A Second Chance at Schooling?
Unintended Consequences of Prison Education*

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Abstract

Despite rapid growth in US prison education programs, causal evidence of their effectiveness is limited. Leveraging detailed administrative data from Iowa, we estimate the impact of prison education on future incarceration, employment, and education, using course availability to instrument for course-taking. We find that course-taking increases reincarceration for technical violations but not for new crimes. Exploring mechanisms, we find that education participants are 14% likelier to be assigned to work release—probably with the aim of supporting their reentry into society. However, work release *increases* revocation risk. A mediation exercise confirms no relationship between education and reincarceration after we control for release type. We also find that course-taking increases the probability of post-release employment, consistent with the increased likelihood of assignment to work release.

Keywords: education, prison, incarceration, recidivism, employment

JEL Codes: K42, I23, H52

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1 Introduction

The United States has one of the world's highest incarceration rates, with nearly 1.8 million people in prison in 2024 (Fair and Walmsley, 2024). This population recidivates at high rates: 46% of released prisoners are rearrested within five years (Kalra et al., 2022). Each recidivism event carries substantial social costs, estimated at $151,662 per incident (Steinfeld, Powers and Saltmarsh, 2018). Despite decades of reform efforts, reducing recidivism remains a significant public policy challenge (Doleac, 2023).

One potential solution garnering renewed attention is prison education programs. US prisons disproportionately house economically vulnerable individuals, many of whom have limited education.[1] Despite a historical lack of availability, prison education consistently draws high interest from prisoners (West, 2025); a 2014 survey found that 70% of prisoners want to enroll in educational programs while incarcerated and that a "majority...are academically eligible for post-secondary-level courses" (Rampey et al., 2016; Oakford et al., 2019). The 2015 Second Chance Pell Grant Pilot Program partially reinstated Pell grant eligibility for prisoners and awarded approximately $180 million in funding to more than 40,000 prisoners between 2016 and 2022. Moreover, in 2023, Pell grant eligibility was fully restored for all otherwise-eligible prisoners, a reform that is expected to further increase participation in college-in-prison programs. However, evidence on the causal impact of these programs remains limited.

This paper estimates the causal effect of prison education as it is typically offered in the US on prisoner outcomes including reincarceration, future employment and education, and in-prison misconduct. Nearly all US state and federal prisons offer educational programming. Most commonly, these programs focus on secondary or remedial education, including preparation for the GED. Postsecondary educational opportunities are typically delivered through

[1]The proportion of state prisoners without a high school diploma or equivalent increased from about 41% in 1997 to roughly 61%, according to recent estimates (Harlow, 2003; Wang and Widra, 2022). The average prisoner in our sample has 11.5 years of education upon entering prison. The scarcity of labor market opportunities for this population also likely contributes to high recidivism rates.

community college partnerships. In most cases, US prison education programming involves only coursework provision, with minimal supplementary support services or complementary interventions (Dewey et al., 2020).[2]

There are several intuitive channels through which prison education may affect individual outcomes. Chief among these are human capital accumulation—allowing prisoners to invest in their human capital development may make them more productive—and signaling—educational opportunities may give prisoners an important means of signaling their quality to decision-makers in the criminal justice system or potential employers after release (Kuziemko, 2013). What is less often considered is whether prison education programs might interact with other features of the prison system in unexpected ways that may have *either* beneficial or adverse consequences for participants. For example, these programs may alter the frequency or dynamics of interactions among participants or between participants and corrections personnel. It is also unclear whether and how corrections staff, including correctional officers, case managers, or parole board members, might respond to the presence of these programs: Anecdotal evidence suggests that some correctional officers may resent the provision of "free" education to incarcerated people (Barrett and Greene, 2023). Because these interactions and behavioral responses could complement or counteract any potential benefits of prison education, careful research into the effects of these interventions is essential.

Causal evidence on the effectiveness of US prison education programs is scarce for two key reasons: data limitations and identification challenges due to selection bias. To overcome the typical data challenges, we combine data from the Iowa Department of Corrections (IDOC), Iowa Department of Education (IDOE), Iowa Workforce Development (IowaWORKS), and Grinnell College to create a unique, comprehensive, individual-level dataset of people released from an Iowa prison between 2014 and 2018. These detailed administrative data allow us to track each person's incarceration and course-taking history and make progress in an important but understudied area.

[2]National estimates suggest that the supply of coursework is also limited, with most students taking only a few courses and not completing any type of degree or certificate (Rampey et al., 2016).

Selection into course participation is a common concern, and our ordinary least squares (OLS) estimates show clear evidence of selection into course-taking, underscoring the need to address selection bias from individuals opting into education. To do so, we employ an instrumental variable (IV) approach. We calculate an opportunity score for each individual as the number of courses that started in their prison while they were incarcerated, and we use this opportunity score to instrument for actual courses taken. Our instrument exploits quasi-random variation in course access driven by the interaction between course start times and prison entry timing, conditional on primary prison, time served, and release year fixed effects.[3] In our setting, course offerings follow the academic calendar, which means that, within a prison, course start times cluster within a few weeks of the year (e.g., the start of the semester).[4] IDOC policy also restricts prisoners from enrolling in courses that have already started. These factors together mean that individuals serving approximately the same amount of time in the same prison who enter just before or after a set of courses begins can experience different course-taking opportunities. We show that our opportunity instrument is predictive of the number of courses taken but is uncorrelated with prisoner characteristics.

Our results show that participating in prison education increases a prisoner's likelihood of reincarceration within three years of release; the result is driven by a 3.4-percentage-point (10.1%) increase in revocations (i.e., technical violations), with no significant effect on new crimes. We also show that our results on reincarceration hold primarily for postsecondary courses and among white individuals. Crucially, we argue that these surprising findings on

[3]It is reasonable to be concerned that time served, and subsequently release year, may be affected by actions an individual takes while incarcerated, including participating in optional programming. We discuss these concerns in further detail in Sections 2 and 4. According to IDOC policy, participating in prison education should not directly affect time served. To investigate concerns that prison education may affect time served, we also include several robustness exercises, including an analysis in which we redefine the instrument to capture only course opportunities available within the first three months of each prisoner's sentence—a specification that allows us to use start year in place of time served and release year fixed effects.

[4]The supply of courses also fluctuates over time. This variability in supply is not unique to Iowa. In many settings, course selection is determined largely by how constrained a prison's capacity is (e.g., whether a correctional officer is available to escort the course instructor or the students to the classroom) and the supply of faculty members who are able to teach in the prisons. See Section 2 for a more detailed discussion.

reincarceration reflect unexpected interactions between education and the broader correctional system, rather than the direct impact of education itself.[5] We show that participation in education affects how individuals are released: It decreases their likelihood of being released free and clear and increases their likelihood of being assigned to work release. Assignment to work release (Iowa's version of a halfway house) represents an increase in exposure to more intensive post-release supervision, which dramatically increases the probability of revocation (Lee, 2023; Sakoda, 2023). Indeed, a mediation analysis with a reduced-form model shows no impact of education on reincarceration or revocation after we control for release type.[6]

These release-type findings add important context for our other results. We find that prison education significantly increases the likelihood of employment but does not affect average quarterly earnings or the number of quarters worked. These results are consistent with an increased likelihood of work release assignment because individuals on work release are required to actively seek employment and receive additional support to do so. Finally, we find that participation in prison education has no detectable effect on participation or performance in community college courses within three years of release.

Because how individuals are released is most likely driving our results, we briefly explore why participation in education might affect release type. This analysis is more speculative because our data are not ideal for studying the elements that determine post-release supervision assignment. We hypothesize that the release-type mechanism operates largely through case managers' release recommendations, which are almost always followed by the Iowa Board of Parole. According to our discussions with IDOC personnel, case managers generally view education, particularly postsecondary education, as a positive signal of prisoners' ability to conform to and benefit from programming and a regimen. In addition, case managers view

[5] As in most US prisons, most of the students in our sample take only one or two courses while incarcerated, implying that the scope for human capital growth in our context is likely limited.

[6] We also conduct the mediation analysis for the education outcomes, finding that in this case controlling for release type has no effect. Unfortunately, we cannot implement a mediation analysis for the employment outcomes because more than 70% of the cohorts include individuals released under different supervision levels.

work release not as a punishment but as a way to provide stable housing, structure, and additional programming.[7] One metric that case managers consider when assessing the need for post-release structure is in-prison misconduct. In an event study design, we show that, consistent with this conjecture, individuals receive more citations for nonviolent misconduct after starting their first course, likely because of a combination of increased opportunities to commit or be cited for misconduct and correctional officer resentment toward the educational opportunities afforded to prisoners. These additional citations may increase the likelihood that case managers assign participants to work release, potentially with the aim of supporting reentry with a structured environment or continued programming.

From a policy perspective, our paper highlights the importance of considering behavioral responses to interventions in the field. In our context, prison education itself does not appear to cause negative outcomes. However, the impact of participation on release decisions more than offsets any potential benefits from education. The policy solution, then, may not be to reduce education but to address its use in release decisions or the negative consequences associated with high-intensity post-release supervision. Cultural change and institutional buy-in from key stakeholders, including correctional officers, may also be crucial before such interventions are undertaken. Finally, course opportunities are limited in our context, with the median student taking only two courses. Prison education might deliver significant benefits if it were scaled up so that participants could earn credentials or degrees, particularly ones well aligned with local labor market needs.

Our study contributes to two strands of literature. First, we build on a substantial body of research demonstrating how unintended consequences may significantly alter the social impact of criminal justice or education policies (Agan and Starr, 2018; Johnson and Raphael,

[7]Work release may be an attractive option for case managers trying to support participants' reentry because formerly incarcerated individuals disproportionately suffer from homelessness (Couloute, 2018). Moreover, a common belief among corrections stakeholders is that more structured, highly supervised environments help reduce recidivism (Federal Bureau of Prisons, 2025; Wong et al., 2025). This perspective, however, is not supported by research, which suggests instead that higher-intensity post-release supervision increases the likelihood of reincarceration by increasing revocations (Petersilia and Turner, 1993)—the mechanism undergirding our results.

2009; Charles and Luoh, 2010; Doleac and Hansen, 2020; Figlio and Ozek, 2024; Barrow, Sartain and De La Torre, 2020). In our setting, any benefits of prison education are outweighed by the increase in revocation risk from the rise in likelihood of assignment to more intensive post-release supervision. Our research sheds light on specific and correctable patterns that may systematically undermine the value of education and highlights an unexpected mechanism through which prison education may actually worsen participants' outcomes—one that is currently absent from prison education policy discussions. Our findings are unlikely to be unique to Iowa because (1) at least 30 other states have discretionary release in which a parole board (sometimes, as in our case, in consultation with case managers) decides how individuals are released from prison (CSG Justice Center, 2024), (2) these decision-makers have immense discretion over which subjective factors (including prison education) to consider in release decisions and how to weigh them (Renaud, 2019), and (3) at least 32 other states and the federal government also use halfway houses or similar transitional housing models to support reentry (Daniel and Sawyer, 2020).

We also add to the literature examining the effect of prison education on prisoner outcomes. While studies show that more years of education reduces criminal activity (Lochner and Moretti, 2004), the impact of prison education on future offending remains an open question. Few studies address this question with rigorous causal designs. Instead, previous work, which generally finds that prison education significantly reduces reoffending, often fails to properly account for selection bias, directly comparing prisoners who did and did not take courses (Linden and Perry, 1983; Denney and Tynes, 2021). Our results are consistent with those of He (2024), who uses facility-level data to show that 12-month reincarceration rates decreased when educational programs were downsized in Texas. In addition to implementing a more credible research design that exploits individual-level variation in educational opportunity, we benefit from rich, unique administrative data that let us examine not only recidivism but also other critical outcomes, including in-prison misconduct, post-release supervision status, and key post-release labor market outcomes such as education,

employment, and wages, which are usually unavailable to researchers in this space.

Recent causal studies on other carceral settings show that educational programming paired with other intensive services and cultural shifts in the attitudes of corrections staff can reduce recidivism (Arbour, Lacroix and Marchand, 2024; Totarelli, 2024; Bhuller, Khoury and Løken, 2025). In a related study, Alsan et al. (2025) show that an intensive education program in Genesee County jail (Project IGNITE) reduced misconduct and 3-month recidivism. While these results are encouraging, Project IGNITE pairs tailored coursework and training with an emphasis on cultural change in the jail (especially among staff) that centers a rehabilitative philosophy.[8] In practice, these additional interventions are not typically present in US prisons. Instead, most prison education programs are designed to mirror education outside of prison—for example, courses may be transplanted from a local community college—with little additional support.[9] Thus, our paper is one of the first to offer a credible causal analysis of the impact of education as it is typically offered in US prisons.

The paper proceeds as follows. In Sections 2 and 3, we provide institutional background and describe our data. Section 4 presents our empirical strategy. Section 5 presents our results. Section 6 discusses mechanisms and Section 7 concludes.

2 Background

Prison Education Across the US 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 inmates, with secondary and remedial education programs focused on preparing students for a GED being the most

[8]This philosophy is similar to that underpinning the rehabilitation approaches in Norwegian and Canadian prisons.

[9]Beyond differences in the nature of the education programs, the populations in US jails and prisons also differ. Prisons, which are operated by state or federal authorities, house individuals who have been convicted (typically of more serious offenses such as felonies) and are serving their sentences. In contrast, jails are locally run facilities mainly holding people awaiting trial or sentencing and a small portion of individuals convicted of misdemeanors and generally serving short sentences (usually less than one year) (Sawyer and Wagner, 2025).

common offering (Harlow, 2003). In addition, most postsecondary prison education in the US (68%) is provided through partnerships with community colleges (Erisman and Contardo, 2005). This programming often involves students taking courses without much in the way of additional support beyond the classroom because this approach allows education to be offered more cheaply than in other settings (Stickle and Schuster, 2023; Wood, 2023).

In Iowa, all prison education is offered through local colleges, primarily community colleges.[10] The state funds all GED preparation courses and some postsecondary and vocational courses.[11] The programming offered through some schools, including Grinnell College and the University of Iowa, is funded by donations. The expectation, both in Iowa and across the country, is that the courses offered in prisons be as similar as possible, within prison guidelines, to their nonprison counterparts (Long, 2023; Wood, 2023).

