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

Marcella Alsan† Arkey Barnett‡ Peter Hull§ Crystal S. Yang¶

February 1, 2024

Abstract

A longstanding and influential view in U.S. correctional policy is that "nothing works" when it comes to rehabilitating incarcerated individuals. We re-examine this hypothesis by studying an innovative law-enforcement-led program recently launched in the county jail of Flint, Michigan: Inmate Growth Naturally and Intentionally Through Education (IGNITE). We develop a new instrumental variable approach to estimate the effects of IGNITE exposure, leveraging quasi-random court delays that cause individuals to spend more time in jail both before and after the program's launch. We find that IGNITE exposure dramatically reduces both within-jail misconduct and post-release recidivism. Qualitative evidence suggests a cultural change within the jail as a key mechanism.

PRELIMINARY AND INCOMPLETE – PLEASE DO NOT CITE OR CIRCULATE

*We thank seminar participants at Opportunity Insights, the University of Connecticut, the University of Michigan, and UCLA for helpful comments. We are grateful to Meghan Beal at the National Sheriffs' Association, Superintendent Mickie Kujat and Principal Brad Basista of Mount Morris Schools, and to the administration and incarcerated individuals of Genesee County and Saginaw County Jails—especially Major Jason Gould, Sheriff Chris Swanson, Undersheriff Mike Gomez and Lieutenant Ebony Rasco. We gratefully acknowledge funding from Arnold Ventures. Xingyou Ye, Miguel Purroy, and Anne Fogarty provided expert research assistance.

†Harvard Kennedy School and NBER. Email: marcella_alsan@hks.harvard.edu

‡University of Michigan. Email: arkeymb@umich.edu

§Brown University and NBER. Email: peter_hull@brown.edu

¶Harvard Law School and NBER. Email: cyang@law.harvard.edu

1 Introduction

The United States has the highest incarceration rate in the world, with over two million people held in correctional facilities each day (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 large numbers partly reflect high rates of recidivism: one in four individuals released from a county jail, for example, are re-jailed within the same year.[1] Addressing such incarceration cycles— and reducing recidivism more broadly—remain persistent policy challenges (Doleac, 2023).

Views on the effectiveness of rehabilitation in U.S prisons have generally been negative and slow to change—typified by the influential "nothing works" doctrine, often attributed to Martinson (1974). Following 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 since been challenged, with more optimistic 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 within much of the correctional community. Correspondingly, 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 seen as integral 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 outside life 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). But whether similar rehabilitative policies and philosophies can work in other contexts—and particularly in U.S. jails, where educational initiatives are rare even in comparison to prisons—remains an open question.

This paper studies an innovative law-enforcement-led rehabilitation program, recently launched in the county jail of Flint, Michigan: the Inmate Growth Naturally and Intentionally Through Education (IGNITE) program. 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."[2] At the same time, a notable contrast with the Norwegian experience—besides the U.S. jail context—is the program's cost: IGNITE is largely funded with existing Genesee County resources and staff, avoiding the kinds of large spending increases Norway

[1] See `https://www.prisonpolicy.org/reports/repeatarrests.html`.

[2] See `https://www.route-fifty.com/management/2023/08/igniteing-educational-fire-us-jails/389402/`.

and others have used to launch rehabilitative systems (Bhuller et al., 2020).[3] This low cost and perceptions of broad success have recently led the National Sherrifs' Association to begin scaling-up programs similar to IGNITE in jails across the U.S.[4]

To estimate the effects of IGNITE exposure, we develop a novel instrumental variable (IV) approach which leverages rich administrative data and idiosyncratic delays in court hearings. Court delays are common for jailed individuals and can significantly extend an individual's 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 on average, both before and after the launch of IGNITE. We use this variation to instrument the time a jailed individual is exposed to IGNITE while 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 time exposed to IGNITE while holding fixed time in jail. We formalize the key new assumption underlying this "difference-in-IVs" approach and relate it to the standard "parallel trends" assumption in conventional difference-in-difference estimation. We also contrast the standard independence, exclusion, and monotonicity conditions of our approach with those underlying more conventional "judge IV" designs (employed, for example, by Bhuller et al. (2020)) which appear less tenable in our context.[5]

Applying our IV strategy, we find that exposure to IGNITE dramatically reduces an individual's propensity for both within-jail misconduct and post-release recidivism. We estimate that one additional month of exposure to IGNITE reduces weekly major misconduct in jail by 16 percentage points (49%) while reducing three-month recidivism by 8 percentage points (18%). These effects are homogeneous across groups of different demographics, prior offense status, and predicted exposure to the Flint water crisis. The recidivism effects, however, 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 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 corresponding 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-IVs as effects of the program itself. Other robustness checks probe the more standard identifying assumptions of 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. We also show that we obtain

[3]For example, Norway spends around $93,000 each year per prisoner in its system. (Beaumont, 2023). In contrast, Genesee County Jail spends around $32 per incarcerated individual per day (Diaz, 2021) or around $13,000 per year.

[4]As of September 2023, county jails in nine states have adopted IGNITE; see `https://www.sheriffs.org/ignite`.

[5]Specifically, 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). Similarly, our exclusion restriction requires that court delays only systematically affect later outcomes through increased time in jail—a likely more plausible condition than the assumption of judges only affecting outcomes through a single treatment channel. Empirical tests of conventional judge IV monotonicity and exclusion restrictions decisively reject in our setting.

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 which uses a neighboring county as a "control" group in the post-IGNITE era, instead of our baseline pre-post comparison in the IGNITE county, also yields similar effect estimates. We further obtain similar estimates from a "double" difference-in-IVs which combines cross-county and over-time comparisons and is valid under weaker 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 lead to substantial human capital upgrades among incarcerated individuals. Comparing standardized test scores before and after enrollment in this programming, we find that on average individuals gained a full grade level in both math and reading achievement from low (around 5th-6th grade) baseline levels. Second, we deployed surveys to several stakeholders—including Flint community members, the formerly incarcerated, and current Genesee County staff—to assess the extent of cultural change alongside formal educational programming. We find that those who had personally been exposed to IGNITE or or had exposed relatives were 23 percentage points (70%) more likely to view law enforcement positively. We also find that—among those who were incarcerated or who had close contacts with the incarcerated—exposure to IGNITE was associated with higher likelihood of engagement in positive 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 support 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.

Taken together, 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 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 2021; Bhuller et al. 2020). The effectiveness of IGNITE thus demonstrates that rehabilitative principles can be implemented even within a U.S. county jail located 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 law-enforcement led program, coupled with cultural change and implemented at minimal cost, resulted in better outcomes for incarcerated individuals and far-reaching improvements in public safety.

Our analysis contributes to a broad 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 T. Schnepel 2021; Augustine et al. 2022), specialized criminal courts (e.g., Golestani, Owens and Raissian 2021), 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 criminal effects of health care, employment, and other programs.[6] 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; Caceres-Bravo 2024).

Methodologically, we contribute a new IV strategy that leverages administrative delays extending 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 multiple-treatment IV model to isolate the causal effects of interest via quasi-experimental shocks. Our approach is closest to Abdulkadiroğlu et al. (2016)'s by combining cross-sectional shocks with variation in potential policy exposure over time. We develop several diagnostic tools and extensions of this "difference-in-IVs" approach, which may be fruitfully applied in other settings both within and outside of criminal justice. We pair this quasi-experimental approach with a series of qualitative analyses to help explore possible mechanism, 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.

2 Institutional Setting

2.1 Genesee County Jail and Court System

The IGNITE program originated in Genesee County Jail, which primarily houses individuals from the surrounding city of Flint, Michigan (see Appendix Figure A1). Flint is a majority-Black city with around one-third of households in poverty, and has has experienced several recent crises—including multiple financial mismanagement crises and the nationally-reported Flint Water Crisis. It consistently ranks as one of the highest crime cities in the U.S., with a homicide rate nearly seven times higher than the national average (46.4 vs. 6.5 per 100,000 in 2020; Stebbins, 2021).

Like the approximately 3,000 U.S. jails across the country, Genesee County Jail's population is primarily comprised of three groups of individuals: (1) arrested individuals who are detained pretrial (representing over 90% of the jail population over our sample period); (2) individuals convicted of offenses but awaiting sentencing; and (3) individuals convicted of charges associated with incarceration time of less than one year. Jail populations in both Genesee County and across the nation are overwhelmingly male, young, and of color. In 2021, for example, 87% of those

[6] 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; Bailey et al. 2020

incarcerated in jails nationally were men, 52% were under the age of 35, and 51% were non-white or Hispanic (Zeng, 2022). Those incarcerated in jail are also much more likely to lack a high school degree compared to the general population, and a 2014 prison study found that 72% lacked literate proficiency compared to 52% of U.S. households (NCES, 2014). Stays in jail have lengthened over time, with the national mean length of stay rising over the last decade from 22.7 days to 32.8 days (Zeng, 2022). Below, we further describe court delays—a primary reason for longer stays in jail and which are at the center of our identification strategy.

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 will decide 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, with Panel A for misdemeanor cases and Panel B for felony cases, and is representative of other counties. 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, crimes of a less serious nature that usually carry a maximum jail term of one year, proceed in the District Court through the trial, plea, and sentencing processes. In 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.

Prior to the launch of the IGNITE program in September 2020, Genesee County Jail had very limited educational programming for incarcerated individuals. The jail offered a GED class to a small group of selected individuals through a local school providing adult education, the Mt. Morris Consolidated Schools. The lack of programming in Genesee prior to IGNITE reflects the state of programming in U.S. jails more broadly.[7] As described by one correctional administrator in 2019 (Barrett and Greene, 2023): "we [in Genesee County Jail] were just kind of functioning.... We were sending people to court, sending people to prison, getting people out."

2.2 IGNITE

In the wake of the murder of George Floyd in May 2020, and the elevated racial tensions that followed in Flint as in other parts of the country, the Sheriff of Genesee County decided to launch a near-universal jail education program with incentives for participation of all incarcerated individuals. The stated mission of the IGNITE program is to reduce recidivism and reverse the cycle of generational incarceration through education.

The IGNITE program started in September 2020 and relied on repurposed jail space and staff. For instance, the day room in Genesee County Jail was transformed into a large classroom that was used by different groups of incarcerated individuals at different times of day (see Appendix

[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 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.

Figure A3) and the GED staff member from Mt. Morris became a circulating teacher. As a result of such repurposing, the overall county budget for correctional services did not substantially change with IGNITE's launch (see Appendix Figure A4). Before and after IGNITE, Genessee County Jail spent around $32 per incarcerated individual per day (Diaz, 2021).

In addition to being available to nearly all incarcerated individuals (with the exception of those medically unstable or released without charge), there are three distinguishing features of IGNITE as a jail-based educational program. First, instruction is tailored to the individual based on their educational background and their (predicted) length of stay (see Appendix Figure A5). Some incarcerated individuals may be working on learning to read at the same time as others are completing their GED, while still others might be enrolled in a program for college credit. Individuals work on Google Chromebooks, which facilitate such educational personalization and allow educators to keep a record of achievement so that individuals may continue their coursework post-release at Mt. Morris. Additional technical programming offered as part of IGNITE include certification for food handling, commercial driving, and masonry. The jail regularly hosts graduation ceremonies, where incarcerated individuals celebrate a new diploma, course completion, or job certification in cap and gowns along with family members (see Appendix Figure A3).

Second, participation in IGNITE is incentivized and takeup rates are high (around 90%, per administrative data detailed below). IGNITE programming occurs during two dedicated hours of instruction woven into the daily schedule (see Appendix Figure A6). During instruction time, all other activities cease. Those opting not to participate remain in their cells, while those who do participate receive tablets to access educational programming which they can also use to access approved entertainment during non-IGNITE hours. Individuals who choose to participate in IGNITE are broadly similar to non-participants (see Appendix Table A1).

Finally, in addition to educational instruction, IGNITE was intended to be a significant cultural change for both incarcerated individuals and correctional officers. Correctional officers were tasked with acting as facilitators for a learning environment and incarcerated individuals treated as students capable of change and growth. This shift was immediate, with one jail administrator noting that the launch 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?"[8] In the post-IGNITE period, incarcerated individuals have been described as not just waiting for court dates but anticipating a productive life post-release because of their participation in the program. And correctional staff have expressed a new view of jailed 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."[9] We return to this idea of cultural change using our original surveys and administrative data in Section 6.3.

[8]See `https://www.route-fifty.com/management/2023/08/igniteing-educational-fire-us-jails/389402/`.

[9]See `https://www.route-fifty.com/management/2023/08/igniteing-educational-fire-us-jails/389402/`.

2.3 Court Delays

We leverage administrative delays in the Genesee County court system to study the effects of IGNITE exposure. Genesee County, as in many other parts of the country, routinely experiences court delays and backlogs which can cause individuals to spend many months or years in jail waiting for cases to be adjudicated. In addition to these routine delays, the start of 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 Genessee County and virtually all other parts the country. Administrative court delays contribute to lengthy jail spells, with a mean length of stay in Genesee County Jail of 1.5 months among those who are arraigned with a long right tail (e.g. the 90th percentile length of stay is around four months).

Routine court delays stem from numerous opportunities for rescheduling, as shown in Appendix Figure A2. Specifically, red arrows in these figures 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. Indeed, according to one jail administrator: "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."[10] Such anecdotes align with patterns observed in our data where over 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 with similar charges. Anecdotally, most delays are due to scheduling changes for the judge or prosecutor assigned to the case and rarely occur at the request of the defendant or defense attorney. While our District Court data do not provide a rationale for each observed delay, we will provide evidence below that incarcerated individuals send internal messages to jail administrators asking about when they will next appear in jail with a greater frequency when there are court delays. This pattern is consistent with the anecdotal evidence that most delays are not caused by the defendant. We perform several robustness checks in Section 5.2 narrowing in on sources of delay that are more likely to be court-induced: (1) instances of two or more delays on a given docket-date as this is even more suggestive it was due to court action and (2) periods of crises when the court was officially shut down or slowed, and thus the rationale for the delay is well-established. We also show that delays are common in Saginaw County and below we find that the same court-induced increase in jail stay does not lead to reduced recidivism in this adjacent county.

[10] See `https://flintbeat.com/more-than-90-of-inmates-at-the-genesee-county-jail-are-awaiting-trial/`.

3 Data and Sample Construction

Our analysis of IGNITE leverages rich administrative data capturing both within-jail misconduct and post-release arrests and bookings, as well as novel surveys of the local community, formerly incarcerated individuals, and correctional staff in order to assess mechanisms. We describe each data source and key variables below. Additional details can be found in Appendix B.

3.1 Data Sources and Key Variables

The Jail Management System (JMS) and Recidivism. The JMS, our primary source of administrative information, is a comprehensive electronic database used by Genesee County Jail and other 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 for each offender (age, name, unique individual identifier, home address), arrest date, booking date, release date, charges, and case disposition outcomes for each incarcerated individual.[11] Data from Genesee County Jail also includes the location of the arrest, which we use to identify the specific District Court that handles the case. We use this data to construct our primary outcome of recidivism, defined as whether an individual is rebooked in jail (leveraging unique individual identifiers) over a given time period. Our baseline recidivism measure considers a period of 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), we define recidivism as rearrest in either Genesee or Saginaw County to allow for mobility. In practice, results are virtually identical when restricting to only Genesee County outcomes. In robustness checks, we also construct alternative definitions of recidivism based on 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-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 identifiers, case number, charges, activities, proceedings, and filings for the case, along with dates and times of new court appointments, judges presiding, and notices of adjourned or rescheduled appointments. We use these records to create a comprehensive timeline of court hearings for the jailed individual. We identify court delays from any change to an originally assigned court hearing that has been "removed from the calendar."An example of a ROA with such an identified delay is shown in Appendix Figure A7.

