hansen, 2020; Visaria et al., 2016; Figlio and Ozek, 2024; Barrow, Sartain and De La Torre, 2020). Our results highlight the importance of considering how institutional agents, in our case corrections staff, may respond to interventions and whether these behavioral responses may undermine an intervention's intended effect.

The paper proceeds as follows. In Section 2, we detail Iowa's correctional system and the education offered to incarcerated people in Iowa. We describe our data in Section 3. Section 4 presents our empirical strategy. Section 5 presents our results. We offer a discussion of our findings and concluding thoughts in Sections 6 and 7.

2 Background

Prison Education Nationally and in Iowa. Prison education in Iowa is similar in many ways to prison education in the country as a whole. Nationally, at least 90% of state prisons and all federal prisons provide educational programs to their inmates with secondary and remedial education programs focused on preparing students for a GED being the most common offering (Harlow, 2003). In addition, most (68%) post-secondary prison education is provided through partnerships with community colleges nationally (Erisman and Contardo, 2005).[2] It often involves students taking courses without much in the way of additional support beyond the classroom because this allows education to be offered more cheaply than in other settings (Stickle and Sprick Schuster, 2023; Wood, 2023).

In Iowa, all prison education is offered through local colleges, primarily community colleges.[3] Both in Iowa and nationally, the expectation is that, to the extent that schools offer the same courses outside of prison, the prison version is as similar as possible to the non-prison version, within prison guidelines (Long, 2023; Wood, 2023). The specific courses

[2]This number has likely increased with more than 40,000 students participating in post-secondary education funded by Second Chance Pell Grants between 2016 and 2022 (Taber and Muralidharan, 2023).

[3]Specifically, Des Moines Area Community College, Grinnell College, Iowa Central Community College, Iowa Western Community College, Kirkwood Community College, Southeastern Community College, and University of Iowa offer courses in Iowa prisons.

available vary widely across prisons and over time. For example, college-level education became available in Iowa for the first time since the 1990s in 2003 when Grinnell College began its Liberal Arts In Prison Program in Newton Correctional Facility. After the reintroduction of Second Chance Pell Grants in 2016, community colleges expanded their course offerings in prisons.[4] We provide summary statistics on the types of courses available in Iowa prisons between 2014 and 2018 and the frequency with which they are taken in Appendix Table A1. Of particular note, and consistent with nationwide statistics, is that 80% of courses offered during this period were either HSE or remedial courses. Moreover, the median number of courses taken by incarcerated people, conditional on taking at least one course, is two. The low number of courses taken, even among people actively taking courses, makes it somewhat less likely that we will find sizable effects of education. In addition, because it was rare for students to take many courses while incarcerated during our sample window, we see very few instances of individuals earning degrees. Less than 4% of our sample earn any type of degree, diploma, or certificate during their incarceration—this number falls to less than 1% if we only consider those who earned degrees.[5,6]

Prison Education and Prison Requirements in Iowa. Every new prisoner in Iowa is first sent to the Iowa Medical and Classification Center (IMCC). Typically, prisoners spend 1-2 months at IMCC and during this period they are evaluated for health concerns, risk level, and programming needs. They are then transferred to another longer-term prison. Prison education has no impact on long-term prison assignment. Instead, individuals are assigned to facilities based on a clear hierarchy that considers: 1) available space, 2) the presence of known accomplices or enemies in the available prisons, 3) the security level of each prison, 4) the prison that can provide the individual's health and programming needs, and 5) the prison

[4]For a discussion of Pell Grant eligibility among prisoners, see Tahamont et al. (2022).

[5]Most courses in our data are offered in person. Unfortunately, we cannot determine which courses are in person and which are online.

[6]The low rate of earned degrees in Iowa is also consistent with national estimates. In 2019, only 2% of Federal prisoners incarcerated earned a GED (Carson, 2021).

closest to the individual's convicting jurisdiction.[7] While prisoners are allowed to request transfers to specific prisons, those transfers are rarely granted and access to education is not considered a valid reason for transferring. In fact, prison officials estimate that only 5% of inmates ever change prison based on their own request (Lee, 2019). Instead, most individual-level movement across prisons results from a change in one of the five factors listed above. Prison transfers, then, offer an excellent source of variation in the opportunities that each individual will have to participate in prison education.

Additionally, in Iowa, incarcerated individuals must either work or attend GED courses. Refusal to work is a class C violation punishable with up to 30 days of disciplinary detention (Savala, 2006). Prisoners can earn 1.2 days off their sentence for every day they participate in either work or education. In practice, almost all prisoners fulfill the work or school requirement and earn the maximum number of possible days. According to IDOC officials, upon entering prison, individuals are given an estimated released date that assumes the maximum earned time, and time is only added back on to an individual's sentence if they commit misconduct. An important implication of this system is that participation in education or any additional programming does not in itself impact release timing. That is, all else equal, a person who participates in additional education, beyond what is required by the prison, should spend the same amount of time incarcerated as a person who does not participate in education.

3 Data

This project combines data from four unique data sources obtained through a research agreement. First, administrative data from the Iowa Department of Corrections (IDOC) provides information on every person released from an Iowa prison between January 1, 2014 and December 31, 2018. During this 5-year period, we observe key information for each

[7]The IDOC only considers court-mandated treatment programs as programming needs when determining prison assignments; prison education is not considered required programming.

individual including all incarceration spells, their daily prison location, the number and type of misconducts committed, education levels, and demographic information. We also observe all incarceration spells before 2014 or after 2018 and up until July 2022, though we have less information about these spells relative to those in our primary analysis period.

Second, administrative data from the Iowa Department of Education (IDOE) provide individual-level information on every course taken by individuals in our sample from the IDOC. The education data range from January 1, 2011 through December 31, 2022 and provides information including the name and subject matter of the course, whether the course was credit bearing, whether the individual earned any degrees, credentials, or certifications, and whether the student passed the course.[8,9] Because all education in prison is offered by local colleges, our data capture nearly all educational opportunities for incarcerated people during this period.[10] Third, we worked with the Grinnell College Liberal Arts in Prison Program to gather data on every person who had taken a course in that program since 2011. Collectively, these data represent the vast majority of college-in-prison opportunities for inmates during our study period.

Finally, we incorporate data on labor force participation and wages from Iowa Workforce

[8]One important caveat with this data is that there is no indication of whether the course took place inside of a prison. Because there is no cross-registration (i.e., no course has both incarcerated and non-incarcerated individuals enrolled), we match the IDs of students in courses to the IDs of current prisoners to determine which courses occur inside prisons between 2014 and 2018. Unfortunately, we do not have complete records of individuals in prison. Instead, we see only those people who were released from prison between 2014 and 2018. Because of this, we are unable to determine whether courses observed in our data before 2014 or after 2018 were offered at a prison. Specifically, if a course was offered in prison, but was taken exclusively by incarcerated people who were not released from between 2014 and 2018, we would not include the course as having been offered in a prison. This also makes it impossible for us to count the number of students in each class as people who are not released before 2018 will be missing from these counts, even if they were enrolled in the class.

[9]We do not observe unique class codes in cases where multiple sections of the same class were offered at the same time in the same prison. To prevent our results being biased by extremely large courses, we assume that classes which appear to have more students than the official course cap of 28 are actually multiple sections of the same course. For example, if we observe 75 students starting the same course in the same prison on the same day, we assume that there were actually three distinct sections of this course and count it as three courses starting on that day.

[10]In 2018, The University of Iowa offered college courses to approximately 130 individuals incarcerated in the Iowa Medical and Classification Center (IMCC). Because we were not able to obtain data from the University of Iowa, we are not able to observe these classes or determine which students were enrolled in them. To ensure students taking courses from the University of Iowa do not bias our results, we drop all individuals released in 2018 who spent the majority of their sentence in IMCC.

Development (IowaWORKS). Due to strict data provision requirements at IowaWORKS, we can only obtain cohort-level data. We describe the process for creating the cohorts, which uses a similar algorithm to that developed by Autor et al. (2016), in the Data Appendix. We were conservative with our choice of cohort sizes because IowaWORKS suppresses all data for any cohorts with fewer than three matched individuals. Moreover, because of the resource investment required to match these cohorts and attach the appropriate information, we only had one opportunity to request labor market information.

All data were sent to the IDOE where it was matched based on social security numbers. The dataset we received included 25,989 prisoner-stints where a prisoner-stint is defined as one complete period of incarceration for one person. From this, we drop all prisoner-stints where the person could not be reincarcerated in Iowa because they either died in prison or were released out of state.[11] We also drop prisoner-stints released from IMCC in 2018 as previously discussed, and individuals who cannot be included in our analysis because they are missing crucial information. We are left with 22,478 observations. Among this sample, we observe nearly 4,000 individual-course observations. Approximately 17% of incarcerated individuals in our sample took at least one course.

In Table 1, we show summary statistics for our exogenous variables. We provide means for incarcerated individuals who did not take any in-prison courses in the first column and for individuals who took at least one in-prison course in the second column. The third column shows the difference between the first and second columns. We see clear evidence that individuals who did and did not take in-prison courses are different on a number of important margins. Given these differences, and the unobservable differences that likely accompany them, any direct comparison of these two groups likely suffers from selection

[11] Of course, it is possible that individuals released in Iowa leave the state, which would prevent them from being reincarcerated, working, or pursuing additional education in our data. We do not believe this is a significant concern for two reasons. First, more than two-thirds of prisoners released in Iowa are released to either parole or work release. These prisoners are given explicit instructions to remain in Iowa. Second, even if some individuals are leaving Iowa shortly after their release from prison, this will not bias our estimates unless individuals who participated in prison education are systematically more or less likely to leave the state than other recently released individuals.

bias. Importantly, as shown in Column 4, some of these differences persist after controlling for primary prison, release year, and time served, suggesting that reduced-form regression analysis is unlikely to return unbiased estimates of the impact of education.