The specific courses available vary across prisons and over time for several reasons. 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.[12] Community colleges expanded their college-level course offerings in prisons under the Second Chance Pell Grant program. We provide summary statistics on the types of courses taken in Iowa prisons between 2014 and 2018 in Appendix Table A1.[13] Consistent with nationwide statistics, 80% of the courses offered during our sample period were either high school equivalence (HSE) or remedial courses.[14] Moreover, again consistent with nationwide estimates, the median number of courses taken, conditional on an inmate's taking at least one course, is two.[15]

The low number of courses taken may be partially due to institutional factors. First,

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

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

[12]In 1965, the federal government funded college courses in prisons through Pell grants. However, Pell grant eligibility was removed for prisoners on death row and for those serving life sentences without parole in 1992. The 1994 Violent Crime and Law Enforcement Act made all prisoners ineligible for these grants.

[13]We also provide additional information about courses and the colleges offering them in Appendix A1.1.

[14]These courses often prepare students to pursue a GED or HiSET degree.

[15]We show the distribution of courses taken in Appendix Figure A1.

substantial upfront investment is required for faculty to teach in prisons. The application process can involve several steps, including screening, training, and briefing on security protocols at the prisons. These investments and other administrative burdens create an incentive for the same faculty members to teach in the same prison repeatedly, even the same course. Second, there are capacity constraints on the part of the prison, which may limit the frequency of courses. Corrections staff are needed to escort the faculty members and students around the prison and to staff the educational spaces, especially in Iowa, where classes occur in a separate building from where prisoners are typically housed.[16] Staffing needs also increase with the security level of the facility. Regardless of the cause, the low level of course-taking makes it somewhat unlikely that we will find sizable effects of education. In addition, we see very few instances of individuals earning degrees. Fewer than 4% of the individuals our sample earned any type of degree, diploma, or certificate while incarcerated; this figure is below 1% if we consider only those who earned degrees.[17]

Prison Education and Prison Requirements in Iowa. According to IDOC policy, participation in prison education should not directly impact either where a prisoner is assigned or how long a prisoner is incarcerated. Each new prisoner in Iowa is first sent to the Iowa Medical and Classification Center (IMCC). Typically, prisoners spend 1–2 months at IMCC, where they are evaluated for health concerns, risk level, and programming needs. They are then transferred to another longer-term prison. The availability of prison education has no impact on long-term prison assignment. Instead, individuals are assigned to facilities on the basis of 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 individual's health and programming needs, and 5) the prison's proximity to the jurisdiction where the individual was convicted (18 U.S.C. §3621).[18] While prisoners are allowed

[16]Most of the courses in our data are offered in person. Unfortunately, we cannot determine which courses are conducted in person and which might be online.

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

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

to request transfers to specific prisons, those transfers are rarely granted, and access to education is not considered a valid reason for transferring (Iowa Department of Corrections, 2025).[19] Instead, most movements across prisons result from a change in one of the five factors listed above. Prison transfers, then, offer an excellent source of variation in individuals' opportunities to participate in prison education.

Participation in prison education also does not itself affect time served. According to IDOC officials, upon entering prison, individuals are given an estimated release date that assumes the maximum possible earned time. Time is added back to an individual's sentence only if they commit misconduct. Incarcerated individuals in Iowa must either work or attend GED courses if they do not have a GED.[20] Prisoners earn 1.2 days off their sentence for every day they participate in either work or GED education. Refusal to work is a class-C violation punishable with up to 30 days of disciplinary detention (Savala, 2006). In practice, almost all prisoners fulfill the work or education requirement and earn the maximum number of possible days. An important implication of this system is that participation in education—or any additional programming, for that matter—does not in itself impact release timing. That is, all else equal, a person who participates in education beyond what is required by the prison should spend the same amount of time incarcerated as a nonparticipant.

Prison Education and Prison Release in Iowa and Across the US. Iowa has a 5-person parole board responsible for making discretionary release decisions. The board works in consultation with case managers, often implementing the latter's release recommendations. Both the case managers and the parole board members take a holistic approach to evaluating cases, which means that these decision-makers are often able to weight subjective factors however they like in their decisions. This system of release, in which a parole board makes

[19] Prison officials estimate that only 5% of prisoners ever transfer based on their own request (Lee, 2019).

[20] Because participating in postsecondary education does not satisfy this requirement, individuals participating in postsecondary courses are still expected to work. Given this, the tradeoff that prisoners face when participating in education depends on the level of education. Individuals taking high school and remedial courses work less than other prisoners, while individuals taking postsecondary courses do not. To the extent that prison work experience influences reincarceration or other outcomes, this may impact the interpretation of our results.

discretionary release decisions based on subjective input factors, is also used in most (33) other states (Renaud, 2019). Education often plays a part in these decisions as it may demonstrate an inmate's "ability to conform to program rules and regimen" (West-Smith, Pogrebin and Poole, 2000; Matejkowski et al., 2011). We further note that many (32) other states also utilize work release or halfway houses as a release option for individuals exiting prison.

3 Data

This project combines data from four unique data sources obtained through a research agreement.[21] First, administrative data from the IDOC provide information on every person released from an Iowa prison between January 1, 2014, and December 31, 2018. We observe key information covering this 5-year period for each individual including all incarceration spells, daily prison location, daily records of the number and type of misconduct committed, education level, and demographic information. We also observe all incarceration spells before 2014 and after 2018 through July 2022, though we have less information about these spells than about those in our primary analysis period.[22]

Second, IDOE administrative data provide individual-level information on every course taken by the individuals in our sample from the IDOC.[23] The education data range from

[21] We include a simplified schematic of where the datasets are from, how they were combined, and the general order of operations we used in Appendix Figure A2.

[22] We do not observe sentence length in our data, only actual time served; nor do we observe the universe of incarcerated individuals, only those released between 2014 and 2018. This is potentially important if participation in education influences time served and therefore affects selection into our sample. However, as we discuss in detail in other sections, IDOC policy suggests that participation in education should not directly affect time served. Our empirical analysis generally indicates that this is indeed the case. Moreover, we show the distribution of time served in Appendix Figure A3, which demonstrates that very few individuals in our sample have time served of close to 5 years. In fact, most of our sample served less than 3 years, with a median of approximately 17 months. This distribution of time served is consistent with overall Iowa sentencing data. In 2023, the median sentence length in Iowa was 78 months, with a mean of 103 months (TenNapel, Yates and Roeder-Grubb, 2023). However, very few prisoners serve their entire sentence in practice. If we naively apply the IDOC's policy of 1.2 days earned for every day served, we should expect most of these sentences to result in time served of less than 3 years. Moreover, we include several robustness exercises in our analysis to investigate concerns that prison education may impact time served.

[23] Appendix Table A1 summarizes the types of courses included in our data. We provide information on the top colleges offering courses and the top courses by enrollment count in Appendix A1.1.

January 1, 2011, to December 31, 2022, and provide 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.[24,25]

Third, we augment these data with data on every person who had taken a course with the Grinnell College Liberal Arts in Prison Program since 2011. Because all prison education in Iowa is offered by local colleges, our data capture nearly all colleges offering educational opportunities for incarcerated people during this period.[26]

Finally, we incorporate data on labor force participation and wages from IowaWORKS. Due to strict data provision requirements at IowaWORKS, we can obtain only cohort-level data. We describe the process for creating the cohorts, which uses an algorithm similar to that in Autor et al. (2016), in Appendix A1.2. We create the cohorts so that all members of a cohort have the same primary prison, time served, and release year (i.e., our randomization fixed effects), as well as a similar number of courses taken and opportunity score. We set the cohort sizes conservatively 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 had only one opportunity

[24]One important caveat with these 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 nonincarcerated enrollees), we match the IDs of the students in the courses to the IDs of current prisoners to determine which courses were held inside prisons between 2014 and 2018. Unfortunately, we do not have complete records of all individuals in prison. We see only individuals released from prison between 2014 and 2018. Because of this, if a course was offered in prison but was taken exclusively by incarcerated people not released between 2014 and 2018, we would not include the course as having been offered in a prison. This limitation of our data also makes it impossible for us to accurately count the number of students in each class, as people not released before 2018 are missing from these counts even if they were enrolled in the class.

[25]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 address this, we use the official course cap of 28 students to divide the course into multiple sections. 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 sections of this course and count it as three courses starting on that day.

[26]In 2018, the University of Iowa offered college courses to approximately 130 individuals incarcerated at the IMCC. Because we could not 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 that the students who took University of Iowa courses do not bias our results, we drop all individuals released in 2018 who spent the majority of their sentence at the IMCC.

to request labor market information from the agency.

All data were sent to the IDOE, where they were matched on the basis of Social Security numbers. The dataset we received includes 25,992 prisoner–stints, where a prisoner–stint is defined as one complete period of incarceration for one person. From this, we drop all prisoner–stints for which the person could not have been reincarcerated in Iowa because they either died in prison or were released out of state (approximately 11% of the sample).[27] We also drop individuals released in 2018 who spent the majority of their sentence in IMCC, as we discussed above, and individuals for whom we lack information on years of education completed before prison (approximately 1% of the sample). We are left with 22,478 prisoner–stint observations.[28] Approximately 17% of our sample took at least one course.

Table 1 reports summary statistics. We provide means for incarcerated individuals who did not take any courses in the first column and for individuals who took at least one 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 courses diverge on a number of important margins. Given these differences, and the unobservable differences that likely accompany them, any direct comparison of these two groups would likely suffer from selection bias. Importantly, some of these differences persist after we include primary prison, release year, and time served fixed effects in Column 4, suggesting that reduced-form regression analysis would likely return biased estimates of the impact of education due to selection. For example, we find that younger individuals, first-time offenders, and individuals with fewer convictions associated with their current prison stint take more courses while incarcerated. While there is clear selection into course-taking, the sign of the bias is ambiguous: Younger individuals typically have higher recidivism rates, whereas first-time

[27] Of course, it is possible that the individuals released in Iowa left the state, which would prevent us from observing them 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 left Iowa shortly after their release from prison, this would not bias our estimates unless the individuals who participated in prison education were systematically more or less likely to leave the state than other recently released individuals.

[28] Our sample is mostly (85%) men.

offenders recidivate at lower rates than the general population.

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

Reincarceration. Our primary outcome of interest is recidivism, as measured by reincarceration in an Iowa prison after release.[29] 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 can also divide reincarcerations into those for new crimes and those due to revocations.[30]

Employment. We also examine labor market participation after release. While we received cohort-level data from IowaWORKS, we assign the corresponding cohort values to each cohort member and run all regressions at the individual level. As previously discussed, cohorts were created so that all members of a cohort had the same primary prison, time served, and release year (i.e., our randomization fixed effects), as well as a similar number of courses taken and opportunity score. For each cohort, we observe the number of matched individuals, the number of quarters worked and average quarterly wages in the three years after release.

Education. We consider the impact of prison education on future education. To do this, we count community college course enrollment within three years of release. We observe only community college education in our data. If prison education prepared students to pursue a four-year or graduate degree after release, we would not observe such an effect. However, the low number of degrees awarded in our setting makes it unlikely that a significant number of individuals pursue four-year or higher degrees after release.

[29]Only incarceration in a prison within Iowa is observed in our data. We do not observe jail sentences in Iowa or incarceration outside the state, so it is possible that we underestimate reincarceration among individuals who leave the state after release. Alper, Durose and Markman (2018) show that among individuals released from prisons in 30 states (including Iowa) in 2005, 68% were arrested for a new crime within 3 years of release, with 7.7% arrested outside the state of release.

[30]Revocations occur when an individual violates the terms of their release but is 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.

In-Prison Misconduct. The IDOC data record every misconduct incident reported by correctional officers. Each report includes a date and description of the incident, which we categorize as a violent, drug, order, or property offense.[31] One advantage of misconduct relative to the other outcome variables is that we observe misconduct records for each day of a person's sentence, which allows us to leverage variation both within and across individuals.

4 Empirical Model

An obvious challenge the researcher faces in evaluating the impact of prison education is selection bias. As demonstrated in Table 1, individuals who take classes while incarcerated are observably different from individuals who do not. More concerningly, individuals enrolling in education may differ in unobservable ways, including in their 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 do and do not take courses while incarcerated will be biased even after controlling for observable factors.

To overcome this challenge, we create a unique opportunity score that measures the availability of prison courses and use this score to instrument for courses taken. Specifically, we count the number of courses that started each day in each prison. We then sum these courses across each individual's stint of 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.[32]

With the opportunity score defined, we estimate the impact of prison education on our outcomes using a two-stage least squares (2SLS) model on our prisoner–stint-level dataset.

[31] A complete breakdown of how misconduct is categorized can be found in Appendix A1.3.

[32] An example of the variation that we exploit using this educational opportunity score is shown in Appendix Figure A4. In the figure, we depict two prisoners who enter and leave Iowa prisons at the same time and spend most of their time in the same prison. Despite these similarities, we observe variation in their course-taking opportunity because prisoner 1 transfers out of IMCC slightly earlier than prisoner 2 and prisoner 2 transfers out of Prison 1 near the end of their sentence.

In the first stage, we estimate:

$$\text{Courses Taken}_i = \beta_0 + \beta_1 * Opportunity_i + X_i'\Gamma + \lambda_p + \lambda_y + \lambda_l + \epsilon_i \quad (1)$$

where $Opportunity_i$ is the opportunity score for prisoner i during a specific stint in prison and Courses Taken$_i$ is the endogenous prison education variable.[33] $X_i'\Gamma$ is a vector of control variables (i.e., all the variables displayed in Table 1).[34] λ_p, λ_y, and λ_l are fixed effects for primary prison, release year, and time served (in 3-month bins), respectively.[35,36]

We next estimate the second stage as:

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

Under this specification, α_1, the coefficient of interest, is the weighted average effect of prison education among compliers.[37] In both stages, we cluster standard errors by primary prison interacted with release year.

Course-taking opportunity varies with a variety of factors, including primary prison, release year, and time served. Panels A–C of Appendix Figure A6 illustrate average opportunity scores by these factors. However, time served, primary prison, and release year may also be problematically correlated with prisoner characteristics. To isolate the variation in course-taking 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

[33] Appendix Figure A5 shows how the variation in our instrument shifts course-taking behavior. For each level of course-taking, x, we create an indicator for whether an individual has taken at least x courses and use this indicator as the outcome in Equation 1. The resulting estimates and 95% confidence intervals are plotted in the figure. The shape of the figure indicates that most of our variation induced by the instrument shifts individuals from taking no courses to taking one, although there is a notable impact along the intensive margin for individuals taking additional courses.

[34] Our results are also robust to our including the number of prison transfers as a control variable.

[35] Primary prison is defined by the prison of longest tenure.

[36] In addition to absorbing significant variation in course-taking opportunities, the release year fixed effects help us control for the impact of the COVID-19 pandemic, which may have influenced 3-year recidivism rates and other outcomes for individuals released in 2018. We also show robustness of our result to alternative ways of addressing concerns about the effect of the COVID-19 pandemic.