Jail Incident Reports and Misconduct: We use Jail Incident Report data from Genesee County Jail to capture with-jail misconduct and medical events for incarcerated individuals. Misconduct is categorized as either major, minor, or medical. Examples of major misconduct include threatening

[11] The Saginaw County JMS data does not include an individual's home address.

another with bodily harm, introducing contraband, violence and disruption and refusing to follow instructions. Minor incidents include disorderly conduct, being in an unauthorized area, possession of unauthorized items, and lying.[12] Medical events including suicidiality and suicide attempts. For each incident, we observe the date of incident and the name of the involved jailed individual.

The Kites Electronic Message System: The Kites System in Genesee County is an administrative database of all electronic internal messages between jail administrators and incarcerated individuals. This messaging system is available to all incarcerated individuals. For each message sent, we observe the identity of the sender, the date of the message, follow-up responses, as well as the entire content of the message. Using tablets or kiosks, incarcerated individuals can send messages requesting services from numerous individuals, such as the food service vendor, commissary services, medical personnel, and other administrative staff. Incarcerated individuals also often send questions about their case and when they will be released. We use these data to assess the reaction of incarcerated individuals and staff to court delays. In addition, we study the sentiment of the messages to explore possible mechanisms.

The Mt. Morris Student Activity Summary Reports: We obtain administrative data from Mt. Morris Consolidated Schools which contain date- and time-stamped course advancement and completion records from 2021 onwards. In addition, we have data from September 2020 to October 2023 on pre- and post-instruction assessments. The pre-assessments are administered to the near universe of incarcerated individuals to place them in the appropriate education level given IGNITE's focus on tailored instruction. Once enrolled in IGNITE, students take post-assessments every 40 hrs of completed instruction to measure progress. In practice, not all participants have post-assessments because tests are not completed if an individual is abruptly discharged.

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

Community and Formerly Incarcerated Surveys: We conduct surveys of the Genesee County community, formerly incarcerated individuals, and correctional staff in Genesee County Jail to better understanding the impact of IGNITE. All surveys ensured anonymity and we did not ask for personally identifying information in order to facilitate honest responses.

[12] Complaints of misconduct are investigated internally; if the detective bureau determines charges should be filed, the bureau sends the potential charges to the prosecutor's office, who will decide whether or not to charge the individual. If the accused is charged and sentenced on the incident, the jailed individual could have their ongoing jail time lengthened. This happens rarely in practice.

The community survey was conducted in December 2023 and was distributed by community "ambassadors" (also formerly incarcerated individuals) and two ministers of local churches to ensure that the survey would be free from the influence of IGNITE administrators. The community survey asked respondents about their own experience being incarcerated in Genesee County Jail, or the experiences of a close friend or family member, in addition to their perceptions of law enforcement, hope for the future, and participation in activities such as working for pay. The surveys were distributed by single-use QR codes printed on postcards. Ambassadors were deployed around barber shops, dollar stores, local community gathering areas, while ministers provided the surveys to their congregation. A total of 120 surveys were deployed over two waves with a overall response rate of 96 percent. Among the 115 survey replies, we discarded 14 that were unintentionally deployed to tethered individuals in the jail. Participants were provided with a restricted-use (i.e., no firearms, tobacco, or alcohol) $25 Walmart gift card.

3.2 Analysis Sample

We combine the above data sources to construct our main analysis sample, merging on unique case or person identifiers. Appendix Figure A8 summarizes the sample construction. We start with the universe of arrests in the 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. We then merge JMS to ROAs and exclude incarcerated individual-spells where the individual was immediately released without charge and therefore did not interact with the court system or were not eligible for IGNITE. We also exclude a small portion of remaining individuals who are not residents of Michigan (4%), since we are unlikely to measure their recidivism accurately, as well as individuals who are missing key demographic information (1%). We then link these data to Jail Incident Data, Kites data, and Mt. Morris data using individual identifiers. Our final sample includes 23,845 incarcerated individual-episodes representing 14,906 unique individuals.

Summary statistics for our main Genesee County analysis sample are shown in Appendix Table A2. The sample is 76% male, 53% Black, and the majority fall into the age range of 25-44. In terms of past criminal activity, 43% were booked in the past year and over 50% are charged with a felony with an average number of charges of 1.4. The average spell in jail is 1.5 months, with a standard deviation of 4.2. Approximately 40% of the incarcerated individual-spells in Genesee County Jail experience a court delay in District Court and 18% have three-month recidivism.

One potential concern using administrative data recorded by correctional staff is data quality. For example, correctional leadership could in theory under-report misconduct to demonstrate the effectiveness of a given program. However, our primary outcome is re-arrest and re-booking across both Genesee and Sagianow counties, which are largely decisions made by the local police force versus the Genesse County Sheriff and IGNITE administrators. While reported within jail and therefore subjective to misreporting, we also document that the share of major misconduct and medical events are constant over time (see Appendix Figure A9). We also find that the schedule outlined in Appendix Figure A6 is roughly adhered to as measured by the floor-specific login in times on Chromebooks (see Appendix Figure A10).

3.3 Motivating Evidence

Figure 1 motivates the impact of IGNITE on recidivism by plotting the relationship between predicted and observed recidivism in Genesee County, before and after the start of IGNITE. Specifically, we plot the average three-month recidivism rates of individuals booked in Genesee County Jail before and after September 2020 by bins of the individuals' predicted recidivism risk, obtained from a logit regression on individual observables in an earlier holdout sample (described above). Prior to the start of IGNITE, recidivism rates closely track the predictions. But after September 2020, actual recidivism rates are significantly lower than expected, uniformly across all levels of predicted risk. This pattern suggests a dramatic change in recidivism outcomes that coincides with the launch of IGNITE programming. We next develop and apply a more sophisticated empirical strategy to explore the possible causal effects underlying this pattern.

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, and let M_i^J and M_i^I respectively count the number of months individual i spends in jail and is exposed to IGNITE within jail. To start simply, we assume nobody booked pre-IGNITE is exposed to IGNITE, such that $M_i^I = M_i^J \times P_i$. Our general IV specification, developed below, relaxes this assumption to allow for individuals booked before IGNITE to be partially exposed to IGNITE because they were still incarcerated in September 2020.

Next, 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 i's incremental effect of IGNITE exposure holding the individual's time in jail fixed. 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 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, further making Z_i positively correlated with M_i^J and M_i^I.

Under these conditions, an IV regression of Y_i on either M_i^J (pre-IGNITE) or M_i^I (post-IGNITE), 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: specifically, the amount of time in jail individual i is induced to via court delays.[13] 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 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-difference analyses. Our condition is satisfied when, if not for the start of IGNITE, the IV estimates would not have changed in September 2020. 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}.

Under this condition, 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 C.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 akin to Angrist and Imbens (1995). This shows that β^{Δ} generally captures a weighted average of incremental IGNITE effects at different margins of exposure time.

[13] 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 C.1 for derivations of Equations (2) and (3).

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. Specifically, 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 which includes the indicator for a post-IGNITE booking, P_i, along with other controls and a constant. We instrument 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 court delay for individual i.

The main 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. Specifically, the IV estimate can in this case be equivalently obtained as $\hat{\beta} = \hat{\beta}^{Post} - \hat{\beta}^{Pre}$ 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 specification, Equation (6), allows for individuals with exposure to jail both pre- and post-IGNITE.

4.3 Identifying Assumptions and Tests

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

Our first identifying assumption is that court delays Z_i are as good-as-randomly assigned controlling for covariates 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 Panels A and B of Table 1. Specifically, Panel A shows that observable individual characteristics are uncorrelated with court delays given a vector of design controls that capture non-randomness in the location, timing, and type of charge.[14] Panel B further shows balance on the characteristics of census tracts in which individuals reside. Section 5.2 summarizes several additional checks of as-good-as-random assignment.[15]

[14] Specifically, design controls include court division fixed effects (FEs), day-of-week FEs, month FEs, the number of charges, and FEs for charge type (felony, misdemeanor traffic, misdemeanor DUI, and other misdemeanor crime).

[15] Appendix Table A3 checks for differential attrition, which could introduce bias to our IV estimates 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: for 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%.

Alongside these balance tests, Panel C of Table 1 shows that court delays significantly extend an individual's time in jail—an implicit instrument relevance condition for our IV strategy. On average, given the same design controls, individuals spend 0.4 months (around two weeks, or 27%) longer in jail when they experience a court delay. 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: 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. We also discuss in Section 5.2 how our exclusion and monotonicity assumptions may be more plausible than in a conventional "judge IV" strategy which leverage as-good-as-random 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, 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—suggesting minimal scope for such time-varying confounds in our setting.

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 will 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.[16] We find no statistically significant differences in these averages, suggesting compliers are broadly comparable before and after the start of IGNITE.[17]

Three further notes on the interpretation of our IV estimates are warranted. First, while our primary interest is on the causal interpretation of the marginal IGNITE exposure effect coefficient β, we note that the combined $\beta + \gamma$ coefficient may be causally interpretable under weaker conditions. Specifically, $\beta+\gamma$ may captures the average effect of increased time in jail in the post-IGNITE period

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

[17]Similarly, Appendix Figure A11 shows that the weights IV puts on different margins of exposure time are relatively similar pre- and post-IGNITE. See again Appendix C.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 in Appendix Figure A11.

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 manifest similarly in the pre- and post-IGNITE periods. The causal interpretation of β is robust, for example, to court delays directly affecting within-jail misconduct by increasing an individual's frustration with the criminal justice system (an exclusion restriction violation) provided such frustration effects are similar before and after the launch of IGNITE.[18] Third, we note that β targets the average effect of *exposure* to IGNITE programming in jail—not the effect of program participation. While participation rates are known to be high—around 90% on average—we lack individual participation data which allows us to study the latter.[19] Under a natural monotonicity condition, a hypothetical extended IV approach would scale our estimates by such a takeup rate.[20] Our effect magnitudes can thus be viewed as giving a lower bound on the magnitude of IGNITE participation effects.

5 Results

5.1 Misconduct and Recidivism Effects

Figure 2 shows the core reduced-form variation underlying our primary IV estimates. Specifically, each point in the figure 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, controlling for the design controls and all covariates in Panels A and B of Table 1. The latter auxilliary controls appear not needed for identification, but are included in some specifications for additional robustness and to potentially increase precision.

The figure shows strikingly different reduced-form effects of court delays pre- and post-IGNITE. Before September 2020, delayed individuals on average see at most a small increase in within-jail misconduct rates and effectively 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 have sizable negative effects on both misconduct and recidivism after the start of IGNITE. In the Section C.1 framework, a difference-in-IVs estimate contrasting these sets of estimates would therefore suggest large negative IGNITE exposure effects.[21] 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

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

[19] Appendix Table A1 shows, via the GTL data, that participation rates are similarly across observables.

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

[21] Appendix Figure A12 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.

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 effects in columns 1 and 3 come from estimating Equation (6) with our primary outcomes and a covariate vector that includes a post-IGNITE dummy and the design controls. In columns 2 and 4 we further include the auxilliary controls from Panels A and B of Table 1; consistent with the balance tests this addition does not materially change the estimates. Reported reduced-form estimates come from regressing outcomes on the court delay instrument, its interaction with the post-IGNITE dummy, and the controls.[22]

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 by 16.0 percentage points and to reduce three-month recidivism by 8.1 percentage points. These represent reductions of 49% and 18%, respectively, relative to reported control complier means.[23] As in Figure 2, we find no effects of additional months in jail on recidivism pre-IGNITE. The combined estimated effect of months in IGNITE and jail is therefore similar to the estimated IGNITE exposure effect for this outcome. We find a larger positive effect of months in jail on misconduct pre-IGNITE. The combined estimated post-IGNITE months-in-jail effect is therefore smaller to the estimated IGNITE exposure effect for this outcome (around -8.2 percentage points). We contextualize our primary IGNITE exposure effect estimates in Section 6.1, below.

Figure 3 shows how estimated misconduct and recidivism effects very over different horizons of the outcomes. Specifically, 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) and whether an individual was rebooked in a given month since release (Panel B). Panel A shows that estimated misconduct effects are relatively stable over time, with around an 8 percentage point reduction in misconduct risk per 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.

Appendix Figures A13 and A14 explore heterogeneity in our baseline misconduct and recidivism effect estimates: by individual demographics, prior offense status, lead exposure from the Flint water crisis given their zip code of residence, and predicted recidivism risk.[24] Specifically, we estimate versions of Equation (6) which add interactions of the months in IGNITE or months in jail treatments with bins of observable characteristics, add to the instrument list interactions of $(Z_i, Z_i \times P_i)$ with the same bins, and add the bin dummies to the control vector. The figures plot resulting estimates of bin-specific months-in-IGNITE and months-in-jail effects, which are valid

[22] Appendix Table A4 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.

[23] 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 auxilliary controls. Following Appendix C.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.

[24] We predict recidivism risk by a logit regression on individual observables in the earlier holdout sample. Appendix Figure A15 shows we do not find heterogeneity by predicted time in jail, constructed analogously by OLS.

under conditional versions of our main identifying assumptions. Overall, we find roughly similar effect estimates across groups of demographics, prior offense status, and lead exposure (Appendix Figure A13). In contrast, we find meaningful heterogeneity by predicted recidivism level (Appendix Figure A14). Misconduct effects are somewhat larger among individuals with lower levels of risk, While recidivism effects are markedly larger among individuals with the highest levels of risk.

Estimated effects on alternative recidivism and misconduct measures, as well as other related outcomes, are shown in Appendix Table A5. Panel A shows we estimate consistent reductions from IGNITE exposure in the three-month probability of an individual being recharged or reconvicted, as well as in rates of weekly minor misconduct. Panel B shows that 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 large post-release recidivism effects we find in Table 3 thus do not seem mediated by these channels. We also find no significant 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.

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 concerns with our baseline analysis, given the timing of IGNITE's launch in September 2020, is the COVID-19 pandemic which reached its height between March 2020 (the start of the pandemic) and June 2021 (when vaccines were first widely distributed). One might imagine, for example, that individuals jailed over this period were of higher criminal risk (given general attempts to reduce jail populations), that misconduct rates declined simply because individuals were more segregated within jail due to COVID quarantine protocols, or that the pandemic and related policies more broadly affected how misconduct and recidivism outcomes were measured. Reassuringly for the first concern, we find if anything larger effects when restricting to individuals with high levels of predicted recidivism risk (recall Appendix Figure A14). For the second and third concerns, Table 4 shows that we obtain estimates similar to our baseline misconduct specification when estimating effects on misconduct not involving others and that we obtain similar estimates for both misconduct and recidivism outcomes when controlling for a time trend (interacted with the court delay instrument) or when altogether excluding March 2020 to June 2021 from the IV sample. Moreover, we show below that recidivism rates declined in Genesee County around September 2020 relative to neighboring Saginaw County, consistently with our baseline estimates, despite both counties being subject to the same statewide COVID-19 protocols. Together, these checks suggest our findings are not driven by changing conditions from COVID.

A second category of 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 estimates when using alternative definitions of the instrument that are less susceptible to manipulation: specifically, including Circuit Court delays, restricting to COVID and fiscal crisis delays, or restricting to delays occurring on days with multiple court delays across different individuals. Panel A of Appendix Figure A16 gives further evidence that delays are not self-initiated, by showing that the probability of an individual sending a Kites message with a communication or court-related request (using the words *talk, speak, need, can, please, court* or *judge*) jumped suddenly in the four weeks before and 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. These checks support the view that court delays are exogenous, together with the balance checks in Tables 1.

A third category of 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 justice system. Table 4 shows we obtain similar estimates when controlling for an individual experiencing 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: any time-invariant "frustration effect" would be differenced out and not bias our main IGNITE effect estimates. Below we summarize robustness checks using alternative differencing strategies, which are valid under different or weaker exclusion restrictions.

A final concern, specific to the within-jail misconduct outcome, is that IGNITE participation simply occupied the time that individual would have otherwise spent engaging in misbehavior. In other words, IGNITE might reduce within-jail misconduct simply via an "incapacitation" effect. This could affect the interpretation of the large misconduct reductions 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.