Outcome Variables. We consider a variety of outcome variables, which we describe below.

Reincarceration. Perhaps the most obvious outcome of interest is reincarceration, i.e., the likelihood of returning to prison after release. We observe prison admissions through July 2022 in our data. This allows us to calculate a 3-year reincarceration rate for every person in our primary analysis sample. We are also able to divide reincarceration into new crimes and revocations.[12]

Employment and Education. Prison education may influence labor market participation after release. While we received cohort-level data from IowaWORKS, we assign cohort values to each cohort member and run all regressions at the individual level. Specifically, we observe the number of quarters worked and average quarterly wages in the three years after release. We also consider the impact of prison education on future education. To do this, we count community college course enrollment within three years of release from prison.[13]

In-Prison Misconduct. Our final outcome variable measures within facility misconduct. The IDOC data include a record of every misconduct incident reported by correctional officers. Each report includes a date and a description of the incident, which we categorize as either violent, drug, order, or property offenses.[14] One particular advantage of misconducts relative to the other outcome variables is that we observe misconducts on each day of a

[12]Revocations occur when an individual violates the terms of their release but are not prosecuted for a new crime. Instead, they return to prison to serve the remainder of their original sentence. Revocations and new crimes are not mutually exclusive. A person can violate the terms of their release by committing a new crime. Anecdotes from parole officers suggest that new crimes are sometimes handled as revocations in an effort to avoid the costs associated with a new trial and conviction.

[13]Recall, we only observe community college education. If prison education prepares students to pursue a four-year or graduate degree after release, we would not observe that effect. However, the low number of degrees awarded makes it unlikely that a significant number of individuals in our sample are pursuing four-year or higher degrees after release.

[14]Order offenses are comprised of incidents in which an inmate was either not where they were supposed to be or engaged in behaviors that the officer felt were disrespectful.

person's sentence. This allows us to leverage variation both within and across individuals when using misconducts as an outcome.

4 Empirical Model

An obvious challenge in evaluating the impacts of prison education is selection bias. As demonstrated in Table 1, individuals who take classes while incarcerated are observably different than individuals who do not. More concerning, individuals enrolling in education may differ from the general prison population in unobservable ways, including increased motivation to positively change the trajectory of their lives. If these unobservable differences are sufficiently large and important, a comparison of outcomes between individuals who take courses while incarcerated and those who do not will be biased even after controlling for observable factors.

To overcome this challenge, we create a unique opportunity metric that measures the availability of prison courses and use this opportunity metric to instrument for courses taken. Specifically, for each day in each prison, we count the number of courses that started. We then sum these courses across each individual's incarceration. This gives us an educational opportunity score for each person during each period of incarceration. To facilitate interpretation, we scale this opportunity score by its standard deviation.

With the opportunity metric defined, we estimate the impact of prison education during incarceration on our outcomes using a two stage least squares (2SLS) model. In the first stage, we estimate:

$$Education_i = \beta_0 + \beta_1 * Opportunity_i + X_i^{'}\Gamma + \lambda_p + \lambda_y + \lambda_l + \epsilon_i \tag{1}$$

where $Opportunity_i$ is the cumulative opportunity for prisoner i during a specific stint in prison and $X_i^{'}\Gamma$ is a vector of control variables, including all variables displayed in Table 1. λ_p, λ_y, and λ_l are fixed effects for primary prison, release year, and time served (in 3-month

bins), respectively.[15]

We then estimate the second stage as:

$$Outcome_i = \alpha_0 + \alpha_1 \widehat{Education}_i + X_i' K + \sigma_p + \sigma_y + \sigma_j + \upsilon_i \quad (2)$$

In both stages, we allow for standard errors to be clustered by the primary prison for each individual interacted with that person's year of release.

Opportunity varies based on a variety of factors. Panels A-C of Figure A3 visually illustrate average opportunity scores by primary prison, release year, and time served. As expected, we find that all three of these factors create significant variation in educational opportunities. On the other hand, time served, primary prison, and release year may be problematically correlated with prisoner characteristics. In order to isolate variation in opportunity that is uncorrelated with prisoner characteristics, we include fixed effects for time served (in 3-month bins), release year, and primary prison in all of our primary specifications. We refer to these collectively as "randomization controls".[16]

One might be concerned that time served (and for related reasons, release year) is endogenous to prison education. For example, one might suspect that education could be viewed favorably and lead people to be released earlier than they otherwise would have been. Moreover, time served is mathematically correlated with both education (i.e., course taking) and educational opportunities because increased time in prison can only increase an individual's opportunity to take courses, particularly for individuals serving in later periods of our sample. Unfortunately, we cannot directly test for this possibility because we do not observe sentence length, only the amount of time they actually served. However, we do not believe that education is problematically correlated with time served for a variety of reasons. First, in conversations with the IDOC, we were explicitly and repeatedly told that participation

[15]In addition to absorbing significant variation in course opportunities, release year fixed effects also help us control for the impacts of the COVID-19 pandemic which may have influenced 3-year recidivism rates and other outcomes for individuals released in 2018.

[16]Time served in our data has right-skewed distribution. To avoid having time served bins with few observations, we group all individuals who served more than five years into a single bin.

in education would not impact release timing. As discussed in the background section, each prisoner's expected release date is calculated at entry assuming that the individual earns all possible time off of their sentence for good behavior and, in practice, time is only added back on for misconduct. Furthermore, even if education directly influenced time served at the margin, we do not expect this effect to be large enough to change an individual's 3-month time served bin. Additionally, as we further discuss in the results section, our results are robust to alternative choices for time served bin size. If participation in education meaningfully reduces time served, we note that this would not invalidate the excludability of educational opportunity as an instrument, because opportunity would still only impact outcomes through education. Instead, this would represent another mechanism or unintended consequence of education, that participation in education leads to earlier released, which, in turn, increases the likelihood of reincarceration.[17] Second, we find directionally consistent estimates of the relationship between participation in education and reincarceration using OLS models with no controls, including no controls for time served.

With the randomization controls in place, we are comparing within individuals in the same prison who were released in the same year and who served a similar amount of time. The remaining variation, then, is based on the precise timing of either prison entry or transfers across prisons. This strategy works in part because of the bunching that occurs in the timing of classes and the policy that inmates cannot enroll in courses after they have started. In Figure 2, we plot the number of courses starting during each week of the calendar year. Because courses are offered through local colleges that follow an academic calendar, most courses start during a few specific weeks of the year. As such, an individual who enters just before many courses begin in his prison will have a much higher opportunity to take courses than an otherwise similar individual who enters a few weeks later.[18] Similarly, an

[17]There is strong evidence that increased time in prison causes reductions in future reincarceration (Kuziemko, 2007; Macdonald, 2024).

[18]This variation also depends on individuals being able to start courses shortly after entering a prison. In Appendix Figure A1, we plot the number of days between when an individual first enters a prison and when they begin their first class. The evidence suggests that students are able to start courses as soon as they enter the prison.

individual who transfers across prisons shortly before a new academic semester begins, will have different opportunity to take courses than a similar individual who transfers later or who does not transfer at all.

In order to serve as a valid instrument, our educational opportunity metric must be relevant, monotonic, and excludable. Relevance implies that educational opportunities must be correlated with actual course participation. The relationship between educational opportunity and actual courses taken is shown in Figure 3. In Figure 3, the light gray bars indicate the distribution of opportunity scores in our sample. The figure omits opportunity scores of exactly 0 as a large fraction of our sample had no opportunity to take courses while incarcerated. As expected, no individuals with an opportunity score of 0 took a course while incarcerated.[19] The solid line in Figure 3 plots a local polynomial based on the average number of courses taken by people with the indicated opportunity score while the dashed lines indicate the 95% confidence interval. As should be expected, we find that increasing opportunities to take courses leads to large and significant increases in courses taken.

To violate monotonicity, some individuals would have to take fewer courses as their opportunities increased which seems unlikely. Because there is no reason why increased opportunities should reduce courses taken, the monotonicity assumption likely holds.[20] Excludability implies that educational opportunities must not be correlated with individual outcomes except through their impact on course participation. We provide some evidence of excludability in Columns 4 and 5 of Table 1. Here, we estimate multivariate regressions using courses taken (Column 4) and our opportunity metric (Column 5) as the dependent

[19] In Figure 3, we also trim observations with the opportunity score greater than 4. Appendix Figure A2 presents an untrimmed version.

[20] We report the results of the joint test of excludability and monotonicity proposed in Frandsen, Lefgren and Leslie (2023) in Appendix Table A2. Because opportunity is conditionally random only within groups of prison, release year, and time served, we include our randomization controls in all specifications. We see no evidence of violations of either monotonicity or excludability using this test. While this test was designed with judge IV papers in mind, the test can be generalized to our setting. In our case, the unique opportunity score values correspond to the unique propensities of each judge. In performing this test, the researcher chooses how much weight to put on the slope and fit components of the tests. We use a fit weight of one as Frandsen, Lefgren and Leslie (2023) finds that this maximizes test power in cases where the number of judges is greater than 20. Using a fit weight below one causes test statistics to decrease and p-values to increase.

variables and the variables listed in Table 1 as the independent variables. We also include the randomization controls. As in the rest of the paper, we allow for robust standard errors to be clustered at the prison by release year level.