[37] We describe the complier population in Section 5.

primary specifications. We refer to these collectively as "randomization fixed effects."[38]

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 to people being released earlier than they otherwise would have been. Moreover, time served is mechanically correlated with both course-taking and educational opportunities because increased time in prison can only increase an individual's opportunity to take courses, particularly for individuals serving stints later in our sample period. Unfortunately, we are limited in our ability to directly test this possibility because we do not observe sentence length, only the amount of time actually served.

However, we do not believe that our use of time served is concerning for several reasons. First, in conversations with the IDOC, we were explicitly and repeatedly told that participation in education would not directly impact release timing. As discussed in the background section, each prisoner's expected release date is calculated at entry under the assumption that the individual earns all possible time off their sentence for good behavior, and in practice, time is added back only for misconduct. Furthermore, even if education influences time served at the margin, we do not expect this effect to be large enough to change which 3-month time served bin an individual is classed within—an assumption supported by our analysis using an alternative instrument. Additionally, as we further discuss in the results section, our results are robust to both alternative choices for time served bin size and alternative specifications using an instrument defined on course-taking opportunity during the first three months of each person's sentence. This alternative IV specification allows us

[38]As shown in Appendix Figure A3, time served in our data has a 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.

to estimate a model without the time served and release year randomization fixed effects.[39] Second, using OLS models, we find directionally consistent estimates (even when we omit the controls) of the relationship between participation in education and reincarceration.

With the randomization fixed effects in place, we are comparing within individuals in the same prison who were released in the same year and who served similar amounts of time. The remaining variation, then, is based on the precise timing of either prison entry or transfer. This strategy works in part because of the bunching in the timing of when classes begin and the IDOC rule against inmates enrolling in courses that have already started. In Figure 1, we plot the number of courses starting during each week of the calendar year. Because courses are offered through local colleges, which follow an academic calendar, most courses start during a few specific weeks of the year. Thus, an individual who enters just before many courses begin in their prison will have a much higher opportunity to take courses than an otherwise-similar individual who enters a few weeks later.[40] Similarly, an individual who transfers across prisons shortly before a new academic semester begins will have different opportunity to take courses from a similar individual who transfers later.[41]

To serve as a valid instrument, our opportunity score must be relevant, monotonic, and excludable. Relevance implies that course-taking opportunities must be correlated with

[39]Consistent with our discussion in Section 2, this analysis indicates that participation in education may lead to slightly longer, not shorter, time served. Defining the opportunity instrument on the basis of the first three months of a prisoner's sentence allows us to estimate effects on time served. Appendix Table A2 shows that, consistent with the observed increase in misconduct citations after participants start courses, participation in education *increases time served by approximately 1.5%. However, we note that even if participation in education meaningfully reduces time served, this does not violate the exclusion restriction for our educational opportunity instrument because opportunity still impacts outcomes only through education. Instead, this represents another mechanism of the education effect, whereby participation in education leads to earlier release, which, in turn, increases the likelihood of reincarceration. There is strong evidence that increased time in prison reduces future reincarceration (Kuziemko, 2013; Macdonald, 2024) and has little impact on labor market outcomes (Kling, 2006).*

[40]This variation also depends on individuals being able to start courses shortly after entering a prison. In Appendix Figure A7, 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.

[41]This identification also relies on a lack of seasonality in new prison entries and in the timing of transfers. In Appendix Figures A8 and A9, we plot the number of people entering prison or transferring into their primary prison each week, both overall and for specific types of crime. We see no evidence of seasonality in either prison starts or transfers.

actual course participation. The relationship between educational opportunity and actual courses taken is shown in Figure 2. In Figure 2, the light gray bars indicate the distribution of opportunity scores in our sample. The solid line in Figure 2 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. The figure omits opportunity scores of exactly 0, as a large fraction of our sample had no opportunity to take courses while incarcerated.[42] We find, as expected, that individuals with a higher course-taking opportunity score took more courses.

For the monotonicity assumption to be violated in our setting, some individuals would have to have taken fewer courses as their opportunities increased, which seems unlikely.[43] The exclusion restriction implies that educational opportunities must not be correlated with individual outcomes except through their impact on course taking.[44] We provide some evidence for excludability in Columns 4 and 5 of Table 1. Here, we estimate multivariate regressions using courses taken (Column 4) and our opportunity score (Column 5) as dependent variables and the variables listed in Table 1 as the independent variables. We also include our randomization fixed effects. As in the rest of the paper, we cluster standard errors at the primary prison by release year level.

[42] As expected, individuals with an opportunity score of 0 did not take courses while incarcerated. We also trim observations with opportunity scores greater than 4 in Figure 2. Appendix Figure A10 presents the untrimmed version.

[43] Similarity of the first-stage estimates across subgroups can help indicate whether compliers are disproportionately represented within any subgroups (Abadie, 2003). We present our first-stage estimates in Appendix Table A3. In columns 2–7, we present first-stage estimates by subgroup and compare these to the estimates for the entire sample with the ratio $\frac{\alpha_X}{\alpha}$, where X represents a particular subgroup. The first-stage estimates are generally consistent across subgroups, although younger individuals are more likely to be compliers.

[44] We report the results of the joint test of excludability and monotonicity proposed in Frandsen, Lefgren and Leslie (2023) in Appendix Table A4. Because opportunity is conditionally random only within groups of prison, release year, and time served, we include our randomization fixed effects 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 must choose how much weight to put on its slope and fit components. 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 the test statistics to decrease and p-values to increase.

Whereas many of the variables in Column 4 significantly predict courses taken, few of the variables in Column 5 predict educational opportunities. More importantly, the result from the joint F-test reported at the bottom of Column 5 confirms that, collectively, the indicated variables do not predict the opportunity score. This is encouraging, as it suggests that prisoner traits are not driving opportunities to take courses. This is perhaps unsurprising given that the timing and location of college-in-prison programs is not a reflection of prison conditions, as colleges in Iowa often simply serve prisons geographically near them. We include further support for this assumption in Appendix Table A5, which shows balance on outcomes predicted on the basis of these observable characteristics. We find no evidence that any of the predicted outcomes are correlated with opportunity. The point estimates for the predicted outcomes are small and not statistically significant.[45]

5 Results

Before estimating our primary model, we visually examine the relationship between education and reincarceration. In Figure 3, we plot the OLS coefficient estimates and 95% confidence intervals using robust standard errors from regressing reincarceration on courses taken with individuals who did not take courses while incarcerated as the omitted category. Even if education has no direct effect on reincarceration, we might expect Figure 3 to display negative estimates due to selection bias among the individuals choosing to take courses. Instead, most of the estimates are positive and significant at the 5% level, suggesting that participating in education significantly increases reincarceration.

We report our main results in Table 2. Columns 1 and 2 present the OLS estimates of the impact of course-taking with and without the control variables. Columns 3 and 4 show the reduced-form estimates of the impact of educational opportunity with and without the control variables. Finally, in Columns 5 and 6, we display the results from the 2SLS model,

[45] As an additional test, we use individuals who served multiple stints in our sample to estimate whether the opportunity score in a previous stint predicts that for a focal stint. Among these cases, we find no evidence that previous opportunities impact future opportunities ($f = 0.10$ and $p = 0.76$).

which uses equations (1) and (2) to estimate the marginal impact of an additional course on the 3-year recidivism rate. We report the Kleibergen–Paap F statistic for the relevance of our instrument in these columns. Here, and throughout the paper, we observe high and significant F statistic values that suggest our instrument strongly predicts course-taking (Kleibergen and Paap, 2006).

In Panel A 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. We estimate larger increases in reincarceration with our IV strategy, consistent with positive selection into education (and with the positively selected individuals being less likely to be reincarcerated, all else equal). Column 6 shows the results from our preferred model, which includes a robust set of controls. The estimates suggest that taking one additional course increases the probability that an individual will recidivate by 2.3 percentage points or 5.2%. However, the result is only marginally significant.

There are two reasons an individual may be reincarcerated: They may be convicted of a new crime, or they may violate the terms of their release, sometimes without having committed a new crime. In the remainder of Table 2, we show results for the different categories of reincarceration: those attributable to new crimes in Panel B and those arising from revocations 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 3.4 percentage points or 10.1%. Our estimates on reincarceration for a new crime, while positive, are small and not statistically significant.

Our IV estimates are valid only for compliers. To better understand the relevance of our estimates, it is useful to characterize the complier population. While we cannot identify individual compliers, we can measure their average observable characteristics (Frandsen, Lefgren and Leslie, 2023). In Appendix Table A6, we present average traits for our entire sample and for compliers. The basic idea is that if opportunity has a stronger relationship with course-taking in a particular subsample, compliers in that subsample are weighted more

heavily. We also report the ratio of the complier-weighted mean to the sample mean. Overall, the compliers in our sample are reasonably similar to the population as a whole, with a few exceptions. Younger prisoners and those with violent crime charges are overrepresented among the compliers.[46]

Robustness. A reasonable concern is that these results are driven by the correlation between time served and courses taken. Individuals who serve longer periods mechanically may have more opportunities to take courses, and longer incarceration may also be related to reincarceration probability. Moreover, there may be concerns that taking courses may affect how long an individual serves.[47] In this section, we show that our results are robust across a variety of alternative specifications that explore different ways of addressing these concerns. First, in Appendix Table A8, we show that our results are similar when we use 1-month or 6-month bins for time served instead of the 3-month bins in our preferred specification. Second, we show that our results are robust to alternative specifications of the model. Specifically, we define an alternative instrument based on opportunity during the first three months of each person's sentence, rather than total time served; the corresponding results are shown in Appendix Table A9.[48] This alternative IV specification allows us to estimate the 2SLS model without the time served or release year fixed effects, including start year fixed effects instead. The estimates are qualitatively similar to our main results.

Another potential concern is that the COVID-19 pandemic may play a role in our results because we use a three-year reincarceration window for individuals released as late as December 31, 2018. To explore this possibility, we limit the sample to individuals released before March 1, 2017, as their outcomes three years after release are unlikely to have been affected by the pandemic. We present these results in Appendix Table A10; they are consistent with

[46] Appendix Table A7 reweights the OLS estimates to match the complier population in groups based on age (older or younger than 25) and whether the case included a violent crime charge. We include our baseline OLS and 2SLS estimates for comparison.

[47] As previously discussed, our understanding of the institutional context suggests that taking courses should not in itself affect time served and that only misconduct in prison can change an inmate's predetermined exit date. Please see our discussion in Section 2 for more information.

[48] Approximately 95% of our sample served at least three months.

our main findings, although the coefficients are generally larger.

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 outcomes may be mechanically reduced given our findings on reincarceration. We present these results in Table 3. In Panel A, we show the impact of prison education on labor market outcomes within three years of release, including the probability of employment, number of quarters with work, and average quarterly wages. We find that individuals participating in prison education are likelier 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% relative to the mean of 77%. On the other hand, we do not find significant gains in the number of quarters worked or quarterly earnings. In Panel B, we show the impact of prison education on future education, specifically community college courses, within three years of release. The results suggest that prison education does not significantly influence participation or performance in community college courses. The point estimates are small and not statistically significant; the negative point estimates might suggest that people who take prison courses while incarcerated may be less likely to pursue education after release.

5.2 Heterogeneity by Race and Course Type

Prisoner Race. Iowa prisons are more racially diverse than is the state as a whole. For example, 23% of the inmates in our sample are Black despite Black people making up only 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 prison education. In Panel A of Table 4, we estimate the effects on recidivism separately for white and nonwhite individuals. We find that the estimated

effects for reincarceration and revocations are driven by white inmates. The results for white inmates not only are statistically significant at the 5% level but also are much larger in magnitude than those for their nonwhite counterparts.

Course Type. Thus 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 prison education. For example, it is reasonable to anticipate that HSE or remedial education might impact inmates differently from postsecondary education.[49] In Panel B of Table 4, we present results for our instrument defined separately for HSE or remedial courses (Columns 1–3) and postsecondary courses (Columns 4–6). With this breakout, we see that the increase in reincarceration and revocations is driven by participation in postsecondary education.

5.3 Placebo Test for Spillover Effects

A reasonable concern about our identification strategy is that increased educational opportunities may change the outcomes of individuals who do not participate in courses through spillovers from peers. We can partially address this concern by looking at longer-term post-release outcomes, which may be less affected by an individual's peers in prison. However, because the effects that we identify are concentrated among those participating in postsecondary courses, in Table 5, we investigate the potential for spillovers by examining effects for individuals with (Panel A) and without (Panel B) 12 years of education upon prison entry.[50] The idea here is that individuals with fewer than 12 years of education are not able to take postsecondary courses. Therefore, any estimated effects of postsecondary course-taking for this group likely reflect spillovers.[51]

We find no evidence of significant spillovers in our setting. The estimated effects are

[49] It would also be interesting to explore whether particular types of courses (e.g., vocational, liberal arts) drive the estimated effects. However, postsecondary courses make up a relatively small portion of the total courses offered, such that we are underpowered to further explore subcategories of these courses.

[50] Because we do not directly observe GED status, we use years of education as a proxy in our data.

[51] We also test whether individuals with 12 years of education are impacted by HSE or remedial courses, although our 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 postsecondary courses.

concentrated among individuals with at least 12 years of education at prison entry. In Panel A of Table 5, we find no evidence that taking HSE or remedial courses impacts individuals who had at least 12 years of education prior to prison. Similarly, in Panel B, we find no significant effects of postsecondary courses on outcomes for individuals who entered prison with fewer than 12 years of education. Unsurprisingly, the first-stage F statistic for this group is quite low, implying that opportunities to take postsecondary courses do not lead individuals with fewer than 12 years of education to take such courses. Appendix Table A11 replicates this analysis using the reduced-form model and produces similar results. Overall, these findings suggest no large spillover effects (from peers or otherwise) when educational opportunities increase in our context.

6 Mechanisms: Exploring the Increase in Reincarceration and Revocations

Our results so far pose an important question. Why does participation in education increase reincarceration? Our finding that the effects are driven by revocations rather than new crimes hint at one possible explanation. A growing literature demonstrates that prisoners released into higher levels of supervision are likelier to be reincarcerated (Sakoda, 2023; Arbour and Marchand, 2024). Particularly relevant to this study is the work by Lee (2023), who finds that individuals who exit prison into work release (i.e., a halfway house) instead of parole were likelier to be reincarcerated because of stricter rules and more intensive supervision. This more intensive supervision creates many opportunities for individuals to make mistakes that would either go unnoticed or not be considered violations if they were on parole. As in this study, the increase in reincarceration was also driven by revocations. Therefore, education in our setting might influence *how* individuals are released from prison.