5.3 Alternative Identification Strategies

Appendix 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 instead of or in addition to our baseline pre-post comparison in Genesee County. Specifically, column 2 estimates Equation (6) with data from both counties in the post-IGNITE period, instrumenting (M_i^I, M_i^J) with $(Z_i, Z_i \times S_i)$ where $S_i \in \{0, 1\}$ indicates a Saginaw booking and including the baseline design and auxilliary 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.[25] This -6.4 percentage point IGNITE effect is similar to our baseline -8.1 percentage point effect, replicated in column 1. Column 3 further shows we obtain a tight null estimate in the change in time-in-jail effects before and after September 2020 in Saginaw County, suggesting there was no simultaneous regional change in jail or rearrest policy confounding our baseline IGNITE exposure effect estimates. Column 4 subtracts this outside-county placebo check from our baseline over-time IGNITE exposure effect in a a "double" difference-in-IV specifi-

[25] Recall we do not observe misconduct outcomes in Saginaw County.

cation, 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 column 1 (or 2).

Appendix Table A6 shows estimates from an alternative IV strategy which, as in Bhuller et al. (2020), instruments with leave-one-out averages of judge "leniency." Specifically, we estimate Equation (7) in both the pre-IGNITE and post-IGNITE periods, instrumenting time in jail M_i^J with L_i: the average time in jail of other individuals besides i who were assigned to i's judges, 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 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. The latter exclusion restriction and monotonicity condition may be less plausible than our baseline identifying assumptions, since judges make many decisions affecting an individual's experience within and beyond jail and since these decisions may result in longer stays in jail for some individuals and shorter stays in jail for others.[26] Nevertheless, the table shows difference-in-IVs estimates that are qualitatively similar (though generally less precise) as our preferred IGNITE exposure effect estimates.

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, are shown in Appendix Figure A17.[27] Specifically, we plot event study coefficients from regressing the recidivism of individuals booked in either county on a Genesee County indicator interacted with four month bins of the individual's booking date relative to December 2019: the period after which a meaningful share of individuals booked in Genesee County were exposed to IGNITE (see Appendix Figure A18). 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 A8). This translates to a reduction of around 18% per month an individual is exposed to IGNITE, which matches the 18% reduction found with our preferred IV strategy.[28] While less fine-grained than this strategy, the event study figure helps build further confidence in its core logic—specifically with the flat pre-trends showing no unusual pre-IGNITE recidivism trends in Genesee County.

[26] Correspondingly, a joint test proposed by Frandsen, Lefgren and Leslie (2023) rejects exclusion and monotonicity decisively, in both pre- and post-IGNITE samples: see Appendix Table A7.

[27] We do not have misconduct data from Saginaw county; see Appendix B.3 for details on the sample construction.

[28] Specifically, individuals in Genesee County spend around 1.5 months in jail both pre- and post-IGNITE (Appendix Figure A12), average three-month recidivism is around 11 percentage points at baseline (see Appendix Table A8), and (3pp/1.5 months)/11pp $\approx$ 18%.

6 Contextualization and Mechanisms

6.1 Cost of Crime Effects

Table A9 translates our main recidivism effect estimates to estimates of the effect of IGNITE exposure on post-release social costs of crime. Specifically, we estimate Equation (6) with an outcome that measures at different horizons the total cost of future crimes, following the most conservative cost estimates in Miller et al. (2021).[29] For our baseline three-month recidivism horizon, we find that one month of IGNITE exposure decreases the social cost of crime by around \$2,957 per person. Unsurprisingly given Figure 3, this estimated effect grows over time: to a per-person-month reduction of around \$5,615 over a horizon of 12 months. These estimates are large, at 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 the apparent lack of increased spending in Genesee County Jail post-IGNITE (recall Appendix Figure A4).

6.2 Literature Comparison

Figure 4 compares our baseline recidivism effect estimates to others 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. We find that our main one-month IGNITE exposure effect (a 17.7% reduction in one-year recidivism) is comparable to the effects from other rehabilitative programs, ranging from CBT programming (Heller et al., 2017), open prisons (Mastrobuoni and Terlizzese, 2022), diversion (Mueller-Smith and T. Schnepel, 2021; Augustine et al., 2022), and restorative justice conferencing (Shem-Tov, Raphael and Skog, 2021), among others. Notably, our effect is also very similar to the effect that Bhuller et al. (2020) estimate for incarceration in Norway. A unique feature of our estimates, however, is the U.S. jail setting. For reference, the figure also compares 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 greater context (Panel C). In sum, these comparisons show that IGNITE—deployed in a U.S. jail among an especially high-risk population—generates qualitatively similar reductions in recidivism compared to programming in other correctional environments and countries.

[29] 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.

6.3 Potential Mechanisms

One obvious candidate driver of the large misconduct and recidivism effects we find—given the educational nature of IGNITE programming—is human capital accumulation. Incarcerated individuals tend to enter IGNITE with very low reading and math achievement, as measured by CASAS scores. Specifically, Figure 5 shows that the distribution of math and reading scores are centered around a 5th-6th grade equivalence and a 6th-7th grade equivalence, respectively.

A comparison of CASAS scores before and after an individual's enrollment in IGNITE programming indeed suggests substantial human capital upgrades. On average, Figure 5 shows that individuals gained the equivalent of around one grade level in both math and reading. These gains moreover appear widespread: the full distribution of post-test scores is shifted to the right in both subjects, with a notable tilt towards higher grade equivalencies in reading. While these gains may not solely reflect human capital upgrades—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 and consistent with policymakers' views that IGNITE's educational programming was a broad success (Erwin, 2023).

Institutional knowledge and our first-stage estimates both suggest, however, 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 be a significant cultural change within the jail, potentially enhancing any formal education and possibly even affecting individuals who did not successfully participate in programming. On average, our court delay instrument only increased exposure to IGNITE by around two weeks on average—making it unlikely that the the misconduct and recidivism reductions effects come from added instruction time alone. To explore the possible role of a within-jail cultural shift, we turn to our survey analyses.

Survey results on impressions of law enforcement from community members and the formerly incarcerated are reported in Table 6. In this sample, IGNITE exposure is defined as an indicator for whether the respondent themselves or someone in their close social network serving time in Genesee County Jail after the program was initiated in September 2020. The question on views on law enforcement was asked to everyone and was considered the primary outcome and main purpose for the survey. We find that IGNITE exposure predicts an individual having positive views of law enforcement—specifically, 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, which interacts exposure with a categorical variable proxying for time exposed to IGNITE, shows this result is driven by those respondents who had a longer time exposed. Turning to Columns 3-6, which are with respect to post-incarceration outcomes and thus the questions were limited to the roughly 70% of our community sample that is justice-affected, we find positive but insignificant effects of IGNITE on engagement in positive activities (defined as employment, education or caretaking), and no effect on generic hopefulness about the future. Appendix Table A10 shows that IGNITE-exposed individuals are observably similar to other individuals in the survey, supporting the interpretation of these coefficients as causal effects.

Appendix Figure A19 gives further support for the IGNITE-induced culture change, via a sentiment analysis of Kites messages sent by incarcerated individuals before and after September 2020. We use the NRC Word-Emotion Association Lexicon of Mohammad and Turney (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 of the figure shows a significant increase in the share of Kites words categorized as having only positive sentiment, of around four percentage points or 14% of the pre-IGNITE level. Correspondingly, shares of words categorized as having only negative sentiment and of words categorized as having "neutral" (either both or neither positive and negative) sentiment fell after the start of IGNITE. Panel B of the figure further shows that shares of words in Kites messages associated with anticipation, anger, fear, and sadness tended to fall post-IGNITE, while the share of words associated with trust increased. Together with the survey analysis, these qualitative findings support the anecdotal evidence of a broad shift in the Genesee County Jail culture.

7 Conclusion

We provide the first quasi-experimental evidence that educational programming in U.S. county jails can reduce post-release recidivism—potentially mitigating the kinds of incarceration cycles that have long stymied criminal justice policymaking. Exposure to the Michigan IGNITE program is found to dramatically and persistently reduce 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 as a mechanism a broad cultural change within the jail that goes beyond educational programming and which resembles the kinds of rehabilitation-oriented policy found outside the U.S.

We expect the IV strategy we develop for estimating IGNITE exposure effects from 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 to healthcare or other benefit programs by comparing individuals whose appointments to change programs are and aren't 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 show can be probed empirically.

An important remaining question is whether the large IGNITE effects we estimate can be replicated in other U.S. jails. An answer may be soon forthcoming, given the recent law-enforcement-led rollout of IGNITE to jails around the country. As usual with scaling-up attempts, rigorous evaluation of these new programs will be essential (Duflo, 2004), potentially using similar tools as those developed here. But broadly our findings suggest that—as in Flint, Michigan—something can work.

References

67th District Court, State of Michigan. 2022. "Administrative Order 2022-05: Assignment of Cases." Available at: https://www.67thdc.com/wp-content/uploads/2022/07/LAO-Assignment-of-Cases.pdf (Accessed: 2023-05-29).

Abdulkadiroğlu, Atila, Joshua D Angrist, Peter D Hull, and Parag A Pathak. 2016. "Charters without lotteries: Testing takeovers in New Orleans and Boston." *American Economic Review*, 106(7): 1878–1920.

Aizer, Anna, and Joseph J Doyle Jr. 2015. "Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges." *The Quarterly Journal of Economics*, 130(2): 759–803.

Andrews, D.A., and J. Bonta. 2010. *The Psychology of Criminal Conduct.* Lexus/Nexus.

Angrist, Joshua D, and Guido W Imbens. 1995. "Two-stage least squares estimation of average causal effects in models with variable treatment intensity." *Journal of the American statistical Association*, 90(430): 431–442.

Arbour, William, Steeve Marchand, and Guy Lacroix. 2023. "Prison rehabilitation programs and recidivism: evidence from variations in availability." *Journal of Human Resources.*

Arenberg, Samuel, Seth Neller, and Sam Stripling. 2024. "The impact of youth Medicaid eligibility on adult incarceration." *American Economic Journal: Applied Economics*, 16(1): 121–156.

Augustine, Elsa, Johanna Lacoe, Steven Raphael, and Alissa Skog. 2022. "The impact of felony diversion in san francisco." *Journal of Policy Analysis and Management*, 41(3): 683–709.

Autor, David H, Nicole Maestas, Kathleen J Mullen, and Alexander Strand. 2017. "Does Delay Cause Decay? The Effect of Administrative Decision Time on the Labor Force Participation and Earnings of Disability Applicants."

Bailey, Martha J, Hilary W Hoynes, Maya Rossin-Slater, and Reed Walker. 2020. "Is the social safety net a long-term investment? Large-scale evidence from the food stamps program." National Bureau of Economic Research.

Barrett, Katherine, and Richard Greene. 2023. "'IGNITE'ing an educational fire in U.S. jails." *Route Fifty.* See https://www.route-fifty.com/management/2023/08/igniteing-educational-fire-us-jails/389402/ accessed on 01/20/2024.

Beaumont, Leland R. 2023. "A case for Norway's Rehabilitation Oriented Prison System." *The Fulcrum.* See https://thefulcrum.us/a-case-for-norways-rehabilitation-oriented-prison-system accessed on 01/22/2024.

Bergman, Peter, Raj Chetty, Stefanie DeLuca, Nathaniel Hendren, Lawrence F Katz, and Christopher Palmer. Forthcoming. "Creating moves to opportunity: Experimental evidence on barriers to neighborhood choice." *American Economic Review.*

Bhatt, Monica P, Sara B Heller, Max Kapustin, Marianne Bertrand, and Christopher Blattman. 2024. "Predicting and preventing gun violence: An experimental evaluation of READI Chicago." *The Quarterly Journal of Economics*, 139(1): 1–56.

Bhuller, Manudeep, Gordon B Dahl, Katrine V Løken, and Magne Mogstad. 2020. "Incarceration, recidivism, and employment." *Journal of Political Economy*, 128(4): 1269–1324.

Blattman, Christopher, Julian C Jamison, and Margaret Sheridan. 2017. "Reducing crime and violence: Experimental evidence from cognitive behavioral therapy in Liberia." *American Economic Review*, 107(4): 1165–1206.

Bondurant, Samuel R, Jason M Lindo, and Isaac D Swensen. 2018. "Substance abuse treatment centers and local crime." *Journal of Urban Economics*, 104: 124–133.

Bushway, Shawn D, and Emily G Owens. 2013. "Framing punishment: Incarceration, recommended sentences, and recidivism." *The Journal of Law and Economics*, 56(2): 301–331.

Caceres-Bravo, Mauricio. 2024. "Prison Effects on Inmate Misconduct: Evidence from Transfers." Working Paper.

Deshpande, Manasi, and Michael Mueller-Smith. 2022. "Does welfare prevent crime? The criminal justice outcomes of youth removed from SSI." *The Quarterly Journal of Economics*, 137(4): 2263–2307.

Deza, Monica, Johanna Catherine Maclean, and Keisha Solomon. 2022. "Local access to mental healthcare and crime." *Journal of Urban Economics*, 129: 103410.

Diaz, Amy. 2021. "Genesee County Jail overcrowded, most inmates unsentenced. What can be done?" *Flint Beat*. See `https://flintbeat.com/genesee-county-jail-overcrowded-most-inmates-unsentenced-what-can-be-done/` accessed on 01/20/2024.

Di Tella, Rafael, and Ernesto Schargrodsky. 2013. "Criminal recidivism after prison and electronic monitoring." *Journal of political Economy*, 121(1): 28–73.

Dobbie, Will, Jacob Goldin, and Crystal S Yang. 2018. "The effects of pre-trial detention on conviction, future crime, and employment: Evidence from randomly assigned judges." *American Economic Review*, 108(2): 201–240.

Doleac, Jennifer L. 2023. "Encouraging desistance from crime." *Journal of Economic Literature*, 61(2): 383–427.

Drago, Francesco, Roberto Galbiati, and Pietro Vertova. 2009. "The deterrent effects of prison: Evidence from a natural experiment." *Journal of political Economy*, 117(2): 257–280.

Duflo, Esther. 2004. "Scaling Up and Evaluation." *Annual World Bank Conference on Development Economics*, 341–369.

Erwin, Alyssa. 2023. "Celebrating 3 years of I.G.N.I.T.E with the launch of new program R.I.S.E." *ABC12 News*. See `https://www.abc12.com/news/celebrating-3-years-of-i-g-n-i-t-e-with-the-launch-of-new/article_31ee47d6-4e74-11ee-a65b-976a4a5eeace.html` accessed on 01/22/2024.

Estelle, Sarah M, and David C Phillips. 2018. "Smart sentencing guidelines: The effect of marginal policy changes on recidivism." *Journal of public economics*, 164: 270–293.

Frandsen, Brigham, Lars Lefgren, and Emily Leslie. 2023. "Judging Judge Fixed Effects." *American Economic Review*, 113(1): 253–77.

Golestani, Aria, Emily Owens, and Kerri Raissian. 2021. "Specialization in criminal courts: Decision making, recidivism, and re-victimization in domestic violence courts in Tennessee." *Recidivism, and Re-victimization in Domestic Violence Courts in Tennessee (September 14, 2021).*

Harlow, Caroline Wolf. 2003. "Education and Correctional Populations. Bureau of Justice Statistics Special Report."

Heller, Sara B, Anuj K Shah, Jonathan Guryan, Jens Ludwig, Sendhil Mullainathan, and Harold A Pollack. 2017. "Thinking, fast and slow? Some field experiments to reduce crime and dropout in Chicago." *The Quarterly Journal of Economics*, 132(1): 1–54.

Henneguelle, Anaïs, Benjamin Monnery, and Annie Kensey. 2016. "Better at home than in prison? The effects of electronic monitoring on recidivism in France." *The Journal of Law and Economics*, 59(3): 629–667.