Whereas many of the variables in Column 4 significantly predict courses taken, few variables in Column 5 predict educational opportunities. More importantly, the joint F-test reported at the bottom of Column 5 indicates that, collectively, the indicated variables do not predict the opportunity metric. This is encouraging as it suggests that prisoner traits are not driving opportunities to take courses during incarceration. This is perhaps unsurprising given that the timing and location of college-in-prison programs is not a reflection of prison conditions as colleges simply serve prisons that are geographically close by.[21]

A reasonable concern with the excludability of the education opportunity metric is that increased educational opportunities may change outcomes for individuals who do not participate in courses through peer effects. Specifically, if my peers participate in education while I do not, I may benefit from their education indirectly. Some of this concern can be addressed by looking at longer-term or post-release outcomes, which may be less affected.[22] We also look for evidence of peer effects in Table 6 and find no evidence that individuals are affected by their peers participating in classes.

5 Results

Before estimating our primary model, we first visually examine the relationship between education and reincarceration. In Figure 4, we plot the OLS coefficient estimates and 95% confidence intervals using robust standard errors from regressing reincarceration on courses taken using individuals who did not take courses while incarcerated as the omitted category.

[21] As an additional test, we also used individuals who served multiple stints in our sample to estimate whether opportunities to participate in education in a previous stint predict opportunities in the current stint. Among these cases, we find no evidence that previous opportunities impact future opportunities (f=0.13 and p =0.72).

[22] The existing literature on prison peer effects has broadly found no evidence that peer effects induce crime in new areas and instead argue that, if anything, peer effects only serve to reinforce criminal capital within certain types of crime (Bayer, Hjalmarsson and Pozen, 2009; Damm and Gorinas, 2020).

Even if education had no direct effect on reincarceration, we might expect Figure 4 to display negative estimates due to selection bias among individuals choosing to take courses. Instead, most of the estimates suggest that participating in education significantly increases reincarceration, with 4 of the 6 estimates significant at the 5% level.

We report our main results in Table 2. Columns 1 and 2 present OLS estimates of the impact of courses taken with and without control variables. Columns 3 and 4 show reduced form estimates of the impact of educational opportunity with and without control variables. Finally, in Columns 5 and 6, we display results from the 2SLS model, which uses equations (1) and (2) to estimate the marginal impact of an additional course on 3-year recidivism rates. We report the Kleibergen-Paap F statistic for the relevance of our instrument. Here, and throughout the paper, we observe high and significant F statistic values that suggest our instrument is strongly predictive of courses taken (Kleibergen and Paap, 2006).

In Panel 1 of Table 2, we show results for any return to prison. Across all six columns, we find that participating in education significantly increases the likelihood of reincarceration within three years of release. As expected, we estimate larger increases in reincarceration when using the instrumental variable strategy. This is consistent with positive selection into education (and thus these individuals being less likely to be reincarcerated, all else equal). Our preferred model is in Column 6 and includes a robust set of controls. This model suggests that taking one additional course increases the probability that an individual will recidivate by 2.2 percentage points or 5%. However, the result is only marginally significant.

There are two reasons an individual may be reincarcerated. Individuals may be convicted of new crimes; or they may violate the terms of their release and be revoked to prison, even without committing a new crime. In the remainder of Table 2, we show results for the different sources of reincarceration: revocations in Panel B and new crimes in Panel C. This breakout shows that our effects are driven by revocations rather than new crimes. In our preferred model, we estimate that taking one additional course increases the likelihood of revocation by 3.2 percentage points or 9.5%. Our estimates on new crime reincarceration,

while positive, are much smaller and are not statistically significant.[23]

5.1 Other Post-Release Outcomes

In this section, we explore whether prison education impacts other post-release outcomes beyond reincarceration, including job attainment and retention, wages, and future education, though some of these may be mechanically induced given our findings on incarceration above.

We first estimate the impact of prison education on employment after release in Table 3. Specifically, we estimate employee outcomes including any employment, number of quarters with work, and average quarterly wages in the three years after release. We find that individuals participating in prison education are more likely to work at some point within three years of release. Our point estimate suggests that taking one additional course increases the likelihood of employment by 3.1 percentage points or 4%. On the other hand, we do not find significant gains in the number of quarters worked or quarterly earnings.

We next estimate the impact of prison education on education after release and present results in Table 4. The results suggest that prison education does not significantly influence participation in community college education within three years of release. Our negative point estimates might suggest that people who take courses while incarcerated are less likely to pursue education after release, all else equal.

5.2 Heterogeneity

Iowa prisons are more racially diverse than the state as a whole. For example, 23% of inmates in our sample are Black despite the fact that Black people make up just 4.3% of Iowa's population. Given the overrepresentation of minority groups in Iowa prisons, it is important to understand whether they are differentially impacted by prison policies and opportunities, including education. In Panel A of Table 5, we estimate our recidivism results

[23]These results are robust to a variety of other specifications. In Appendix Table A3, we show that our results are similar when using 1-month or 6-month bins for time served instead of the 3-month bins we use in our preferred specification.

separately for white and non-white individuals. We find that our estimated effects for both reincarceration and revocations are driven by white inmates. The results for white inmates are not only statistically significant, they are also much larger in magnitude.

So far, our analysis has lumped together all courses offered in Iowa prisons. In practice, there may be significant differences in the impacts of different types of education. For example, it is reasonable to anticipate that HSE or remedial education might impact inmates differently than post-secondary education. In Panel B of Table 5, we present results for our instrument defined for HSE or remedial courses (Columns 1-3) and post-secondary courses (Columns 4-6). With this breakout, we see that the increase in both reincarceration overall and revocations specifically is driven by participation in post-secondary education.[24]

5.3 Placebo Test

A reasonable concern is that peer effects are impacting our results. That our results seem to be driven by post-secondary course taking helps us to provide a useful test for this possibility. In Table 6, we separately estimate effects for individuals who did and did not have 12 years of education upon entering prison. The idea here is that individuals with less than 12 years of education will not be able to take post-secondary courses. Therefore, any estimated effects of post-secondary courses for this group likely reflects peer effects.[25]

In Panel A of Table 6, we find no evidence that taking HSE or remedial courses impacts individuals who had at least 12 years of school prior to incarceration. Similarly, in Panel B, we do not significant effects of post-secondary courses on outcomes for individuals who entered prison with fewer than 12 years of education. Moreover, we note that the first stage F-statistic for this group is quite low, implying that increased opportunities do not actually lead to increased participation in post-secondary courses among this group. These

[24]While it would be interesting to explore whether particular types of courses (e.g., vocational, liberal arts) are important to our estimated effects, because post-secondary courses make up a relatively small portion of total courses, we are underpowered to further explore subdivisions of these courses.

[25]We also test whether individuals with 12 years of education are impacted by HSE or remedial courses although expectations for this test are less clear. Individuals with 12 or more years of education may still take and benefit from remedial coursework, particularly in preparation for taking post-secondary courses.

findings are reinforced by Appendix Table A4, which replicates Table 6 using the reduced form model. Overall, Tables 6 and A4 suggest that there are not large spillover effects (from peers or otherwise) when educational opportunities increase in our context.

5.4 Mechanisms: Exploring the Increase in Reincarceration

Our results so far pose an important question. Why is participation in education increasing reincarceration? Our finding that the effects are driven by revocations rather than new crimes provide a hint to one possible explanation. A growing literature demonstrates that prisoners released into higher levels of supervision are more likely to be reincarcerated (Sakoda, 2023; Arbour and Marchand, 2024). Particularly relevant to this study is Lee (2023), who studies Iowa specifically, finding that individuals released into work release (Iowa's version of a halfway house), were more likely to return to incarceration than those released to parole. Lee (2023) argues that the strict rules and intensive supervision associated with work release create many opportunities for individuals to make mistakes that either would go unnoticed or not be considered violations if the individual was on parole. As in this study, the increase was driven by revocations. Therefore, one possible explanation is that education in our setting influences the way that people are released from prison.

We begin with visual evidence in Figure 5 where we plot OLS estimates showing the relationship between courses taken during incarceration and the probability of being released to work release, parole, or free.[26] We see clear evidence that taking courses while incarcerated is negatively correlated with the likelihood of being released without post-release supervision. To confirm this finding, we re-estimate our preferred specification using release type as outcomes. Specifically, Columns 1–3 of Table 7 present the change in a person's probability of being released free, to parole, or to work release, respectively. We find that participating in education increases the likelihood of assignment to work release by 4.9 percentage points

[26]The "free" category in this case implies release without any form of post-release supervision.

or 14.1%.[27] Furthermore, our finding that education impacts release type is also consistent with our employment results. One of the key aspects of work release is that individuals are required to be actively seeking employment and are given additional support to do so. This may partially explain our finding of increased employment match probability without effects on margins of employment.

With the relationship between participation in education and release type established, we next turn to an analysis of the extent to which release type is factoring into our estimated effects on reincarceration and revocation. Our first exercise is a mediation analysis, similar to that presented in Finkelstein et al. (2024), in which we directly control for the confounding variable. In Table 8, we present reduced form results with and without controls for release type. When controlling for release type, we find no evidence that education influences any form of reincarceration. Moreover, the inclusion of these controls significantly reduces the magnitude of the estimated impact of education.