We begin with visual evidence in Figure 4, where we plot OLS estimates of the relationship between course-taking during incarceration and the probability of exit into free release,

parole, or work release.[52] We see clear evidence that course-taking during incarceration is negatively correlated with the likelihood of release without post-release supervision. To confirm this finding, we reestimate our preferred IV specification using release type as the outcome. Columns 1–3 of Table 6 show how course-taking changes the probability of free release, parole, or work release, respectively. We find that participating in education increases the likelihood of assignment to work release by 5 percentage points or 14.4%.[53] Note that our finding of an increasing probability of assignment to work release is also consistent with the estimated effects on the probability of post-release employment. One of the key aspects of work release is that individuals are required and given additional support to actively seek employment. This may partially explain our finding of increased employment match probability without effects on the intensive margin of employment.[54]

With the relationship between participation in education and release type established, we next analyze the extent to which release type factors into the estimated effects on reincarceration and revocation. One way to explore whether the increases we observe in reincarceration and revocations are driven by an increase in the probability of work release is to directly compare our estimates with the results of Lee (2023). Lee finds that assignment to work release instead of parole increases an individual's probability of reincarceration and 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 at least 51% and 61% of the effects estimated here on reincarceration and revocation, respectively.[55]

Next, we conduct a mediation analysis similar to that presented in Finkelstein et al. (2025)

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

[53]One way to interpret these findings is substitution from no post-release supervision to work release. It is also possible that these results represent 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.

[54]We also present prisoner descriptive statistics by release type in Appendix Table A12, which also shows greater course-taking among individuals sent to work release.

[55]This estimate is conservative because it assumes that substitution along the extensive margin (i.e., a shift from no supervision to any level of supervision) has the same effect as intensive-margin substitution (i.e., a shift from parole to work release). This is unlikely to be true. For example, revocation is possible only while the individual is under supervision; therefore, the effect on revocations of a shift from no supervision to any level supervision should be greater than that of a move between levels of supervision.

in which we directly control for the mediator variable (i.e., release type) using reduced-form models, comparing the coefficients across models with and without the release-type variables. This approach relies on the assumption that any unobservable mediating factors are uncorrelated with either release type (the observable mediator) or observable variables determined before entry into prison (other controls included in our model) (Heckman and Pinto, 2015; Fagereng, Mogstad and Rønning, 2021; Heckman, Pinto and Savelyev, 2013). We present these results in Table 7, finding no evidence that education influences reincarceration or revocations when release type is controlled for.[56] Moreover, the inclusion of these controls significantly reduces the magnitude of the coefficients. We take these suggestive analyses of mechanisms as evidence that the effects we identify come largely from participation in prison education changing the release type.

6.1 Discussion: Why Might Release Type Change?

A final puzzle is why participation in education causes people to exit into work release, i.e., under more intensive supervision. Unfortunately, our data are not well suited for a thorough analysis of this question, so this section will proceed in a more speculative manner.

While Iowa has a parole board, it often is not the primary decision-maker about either the timing of an individual's release or their post-release supervision level. Release timing is determined primarily through the system of earned time described in Section 2. Prisoners automatically earn time as long as they are either working or participating in GED education and avoid misconduct. Post-release supervision intensity is essentially determined by case manager recommendations to the parole board. Although the board has official jurisdiction and can override either the release date or the level of supervision, it rarely does so in practice.[57] Therefore, for our purposes, release decisions in the IDOC are based almost

[56] In Appendix Table A13, we conduct a similar mediation exercise using the education outcomes and find that our baseline results are unchanged. We are unable to implement a mediation analysis for employment outcomes because over 70% of the cohorts include individuals released to different supervision levels, which prevents accurate mapping of average employment outcomes to supervision levels at release.

[57] The vast majority (93%) of case manager recommendations are accepted by the parole board.

entirely on the recommendations of case managers.

Our conversations with case managers and other IDOC personnel revealed three important factors that may push case managers to increasingly recommend work release for participants in prison education. First, education, particularly postsecondary education, is generally viewed favorably in release decisions.[58] Case managers positively weight the initiative shown by an inmate's voluntary course-taking. Second, 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 upon release. Work release may also be viewed as providing an opportunity for additional programming for individuals who need it.[59] Finally, misconduct during incarceration may influence a case manager's perception of an individual's risk level or rehabilitation, including whether they may benefit from additional programming, which may be facilitated by work release. 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. 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, misconduct, and other factors not captured in our data. Therefore, while an analysis of misconduct is likely to provide important insights, we doubt that misconduct citations are dispositive. Moreover, misconduct citations are relatively infrequent, with the average prisoner in our data receiving approximately one every

[58]This may also explain our finding that the effects are concentrated among postsecondary courses. Unlike HSE/remedial courses, which are required, when available, for individuals without a high school degree, postsecondary courses are optional. Therefore, case managers might consider participation in postsecondary education an important signal when making release recommendations.

[59]A prevailing view in the criminology and corrections literature is that housing insecurity or housing in high-crime neighborhoods poses a significant barrier to successful reentry (Morenoff and Harding, 2014; Lutze, Rosky and Hamilton, 2014; Western et al., 2015; Clark, 2016; Leasure and Martin, 2017; Keene, Smoyer and Blankenship, 2018). Work release programs such as the one in Iowa were developed, in part, as a solution to this problem. Lee (2023) documents significant variation across case managers in the propensity to assign to work release, which may reflect variation in views of the utility of work release. Appendix Table A12 presents descriptive statistics for individuals by post-release supervision type. These characteristics do not indicate strong negative selection among individuals assigned to work release. On average, those assigned to work release serve longer sentences, which may be partly explained by their having more convictions, particularly felony convictions. However, they are less likely to have violent charges associated with their convictions and likelier instead to have drug- and public-order-related charges.

5 months.

To investigate the relationship between course-taking and misconduct, we construct a balanced panel at the prisoner–week level and use an event study framework to understand how misconduct changes when an inmate starts their first course in prison. We estimate an event study rather than the 2SLS specification used throughout our analysis because, in contrast to the other outcomes we study, misconduct occurs during an individual's sentence rather than after their release. This means that temporal variation in misconduct may be important to consider. For example, higher levels of misconduct, or particularly severe (e.g., violent) misconduct early in a prisoner's sentence, could preclude them from taking courses (e.g., if they commit violent misconduct and are placed in solitary confinement when the course is starting). We estimate Poisson regressions with weekly misconduct citations as our dependent variable. Appendix Table A14 reports these results. Each regression includes individual and prison fixed effects and flexible controls for fraction of time served decile. The first column presents the results for total misconduct, while the subsequent columns present the results for specific types of misconduct. The Poisson regression coefficients represent semi-elasticities, which, to a first-order approximation, correspond to percentage changes in the expected outcome. We find that participating in education increases nonviolent misconduct. We do not find significant effects on violent misconduct. Accordingly, we hypothesize that the estimated effects of prison education on release type reflect an effort by case managers to weight the positive signal from participation in education relative to its negative effects such as an increase in misconduct.

There are a number of potential explanations for the increase in misconduct that we observe. For example, in most Iowa prisons, the education building is separate from inmate living areas, creating both additional opportunities to engage in activities that may result in misconduct citations and more opportunities to interact with the correctional officers responsible for issuing citations. During the course, there is additional movement on the part of prisoners. This may create more opportunities for prisoners to interact with each

other, potentially establishing new networks, which may promote transfers of contraband. Education also provides access to some materials that would otherwise be limited, including computers, paper, and writing implements, increasing the potential for these materials to be abused or stolen, which would result in more nonviolent misconduct citations. These factors all likely contribute to increased misconduct while the course is taking place. However, some of these effects should dissipate once the course ends.

Finally, both anecdotal evidence from other settings and our conversations with Iowa corrections staff point to an additional possibility. Some officers feel that "free" prison education, particularly postsecondary education, is deeply unfair. Most correctional officers are not college educated, and many believe the inmates are being given opportunities that the officers themselves were never afforded.[60] Similar sentiments were also initially expressed among jail staff where Project IGNITE was first implemented. A news article notes that staff "initially struggled with the idea, wondering why the jail was providing free education for lawbreakers 'when a lot of us have to go out and pay for our own education'" (Barrett and Greene, 2023). The article highlights:

> Rolling out IGNITE ... was not easy. "This was a shock to the jail culture," says [Genesee County Sheriff Christopher] Swanson, who reports that correctional officers would say, "This is insane. We're doing what? We're bringing in teachers? We're providing tablets? Are you kidding?" (Barrett and Greene, 2023)

Eventually, staff members warmed up to the program, seeing its benefits, and even participating in graduation celebrations with inmates and their families. This experience is likely not universal. Therefore, it seems reasonable to hypothesize that the drastic cultural change in the jail, particularly among staff, likely plays a role in explaining the differences between the misconduct effects estimated here and those reported for the IGNITE program. Because of discretion in determining what constitutes misconduct, particularly nonviolent miscon-

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

duct, such resentment may make officers likelier to issue misconduct citations in marginal cases for people participating in education. While we are unable to test this hypothesis in our setting, discussions with staff from prison education programs in Massachusetts, New York, and California provide qualitative evidence that correctional officers in other contexts express similar sentiments, which influence their actions. For example, a faculty member in one prison education program noted that "staff do sometimes complain about access inequities, perceiving an injustice if incarcerated students have access to a lower-cost education than staff have access to" and that "staff, at least anecdotally, occasionally describe paying especially close attention to communications between incarcerated students and educational providers, in a targeted effort to identify possible breaches of safety and security protocols arising through educational access."

7 Conclusion

This paper estimates the impact of prison education on several post-release outcomes including reincarceration, employment, and education. We find that participation in education leads to individuals exiting prison under higher levels of post-release supervision, increasing their probability of ending up on work release, which in turn causes higher rates of revocations and reincarceration. Consistent with the higher rates of assignment to work release, we also find that participation in education significantly improves the likelihood of employment within three years of release. We find no effects on other aspects of employment or education. While the scope of our data limits our ability to explore why participation in education might influence individuals' terms of release, our hypothesis, based on discussions with IDOC and other correctional staff, is that these effects likely operate through case managers' release recommendations and their attempt to balance the positive signals from participation in education with its negative consequences such as an increase in misconduct citations after individuals start taking courses.

Our study has important policy implications. Crucially, we do not think that this research

should be used to retrench prison education in the US. The median number of courses taken among people in our sample who took at least one course is only two. While we expect this number to be similar across many prison education programs around the country, it is also likely insufficient for prisoners to meaningfully develop their human capital and reap the associated benefits. As we previously mentioned, most of our sample (96%) did not earn any degree or accreditation while incarcerated. In our setting, any benefits that may have accrued from education are more than offset by its unintended consequences. Specifically, participation in education appears to impact how individuals move through the correctional process because of changes in how they are treated by prison staff.

Our results point to three important considerations. First, the findings are a stark reminder of the potential for unintended consequences to derail policies that may otherwise be beneficial and of the need to account for how institutional agents may respond to interventions. In our setting, individuals who participate in education should not be released differently from those who do not. Second, prison education offerings should be deliberate to ensure students have the ability to earn degrees or certificates. Some evidence—albeit scarce for US prisons—suggests that better targeting of the type of education offered (e.g., vocational, career, technical) could also improve outcomes. Finally, for education to be effective in US carceral settings, there may need to be a cultural shift among both prisoners and staff. Alsan et al. (2025) highlight how Project IGNITE offered education but centered opportunities to celebrate that education and crucially worked to generate buy-in among prison staff to further the goals of the program. In our setting, institutional support for education is much weaker, with some programs facing the threat of administrative elimination and anecdotal evidence of correctional officers often targeting students pursuing postsecondary education.

Future research should explore what happens when educational programs are scaled such that people can take enough classes in prison to earn accreditation and develop useful skills. Given the recent evidence that intensive, wraparound programs with an educational component can significantly reduce reincarceration, it is critical to understand the role of education

and degree-earning in delivering these positive outcomes.

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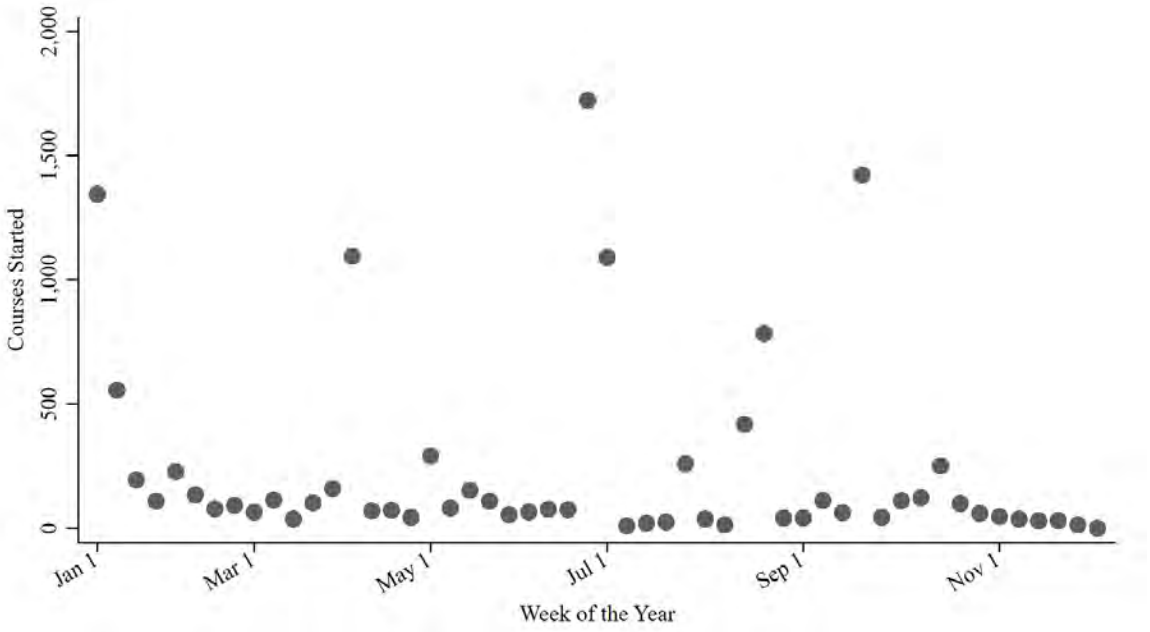
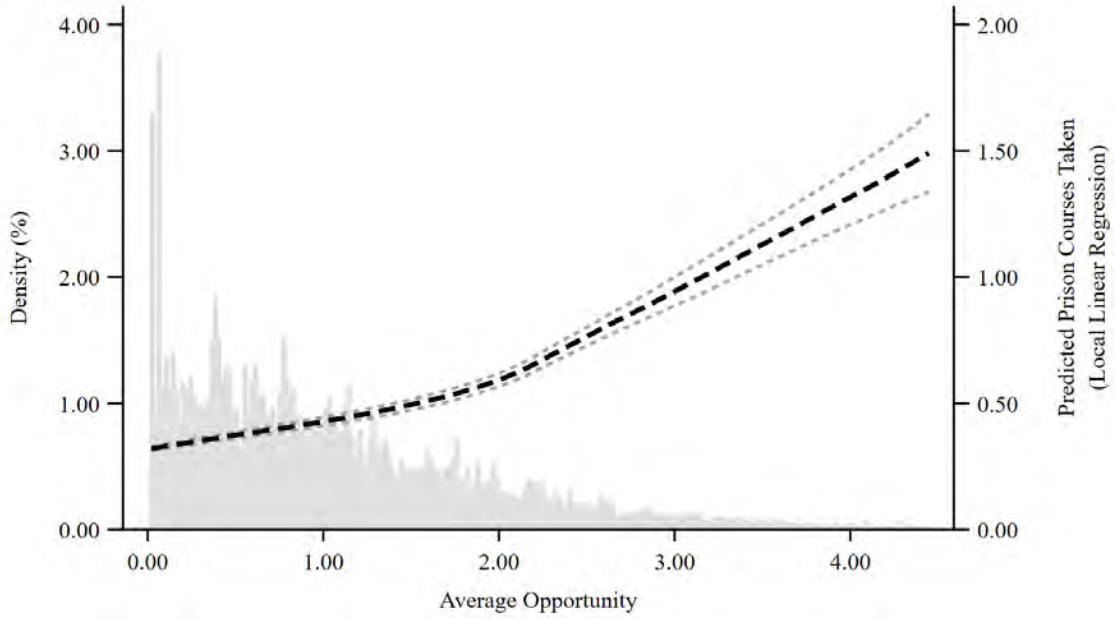
8 Figures & Tables

