"Something Works" in U.S. Jails: Misconduct and Recidivism Effects of the IGNITE Program*

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Abstract

A longstanding and influential view in U.S. correctional policy is that "nothing works" when it comes to rehabilitating incarcerated individuals. We revisit this hypothesis by studying an innovative law-enforcement-led program launched in the county jail of Flint, Michigan: Inmate Growth Naturally and Intentionally Through Education (IGNITE). We develop an instrumental variable approach to estimate the effects of IGNITE exposure, which leverages quasi-random court delays that cause individuals to spend more time in jail both before and after the program's launch. Holding time in jail fixed, we find that one additional month of IGNITE exposure reduces within-jail misconduct by 49% and reduces three-month recidivism by 18%, with the recidivism effects growing over time. Surveys of staff and community members, along with administrative test score records and within-jail text messages, suggest that cultural change and improved literacy and numeracy scores are key contributing mechanisms.

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1 Introduction

The US has one of the highest incarceration rates in the world, with over two million people in correctional facilities daily (Zeng, 2022). Over 600,000 of these individuals are held in local jails, the vast majority of whom are unconvicted or awaiting sentencing (Sawyer and Wagner, 2023). These numbers partly reflect high rates of recidivism: one in four individuals released from jail, for example, are re-jailed within the same year (Jones and Sawyer, 2019). Addressing such incarceration cycles, and reducing recidivism more broadly, remain persistent policy challenges (Doleac, 2023).

Views on the effectiveness of rehabilitation in US prisons have generally been negative and slow to change since the influential "nothing works" doctrine, often attributed to Martinson (1974). From a review of observational studies of prison rehabilitation programs in the 1970s, Martinson concluded that with "isolated exception" there was no "appreciable effect on recidivism." While this conclusion has been challenged, with more optimistic recent findings on the efficacy of certain rehabilitative programs for certain populations (e.g., Weisburd, Farrington and Gill 2017; Heller et al. 2017; Arbour, Marchand and Lacroix 2023), the "nothing works" doctrine became mantra for much of the U.S. correctional community. In the years following the Martinson report, U.S. correctional policy largely shifted away from principles of rehabilitation to a focus on deterrence and incapacitation (Andrews and Bonta, 2010), with rehabilitative programs receiving even less investment within U.S. jails—where resources are scant and stays are presumed short.

Outside the U.S., however, rehabilitative programming has become a mainstay of incarceration and is often seen as crucial for reintegrating incarcerated individuals into society. Correctional policy in Norway, for example, bases rehabilitation efforts around the "principle of normality": that life inside a correctional facility should resemble life outside as closely as possible. A recent quasi-experimental analysis finds that time spent in such facilities leads to large reductions in recidivism and other adverse outcomes (Bhuller et al., 2020). Inspired by these principles of rehabilitation, some U.S. cities have begun to incorporate these ideas into the design of their correctional facilities. For example, in 2020, a medium-security state prison located just outside of Philadelphia established a small housing unit known as "Little Scandinavia," originally available to six individuals serving life sentences (Strange, 2023). But whether such rehabilitative policies and philosophies can work in other contexts—and particularly in U.S. jails—remains an open question.

This paper studies an innovative law-enforcement-led rehabilitation program, launched in September 2020 in the county jail of Flint, Michigan: Inmate Growth Naturally and Intentionally Through Education (IGNITE). Nominally, IGNITE is an educational program which offers tailored coursework and training to all jailed individuals with high takeup rates. In practice, however, IGNITE administrators emphasize a cultural change in the jail that goes well beyond coursework and embodies a rehabilitative philosophy not unlike Norway's principle of normality. Administrators say, for example, that IGNITE is "much more than giving people a free education. It's about giving people hope when they have no hope." (Barrett and Greene, 2023). At the same time, a notable difference with the Norwegian experience—besides the U.S. jail context—is the program's cost: IGNITE is largely funded with existing county resources and staff, avoiding the kinds of large spending Norway and other countries have used to launch rehabilitative systems.[1] This low cost and perceptions of broad success have recently led the National Sherrifs' Association to begin scaling-up programs similar to IGNITE in many jails across the U.S.[2]

To estimate the effects of IGNITE exposure, we leverage unique administrative data and a novel instrumental variable (IV) approach based on idiosyncratic delays in court appointments. Court delays are common for jailed individuals and can significantly extend their time spent in jail. In our setting, District Court delays appear conditionally as-good-as-randomly assigned and extend time in jail by around two weeks (27%) on average, both before and after the launch of IGNITE. We use this variation to instrument for the time a jailed individual is exposed to IGNITE, accounting for any baseline (i.e., non-IGNITE) effects of increased jail time, via a two-treatment IV specification. Effectively, this specification differences post- vs. pre-IGNITE IV estimates to isolate the marginal effect of IGNITE exposure while holding fixed time in jail and netting out any potential direct effects of delays. We formalize the key new assumption underlying this "difference-in-IVs" approach and develop graphical diagnostics akin to standard "pre-trend" checks in conventional difference-in-differences strategies.[3] We also contrast the IV independence, exclusion, and monotonicity assumptions in our approach with those underlying more conventional "judge IV" designs (employed, e.g., in Bhuller et al. 2020), which appear less tenable in our context.[4]

We find that exposure to IGNITE dramatically reduces an individual's propensity for both within-jail misconduct and post-release recidivism. One additional month of exposure to IGNITE is estimated to reduce the number of weekly major misconduct incidents by 0.16 (49%) and to reduce three-month recidivism by 8 percentage points (18%). These effects are similar across different demographic groups, prior offense status, and predicted exposure to the Flint water crisis. Estimated recidivism effects grow over time—to around a 15 percentage point reduction in one-year recidivism—and are concentrated among individuals with high predicted recidivism risk. In economic terms, we find that one additional month of IGNITE exposure reduces the three-month social cost of crime post-release by at least \$3,000 per incarcerated individual. Over a year, the social cost of crime reduction is at least \$5,600 per person-month.

These main findings are robust to a number of potential threats to our IV strategy. Notably, we find that instrument compliers are similar on a wide range of observable characteristics before and after the launch of IGNITE, supporting our interpretation of difference-in-IV estimates as effects of the program itself. Other robustness checks probe the more standard identifying assumptions of

[1]For example, Norway spends around \$93,000 each year per prisoner in its system (Beaumont, 2023) while Genesee County Jail spent around \$70 per incarcerated individual per day in 2021 (Finley, 2019) or around \$25,500 per year.

[2]As of February 2024, county jails in eleven states have adopted IGNITE and jails in seven more states have begun plans for adoption; see NSA (2024).

[3]Importantly, our IV strategy does not leverage a conventional "parallel trends" restriction on untreated potential outcomes. Instead, we leverage the as-good-as-random assignment of court delays and an assumption that delay-induced time-in-jail effects would have been similar after the start of IGNITE if not for the program's launch.

[4]For example, our monotonicity assumption requires that court delays weakly increase an individual's time in jail—a likely more plausible condition than the often-critiqued assumption of monotonic decision-making across heterogeneous judges (Mueller-Smith, 2015; Frandsen, Lefgren and Leslie, 2023). Empirical tests of conventional judge IV monotonicity and exclusion restrictions reject decisively in our setting.

as-good-as-random instrument assignment, exclusion, and monotonicity, and show that our findings are not driven by changing conditions from the COVID-19 pandemic or changes in reporting behavior. We also obtain qualitatively similar (though generally less precise) estimates from more conventional difference-in-differences and judge IV strategies, despite these alternative strategies relying on different (and arguably less tenable) assumptions and using less complete or fine-grained data. An alternative difference-in-IVs strategy that uses a neighboring county as a "control" group in the post-IGNITE era, instead of our baseline pre-post comparison in the treated county, also yields similar recidivism effect estimates. We further obtain similar estimates from a "double" difference-in-IVs specification, which combines cross-county and over-time comparisons to relax our baseline identifying assumptions.

We explore two primary drivers of these large misconduct and recidivism effects. First, we show that exposure to the formal educational programming in IGNITE likely led to substantial improvements in literacy and numeracy scores among incarcerated individuals. Comparing standardized test scores before and after enrollment in this programming, we find that individuals gained a full grade level, on average, in both math and reading from low baseline levels. While some of these gains may reflect heightened attention or improved test-taking, rather than human capital accumulation per se, they are massive even when compared to some of the most effective educational interventions documented in recent quasi-experimental literatures (e.g., Cohodes and Roy, 2023).

Second, we deploy surveys to several stakeholders—including Flint community members, the formerly incarcerated, and current Genesee County custody staff—to assess the extent of cultural change alongside formal educational programming. We find that individuals who had personally been exposed to IGNITE or have relatives who were exposed are 23 percentage points (70%) more likely to view law enforcement favorably, suggesting a positive spillover effect from jail experience to perceptions about police. Furthermore, among those who were incarcerated or who had close contacts with the incarcerated, exposure to IGNITE is associated with a higher likelihood of engagement in positive social activities (though this effect is not statistically significant). A sentiment analysis of administrative data collected from text messages sent from the incarcerated individuals to jail staff supports the survey findings: IGNITE-exposed incarcerated individuals are more likely to use words categorized as positive and associated with trust than those incarcerated before IGNITE. Custody officers who interact more with IGNITE participants are also twice as likely to view educational programs for incarcerated individuals as worthwhile compared to officers that do not regularly interact with IGNITE participants. Taken together, these qualitative analyses support the view that a widespread cultural change occurred for both participants and staff.

Broadly, our findings suggest that "something works" when it comes to the rehabilitation of incarcerated individuals in U.S. jails. In fact, we find that IGNITE generates recidivism reductions comparable to or larger than a range of rehabilitative programs in varied settings and countries, including Norway (e.g., Heller et al. 2017; Mastrobuoni and Terlizzese 2022; Arbour, Marchand and Lacroix 2023; Shem-Tov, Raphael and Skog 2022; Bhuller et al. 2020). Notably, the recidivism reduction from IGNITE is similar to the impact of cognitive behavioral therapy (CBT) administered to juvenile arrestees (Heller et al. 2017), despite IGNITE serving high-risk adults whose behavior is arguably less malleable. The effectiveness of IGNITE thus demonstrates that rehabilitative principles can be successfully implemented even within a county jail in one of the most disadvantaged cities in America: Flint, Michigan, which has been described as a once prosperous city "devastated by global economic forces, population loss, racism, disinvestment, and breakdowns in accountability at multiple levels of government" (Leiser, Wang and Tatum III, 2022). Despite these inherent challenges, we show that a relatively low-cost and law-enforcement-led program resulted in better outcomes for incarcerated individuals and far-reaching improvements in public safety.

Our analysis contributes to a large literature studying the impact of various interventions on crime and recidivism. In particular, we add to a growing body of work documenting beneficial effects of rehabilitative programming (described above), diversion from the criminal justice system itself (e.g., Mueller-Smith and Schnepel 2021; Augustine et al. 2022), specialized criminal courts (e.g., Golestani, Owens and Raissian 2024), improvements in prison conditions (e.g., Tobón 2022), and alternatives to incarceration (e.g., Di Tella and Schargrodsky 2013; Lee 2023; Henneguelle, Monnery and Kensey 2016; Williams and Weatherburn 2022). Our paper also relates to work documenting the impact of education on crime more broadly (e.g., Lochner and Moretti 2004; Lavecchia, Oreopoulos and Spencer 2024), as well as work studying the effects of health care, employment, and other programs on crime.[5] In addition to conventional recidivism outcomes, we estimate effects on within-facility misconduct—adding to a small but growing quasi-experimental literature with access to such outcomes (Arbour, Marchand and Lacroix 2023; Bravo 2024).

Methodologically, we contribute a new IV strategy that leverages administrative delays which extend an individual's time exposed to an institution before and after a policy reform. Like Abdulkadiroğlu et al. (2016) and Autor et al. (2017), we use a two-treatment IV model to isolate the causal effects of interest via quasi-experimental shocks. Our approach is closest to Abdulkadiroğlu et al. (2016) in that we combine cross-sectional shocks with variation in potential policy exposure over time; it differs from Autor et al. (2017)'s and those in other papers studying the effects of delays or administrative interruptions themselves (e.g., Yang 2016; Iverson 2018; Ho, Hamilton and Roos 2000) in that we use quasi-experimental delay shocks as an instrument for the policy's exposure. We develop several diagnostic tools and extensions of this "difference-in-IVs" approach, which may be fruitfully applied both within and outside of criminal justice settings. We pair this quasi-experimental approach with a series of qualitative analyses to help explore possible mechanisms, in the same mixed-methods spirit as Bergman et al. (Forthcoming).

The remainder of this paper is organized as follows. Section 2 details the institutional setting. Section 3 describes data sources and the analysis sample. Section 4 develops our IV strategy. Section 5 presents the main results and extensions. Section 6 contextualizes our findings and explores possible mechanisms. Section 7 concludes.

[5] See, e.g., Packham and Slusky (2023); Raphael and Weiman (2007); Sabol (2007); Yang (2017); Bhatt et al. (2024); Deza, Maclean and Solomon (2022); Bondurant, Lindo and Swensen (2018); Jácome (2020); Tuttle (2019); Deshpande and Mueller-Smith (2022); Arenberg, Neller and Stripling (2024); Darolia, Mueser and Cronin (2021); Tyler and Kling (2006) and Bailey et al. (2020).

2 Institutional Setting

2.1 Genesee County Jail and Court System

IGNITE was launched September 2020 in Genesee County Jail, which holds individuals primarily from the surrounding city of Flint, Michigan (see Appendix Figure A1). Flint is a majority-Black city with around one-third of households living in poverty. It has experienced several major crises in recent years, including the Flint Water Crisis and multiple cases of financial mismanagement. Flint also consistently has one of the highest crime levels among U.S. cities, with a homicide rate exceeding seven times the national average (Stebbins, 2021).

As with the approximately 3,000 U.S. jails across the country, Genesee County Jail primarily holds three groups of individuals: (1) arrested individuals who are being detained before trial (representing over 90% of the jail population), (2) convicted individuals who are awaiting sentencing, and (3) sentenced individuals with incarceration time of less than one year. Jail populations in Genesee County and across the U.S. are overwhelmingly male, young, and non-white.[6] Incarcerated individuals are also much more likely to lack a high school degree compared to the general population; a 2014 prison study found that 72% lacked literate proficiency compared to 52% of U.S. households (NCES, 2014). Time spent in jail has increased over time, with the national mean length of stay rising over the last decade from 22.7 days to 32.8 days (Zeng, 2022). Court delays, described further below, are a primary reason for longer jail spells.

Individuals' first point of contact with Genesee County Jail occurs shortly after their arrest. On the basis of arrest charges and other considerations, a prosecutor decides whether to file criminal charges. At this point, the case formally enters into the court system and follows a particular sequence of required events. The typical flow of a case through the Genesee County Court System is shown in Appendix Figure A2. Defendants usually start their case in the District Court, which handles all initial arraignments, probable cause conferences, and preliminary examinations, with cases assigned to a particular court based on location of arrest (67th District Court, 2022; Supreme Court, 2023). Misdemeanor offenses (less serious crimes that usually carry a maximum jail term of one year) proceed in the District Court through the trial, plea, and sentencing processes. In more serious felony cases with sufficient evidence, the case is "bound over" (i.e., transferred) to the Circuit Court, which handles the pretrial, trial, plea, and sentencing processes. The District Court and Circuit Court can also reconsider an individual's bond amount and decide whether to release them on electronic tether.

Before IGNITE, Genesee County Jail had very limited educational programming for incarcerated individuals. The jail only offered a GED class to a small group of selected individuals through a local school providing adult education (Mt. Morris Consolidated Schools). Such limited programming is typical of U.S. jails.[7] As described by one correctional administrator in 2019: "we [in

[6] In 2021, for example, 87% of those incarcerated in jails nationally were men, 52% were under the age of 35, and 51% were non-white or Hispanic (Zeng, 2022).

[7] For example, according to Harlow (2003), only 60% of U.S. jails reported any educational or training programming in its last census (1999) and the quality and accessibility of that programming varies substantially and often

Genesee County Jail] were just kind of functioning.... We were sending people to court, sending people to prison, getting people out" (Barrett and Greene, 2023).

2.2 IGNITE

In the wake of the murder of George Floyd in May 2020, and the elevated racial tensions that followed in Flint and other parts of the country, the Genesee County Sheriff launched IGNITE: a new jail education program which was available to nearly all incarcerated individuals with incentives for participation. The stated mission of this program is to reduce recidivism and end the cycle of generational incarceration through education. Here we summarize key features of the program and its launch; Appendix B gives further institutional details.

Since its September 2020 launch, IGNITE has relied on repurposed jail space and staff. For instance, the day room in Genesee County Jail was transformed into a large classroom used by different groups of incarcerated individuals at different times of the day (see Appendix Figure A3). Staffing consisted of two full-time deputies, who oversaw the day-to-day operations of the program, and a GED teacher from the nearby Mt. Morris Consolidated Schools who acted as a circulating educator. The jail installed two dedicated WiFi networks for access to the internet and initially purchased 600 tablets for IGNITE participants. Program costs were largely covered by revenue generated from the use of commissary tablets, which participants could use in off-hours to purchase and access games and puzzles. Consequently, the county budget for correctional services did not substantially change with IGNITE's launch (see Appendix Figure A4). Both before and after IGNITE, Genesee County Jail spent around $70 per individual-day (Finley, 2019).

In addition to being available to nearly all incarcerated individuals (with the exception of those deemed medically unstable or who were immediately released without charge), IGNITE has three distinguishing features. First, instruction is tailored to each individual based on their educational background and baseline testing (see Appendix Figure A5 for examples). Participants are enrolled in class five days per week for two hours per day and are placed at intake into short-term, medium-term, or long-term coursework depending on their predicted length of stay. Some incarcerated individuals work on basic literacy while others work towards completing their GED and others complete programs for college credit. Individuals work on Chromebooks, allowing for more personalized instruction and for educators to float around the classroom, monitoring progress and answering questions. IGNITE also offers additional technical programming, including certification for food handling, commercial driving, masonry, and welding training. The program regularly hosts graduation ceremonies (see Appendix Figure A3), where incarcerated individuals celebrate a new diploma, course completion, or job certification in cap and gown alongside family and friends.

Second, participation in IGNITE is incentivized and takeup rates are high (around 90%, per administrative data detailed below). Educational programming occurs during two dedicated hours of instruction woven into the daily schedule (see Appendix Figure A6). During instruction time, all

depends on the discretion of jail administrators. The programming itself is generally carried out in a small classroom of dedicated space with capacity and staffing constraints.

other jail activities cease. Non-participating individuals remain in their cells, while participating individuals receive tablets to access educational programming, which they can also use to access approved entertainment during non-IGNITE hours.

Finally, in addition to providing a wide range of education, IGNITE was intended to launch a meaningful cultural change for both incarcerated individuals and correctional officers. Correctional officers were asked to facilitate a learning environment by treating incarcerated individuals as students capable of change and growth. This perspective shift was felt immediately, with one jail administrator noting that the start of IGNITE represented a "shock to the jail culture" with officers saying, "We're doing what? We're bringing in teachers? We're providing tablets? Are you kidding?" (Barrett and Greene, 2023). In the post-IGNITE period, incarcerated individuals were described as not just waiting for court dates but as anticipating a productive life post-release because of their participation in the program. Correctional staff also expressed new views of incarcerated individuals. At a graduation ceremony, a correctional officer recalls holding the door for the graduating individuals and shaking their hands, stating that "It really humanizes people...It humanizes the inmate population, and it humanizes the deputy population." (Barrett and Greene, 2023). We return to this idea of cultural change using original survey data in Section 6.3.

2.3 Court Delays

We leverage administrative delays in the Genesee County court system to estimate the effects of IGNITE exposure. Genesee County, as in many other parts of the U.S., routinely experiences court delays and backlogs that can cause individuals to spend many months or even years in jail waiting for cases to be adjudicated. In addition to these routine delays, the start of policy responses to the COVID-19 pandemic in March 2020 and the subsequent spread of different variants exacerbated delays and resulted in major court closures which suspended trials indefinitely in both Genesee County. Administrative court delays contribute to lengthy jail spells. Among arrested individuals, the mean length of stay in Genesee County Jail is 1.5 months with a long right tail: the 90th percentile of time in jail is around four months.

Court delays stem from numerous opportunities for rescheduling, as shown in Appendix Figure A2. Red arrows show the primary court hearings that can be rescheduled in the District Court (which primarily handles initial hearings and misdemeanors) while purple arrows denote additional opportunities for court delays that can occur for felony cases in the Circuit Court. Because these court hearings occur prior to a finding of guilt or innocence, delays in the timing of these events will primarily affect individuals who are incarcerated pretrial and awaiting case disposition. In practice, these delays are common and highly impactful for an individual's time in jail. One jail administrator notes: "if you think about the people that are in jail, they expect to go to court. But it gets adjourned, they get another court date, it gets adjourned, another court date, it gets dismissed, they have to reissue a warrant, and they never leave jail" (Diaz, 2020). Such anecdotes align with patterns observed in our data where nearly 40% of all scheduled court dates are delayed, mostly by the District Court.

As we show below in Section 4.3, court delays appear largely idiosyncratic among individuals assigned to the same court during similar time periods and facing similar charges. Anecdotally, most delays are due to changes in the schedule of the judge or prosecutor assigned to the case and rarely occur at the request of the defendant or defense attorney.[8] While our data do not usually provide a rationale for each observed delay, we provide evidence below that incarcerated individuals send internal messages to jail administrators asking when they will next appear in court with a greater frequency when there are court delays. This pattern is consistent with anecdotal evidence that most delays are not caused by the defendant. We perform several robustness checks in Section 5.2 that narrow in on sources of delay which are more likely to be court-induced. We also use the fact that delays are similarly common in neighboring Saginaw County to conduct placebo checks.

3 Data and Sample Construction

Our analysis of IGNITE leverages administrative data from several sources along with original surveys of the local community, formerly incarcerated individuals, and correctional staff. This section describes each data source and key variables; Appendix C gives additional details.

3.1 Data Sources and Key Variables

The Jail Management System (JMS) and Recidivism. The JMS is a comprehensive electronic database, used by Genesee County Jail and other Michigan jails in Michigan since 2015, that tracks incarcerated individuals from booking to release. We obtained JMS data for both Genesee County Jail and neighboring Saginaw County Jail from January 2015 to May 2023. The JMS data include demographic information (age, name, race, home address), the arrest date, the booking date, the release date, charges, and case disposition outcomes for each incarcerated individual. Data from Genesee County also include the arrest location, which we use to identify the specific District Court that handles the case. We use these data to construct our primary outcome of recidivism, defined as whether an individual is rebooked in jail over a given time period.[9] Our baseline recidivism outcome is measured over three months, though we study recidivism for up to one year post-release. Although 90% of recidivism occurs within the same county (Yang, 2017; Raphael and Weiman, 2007; Sabol, 2007; Schnepel, 2018; Alper, Durose and Markman, 2018), we define recidivism as rearrest in either Genesee or Saginaw Counties to allow for mobility. In practice, results are virtually identical when we restrict to only Genesee County. We also check robustness to alternative measures of recidivism based on an individual being recharged or reconvicted.

The District and Circuit Court Register of Actions (ROA) and Court Delays. We collect ROAs from District and Circuit Courts in Genesee and Saginaw Counties by scraping publicly-

[8] Delays can also stem from changes in courtroom availability. For example a Genesee County District Court courtroom closed temporarily in 2023 because of a sewage leak (Jeltema, 2023).

[9] Officers that make arrests in the community are different from custody officers that run IGNITE within the jail. See Appendix C for more details.

available online case management systems. The ROAs represent permanent case histories of all hearings and events during an individual's case. These data include information on defendant charges, activities, proceedings, and filings for the case, along with dates and times of new court appointments, presiding judges, and notices of adjourned or rescheduled appointments. We use these records to create a comprehensive timeline of court hearings for each incarcerated individual. We identify court delays by changes to scheduled hearings that result in them being "removed from the calendar." Appendix Figure A7 shows an example ROA with such an identified delay. Our baseline specification uses an indicator for any District Court delay as an instrument, though we consider robustness to several other instrument specifications.

Jail Incident Reports and Misconduct. We use Jail Incident Report data from Genesee County Jail to capture within-jail misconduct and medical events for incarcerated individuals. Misconduct is categorized as either major or minor. Examples of major misconduct include threatening another with bodily harm, introducing contraband, violence and disruption, and refusing to follow instructions. Examples of minor misconduct include disorderly conduct, being in an unauthorized area, possession of unauthorized items, and lying.[10] Medical events include suicidality and suicide attempts. We observe each incident date and the name of the involved individuals. Our main analysis uses major misconduct as a primary outcome of interest, as this is both more consequential for staff and incarcerated individuals and also less likely to suffer from misreporting. We study minor misconduct and medical outcomes as secondary outcomes.