Another way to explore whether the increases we observe in reincarceration and revocations are driven by an increase in the probability of being released to work release is to compare our estimates with Lee (2023). Lee finds that assignment to work release instead of parole increases an individual's probability of reincarceration or revocation within three years by 8.2 and 14.4 percentage points, respectively. These estimates imply that the shift to work release in our setting explains 53% and 64% of our estimated effects on reincarceration and revocation, respectively.[28] We take this as further evidence that our estimated effects largely come from education changing release type.

[27] One way to interpret these findings is substitution from no post-release supervision to work release. However, direct substitution from the least to most intensive form of supervision seems unlikely. Our interpretation of these results is a general shift toward slightly more intensive post-release supervision. That is, some individuals were moved from no post-release supervision to parole and others from parole to work release.

[28] This is likely a conservative estimate because it assumes that substitution along the extensive margin has the same effect as intensive-margin substitution, i.e., going from no supervision to any level of supervision has the same impact as going from parole to work release. This is unlikely to be true. For example, revocation is only possible while on supervision, therefore the effect on revocations for moving from non-supervision to any level supervision should be greater than moving between levels of supervision.

6 Discussion

A final puzzle is why participation in education is causing people to be released into work release, i.e., higher levels of supervision. Unfortunately, our data are not well-positioned for a thorough analysis of this question, so this section will proceed in a more speculative manner. Release decisions in IDOC are almost entirely based on the recommendations of case managers. There are three important factors that might shed light on why education in our context may be shifting release type, all of which might push case managers to increasingly recommend work release for participants in education. The first is that education, particularly post-secondary education, is generally viewed favorably. The second is that work release is not generally viewed disfavorably and can be thought of as providing stable housing to an individual who would not otherwise have it following release.[29] Finally, misconducts during incarceration may influence the perceived risk level or rehabilitation of the individual being released. Case managers weigh all parts of an individual's profile when making release recommendations including access to stable housing after release, participation in education and other programming, and misconduct.

While we cannot estimate how either education or work release is viewed by case managers, we can estimate whether participation in education affects misconduct citations. We first estimate this relationship using our preferred 2SLS specification. The results are presented in Appendix Table A5 and show that participating in education results in fewer violent misconducts, with no significant effects on other forms of misconduct. However, we interpret these estimates cautiously. Unlike our post-release outcomes, misconduct in this framework suffers from a potential endogeneity problem, because misconduct early in a prisoner's sentence could preclude them participating in education (e.g., if one commits a violent misconduct and is placed in solitary confinement when the course is starting). Fortunately, our data also allow us to employ a different identification strategy to analyze misconduct

[29]Lee (2023) documents significant variation across case managers in propensity to assign to work release, potentially reflecting variation in views of the utility versus potential harm of work release.

around the start of courses.

In Appendix Table A6, we use individual-daily-level data in an event study framework to understand how misconduct changes when an inmate starts their first course in prison. We estimate Poisson regressions with daily misconduct citations as the dependent variable. These regressions include individual and prison fixed effects, flexible controls for fraction of time served decile, as well as controls for visits. The first column presents results for misconducts overall, while the subsequent columns present results for specific types of misconducts. We find that participating in education increases misconducts overall with drug and property misconducts increasing significantly. Accordingly, we hypothesize that our estimated effects of prison education on release type reflect an effort by case managers to weigh the positive signal from participation in education with negative aspects such as misconduct.

There are a number of potential explanations for the increased misconduct that we observe. For example, in most Iowa prisons, the education building is distinct from inmate living areas. This means that participating in education creates additional movement for prisoners. It may also create more opportunities for individuals to interact, which may promote transferring contraband. Education also gives students access to some materials that would otherwise be limited to them including computers, paper, and writing implements, increasing the potential for these materials to be abused or stolen. These factors all likely contribute to increased misconduct while the course is taking place. Finally, conversations with corrections staff also suggest that some officers feel that "free" prison education for prisoners, particularly post-secondary education, is deeply unfair. Most correctional officers are not college educated, and many believe the inmates are being given opportunities that they were never afforded.[30] Because of discretion in determining misconduct, particularly non-violent misconduct, this may lead officers to be more likely to issue misconduct citations in marginal cases for people participating in education.

[30]The education requirement for correctional officers is graduation from high school or equivalent. See https://das.iowa.gov/media/2874/download?inline=.

7 Conclusion

In this paper, we estimate the impact of prison education on a variety of prisoner outcomes, including reincarceration, in-prison misconduct, as well as employment and education after release. In our context, we find that participating in education appears to shift individuals to higher levels of post-release supervision, and this higher level of supervision causes higher rates of reincarceration. Our study has important policy implications. Crucially, we do think that this research should not be used to eliminate or reduce prison education in U.S. Recall, the median number of courses taken in our sample, among people taking at least one course, is just two. While we expect that number to be similar to many prison education programs around the country, it is also likely insufficient to achieve meaningful improvement in outcomes. Former prisoners likely experience significant sheepskin effects with tangible degrees having far more impact than participation in a few courses (Hungerford and Solon, 1987; Darolia, Mueser and Cronin, 2021). Most (96%) of the students in our sample did not earn a degree or accreditation while incarcerated. This likely undercuts the most significant benefits of prison education.

Instead, our results point to two important considerations. First, that prison education is deliberate with its offerings to ensure students have the ability to earn degrees or certificates. There is also suggestive evidence that better targeting for the type of education offered (e.g., more vocational, career, technical) could yield improved results, though evidence is also scarce in U.S. prisons (Alsan et al., 2025; Arbour, Lacroix and Marchand, 2024). Second, our results are a stark reminder of the potential for unintended consequences to derail policies that may otherwise be beneficial, and the need to account for how institutional agents may respond to interventions. From a policy standpoint, individuals participating in education should not be released differently than those who do not.

Future research should explore what happens when educational programs are scaled up in such a way that people can take enough classes in prison to earn accreditation and develop

useful post-release skills. Given recent evidence that intensive, wraparound programs that include an educational component can significantly reduce reincarceration, it is critical to understand the role of education and degrees in achieving these positive outcomes.

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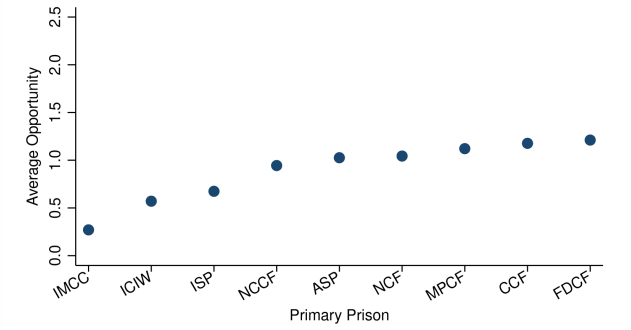
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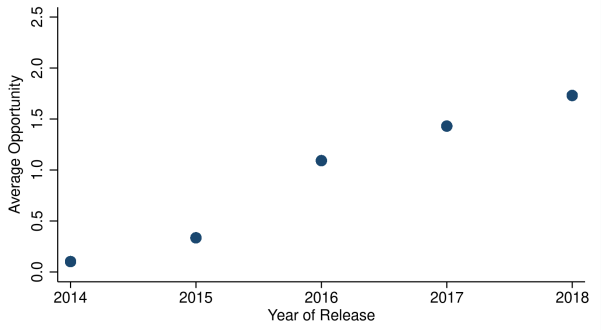
8 Tables and Figures