Figure 1: Random Variation in Timing of Course Starts, 2014–2018



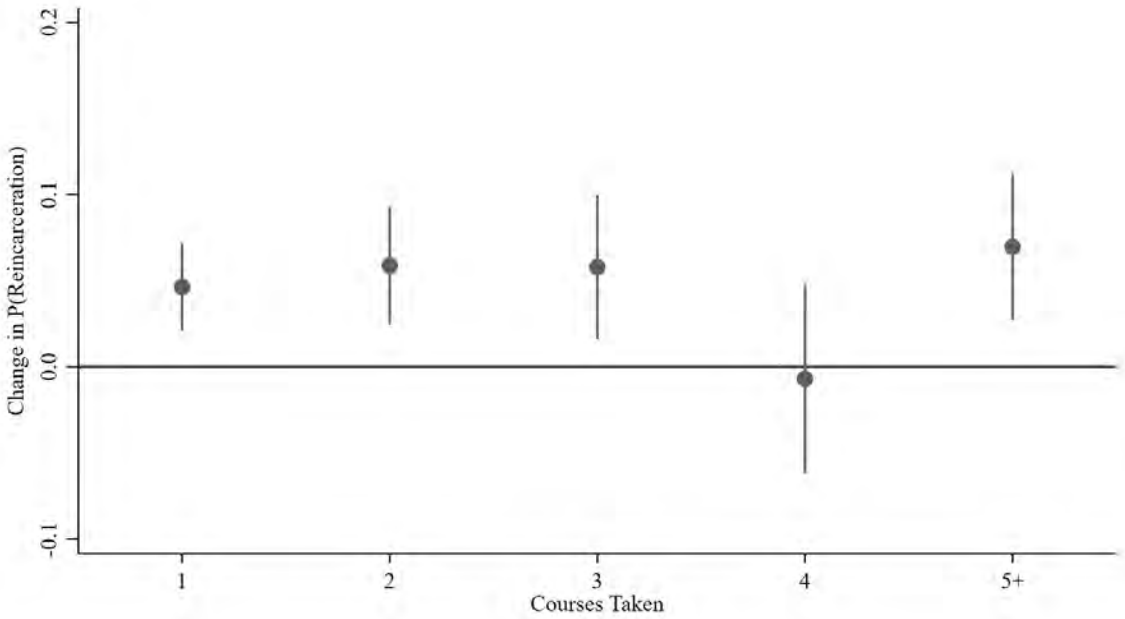
Notes: This figure displays the number of courses starting each week of the year during our primary sample period from January 1, 2014, through December 31, 2018.

Figure 2: Increased Course-Taking Opportunities Predict More Participation in Prison Courses



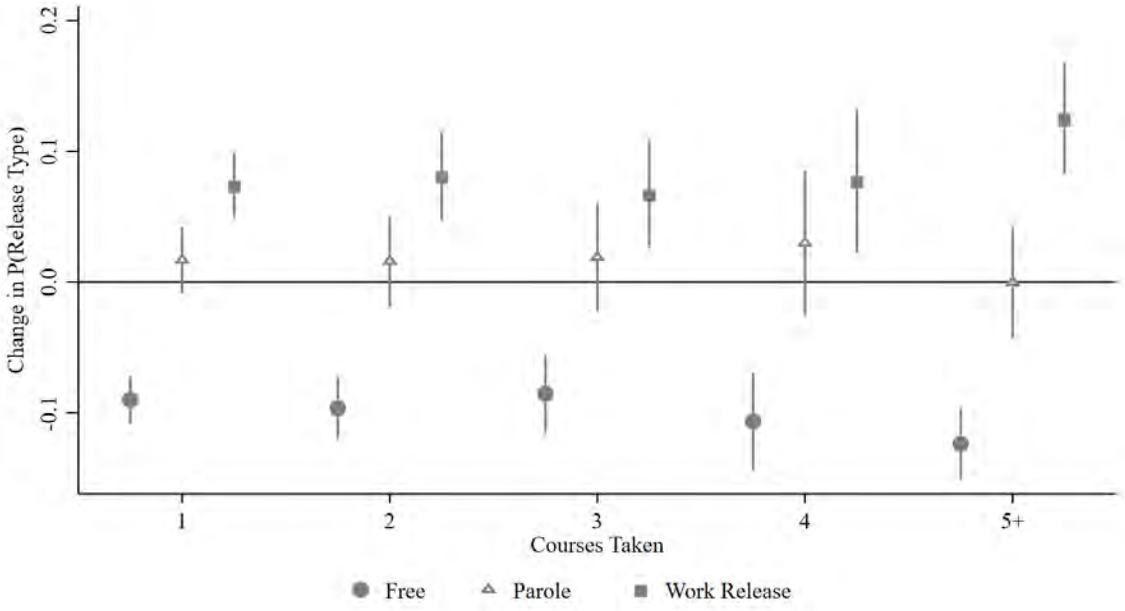
Notes: This figure displays a histogram of the course-taking opportunity instrument described in Section 4. The solid line represents a local linear regression of the number of courses taken on the opportunity instrument. The 95% confidence interval is indicated with dashed lines. We omit prisoners with an opportunity score of 0 from the figure to preserve the density scaling. Every individual with an opportunity score of 0 took 0 courses while incarcerated. We also omit the top 1% of opportunity scores from the figure. Appendix Figure A10 shows the figure without these omissions.

Figure 3: Course-Taking and Reincarceration with Three Years of Release



Notes: This figure plots 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 4: Course-Taking 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 & Balance Test

	No Courses	1+ Course	Difference	Courses Taken	Opportunity
Age 25-29	0.189	0.214	0.025***	-0.131***	-0.015
			(0.008)	(0.040)	(0.014)
Age 30-35	0.216	0.209	-0.008	-0.180***	-0.015
			(0.010)	(0.039)	(0.015)
Age 36-43	0.185	0.151	-0.034***	-0.236***	-0.021
			(0.010)	(0.040)	(0.014)
Age 43+	0.209	0.108	-0.101***	-0.361***	-0.017
			(0.007)	(0.053)	(0.017)
Prior Recidivist	0.465	0.387	-0.078***	-0.083***	0.004
			(0.015)	(0.027)	(0.010)
Felony Convictions	1.556	1.787	0.231***	0.015***	0.006*
			(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***	0.051	0.032*
			(0.018)	(0.033)	(0.018)
Any Drug Crime?	0.378	0.376	-0.002	0.010	-0.015
			(0.010)	(0.017)	(0.012)
Any Property Crime?	0.422	0.473	0.051***	0.001	0.009
			(0.013)	(0.022)	(0.011)
Any Public Order Crime?	0.395	0.364	-0.031***	-0.013	0.006
			(0.009)	(0.016)	(0.012)
White	0.705	0.604	-0.102***	-0.045**	0.010
			(0.017)	(0.022)	(0.009)
Years of Education	11.669	10.933	-0.736***	-0.144***	-0.002
			(0.072)	(0.027)	(0.004)
Violence Score	5.868	5.376	-0.491*	0.001	0.000
			(0.253)	(0.004)	(0.003)
Missing Violence Score	0.126	0.054	-0.072***	-0.088	-0.005
			(0.013)	(0.079)	(0.050)
Observations	18,594	3,884	22,478	22,478	22,478
F-Stat				5.76	1.11
P-value				0.00	0.37

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 and the opportunity score as the dependent variables. These regressions include our randomization fixed effects. Both columns also report the F statistic and p value from a joint test of significance. Standard errors clustered at the prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effect on 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*	0.023*
	(0.002)	(0.002)			(0.015)	(0.013)
Opportunity			0.012*	0.011*		
			(0.006)	(0.006)		
Outcome Mean	0.445	0.445	0.445	0.445	0.445	0.445
Panel B. New Crime						
Courses Taken	0.005***	0.004**			0.011	0.007
	(0.002)	(0.002)			(0.012)	(0.010)
Opportunity			0.005	0.003		
			(0.005)	(0.004)		
Outcome Mean	0.268	0.268	0.268	0.268	0.268	0.268
Panel C. Revocation						
Courses Taken	0.008***	0.007***			0.035**	0.034**
	(0.002)	(0.002)			(0.016)	(0.015)
Opportunity			0.016**	0.016**		
			(0.007)	(0.007)		
Outcome Mean	0.337	0.337	0.337	0.337	0.337	0.337
Observations	22,478	22,478	22,478	22,478	22,478	22,478
First Stage F-Stat					141.64	131.16
Randomization FE	X	X	X	X	X	X
Controls		X		X		X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. Columns 1 and 2 report OLS estimates for recidivism on courses taken. Columns 3 and 4 report reduced-form estimates for recidivism on the opportunity score. Columns 5 and 6 report the results from estimating our 2SLS model (Equations 1 and 2). Within column pairs, the first column includes only our randomization fixed effects, while the second includes both our randomization fixed effects and the full set of controls. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effect on Other Outcomes Within Three Years of Release

	Panel A. Employment and Wages			
	P(Employed)	Quarters Worked	Quarterly Wages	Nonzero Quarterly Wages
Courses Taken	0.031***	0.179	50.934	-60.367
	(0.010)	(0.116)	(76.657)	(104.101)
Outcome Mean	0.771	4.432	1,839.952	4,732.242
Observations	21,647	21,647	21,647	21,647
First Stage F-Stat	92.53	92.53	92.53	92.53
	Panel B. Education (Community College Courses)			
	Courses	Credit Courses	Noncredit Courses	Passed Courses
Courses Taken	-0.013	-0.006	-0.008	0.008
	(0.053)	(0.044)	(0.022)	(0.038)
Outcome Mean	0.562	0.292	0.270	0.356
Observations	22,478	22,478	22,478	22,478
First Stage F-Stat	131.16	131.16	131.16	131.16
Randomization FE	X	X	X	X
Controls	X	X	X	X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. Panel A includes employment-related outcomes within three years of release, while Panel B covers enrollment in community college courses within three years of release. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects on Recidivism by Race and Course Type

	Reincarceration	New Crime	Revocation	Reincarceration	New Crime	Revocation
	Panel A. Heterogeneity by Race					
	White			Nonwhite		
Courses Taken	0.043**	0.018	0.051**	-0.000	-0.006	0.016
	(0.018)	(0.016)	(0.020)	(0.017)	(0.013)	(0.018)
Outcome Mean	0.442	0.267	0.332	0.450	0.269	0.346
Observations	15,453	15,453	15,453	7,025	7,025	7,025
First Stage F-Stat	81.77	81.77	81.77	97.02	97.02	97.02
	Panel B. Heterogeneity by Course Type					
	HSE or Remedial Courses			Post-Secondary Courses		
Courses Taken	0.015	0.015	0.023	0.040**	0.006	0.059***
	(0.022)	(0.018)	(0.027)	(0.018)	(0.014)	(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	88.50	88.50	88.50	155.51	155.51	155.51
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. Panel A examines effect heterogeneity by race (white versus nonwhite). Panel B investigates effect heterogeneity by course type (HSE or remedial courses versus post-secondary courses) by redefining the opportunity instrument for course type. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Placebo Test for Spillovers 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	0.010	0.023	0.029	0.033*	0.008	0.052***
	(0.047)	(0.041)	(0.052)	(0.018)	(0.013)	(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	120.12	120.12	120.12	163.59	163.59	163.59
Panel B. Individuals with Fewer Than 12 Years of Education						
Courses Taken	0.019	0.009	0.015	0.297	-0.095	0.315
	(0.017)	(0.013)	(0.017)	(0.312)	(0.311)	(0.305)
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	85.15	85.15	85.15	5.49	5.49	5.49
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents 2SLS regression results from estimating the effect of prison education on recidivism separately for HSE or remedial and postsecondary courses in a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. We investigate the possibility of spillover effects by estimating separate regressions for individuals with and without 12 years of education. Because our effects are concentrated among postsecondary courses, any estimated effects among individuals without 12 years of education are likely to reflect spillover effects. Panel A presents these results for individuals with at least 12 years of education upon entering prison. Panel B shows the results for those with fewer than 12 years of education when they entered prison. We include the reduced-form version of these results in Appendix Table A11. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Does Education Change Release Type?

	Free	Parole	Work Release
Courses Taken	-0.049**	-0.001	0.050**
	(0.021)	(0.019)	(0.023)
Outcome Mean	0.219	0.433	0.347
Observations	22,478	22,478	22,478
First Stage F-Stat	131.16	131.16	131.16
Randomization FE	X	X	X
Controls	X	X	X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Does Release Type Mitigate Recidivism Outcomes?