The Kites Electronic Message System. Kites is an electronic internal messaging system between jail administrators and incarcerated individuals in Genesee County. This messaging system is available to all incarcerated individuals via a kiosk in the jail. We observe the content of all sent messages along with the sender's identity, the date of the message, and any follow-up responses. Incarcerated individuals can send messages to request services from numerous individuals, including food service vendors, commissary services, medical personnel, and other administrative staff. Incarcerated individuals also often send questions about their case, including inquiries about court-initiated delays and when they will be released (see Appendix Figure A8). We use these data to assess the reaction of incarcerated individuals to court delays. We also study message sentiment to explore possible mechanisms.

Mt. Morris Educational Data. We obtain administrative data from Mt. Morris Consolidated Schools, which contain date- and time-stamped course advancement and completion records from 2021 onwards. The data also include pre- and post-instruction test scores from September 2020 to October 2023. Test scores are from Comprehensive Adult Student Assessment Systems (CASAS) exams in math and reading. Pre-assessments are administered to all IGNITE participants in order to place them in appropriate educational programming. Once enrolled in IGNITE, incarcerated

[10] Complaints of misconduct are investigated internally. If the investigator determines charges should be filed, they send the potential charges to a prosecutor who decides whether or not to charge the individual. If the individual is charged and sentenced they may have their ongoing jail time lengthened, though this is rare in practice.

individuals take post-assessments every 40 hours of completed instruction in order to measure their progress. In practice, we do not have post-assessments for all participants since tests are not completed if an individual is discharged without sufficient lead time to inform staff. In addition, electronic testing was only made available recently. In the end, only a few hundred paired pre- and post-assessments are available for analysis. Nevertheless, we find that incarcerated individuals with these assessments are representative of the jail population (see Appendix Table A2).

ViaPath Data. ViaPath is the internet service provider for Genesee County Jail and supplies connectivity for the Chromebook-based educational and tablet-based entertainment content available to IGNITE participants. In addition, ViaPath maintains logs of the amount of time incarcerated individuals spend in video calls and telephone calls to individuals outside of the jail. We have access to all ViaPath data, including individual identifiers, from February 2021 onward. We link these records to JMS data to determine the rate of IGNITE participation from tablet use. This exercise shows that 90% of individuals incarcerated on or after February 2021 participated in IGNITE.[11]

Community Survey. We conducted the Flint Community Survey in December 2023. Community members and two ministers of local churches distributed the survey to ensure it would not be influenced by IGNITE administrators. The survey was anonymous and asked respondents about their own experience being incarcerated in Genesee County Jail or the experiences of a close friend or family member. Importantly, the survey did not mention IGNITE in these questions. Respondents received a $25 restricted-use Walmart eGift card for completing the survey; the overall response rate was 87%. We construct our main exposure variable as an indicator for the respondent being held in Genesee County Jail after IGNITE was introduced and find that participants are well-balanced across this measure (see Appendix Table A3). The primary survey outcome is a respondent's perception of local law enforcement (this was elicited from all respondents). Secondary outcomes elicited from those with direct or indirect jail experience include their level of hopefulness for the future and their participation in positive activities (i.e., employment, education, or caregiving). The recruitment flyer is shown in Panel A of Appendix Figure A9. A link to the full survey and further details are given in Appendix D.

Custody Staff Survey. We administered The Genesee County Jail Custody Staff Survey to all current staff in January 2024. The purpose of the survey was to assess staff views towards incarcerated individuals in general, rehabilitation programs and educational opportunities in particular, and overall job satisfaction. The survey was anonymous and incentivized with a restricted-use $25 Walmart eGift card. The overall response rate was 44%. Here exposure is defined as spending more vs. less time with IGNITE participants; Appendix Table A4 shows staff characteristics are well-balanced across this measure. The recruitment flyer is shown in Panel B of Appendix Figure A9. A link to the full survey and further details are given in Appendix D.

[11] The ViaPath data is not timestamped, however, preventing measurement of participation in individual jail spells.

3.2 Main Analysis Sample

We combine the above data sources, merging on unique case or person identifiers, to construct our main analysis sample. Appendix Figure A10 summarizes the sample construction. We start with the universe of arrests in the Genesee County JMS data and set aside those booked before January 1, 2016, which we use as a hold-out sample to predict recidivism risk for certain analyses. We also exclude individuals booked after May 2022, so as to have enough time to measure 12-month recidivism for all individuals. We then merge JMS to ROAs and exclude incarcerated individual-spells where the individual was immediately released without charge, since these individuals did not interact with the court system and were ineligible for IGNITE. We also exclude a small portion (4%) of remaining individuals who are not Michigan residents, since we are unlikely to accurately measure their recidivism, individuals who are missing demographic information (1%), and individuals who are not yet released for at least three months (1%). We link these data to Jail Incident Data, Kites data, and Mt. Morris data using individual identifiers. The resulting sample includes 23,610 incarcerated individual-spells representing 14,794 unique individuals. When studying recidivism outcomes, we further drop a small minority (around 6%) of individuals who leave jail via a transfer to prison in order to avoid mechanical incapacitation effects.[12]

Summary statistics for this sample are shown in Appendix Table A5. The sample is 76% male, 53% Black, and the majority fall into the age range of 25-44.[13] 43% of individuals were booked in the past year and 53% are charged with a felony, with an average number of charges of 1.4. The average time in jail is 1.5 months, with a standard deviation of 4.2. Nearly 40% of the incarcerated individual-spells in Genesee County Jail experience a court delay in District Court and 18% are rebooked in the three months after release.

A potential concern when using administrative crime data is reporting behavior. In principle, correctional officers could under-report within-jail misconduct to demonstrate the effectiveness of IGNITE. In practice, this concern is lessened for our identification strategy, which uses variation in court delays rather than simple cross-sectional or over-time comparisons. Strategic misreporting would have to be correlated with the court delay instrument. Moreover, there is minimal concern of strategic misreporting for our primary recidivism outcomes, as rearrest and rebooking decisions in Genesee and Saginaw Counties are made by the local police forces—not by the Genesee County Sheriff or jail administrators.[14]

[12]The vast majority of incarcerated individuals are released from jail without conditions, in part because judges usually consider time spent in jail when sentencing. Below we show that we find no effects of time in IGNITE on different release conditions or on post-conviction outcomes, including being sentenced to state prison.

[13]The Black share is high relative to that of Genesee County (20%) and comparable to that of Flint (56%).

[14]While reported within jail and therefore subjective to some misreporting concerns, the shares of major misconduct and medical incidents are constant over time (see Appendix Figure A11). We also find that the schedule outlined in Appendix Figure A6 is roughly adhered to as measured by the floor-specific login times on Chromebooks (see Appendix Figure A12).

3.3 Motivating Evidence

Figure 1 motivates further study of the impact of IGNITE on recidivism by plotting the relationship between predicted and observed recidivism among those booked in Genesee County before and after the start of IGNITE. Specifically, we plot the average three-month recidivism rates of individuals booked before and after September 2020 by bins of the individuals' predicted recidivism risk, obtained from a logit regression on individual observables in a 2015 holdout sample (described above). Prior to the start of IGNITE, actual recidivism rates closely track these predictions. However, after IGNITE was launched, actual recidivism rates are significantly lower, uniformly across all levels of predicted risk. This pattern suggests a dramatic change in recidivism outcomes that coincides with the launch of IGNITE programming, though the purely time-series analysis is far from conclusive. We next develop and apply a more sophisticated quasi-experimental strategy to estimate causal effects of IGNITE exposure.

4 Empirical Strategy

4.1 Difference-in-IVs Approach

To formalize our IV strategy, consider a population of individuals booked into Genesee County Jail either before or after the launch of IGNITE in September 2020. Let $P_i \in \{0, 1\}$ indicate that individual i was booked post-IGNITE, let M_i^J count the number of months individual i spends in jail, and let M_i^I count the number of months i is exposed to IGNITE within the jail. To start simply, we assume that nobody booked pre-IGNITE is exposed to IGNITE: i.e., that $M_i^I = M_i^J \times P_i$. Our general IV strategy, developed below, relaxes this assumption to allow individuals booked before IGNITE to be partially exposed by virtue of their continued incarceration in September 2020.

Consider a simple causal model relating M_i^J and M_i^I to their effects on an outcome Y_i:

$$Y_i = Y_i(0) + \gamma_i M_i^J + \beta_i M_i^I, \tag{1}$$

where $Y_i(0)$ is an untreated potential outcome, i.e., the outcome that individual i would see with no time in jail or IGNITE. Here γ_i denotes the incremental effect of time in jail for individual i in the absence of IGNITE, while β_i denotes the incremental effect of IGNITE exposure of individual i holding fixed their time in jail. We assume these potentially heterogeneous causal effects are linear in time only for initial ease of exposition; below we discuss a more general causal model.

To estimate causal effects, we assume that individuals are as-good-as-randomly assigned to a court delay indicator $Z_i \in \{0, 1\}$. Here, again only for initial simplicity, we imagine Z_i is unconditionally randomly assigned (i.e., without any controls) and known to have no direct effect on outcomes, making it statistically independent of $(P_i, Y_i(0), \gamma_i, \beta_i)$. Court delays extend time in jail both pre- and post-IGNITE, making Z_i positively correlated with both M_i^J and M_i^I.

Under these conditions, an IV regression of Y_i on either M_i^J (in the pre-IGNITE period) or M_i^I

(in the post-IGNITE period), instrumenting with Z_i, identifies a weighted average of causal effects:

$$\beta^{Pre} \equiv \frac{Cov(Z_i, Y_i \mid P_i = 0)}{Cov(Z_i, M_i^J \mid P_i = 0)} = E\left[\omega_i^{Pre}\gamma_i \mid P_i = 0\right] \tag{2}$$

$$\beta^{Post} \equiv \frac{Cov(Z_i, Y_i \mid P_i = 1)}{Cov(Z_i, M_i^I \mid P_i = 1)} = E\left[\omega_i^{Post}(\gamma_i + \beta_i) \mid P_i = 1\right], \tag{3}$$

where ω_i^{Pre} and ω_i^{Post} are weights that average to one and capture the relative "complier" status of individual i: the relative amount of time in jail individual i is induced to via court delays.[15] When delays only weakly increase time in jail pre- or post-IGNITE (a natural first-stage monotonicity condition), both weighting schemes are convex: $\omega_i^{Pre} \geq 0$ and $\omega_i^{Post} \geq 0$.

Equation (2) shows that the pre-IGNITE IV identifies a convex weighted average of time-in-jail effects γ_i while Equation (3) shows the post-IGNITE IV identifies a weighted average of $\gamma_i + \beta_i$. The latter combines marginal IGNITE exposure effects, β_i, with baseline time-in-jail effects γ_i. To isolate IGNITE exposure effects, we consider the following condition on jail effects over time:

$$E\left[\omega_i^{Pre}\gamma_i \mid P_i = 0\right] = E\left[\omega_i^{Post}\gamma_i \mid P_i = 1\right]. \tag{4}$$

Equation (4) restricts heterogeneity in baseline time-in-jail effects pre- and post-IGNITE, similar in spirit to a conventional "parallel trends" restriction on untreated potential outcome changes before and after a policy change in conventional difference-in-differences analyses. Our condition is satisfied when, if not for the start of IGNITE, the IV estimates would not have changed in September 2020.[16] Clearly, this condition is satisfied when time-in-jail effects γ_i are homogenous or otherwise uncorrelated with the IV weights ω_i^{Pre} and ω_i^{Post}. Below we show how the condition can be probed graphically, as with "pre-trend" checks in conventional difference-in-differences; we also relax it by incorporating additional cross-sectional comparisons with neighboring Saginaw County.

Under Equation (2), a difference-in-IVs identifies a weighted average of IGNITE exposure effects. Specifically, differencing Equations (3) and (2), we have by Equation (4):

$$\beta^{\Delta} \equiv \beta^{Post} - \beta^{Pre} = E\left[\omega_i^{Post}\beta_i \mid P_i = 1\right], \tag{5}$$

where again $\omega_i^{Post} \geq 0$ when court delays do not reduce time in jail. β^{Δ} then captures a convex average of incremental effects of additional time exposed to IGNITE, β_i, holding time in jail fixed. Appendix E.2 generalizes this result to nonlinear causal effects of M_i^J and M_i^I, showing that under an appropriate generalization of Equation (4), the difference-in-IVs identifies an average causal response (ACR) function, as in Angrist and Imbens (1995). This shows that β^{Δ} generally captures

[15] Formally, $\omega_i^{Pre} = (M_i^J(1) - M_i^J(0))/E[M_i^J(1) - M_i^J(0) \mid P_i = 0]$ where $M_i^J(z)$ denotes individual i's potential time in jail when $Z_i = z$ and $\omega_i^{Post} = (M_i^I(1) - M_i^I(0))/E[M_i^I(1) - M_i^I(0) \mid P_i = 1]$ where $M_i^I(z)$ denotes individual i's potential time in IGNITE when $Z_i = z$. See Appendix E.1 for derivations of Equations (2) and (3).

[16] Note that, unlike a conventional parallel trends assumption, Equation (4) imposes no model on untreated potential outcomes $Y_i(0)$. Our strategy to avoiding omitted variable bias from $Y_i(0)$ can instead be viewed as "design-based" (Borusyak and Hull, Forthcoming), leveraging the as-good-as-random assignment of court delay shocks.

a weighted average of incremental IGNITE exposure effects at different margins of exposure time.[17]

4.2 IV Specification

Our main estimates come from a two-treatment IV specification that applies the above difference-in-IVs logic while accommodating additional controls and the possibility that individuals booked before the launch of IGNITE were nevertheless exposed to the program after September 2020. For a given outcome Y_i, we estimate:

$$Y_i = \beta M_i^I + \gamma M_i^J + X_i'\delta + \varepsilon_i \tag{6}$$

where again M_i^I and M_i^J count the months individual i is exposed to IGNITE and jail, respectively. Here X_i is a covariate vector that includes the indicator for a post-IGNITE booking P_i, along with other controls and a constant. We instrument for the two endogenous variables, M_i^I and M_i^J, with Z_i and $Z_i \times P_i$ (controlling for X_i), where Z_i again indicates a District Court delay.

The main IV coefficient of interest, β, reduces to a difference-in-IVs when no individuals booked pre-IGNITE are exposed to IGNITE (i.e., $M_i^J \times P_i$), and when the controls in X_i are saturated in P_i. In this case, the IV estimate is given by $\hat{\beta} = \hat{\beta}^{Post} - \hat{\beta}^{Pre}$ where $\hat{\beta}^{Post}$ and $\hat{\beta}^{Pre}$ are estimates from two separate IV specifications:

$$Y_i = \beta^{Pre} M_i^J + X_i'\delta^{Pre} + \varepsilon_i^{Pre} \tag{7}$$

$$Y_i = \beta^{Post} M_i^I + X_i'\delta^{Post} + \varepsilon_i^{Post}, \tag{8}$$

estimated in the $P_i = 0$ and $P_i = 1$ subsamples, respectively. The general two-treatment IV specification, Equation (6), allows for individuals with exposure to jail both pre- and post-IGNITE.

We include two types of controls in X_i in addition to P_i. First, in all IV specifications, we include a set of design controls intended to account for any non-randomness in the assignment of court delays, Z_i. The design controls are court division fixed effects (FEs) based on the location of arrest (since delays may be more likely for some divisions of Genesee County than others), booking month and day-of-week FEs for the first scheduled hearing (since delay propensities may vary over time), the number of charges and FEs for charge type (felony, misdemeanor traffic, misdemeanor DUI, and other misdemeanor crime) to account for different probabilities of delay by the seriousness of the charge. Second, in some IV specifications, we include auxiliary controls reflecting individual demographics and other characteristics. These controls are not needed for identification, but may yield precision gains by absorbing residual variation in the outcomes.

[17] The appendix model continues to impose additive separability of time-in-jail and time-in-IGNITE effects, while allowing for arbitrary effect heterogeneity for these two treatments individually (i.e. fully relaxing the linear dose-response form of (1)). Without separability, simple differencing as in (5) may not suffice to fully isolate IGNITE effects from non-IGNITE time-in-jail effects, although this is less of an issue for recidivism outcomes where pre-IGNITE time-in-jail effects are small and insignificant. In practice, we find minimal effect heterogeneity across many observables including predicted time-in-jail—suggesting the baseline linear model gives a reasonable approximation.

4.3 Identifying Assumptions and Tests

IV estimates of (6) capture average causal effects of IGNITE exposure and non-IGNITE time in jail under four assumptions. The first three assumptions are standard in IV analyses and the fourth follows Equation (4). Here we discuss each assumption and provide some initial empirical support.

Our first identifying assumption is that the court delay indicator Z_i is as-good-as-randomly assigned given the design controls in X_i. This assumption is consistent with the institutional setting (see Section 2.3) as well as a number of empirical balance tests shown in Table 1. Panel A of this table shows that several observable individual characteristics are uncorrelated with Z_i given the design controls, while Panel B further shows balance on the characteristics of census tracts in which individuals reside. The characteristics in these two panels constitute our auxiliary controls. Section 5.2 summarizes additional checks of as-good-as-random assignment.[18]

Alongside these balance tests, Panel C of Table 1 shows that experiencing a court delay significantly extends an individual's time in jail—an implicit instrument relevance condition for our IV strategy. On average, individuals spend 0.4 months (around two weeks, or 27%) longer in jail when they experience a court delay (given the design controls). This first stage is highly significant, with an F-statistic of around 40.5. We further explore the first-stage relationship below.

Our second and third identifying assumptions are a standard IV exclusion restriction and monotonicity condition: i.e., that court delays do not affect our outcomes of interest except by extending time in jail, and that delays only weakly increase time in jail (both pre- and post-IGNITE). These assumptions are also consistent with the institutional setting, and we probe them empirically in Section 5.2. In Section 5.3 we further discuss how our exclusion and monotonicity assumptions may be more plausible than in a conventional "judge IV" strategy which leverages as-good-as-random District Court judge assignment instead of court delays (while also showing estimates from this alternative strategy).

The final identifying assumption follows Equation (4) and allows us to interpret estimates of β in terms of the causal effects of additional IGNITE exposure holding time in jail fixed. Intuitively, the assumption is satisfied when IV estimates of time-in-jail effects would not have systematically changed in September 2020 if not for the launch of IGNITE. There are two primary threats to this assumption. First, as in a conventional difference-in-differences approach, our identifying assumption could be violated if another unobserved policy change or broader change in Genesee County occurred around the start of IGNITE. Unlike with a conventional difference-in-differences approach, however, such time-varying confounds would have to affect the *effects* of time in jail rather than potential outcome *levels*. Below we conduct a non-parametric analysis of court delay effects over time—akin to the standard "pre-trend" check in conventional difference-in-differences analyses—which suggests minimal scope for such time-varying confounds in our setting.

[18] Appendix Table A6 checks for differential attrition, which could introduce bias even when delays are as-good-as-randomly assigned. Reassuringly, we find that court delays do not cause individuals to exit our baseline three-month recidivism analysis sample at a significantly higher rate. For longer windows there is some evidence of differential attrition but effect sizes are small. With 12-month recidivism, for example, court delays are found to make individuals 1.1 percentage points less likely to stay in the sample off a baseline follow-up rate of 98%.

The second potential threat to this assumption is that the types of individuals who comply with the court delay instrument changed before and after IGNITE. More formally, Equation (4) could fail if β^{Pre} and β^{Post} put different weight on heterogeneous time-in-jail effects. Below, we show these effects are relatively small in the pre-IGNITE period and that causal effects are generally homogeneous across observable characteristics—reducing concerns of bias from effect heterogeneity. More direct evidence comes from Table 2, which shows the average observable characteristics of instrument compliers before and after IGNITE.[19] We find no statistically significant differences in these averages, suggesting compliers are broadly comparable before and after the start of IGNITE.[20]

We make three further points on the interpretation of our IV estimates. First, while our primary interest is on the causal interpretation of the IGNITE exposure effect β, we note that the combined $\beta + \gamma$ coefficient may be causally interpretable under weaker conditions. Specifically, $\beta + \gamma$ captures the average effect of increased time in jail in the post-IGNITE period when baseline time-in-jail effects are not comparable pre- and post-IGNITE (i.e., when Equation (4) fails). Correspondingly, we report estimates of this combined effect along with estimates of β.

Second, we note that β may retain its interpretation as an average causal effect of IGNITE exposure when the conventional IV assumptions (as-good-as-random assignment, exclusion, and monotonicity) fail, provided the bias from such violations manifests similarly in the pre- and post-IGNITE periods. For example, the causal interpretation of β is robust to court delays directly affecting within-jail misconduct by increasing an individual's frustration with the criminal justice system (a potential exclusion restriction violation) provided such frustration effects are similar pre- and post-IGNITE.[21] In Section 5.3 we develop extensions of our baseline approach that further weakens this exclusion restriction by incorporating additional comparisons to Saginaw County.

Third, we note that β targets the average effect of exposure to IGNITE programming and not the effect of program participation itself. While participation rates are known to be high—around 90% on average—we do not have individual participation data that would let us study the latter. Under a plausible monotonicity condition, a hypothetical extended IV approach would scale our estimates by such a takeup rate.[22] In this scenario, the magnitude of our effects can be viewed as giving a lower bound on the magnitude of effects from IGNITE participation.

[19] Specifically, we report estimated means of individual characteristics weighted by the same measures of compliance status that underlie the IV estimates of jail exposure effects pre- and post-IGNITE. See Appendix E.3 for details.

[20] Similarly, Appendix Figure A13 shows that the weights our baseline IV specification puts on different margins of exposure time are relatively similar pre- and post-IGNITE. See again Appendix E.3 for details on these calculations. Below we show effects are homogeneous across individuals with different predicted time in jail, reducing concerns about any pre-post differences in the exposure time weights.

[21] Estimates of γ or the combined $\beta + \gamma$ coefficient would not, however, be causally interpretable in such cases.

[22] The exclusion restriction in this hypothetical specification would generally rule out within-jail spillovers across individuals who do and do not participate in IGNITE, in contrast with our preferred exposure treatment specification.

5 Main Findings

5.1 Misconduct and Recidivism Effects

Figure 2 plots the reduced-form variation underlying our primary IV estimates. Each point shows the estimated effect of court delays on one of our primary outcomes—either weekly major misconduct or three-month recidivism—separately by an individual's booking month. We obtain these estimates by regressing the outcome on the court delay instrument, adjusting for the design controls and the auxiliary controls from Panels A and B of Table 1.

The figure shows strikingly different reduced-form effects of court delays pre- and post-IGNITE. Before September 2020, delayed individuals on average saw at most a small increase in within-jail weekly misconduct rates and no increase in post-release recidivism. A pre-IGNITE IV specification would scale these reduced-form effects by the corresponding first stage to find small or no effects on time in jail before the start of IGNITE. In contrast, court delays had sizable negative effects on both misconduct and recidivism after the start of IGNITE. As described in Section 4.1, a difference-in-IVs estimate contrasting these pre- and post-IGNITE estimates would therefore suggest large negative IGNITE exposure effects.[23] Importantly for this interpretation, the figure shows no clear trends in the reduced-form effects of either outcome either before or after September 2020. It is therefore plausible that, if not for the start of IGNITE, the time-in-jail effects on misconduct or recidivism would have remained slightly positive or insignificant.

Table 3 reports our main reduced-form and IV estimates of misconduct and recidivism effects. IV estimates in columns 1 and 3 are from Equation (6), with a post-IGNITE indicator and all design controls in X_i. In columns 2 and 4 we further include the auxiliary controls; consistent with the balance tests in Table 1, these controls do not materially change the estimates. Reduced-form estimates come from regressing outcomes on the two instruments, the court delay indicator and its interaction with the post-IGNITE dummy, adjusting for the controls.[24]

The table reports large estimated effects of IGNITE exposure on both within-jail misconduct and post-release recidivism. On average, one additional month in IGNITE is estimated to reduce weekly major misconduct incidents by 0.16 and three-month recidivism by 8.1 percentage points. These represent reductions of 49% and 18%, respectively, relative to reported control complier means.[25] As in Figure 2, we find no effects of additional months in jail on recidivism pre-IGNITE. The combined recidivism effect of months in IGNITE and jail is therefore similar to the estimated IGNITE exposure effect. We find a larger positive effect of months in jail on misconduct pre-IGNITE. The combined misconduct effect of post-IGNITE months-in-jail is therefore smaller than the estimated IGNITE exposure effect (around -8.2 percentage points). We further contextualize

[23] Appendix Figure A14 shows the corresponding first-stage plot. The average effect of court delays on time in jail is roughly constant pre- and post-IGNITE, at around two weeks.

[24] Appendix Table A7 shows corresponding first-stage estimates. First-stage F-statistics, computed as in Sanderson and Windmeijer (2016), are around 80 for the time-in-jail treatment and around 56 for the time-in-IGNITE treatment.

[25] Control complier means come from IV regressions of $Y_i \cdot \mathbf{1}[M_i^J < m]$ on $\mathbf{1}[M_i^J < m]$ instrumenting by Z_i with both design and auxiliary controls. Following Appendix E.3, this estimates average outcomes when individuals spend less than m months in jail. We set m to correspond to a "control" condition of less than one week in jail.

our primary IGNITE exposure effect estimates in Section 6.1, below.