Figure 1: Nonrandom Variation in Educational Opportunities

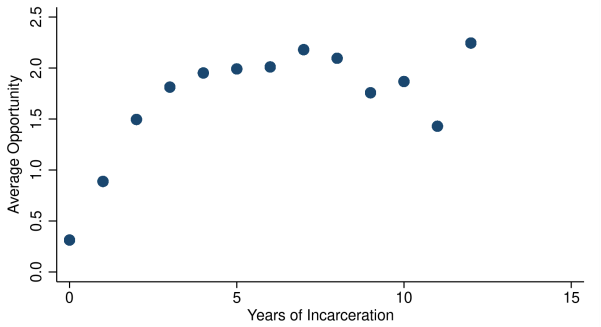
(A) Primary Prison



(B) Release Year

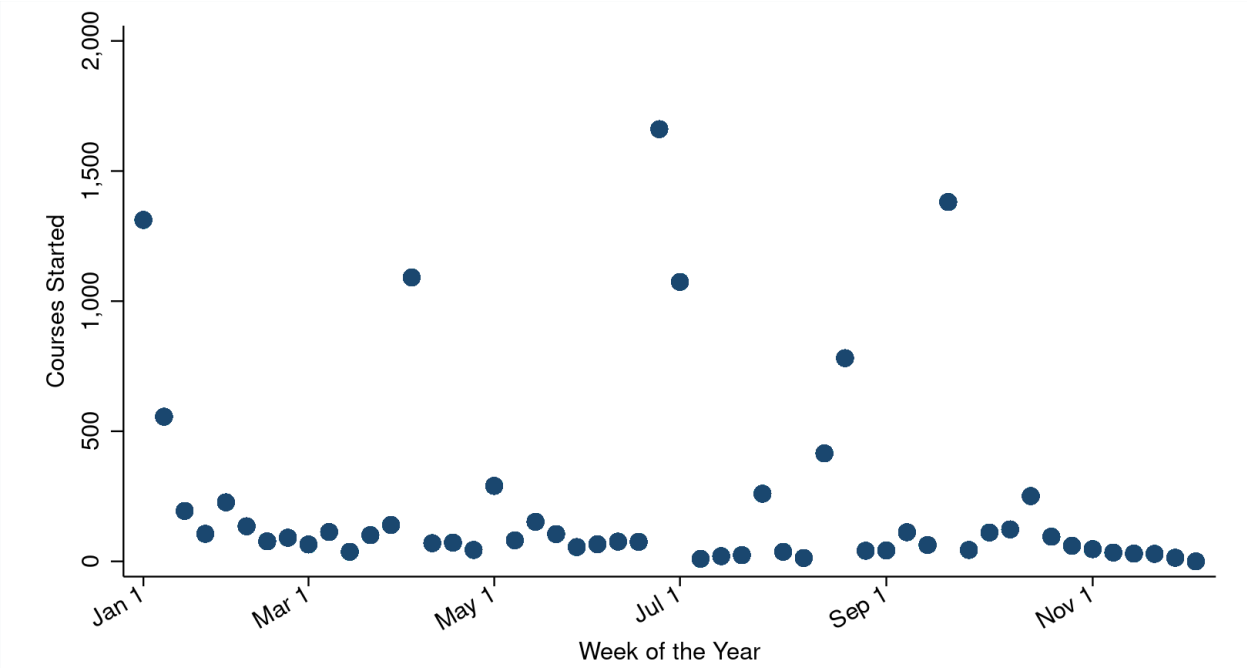


(C) Time Served



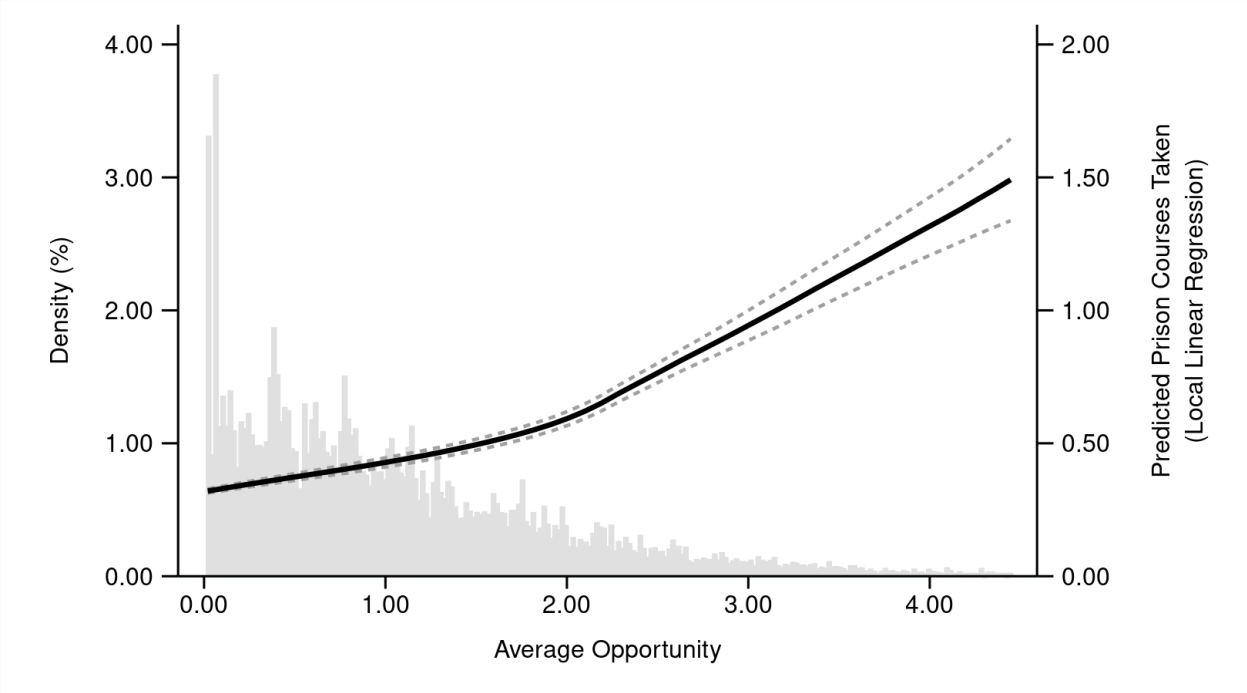
Notes: This figure displays average opportunity score by primary prison assignment (Panel A), release year (Panel B), and time served (Panel C).

Figure 2: Random Variation in Opportunity due to Timing of Course Starts



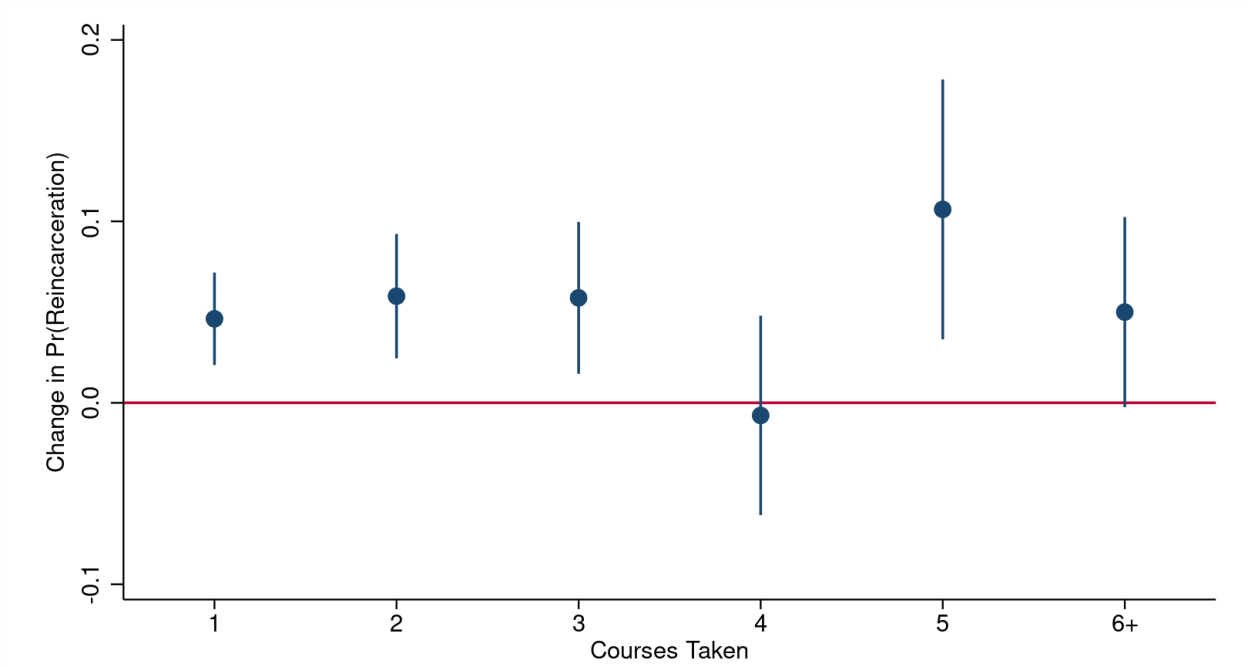
Notes: This figure displays the number of courses starting each week of the year during our primary sample window.

Figure 3: Increased Opportunities Predict More Participation in Prison Courses



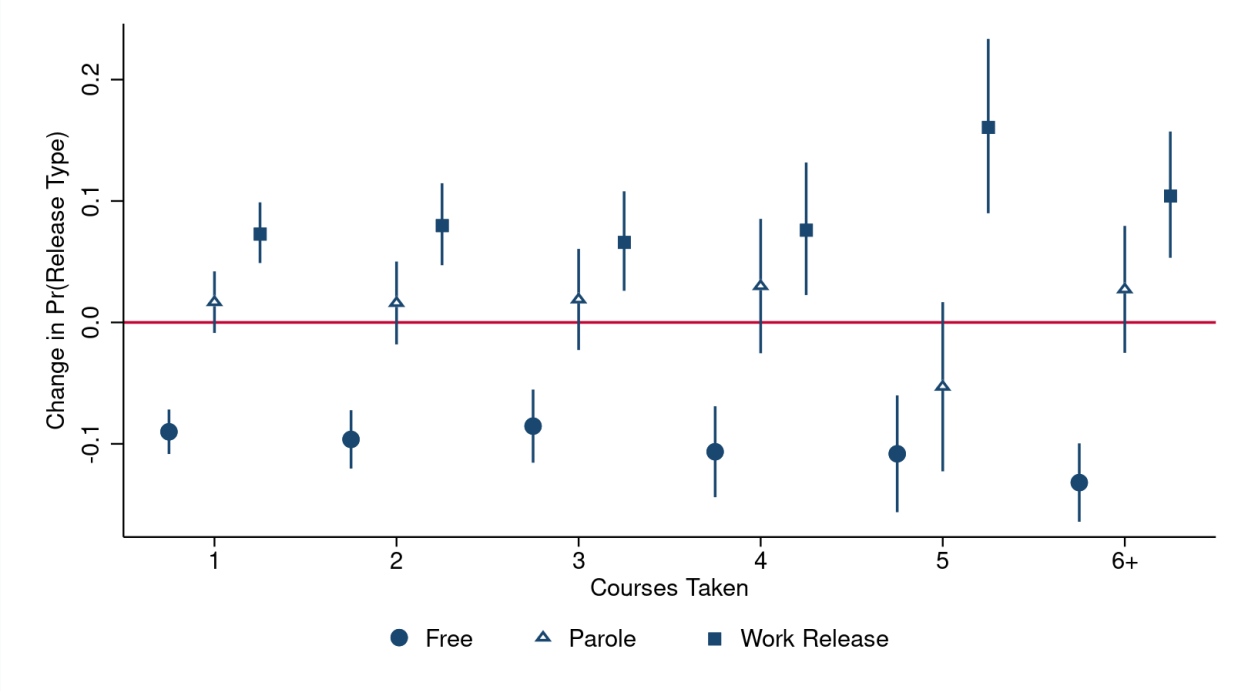
Notes: This figure displays a histogram of the opportunity instrument as described in Section 4. The solid line represents a local linear regression of courses taken on the opportunity instrument. The 95% confidence interval is indicated with dashed lines. We omit prisoners who had an opportunity score of 0 from the figure to preserve the density scaling. Every individual with 0 opportunity score took 0 courses while incarcerated. We also omit the top 1% of opportunity scores from the figure. Appendix Figure A2 shows the figure without these omissions.