Hjalmarsson, Randi, and Matthew J Lindquist. 2022. "The health effects of prison." *American Economic Journal: Applied Economics*, 14(4): 234–70.

Jácome, Elisa. 2020. "Mental health and criminal involvement: Evidence from losing medicaid eligibility." *Job Market Paper, Princeton University.*

Kuziemko, Ilyana. 2013. "How should inmates be released from prison? An assessment of parole versus fixed-sentence regimes." *The Quarterly Journal of Economics*, 128(1): 371–424.

Lavecchia, Adam M, Philip Oreopoulos, and Noah Spencer. 2024. "The Impact of Comprehensive Student Support on Crime: Evidence from the Pathways to Education Program." National Bureau of Economic Research.

Lee, Logan M. 2023. "Halfway home? residential housing and reincarceration." *American Economic Journal: Applied Economics*, 15(3): 117–149.

Leiser, S, S Wang, and JL Tatum III. 2022. "A twenty-year review of Flint's financial condition." *The Center for Local, State, and Urban Policy Fiscal Health Project*, 1–30.

Lochner, Lance, and Enrico Moretti. 2004. "The effect of education on crime: Evidence from prison inmates, arrests, and self-reports." *American economic review*, 94(1): 155–189.

Lotti, Giulia. 2022. "Tough on Young Offenders Harmful or Helpful?" *Journal of Human Resources*, 57(4): 1276–1310.

Martinson, Robert. 1974. "What works?-Questions and answers about prison reform." *The public interest*, 35: 22–54.

Mastrobuoni, Giovanni, and Daniele Terlizzese. 2022. "Leave the door open? Prison conditions and recidivism." *American Economic Journal: Applied Economics*, 14(4): 200–233.

May, Jake. 2021. "12 more inmates graduate from Genesee County Jail's IGNITE program." *MLive Flint.* See `https://www.mlive.com/galleries/TFMHNGKMPZBXJIYVNHUDNAL4ZQ/` accessed on 01/22/2024.

Miller, Ted R, Mark A Cohen, David I Swedler, Bina Ali, and Delia V Hendrie. 2021. "Incidence and costs of personal and property crimes in the USA, 2017." *Journal of Benefit-Cost Analysis*, 12(1): 24–54.

Mohammad, Saif, and Peter Turney. 2010. "Emotions evoked by common words and phrases: Using mechanical turk to create an emotion lexicon." 26–34.

Mueller-Smith, Michael. 2015. "The criminal and labor market impacts of incarceration." *Unpublished working paper*, 18.

Mueller-Smith, Michael, and Kevin T. Schnepel. 2021. "Diversion in the criminal justice system." *The Review of Economic Studies*, 88(2): 883–936.

NCES, National Center for Education Statistics. 2014. "U.S. Program for the International Assessment of Adult Competencies (PIAAC), U.S. National Supplement: Prison Study 2014."

NSA, National Sheriffs' Association. 2022. "Inmate Growth Naturally and Intentionally Through Education." Accessed: 2022–05-12.

Packham, Analisa, and David Slusky. 2023. "Accessing the Safety Net: How Medicaid Affects Health and Recidivism." National Bureau of Economic Research.

Raphael, Steven, and David F Weiman. 2007. "The impact of local labor market conditions on the likelihood that parolees are returned to custody." *Barriers to reentry? The labor market for released prisoners in post-industrial America*, 304–332.

Roach, Michael A, and Max M Schanzenbach. 2015. "The effect of prison sentence length on recidivism: Evidence from random judicial assignment." *Northwestern Law & Econ Research Paper*, , (16-08).

Rose, Evan K, and Yotam Shem-Tov. 2021. "How does incarceration affect reoffending? Estimating the dose-response function." *Journal of Political Economy*, 129(12): 3302–3356.

Sabol, William J. 2007. "Local labor market conditions and post-prison employment: Evidence from Ohio." *Barriers to reentry*, 257–303.

Sanderson, Eleanor, and Frank Windmeijer. 2016. "A weak instrument F-test in linear IV models with multiple endogenous variables." *Journal of Econometrics*, 190(2): 212–221. Endogeneity Problems in Econometrics.

Sawyer, Wendy, and Peter Wagner. 2023. "Mass incarceration: The whole pie 2023." *Prison Policy Initiative*.

Schnepel, Kevin T. 2018. "Good jobs and recidivism." *The Economic Journal*, 128(608): 447–469.

Shem-Tov, Yotam, Steven Raphael, and Alissa Skog. 2021. "Can restorative justice conferencing reduce recidivism? Evidence from the make-it-right program." National Bureau of Economic Research.

Stebbins, Samuel. 2021. "Flint, MI Reported One of the Highest Murder Rates in the US." *247wallst*. Available at: `https://247wallst.com/city/flint-mi-reported-one-of-the-highest-murder-rates-in-the-us/` (Accessed: 2023-05-29).

Supreme Court, Michigan. 2023. "MICHIGAN COURT RULES OF 1985 Chapter 6. Criminal Procedure Updated May 10, 2023." Available at: `https://www.courts.michigan.gov/siteassets/rules-instructions-administrative-orders/michigan-court-rules/court-rules-book-ch-6-responsive-html5.zip/index.html#t=Court_Rules_Book_Ch_6%2FCourt_Rules_Chapter_6%2FCourt_Rules_Chapter_6.htm` (Accessed: 2023-05-29).

Tobón, Santiago. 2022. "Do better prisons reduce recidivism? Evidence from a prison construction program." *Review of Economics and Statistics*, 104(6): 1256–1272.

Tuttle, Cody. 2019. "Snapping back: Food stamp bans and criminal recidivism." *American Economic Journal: Economic Policy*, 11(2): 301–327.

Weisburd, David, David P Farrington, and Charlotte Gill. 2017. "What works in crime prevention and rehabilitation: An assessment of systematic reviews." *Criminology & Public Policy*, 16(2): 415–449.

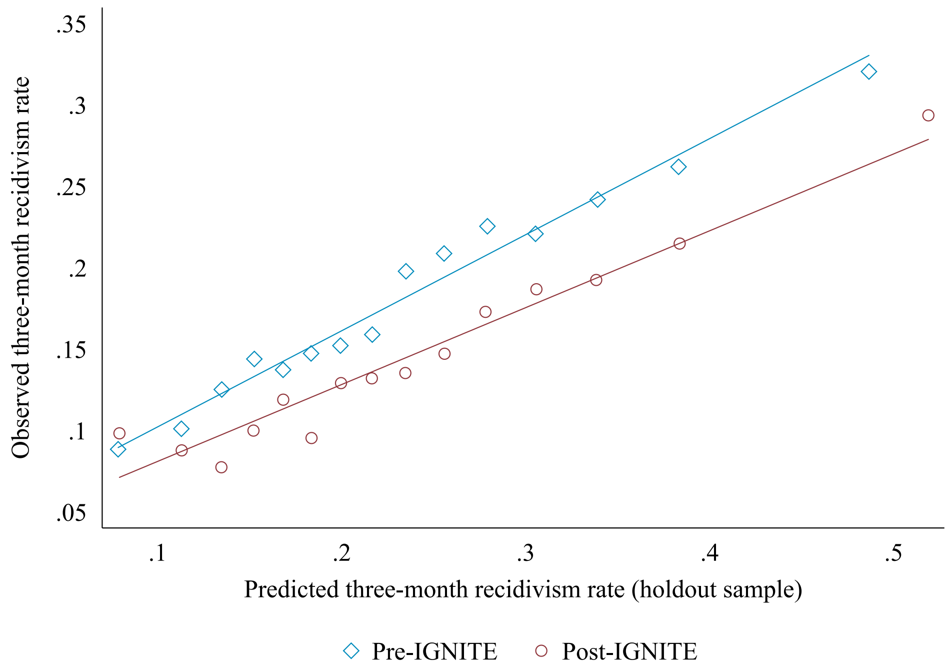
Williams, Jenny, and Don Weatherburn. 2022. "Can electronic monitoring reduce reoffending?" *The Review of Economics and Statistics*, 104(2): 232–245.

Yang, Crystal S. 2017. "Local labor markets and criminal recidivism." *Journal of Public Economics*, 147: 16–29.

Zapryanova, Mariyana. 2020. "The effects of time in prison and time on parole on recidivism." *The Journal of Law and Economics*, 63(4): 699–727.

Zeng, Zhen. 2022. "Jail Inmates in 2021 – Statistical Tables BJS Statistician. NCJ 304888."

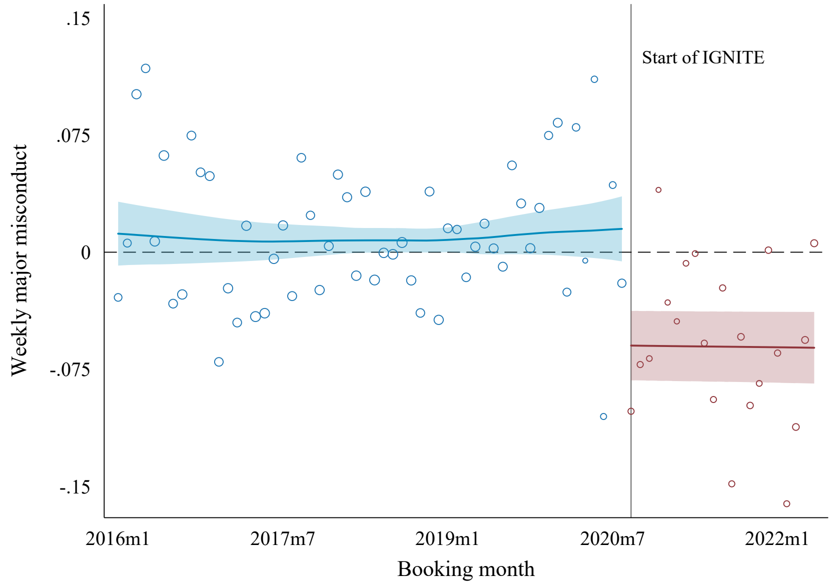
Figure 1: Observed vs. Predicted Recidivism, Before and After IGNITE



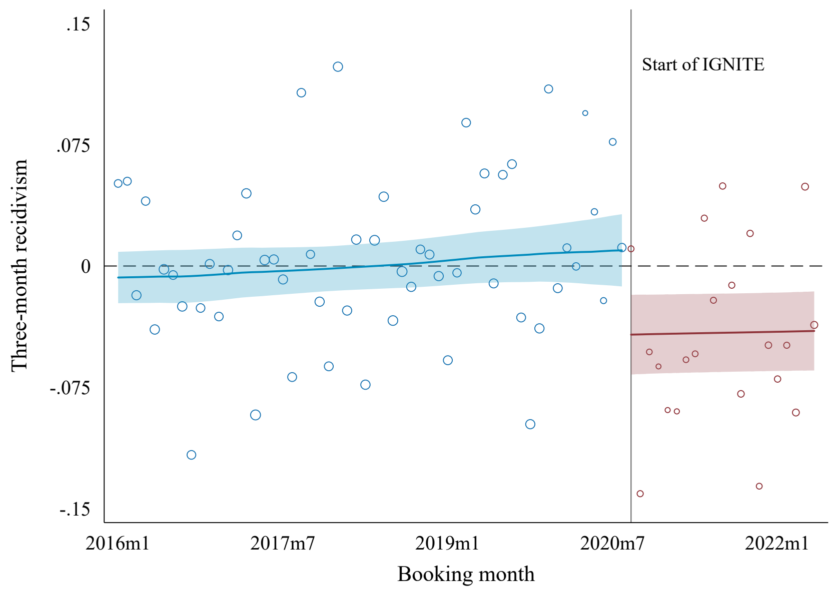
Notes: This figure plots observed three-month recidivism rates by evenly-spaced bins of predicted three-month recidivism risk, before and after the start of IGNITE. Recidivism risk is estimated by a logit regression fit on the 2015 holdout sample; predictors are the design controls and auxiliary controls listed in Table 1, panels (a) and (b).

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

(a) Misconduct



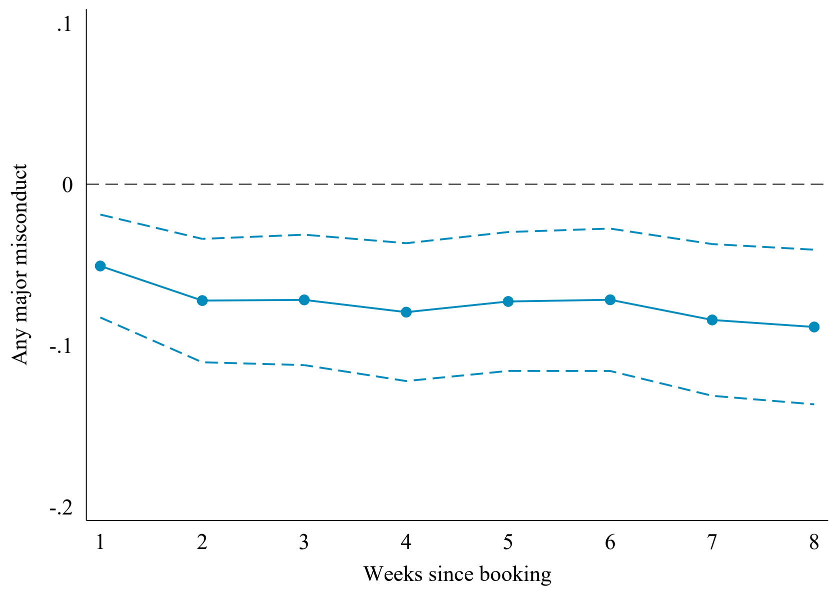
(b) Recidivism



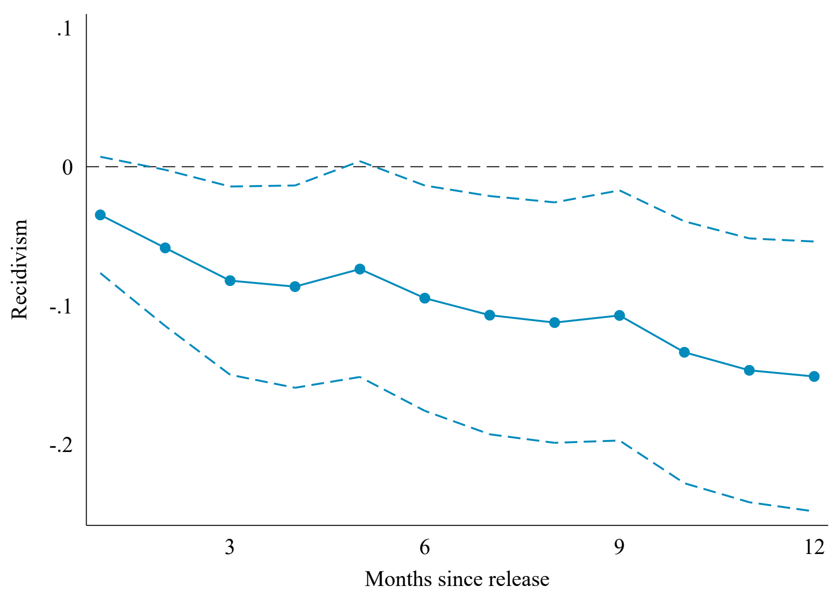
Notes: This figure plots covariate-adjusted monthly average differences in rates of major misconduct and three-month recidivism between individuals who do and do not experience a court delay. Major misconduct rates are defined as the total number of major misconduct events observed for a unique incarceration episode divided by the number of weeks spent in jail for that episode. Recidivism is an indicator for whether the the incarcerated individual was booked again within three months since the last episode's release. Incarcerated individuals sentenced to prison or not observed for three months after their last release are excluded from the recidivism analysis. Each dot indicates the point estimate of the difference in means for incarcerated individuals booked in that month, whereas the fitted lines and associated 95% confidence intervals are computed using local linear regression weighted by number of incarcerated individuals booked that month. Covariates include all design controls (court division fixed effects (FE), day of week for the first scheduled hearing, month FE, number of charges, and case type FE.) and the auxiliary controls listed in Table 1, Panels A and B. The vertical line indicates the beginning of the IGNITE program (September 2020).