	Reincarceration	New Crime	Revocation	Reincarceration	New Crime	Revocation
Opportunity	0.011*	0.003	0.016**	0.004	0.003	0.005
	(0.006)	(0.004)	(0.007)	(0.005)	(0.004)	(0.005)
Parole				0.126***	-0.054***	0.313***
				(0.011)	(0.010)	(0.010)
Work Release				0.283***	0.017	0.494***
				(0.012)	(0.012)	(0.012)
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
Randomization FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X

Notes: This table presents reduced-form regression results from estimating the effect of opportunity on recidivism using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A1 Data Appendix

A1.1 Course Data

In this section, we provide details about the courses offered in prisons in Iowa between 2014 and 2018 and the colleges offering them. Most of the courses in our sample are not for credit (81%), and for some of these noncredit courses, we do not observe whether the student successfully completed the class. Among the classes for which we have information on whether the student passed, 97% of the students passed. This fraction is 94% if we restrict attention to the courses offered for credit.

Colleges Offering Courses

College Name	Students Enrolled	Courses	Prisons Served
Des Moines Area Community College	2124	282	ICIW, NCF
Grinnell College	266	76	NCF
Iowa Central Community College	4191	354	FDCF, NCCF
Iowa Western Community College	1288	149	CCF
Kirkwood Community College	677	164	ASP, IMCC
Southeastern Community College	1655	191	ISP, MPCF

Notes: Some courses have multiple sections, but we do not see the individual sections in our data. Thus, courses with more than the official course cap of 28 students are counted as multiple sections. We also do not observe students who participated in these courses but were not released between 2014 and 2018, meaning that the number of students enrolled that we calculate is likely underestimated.

Top 10 Courses by Student Enrollment

Course Name	Students Enrolled	Category	Subcategory
Basic Skills and Developmental/Remedial Education	5404	HSE or Remedial	Remedial
High School Equivalence Certificate Program	1049	HSE or Remedial	HSE
Welding Technology/Welder	882	Post Secondary	Blue Collar
Logistics, Materials, and Supply Chain Management	369	Post Secondary	White Collar
Machine Tool Technology/Machinist	341	Post Secondary	Blue Collar
Liberal Arts and Sciences/Liberal Studies	225	Post Secondary	Liberal Arts
Developmental/Remedial English	214	HSE or Remedial	Remedial
Business/Office Automation/Technology/Data Entry	207	Post Secondary	White Collar
Occupational Safety and Health Technology/Technician	196	Post Secondary	Other
Baking and Pastry Arts/Baker/Pastry Chef	149	Post Secondary Training	Other

Notes: This table lists the top ten course descriptions by total enrollment. This list represents 89% of the course enrollments in our data.

Table A1: Participation Statistics by Course Type

Course Type	Participation Rate	Participants	Average Courses|Participation
All Courses	0.17	3,884.00	2.63
	[0.38]		[2.36]
HSE or Remedial Courses	0.14	3,074.00	2.21
	[0.34]		[1.65]
Post-Secondary Courses	0.05	1,019.00	3.35
	[0.21]		[3.34]
HSE Courses	0.02	553.00	2.00
	[0.15]		[1.05]
Remedial Courses	0.12	2,791.00	2.04
	[0.33]		[1.57]
Blue Collar Training Courses	0.01	307.00	4.46
	[0.12]		[3.32]
White Collar Training Courses	0.01	180.00	2.75
	[0.09]		[1.92]
Liberal Arts Courses	0.01	139.00	3.68
	[0.08]		[3.43]

Notes: This table shows a breakdown of the types of courses taken in our sample. Column 1 shows the proportion of individuals in the sample who participated in at least one class of the indicated type. 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. HSE stands for high school equivalence and represents students pursuing a GED or HiSET degree. Standard deviations are reported in brackets.

A1.2 Labor Market Outcomes Data

The wage and employment data in this project come from IowaWORKS. Unfortunately, because of 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 had only one opportunity to request this labor market information. Using Carson et al. (2021), we predicted that approximately two-thirds of the individuals in our sample would have gainful employment in the three years after release. We used this value to calculate an optimal cohort size of 9, which would keep the cohorts as small as possible while minimizing the number of cohorts likely to be suppressed by IowaWORKS.

To construct our cohorts, we follow a method similar to the algorithm developed in Autor et al. (2016). We group individuals on the basis of a list of characteristics including primary prison (i.e., one of the nine prisons in Iowa), release year (e.g., 2014), time served (in 6-month bins up to 5 years), number of courses taken (winsorized at 2), and opportunity score.[61,62,63] The list of variables below is also used to create our cohorts in the following order:

1. Release type (i.e., free release, parole, or work release)
2. Race (i.e., white or nonwhite)
3. Age at sentence start quintiles
4. Years of education before sentence (i.e., indicator for 12 or more years).

We constructed our cohorts so that there are between nine and 17 individuals in each cohort. The number of variables used in the grouping varied across cohorts. In some cases, a cohort reached 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 were not further subdivided according to the next variable. If a cohort had fewer than nine individuals, we sorted on the current matching variable and merged the cohort with an adjacent cell with nine individuals. The resulting cohort was not subdivided any further. After matching on all variables, we split all the cohorts in the maximum number of cells with size 9 or more.

Following this procedure, we created cohorts with the following distribution of cohort size. All cohorts contain members with the same primary prison, time served, release year (i.e., our randomization fixed effects), as well as a similar number of courses taken and opportunity score. We also include in the table the distribution of matched cohorts received from IowaWORKS. Under our approach, only 0.4% of the provided cohorts were suppressed.

[61] Because the distribution of time served has a long right tail, individuals who served 5 or more years are assigned the same time served value for the purposes of cohort creation.

[62] Individuals with more than 1 class are grouped together to address the long-right-tailed distribution.

[63] The opportunity score is rounded to the closest integer and winsorized at 3.0, creating 31 bins.

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

A1.3 Misconduct Categories

We use the description text field in the data to classify all misconduct citations into four categories: drug, order, property, and violent. Below, we list the text descriptions included in each category.

Drug: Abuse of Medication; Adulteration of Food or Drink; Being Intoxicated or Under the Influence; Dealing in Dangerous Drugs/Intoxicants; Medication Violations; Possession of Dangerous Contraband; Possession of Drugs, Intoxicants; Possession/Manufacture of Drugs, Intoxicants; Unauthorized Possession/Exchange.

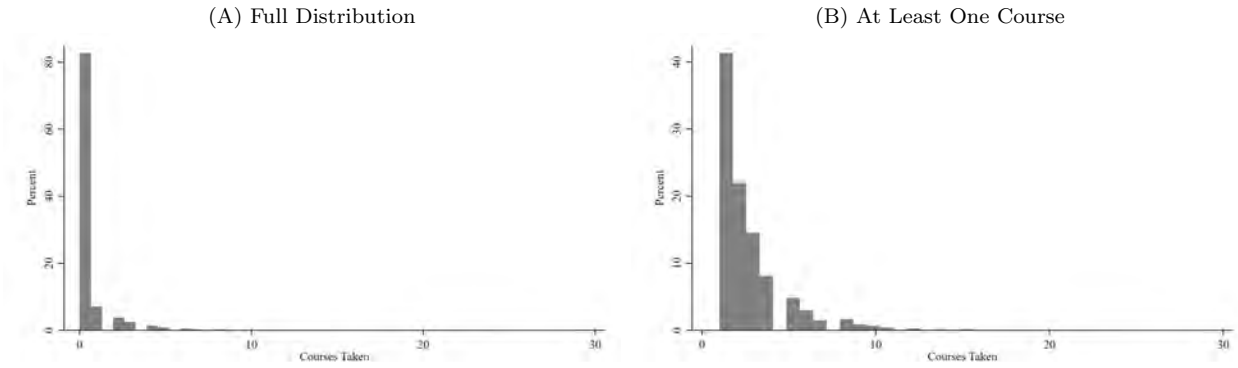
Order: Attempt of Complicity; Bartering, Selling Goods or Services, etc.; Bribery; Counterfeiting, Forging; Criminal Conduct; Criminal or Unlawful Conduct; Disobeying a Lawful Order/Direction; Entering into Contracts, Agreements, Operating Businesses; Escape; False Statements; Gambling, Debts, etc.; Habitual Minor Violator; Habitual Minor Offender; Misuse of Mail, O-Mail, Telephone, or Other Communications; Obstructive/Disruptive Conduct; Out of Place of Assignment; Participation in a Disturbance; Possession of Key or Key Pattern; Refusal or Failing to Participate in Treatment; Refusal or Failing to Work; Safety, Sanitation, Tattooing, and Piercing; Security Threat Groups; Unauthorized Group/Gang Conduct; Verbal Abuse; Violating a Condition of Leave/Furlough.

Property: Damage to Property; Tampering/Interfering with Locks, Security Items, Computers, or Electronic Devices; Theft.

Violent: Arson; Assault; Attempted Suicide, Self-Mutilation; Blackmail, Protections (Strong-Arming); Inappropriate Sexual Conduct and Sexual Violence; Inappropriate Sexual Harassment and Sexual Abuse; Kidnapping; Killing; Rioting; Robbery; Sexual Misconduct; Sexual Violence; Threats/Intimidation.

A2 Supplemental Figures & Tables

Figure A1: Distribution of Courses Taken



Notes: This figure shows histograms of the number of courses taken by individuals in our sample. In Panel A, we present the entire distribution, while in Panel B, we restrict attention to individuals who took at least once course.

Figure A2: Data Collection

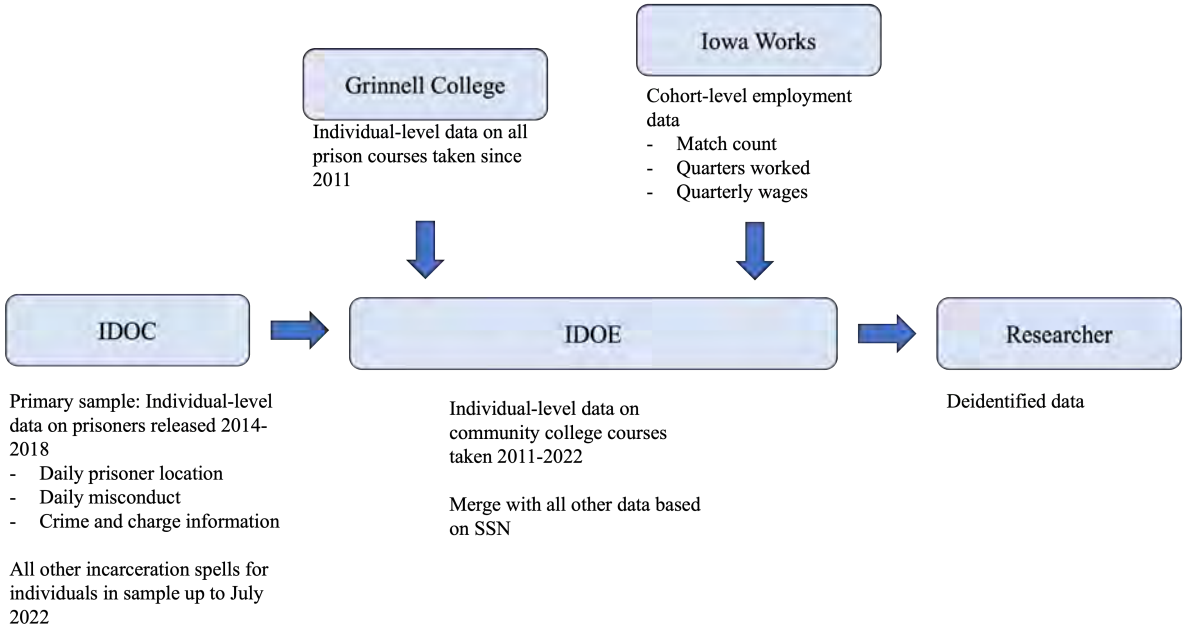
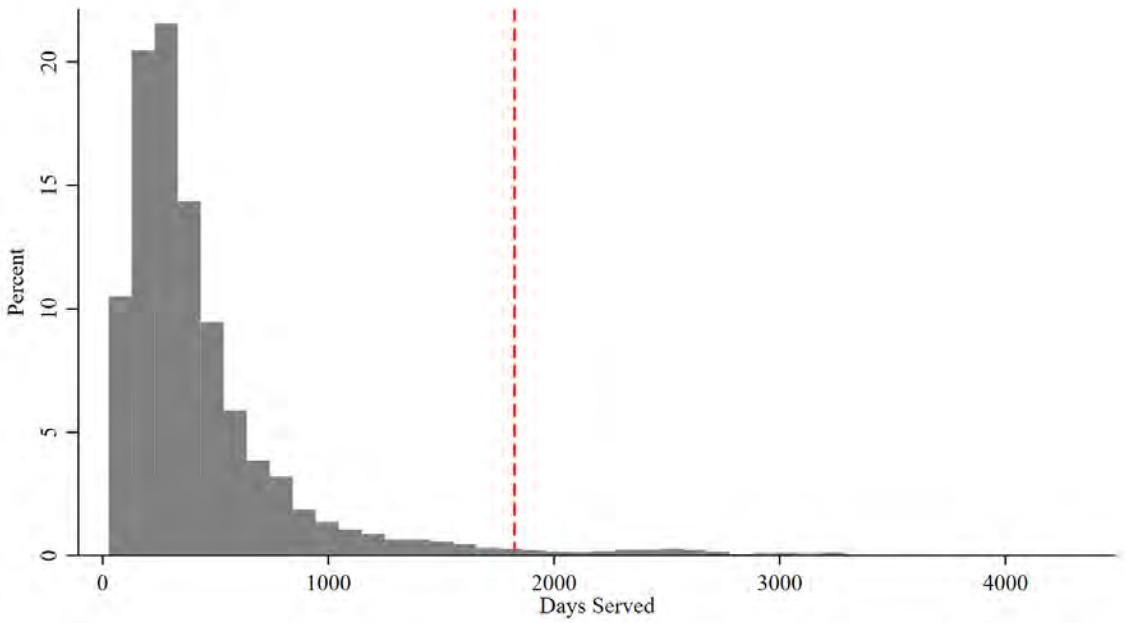
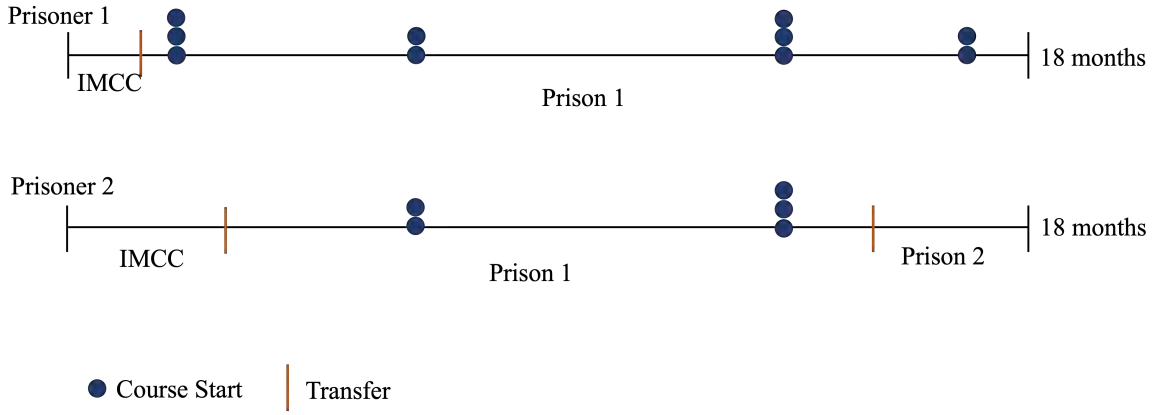