Figure 4: Prison Education and Reincarceration



Notes: This figure shows the descriptive relationship between courses taken and recidivism by plotting the coefficient estimates and confidence intervals from an OLS regression of reincarceration within three years of release on indicators for the number of courses taken while incarcerated. The omitted group is individuals who took no courses while incarcerated. Standard errors are robust to heteroskedasticity.

Figure 5: Prison Education and Release Type



Notes: This figure displays the coefficient estimates and confidence intervals from OLS regressions of release type on indicators for the number of courses taken while incarcerated. The omitted group is individuals who took no courses while incarcerated. Standard errors are robust to heteroskedasticity.

Table 1: Summary Statistics

	No Courses	1+ Course	Difference	Courses Taken	Opportunity
Number of Prison Transfers	1.686	1.869	0.182**	-0.022**	-0.001
			(0.087)	(0.011)	(0.010)
Age 25-29	0.189	0.214	0.025***	-0.136***	-0.016
			(0.008)	(0.040)	(0.014)
Age 30-35	0.216	0.209	-0.008	-0.183***	-0.018
			(0.010)	(0.041)	(0.015)
Age 36-43	0.185	0.151	-0.034***	-0.238***	-0.025
			(0.010)	(0.041)	(0.015)
Age 43+	0.209	0.108	-0.101***	-0.361***	-0.020
			(0.007)	(0.054)	(0.018)
Prior Recidivist	0.771	0.643	-0.128***	-0.037***	0.006
			(0.037)	(0.013)	(0.005)
Felony Convictions	1.556	1.787	0.231***	0.016***	0.005*
			(0.041)	(0.004)	(0.003)
Total Convictions	2.725	2.982	0.256***	-0.005**	-0.001
			(0.059)	(0.002)	(0.001)
Any Violent Crime?	0.280	0.368	0.087***	0.052	0.034*
			(0.018)	(0.033)	(0.018)
Any Drug Crime?	0.378	0.376	-0.002	0.009	-0.017
			(0.010)	(0.017)	(0.012)
Any Property Crime?	0.422	0.473	0.051***	0.003	0.009
			(0.013)	(0.023)	(0.011)
Any Public Order Crime?	0.395	0.364	-0.031***	-0.014	0.004
			(0.009)	(0.016)	(0.012)
White	0.705	0.604	-0.102***	-0.046**	0.010
			(0.017)	(0.023)	(0.009)
Highest Grade Completed	11.669	10.933	-0.736***	-0.145***	-0.002
			(0.072)	(0.027)	(0.004)
Share of Sentence in Hospital	0.009	0.008	-0.001	-0.152**	-0.180**
			(0.002)	(0.059)	(0.086)
Violence Score	5.868	5.381	-0.487*	0.001	-0.000
			(0.254)	(0.005)	(0.003)
Missing Violence Score	0.126	0.054	-0.072***	-0.093	-0.003
			(0.013)	(0.080)	(0.050)
Observations	18,594	3,884	22,478	22,478	22,478
F-Stat				5.03	1.20
P-value				0.00	0.30

Notes: Columns 1 and 2 report the means for the indicated groups. Column 3 shows the difference in group means. Columns 4 and 5 present multivariate regression results with courses taken (Column 4) and course opportunities (Column 5) as the dependent variables. Columns 4 and 5 include our randomization fixed effects, and standard errors clustered at the prison by release year level are reported in parentheses. Both columns also report the F-statistic and p-value from a joint test of significance. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2: Recidivism within Three Years of Release

	OLS		Reduced Form		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A. Reincarceration			
Courses Taken	0.006***	0.005**			0.026*	0.022*
	(0.002)	(0.002)			(0.015)	(0.013)
Opportunity			0.012*	0.010*		
			(0.006)	(0.005)		
Outcome Mean	0.445	0.445	0.445	0.445	0.445	0.445
			Panel B. Revocation			
Courses Taken	0.008***	0.007***			0.035**	0.032**
	(0.002)	(0.002)			(0.016)	(0.015)
Opportunity			0.016**	0.015**		
			(0.007)	(0.007)		
Outcome Mean	0.337	0.337	0.337	0.337	0.337	0.337
			Panel C. New Crime			
Courses Taken	0.005***	0.004**			0.011	0.006
	(0.002)	(0.002)			(0.012)	(0.009)
Opportunity			0.005	0.003		
			(0.005)	(0.004)		
Outcome Mean	0.268	0.268	0.268	0.268	0.268	0.268
Observations	22,478	22,478	22,478	22,478	22,478	22,478
First Stage F Stat					141.751	133.772
Randomization FE	X	X	X	X	X	X
Controls		X		X		X

Notes: This table presents regression results estimated on a prisoner-stint level dataset. Columns 1 and 2 report OLS estimates for recidivism on courses taken. Columns 3 and 4 report reduced form estimates for recidivism on course opportunities, and Columns 5 and 6 report the results from estimating Equations 1 and 2. Within column pairs, the first column includes only randomization fixed effects while the second includes both randomization fixed effects and the full set of controls. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Employment and Wages within Three Years of Release

	Pr(Employed)	Quarters Worked	Quarterly Wages	Nonzero Quarterly Wages
Courses Taken	0.031***	0.174	48.397	-61.710
	(0.009)	(0.114)	(75.778)	(103.952)
Observations	21,647	21,647	21,647	21,647
Outcome Mean	0.771	4.432	1839.952	4732.242
First Stage F Stat	93.467	93.467	93.467	93.467
Randomization FE	X	X	X	X
Controls	X	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Education within Three Years of Release

	Courses	Credit Courses	Noncredit Courses	Passed Courses
Courses Taken	-0.011	-0.004	-0.007	0.009
	(0.053)	(0.044)	(0.022)	(0.039)
Observations	22,478	22,478	22,478	22,478
Outcome Mean	0.562	0.292	0.270	0.356
First Stage F Stat	133.772	133.772	133.772	133.772
Randomization FE	X	X	X	X
Controls	X	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects on Recidivism by Race and Course Type

	Reincarceration	New Crime	Revocation	Reincarceration	New Crime	Revocation
			Panel A. By Race			
		White			Nonwhite	
Courses Taken	0.037**	0.013	0.046**	0.004	-0.003	0.017
	(0.018)	(0.016)	(0.020)	(0.017)	(0.013)	(0.018)
Outcome Mean	0.442	0.267	0.333	0.450	0.269	0.346
Observations	15,453	15,453	15,453	7,025	7,025	7,025
First Stage F Stat	82.091	82.091	82.091	96.969	96.969	96.969
			Panel B. By Course Type			
		HSE or Remedial Courses			Post-Secondary Courses	
Courses Taken	0.013	0.013	0.019	0.038**	0.004	0.057***
	(0.022)	(0.018)	(0.027)	(0.017)	(0.013)	(0.018)
Outcome Mean	0.445	0.268	0.337	0.445	0.268	0.337
Observations	22,478	22,478	22,478	22,478	22,478	22,478
First Stage F Stat	89.585	89.585	89.585	159.473	159.473	159.473
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Placebo Test using 2SLS Specification

	HSE or Remedial Courses			Post-Secondary Courses		
	Reincarceration	New Crime	Revocation	Reincarceration	New Crime	Revocation
	Panel A. Individuals with At Least 12 Years of Education					
Courses Taken	0.004	0.018	0.022	0.030*	0.006	0.050***
	(0.046)	(0.040)	(0.051)	(0.018)	(0.012)	(0.018)
Outcome Mean	0.445	0.268	0.339	0.445	0.268	0.339
Observations	18,402	18,402	18,402	18,402	18,402	18,402
First Stage F Stat	119.922	119.922	119.922	166.173	166.173	166.173
	Panel B. Individuals with Fewer Than 12 Years of Education					
Courses Taken	0.021	0.010	0.013	0.332	-0.077	0.342
	(0.017)	(0.013)	(0.016)	(0.310)	(0.295)	(0.294)
Outcome Mean	0.442	0.265	0.326	0.442	0.265	0.326
Observations	4,076	4,076	4,076	4,076	4,076	4,076
First Stage F Stat	85.287	85.287	85.287	5.723	5.723	5.723
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Does Education Change Release Type?

	Free	Parole	Work Release
Courses Taken	-0.048**	-0.001	0.049**
	(0.021)	(0.019)	(0.023)
Observations	22,478	22,478	22,478
Outcome Mean	0.219	0.433	0.347
First Stage F Stat	133.772	133.772	133.772
Randomization FE	X	X	X
Controls	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Does Release Type Mitigate Reincarceration Outcomes?