Figure 3: Misconduct and Recidivism Effects Over Time

(a) Misconduct



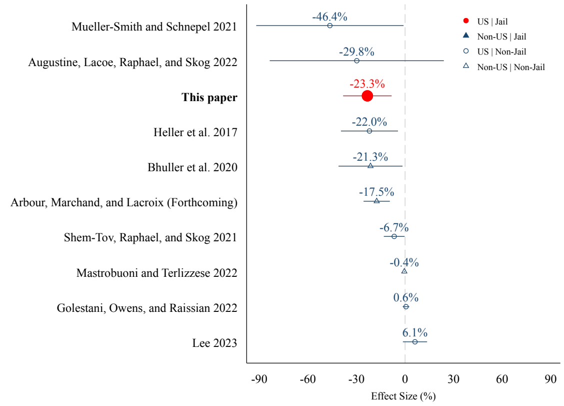
(b) Recidivism



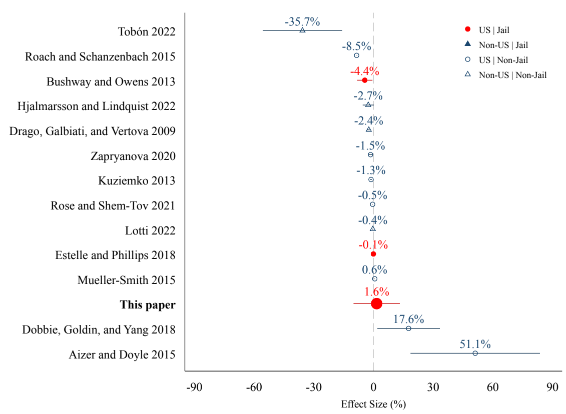
Notes: This figure plots IV estimates and associated 95% confidence intervals of coefficients on Months in IGNITE as in the main specification with outcomes being the probability of involvement in major misconduct in t weeks since booking (Panel A) and the probability of ever being rebooked in t months (Panel B) for $t = 1, \ldots, 12$. We exclude incarcerated individuals sentenced to prison or not observed for t months after release in Panel B. All specifications include the design controls and auxilliary controls described in the text. Dashed lines indicate 95% confidence intervals derived from robust standard errors, clustered at the individual level.

Figure 4: Literature Comparisons

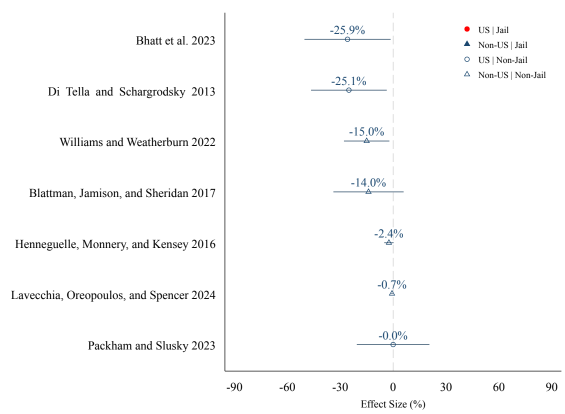
(a) Time in Programs



(b) Time Incarcerated

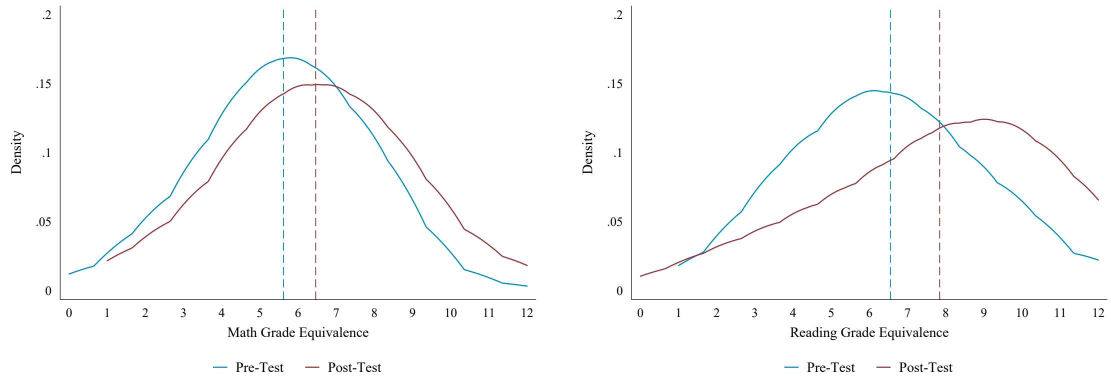


(c) Alternatives to Incarceration



Notes: This figure plots treatment effect sizes found in the criminal deterrence literature. We stratify papers by treatment categories time in programs (Mueller-Smith and T. Schnepel, 2021; Augustine et al., 2022; Heller et al., 2017; Bhuller et al., 2020; Arbour, Marchand and Lacroix, 2023; Shem-Tov, Raphael and Skog, 2021; Mastrobuoni and Terlizzese, 2022; Golestani, Owens and Raissian, 2021; Lee, 2023), time incarcerated (Tobón, 2022; Roach and Schanzenbach, 2015; Bushway and Owens, 2013; Hjalmarsson and Lindquist, 2022; Drago, Galbiati and Vertova, 2009; Zapryanova, 2020; Kuziemko, 2013; Rose and Shem-Tov, 2021; Lotti, 2022; Estelle and Phillips, 2018; Mueller-Smith, 2015; Dobbie, Goldin and Yang, 2018; Aizer and Doyle Jr, 2015), and alternative solutions to incarceration (Bhatt et al., 2024; Di Tella and Schargrodsky, 2013; Williams and Weatherburn, 2022; Blattman, Jamison and Sheridan, 2017; Henneguelle, Monnery and Kensey, 2016; Lavecchia, Oreopoulos and Spencer, 2024; Packham and Slusky, 2023) and distinguish between U.S. and non-U.S. studies and jail and non-jail context. Each point indicates the effect of one month of treatment on one-year recidivism rates as a percent of the control mean. When possible, we scale effect sizes assuming linearity in treatment effects. Recidivism varies by paper but generally is an indicator for whether an individual was arrested, charged, booked, or re-incarcerated within one year since the last episode. When available we use complier control means to compute effect sizes. When linear projections of the effect sizes are not possible, we default to the effect size as given in the paper.

Figure 5: Pre- and Post- Grade-Equivalent Test Performance



(a) Math

(b) Reading

Notes: This figure plots densities for a balanced pre- and post- Math (N: 439) and Reading (N: 309) Grade-equivalent test-scores from jailed individuals in Genesee County, MI Jail. Blue and Red lines represent 1.5 bandwidth densities of pre- and post- grade-equivalent testing scores, respectively from Grades K (0)-12. The Comprehensive Adult Student Assessment Systems (CASAS) exams in Reading and Math were administered by Mt. Morris Consolidated Schools educators and Grades are calculated using CASAS-derived Grade Level Equivalencies for CASAS standardized exams.

Table 1: Court Delay Balance and First Stage

	Overall Mean	Difference in Means	Standard Error
	(1)	(2)	(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)
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: This table reports balance tests for the court delay instrument in the District court. Column 1 reports inconditional mean across the sample, and Columns 2 and 3 report regression coefficients and associated standard errors of incarcerated individual as well as Census Tract characteristics on *Court Delay*. *Court Delay* is an indicator that equals one if the phrase "Removed from Calendar" ever appeared in the District ROA and zero otherwise. Panel A reports results for incarcerated individual level variables, where sex, age, race, and whether booked in past year are obtained from Jail Management System (JMS) data, and whether the attorney is a public defender is obtained from the ROA. Panel B reports results for Census Tract level variables linked to the residential address of incarcerated individuals recorded in JMS data. Share with Elevated Blood Lead Level refers to share with blood lead levels above 4.5 micrograms of lead per deciliter of blood among the tested in 2017 (data collected by Michigan Department of Health and Human Services; available at `https://catalog.data.gov/no/dataset/leadbloodlevels-2017-bytract-20181129-dec8f`). Share Black, Share High School Graduate or Higher, and Log Median Household Income are from 2016 ACS 5-year Estimates. Missing Census Information is an indicator that equals one if an observation is not matched to a Census Tract. Panel C reports the first stage for Months in Jail, the difference between release date and booking date for each unique incarceration episode. All estimations include the following design controls: court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE. Reported F-statistic refers to a joint test of the null hypothesis for all variables. Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Table 2: Complier Characteristics, Before and After IGNITE

	Pre-IGNITE	Post-IGNITE	Pre − Post	Full Sample
	(1)	(2)	(3)	(4)
Panel A: Individual Characteristics				
Peer Pred. Risk				
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 of this table report IV estimates for the regression of the interaction between a characteristics variable and months in jail on months in jail instrumented by court delays pre- and post-IGNITE, Column 3 report the equality test between the two coefficients, and Column 4 reports the unconditional mean across the sample. Crime Characteristics are a set of indicators that equal one if a charge recorded in the ROA falls into one of the mutually exclusive categories listed. Note that it is possible to be charged with multiple crimes simultaneously. Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** 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 for Number of Major Misconduct per Week (Columns 1 and 2) and Ever Rebooked in three Months (Columns 3 and 4). *Misconduct* is meaures as the total number of major misconduct incidents an incarcerated individual was involved in for a unique incarceration episode dividing the number of weeks spent in jail for this episode, and *Recidivism* is an indicator for whether the incarcerated individual was booked within three months since the last release. For incarcerated individuals not observed for three months after release, we exclude them from the three-month recidivism analysis. *Months in Jail* is the difference between release date and booking date, and *Months in IGNITE* is defined as the time spent in Jail after September 2020 for each unique incarcerated individual-spell observation. *Court Delay* is an indicator that equals one if the phrase "Removed from Calendar" ever appeared in the District ROA and zero otherwise. *Design controls* include court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE. *Auxiliary controls* include demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** 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**
($N = 20,658$)	(0.073)	(0.081)
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)
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)	

Notes: This table summarizes additional robustness checks for the primary misconduct and recidivism effect estimates. The first row reports our baseline specification. The second row restricts the sample to individuals with the top quartile of predicted recidivism risk, as in Figure A14. The third row estimates effects on rates of weekly major misconduct not involving other individuals. The fourth row includes a linear time trend interacted with the court delay instrument as a control. The fifth row excludes the March 2020 - June 2021 COVID period. The sixth row includes Circuit Court delays to define the court delay instrument. The seventh row uses only delays associated with the COVID-19 pandemic or fiscal crises to define the instrument. The eighth row only uses delays on days with there were multiple rescheduling events to define the instrument. The ninth row includes an indicator for having experienced multiple delay events as a control. The tenth row estimates effects on rates of weekly major misconduct restricted to times of day with no IGNITE Programming.

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

	Difference-in-IVs			
	Post vs. Pre, Genesee (Baseline)	Genesee vs. Saginaw, Post	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 for Ever Rebooked in three Months using various identification strategies. Column 1 reports the basline estimates from Table 3 column 4. Column 2 reports a regression estimating the difference between Genesee County and Saginaw County in the post-IGNITE period. Column 3 reports results applying the identical strategy as column 1 using Saginaw sample. Column 4 reports the difference in the coefficients on *Months in IGNITE* between columns 1 and 3. All estimates include *Design controls* (court division FE, day of week for the first scheduled hearing, month FE, number of charges, case type FE) and *Auxiliary controls* (demographics, whether booked in past year, whether has a public defender). Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

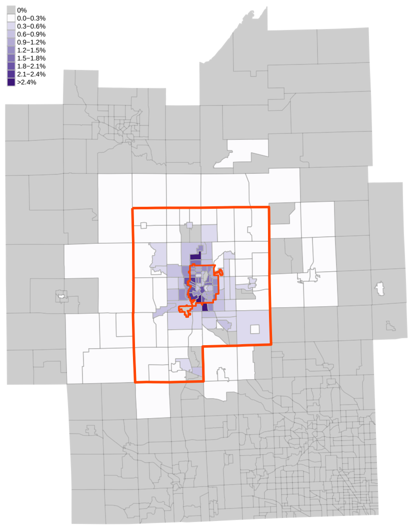
Table 6: Community Survey Results

	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

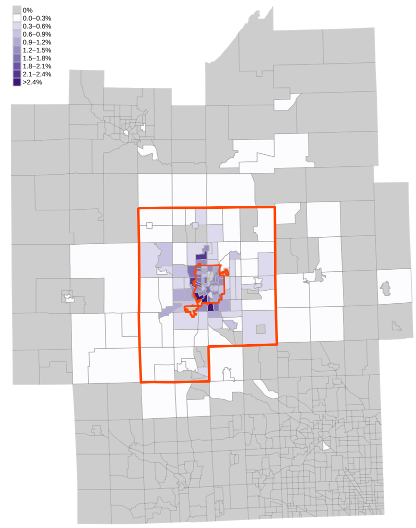
Notes: This table reports the effect of IGNITE exposure on the binary outcomes of positive views of law enforcement (Columns 1 and 2) engagement in positive activities post-incarceration (Columns 3 and 4) and hopefulness post-incarceration (Columns 5 and 6). An indicator for survey wave is included in all regressions. Even numbered columns also report the interaction of IGNITE exposure with time in jail, which was categorical on the survey ranging for less than one month to over one year. IGNITE exposure is an indicator for whether the respondent was personally incarcerated or had a close friend or family member incarcerated in Genesee County Jail in the post-IGNITE period. Questions on post-incarceration outcomes were only asked to those who had themselves or had a close relation incarcerated recently. See Data Appendix B.4 for further details. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

A Appendix Figures and Tables

Appendix Figure A1: Rearrest Locations



(a) Percent Arrests, Pre-IGNITE

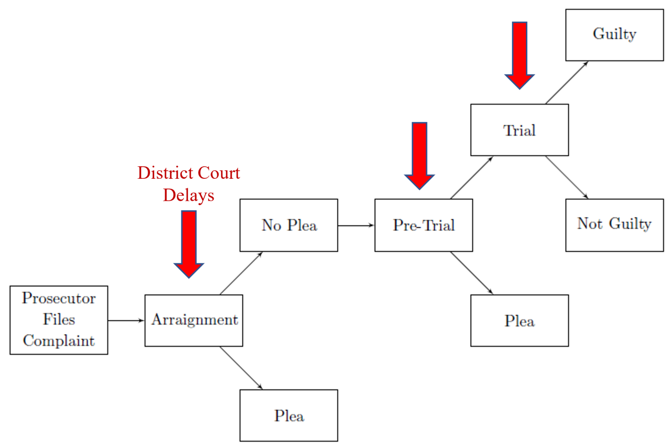


(b) Percent Arrests, Post-IGNITE

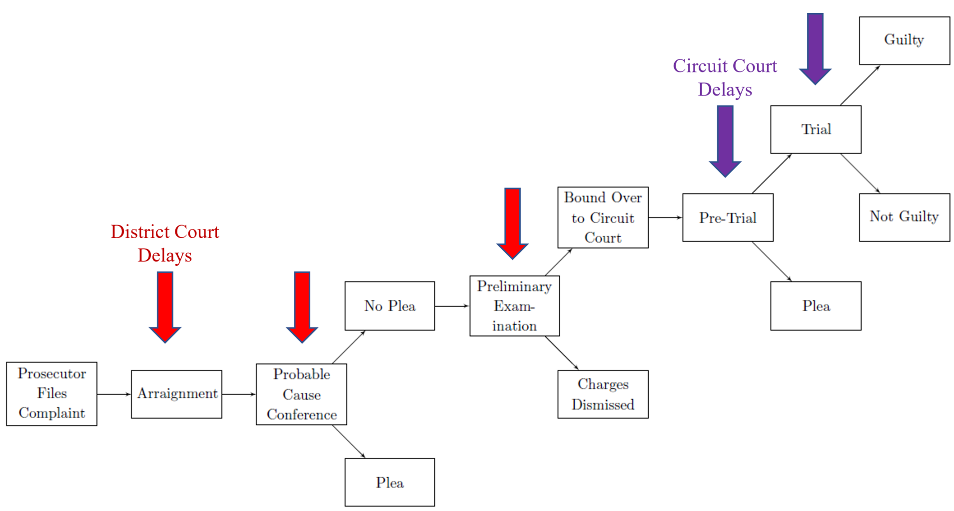
Notes: This figure plots the share of individuals rearrested among total arrests in Genesee County, before and after the start of IGNITE, by home address Census tract. Regions include Genesee County and adjacent counties (Lapeer, Tuscola, Saginaw, Shiawassee, Livingston, and Oakland). Borders of Genesee County and Flint City are highlighted.