Figure 3 shows how estimated misconduct and recidivism effects vary over time. We plot IV estimates of IGNITE exposure effects (obtained as in columns 2 and 4 of Table 3) for two alternative outcomes: whether an individual experienced any major misconduct in a given week since booking (Panel A of Figure 3) and whether an individual was rebooked in a given month since release (Panel B of Figure 3). Panel A shows that estimated misconduct effects are relatively stable over time, with around an 8 percentage point reduction in misconduct risk in any given week since booking. In contrast, Panel B shows that estimated recidivism effects grow steadily over time—to around a 15 percentage point reduction in one-year recidivism post-release.

Appendix Figures A15 and A16 explore heterogeneity in our baseline misconduct and recidivism effect estimates by individual demographics, prior offense status, high vs. low predicted lead exposure from the Flint water crisis, and predicted recidivism risk.[26] Specifically, we estimate versions of Equation (6) which (i) add as treatments interactions of the months in IGNITE or months in jail treatments with bins of observable characteristics, (ii) add to the instrument list interactions of $(Z_i, Z_i \times P_i)$ with the same bins, and (iii) add the bin dummies as controls. The figures plot resulting estimates of bin-specific months-in-IGNITE and months-in-jail effects, which are valid under conditional versions of our main identifying assumptions. Overall, we find roughly similar effect estimates across demographic groups, prior offense status, and predicted lead exposure (see Appendix Figure A15). We do, however, find meaningful heterogeneity by predicted recidivism risk (see Appendix Figure A16): recidivism reductions are larger for riskier individuals.

Estimated effects on alternative recidivism and misconduct measures, along with other related outcomes, are shown in Appendix Table A1. Panel A shows significant IGNITE effects on the three-month probability of an individual being recharged or reconvicted, as well as in rates of weekly minor misconduct. In addition, we find significant IGNITE effects on the rate of weekly serious violent misconduct within jail, which is less susceptible to reporting bias.[27] Panel B shows we find no significant effects of IGNITE exposure on whether an individual is released on tether, released on bail, sentenced to prison, convicted, or released to a rehabilitation center. The lack of effects here is unsurprising since pretrial judges are not permitted to consider in-jail misconduct. Judges are also not provided such information by custody staff prior to making pretrial, conviction, or sentencing decisions. The large post-release recidivism effects we find in Table 3 thus do not appear to be mediated by the channels in Panel B of Appendix Table A1 (e.g. incapacitation effects). We also find no effects of IGNITE exposure on suicide attempts or other medical incidents within jail, in contrast to the large major misconduct effect estimates in Table 3.

[26] Lead exposure is predicted from an individual's residential zip code. We predict recidivism risk by a logit regression on the auxiliary controls in the 2015 holdout sample. Appendix Figure A17 shows we do not find heterogeneity by predicted time in jail, constructed analogously by OLS.

[27] More serious misconduct includes threatening another with bodily harm; escaping, attempting to escape or helping another to escape from the law; and inflicting bodily injury upon another person.

5.2 Robustness Checks

As discussed above, Tables 1-2 and Figure 2 show balance and trend analyses that broadly support our IV strategy. Here we discuss a number of additional robustness checks, summarized in Table 4.

One category of potential concerns stems from the COVID-19 pandemic. Major pandemic policy responses occurred between March 2020 (the start of lockdowns) and June 2021 (when vaccines were first widely distributed), which overlaps with the launch of IGNITE in September 2020. Around this period, one might imagine that jailed individuals were of relatively higher criminal risk (if lower-risk individuals were released to reduce jail populations), that misconduct rates declined simply because individuals were more segregated within the jail (due to COVID-19 quarantine protocols), or that the pandemic and related policies more broadly affected how misconduct and recidivism outcomes were measured.[28] For the first concern, it is reassuring that we find, if anything, larger effects among individuals with high levels of predicted recidivism risk (see Appendix Figure A16). For the second and third concerns, Table 4 reassuringly shows that we obtain similar results as our baseline estimates when estimating effects on misconduct not involving others (e.g., counterfeiting or forgery) and that we obtain similar estimates for both misconduct and recidivism when controlling for a time trend (interacted with the court delay instrument) or when altogether excluding March 2020 to June 2021 from the analysis sample. We further show in Section 5.3 that recidivism rates declined in Genesee County around September 2020 relative to Saginaw County, despite both counties being subject to the same statewide COVID-19 protocols, and that the same IV specification deployed in Saginaw County finds no placebo IGNITE effects. Together, these checks suggest our findings are not driven by changing conditions from the pandemic.

A second category of potential concerns is violations of as-good-as-random instrument assignment. One might be concerned, for example, that some District Court delays are initiated by the incarcerated individual and thus are potentially endogenous. Reassuringly, Table 4 shows we obtain similar or larger estimates when using alternative definitions of the instrument that are less susceptible to manipulation: i.e., when including Circuit Court delays, restricting to COVID-19 and fiscal crisis delays, using only delays around Federal holidays, or restricting to delays occurring on days with multiple court delays across different individuals. Panel A of Appendix Figure A18 gives further evidence that delays are not self-initiated. We find that the probability an incarcerated individual sent a Kites message with a communication or court-related request (using the words *talk, speak, need, can, please, court* or *judge*) jumped in the four weeks after the COVID-19-induced court closure on March 17, 2020. Panel B shows no such increase in an analogous event study one year prior. Together with the balance checks in Table 1, these checks broadly support the view that court delays are as-good-as-randomly assigned.

A third category of potential concerns focuses on the IV exclusion restriction. Even when delays are as-good-as-randomly assigned, one might be concerned that they have direct effects on misconduct or recidivism by, for example, increasing an individual's frustration with the criminal

[28]The total Genesee County Jail population declined by around 40% in the onset of the pandemic before recovering to pre-pandemic levels in the winter of 2020.

justice system. Table 4 shows we obtain similar estimates when controlling for whether an individual experienced multiple court delays, as one proxy for such frustration. Recall also that our baseline IV approach allows for any direct effects of the instrument provided they are similar in the pre- and post-IGNITE period. Therefore, any time-invariant "frustration effect" would be differenced out and would not bias our IGNITE effect estimates. In Section 5.3 we discuss checks using alternative differencing strategies, which are valid under different or weaker exclusion restrictions.

A final potential concern, specific to the within-jail misconduct outcome, is that IGNITE participation or access to tablets through the program simply occupied the time that individual would have otherwise spent engaging in misbehavior. In other words, IGNITE might have reduced within-jail misconduct simply via an within-jail "incapacitation" effect. This could affect the interpretation of the large reductions in misconduct we find in our baseline specification, but would not introduce bias. Reassuringly, Table 4 shows we obtain similar misconduct effects when restricting to times of day when there was no IGNITE programming as well as to hours when individuals did not have access to commissary tablets for entertainment purposes.

5.3 Alternative Identification Strategies

Table 5 shows we obtain very similar recidivism effect estimates from alternative difference-in-IVs specifications which use Saginaw County as a "control" group in the post-IGNITE period. Column 1 presents our baseline estimates from Genesee County. Column 2 estimates Equation (6) with data from both counties in the post-IGNITE period only, instrumenting (M_i^I, M_i^J) with $(Z_i, Z_i \times S_i)$, where $S_i \in \{0, 1\}$ indicates that individual i was booked in Saginaw County, and including the baseline design and auxiliary controls (with S_i replacing P_i). As in our baseline analysis, we find no recidivism effect of increased time in jail in the absence of IGNITE but a large negative recidivism effect from IGNITE exposure.[29] The -6.4 percentage point IGNITE effect is similar to our baseline -8.1 percentage point effect, replicated in column 1. In column 3, we estimate our main specification in Saginaw County only, obtaining a tight null estimate of the change in time-in-jail effects before and after September 2020. This suggests there was no simultaneous regional change in jail or rearrest policy confounding our baseline Genesee County estimates. Finally, column 4 subtracts this Saginaw County placebo check from our baseline over-time IGNITE exposure effect in a "double" difference-in-IVs specification, showing again a large recidivism effect of -7.5 percentage points. Notably, this specification weakens our baseline identifying assumptions by differencing out any direct time (or county) effects that would otherwise confound the estimates in columns 1 or 2.

Appendix Table A8 shows estimates from an alternative IV strategy which, as in Bhuller et al. (2020), instruments with leave-one-out averages of District Court judge "leniency." We estimate Equation (7) in both the pre-IGNITE and post-IGNITE periods, instrumenting for time in jail M_i^J with L_i, defined as the average time in jail of individuals other than i who were assigned to individual i's District Court judge, residualized on court division fixed effects (FEs), day-of-week FEs, and month FEs. This judge IV strategy is valid when judges are as-good-as-randomly

[29] We do not observe misconduct outcomes in Saginaw County; see Appendix C.3 for details.

assigned given the controls, with their assignment only affecting outcomes through time in jail and shifting all individuals' time in jail in the same direction across judges. These exclusion restriction and monotonicity conditions may be less plausible than our baseline identifying assumptions, since judges make many decisions that affect an individual's experience within and beyond jail and also because these decisions may result in longer stays in jail for some individuals and shorter stays for others. Correspondingly, a joint test proposed by Frandsen, Lefgren and Leslie (2023) rejects judge IV exclusion and monotonicity decisively, in both pre- and post-IGNITE samples (see Appendix Table A9). Nevertheless, the table shows difference-in-IVs estimates that are qualitatively similar (though generally less precise) as our preferred IGNITE exposure effect estimates.

Appendix Figure A19 shows estimates from an alternative difference-in-differences strategy, which compares overall trends in three-month recidivism rates from Genesee County to corresponding trends from neighboring Saginaw County. We plot event study coefficients from regressing the recidivism of individuals booked in either county on a Genesee County indicator interacted with the individual's booking date relative to December 2019: the period after which a nontrivial share of individuals booked in Genesee County were exposed to IGNITE starting in September 2020 (see Appendix Figure A20).[30] Recidivism trends are similar between the two counties prior this period but diverge sharply thereafter, with individuals booked in Genesee County seeing an average reduction in recidivism of around 3 percentage points (see Appendix Table A10). This translates to a reduction of around 11% for one month of exposure to IGNITE, similar to the 18% reduction found using our preferred IV strategy.[31] While less fine-grained than our IV strategy, the event study helps build further confidence in its core logic with flat pre-trends showing no unusual pre-IGNITE recidivism dynamics in Genesee County.

6 Contextualization and Mechanisms

6.1 Cost of Crime Effects

Appendix Table A11 translates our main recidivism effect estimates into estimates of the effect of IGNITE exposure on post-release social costs of crime. Specifically, we estimate Equation (6) with an outcome that measures the total cost of future crimes at different horizons. We following the most conservative cost of crime estimates in Miller et al. (2021).[32] For our baseline three-month recidivism horizon, we find that one additional month of IGNITE exposure decreases the social cost of crime by around $2,957 per person. As in Figure 3, this estimated effect grows over time,

[30] We do not use an individual's time to IGNITE exposure because this is endogenous for individuals booked prior to September 2020.

[31] Individuals in Genesee County spent around 1.5 months in jail both pre- and post-IGNITE (Appendix Figure A14), average three-month recidivism is around 19 percentage points at baseline (see Appendix Table A10), and (3pp/1.5 months)/19pp $\approx$ 11%.

[32] This calculation divides future crimes into DUIs, drug offenses, motor vehicle offenses, persons offenses, property offenses, public order offenses, weapons offenses, and other offenses. Within each of these crime types, we take the lowest social cost estimate from Miller et al. (2021) to provide the most conservative estimate; for example, we use the cost estimate for assault instead of murder for persons offenses.

to a per-person-month reduction of around \$5,615 over a horizon of 12 months. These estimates are around 24% and 12% of the control complier means, respectively, and are likely to understate true crime cost reductions both because we use conservative cost estimates and because we do not count any costs of within-jail misconduct. The large crime cost reductions are especially notable given stable spending in Genesee County Jail pre- and post-IGNITE (see Appendix Figure A4).

6.2 Literature Comparison

Figure 4 compares our baseline recidivism effect estimates to other quasi-experimental estimates in the literature. Panel A compares the estimated relative effect of IGNITE exposure on one-year recidivism to comparable relative effect sizes of other rehabilitative programs for justice-involved individuals. When possible, we compute one-month effects of these programs by assuming linear effects. See Appendix Section F for details on the effect size calculations.

This literature comparison shows that our main one-month IGNITE exposure effect (a 23.3% reduction in one-year recidivism) is comparable to or larger than the effects from other rehabilitative programs, including CBT programming (Heller et al., 2017), open prisons (Mastrobuoni and Terlizzese, 2022), diversion (Mueller-Smith and Schnepel, 2021; Augustine et al., 2022), and restorative justice conferencing (Shem-Tov, Raphael and Skog, 2022). We also compare our months-in jail-estimates to incarceration effects found in other studies of jail and prison (Panel B) and present estimates from programming geared towards high-risk (but not necessarily justice-involved) individuals for further context (Appendix Figure A21). Together, these comparisons show that IGNITE—deployed in a U.S. jail among an especially high-risk adult population—generates similar reductions in recidivism as programming in other correctional environments and countries.[33]

6.3 Potential Mechanisms

Given the educational nature of IGNITE programming, one obvious candidate driver of the large decrease in misconduct and recidivism we find is increased literacy and numeracy. Incarcerated individuals tend to enter IGNITE with very low reading and math achievement as measured by CASAS scores. Figure 5 shows that the distribution of pre-assessment math and reading scores are centered around a 5th-6th grade equivalence and a 6th-7th grade equivalence, respectively.

A comparison of test scores before and after an individual's enrollment in IGNITE programming suggests substantial improvements in math and reading skill. Figure 5 shows that, on average, individuals gained the equivalent of around one grade level in both subjects. These gains also appear widespread: the full distribution of post-test scores is shifted to the right in both subjects, with a marked tilt towards higher grade equivalencies in reading. While these gains may not solely reflect human capital accumulation (for example because of the possibility of an increased routine and reduced distraction in IGNITE improving individuals' test-taking skill), they are nevertheless suggestive of improved educational achievement. The magnitude of achievement gains are massive

[33]Notably, the 95% confidence interval from our IV specification overlaps with that of Bhuller et al. (2020) for incarceration in Norway.

even when compared to some of the most effective educational interventions documented in recent quasi-experimental literatures (e.g., Cohodes and Roy 2023) and consistent with policymakers' views that IGNITE's educational programming was a broad success (Erwin, 2023).

However, institutional knowledge and our first-stage estimates both suggest that formal educational programming is not the full story behind the large misconduct and recidivism effects. As noted in Section 2.2, IGNITE was intended to create significant cultural change within the jail, which could potentially enhance formal education and even possibly affect individuals who did not participate in programming. Our court delay instrument only increased exposure to IGNITE by around two weeks on average, making it unlikely that the misconduct and recidivism reductions effects came from added instruction time alone. To explore the possible role of a within-jail cultural shift, we next turn to our survey analyses.

Table 6 reports the effect of IGNITE exposure on impressions of law enforcement among community members and the formerly incarcerated. IGNITE exposure is defined as an indicator for whether the survey respondent or someone in their close social network served time in Genesee County Jail after the program was launched in September 2020 (about one-third of those surveyed were exposed). The primary outcome is respondents' views of law enforcement, which was elicited from everyone. We find that IGNITE exposure predicts a positive view: agreement with the phrase "Law enforcement looks out for me and my community" is 23 percentage points for IGNITE-exposed respondents which is roughly 75% of the unexposed mean. Column 2 moreover shows this effect is driven by respondents who had a longer IGNITE exposure. These findings indicate that IGNITE improved perceptions of procedural justice and police legitimacy, a core component of effective policing (Tyler and Fischer, 2014). Columns 3-6 show results on post-incarceration outcomes which were elicited from the roughly 70% of respondents that were justice-affected. Here we find positive but insignificant effects of IGNITE on engagement in positive activities (defined as employment, education, or caretaking), and no effect on hopefulness about the future. Appendix Table A3 shows that IGNITE-exposed individuals are observably similar to other individuals in the survey, supporting the interpretation of these coefficients as estimates of causal effects.

The more favorable views of law enforcement by formerly incarcerated individuals are mirrored by more favorable views of educational programs for incarcerated individuals by custody staff at Genesee County Jail. Staff who regularly interact with IGNITE participants are 34 percentage points more likely to view education in jails as worthwhile (see Table 7 and Appendix Figure A22).[34] In contrast, there is no overall effect on job satisfaction (see Appendix Table A12). Exposure to IGNITE participants is well-balanced among respondents (Appendix Table A4), again supporting a causal interpretation of these findings.

Appendix Figure A23 further supports the apparent IGNITE culture change, via a sentiment analysis of Kites messages sent by incarcerated individuals to custody staff before and after September 2020. We use the NRC Word-Emotion Association Lexicon from Mohammad and Turney

[34] Appendix Table A13 lists some representative answers to a free-response question asking staff for ways IGNITE has changed their experience working in Genesee County Jail.

(2010), which labels each English word as being associated with up to two sentiments (negative and positive) and up to eight emotions (anger, anticipation, disgust, fear, joy, sadness, surprise, and trust). Panel A shows a significant increase in the share of Kites words categorized as having only positive sentiment of around four percentage points or 14% higher than the pre-IGNITE level. The share of words categorized as having only negative sentiment and of words categorized as having "neutral" sentiment (either both positive and negative or neither positive nor negative) fell after IGNITE. Panel B further shows that shares of words in Kites messages associated with anticipation, anger, fear, and sadness fell post-IGNITE, while the share of words associated with trust increased. Together with the survey analysis, these qualitative findings support anecdotal evidence of a broad shift in Genesee County Jail culture for both incarcerated individuals and staff.

7 Conclusion

This study provides the first quasi-experimental evidence that educational programming in U.S. county jails can reduce post-release recidivism and potentially mitigate the kinds of incarceration cycles that have long stymied criminal justice policymaking. Exposure to the Genesee County Jail IGNITE program dramatically and persistently reduces both within-jail misconduct and post-release recidivism, with relatively similar effects by race, sex, age, and prior offense status. We estimate that one additional month of IGNITE exposure reduces the social cost of crime by at least $5,300 per person in the year after their release. Qualitative evidence suggests a broad cultural change within the jail as a core mechanism, resembling the kinds of rehabilitation-oriented policies found outside the U.S.

We expect the IV strategy we develop for estimating IGNITE exposure effects using quasi-random court delays to prove useful in other settings where idiosyncratic delays in administrative policy extend an individual's time exposed to an institution before and after a policy reform. One could imagine, for example, evaluating the effects of reforms on healthcare or other benefit programs by comparing individuals whose appointments are and are not affected by idiosyncractic rescheduling in the pre- and post-reform period. Our empirical framework shows how such variation can be leveraged with standard IV assumptions and a novel restriction on over-time effect homogeneity, which we demonstrate can be validated empirically.

An important question for future research is whether the large IGNITE effects we estimate can be replicated in other U.S. jails. Indeed, the National Sheriffs Association is committed to bringing IGNITE to all jails in the country, though this process is still in early stages. As is often the case with scaling-up attempts, rigorous evaluation of this process will be essential (Duflo, 2004), potentially using similar tools as those developed here. But broadly our findings suggest that—as in distressed Flint, Michigan—something can work.

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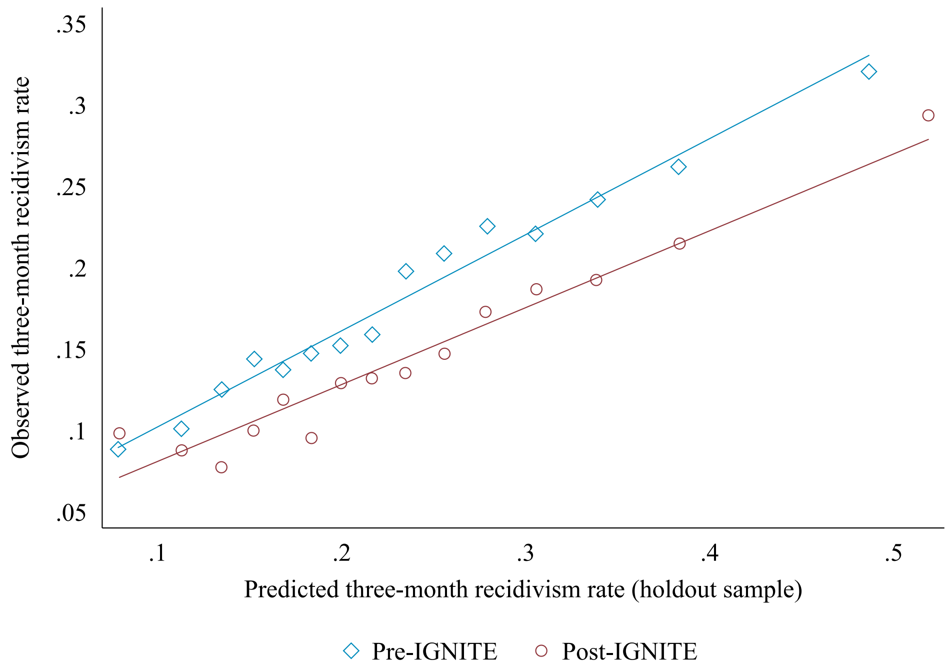
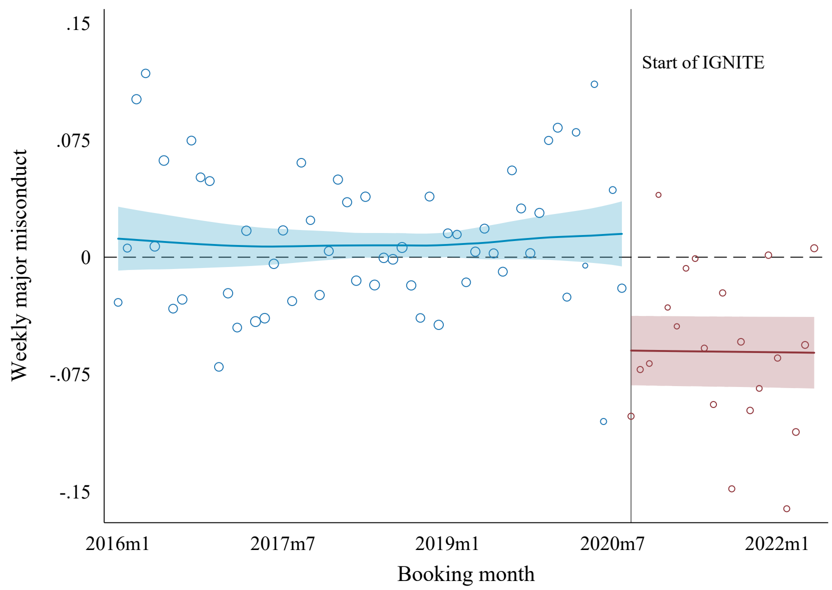
Figure 1: Observed vs. Predicted Recidivism, Before and After IGNITE



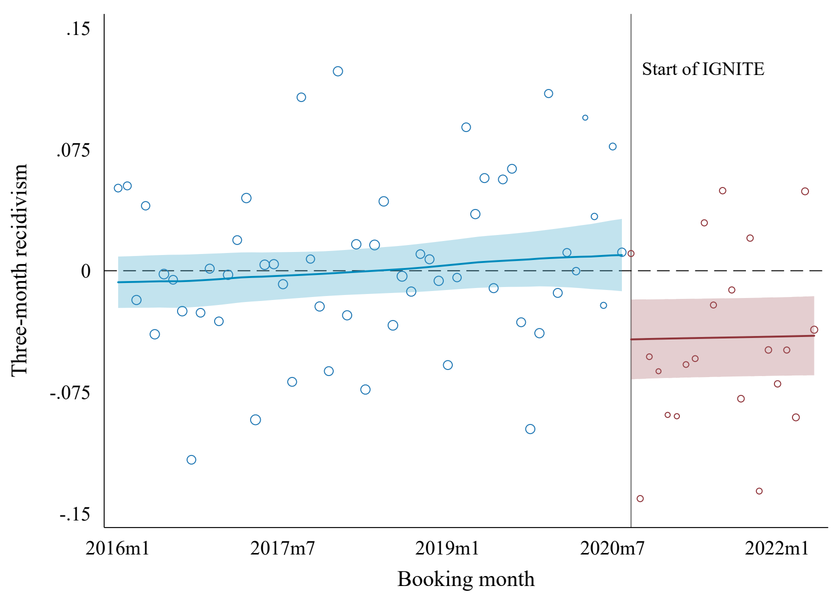
Notes: This figure plots observed three-month recidivism rates in Genesee and Saginaw Counties by equal-sized bins of predicted three-month recidivism risk, before and after the start of IGNITE. Predicted recidivism risk is estimated by a logit regression in the 2015 holdout sample. Predictors are the design and auxiliary controls discussed in the main text.