	Reincarceration	New Crime	Revocation	Reincarceration	New Crime	Revocation
Opportunity	0.010*	0.003	0.015**	0.004	0.002	0.004
	(0.005)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)
Parole				0.129***	-0.052***	0.314***
				(0.010)	(0.009)	(0.010)
Work Release				0.284***	0.019	0.494***
				(0.012)	(0.011)	(0.012)
Observations	22,478	22,478	22,478	22,478	22,478	22,478
Outcome Mean	0.445	0.268	0.337	0.445	0.268	0.337
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents reduced form results for recidivism on course opportunities using a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A1 Data Appendix

The wage and employment data in this project comes from IowaWORKS. Unfortunately, due to strict data provision requirements, Iowaworks is unable to provide external researchers with individual-level data. Instead, they allowed us to create cohorts of individuals and agreed to provide wage and employment information for each cohort as long as each cohort contained at least three people who matched to the employment data (i.e., were gainfully employed) in at least 1 of the 12 quarters following release from incarceration. If fewer than three individuals were matched to a cohort, the information for the entire cohort would be suppressed. Moreover, because of the resource investment required for IowaWORKS to match these cohorts and attach the appropriate information, we only had to one opportunity to request this labor market information. Using Carson, Bhaskar and Porter (2021), we predicted that roughly two-thirds of the individuals in our sample would have gainful employment in the three years after release. We used this value to calculate optimal cohort size of 9, which would keep the cohorts as small as possible while minimizing the number of cohorts that likely would be suppressed by IowaWORKS.

To construct our cohorts, we followed a method similar to the algorithm developed in Autor et al. (2016). We group individuals based on a list of characteristics including primary prison, release year, and time served in 6-month bins up to 5 years. The list of variables used to create our cohorts in the order in which they were applied is:

1. Primary prison (i.e., one of the nine prisons in Iowa:)
2. Release year (e.g., 2014)
3. Time served category (i.e., 6-month bins up to 5 years)[31]
4. Number of Courses Taken (winsorized to 2)[32]
5. Opportunity Score[33]
6. Release type (e.g., free, parole, or work-release)
7. Race (e.g., white or non-white)
8. Age at sentence start quintiles
9. Years of education before sentence (i.e., indicator for 12 or more years).

[31] Because the distribution of time served has a long right tail, time served was winsorized to 5 years (i.e., all individuals who served 5 or more years in prison were assigned the same time served value for the purposes of cohort creation).

[32] All individuals taking more than 1 class were grouped together to deal with the long right tail in the distribution.

[33] We rounded this continuous variable to the closest integer and winsorized the right tail of the distribution to 3.0, creating 31 bins.

We construct our cohorts so that there are between nine and 17 individuals in each cohort. The number of variables used in the grouping varies across cohorts. In some cases, a cohort reaches a size of between nine and 17 after matching on a small number of variables. After matching on a given variable in the above hierarchy, cohorts with fewer than 18 individuals are not further subdivided using the next variable. If a cohort has fewer than nine individuals, we sort on the current matching variable, and merge the cohort with an adjacent cell with nine individuals. The resulting cohort is not subdivided any further. After matching on all variables, we split all cohorts in the maximum number of cells with size 9 or more.

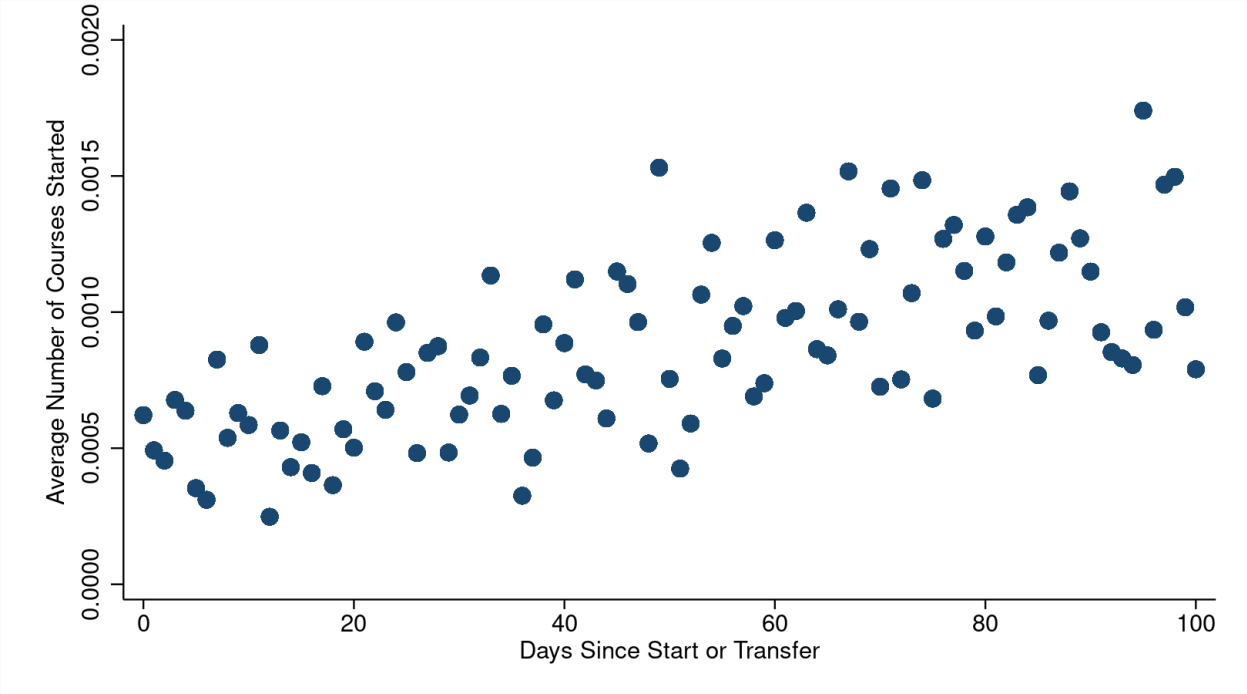
Following this procedure, we create cohorts with the following distribution of cohort size. We also include in the table the distribution of matched cohorts received from IowaWORKS. Using our approach, only 0.4% of provided cohorts were suppressed.

Matched vs. Created Cohorts

In cohort of size	Created		Matched	
	Freq.	Percent	Freq.	Percent
9	15,075	69.38	14,994	69.26
10	700	3.22	700	3.23
11	748	3.44	748	3.46
12	756	3.48	756	3.49
13	780	3.59	780	3.60
14	854	3.93	854	3.94
15	870	4.00	870	4.02
16	960	4.42	960	4.43
17	986	4.54	986	4.55
Total	21,729	100.00	21,648	100.00