Appendix Figure A2: Court Process Flow Chart

(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 where District Court delays are possible. Purple arrows indicate the stages where Circuit Court Delays are court is possible.

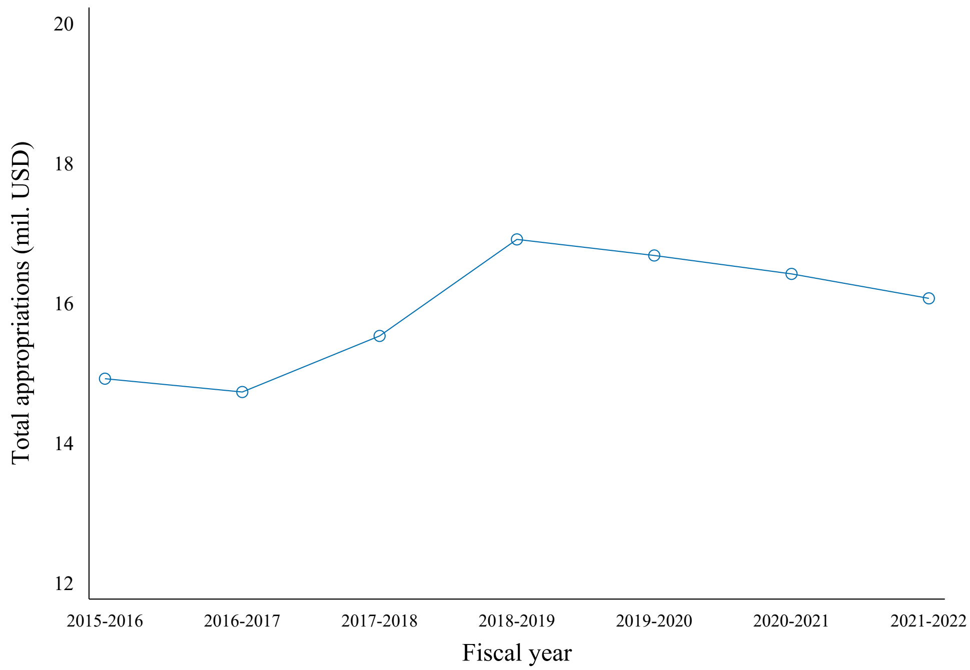
Appendix Figure A3: IGNITE in Pictures

(a) Classroom

(b) Graduation

Notes: Panel (a) shows individuals participating in the IGNITE program within Genesee County Jail, from NSA (2022). Panel (b) shows an IGNITE graduation ceremony, from May (2021).

Appendix Figure A4: Budget of Genesee County Corrections Division



Notes: This figure e plots the total appropriations of the Genesee County Sheriff Corrections Division from fiscal years 2015-2016 to 2021-2022. Data for fiscal years 2016-2016 to 2018-2019 are available at `https://www.dropbox.com/s/he0z2jmfh1zpg00/2018-2019%20Adopted%20Budget.pdf?dl=0`, and data for fiscal years 2019-2020 and onward are available at `https://www.dropbox.com/s/axl64tqx31lin6c/2022-2023%20Adopted%20Budget.pdf?dl=0`. Note that capital outlay is subtracted from total appropriations for fiscal years 2019-2020 and onward for consistency with fiscal years 2016-2016 to 2018-2019.

Appendix Figure A5: Example of IGNITE Program Tailoring

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: NSA (2022).

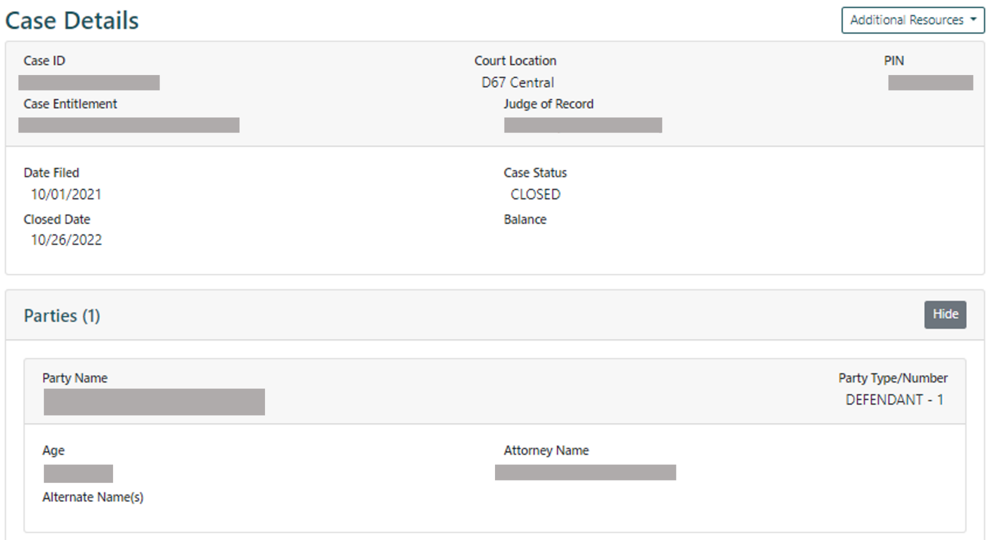
Appendix Figure A6: Example IGNITE Schedule

		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					

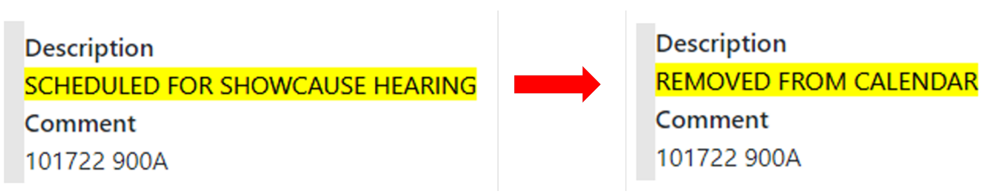
Notes: This figure shows an example schedule of IGNITE program times and other Genesee County Jail activities, by the cell floor of incarcerated individuals. Source: NSA (2022)

Appendix Figure A7: Example ROA

(a) Case Information

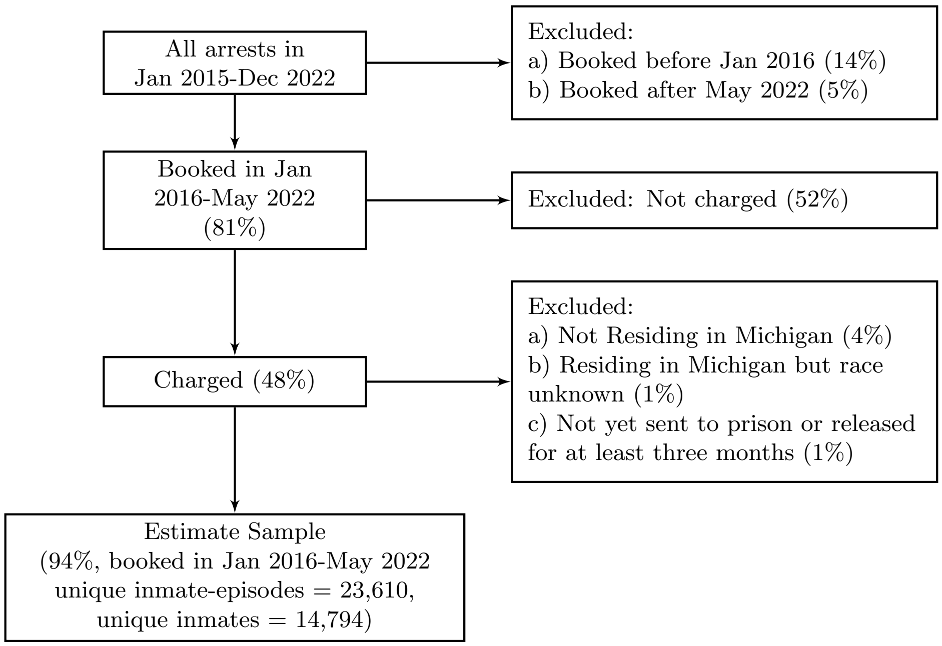


(b) Court Delay



Notes: Panel (a) of this figure shows an example Genesee County ROA. Panel (b) shows an example court delay as identified in the ROA description.

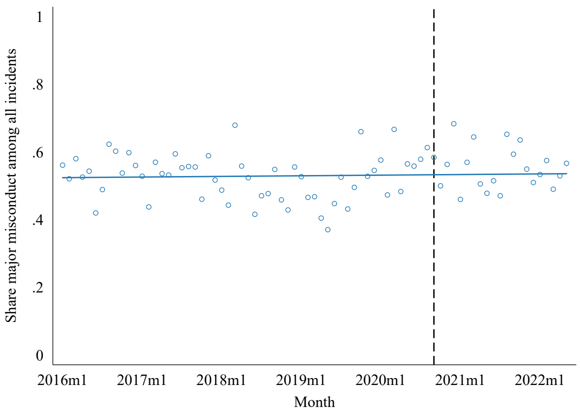
Appendix Figure A8: Analysis Sample Construction



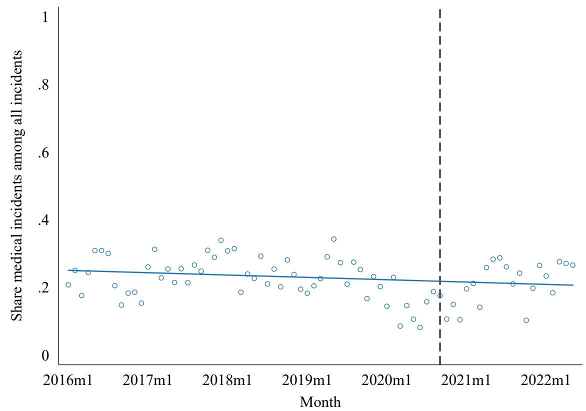
Notes: This figure summarizes the construction of our primary analysis sample.

Appendix Figure A9: Share Major Misconduct and Medical Incidents

(a) Major Misconduct

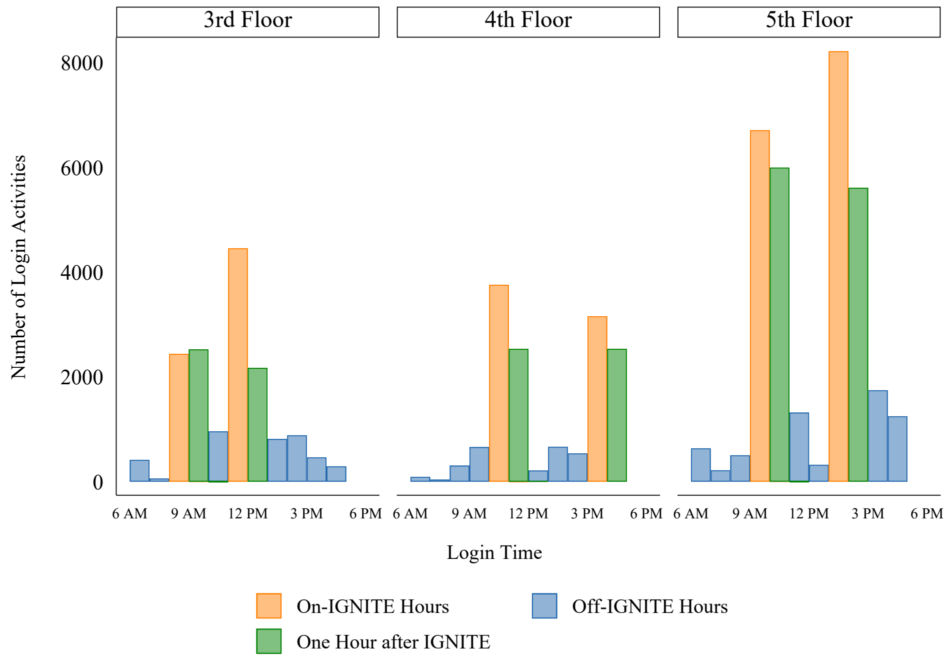


(b) Medical Incidents



Notes: Panel (a) of this figure plots the monthly share of major misconduct among all incidents recorded in the JMS data by booking month. Panel (b) plots the monthly share of medical incidents attempts across all incidents. The vertical dashed lines indicates the beginning of IGNITE (September 2020). The horizontal lines indicate best-fit trends.

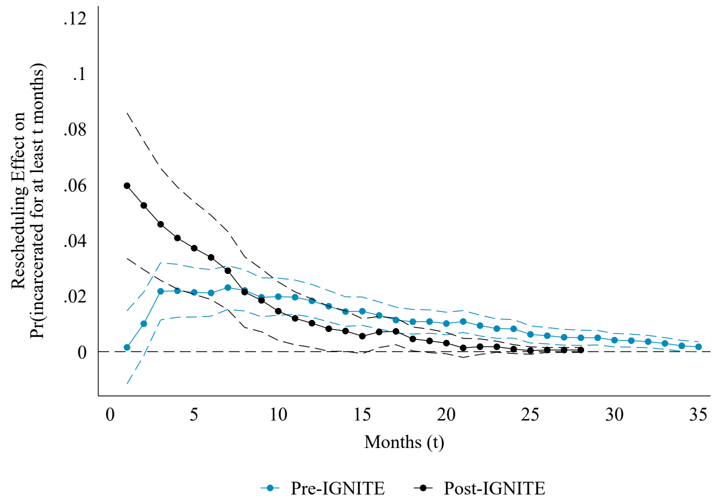
Appendix Figure A10: Chromebook Usage by Floor



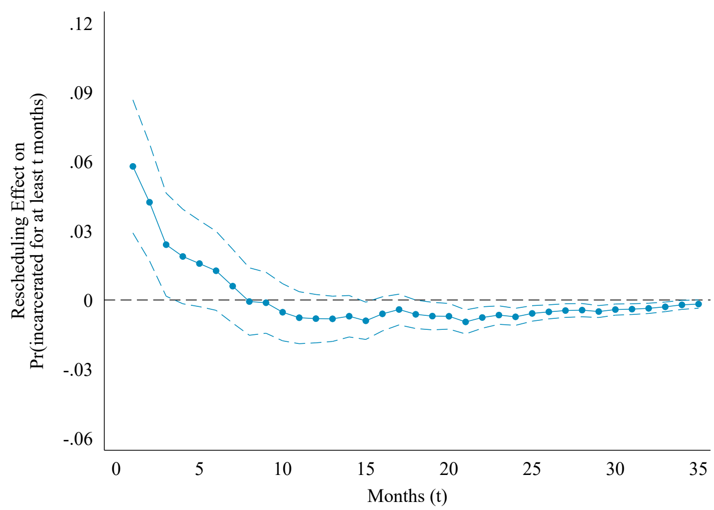
Notes: This figure plots the distribution of login activities on program Chromebooks by time of day, with each bin being one hour by jail cell floor. Chromebook usage comes from the Mt Morris administrative data.