Figure 2: Reduced-Form Effects of Court Delays by Booking Month

(a) Misconduct



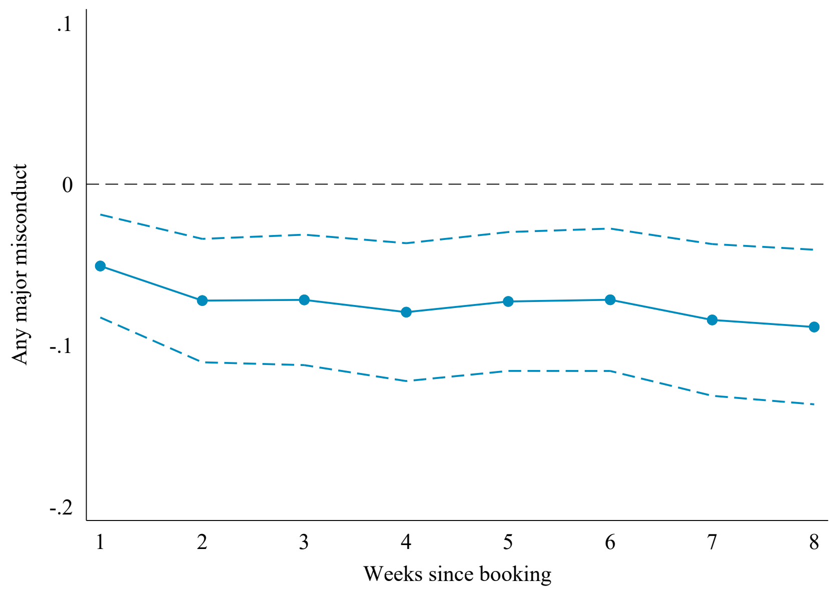
(b) Recidivism



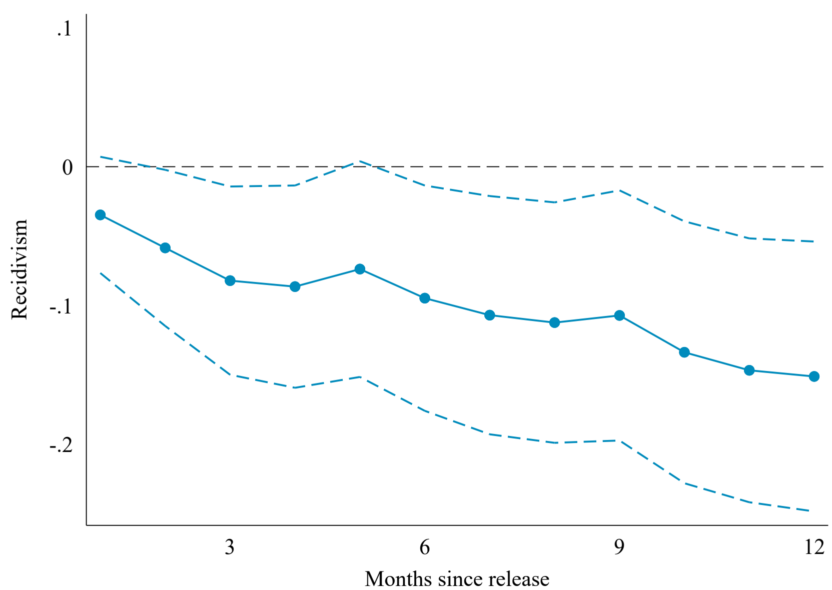
Notes: This figure plots covariate-adjusted monthly average differences in weekly major misconduct rates and three-month recidivism rates between individuals who do and do not experience a court delay. Major misconduct rates are given by the total number of major misconduct events observed for a unique incarceration episode divided by the number of weeks spent in jail for that episode. Recidivism is an indicator for whether the incarcerated individual was rebooked within three months since the last episode's release. Each point indicates the coefficient from regressing the outcome on a court delay indicator and covariates among individuals booked in a given month. Covariates include the design and auxiliary controls discussed in the main text. The fitted lines are obtained by local linear regression with rule-of-thumb bandwidths, weighting by the number of incarcerated individuals booked in a month. Shading indicates 95% confidence intervals derived from individual-clustered standard errors. The vertical lines indicate the beginning of the IGNITE program in September 2020.

Figure 3: Misconduct and Recidivism Effects Over Time

(a) Misconduct



(b) Recidivism

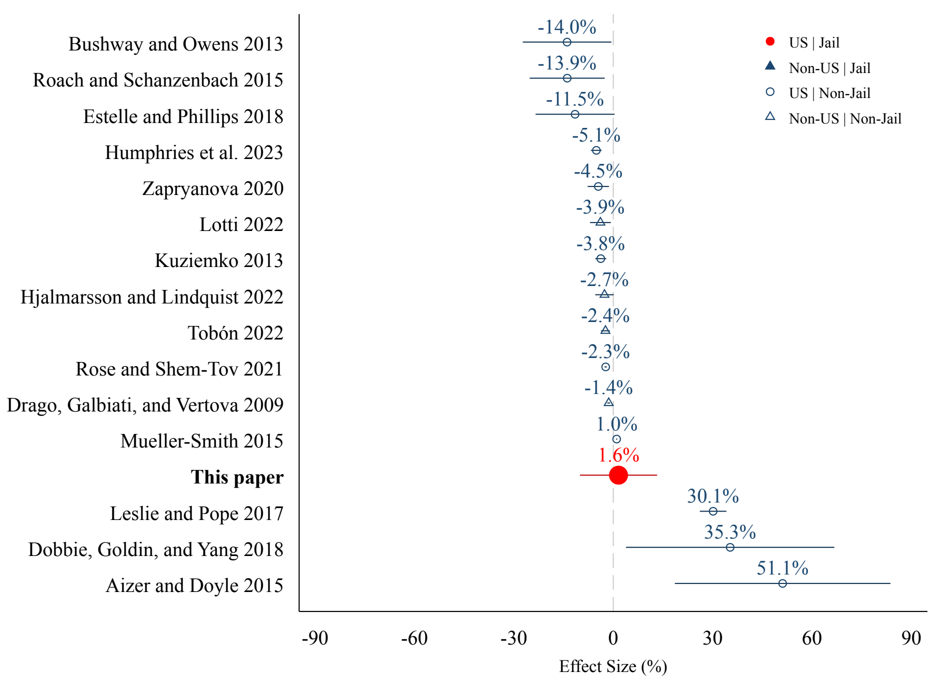


Notes: This figure plots estimated time-in-IGNITE effects from our main IV specifications, with outcomes being the probability of involvement in major misconduct in t weeks since booking (Panel A) and the probability of ever being rebooked within t months of release (Panel B) for $t = 1, \ldots, 12$. All specifications include the design and auxiliary controls discussed in the main text. Blue dashed lines indicate 95% confidence intervals derived from individual-clustered standard errors.

Figure 4: Literature Comparisons

(a) Time in Programs

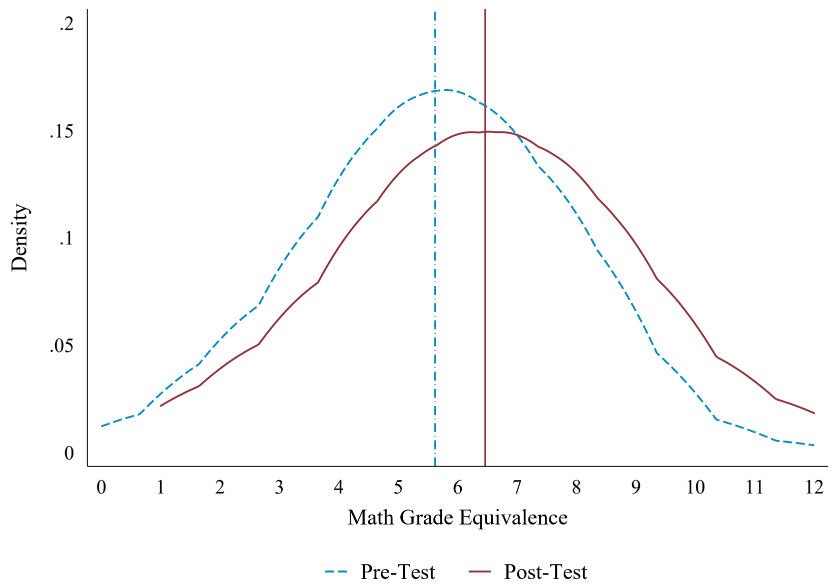
(b) Time Incarcerated



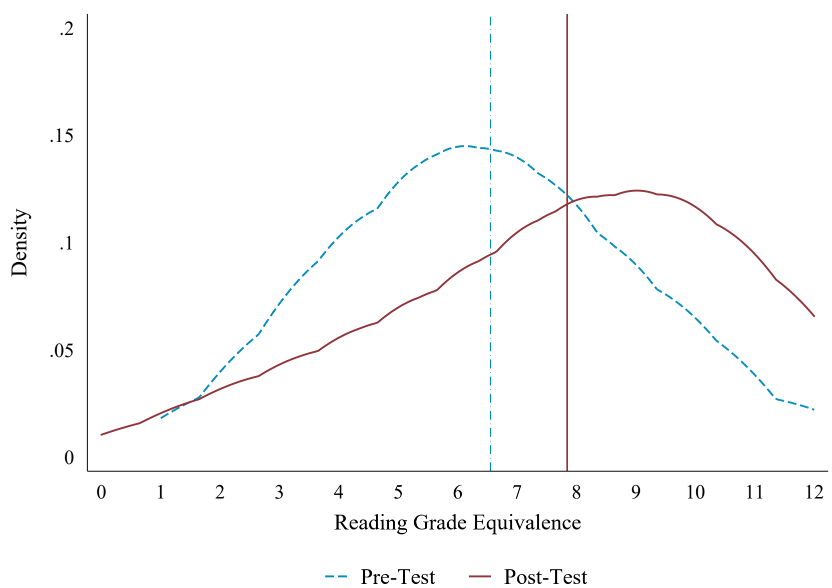
Notes: This figure compares estimated treatment effect sizes from this paper to others from the literature. See Appendix F for details on the papers and effect size construction. We distinguish between U.S. vs. non-U.S. studies and studies in jail vs. non-jail contexts. Each point indicates the estimated effect of treatment on recidivism as a percent of the control mean. When possible, we use one-year recidivism outcomes and scale effects by time in treatment. 95% confidence intervals are shown around each point.

Figure 5: Pre- and Post-Test Performance

(a) Math



(b) Reading



Notes: This figure plots kernel density estimates of the distribution of math (N: 439) and reading (N: 309) test scores before and after IGNITE program participation, of individuals who completed both tests. Scores come from Comprehensive Adult Student Assessment Systems (CASAS) exams administered by Mt. Morris Consolidated Schools and are normalized to standard grade equivalents. Each distribution is estimated with a Gaussian kernel and 1.5 times the rule-of-thumb bandwidth.

Table 1: Court Delay Balance Tests and First Stage

	Overall Mean (1)	Difference in Means (2)	Standard Error (3)
Panel A: Individual Characteristics			
Female	0.240	0.005	(0.007)
Age 25-34	0.378	0.007	(0.008)
Age 35-44	0.225	-0.008	(0.007)
Age 45-54	0.122	-0.009*	(0.005)
Age 55-64	0.058	-0.001	(0.004)
Age 65+	0.009	0.001	(0.001)
Black	0.534	-0.013	(0.008)
Booked in Past Year	0.433	-0.000	(0.007)
Has a Public Defender	0.116	0.005	(0.005)
Panel B: Census Tract Characteristics			
Share with Elevated Blood Lead Level	0.031	-0.004	(0.004)
Share Black	0.429	-0.011	(0.008)
Share High School Graduate or Higher	0.848	-0.002	(0.007)
Log Median Household Income	10.322	-0.011	(0.044)
Missing Census Tract Information	0.055	0.003	(0.004)
F-Statistic for Joint Test [p-value]		1.353 [0.204]	
Panel C: First Stage			
Months in Jail	1.558	0.396***	(0.061)
Observations		23,610	

Notes: Panels A and B summarize balance tests for the court delay instrument. Column 1 reports the sample mean of different individual characteristics. Columns 2 and 3 report estimated coefficients and standard errors from regressing the characteristics on the instrument. All regressions include the design controls discussed in the main text. The census tract characteristics in Panel B are linked to an individual's residential address as recorded in the JMS data. A tract's share with elevated blood lead level refers to the proportion of individuals with above 4.5 micrograms of lead per deciliter of blood among those tested in the census tract in 2017. A tract's share Black, share high school graduate or higher, and log median household income are obtained from the 2016 American Community Survey. The missing census tract information indicator equals one if an individual cannot be matched to a census tract. The F-statistic is for the joint test of balance across all individual and census tract characteristics. Panel C reports the coefficient from a first-stage regression of months in jail on the instrument including the design controls. Standard errors are clustered by individual. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 2: Complier Characteristics, Before and After IGNITE

	Pre-IGNITE (1)	Post-IGNITE (2)	Pre − Post (3)	Full Sample (4)
Panel A: Individual Characteristics				
Female	0.151	0.086	0.064	0.240
	(0.066)	(0.054)	(0.083)	
Age 25-34	0.223	0.296	-0.073	0.378
	(0.116)	(0.100)	(0.149)	
Age 35-44	0.237	0.103	0.134	0.226
	(0.091)	(0.088)	(0.125)	
Age 45-54	0.163	0.213	-0.050	0.122
	(0.084)	(0.075)	(0.111)	
Age 55-64	0.116	0.156	-0.040	0.058
	(0.047)	(0.055)	(0.070)	
Age 65+	0.061	0.044	0.017	0.009
	(0.035)	(0.027)	(0.042)	
Black	0.593	0.461	0.132	0.534
	(0.115)	(0.107)	(0.154)	
Booked in Past Year	0.407	0.320	0.086	0.433
	(0.118)	(0.102)	(0.152)	
Panel B: Crime Characteristics				
Crimes against Persons	0.942	0.774	0.168	0.387
	(0.100)	(0.091)	(0.132)	
Crimes against Property	0.303	0.079	0.224	0.224
	(0.103)	(0.096)	(0.137)	
Crimes against Public Order	0.050	0.019	0.031	0.099
	(0.059)	(0.045)	(0.073)	
Drug Crimes	0.134	0.124	0.009	0.170
	(0.082)	(0.075)	(0.108)	
Weapons Crimes	0.328	0.360	-0.032	0.148
	(0.117)	(0.102)	(0.152)	
Traffic Crimes	0.098	0.179	-0.082	0.278
	(0.070)	(0.069)	(0.095)	
Other Crimes	0.252	0.133	0.119	0.166
	(0.105)	(0.097)	(0.140)	
Panel C: Census Tract Characteristics				
Share with Elevated Blood Lead Level	0.038	0.033	0.005	0.031
	(0.009)	(0.006)	(0.011)	
Share Black	0.488	0.356	0.131	0.429
	(0.093)	(0.096)	(0.132)	
Share High School Graduate or Higher	0.862	0.836	0.026	0.848
	(0.018)	(0.016)	(0.023)	
Log Median Household Income	10.323	10.298	0.025	10.322
	(0.106)	(0.097)	(0.143)	
Missing Census Tract Information	0.167	0.215	-0.049	0.055
	(0.062)	(0.067)	(0.089)	

Notes: Columns 1 and 2 report estimated coefficients from IV regressions of the interaction between a given characteristic and months in jail on months in jail, instrumenting with a court delay indicator and controlling for the design and auxiliary controls, for individuals booked pre- and post-IGNITE. As discussed in Appendix E, the estimated coefficients can be interpreted as a weighted average of instrument complier characteristics along different margins of time-in-jail response. Column 3 reports the difference between the pre- and post coefficients while Column 4 reports the sample mean of the characteristic. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 3: Effects of IGNITE on Misconduct and Recidivism

	Misconduct		Recidivism	
	(1)	(2)	(3)	(4)
Panel A: Reduced Form				
Court Delay × Post-IGNITE	-0.074***	-0.074***	-0.039***	-0.040***
	(0.016)	(0.016)	(0.013)	(0.012)
Court Delay	0.010	0.010	-0.002	-0.003
	(0.007)	(0.007)	(0.007)	(0.006)
Panel B: IV				
Months in IGNITE	-0.161***	-0.160***	-0.081***	-0.081***
	(0.041)	(0.041)	(0.032)	(0.032)
Months in Jail	0.079***	0.077***	0.014	0.009
	(0.030)	(0.029)	(0.031)	(0.029)
Months in IGNITE+Months in Jail	-0.082***	-0.082***	-0.067***	-0.071***
	(0.025)	(0.025)	(0.021)	(0.021)
Control Complier Mean	0.329	0.329	0.457	0.457
Design Controls	Yes	Yes	Yes	Yes
Auxiliary Controls	No	Yes	No	Yes
Observations	23,610	23,610	22,191	22,191

Notes: This table reports reduced-form and IV estimates of effects on weekly major misconduct incidents in jail (Columns 1 and 2) and three-month recidivism after release (Columns 3 and 4). Reduced-form estimates come from regressions of the outcome on the court delay instrument interacted with an indicator for whether an individual was booked post-IGNITE. IV estimates come from the specification discussed in the main text. All specifications include the design controls discussed in the main text; Columns 2 and 4 also include the auxiliary controls. Control complier means are computed as discussed in Appendix E. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 4: Robustness Checks

	Misconduct (1)	Recidivism (2)
Baseline Specification	-0.160***	-0.081***
($N = 23,610$)	(0.041)	(0.032)
High Predicted Risk Sample	-0.123*	-0.247**
($N = 5,810$)	(0.064)	(0.101)
Misconduct not Involving Others	-0.048***	
($N = 23,610$)	(0.012)	
Time Trend × Delay Control	-0.069***	-0.101***
($N = 23,610$)	(0.018)	(0.025)
Excluding COVID Period	-0.108***	-0.103***
($N = 20,658$)	(0.033)	(0.041)
Including Circuit Court Delay	-0.223*	-0.151**
($N = 23,610$)	(0.120)	(0.070)
COVID/Fiscal Crisis Delays Only	-0.078*	-0.091*
($N = 23,610$)	(0.047)	(0.050)
Delays Close to Holidays	-0.124***	-0.127***
($N = 23,610$)	(0.042)	(0.038)
Multiple Delays per Day	-0.163***	-0.094***
($N = 23,610$)	(0.043)	(0.035)
Multiple Delay Events Control	-0.098***	-0.088**
($N = 23,610$)	(0.028)	(0.036)
Non-IGNITE Hours Misconduct	-0.127***	
($N = 23,610$)	(0.039)	
Non-Tablet Hours Misconduct	-0.105***	
($N = 23,610$)	(0.041)	

Notes: This table summarizes robustness checks for the primary IV estimates of months-in-IGNITE effects on weekly major misconduct and three-month recidivism. The first row reports estimates from our baseline specification (Columns 2 and 4 of Table 3). The second row restricts the sample to individuals with the top quartile of predicted recidivism risk, as in Appendix Figure A16. The third row estimates effects on rates of weekly major misconduct not involving other individuals. The fourth row adds a linear time trend interacted with the court delay instrument as controls. The fifth row excludes observations from March 2020 to June 2021. The sixth row adds Circuit Court delays to define the instrument. The seventh row uses only delays associated with the COVID-19 pandemic or fiscal crises to define the instrument. The eighth row uses only delays within two weeks before or after a federal holiday to define the instrument. The ninth row only uses delays on days with there were multiple rescheduling events to define the instrument. The tenth row controls for an individual experiencing multiple delay events. The eleventh row estimates effects on rates of weekly major misconduct restricting to times of day with no IGNITE Programming. The twelfth row estimates effects on rates of weekly major misconduct restricted to times of day with no tablet access. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 5: Alternative Difference-in-IVs Estimates of Recidivism Effects

	Difference-in-IVs			
	Post vs. Pre, Genesee (Baseline)	Post, Genesee vs. Saginaw	Post vs. Pre, Saginaw	Double Diff-in-IVs
	(1)	(2)	(3)	(4)
Months in IGNITE	-0.081***	-0.064***		-0.075**
	(0.032)	(0.024)		(0.030)
Months in Jail	0.009	0.021	0.009	
	(0.029)	(0.017)	(0.035)	
Months in Jail × Post			-0.006	
			(0.007)	
Observations	23,610	6,380	14,227	37,837

Notes: This table reports IV estimates of effects on three-month recidivism with alternative IV specifications. Column 1 reports the baseline estimates from Column 4 of Table 3. Column 2 reports estimates from a specification estimated in the post-IGNITE period comparing Genesee County and Saginaw County, as described in the main text. Column 3 estimates our baseline specification in Saginaw County, with the Months in Jail × Post treatment replacing the Months in IGNITE treatment in Genesee County. Column 4 reports the difference in the estimated coefficient on Months in IGNITE in column 1 and the estimated coefficient on Months in Jail × Post in Column 3. All estimates include the design and auxiliary controls discussed in the main text. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 6: Community Survey Results

	Positive View of Law Enforcement		Engaged in Positive Activities		Hopeful about the Future	
	(1)	(2)	(3)	(4)	(5)	(6)
IGNITE Exposure	0.233**	-0.126	0.087	0.093	-0.051	-0.103
	(0.112)	(0.214)	(0.117)	(0.242)	(0.126)	(0.256)
IGNITE Exposure × Months in Jail		0.187*		-0.031		0.039
		(0.094)		(0.103)		(0.102)
Control Mean	0.333	0.333	0.656	0.656	0.656	0.656
Observations	87	87	62	62	62	62

Notes: This table reports estimated effects of IGNITE exposure on the binary outcomes of a community individual having a positive views of law enforcement (Columns 1 and 2), engagement in positive activities post-incarceration (Columns 3 and 4), and hopefulness about the future post-incarceration (Columns 5 and 6). A survey wave indicator is included as a control in all specifications and Columns 2, 4, and 6 additionally control for months in jail. Questions on post-incarceration outcomes were only asked to those who had themselves or had a close relation incarcerated recently. See Data Appendix D for details on the outcomes and exposure variable. Robust standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 7: Custody Staff Survey Results

Outcome Variables	High IGNITE Exposure (1)	(2)	Mean of Control Group (3)	Observations (4)
Rehab. Programs Worth Time & Money	0.018	(0.154)	0.611	45
Educ. Programs Worth Time & Money	0.344**	(0.144)	0.333	45
Rehab. as Important as Punishment	0.045	(0.153)	0.611	45
F-Statistic for Joint Test [p-value]	2.278*	[0.094]		45

Notes: This table reports estimated effects of high vs. low IGNITE exposure on whether custodial staff in Genesee County Jail agree or strongly agree with a listed statement. The high IGNITE exposure treatment is an indicator for a respondent answering the question "How often do you interact with inmates in IGNITE?" with "Usually" or "Always." Robust standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix

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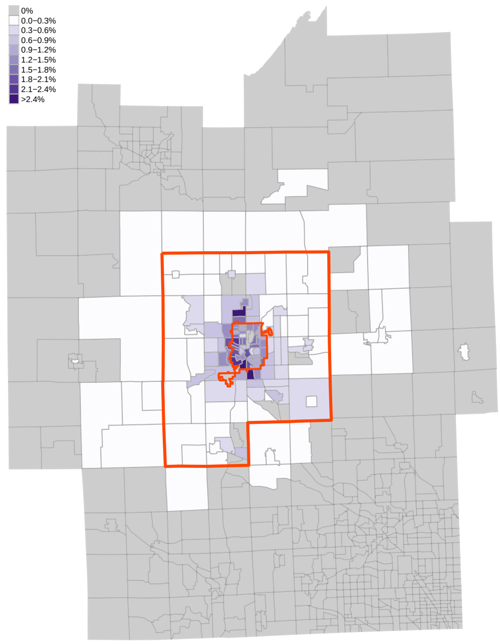
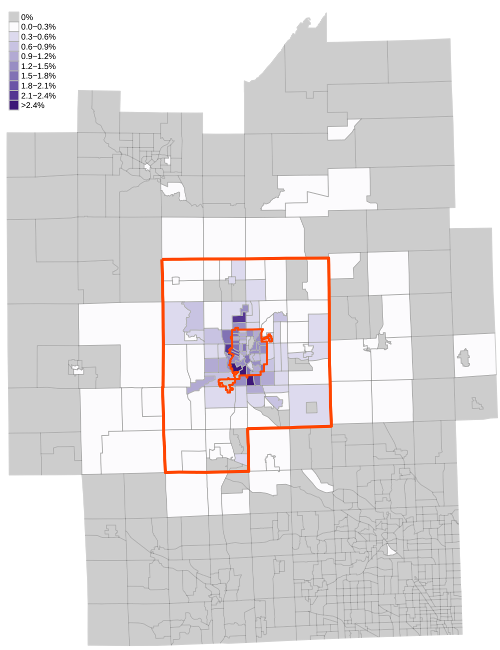
A Appendix Figures and Tables

Appendix Figure A1: Residential Address of Individuals Booked into Genesee County Jail

(a) Share of Arrests, Pre-IGNITE



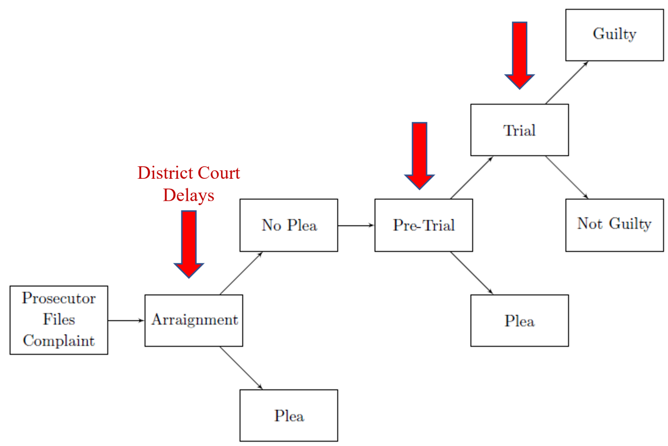
(b) Share of Arrests, Post-IGNITE



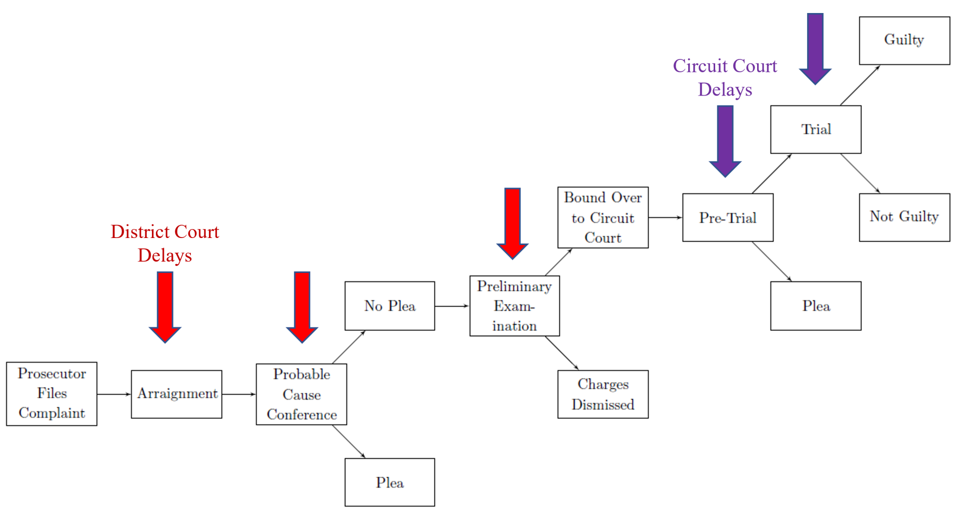
Notes: This figure shows the census tract of residence for individuals booked into Genesee County Jail before and after the start of IGNITE. The map includes Genesee County and adjacent counties (Lapeer, Tuscola, Saginaw, Shiawassee, Livingston, and Oakland). Borders of Genesee County and Flint are outlined.