Figure A3: Distribution of Time Served



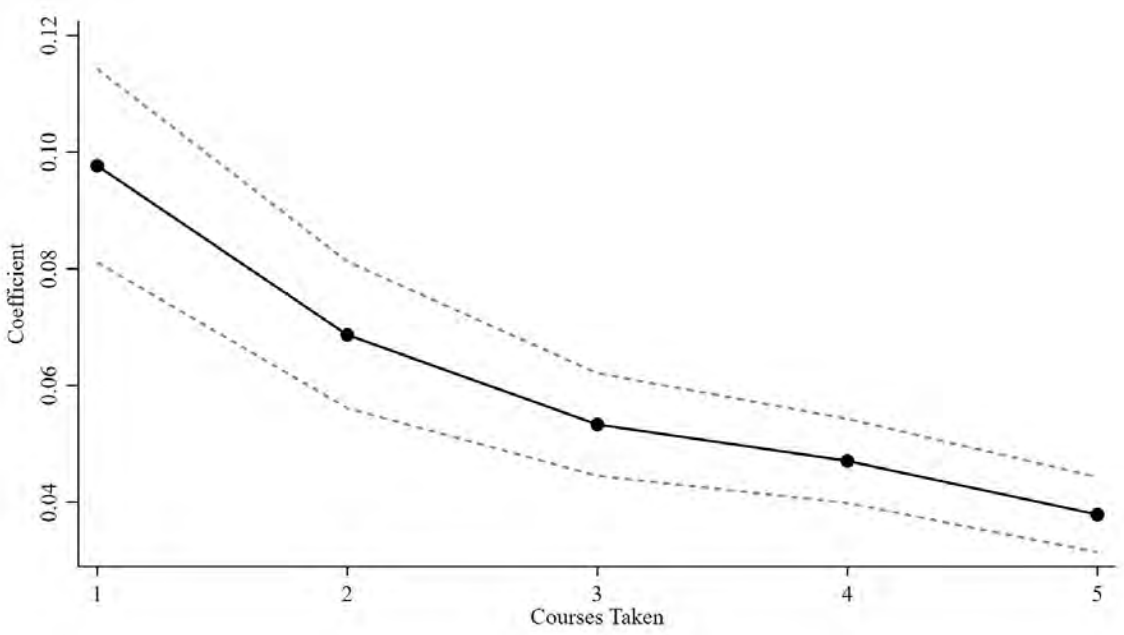
Notes: This figure shows a histogram of the number of days served by the individuals in our sample. The red dashed vertical line indicates incarceration spells of 5 years. To make the figure more readable, we suppress the 1% and 99% tails of the distribution.

Figure A4: Illustration of Variation in Course Opportunities



Notes: This figure displays a hypothetical comparison of two individual prisoners who served the same amount of time in prison and within the same primary prison but who had different opportunities to participate in courses because of the timing of their transfers.

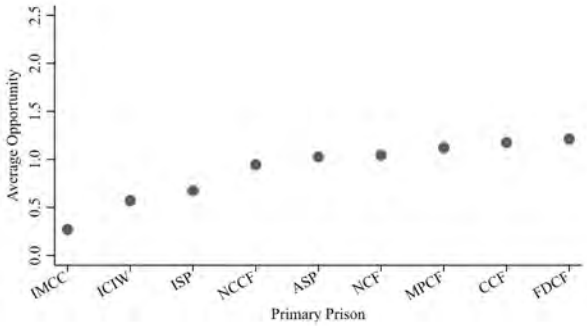
Figure A5: First-Stage Estimates with 95% Confidence Interval



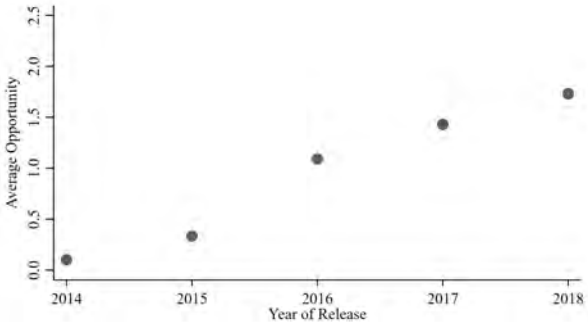
Notes: This figure plots the first-stage coefficient estimates and the 95% confidence interval for different levels of course-taking. In practice, for each level of course-taking, x, we create an indicator for whether individuals have taken at least x courses and use that indicator as the outcome in Equation 1.

Figure A6: Nonrandom Variation in Educational Opportunities

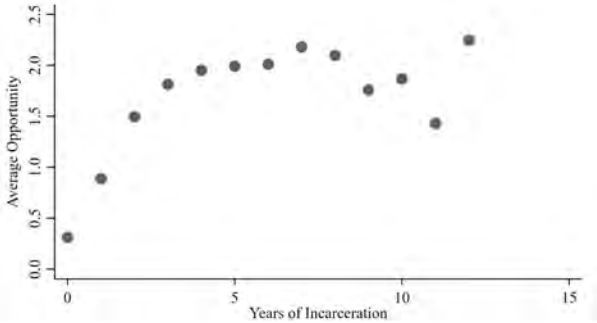
(A) Primary Prison



(B) Release Year

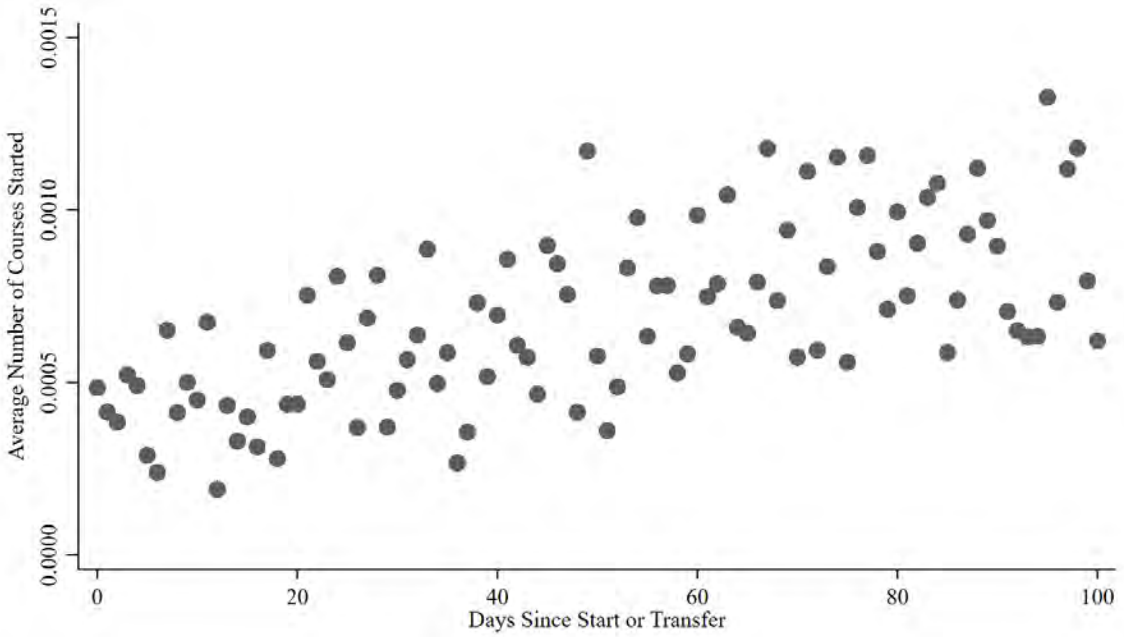


(C) Time Served



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

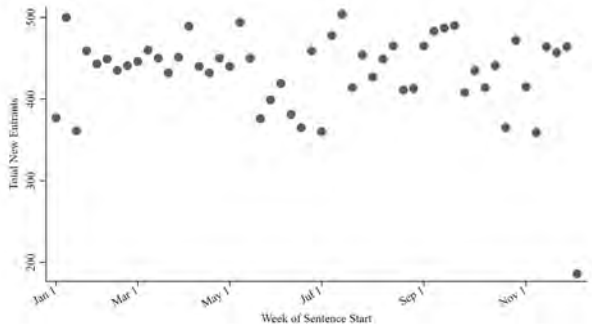
Figure A7: Course Starts After Transfers



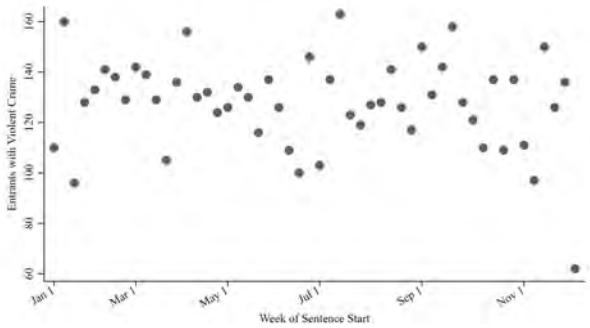
Notes: This figure displays the average number of courses started by individuals who transferred X days ago.

Figure A8: No Seasonality in Prison Entry

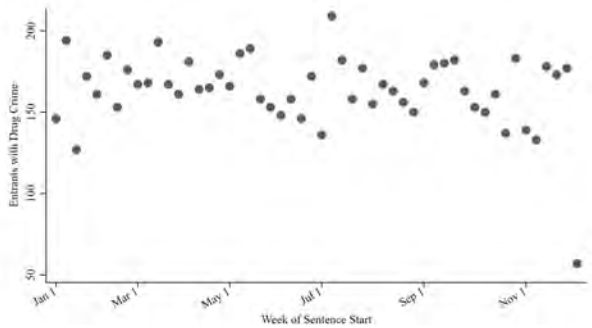
(A) Total New Entrants



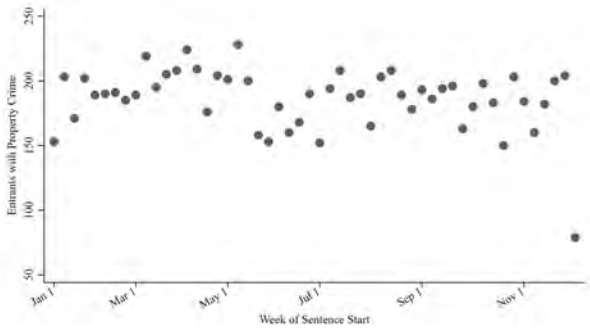
(B) Entrants with a Violent Crime



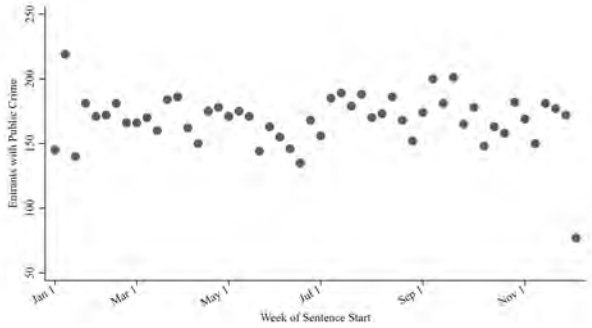
(C) Entrants with a Drug Crime



(D) Entrants with a Property Crime



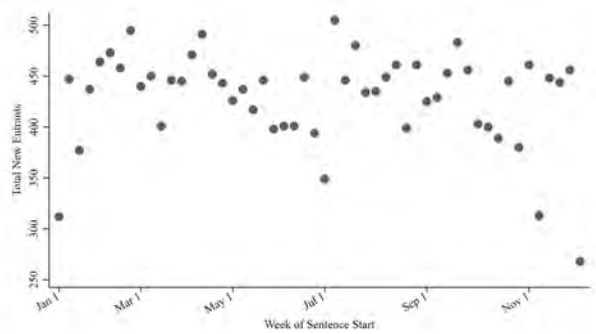
(E) Entrants with a Public Order Crime



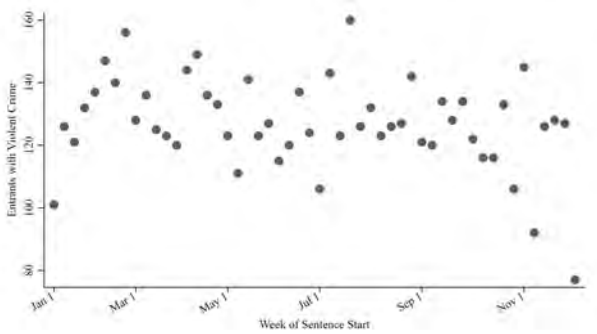
Notes: This figure displays the number of people entering prison during each week of the year. Panel A shows this for all entrants. Panel B shows the number of new entrants with at least one violent crime conviction, Panel C shows the number of new entrants with at least one drug crime conviction, Panel 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.