Appendix Figure A11: ACR Weights

(a) Estimated Weights

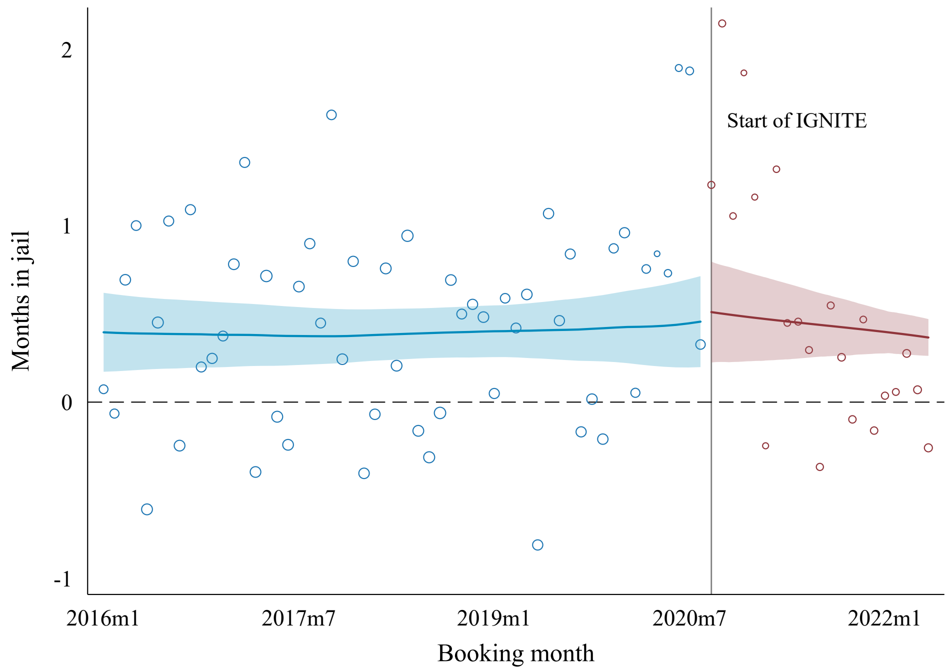


(b) Difference between Pre-/Post-IGNITE



Notes: This figure plots estimated Average Causal Response (ACR) weights (Angrist and Imbens, 1995) before and after IGNITE. Panel (a) plots estimated coefficients from regressing indicators for whether time in jail exceeds t months on the court delay instrument, for $t = 1, \ldots, 35$. Panel (b) plots the difference between the pre- and post-IGNITE estimates. The dashed lines are associated robust 95% confidence intervals. All estimations include all design controls and auxiliary controls.

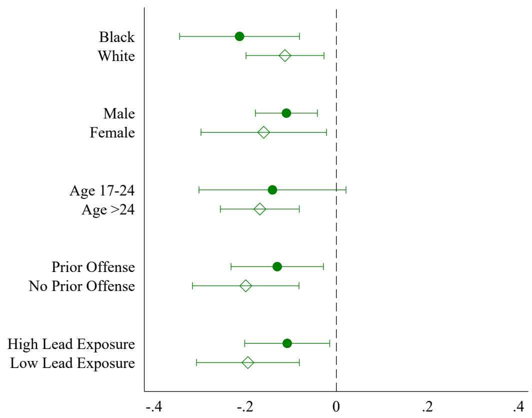
Appendix Figure A12: First-Stage Effects of Court Delays by Booking Month



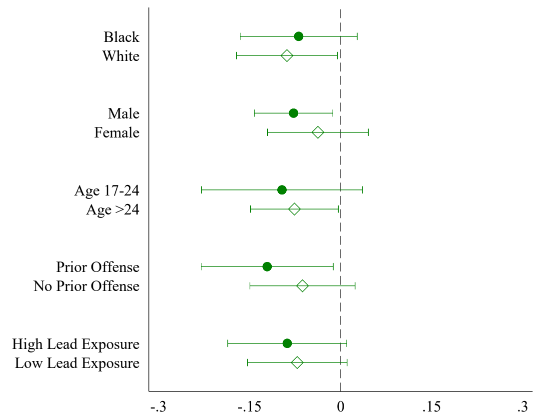
Notes: This figure plots covariate-adjusted monthly average difference in incarceration length. Each dot indicates the point estimate of the difference in means for incarcerated individuals booked in that month, whereas the fitted lines and associated 95% confidence intervals are computed using local linear regression weighted by number of incarcerated individuals booked that month. Covariates include all design controls (court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE.) and variables listed in Table 1 Panels A and B. The vertical line indicates the beginning of the IGNITE program (September 2020), and the horizontal line indicates 0.

Appendix Figure A13: Heterogeneity by incarcerated individual Demographics and Residential Zip Code Characteristics

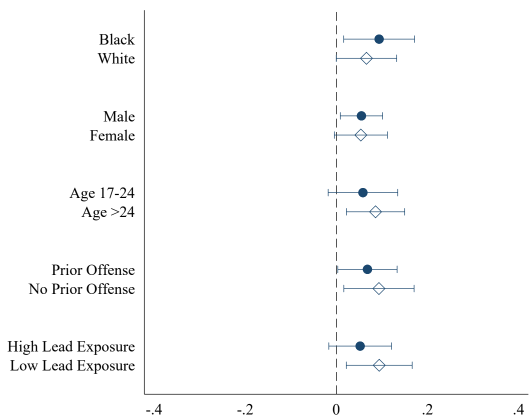
(a) Effects of Months in IGNITE on Misconduct



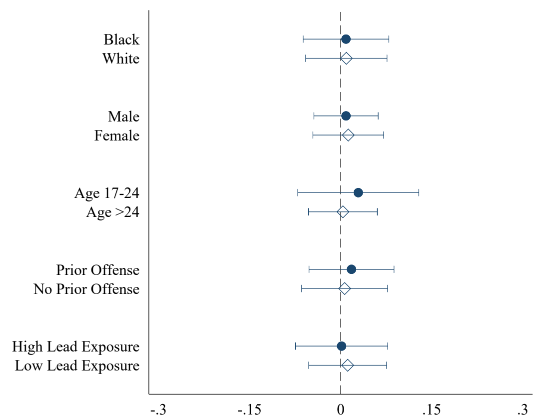
(b) Effects of Months in IGNITE on Recidivism



(c) Effects of Months in Jail on Misconduct



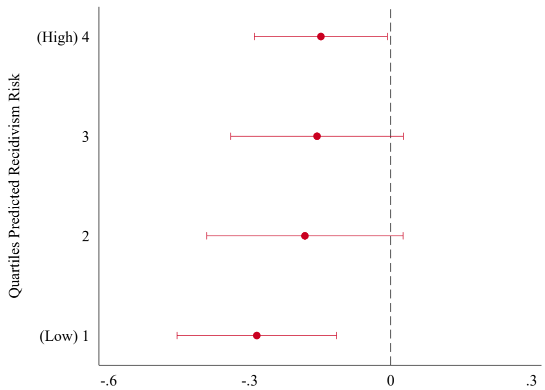
(d) Effects of Months in Jail on Recidivism



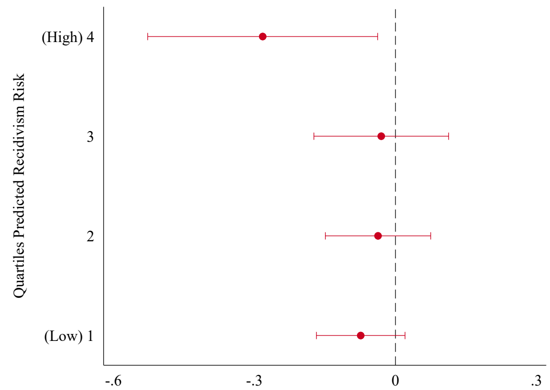
Notes: This figure plots the IV estimates and associated 95% confidence intervals of the sum of coefficients on Months in IGNITE (Jail) and the interaction between Months in IGNITE (Jail) and an indicator for race is Black, sex is Male, age is between 17-24, had prior offense, and being in the 4-th (highest) quartile of Census Tract Level Share with Elevated Blood Lead Level. Panels A and C report results for *Number of Major Misconduct per Week*, defined as the total number of major misconduct an incarcerated individual was involved in for a unique incarceration episode dividing the number of weeks spent in jail for this episode, and Panels B and D for *Ever Rebooked in Three Months*, an indicator for whether booked again within three months since the last release. incarcerated individuals sentenced to prison or not observed for three months after their last release are excluded from the recidivism analysis. All estimations include all *Design controls*: court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE and *Auxiliary controls*: demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Figure A14: Heterogeneity by Predicted Recidivism Quartiles

(a) Misconduct



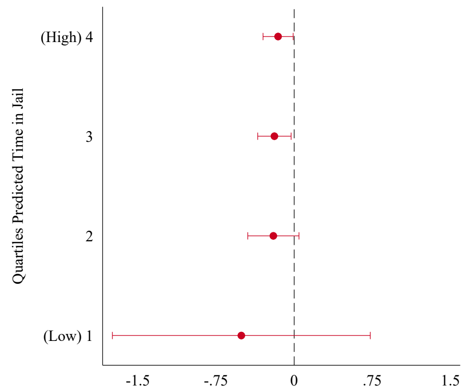
(b) Recidivism



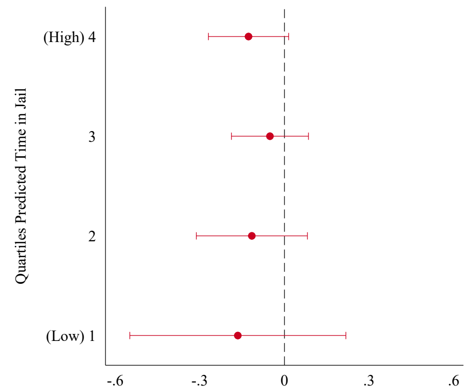
Notes: This figure plots the IV estimates and associated 95% confidence intervals of the sum of coefficients on Months in IGNITE and the interaction between Months in Jail and an indicator for being in the x-th quartile of the predicted three-month recidivism probability for $x = 1, 2, 3, 4$. Panel A reports results for *Rate of Major Misconduct per Week*, defined as 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., and Panel B for *Ever Rebooked in Three Months*, an indicator for whether booked again within three months since the last release. Predicted three-month recidivism risk is produced by applying a logit regression fitted on the 2015 sample on the estimate sample that ranges from 2016 to 2022. incarcerated individuals sentenced to prison or not observed for three months after their last release are excluded from the recidivism analysis. All specifications include *Design controls*: court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE and *Auxiliary controls*: demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors are clustered by incarcerated individual.

Appendix Figure A15: Heterogeneity by Predicted Time-in-Jail Quartiles

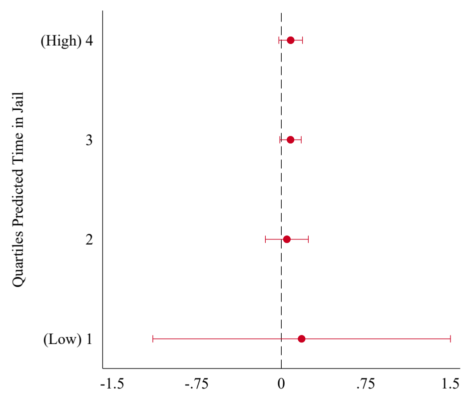
(a) Effects of Months in IGNITE on Misconduct



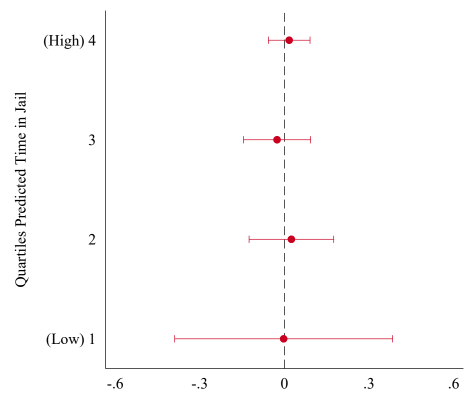
(b) Effects of Months in IGNITE on Recidivism



(c) Effects of Months in Jail on Misconduct

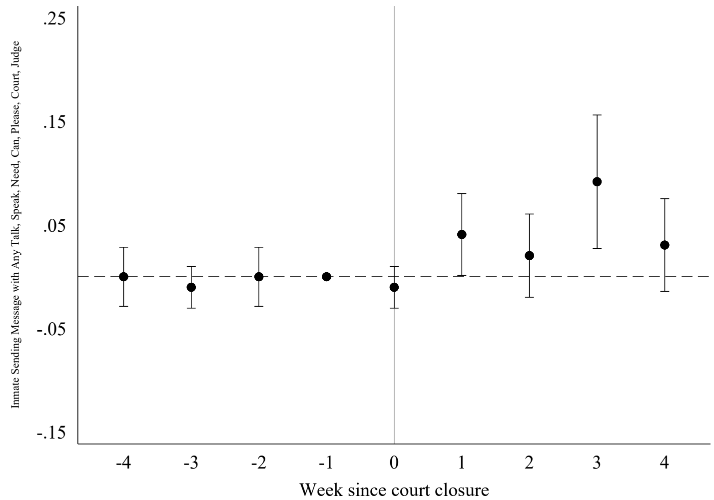


(d) Effects of Months in Jail on Recidivism

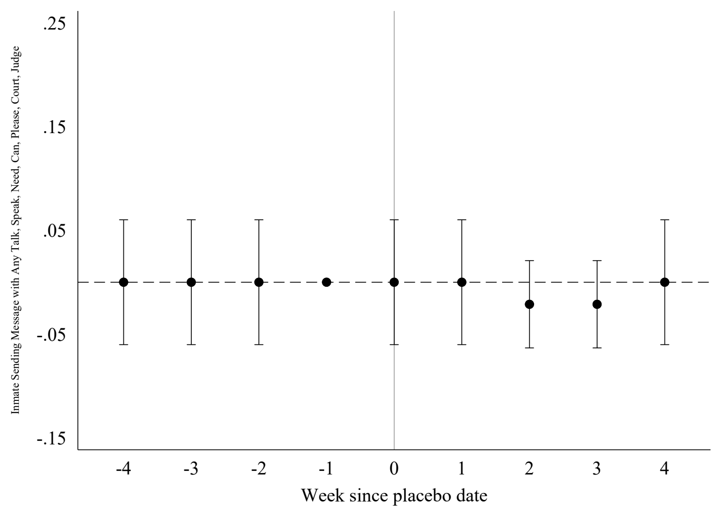


Notes: This figure plots the IV estimates and associated 95% confidence intervals of the sum of coefficients on Months in IGNITE (Jail) and the interaction between Months in IGNITE (Jail) and an indicator for being in the x-th quartile of the predicted incarceration length for $x = 1, 2, 3, 4$. Panel A reports results for *Rate of Major Misconduct per Week*, defined as 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., and Panel B for *Ever Rebooked in Three Months*, an indicator for whether booked again within three months since the last release. Predicted three-month recidivism risk is produced by applying a logit regression fitted on the 2015 sample on the estimate sample that ranges from 2016 to 2022. incarcerated individuals sentenced to prison or not observed for three months after their last release are excluded from the recidivism analysis. All specifications include *Design controls*: court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE and *Auxiliary controls*: demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors are clustered by incarcerated individual.