Appendix Figure A2: Court Processes in Genesee County

(a) Misdemeanors



(b) Felonies



Notes: This figure illustrates the court process of typical cases in Genesee County by case type. Red arrows indicate the stages at which District Court delays are possible. Purple arrows indicate the stages at which Circuit Court Delays are possible.

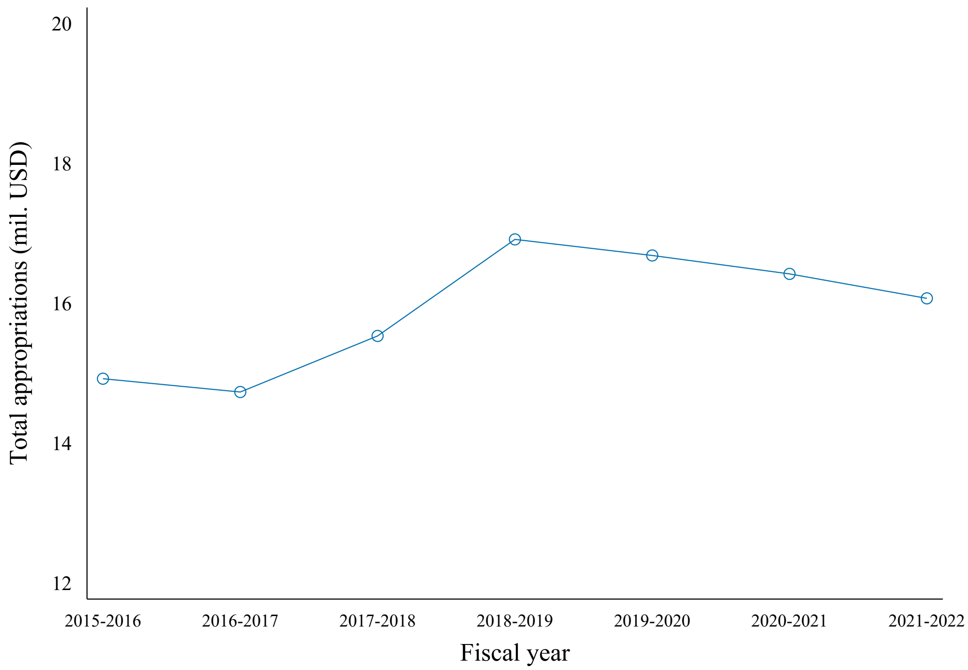
Appendix Figure A3: Photos of IGNITE

(a) Classroom

(b) Graduation

Notes: Panel A shows individuals participating in the IGNITE program within Genesee County Jail, from NPI and NSA (2023). Panel B shows an IGNITE graduation ceremony, from May (2021).

Appendix Figure A4: Budget of Genesee County Corrections Division



Notes: This figure plots the total appropriations of the Genesee County Sheriff Corrections Division from fiscal year 2015-2016 to fiscal year 2021-2022. Data for fiscal years 2015-2016 to 2018-2019 are available at Genesee County Controller's Office (2019) and data for fiscal years 2019-2020 onward are available at Genesee County Controller's Office (2022). We subtract capital outlay from total appropriations in fiscal years 2019-2020 onward for consistency with fiscal years 2016-2016 to 2018-2019.

Appendix Figure A5: Examples of IGNITE Programming Tracks

Inmate A	Inmate B	Inmate C
Has high school diploma	No high school diploma/GED	Limited grade school education
Goal: begin an associates degree or specialty field	Goal: Obtain a GED and learn about opportunities	Goal: Learn to read and write
Enrolls in:	Enrolls in:	Enrolls in:
College-level classes	GED program	Reading, Writing, Math
CDL program	Trade School/VR Simulator	Health & Wellness
Serve safe program	Financial Literacy	

Notes: This figure shows example tailoring of IGNITE education to incarcerated individuals with different educational backgrounds and goals. Source: NPI and NSA (2023).

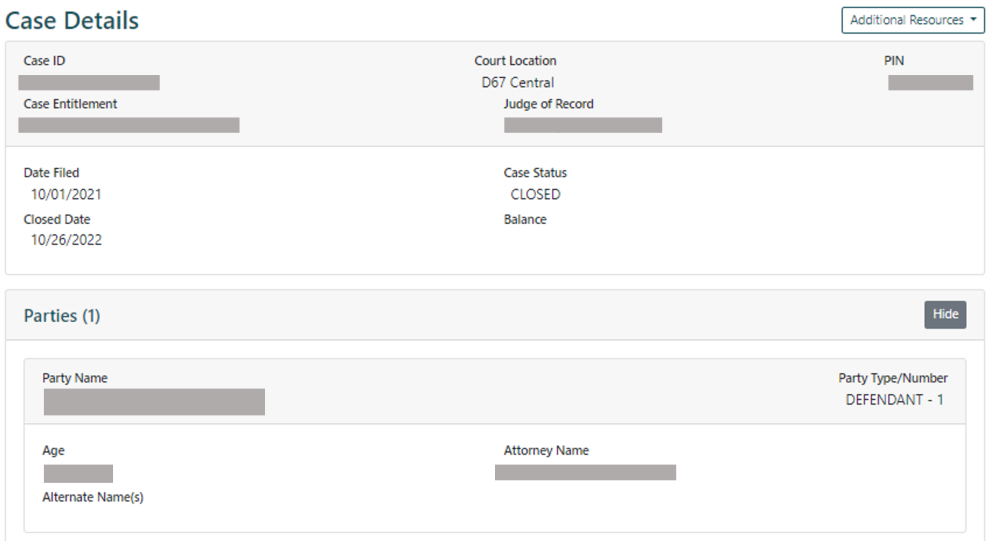
Appendix Figure A6: Examples of IGNITE Daily Schedules

		3rd floor	4th floor	5th floor	Reminders
6am		shift change	shift change	shift change	
6:30am		Breakfast	Breakfast	Breakfast	trays off floor by 7:15
8am		IGNITE	Mandatory Cleaning	Mandatory Cleaning	
9am		Cleaning/ Hour out's	Dayroom open to all	IGNITE	
10am		Dayroom open to all	IGNITE	Dayroom open to all	
11am		IGNITE	Dayroom open to all	Dayroom open to all	
noon		Lunch	Lunch	Lunch	trays off floor by 12:45
1p		Dayroom open to all	Dayroom open to all	IGNITE	
2p		Dayroom open to all	Dayroom open to all	Dayroom open to all	
3p		Dayroom open to all	IGNITE	Dayroom open to all	
4pm		Dinner	Dinner	Dinner	trays off floor by 4:45
5p-5:30p		Mandatory Cleaning	Mandatory Cleaning	Mandatory Cleaning	
5:30p-6:30p		Shift change	Shift change	Shift change	
6:30p		Dayroom open to all	Dayroom open to all	Dayroom open to all	Dayroom open no later than 6:30pm
8p		Dayroom open to all	Dayroom open to all	Dayroom open to all	
9p		Dayrooms Closed	Dayrooms Closed	Dayrooms Closed	Dayroom open until 9pm
Service such as Laundry, Commissary, Medical, PO visits are not allowed on housing units during IGNITE time					

Notes: This figure shows an example schedule of IGNITE program times and other Genesee County Jail activities, by the cell floor of incarcerated individuals. Source: NPI and NSA (2023)

Appendix Figure A7: Example ROA

(a) Case Information



(b) Court Delay

Notes: Panel A shows an example Genesee County ROA. Panel B shows an example court delay as identified in the ROA description.

Appendix Figure A8: Example Kites Messages

Example 1. "when will i see the judge for my court date really??? i had an original court date to see judge [redacted] on [redacted] it got cancelled along with yesterdays i guess??? what's up??"

Example 2. "when is my next court date or when do i get released"

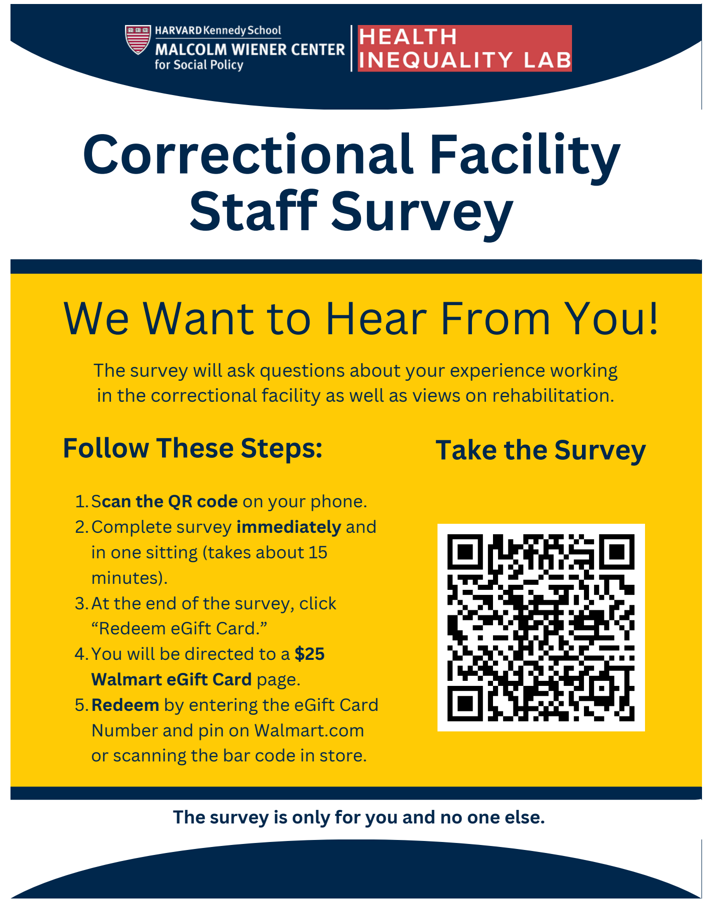
Example 3. "do i have any new court dates? thank u....."

Notes: This figure shows example Kites text messagess sent by incarcerated individuals to custody staff in Genesee County Jail.

Appendix Figure A9: Survey Recruitment Flyers

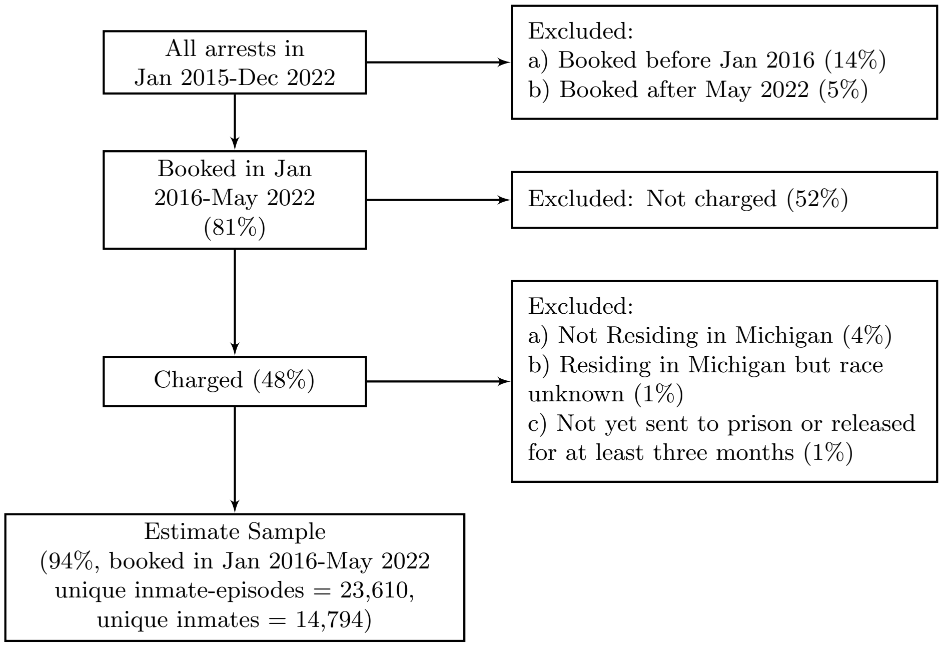
(a) Community Survey

(b) Custody Staff Survey



Notes: This figure shows the recruitment flyers used for the Flint Community Survey and Genesee County Jail Custody Staff Survey. The QR codes on the actual flyers linked to a personal survey page; the codes in this figure link to a survey demo.

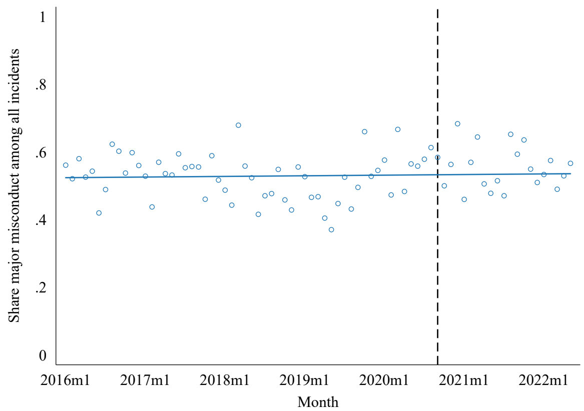
Appendix Figure A10: Main Analysis Sample Construction



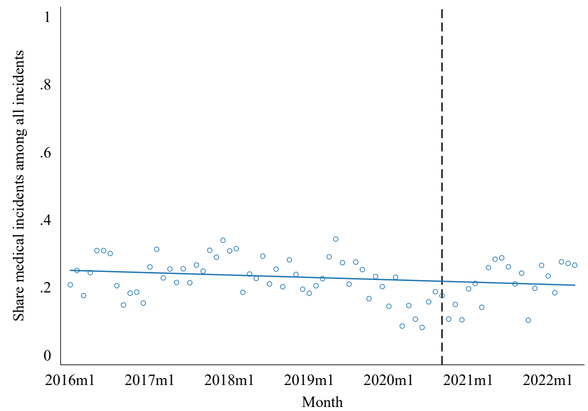
Notes: This figure summarizes the construction of our main analysis sample. See Appendix C for more details.

Appendix Figure A11: Share of Major Misconduct and Medical Incidents

(a) Major Misconduct

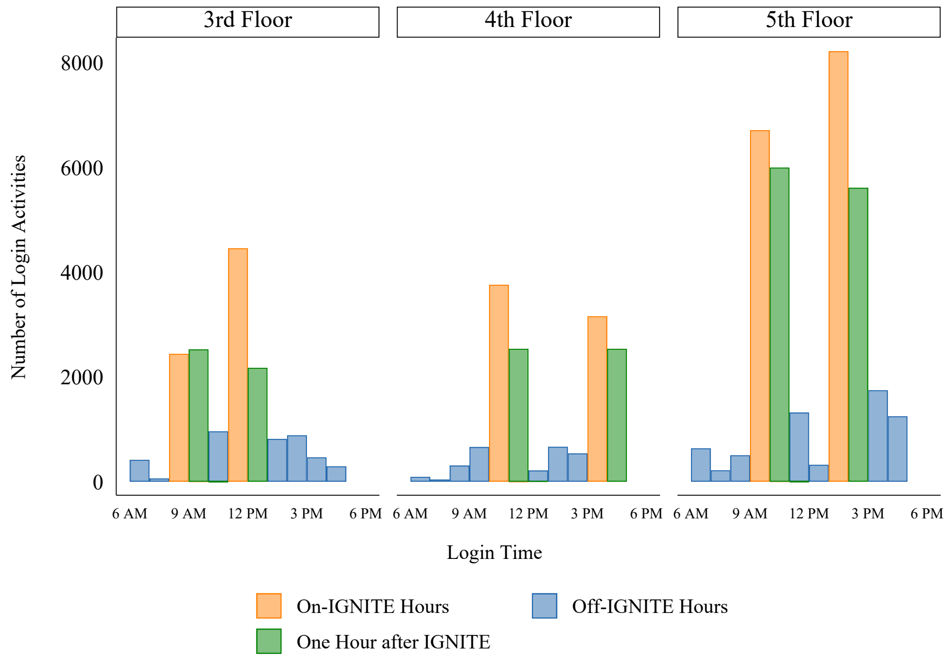


(b) Medical Incidents



Notes: Panel A plots the monthly share of major misconduct among all misconduct incidents in Genesee County Jail. Panel B plots the monthly share of medical incidents among all incidents. The vertical dashed lines indicates the start of IGNITE (September 2020). The horizontal lines indicate best-fit trends.

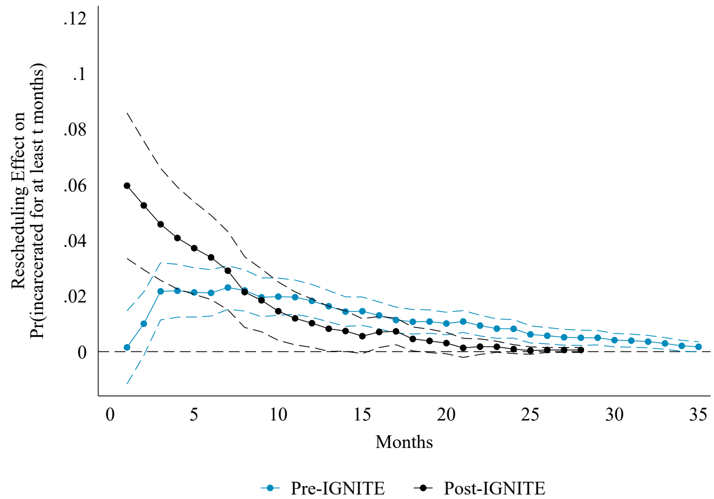
Appendix Figure A12: Chromebook Usage by Floor



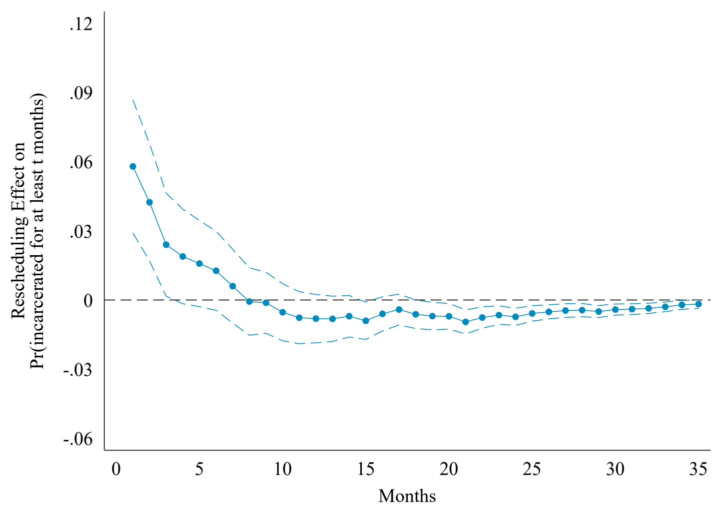
Notes: This figure plots the distribution of login activities on IGNITE Chromebooks by time of day and jail cell floor. Chromebook usage data come from Mt. Morris Consolidated Schools.

Appendix Figure A13: Average Causal Response Weights

(a) Estimated Weights

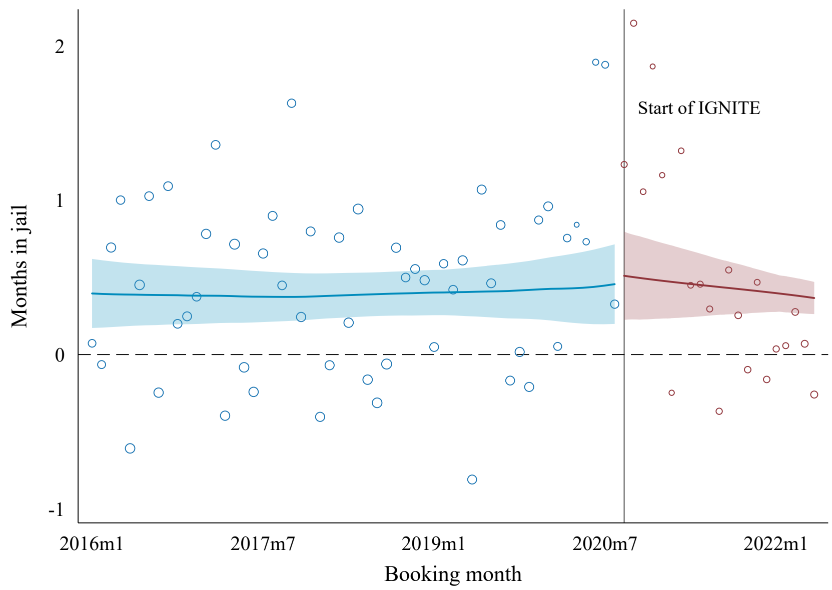


(b) Pre- vs. Post-IGNITE Difference



Notes: Panel A plots the estimated Average Causal Response (ACR) weights for our baseline IV specification, following Appendix E. Each point is an estimated coefficient from regressing indicators for whether time in jail exceeds t months on the court delay instrument, for $t = 1, \ldots, 35$. All regressions include the design and auxiliary controls discussed in the main text. Panel B plots the difference between the pre- and post-IGNITE estimates. Dashed lines show 95% confidence intervals from individual-clustered standard errors.

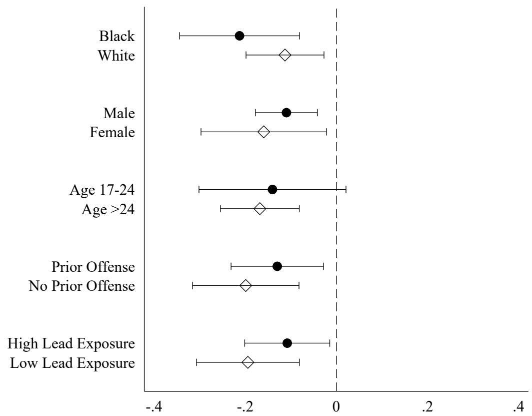
Appendix Figure A14: First-Stage Effects of Court Delays by Booking Month



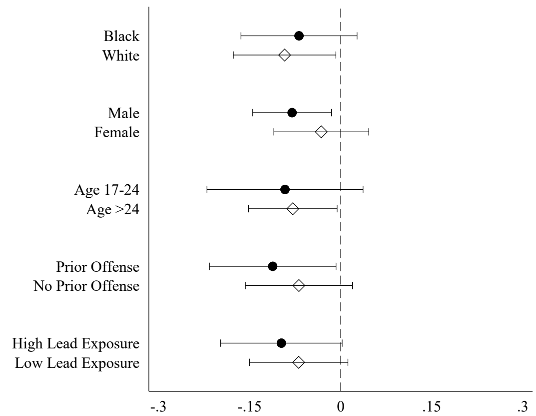
Notes: This figure plots covariate-adjusted monthly average differences in time in jail between individuals who do and do not experience a court delay. Each point indicates the coefficient from regressing time in jail on a court delay indicator and covariates among individuals booked in a given month. Covariates include the design and auxiliary controls discussed in the main text. The fitted line is obtained by a local linear regression with a rule-of-thumb bandwidth, weighting by the number of incarcerated individuals booked in a given month. Shading indicates 95% confidence intervals derived from individual-clustered standard errors. The vertical line indicates the beginning of the IGNITE program in September 2020.