Figure A9: No Seasonality in Transfers to Primary Prison

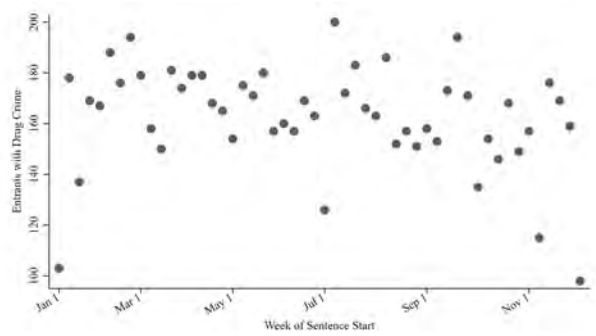
(A) Total First Transfers



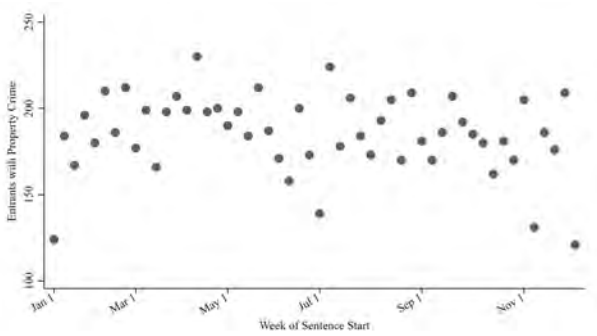
(B) Tranfsers with a Violent Crime



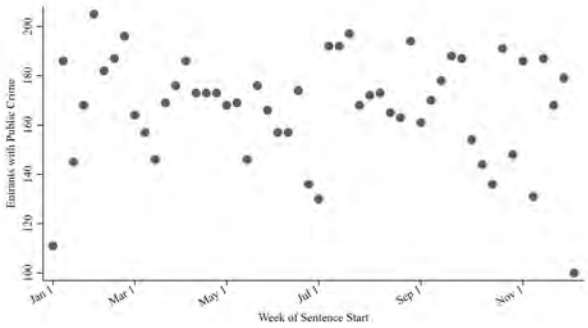
(C) Transfers with a Drug Crime



(D) Transfers with a Property Crime

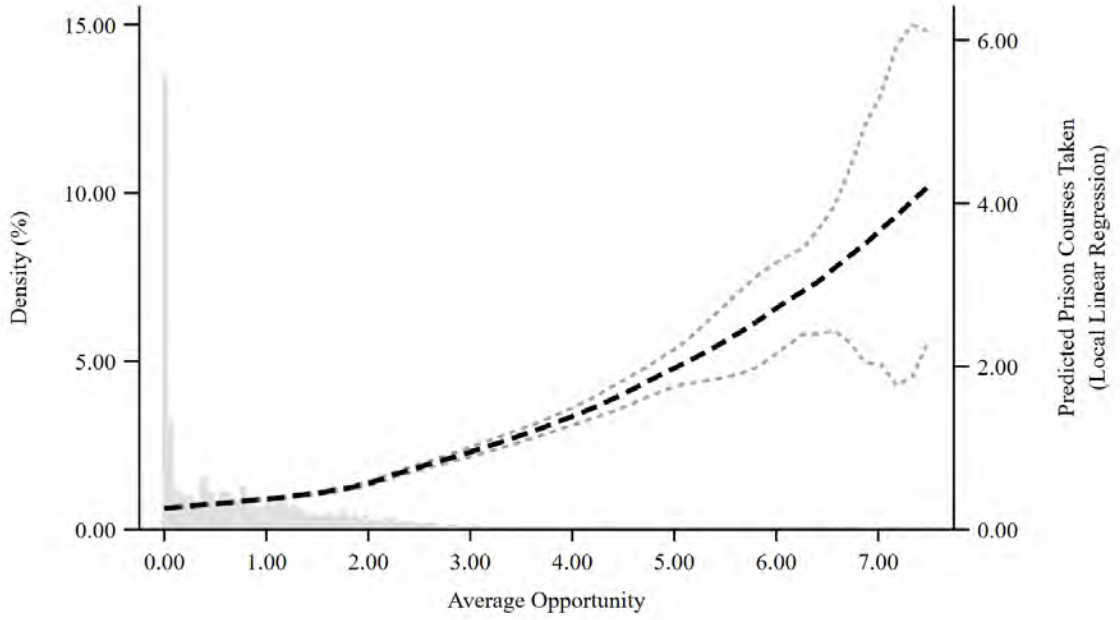


(E) Transfers with a Public Order Crime



Notes: This figure displays the number of people transferring into their primary prison for the first time during each week of the year. Panel A shows the number for all transfers. Panel B shows the number of transfers with at least one violent crime conviction, Panel C shows the number of transfers with at least one drug crime conviction, Panel D shows the number of transfers with at least one property crime conviction, and Panel E shows the number of transfers with at least one public order crime.

Figure A10: Increased Course-Taking Opportunities Predict More Participation in Prison Courses



Notes: This figure displays a histogram of the opportunity instrument 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.

Table A2: Effect on Time Served Estimated with First Three Months of Opportunity Instrument

	Days Served
Courses Taken	7.771**
	(3.769)
Outcome Mean	513.29
Observations	22,475
First Stage F-Stat	126.06
Primary Prison FE	X
Start Year FE	X
Controls	X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. In this model, we redefine the opportunity instrument to include only course availability during the first three months of each prisoner's sentence. This specification also replaces the time served and release year fixed effects with start year fixed effects. Standard errors clustered at the primary prison×start year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1..

Table A3: First Stage by Subgroup

	All	Age < 25	Prior Recidivist	Violent Charge	Felony Charge	White	Years of Education ≥ 12
Opportunity	0.455***	0.590***	0.427***	0.561***	0.463***	0.403***	0.425***
	(0.040)	(0.061)	(0.058)	(0.071)	(0.044)	(0.045)	(0.050)
Observations	22,478	4,958	10,154	6,641	18,254	15,453	18,402
F-Stat	131.16	93.80	54.08	62.81	109.12	81.76	72.30
Ratio		1.297	0.939	1.233	1.018	0.886	0.933
Difference		0.135*	-0.028	0.106	0.008	-0.052	-0.030
		(0.073)	(0.070)	(0.081)	(0.060)	(0.060)	(0.064)

Notes: This table reports the first stage of courses taken on the opportunity score as described in Equation 1. Each column heading describes the relevant sample restriction. The ratio and difference between the first-stage coefficient for each subsample and the overall sample are also reported. Difference standard errors are calculated as $\sqrt{(SE_{\beta_{all}})^2 + (SE_{\beta_{subgroup}})^2}$ (Clogg, Petkova and Haritou, 1995). Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Monotonicity Test

	5 knots	10 knots	15 knots	20 knots
Test Statistic	316.10	307.74	294.97	243.19
Degrees of Freedom	(303)	(298)	(293)	(288)
P-value	[0.291]	[0.337]	[0.457]	[0.974]

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 A5: Alternative Balance Tests Using Predicted Outcomes

	Coefficient	P-value
Reincarceration	0.033	0.552
New Crime	0.059	0.447
Revocation	0.014	0.769
Courses	0.039	0.177
Noncredit Courses	0.028	0.331
Credit Courses	0.036	0.313
Passed Courses	0.065	0.130
P(Employed)	0.151	0.374
Quarters Worked	-0.002	0.945
Quarterly Wages	-0.000	0.548
Nonzero Quarterly Wages	-0.000	0.458

Notes: This table presents the results of balance tests using predicted outcomes. We first regress our controls on the indicated outcome variable, predict the value of that outcome, and then test whether the opportunity score is explained by the predicted outcome variable. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Complier Characteristics

	Mean	Complier-Weighted Mean	Ratio
Age 25-29	0.194	0.231	1.195
Age 30-35	0.215	0.151	0.704
Age 36-43	0.179	0.128	0.712
Age 43+	0.192	0.075	0.390
Prior Recidivist	0.452	0.391	0.866
Felony Convictions	1.596	2.385	1.494
Total Convictions	2.770	3.263	1.178
Any Violent Crime?	0.295	0.672	2.276
Any Drug Crime?	0.378	0.236	0.625
Any Property Crime?	0.431	0.393	0.913
Any Public Order Crime?	0.390	0.271	0.696
White	0.687	0.559	0.813
Years of Education	11.542	11.531	0.999
Violence Score	5.783	6.792	1.174
Missing Violence Score	0.113	0.063	0.552

Notes: This table presents summary statistics for the entire sample and for compliers. In the third column, we present the ratio of the complier-weighted mean to the sample mean.

Table A7: Effect on Recidivism Within Three Years of Release, OLS Comparison

	OLS	IV-Reweighted OLS	2SLS
Panel A. Reincarceration			
Courses Taken	0.005**	0.004	0.023*
	(0.002)	(0.002)	(0.013)
Outcome Mean	0.445	0.445	0.445
Panel B. New Crime			
Courses Taken	0.004**	0.004	0.007
	(0.002)	(0.003)	(0.010)
Outcome Mean	0.268	0.268	0.268
Panel C. Revocation			
Courses Taken	0.007***	0.006**	0.034**
	(0.002)	(0.002)	(0.015)
Outcome Mean	0.337	0.337	0.337
Observations	22,478	22,478	22,478
First Stage F-Stat			131.16
Randomization FE	X	X	X
Controls	X	X	X

Notes: This table reports OLS estimates of the relationship between courses taken while incarcerated and three-year recidivism rates. Column 2 reweights the OLS estimates to match the complier population in terms of the share of observations in the groups defined by age (older or younger than 25) and whether the case included a violent charge. Column 3 reports the baseline 2SLS estimates for comparison. All specifications include our randomization fixed effects and the full set of controls. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Time Served Robustness

	Reincarceration	New Crime	Revocation
Panel A. 1-month Time Served Bins			
Courses Taken	0.023*	0.008	0.034**
	(0.013)	(0.010)	(0.015)
First Stage F-Stat	128.59	128.59	128.59
Panel B. 6-month Time Served Bins			
Courses Taken	0.023*	0.008	0.035**
	(0.012)	(0.010)	(0.015)
Outcome Mean	0.445	0.268	0.337
Observations	22,478	22,478	22,478
First Stage F-Stat	133.57	133.57	133.57
Randomization FE	X	X	X
Controls	X	X	X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. In Panel A, we use 1-month time served bins instead of the 3-month bins used in our baseline model. In Panel B, we use 6-month bins. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Robustness to Alternative Instrument Using First Three Months of Opportunity

	Reincarceration	New Crime	Revocation
Courses Taken	0.008	0.007	0.013**
	(0.007)	(0.007)	(0.006)
Outcome Mean	0.445	0.268	0.337
Observations	22,475	22,475	22,475
First Stage F-Stat	126.06	126.06	126.06
Primary Prison FE	X	X	X
Start Year FE	X	X	X
Controls	X	X	X

Notes: This table presents 2SLS regression results using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. In this model, we redefine the opportunity instrument to include only course availability during the first three months of each prisoner's sentence. This specification also replaces the time served and release year fixed effects with start year fixed effects. Standard errors clustered at the primary prison×start year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Base Results With Adjustment for COVID-19 Pandemic

	Reincarceration	New Crime	Revocation
Courses Taken	0.051***	0.005	0.100***
	(0.018)	(0.017)	(0.025)
Outcome Mean	0.442	0.265	0.326
Observations	14,034	14,034	14,034
First Stage F-Stat	101.33	101.33	101.33
Randomization FE	X	X	X
Controls	X	X	X

Notes: This table replicates our baseline 2SLS results but excludes all prisoners released after March 1, 2017, to test whether our estimates are impacted by the COVID-19 pandemic. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Placebo Test for Spillovers 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	0.001	0.003	0.004	0.011*	0.003	0.017***
	(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	0.015	0.007	0.011	0.012	-0.004	0.013
	(0.013)	(0.010)	(0.013)	(0.014)	(0.012)	(0.014)
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 reduced-form regression results from estimating the effect of opportunities for prison education on recidivism separately for HSE or remedial and postsecondary courses using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. We investigate the possibility of spillover effects by estimating separate regressions for individuals with and without 12 years of education. Because our effects are concentrated among postsecondary courses, any estimated effects among individuals without 12 years of education are likely to reflect spillover effects. Panel A presents these results for individuals with at least 12 years of education upon entering prison. Panel B shows the results for those with fewer than 12 years of education when they entered prison. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Prisoner Characteristics by Release Type

	Free	Parole	Work Release
Courses Taken	0.261	0.469	0.557
Days Served	455.823	441.425	643.886
Release Year	2015.930	2016.029	2016.113
Number of Prison Transfers	1.622	1.653	1.862
Age 15-24	0.249	0.206	0.221
Age 25-29	0.215	0.186	0.189
Age 30-35	0.191	0.215	0.229
Age 36-43	0.162	0.189	0.178
Age 43+	0.183	0.203	0.183
Prior Recidivist	0.475	0.423	0.473
Felony Convictions	0.858	1.722	1.907
Total Convictions	2.236	2.803	3.066
Any Violent Crime?	0.407	0.227	0.310
Any Drug Crime?	0.216	0.473	0.362
Any Property Crime?	0.415	0.430	0.441
Any Public Order Crime?	0.392	0.363	0.420
White	0.665	0.721	0.661
Years of Education	11.418	11.606	11.540
Violence Score	8.601	4.211	5.949
Missing Violence Score	0.240	0.053	0.108
Weekly Total Misconduct	0.060	0.022	0.027
Weekly Violent Misconduct	0.008	0.002	0.003
Observations	4,925	9,740	7,802

Notes: This table presents summary statistics for individuals released without supervision, to parole, and to work release.

Table A13: Mediation Exercise Using Education Outcomes

	Courses	Credit Courses	Noncredit Courses	Passed Courses	Courses	Credit Courses	Noncredit Courses	Passed Courses
Opportunity	-0.006	-0.003	-0.004	0.003	-0.010	-0.003	-0.007	0.001
	(0.024)	(0.020)	(0.010)	(0.018)	(0.024)	(0.020)	(0.011)	(0.017)
Parole					0.128***	0.057*	0.072***	0.100***
					(0.034)	(0.031)	(0.016)	(0.029)
Work Release					0.163***	0.030	0.132***	0.096***
					(0.040)	(0.031)	(0.020)	(0.033)
Outcome Mean	0.562	0.292	0.270	0.356	0.562	0.292	0.270	0.356
Observations	22,478	22,478	22,478	22,478	22,478	22,478	22,478	22,478
Randomization FE	X	X	X	X	X	X	X	X
Controls	X	X	X	X	X	X	X	X

Notes: This table presents reduced-form regression results from estimating the effect of opportunity on recidivism using a prisoner–stint-level dataset that spans January 1, 2014, through December 31, 2018. Standard errors clustered at the primary prison×release year level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Poisson Model Event Study Estimates for Effect on Weekly In-Prison Misconduct

	Total	Drug	Order	Property	Violent
After First Course	0.092*	0.171***	0.095*	0.202*	-0.030
	(0.051)	(0.060)	(0.054)	(0.112)	(0.083)
Outcome Mean	0.048	0.012	0.035	0.006	0.011
Observations	1,780,620	1,223,272	1,657,065	448,447	924,557
Individual FE	X	X	X	X	X
Prison FE	X	X	X	X	X
Fraction of Time Served FE	X	X	X	X	X

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