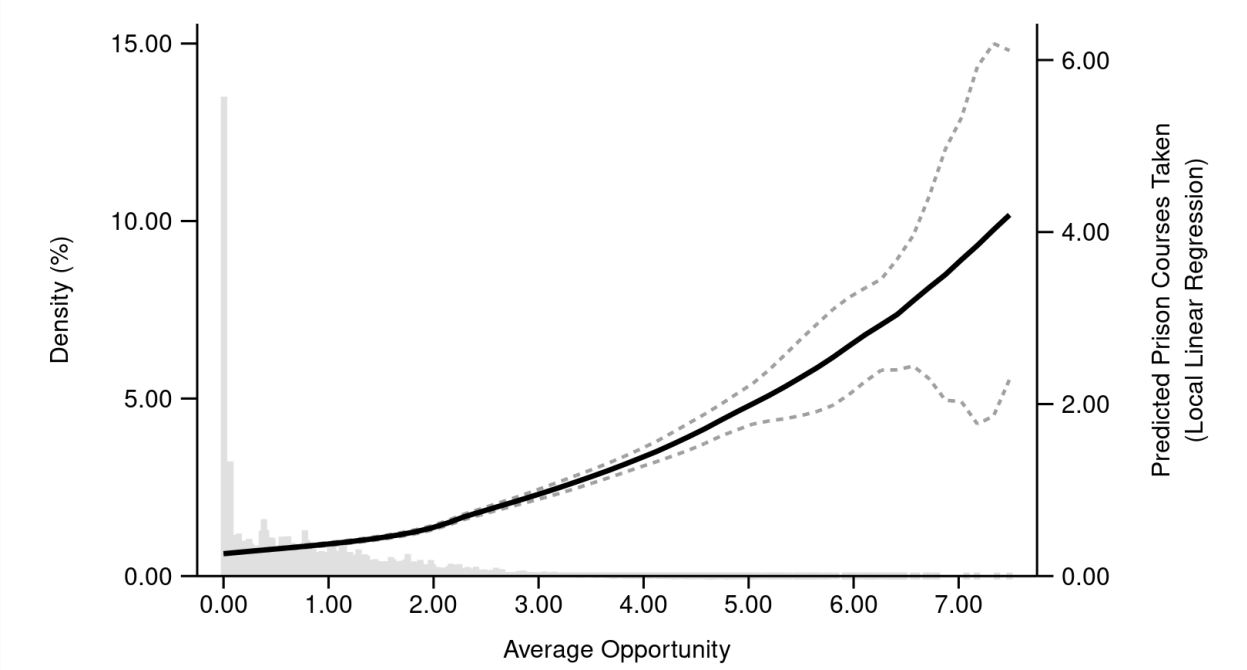
A2 Results Appendix

Figure A1: Course Starts After Transfers



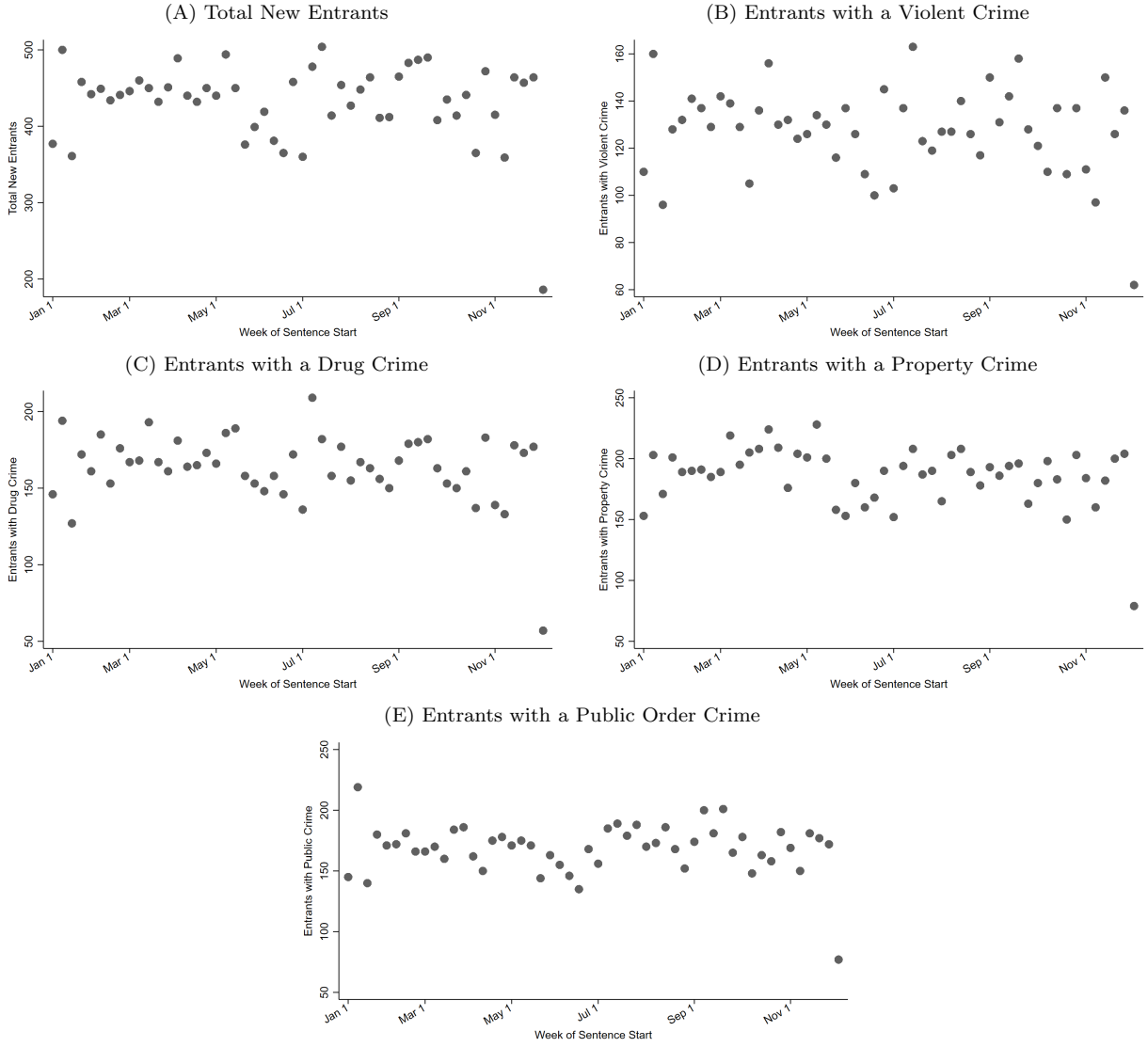
Notes: This figure displays the average number of courses started by people who had transferred X days ago.

Figure A2: Increased Opportunities Predict More Participation in Prison Courses



Notes: This figure displays a histogram of the opportunity instrument as described in Section 4. The solid line represents a local linear regression of prison course participation on the opportunity instrument.The 95% confidence interval is indicated with dashed lines.

Figure A3: No Seasonality in Prison Entry



Notes: This figure displays the number of people entering prison during each week of the year. Panel B shows the number of new entrants with at least one violent crime conviction, Panel C shows the number of new entrants with at least one drug crime conviction, Panel D shows the number of new entrants with at least one property crime conviction, and Panel E shows the number of new entrants with at least one public order crime.

Table A1: Participation Statistics by Education Category

Education Type	Participation Rate	Participants	Average Courses|Participation	Average Opportunity
All Courses	0.17	3,884.00	2.63	46.90
	[0.38]		[2.36]	[49.03]
HSE or Remedial Courses	0.14	3,074.00	2.21	25.44
	[0.34]		[1.65]	[28.50]
Post-Secondary Courses	0.05	1,019.00	3.35	21.46
	[0.21]		[3.34]	[27.39]
HSE Courses	0.02	553.00	2.00	5.30
	[0.15]		[1.05]	[9.14]
Remedial Courses	0.12	2,791.00	2.04	20.14
	[0.33]		[1.57]	[23.19]
Blue Collar Training Courses	0.01	307.00	4.46	7.14
	[0.12]		[3.32]	[14.38]
White Collar Training Courses	0.01	180.00	2.75	2.34
	[0.09]		[1.92]	[3.97]
Liberal Arts Courses	0.01	139.00	3.68	4.67
	[0.08]		[3.43]	[7.40]

Notes: Column 1 shows the proportion of individuals in the sample who participate in at least one class of the indicated type during their incarceration. Column 2 shows the number of students who participated in at least one course of the indicated type. Column 3 shows the average number of classes of the indicated type taken by students who took at least one class of that type. Column 4 shows the average number of courses of the indicated type that started during each person's sentence. HSE stands for High School Equivalence and represents students pursuing a GED or HiSET degree. Standard deviations are reported in square brackets.

Table A2: Monotonicity Test

	5 knots	10 knots	15 knots	20 knots
Test Statistic	314.78	306.46	292.10	244.40
Degrees of Freedom	(303)	(298)	(293)	(288)
P-value	[0.309]	[0.355]	[0.504]	[0.971]

Notes: This table displays the test statistics, degrees of freedom in parentheses, and p-values in brackets from the monotonicity test proposed in Frandsen, Lefgren and Leslie (2023). Each column uses the indicated number of knots in a spline function and controls for randomization fixed effects. All columns use a fit weight of 1.

Table A3: Time Served Robustness

	Reincarceration	New Crime	Revocation
Panel A. 1-month Time Served Bins			
Courses Taken	0.021	0.006	0.032**
	(0.013)	(0.010)	(0.016)
First Stage F Stat	131.232	131.232	131.232
Panel B. 6-month Time Served Bins			
Courses Taken	0.021*	0.006	0.032**
	(0.012)	(0.009)	(0.015)
Outcome Mean	0.445	0.268	0.337
Observations	22,478	22,478	22,478
First Stage F Stat	136.493	136.493	136.493
Randomization FE	X	X	X
Controls	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Placebo Test using Reduced Form Specification

	HSE or Remedial Courses			Post-Secondary Courses		
	Reincarceration	New Crime	Revocation	Reincarceration	New Crime	Revocation
Panel A. Individuals with At Least 12 Years of Education						
Opportunity	0.000	0.002	0.003	0.010*	0.002	0.016***
	(0.006)	(0.005)	(0.007)	(0.006)	(0.004)	(0.006)
Outcome Mean	0.445	0.268	0.339	0.445	0.268	0.339
Observations	18,402	18,402	18,402	18,402	18,402	18,402
Panel B. Individuals with Fewer Than 12 Years of Education						
Opportunity	0.016	0.008	0.010	0.014	-0.003	0.014
	(0.013)	(0.010)	(0.013)	(0.013)	(0.012)	(0.013)
Outcome Mean	0.442	0.265	0.326	0.442	0.265	0.326
Observations	4,076	4,076	4,076	4,076	4,076	4,076
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Standard errors clustered at the primary prison by release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: 2SLS Estimates for Effect of Prison Education on Monthly In-Prison Misconduct

	Total	Drug	Order	Property	Violent
Courses Taken	-0.023	-0.002	-0.015	-0.001	-0.006**
	(0.019)	(0.003)	(0.014)	(0.001)	(0.003)
Observations	22,478	22,478	22,478	22,478	22,478
Outcome Mean	0.139	0.023	0.097	0.004	0.016
First Stage F Stat	133.772	133.772	133.772	133.772	133.772
Randomization FE	X	X	X	X	X
Controls	X	X	X	X	X

Notes: This table presents regression results from estimating Equations 1 and 2 on a prisoner-stint level dataset. Dependent variable is the number of misconduct incidents of the indicated type per month of incarceration. Standard errors clustered at the individual level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Poisson Model Event Study Estimates for Effect of Prison Education on In-Prison Misconduct

	Total	Drug	Order	Property	Violent
After First Course	0.086*	0.153**	0.090	0.200*	-0.030
	(0.051)	(0.061)	(0.054)	(0.113)	(0.083)
Observations	12,231,879	8,444,387	11,387,839	3,110,224	6,355,331
Individual FE	X	X	X	X	X
Prison FE	X	X	X	X	X
Fraction of Time Served Decile	X	X	X	X	X

Notes: This table reports results from estimating Poisson regressions on a prison-by-day level dataset. All regressions include individual and prison fixed effects, flexible controls for fraction of time served decile, as well as controls for visits. Standard errors clustered at the individual level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.