Appendix Figure A15: Heterogeneity by Incarcerated Individual Characteristics

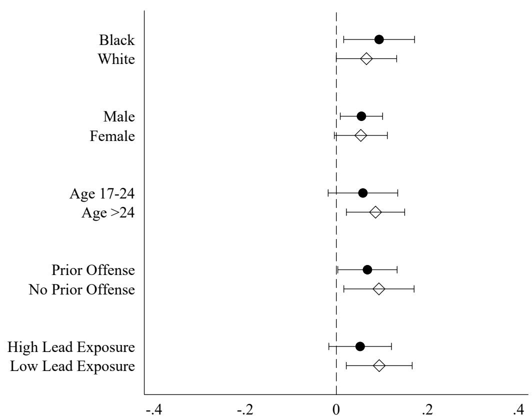
(a) Effect of Months in IGNITE on Misconduct



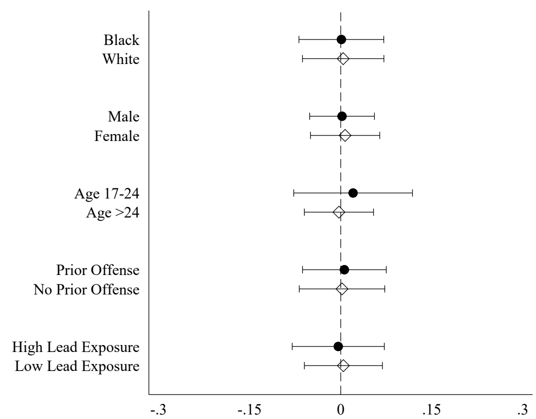
(b) Effect of Months in IGNITE on Recidivism



(c) Effect of Months in Jail on Misconduct



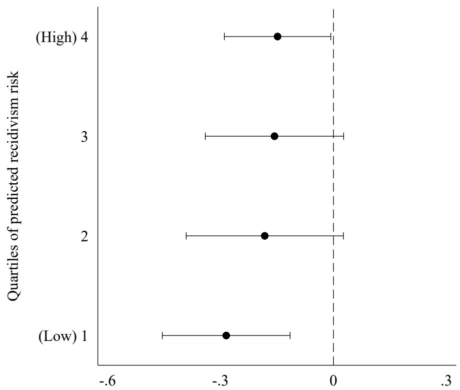
(d) Effect of Months in Jail on Recidivism



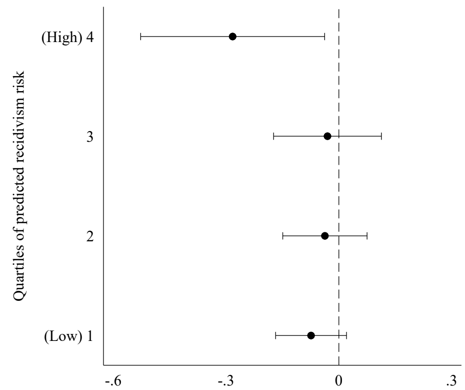
Notes: This figure plots IV estimates and associated 95% confidence intervals for the effects of months in IGNITE or jail on either weekly major misconduct or three-month recidivism by individual characteristics. Estimates are obtained by interacting either treatment with each observable characteristic as described in the main text. The characteristics are an indicators for race, sex, age, having a prior offense, and being in the fourth quartile of census tracts by elevated blood lead levels. All specifications include the design and auxiliary controls discussed in the main text. 95% confidence intervals are derived from individual-clustered standard errors.

Appendix Figure A16: Heterogeneity by Predicted Recidivism Risk

(a) Misconduct



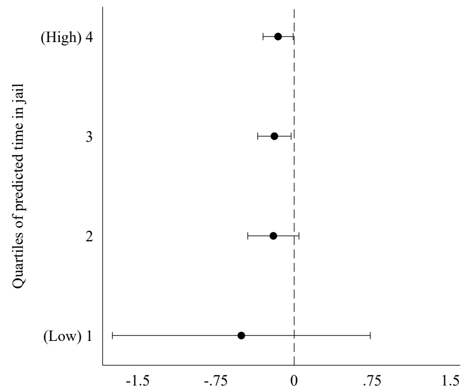
(b) Recidivism



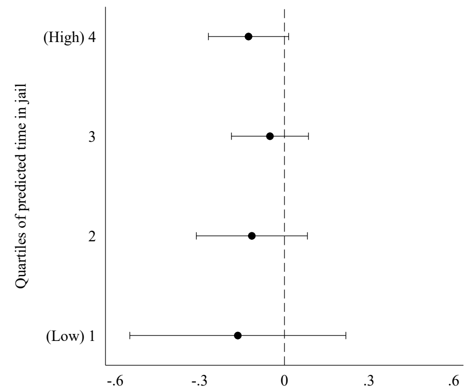
Notes: This figure plots IV estimates and associated 95% confidence intervals for the effects of months in IGNITE by quartiles of predicted three-month recidivism risk. Panel A reports results for weekly major misconduct while Panel B reports results for for three-month recidivism. Predicted recidivism risk is obtained by a logit regression on the 2015 holdout sample. All specifications include the design and auxiliary controls discussed in the main text. 95% confidence intervals are derived from individual-clustered standard errors.

Appendix Figure A17: Heterogeneity by Predicted Time-in-Jail Quartiles

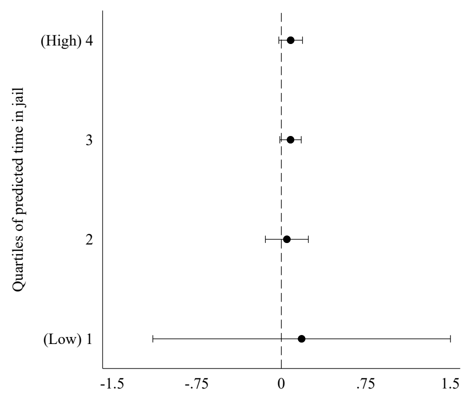
(a) Effect of Months in IGNITE on Misconduct



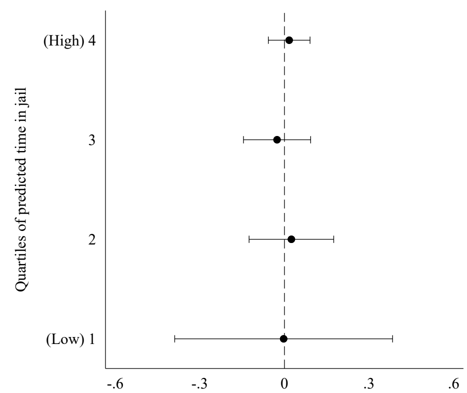
(b) Effect of Months in IGNITE on Recidivism



(c) Effect of Months in Jail on Misconduct



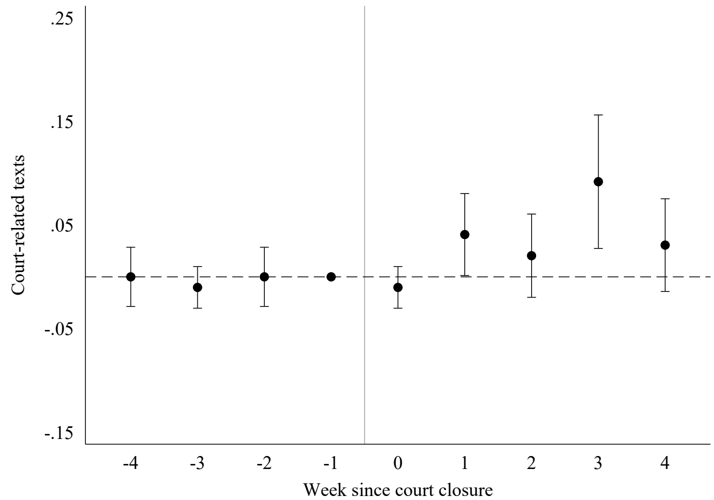
(d) Effect of Months in Jail on Recidivism



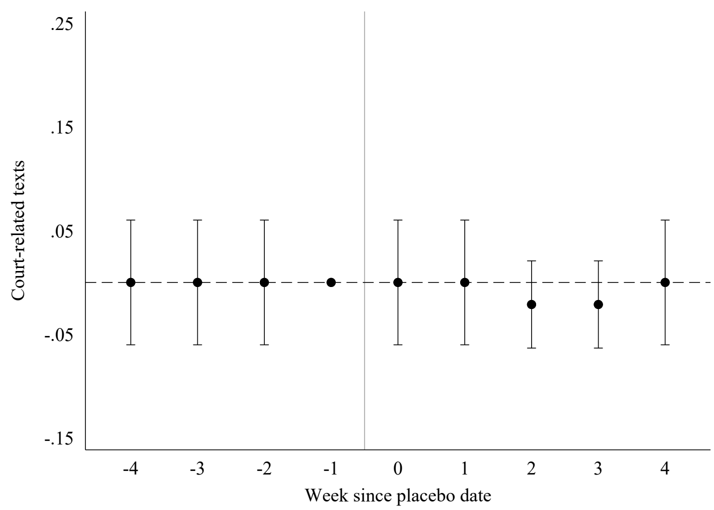
Notes: This figure plots IV estimates and associated 95% confidence intervals for the effects of months in IGNITE or jail by quartiles of predicted time in jail. Panel A reports results for weekly major misconduct outcome while Panel B reports results for for three-month recidivism. Predicted time in jail is produced by a regression on the 2015 holdout sample. All specifications include the design and auxiliary controls discussed in the main text. 95% confidence intervals are derived from individual-clustered standard errors.

Appendix Figure A18: Kites Court Closure Event Study

(a) Court Closure

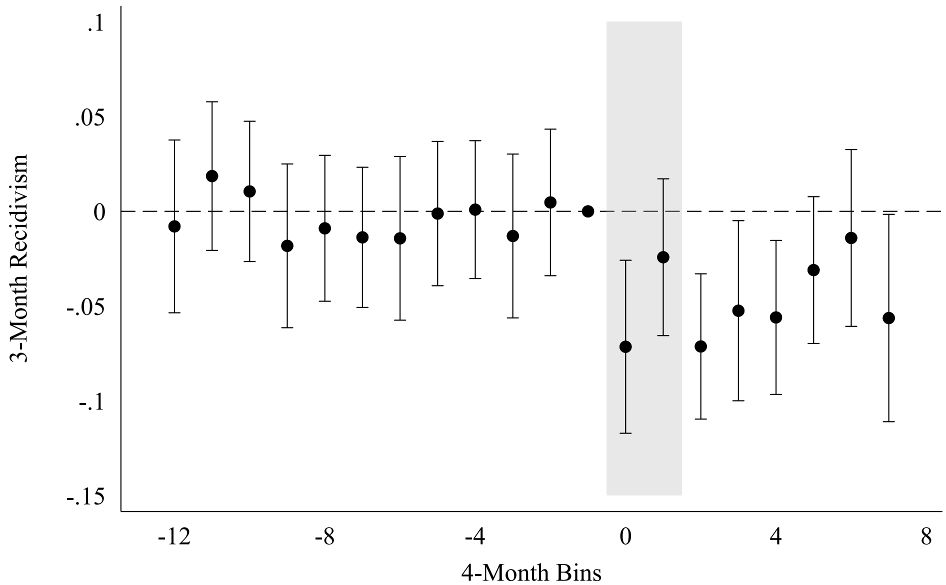


(b) Placebo



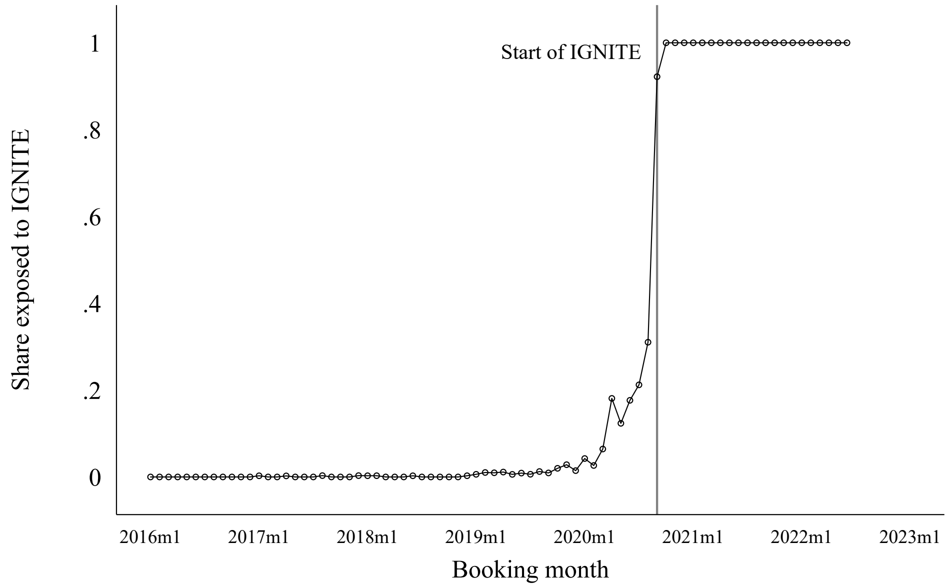
Notes: This figure plots coefficients and 95% confidence intervals from an event study regression of an indicator for a Kites message including the words "talk," "speak," "need," "can," "please," "court," or "judge." Panel A uses one week prior to the court closure due to COVID-19 (March 17, 2020) as the base period. Panel B uses a placebo date (May 17, 2019). 95% confidence intervals are derived from individual-clustered standard errors.

Appendix Figure A19: Effects of IGNITE on Recidivism: Event Study Estimates



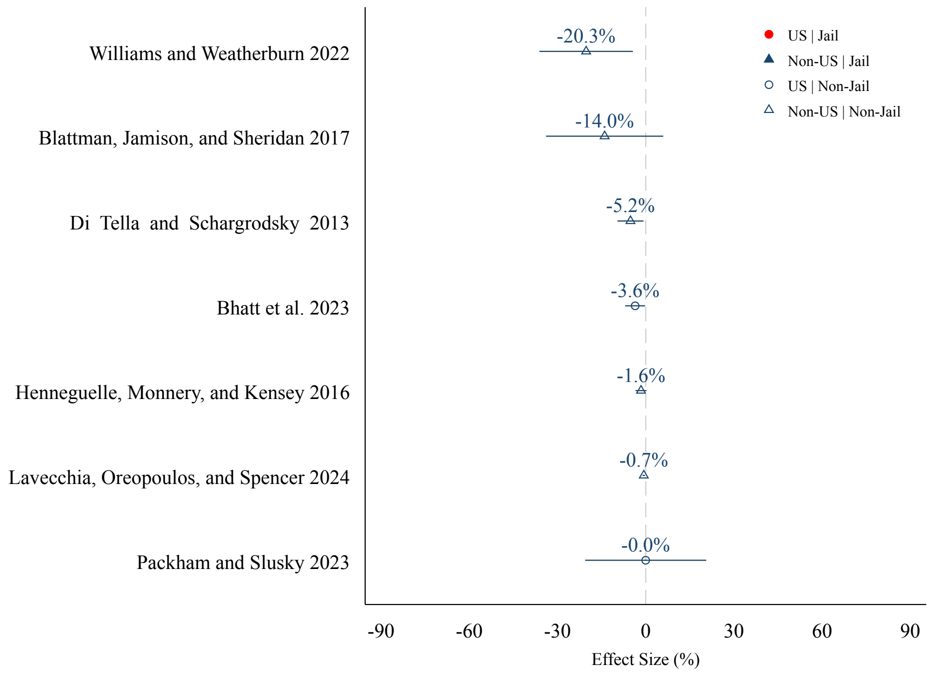
Notes: This figure plots coefficients and 95% confidence intervals from an event study regression of three-month recidivism, measured in either Genesee or Saginaw County. The treatment is an indicator for the individual being booked in Genesee County and the base period is December 2019. Outcomes are binned in four-month intervals. The shaded area denotes the period when individuals booked pre-IGNITE saw nontrivial exposure to IGNITE after its launch in September 2020.

Appendix Figure A20: Probability of Exposed to IGNITE



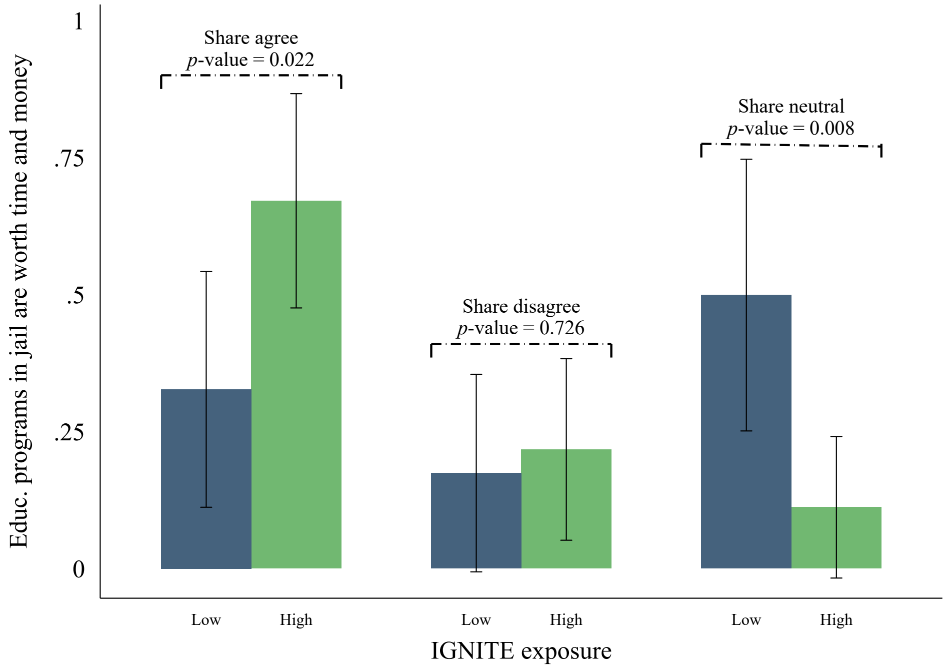
Notes: This figure plots the probability of being exposed to IGNITE (i.e., being in Genesee County Jail on or after September 2020) by booking month.

Appendix Figure A21: Literature Comparison: Alternatives to Incarceration



Notes: This figure summarizes estimated treatment effect sizes from the literature on alternatives to incarceration. See Appendix F for details on the papers and effect size construction. We distinguish between U.S. vs. non-U.S. studies and studies in jail vs. non-jail contexts. Each point indicates the estimated effect of treatment on recidivism as a percent of the control mean. When possible, we use one-year recidivism outcomes and scale effects by time in treatment. 95% confidence intervals are shown around each point.

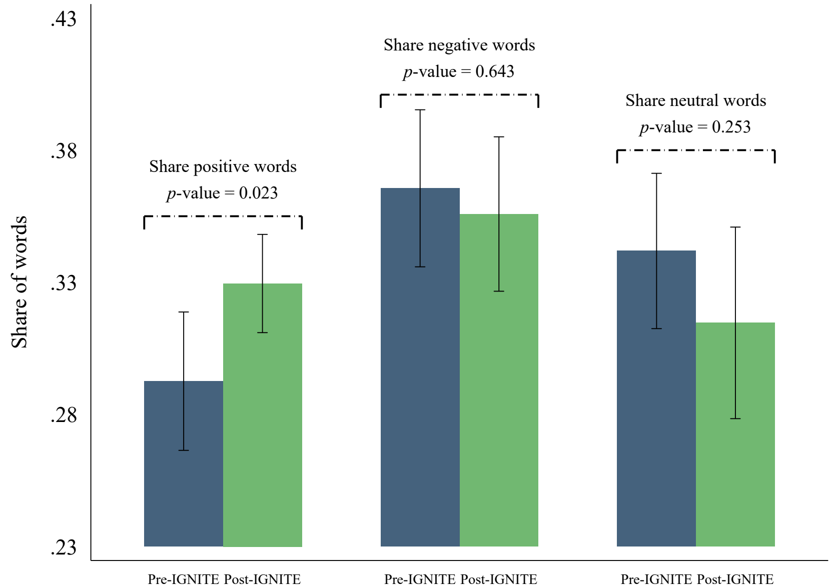
Appendix Figure A22: Custody Staff Views on Education Programs in Jail by IGNITE Exposure



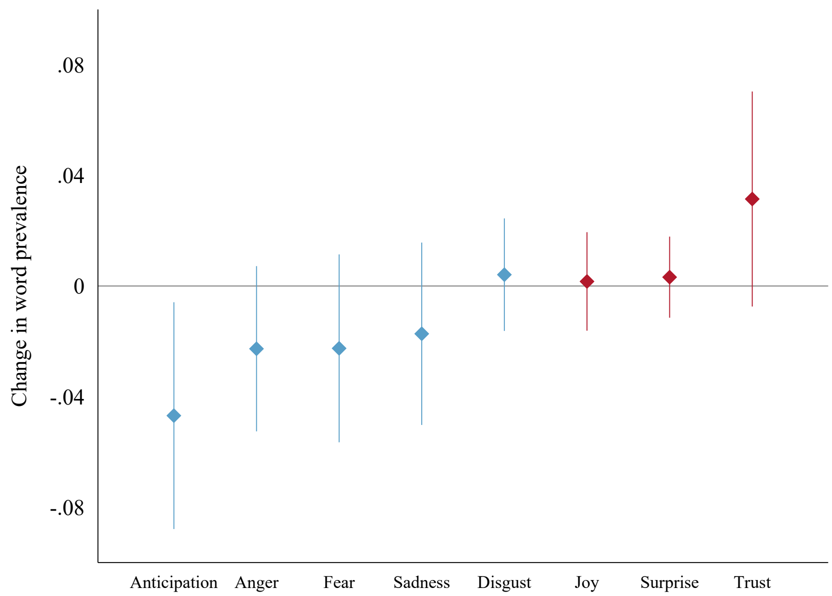
Notes: This figure plots means and 95% confidence intervals for responses in the Genesee County Jail Custody Staff Survey to the question of whether "Education programs in jail are worth the time and money." High IGNITE Exposure is an indicator for the respondent answering the question "How often do you interact with inmates in IGNITE?" with "Usually" or "Always."

Appendix Figure A23: Kites Sentiment Analysis

(a) Positive/Negative/Neutral Sentiment



(b) Emotion Prevalence, Post- vs. Pre-IGNITE



Notes: This figure summarizes the sentiment of Genesee County Jail Kites messages, pre- and post-IGNITE, according to the NRC Word-Emotion Association Lexicon from Mohammad and Turney (2010). Panel A plots the share of words in text messages categorized as positive, negative, and neutral for each time period. The p-values are obtained from a word-level regression on a post-IGNITE indicator weighted by word frequency with standard errors clustered at the individual level. Panel B plots the coefficient on post-IGNITE from analogous regressions of an indicator for the word being associated wih an emotion. Emotions are categorized as negative (blue) and positive (red) by ChatGPT.

Appendix Table A1: Effects on Secondary Outcomes

	Months in IGNITE (1)	Months in Jail (2)
Panel A: Alternative Recidivism/Misconduct Measures		
Recharged	-0.060***	0.013
($N = 22,191$)	(0.027)	(0.014)
Reconvicted	-0.051**	0.028
($N = 22,191$)	(0.021)	(0.018)
Minor Misconduct	-0.021**	0.011***
($N = 23,610$)	(0.010)	(0.004)
More Serious Misconduct	-0.029**	0.011***
($N = 23,610$)	(0.012)	(0.005)
Panel B: Other Outcomes		
Tether	0.002	-0.000
($N = 23,610$)	(0.009)	(0.007)
Bail Posted	-0.078	0.154***
($N = 23,610$)	(0.053)	(0.048)
Sentenced to Prison	0.005	-0.024
($N = 23,610$)	(0.020)	(0.017)
Convicted	-0.048	0.190***
($N = 23,610$)	(0.060)	(0.055)
Released to Rehab. Centers	-0.001	-0.003
($N = 23,610$)	(0.010)	(0.008)
Suicide	-0.008	-0.020
($N = 23,610$)	(0.030)	(0.022)
Other Medical	-0.041	-0.006
($N = 23,610$)	(0.040)	(0.027)

Notes: This table reports reduced form and IV estimates for alternative measures of recidivism and misconduct, as well as other outcomes. Panel A reports results for Recharged (charged within three months since release), Reconvicted (convicted within three months since release), Minor Misconduct, and More Serious Misconduct (as defined in the text). Panel B reports results for Tether (an indicator for released with electronic monitoring), Bail Posted (an indicator for released on bond), Sentenced to Prison (an indicator for sentenced to prison), Convicted (an indicator for either found or plead guilty), Released to Rehabilitation Centers (an indicator for released to rehabilitation centers), Number of Suicide Attempts per Week, and Number of Other Medical Incidents per Week. All rows include the design controls and auxiliary controls discussed in the main text. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A2: Representativeness of Test Takers

	Overall Mean (1)	Difference in Means (2)	Standard Error (3)
Panel A: Individual Characteristics			
Female	0.209	0.004	(0.019)
Age 25-34	0.388	-0.024	(0.023)
Age 35-44	0.247	0.004	(0.021)
Age 45-54	0.116	-0.030*	(0.016)
Age 55-64	0.058	-0.020*	(0.011)
Age 65+	0.012	-0.004	(0.005)
Black	0.532	-0.067	(0.053)
Has a Public Defender	0.299	0.026	(0.020)
Panel B: Census Tract Characteristics			
Share with Elevated Blood Lead Level	0.031	-0.020	(0.014)
Share Black	0.417	-0.034	(0.024)
Share High School Graduate or Higher	0.849	-0.039	(0.025)
Log Median Household Income	10.336	-0.231	(0.151)
F-Statistic for Joint Test [p-value]		1.099 [0.122]	
Observations		4451	

Notes: This table summarizes the sample of incarcerated individuals who can be linked to the Mt. Morris data and compares those who do and do not have CASAS test scores. See Table 1 for details on the variables. standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A3: IGNITE Exposure Balance Test: Community Survey

	Overall Mean	Difference in Means	Standard Error
	(1)	(2)	(3)
Panel A: Site			
Barbershop	0.092	-0.039	(0.063)
Church	0.230	0.158	(0.102)
General Store	0.253	-0.132	(0.091)
Other	0.414	0.031	(0.112)
Social Security Office	0.011	-0.017	(0.018)
Panel B: Demographics			
Male	0.287	0.121	(0.107)
Black	0.793	-0.041	(0.095)
White	0.207	0.041	(0.094)
Hispanic	0.023	-0.035	(0.025)
Completed college or more	0.391	-0.086	(0.109)
High school degree or GED	0.322	0.067	(0.106)
No high school degree	0.034	0.049	(0.049)
Some college	0.253	-0.030	(0.098)
Age 18-24	0.080	0.080	(0.070)
Age 25-34	0.299	0.052	(0.104)
Age 35-44	0.184	-0.026	(0.087)
Age 45-54	0.264	-0.047	(0.099)
Age 55-64	0.149	-0.075	(0.076)
Age 65+	0.023	0.016	(0.038)
F-Statistic for Joint Test [p-value]		0.851 [0.626]	
Observations		87	

Notes: This table summarizes the Flint Community Survey sample and reports balance tests for IGNITE exposure. Column 1 reports sample means and Columns 2 and 3 report regression coefficients and associated standard errors from regressing respondent characteristics on an indicator for whether the individual was personally released from Genesee County Jail after September 2020 or had a friend or family member who was released from Genesee County Jail after September 2020. Panel A summarizes the survey recruitment location. Panel B reports results for respondent-level demographics, including sex, race, educational attainment, and age. All estimations include a survey cohort wave control. Robust standard errors are in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A4: IGNITE Exposure Balance Test: Custody Staff Survey

	Overall Mean (1)	Difference in Means (2)	Standard Error (3)
Female	0.267	-0.006	(0.135)
Black	0.022	0.035	(0.035)
Age 18-24	0.089	-0.039	(0.090)
Age 25-34	0.289	-0.267*	(0.143)
Age 35-44	0.356	0.158	(0.141)
Age 45-54	0.200	0.128	(0.110)
Started Work after IGNITE	0.400	0.009	(0.153)
Started Work after COVID-19	0.422	0.044	(0.154)
F-Statistic for Joint Test [p-value]		1.067 [0.408]	
Observations		45	

Notes: This table summarizes the Genesee County Jail Custody Staff Survey and reports balance tests for high IGNITE exposure. Specifically, Column 1 reports sample means and Columns 2 and 3 report regression coefficients and associated standard errors from regressing respondent characteristics on an indicator for whether the individual answered the question "How often do you interact with inmates in IGNITE" with "Usually" or "Always." Robust standard errors are in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A5: Summary Statistics, Main Analysis Sample

	Mean (1)	SD (2)	N (3)
Panel A: Instrument and Outcomes			
Any District Court Delay	0.381	(0.486)	23,610
Months in IGNITE	0.434	(2.091)	23,610
Months in Jail	1.558	(4.212)	23,610
Ever Rebooked in 3 Months after Release	0.175	(0.380)	22,147
Convicted	0.495	(0.500)	23,610
Sentenced to Prison	0.060	(0.238)	23,573
Any Major Misconduct	0.092	(0.289)	23,610
Any Medical or Suicidal Incident	0.061	(0.240)	23,610
Any Incident	0.160	(0.366)	23,610
Panel B: Individual and Case Characteristics			
Female	0.240	(0.427)	23,610
Age 25-34	0.378	(0.485)	23,610
Age 35-44	0.225	(0.418)	23,610
Age 45-54	0.122	(0.327)	23,610
Age 55-64	0.058	(0.234)	23,610
Age 65+	0.009	(0.092)	23,610
Black	0.534	(0.499)	23,610
Booked in Past Year	0.433	(0.496)	23,610
Felony Charge	0.534	(0.499)	23,610
Number of Charges	1.385	(0.867)	23,610
Panel C: Census Tract Characteristics			
Share with Elevated Blood Lead Level	0.031	(0.028)	22,318
Share Black	0.429	(0.354)	22,320
Share High School Graduate or Higher	0.848	(0.066)	22,320
Log Median Household Income	10.322	(0.425)	22,318
Missing Census Tract Information	0.055	(0.228)	23,610

Notes: This table reports the sample mean, standard deviation, and number of non-missing observations of variables in the main analysis sample.

Appendix Table A6: Differential Attrition

	Observed for 3 Months after Release (1)	Observed for 6 Months after Release (2)	Observed for 9 Months after Release (3)	Observed for 12 Months after Release (4)
Court Delay	0.000	-0.001	-0.004***	-0.011***
	(0.000)	(0.001)	(0.001)	(0.002)
Control Mean	1.000	0.998	0.995	0.976
Observations	23,610	23,610	23,610	23,610

Notes: This table reports differential attrition by the court delay instrument over various time horizons. Each column reports a regression of an indicator that equals one if an individual is observed for t months after release on the Court Delay instrument for $t = 3, 6, 9, 12$. All regressions include the design and auxiliary controls discussed in the main text. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A7: First-Stage Effects of Court Delays

	Months in IGNITE		Months in Jail	
	(1)	(2)	(3)	(4)
Court Delay × Post-IGNITE	0.521***	0.519***	0.120	0.113
	(0.096)	(0.096)	(0.119)	(0.119)
Court Delay	0.128***	0.130***	0.393***	0.401***
	(0.029)	(0.029)	(0.073)	(0.073)
Control Mean	0.102	0.102	1.311	1.311
F-Stat.: Court Delay × Post-IGNITE	79.909	79.611	79.909	79.611
F-Stat.: Court Delay	55.159	56.549	55.159	56.549
Design Controls	Yes	Yes	Yes	Yes
Auxiliary Controls	No	Yes	No	Yes
Observations	23,610	23,610	23,610	23,610

Notes: This table reports first-stage estimates for months in IGNITE (Columns 1 and 2) and months in jail (Columns 3 and 4). All columns include the design controls discussed in the main text. Columns 2 and 4 also include the auxiliary controls. Individual-clustered standard errors are reported in parentheses. F-statistics are from Sanderson and Windmeijer (2016). *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A8: Judge IV Estimates

	Misconduct		Recidivism	
	Pre-IGNITE	Post-IGNITE	Pre-IGNITE	Post-IGNITE
	(1)	(2)	(3)	(4)
Months in Jail	0.010	-0.011	0.073	-0.054**
	(0.026)	(0.024)	(0.062)	(0.024)
Post-Pre		-0.021		-0.127*
		(0.035)		(0.067)
Design Controls	Yes	Yes	Yes	Yes
Auxiliary Controls	Yes	Yes	Yes	Yes
Observations	19093	4751	17887	4484

Notes: This table reports judge IV estimates for the effects of time in jail on weekly major misconduct and three-month recidivism in the pre- and post-IGNITE periods, estimated as in the main text. All columns include the design controls and auxiliary controls discussed in the main text. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A9: Judge IV Specification Tests

	Number of Spline Knots			
	One	Two	Three	Four
	(1)	(2)	(3)	(4)
Panel A: Pre-IGNITE				
Test Statistic	32.4	31.9	31.1	30.9
Deg. of Freedom	17	16	15	14
p-value	0.013	0.010	0.008	0.006
Panel B: Post-IGNITE				
Test Statistic	27.8	24.6	23.8	25.3
Deg. of Freedom	14	13	12	11
p-value	0.019	0.020	0.019	0.008
Panel C: Overall				
Test Statistic	50.2	48.7	50.6	50.5
Deg. of Freedom	17	16	15	14
p-value	$<$0.001	$<$0.001	$<$0.001	$<$0.001

Notes: This table reports the results of the tests of judge IV monotonicity and exclusion proposed by Frandsen, Lefgren and Leslie (2023), computed both for the overall sample and separately by pre- versus post-IGNITE. Test statistics are based on quadratic b-spline estimates of the relationship between recidivism outcomes and District Court judge leniency, with the number of knots specified in each column. All specifications include the design controls.

Appendix Table A10: County Difference-in-Differences Estimates

	Recidivism Measured in Each County		Recidivism Measured in Both Counties	
	(1)	(2)	(3)	(4)
Post × Genesee	-0.039***	-0.026***	-0.035***	-0.023***
	(0.007)	(0.007)	(0.007)	(0.007)
Genesee	0.069***	0.040***	0.072***	0.042***
	(0.004)	(0.003)	(0.004)	(0.004)
Post	0.006	-0.020	0.004	-0.023
	(0.010)	(0.016)	(0.010)	(0.016)
Control Mean	0.182	0.182	0.189	0.189
Ind. Chars. × Post	No	Yes	No	Yes
Observations	40,655	40,655	40,655	40,655

Notes: This table reports the results from regressing, in a sample of individuals booked in either Genesee County or Saginaw County, three-month recidivism on an indicator for being booked in Genesee County, an indicator for being booked after January 2020, and their interaction. Columns 1 and 2 measure recidivism separately for Genesee and Saginaw Counties while Columns 3 and 4 combine recidivism in both counties. Month and year fixed effects are included in all estimates. Columns 2 and 4 additionally control for individual characteristics (indicators for Black, female, age groups, whether an individual was booked in the past year) interacted with the post indicator. The control mean is for Genesee County in the pre-period. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A11: Social Cost of Crime Effects

	3 Months (1)	6 Months (2)	9 Months (3)	12 Months (4)
Months in IGNITE	-2957.46**	-3943.38**	-5293.63***	-5614.70**
	(1238.02)	(1653.90)	(1972.45)	(2197.60)
Control Complier Mean	12212.62	26148.83	39364.44	45535.49
Observations	22,191	21,525	21,139	20,766

Notes: This table reports IV estimates for the social costs induced by future crimes. Future crimes are obtained from ROAs associated with observed future bookings and divided into the following categories: DUIs, drug offenses, motor vehicle offenses, persons offenses, property offenses, public order offenses, weapons offenses, and other offenses. Within each of these crime types, we take the lowest social cost estimate from Miller et al. (2021). We then use the total social cost (sum of frequency of crime × cost crime) as an outcome in the main IV specification discussed in the text. Individuals sentenced to prison or not observed for enough time since their last release are excluded from the analysis. All columns include the design controls and the auxiliary controls discussed in the main text. Individual-clustered standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A12: Effects of High IGNITE Exposure: Views on Own Work Experience

Outcome Variables	High IGNITE Exposure (1)	(2)	Mean of Control Group (3)	Observations (4)
Satisified with Own Job	0.135	(0.151)	0.588	45
Benefit Package is Competitive	0.044	(0.155)	0.353	45
Pay is High Enough	-0.013	(0.133)	0.235	45
Joint F-Test [p-Value]	0.306	[0.821]		45

Notes: This table reports estimated effects of high vs. low IGNITE exposure on whether custodial staff in Genesee County Jail agree or strongly agree with the listed statement. The high IGNITE exposure treatment is an indicator for a respondent answering the question “How often do you interact with inmates in IGNITE?” with “Usually” or “Always.” Robust standard errors are reported in parentheses. *, **, and *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A13: Staff Quotes on IGNITE

	Selected Quotes
Positive	"Ignite [sic] has changed the culture at the Genesee County jail."
	"It has given inmates something to focus on and invest instead of worrying about drama and wrong doings. Their mind is being put to use and the rewards helps encourage them to do better."
	"It has created a safer work place."
	"I've seen inmates that are constant problems change their behavior dramatically once they have seen the benefits of Ignite. Ignite seems to provide self-worth to people that maybe never had any."
	"I believe it has helped to bring the unity and humanity aspect back to the jail."
Neutral	"The program is a great idea, however, we seem to be neglecting other areas of jail operations to cater to IGNITE."
	"It makes things a little more difficult for staff and the security of the facility. Helps some with the behavioral aspects."
	"Ignited [sic] sometimes made it harder for us to do our jobs and decrease the Deputy security and safety. But at the same time it has reduced some recidivism."
Negative	"I have experience added [sic] mandatory overtime, time away from my family. We have had security breaches putting the safety and security of the facility at risk."
	"There are additional duties placed on the housing unit deputies. Outside of that, IGNITE does not directly effect [sic] my day to day experience or operation."
	"Ignite has taken over most of the priorities at the jail. Ignite comes before anything else."

Notes: This table lists example custody staff quotes on their overall views on IGNITE, grouped by positive, neutral, or negative views, from the Genesee County Jail Custody Staff Survey. Quotes are selected from the answers to the question: "Can you describe any ways in which IGNITE has changed your experience working at the Genesee County Jail?"

B Institutional Setting Appendix

This appendix details the IGNITE program and its launch. Most information comes from original field interviews with Genesee County community members and custody staff.

B.1 Origins of IGNITE

Prior to IGNITE, there were minimal educational opportunities available to incarcerated individuals in Genesee County Jail. Mt. Morris Consolidated Schools, the school partner for Genesee County Jail since 2004, offered a small GED prep program that worked with 2 to 20 people at a time. Participation in the GED prep program was limited to individuals chosen by custody staff. There were no educational activities or training opportunities for those who needed more elementary educational training or who had already obtained their GED or high school diploma.

IGNITE was a concept originated by Genesee County Sheriff Chris Swanson. A long-time undersheriff for the county, Sheriff Swanson was elected in January 2020 into an interim-sheriff position to complete the 1-year remaining term of retiring Sheriff Robert Pickell. In July 2019, Sheriff Swanson planned a new educational program within the jail which was informed by his master's thesis at the University of Michigan. Having seen three generations of families go through the jail system over his career in corrections, Sheriff Swanson was determined to transform incarceration through education.

Sheriff Swanson had originally hoped to start an education program in the jail later in his tenure, but the pandemic and the murder of George Floyd in May 2020 accelerated these plans.[35] Escalating racial tensions, difficult community-police relations, and a "broken" jail system led Sheriff Swanson and jail administrators to launch IGNITE on September 8th, 2020. According to the National Policing Institute and National Sheriff's Association (NPI and NSA, 2023):

> "The goal of the program is to create a structured and safe environment that is conducive to education. Having this type of environment in place can facilitate the implementation of more diverse educational programming, which can help re-energize inmates, motivate them once again, and provide them with an education and new opportunities once they return to their communities."

B.2 IGNITE Coursework

IGNITE includes traditional educational coursework as well as skill-building and lifestyle promotion. The traditional educational coursework occurs in the jail's open activities space. Incarcerated individuals take courses offered through Odysseyware software on Chromebooks. These courses include enrichment classes, GED preparation, high school classes, as well as classes in basic reading, writing, and arithmetic. Prior to beginning coursework, incarcerated individuals take a placement test using the CASAS testing system. After individuals are placed into a track, Mt. Morris teachers

[35] Viral YouTube videos even showed Sheriff Swanson marching through the streets of Flint alongside protesters. See, e.g., https://www.youtube.com/watch?v=fBo9E9Be_kw (7NEWS Australia, 2020).

monitor their progress and provide support. Mt. Morris also conducts post-assessment testing with CASAS. Ideally, all incarcerated individuals would receive both pre and post-assessments. However, individuals are often released before post-assessment testing can be arranged.

In addition to the traditional curriculum, IGNITE offers enrichment classes including nutritional courses, financial literacy, and training for certain trades (e.g., welding) via virtual reality software. Aramark, a food service provider, also offers a ServSafe food certification course and there are opportunities to work on obtaining a commercial driver's license (CDL) and even to complete college level coursework.

B.3 IGNITE Schedule

IGNITE coursework runs five days a week in two hour-long program blocks. All other jail activities are paused during IGNITE study time. Tablet computers, provided by ViaPath, are distributed daily to IGNITE participants who completed their coursework. Tablets are typically available from 5PM to 9PM and used as rewards for IGNITE participation with approved content such as games and puzzles.

B.4 IGNITE Expenses

ViaPath provides internet and communications technology to Genesee County Jail, including the Chromebooks and tablets used for IGNITE. The additional costs of the program are partially offset by service fees paid from commissary funds. Philanthropy also plays a role (e.g., the virtual reality training system was donated). IGNITE also saves on administrative costs by relying on existing custody staff to implement the program. Custody staff provide security for teachers, supervise tablet time, and run the virtual reality system.

B.5 IGNITE Graduation

Genesee County Jail holds graduation ceremonies every 1-3 months to celebrate the achievements of individuals who participated in IGNITE. As part of the ceremony, the Sheriff hands out certificates to graduating individuals. Family and friends are invited to attend, with a Deputy reaching out to them as part of the event organization. Incarcerated individuals wear gowns over their jumpsuits and listen to a keynote speaker. The commencement ceremonies are frequently broadcast on social media and covered by the local press.[36].

B.6 IGNITE Culture

IGNITE is described by administrators as a law-enforcement-led program that changes attitudes and the culture of corrections both directly and indirectly through education and opportunities.

[36] An example of an IGNITE graduation ceremony can be viewed at: https://www.abc12.com/video/genesee-county-sheriffs-office-holds-ignite-graduation/video_7badfb62-21d7-59d5-8eea-a0f0b7faece0.html (ABC 12 News, 2024)

The jail views IGNITE as a vehicle to rebuild trust in a community that has been rocked by a public health crisis, fiscal crises, and government corruption. IGNITE purports to institute this cultural change by giving value and respect to incarcerated individuals and operating under a meritocratic system where those who work hard can succeed no matter what their background.

C Data Appendix

C.1 Construction of Sample and Key Variables

To construct our analysis sample, we start with the universe of individuals in the JMS who were booked and detained in Genesee County Jail from January 2016 to May 2022. The JMS data include case numbers which we match to ROAs from the online Michigan court records database (Michigan Judiciary, 2024). We scrape records for cases seen in the 67th Judicial District Courts, where both criminal misdemeanor and felony cases are seen in Genesee County along with traffic and civil infractions, and in the Seventh Judicial Circuit Court where felony cases are bound over and tried. These records create a timeline of court appointments for jailed individuals from the time their case was filed to when it closed, including all hearings, trials, and sentencing motions and proceedings.

Among the set of individuals booked and charged between January 2016 and May 2022, we exclude 4% of individuals not residing in Michigan, 1% of individuals with incomplete demographic information (specifically, race), and 1% of individuals who have not yet been sent to prison or released for at least three months. This leaves a sample of 23,610 incarceration spells involving 14,794 unique individuals.

Instrument: Court Delay. The ROAs are used to define court delay incidents. In Genesee County, when a court date is assigned, a scheduling notice appears in the Description section of an event entry in the ROA, along with the date and time of court appearance in the Comment section (see Appendix Figure A7). We define a specific court event, such as a pretrial arraignment as being delayed if, for the same date in the Comments section, a new event appears in the ROA with "Removed from Calendar" in the Description section.[37] We define our instrument by the presence of any such delays in an individual's District Court history. We also use the number of delays across an individual's court history, as well as the number of delays occurring across individuals on a given day, to construct controls for certain robustness checks. In another check, we construct an alternative instrument by the presence of delays in either District or Circuit Court ROAs.

Treatments: Months in Jail/IGNITE. The JMS dataset is used to determine an individual's time spent in jail, defined either as the difference between their release date and booking date or the

[37] This measure of court delay is not without limitations. Most importantly, we do not capture all forms of possible court delays which could include adjournments. One reason for excluding adjournments is that they are less likely to be as-good-as-randomly assigned, since they are more likely to be requested by the prosecutor or defense counsel than are removed events.

difference between their transfer date and booking date if the individual experienced an external transfer to another facility (such as prison or rehabilitation center). In some cases the JMS data records multiple release dates present for the same booking date; we use the most recent release date in these cases. For the very small minority of individuals still in jail in May 2023 (the last month in our sample), we define time spent in jail as the difference between May 2023 and their booking date. We determine the reason for release through the Release Checklist file in JMS.

To construct our measure of IGNITE exposure, we use the program's start date of September 8, 2020. We define an individual's months in IGNITE as the total amount of time in jail occurring since this date.

Outcomes: Misconduct and Recidivism. We obtain information on within-jail misconduct incidents from JMS Jail Incident Reports. These reports contains an incident log number, the date and time of the incident, the name of involved individual(s), the type of incident, the incident code associated with the action, and a description of the event. These reports also include the location of the incident, the status or resolution of the incident, and whether force was used.

We define an incident occurring for an individual in the jail if the associated incident log number is present and non-missing. Incidents are classified as major, minor, medical, or suicide attempt. We order all incidents by date. The number of incidents and the number of incidents of a single incident type is defined as the sum of the incidents an individual has during their jail stay. We divide this number by an individual's number of weeks in jail to obtain our measures of weekly misconduct. We further categorize incidents into disobedience and violent incidents, based on the incident classification numbers used in the incident description.

Our main three-month recidivism outcome is defined as an individual for whether an individual is rebooked in either Genesee County Jail or Saginaw County Jail within three months after release. Recidivism outcomes over longer horizons are constructed similarly. In most cases, individuals are brought to the jail for booking from local municipal arresting agencies throughout the county that operate independently of the county Sheriff's office.

C.2 Other Administrative Data

Kites Messaging. We merge in Kites Messaging data, which consists of all time-stamped messages sent between incarcerated individuals and the monitoring correctional staff, by individual identification number and jail stay.

Mt. Morris Consolidated Schools. The Mt. Morris Schools education data consists of two components. The first component consists of the amount of Chromebook usage, organized by individual-login time. Mt. Morris Schools use Chromebooks to administer classes in Genesee County Jail and each login reflects a student accessing their schooling program. We link these data at the individual-case level by first and last name and time of jail stay.

The second component consists of individual-level data on incarcerated individuals who participated in adult education programming in Genesee County Jail. Variables include High school and GED completion status, pre- and post-reading and math assessments, and GED subject test scores and dates. We convert the pre-and-post reading and math scores to their grade-level equivalence using the derived Grade Level Equivalencies for CASAS standardized exams. We link this dataset by first name, last name, and date of birth to the JMS data.

ViaPath Data. ViaPath, formally known as Global Tel Link (GTL), provides phone, tablets, and internet service to the jail. ViaPath data provides individual-level measures of total phone, app, and video visit usage. We observe the number of logins or calls an individual has ever made and the total number of logins or minutes used for each ViaPath service. Apps, which are accessible to incarcerated individuals via tablet computers, are further disaggregated into education and entertainment. We merge the ViaPath data by individual identifier to the JMS data.

Lead Exposure and Census Data. The residential address of incarcerated individuals recorded in JMS data are used to link census-tract variables, by zip code. Elevated Blood Lead Level data comes from the Michigan Department of Health and Human Services. Data are available at https://catalog.data.gov/no/dataset/leadbloodlevels-2017-bytract-20181129-dec8f (Data Driven Detroit, 2022). We define a missing indicator variable that equals one if an observation is not matched to a census tract. We obtain the population shares of Black, High School Graduate or Higher, and the Log Median Household Income from 2016 ACS 5-year Estimates (U.S. Census Bureau, 2016), linking individuals to the lead exposure data by zipcode of residence.

C.3 Saginaw County Data

For certain robustness checks we construct an analogous analysis sample using JMS data for Saginaw County Jail from 2016 to 2023. The booking records, organized at the individual-booking level include the defendant's full name, date of birth, race and sex identifiers, booking and release dates, arresting officer, jail classification, and case number.

We merge these data to an analogous court date delay measure for Saginaw County, using case numbers from the Saginaw County booking records to scrape ROAs from the 10th Judicial Circuit Court and 70th Judicial District Court. We parse the ROAs in a similar as was done for Genesee County. Since Genesee County and Saginaw County are served by different courts with their own practices for describing events, we alter our definition of a court date delay in Saginaw County to include the use of "Adjourn" or "Resched" in the event description.

C.4 Sentiment Analysis

Variable definition for Table 6 include the following:

- *Positive View on Law Enforcement* is an indicator for whether the respondent answered "Somewhat agree" or "Strongly agree" to the following question: "To what extent do you agree or disagree with the following statement: Law enforcement looks out for me and my community."

- *Engagement in Positive Activities* is an indicator for whether the respondent said they were "Looking for work," "Taking classes," "Working for pay that does not involve crime," or "Taking care of children or elderly family members," in response to the question: "Are you currently doing any of the following [Select all the apply]."
- *Hopeful about the Future* is an indicator for whether the respondent answered "More Hopeful" to the question, "Are you more or less hopeful about your future compared to before you were incarcerated?"

D Survey Appendix

Flint Community Survey. The Flint Community Survey was distributed in Flint, MI by four local community members between October 2023 and December 2023. Participants were recruited from locations throughout the the city, including grocery stores, libraries, barbershops, and churches. An example of the recruitment flyer is shown in Panel A of Appendix Figure A9. Participants scanned the QR code with their phone to access the survey. After providing informed consent, the participants were asked about their personal incarceration experience, and if not applicable, the incarceration experience of close friends and family members. Our primary outcome of interest was views on local law enforcement. Community members who had personally spent time in jail were also asked about their current activities. The survey was anonymous. A copy of the survey can be found at:

https://harvard.az1.qualtrics.com/jfe/form/SV_2fOdxQEsvmAO5U2

Custody Staff Survey. The Custody Staff Survey was distributed as a QR code on a flyer that was circulated in Genesee County Jail in January 2024 (see Panel B of Appendix Figure A9). Custody staff were asked about their tenure at the facility, their exposure to IGNITE participants, and their views on rehabilitation and reform in jails. Given ongoing negotiations with the jail administration, we included questions on job satisfaction—including whether staff thought they received a competitive benefits package and whether they thought pay was high enough. The survey was anonymous. A copy of the survey can be found at:

https://harvard.az1.qualtrics.com/jfe/form/SV_3xQqkubYEsu6zeS

E Econometric Appendix

E.1 Derivation of Equations (2) and (3)

Equations (2) and (3) follow by substituting the causal model (1) into the two IV estimands:

$$\beta^{Pre} = \frac{Cov(Z_i, Y_i(0) + \gamma_i M_i^J + \beta_i M_i^I \mid P_i = 0)}{Cov(Z_i, M_i^J \mid P_i = 0)} = \frac{Cov(Z_i, \gamma_i M_i^J \mid P_i = 0)}{Cov(Z_i, M_i^J \mid P_i = 0)},$$

and

$$\beta^{Post} = \frac{Cov(Z_i, Y_i(0) + \gamma_i M_i^J + \beta_i M_i^I \mid P_i = 1)}{Cov(Z_i, M_i^I \mid P_i = 1)} = \frac{Cov(Z_i, (\gamma_i + \beta_i) M_i^I \mid P_i = 1)}{Cov(Z_i, M_i^I \mid P_i = 1)}.$$

The second equalities follow from the facts that $Z_i \perp\!\!\!\perp (P_i, Y_i(0), \gamma_i, \beta_i)$ and $M_i^I = M_i^J \times P_i$. Let $M_i^J(z)$ be individual i's potential time in jail when $Z_i = z$. Then, by independence of Z_i:

$$\begin{aligned} Cov(Z_i, \gamma_i M_i^J \mid P_i = 0) &= E[\gamma_i M_i^J(1) \mid Z_i = 1, P_i = 0] - E[\gamma_i M_i^J(0) \mid Z_i = 1, P_i = 0] \\ &= E[(M_i^J(1) - M_i^J(0))\gamma_i \mid P_i = 0]. \end{aligned}$$

Similarly, with $M_i^I(z)$ denoting individual i's potential time in IGNITE when $Z_i = z$:

$$\begin{aligned} Cov(Z_i, (\gamma_i + \beta_i) M_i^I \mid P_i = 1) &= E[(\gamma_i + \beta_i) M_i^I \mid Z_i = 1, P_i = 1] - E[(\gamma_i + \beta_i) M_i^I \mid Z_i = 1, P_i = 1] \\ &= E[(M_i^I(1) - M_i^I(0))(\gamma_i + \beta_i) \mid P_i = 1]. \end{aligned}$$

Moreover:

$$\begin{aligned} Cov(Z_i, M_i^J \mid P_i = 0) &= E[M_i^J(1) \mid Z_i = 1, P_i = 0] - E[M_i^J(0) \mid Z_i = 1, P_i = 0] \\ &= E[M_i^J(1) - M_i^J(0) \mid P_i = 0] \end{aligned}$$

and

$$\begin{aligned} Cov(Z_i, M_i^I \mid P_i = 1) &= E[M_i^I(1) \mid Z_i = 1, P_i = 1] - E[M_i^I(0) \mid Z_i = 1, P_i = 1] \\ &= E[M_i^I(1) - M_i^I(0) \mid P_i = 1]. \end{aligned}$$

Hence $\beta^{Pre} = E[\omega_i^{Pre}\gamma_i \mid P_i = 0]$ and $\beta^{Post} = E[\omega_i^{Post}(\gamma_i + \beta_i) \mid P_i = 1]$ for $\omega_i^{Pre} = \frac{M_i^J(1) - M_i^J(0)}{E[M_i^J(1) - M_i^J(0) | P_i = 0]}$ and $\omega_i^{Post} = \frac{M_i^I(1) - M_i^I(0)}{E[M_i^I(1) - M_i^I(0) | P_i = 1]}$.

E.2 Difference-in-IVs With Nonlinear Causal Effects

Consider a general (partially linear) causal model, in place of Equation (1):

$$Y_i = G_i(M_i^J) + B_i(M_i^I)$$

where $G_i(\cdot)$ and $B_i(\cdot)$ are unconstrained potential outcome functions. Let $\gamma_i(m) = \frac{\partial}{\partial m} G_i(m)$ and $\beta_i(m) = \frac{\partial}{\partial m} B_i(m)$ denote marginal effects of $M_i^J \geq 0$ and $M_i^I \geq 0$, respectively, for individual i at margin m. Assume Z_i is independent of $(P_i, G_i(\cdot), M_i(\cdot))$ and that $M_i^I = M_i^J \times P_i$. Consider:

$$\begin{aligned}
Cov(Z_i, Y_i \mid P_i = 0) &= Cov(Z_i, G_i(M_i^J) \mid P_i = 0) \\
&= Cov(Z_i, G_i(0) \mid P_i = 0) + Cov\left(Z_i, \int_0^{M_i^J} \frac{\partial}{\partial m} G_i(m) dm \mid P_i = 0\right) \\
&= E\left[\int_0^\infty \left(\mathbf{1}[M_i^J(1) \geq m] - \mathbf{1}[M_i^J(0) \geq m]\right) \gamma_i(m) dm \mid P_i = 0\right],
\end{aligned}$$

where again $M_i^J(z)$ denotes individual i's potential time in jail when $Z_i = z$. The first equality uses $M_i^I = M_i^J \times P_i$, the second equality applies the causal model, and the third equality uses independence of Z_i. By the same steps:

$$\begin{aligned}
Cov(Z_i, Y_i \mid P_i = 1) &= Cov(Z_i, G_i(M_i^I) + B_i(M_i^I) \mid P_i = 1) \\
&= E\left[\int_0^\infty \left(\mathbf{1}[M_i^I(1) \geq m] - \mathbf{1}[M_i^I(0) \geq m]\right) (\gamma_i(m) + \beta_i(m)) dm \mid P_i = 1\right],
\end{aligned}$$

where again $M_i^I(z)$ denotes individual i's potential time in IGNITE when $Z_i = z$. Moreover:

$$Cov(Z_i, M_i^J \mid P_i = 0) = E\left[\int_0^\infty \left(\mathbf{1}[M_i^J(1) \geq m] - \mathbf{1}[M_i^J(0) \geq m]\right) dm \mid P_i = 0\right]$$

and

$$Cov(Z_i, M_i^I \mid P_i = 1) = E\left[\int_0^\infty \left(\mathbf{1}[M_i^I(1) \geq m] - \mathbf{1}[M_i^I(0) \geq m]\right) dm \mid P_i = 1\right].$$

Now consider the following generalization of Equation (4):

$$E\left[\int_0^\infty \omega_i^{Pre}(m) \gamma_i(m) \mid P_i = 0\right] = E\left[\int_0^\infty \omega_i^{Post}(m) \gamma_i(m) \mid P_i = 1\right]$$

for

$$\omega_i^{Pre}(m) = \frac{\left(\mathbf{1}[M_i^J(1) \geq m] - \mathbf{1}[M_i^J(0) \geq m]\right)}{E[\int_0^\infty \left(\mathbf{1}[M_i^J(1) \geq m] - \mathbf{1}[M_i^J(0) \geq m]\right) dm \mid P_i = 0]}$$

and

$$\omega_i^{Post}(m) = \frac{\left(\mathbf{1}[M_i^I(1) \geq m] - \mathbf{1}[M_i^I(0) \geq m]\right)}{E[\int_0^\infty \left(\mathbf{1}[M_i^I(1) \geq m] - \mathbf{1}[M_i^I(0) \geq m]\right) dm \mid P_i = 1]}.$$

Under this condition, the difference-in-IVs identifies:

$$\begin{aligned}
\beta^{\Delta} &= \frac{Cov(Z_i, Y_i \mid P_i = 1)}{Cov(Z_i, M_i^I \mid P_i = 1)} - \frac{Cov(Z_i, Y_i \mid P_i = 0)}{Cov(Z_i, M_i^J \mid P_i = 0)} \\
&= E\left[\int_0^\infty \omega_i^{Post}(m)(\gamma_i(m) + \beta_i(m)) \mid P_i = 1\right] - E\left[\int_0^\infty \omega_i^{Pre}(m)\gamma_i(m) \mid P_i = 0\right] \\
&= E\left[\int_0^\infty \omega_i^{Post}(m)\beta_i(m) \mid P_i = 1\right],
\end{aligned}$$

generalizing Equation (5). Here β^{Δ} captures a weighted average of incremental IGNITE effects $\beta_i(m)$ at different margins of exposure time m. The $\omega_i^{Post}(m)$ weights are convex when court delays weakly increase time in IGNITE, i.e., $\mathbf{1}[M_i^I(1) \geq m] - \mathbf{1}[M_i^I(0) \geq m] \geq 0$.

E.3 Identification of ACR Weights and Complier Characteristics

As in Angrist and Imbens (1995), the difference-in-IVs estimand in Appendix E.2 can be written:

$$\beta^{\Delta} = \int_0^\infty \phi(m) E[\beta_i(m) \mid M_i^I(1) \geq m > M_i^I(0), P_i = 1] dm,$$

where

$$\phi(m) = \frac{Pr(M_i^I(1) \geq m > M_i^I(0) \mid P_i = 1)}{\int_0^\infty Pr(M_i^I(1) \geq m' > M_i^I(0) \mid P_i = 1) dm'}.$$

The weight that β^{Δ} puts on $E[\beta_i(m) \mid M_i^I(1) \geq m > M_i^I(0), P_i = 1]$, i.e. the complier-average effect for margin m, is identified by a conditional IV regression of $\mathbf{1}[M_i^I \geq m]$ on M_i^I:

$$\frac{Cov(Z_i, \mathbf{1}[M_i^I \geq m] \mid P_i = 1)}{Cov(Z_i, M_i^I \mid P_i = 1)} = \frac{Pr(M_i^I(1) \geq m > M_i^I(0) \mid P_i = 1)}{\int_0^\infty Pr(M_i^I(1) \geq m' > M_i^I(0) \mid P_i = 1) dm'} = \phi(m).$$

Moreover, the average characteristics of compliers with the same weighting scheme are identified. Letting X_i be an observed characteristic, with $X_i \perp\!\!\!\perp Z_i$:

$$\frac{Cov(Z_i, X_i M_i^I \mid P_i = 1)}{Cov(Z_i, M_i^I \mid P_i = 1)} = \int_0^\infty \phi(m) E[X_i \mid M_i^I(1) \geq m > M_i^I(0), P_i = 1] dm,$$

following the same steps as above. The left-hand side of this expression comes from a conditional IV regression of $X_i M_i^I$ on M_i^I that instruments with Z_i. Analogous versions of these two IV regressions identify pre-IGNITE ACR weights and complier characteristics. In practice, we estimate ACR weights and complier characteristics by versions of these IV regressions that include the design and auxiliary controls from our baseline estimation procedure.

F Literature Comparison

We compare our baseline estimated effect of IGNITE on recidivism with the effects of other programs evaluated in the literature. To benchmark our findings, we calculate the effect sizes for related work against the control complier mean, the complier mean, the control mean, or the mean value of the recidivism measure, in that order of priority based on availability. When possible, we also compute one-month treatment effects of these programs by assuming linear effects. We apply the same transformations to the confidence intervals. Below we detail this calculation for each paper included in the literature comparison plots in Figure 4.

F.1 Time in Programming

1. Mueller-Smith and Schnepel (2021) use a fuzzy regression discontinuity (RD) design to study two two natural experiments in Harris County, TX, that shifted the probability of diversion referral for first-time felony defendants. They find that diversion reduced having any convictions 10 years post-disposition by 46.4% ([Table 3, Column 2] -0.26/0.56). The treatment is not comparable so we use this effect size.

2. Heller et al. (2017) use an experiment to study the effects of a cognitive behavioral therapy (CBT) Program for youth in a juvenile detention center in Cook County, Illinois. They find that CBT participation reduced the probability of readmission in the 12 months after release by 22.0% ([T8C6] -0.1689/0.768). Given the program lasted for approximately one month, we use this effect size.

3. Arbour, Marchand and Lacroix (2023) study prison rehabilitation programs in Canada using variation in program availability as an instrument for participation. They find that participation in one additional program reduces the probability of recidivism within one year after release by 17.5% ([T5C1] -0.038/0.217). The treatment is not comparable so we use this effect size.

4. Bhuller et al. (2020) study prisons in Norway. Leveraging random assignment of cases to judges and the variation in judge stringency in an IV design, they find that incarceration reduced the probability of being charged with at least one crime within two years of the case decision by 42.7% ([T4C1] -0.239/0.56). Being incarcerated increases the number of prison days served to 183.83 days or 6.1 months. We convert the estimate to a one month treatment effect of -7.0%.

5. Shem-Tov, Raphael and Skog (2022) study randomized assignment to a restorative justice intervention for youth facing felony charges in San Francisco, CA. The program replaced traditional felony prosecution. They find that program participation reduces the likelihood of being arrested in the 12 months after randomization by 40.3% ([T3C2] -0.228/0.566). The intervention lasts approximately 6 months from enrollment to completion. We convert the estimate to a one month treatment effect of -6.7%.

6. Augustine et al. (2022) leverage the random assignment of felony cases to arraignment judges and the variation in diversion referral rates as an instrument for diversion referral in San Francisco County, CA. They find that being referred to a diversion program reduced new arrests within one year post-arraignment by 29.8% ([T7C4 and T7C1] -0.150/0.503). Diversion programming increases time to disposition by 288.5 days or 9.6 months [T6C4]. We convert the estimate to a one month treatment effect of -3.1%.

7. Mastrobuoni and Terlizzese (2022) leverage prison overcrowding in an IV design to study the use of open prisons in Italy. They find that one extra year in open prisons reduced reincarceration within 3 years from the end of an individual's custodial term by 26.6% ([T4C4 and T4-Notes] -0.105/0.395). Considering open prisons as a program, we convert the estimate to a one month treatment effect of -2.2%.

8. Golestani, Owens and Raissian (2024) exploit random court room assignment of low-income defendants in Nashville and Davidson County, TN in an IV design. They find that having a domestic violence case heard in a specialized domestic violence division court increases the probability of subsequently appearing in court for a new crime within three years of initial case disposition by 1.7% ([T8C3] 0.015/0.865). The treatment is not comparable so we use this effect size.

9. Lee (2023) estimates the effect of residential housing on reincarceration in Iowa using the housing recommendation rate of randomly assigned case managers as an IV. He finds that former prisoners assigned to housing after release had a 18.4% ([T4C6] 0.082/0.446) higher probability of returning to prison within 3 years of release. Individuals typically stay in their assigned housing facility for 4 months. We convert the estimate to a one month treatment effect of 4.6%.

F.2 Time Incarcerated

1. Bushway and Owens (2013) use a law change in Maryland that altered recommended (but not actual) sentences for a subset of offenders in jail or prison to study the impact of expectations on future criminal behavior in an IV design. They find that holding actual punishment constant, a 100 percent reduction in recommended sentences reduced the probability of rearrest for men within 3 years after release by 14.0% ([T4C3] -0.090/0.645). The treatment is not comparable so we use this effect size.

2. Roach and Schanzenbach (2015) use random judge assignment for defendants in Seattle, WA, who plead guilty as an IV for time in prison. They find that extending the prison sentence by an additional month reduces another sentencing for one year post-release by 13.9% ([T5C1 and T1C1] -0.0167/0.12).

3. Estelle and Phillips (2018) study the effect of additional time in a Michigan jail or prison using both a judge IV and sentence guideline discontinuities. They find that an additional

day spent incarcerated reduces the number of future felony convictions 5 years after the start of initial sentence by 0.4% ([T10C1] -0.0055/1.43). We convert this estimate to a one month treatment effect of -11.5%.

4. Humphries et al. (2023) study the effects of Virginia felony conviction and incarceration using both a judge IV and sentencing guideline discontinuities. They find that one year after sentencing, those right above the sentence guideline cutoff spend 8 more months incarcerated and experience a reduction in recidivism by 40.8% ([T7C3] -0.049/0.12). We convert the estimate to a one month treatment effect of -5.1%.

5. Zapryanova (2020) uses both a judge IV and a fuzzy RD design to study the effects of time in prison and time on parole in Georgia. She finds that an additional month in prison decreases the probability of returning to prison within three years of release while on parole by 4.5% ([T5C4 and T1C1] -0.0104/0.23). We use this effect size.

6. Lotti (2022) uses a fuzzy RD design around an offender's 21st birthday to compare harsh and rehabilitative criminal incarceration practices among youthful offenders in England and Wales. She finds that young offenders at the margin of the age cutoff and who experience custody in prison were 36.9% less likely to reoffend than those exposed to youth custody centers over an 8-year time span ([T3C1 and T1C1] -0.265/0.719). The mean sentence length for the 1963 cohort was 9.5 months [T1C1]. We convert the estimate to a one month treatment effect of -3.9%.

7. Kuziemko (2013) studies discontinuities in Georgia's parole guidelines using an RD design. She finds that each month in prison reduces recidivism risk within 3 years after release by 3.8% ([T2C3] -0.0130/0.344).

8. Hjalmarsson and Lindquist (2022) study Swedish prison reforms which changed the share of time spent in prison without shifting sentence length. They find that an increase in the share of time individuals were required to serve reduced the probability of having any convictions in the one year post-release by 2.7% ([T6C1] -0.015/0.563). The average prison sentence was 11.7 months [T1C1], and since the reform would hypothetically increase the share of time served from 52% to 62%, we interpret the results as a one-month treatment effect.

9. Tobón (2022) studies the quasi-random assignment of individuals in the same judicial district to newer and higher quality prisons in Columbia. He finds that being released from a new prison reduced prison reentry in the twelve months after release by 35.7% ([T4C1-2] -0.035/0.098). Given an average prison length of 15.11 months [T2C1, 453.32 days], we convert the estimate to a one month treatment effect of -2.4%.

10. Rose and Shem-Tov (2021) use discontinuities in North Carolina's sentencing guidelines to study the effect of increased time in prison. They find that one month of prison exposure

reduces the likelihood of reincarceration within 5 years by 2.3% ([T2C3] -0.0115/0.5). We use this effect size.

11. Drago, Galbiati and Vertova (2009) study a collective clemency bill in Italy that exogenously shifts remaining sentences at the time of pardon using a regression analysis conditioning on initial sentence length. They find that an additional month in the residual sentence decreases the probability of returning to prison 7 months after release by 1.4% ([T2C1 and T1C1] -0.0016/0.115). We use this effect size.

12. Mueller-Smith (2015) uses random assignment of defendants to courtrooms in Harris County, Texas in an IV design. He finds that for an additional year a felony defendant is incarcerated, the probability of being rebooked in county jail for a new arrest increases by 12.2% ([T4C2 and T1C2] 0.067/(1 - 0.45)). The outcome measures recidivism per quarter and the treatment margin is an additional year incarcerated. We convert the estimate to a one month treatment effect of 1.0%.

13. Leslie and Pope (2017) instrument for pretrial detention status using variation in judge detention rates across judges in New York City criminal courts. They find that being detained leads to a 30.1% increase in being rearrested within two years for misdemeanor defendants ([T5C6 and T1C3-4] 0.118/[1-(0.66×639,141 + 0.24×89,614)/(639,141+89,614)]). The median length of detention was approximately 1 month for misdemeanor defendants, so we use this effect size.

14. Dobbie, Goldin and Yang (2018) leverage the detention tendencies of quasi-randomly assigned bail judges in Philadelphia County, PA and Miami-Dade County, FL in an IV design. They find that pretrial release decreases the probability of rearrest two years following case disposition by 35.3% ([T4C6 and T4C1] -0.121/0.343). The treatment is not comparable so we use this effect size.

15. Aizer and Doyle Jr (2015) study the effect of juvenile detention on adult incarceration, using random assignment of judges in Chicago, IL. Using an IV design, they find that juvenile incarceration increases the probability of adult imprisonment by age 25 by 71.6% ([T5C7 and T5C4] 0.234/0.327). The average incarceration length was 42 days, so we convert the estimate to a one-month treatment effect of 51.1%.

F.3 Alternatives to Incarceration

1. Williams and Weatherburn (2022) study electronic monitoring as an alternative to prison for nonviolent offenses in Sydney, Australia. Leveraging quasi-random assignment of cases to judges in an IV design, they find that being sentenced to electronic monitoring reduces the probability of committing any reoffense within 84 months of their case being finalized by 20.2 percent ([T5C3] -0.15/0.74). The treatment is not comparable so we use this effect size.

2. Blattman, Jamison and Sheridan (2017) explore the use of two randomly assigned interventions in Monrovia, Liberia, which included an eight-week program of group CBT and a $200 grant. They find that participants who received the treatment saw a 28.0% reduction in participants saying they have been arrested within the past two weeks in a survey conducted 12-13 months after receipt of grants ([T2C10 and T2C1] -0.033/0.118). The treatment period was two months long. We convert the estimate to a one month treatment effect of -14.0%.

3. Di Tella and Schargrodsky (2013) use randomly assigned judges in an IV design to study the effects of electronic monitoring in Buenos Aires, Argentina. They find that assignment to electronic monitoring decreases the probability of returning to prison for a new crime after supervision by 71.5% ([T5C3 and Section IV Paragraph 1] -0.16/0.2237). The mean post-release period was 2.85 years and individuals spent on average 420 days or 14.0 months on electronic monitoring. We convert the estimate to a one month treatment effect of -5.2%.

4. Bhatt et al. (2024) use an experiment of an 18-month long program in Chicago, IL that couples short-term employment with cognitive behavioral therapy and other social support. They find that participation in the program leads to a 64.7% reduction in shooting and homicide arrests 20 months post-randomization ([T4C4 and T4C3] -0.0220/0.0340). We convert the estimate to a one month treatment effect of -3.6%.

5. Henneguelle, Monnery and Kensey (2016) use two IVs that exploit the staggered rollout of electronic monitoring in French courts and the tendency of courts to utilize electronic monitoring. They study the effects of electronic monitoring in place of serving prison time and find that electronic monitoring decreases any re-conviction within 5 years of release for prisoners by 8.6% ([T2C6 and TA1C3] -0.0571/0.662). The average initial sentence for electronic monitoring is 5.4 months, so we convert the estimate to a one month treatment effect of -1.6%.

6. Lavecchia, Oreopoulos and Spencer (2024) use a difference-in-differences framework to study a high school support program for disadvantaged youth living in public housing projects in Toronto, Canada. They find that being eligible for the program in one's neighborhood reduces ever being charged with a crime by 31.6% ([T1C1-2] -0.06/0.19). The program lasted for the entirety of a child's high school career (4 years). We convert the estimate to a one month treatment effect of -0.66%.

7. Packham and Slusky (2023) explore the effects of Medicaid access in South Carolina using an RD design and changes in access policies. They find that Medicaid enrollment within six months of release reduces offenses committed within 1 year of release by 0.02% ([T3C1] -0.00002/ 0.097). The treatment is not comparable so we use this effect size.