Appendix Figure A16: Event Study - Court Closure [March 17, 2020]



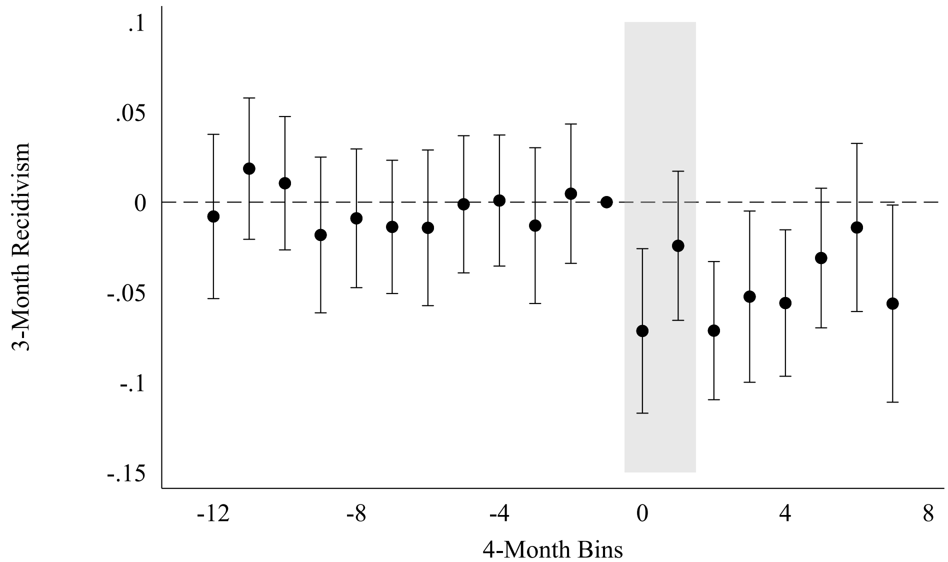
(a) Court Closure



(b) Placebo

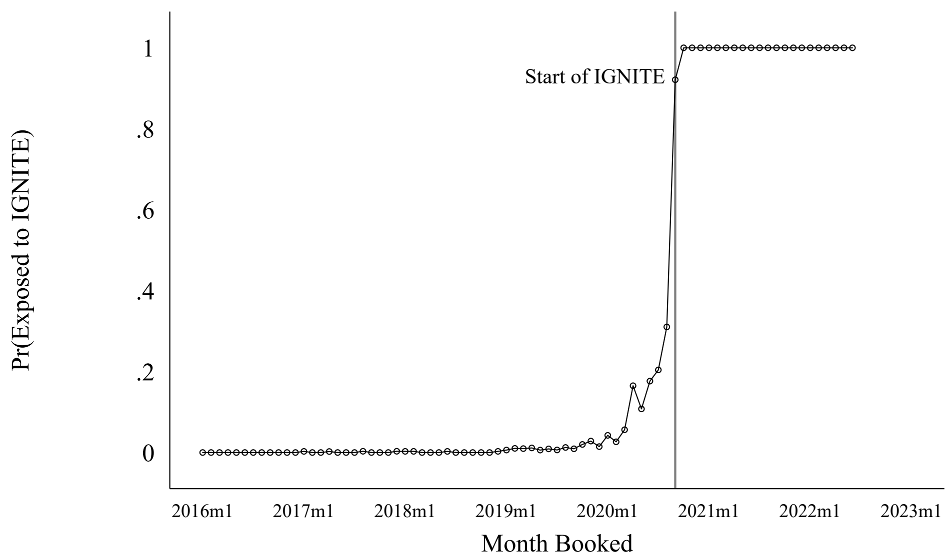
*Notes:*This figure plots an event study regression of an indicator for sending messages using the words talk, speak, need, can, please, court or judge. Panel A uses one week prior to the court closure due to COVID 19 (March 17, 2020) as base period. Panel B uses a placebo date (May 17, 2019) as period 0. Standard errors are clustered at the individual level.

Appendix Figure A17: Effects of IGNITE on Recidivism: Event Study Estimates



Notes: This figure plots an event study regression of three-month recidivism combining data from Genesee and Saginaw Counties. The unit for time periods is four months. Base period is Dec 2019. No additional controls are included.

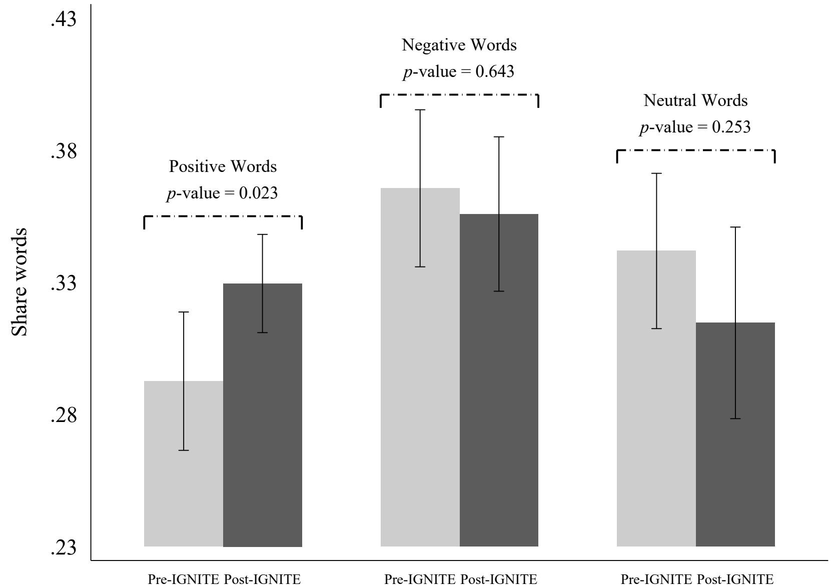
Appendix Figure A18: Probability of Exposed to IGNITE



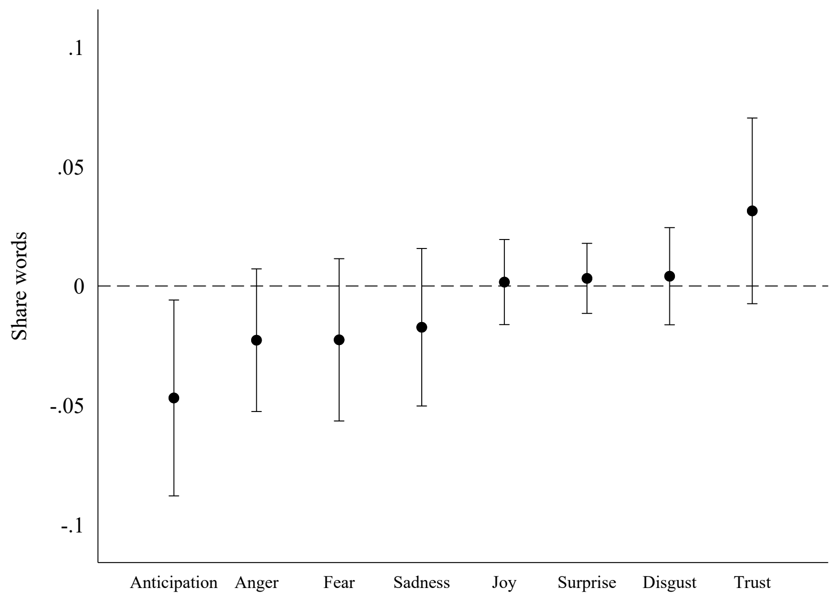
Notes: This figure plots the probability of being exposed to IGNITE as a function of month booked.

Appendix Figure A19: Kites Sentiment Analysis

(a) Share of Words



(b) Post − Pre



Notes: This figure plots the sentiment pre- and post-IGNITE in Kites messages. Panel A plots share of words categorized as positive, negative, and neutral by time period. The p-values are obtained from a regression weighted by word frequency at unique word level, and the standard errors are clustered at individual level. Panel B plots the coefficient on post-IGNITE from a similar regression of an indicator for the word being associated wih an emotion.

Appendix Table A1: IGNITE Participation Balance

	Overall Mean	Difference in Means	Standard Error
	(1)	(2)	(3)
Panel A: Individual Characteristics			
Female	0.131	0.056	(0.046)
Age	34.291	2.450	(2.003)
Black	0.548	-0.082	(0.091)
Booked in Past Year	0.458	-0.001	(0.001)
Public Defender	0.191	0.043	(0.052)
Panel B: Census Tract Characteristics			
Share with Elevated Blood Lead Level	0.030	-0.001	(0.001)
Share Black	0.433	0.027	(0.033)
Share High School Graduate or Higher	0.838	-0.027	(0.030)
Log Median Household Income	10.545	-0.550	(0.672)
Missing Census Tract Information	0.032	0.002	(0.003)
F-Statistic for Joint Test [p-value]		1.021 [0.321]	
Observations		227	

Notes: Table reports summary statistics and T tests between individuals who appeared/did not appear in the IGNITE name list for incarcerated individuals that were still in jail as of June 2020.

Appendix Table A2: Summary Statistics

	Mean (1)	SD (2)	N (3)
Panel A: Instrument and Outcomes			
Any 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)	22,191
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	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

Notes: This table reports the unconditional mean, standard deviation, and number of non-missing observations of variables for the estimate sample.

Appendix Table A3: 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)
Any 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: Table reports differential attrition over various time horizons. Each column reports a regression of an indicator that equals one if observed for X months after release on *Any Rescheduling* for $X = 3, 6, 9, 12$. All controls are included. Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A4: 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
SW F-Court Delay × Post-IGNITE	79.909	79.611	79.909	79.611
SW F-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)). *Months in Jail* is the difference between release date and booking date, and *Months in IGNITE* is defined as the time spent in Jail after September 2020 for each unique incarcerated individual-spell observation. incarcerated individuals sentenced to prison or not observed for three months after their last release are excluded from the recidivism analysis. *Court Delay* is an indicator that equals one if the phrase "Removed from Calendar" ever appeared in the District ROA and zero otherwise. *Design controls* include court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE. *Auxiliary controls* include demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors clustered by incarcerated individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A5: Effects of IGNITE 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)
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), and Minor Misconduct. Panel B reports results for 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. *Months in Jail* is the difference between release date and booking date, and *Months in IGNITE* is defined as the time spent in Jail after September 2020 for each unique spell observation. *Court Delay* is an indicator that equals one if the phrase "Removed from Calendar" ever appeared in the District ROA and zero otherwise. *Design controls* include court division FE (defined based on arresting location of the individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE. *Auxiliary controls* include demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors clustered by individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A6: 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 IGNITE on three-month recidivism by pre- and post-IGNITE. All *Desgin Controls* and *Auxiliary Controls* are included. *Design controls* include court division FE (defined based on arresting location of incarcerated individuals), day of week for the first scheduled hearing, month FE, number of charges, case type FE. *Auxiliary controls* include demographics (gender, age, race), whether booked in past year, whether has a public defender and census tract characteristics (lead exposure, share Black, education attainment, income). Robust standard errors clustered by individual are in parentheses. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A7: Monotonicity Test for Judge IV

	Number of Spline Knots			
	1	2	3	4
	(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 monotonicity proposed by Frandsen, Lefgren and Leslie (2023), computed separately by pre-IGNITE, post-IGNITE, and overall. Test statistics are based on quadratic b-spline estimates of the relationship between recidivism outcomes and judge stringency, with the number of knots specified in each column, controlling for fixed effects for month, day-of-week, and court division.

Appendix Table A8: County Difference-in-Difference 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	40655	40655	40655	40655

Notes: This table reports a regression of 3-month recidivism on the interaction of and indicator for pst-IGNITE and an indicator for Genesee county. Post period is shifted to Jan 2020 and onward. Columns 1 and 2 use the recidivism measured within each county, and columns 3 and 4 use the recidivism measured using both counties. Month FE for seasonality and Year FE are included in all estimates. Control mean is the unconditional mean of the dependent variable for Genesee county in the pre period. Individual characteristics include a set of indicators for Black, female, age groups, and booked in past year.

Appendix Table A9: Social Cost of Crime Effects

	3 Months	6 Months	9 Months	12 Months
	(1)	(2)	(3)	(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 ROA associated with observed future booking 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) to provide the most conservative estimate possible (e.g., we use the cost estimate for assault instead of murder for persons offenses). We then use use the total social cost (sum of frequency of crime * cost crime) and instrument for IGNITE using the usual procedure to produce the estimates. Individuals sentenced to prison or not observed for long enough since their last release are excluded from the analysis.

Appendix Table A10: Community Survey Balance Table by IGNITE Exposure

	Overall Mean (1)	Difference in Mean (2)	Standard Error (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)

Notes: Table reports balance tests for IGNITE Exposure. Column 1 reports unconditional means across the sample, and Columns 2 and 3 report regression coefficients and associated standard errors of respondent characteristics on *IGNITE Exposure*. *IGNITE Exposure* is an indicator for whether the individual was personally or had a friend or family member who was released from Genesee County Jail in September 2020 or later and zero otherwise. Panel A reports results for 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 clustered by incarcerated individual are in parentheses. N = 87. *, **, *** refer to statistical significance at the 10, 5, and 1 percent level, respectively.

Appendix Figure A20: Flint Community Survey Recruitment Flyer

Notes: This figure displays the recruitment flyer used to distribute the Flint Community Survey. The QR Code presented on the flyer links to a personal survey link. The survey concluded with a $25 Walmart Gift Card link for those who completed the survey. The link currently presented on this flyer links to a demo of our survey.

B Data Appendix

B.1 Details on Analysis Sample Construction 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 (`https://micourt.courts.michigan.gov/case-search/`). We scrape records for cases seen in the Sixty-Seventh 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 in Jan 2016-May 2022, we exclude 4% of individuals not residing in Michigan, 1% of individuals with incomplete demographic (specifically, race) information, 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 cases involving 14,794 unique individuals.

Instrument: Court Date Rescheduling 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 as illustrated in Figure A7. We define a specific court event, such as a pre-trial 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.[30] We define our instrument by the presence of any such delays in an individuals' 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 some robustness checks. In another check, we define an 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 length of jail stay, defined either as the difference between their release date and booking date or an the difference between their transfer date and booking date if the individual had 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 of an individual; we use the recent release date in these cases. For a small minority of individuals still in jail in May 2023 (the endpoint of our data), we define jail stay as the difference between May 2023 and their booking date. We determine the reason for release through the Release Checklist file in JMS.

[30] 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 removed events.

To define our measure of IGNITE exposure, we use the program's start date of September 8, 2020. We define an individual's months in IGNITE months as the total amount of time in jail occurring on or after 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 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 from the first incident to the last incident defined by the booking and release dates recorded in the JMS. 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 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 being rebooked in eitherGenesee County Jail or Saginaw County Jail within three-months after release. Recidivism outcomes over longer horizons are constructed similarly.

B.2 Other Administrative Data

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

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

The second dataset consists of individual-level data on jailed individuals who participate in their 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 tests scores and dates. We convert the pre-and-post reading and math scores to their grade-level equivalence using CASAS-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.

GTL Data GTL, recently rebranded as ViaPath, provides phone, tablets, and internet service to the jail. GTL data provides individual-level measures of total phone, app, video visit usage. Variables include number of login or calls an individual has ever made and the total number of logins or minutes used for each GTL service. Apps, accessible to jailed individuals via tablet computers are further disaggregated into education and entertainment. We merge the GTL data by individual identifier to the JMS data.

Other Datasets We use surnames from the merged JMS and ROA files to predict Hispanic identity. 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.[31] We obtain the population shares of Black, High School Graduate or Higher, and the Log Median Household Income from 2016 ACS 5-year Estimates. Individuals not matched to census information are flagged with a binary missing indicator.

B.3 Saginaw County Data

For some of our robustness checks we construct an analogous analysis sample from JMS data for Saginaw County Jail from 2015-2023. 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 date, 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 Tenth Judicial Circuit Court and Seventieth Judicial District Court. We parse the ROAs in a similar as was done for Genesee County. Given that 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.

B.4 Survey Data

Details on Flint Community Survey Instrument The Flint Community Survey was distributed in Flint, MI by four local community between October 2023 and December 2023. Participants, recruited from various locations throughout the the city, including grocery stores, libraries, barbershops, churches, and social services buildings were handed the recruitment flyer, as shown in appendix figure A20.[32] The flyer contained a QR code with a personalized link to the survey. Survey ambassadors were instructed to demonstrate how to access the survey with a phone or tablet using a similar test flyer.

Upon accessing the survey, participants were given a brief introduction to the survey and additional information to decide whether to continue. Those who clicked "I do not agree to participate

[31] Data is available at `https://catalog.data.gov/no/dataset/leadbloodlevels-2017-bytract-20181129-dec8f`.

[32] The Genesee County Flint Community Survey can be accessed in the link below
`https://harvard.az1.qualtrics.com/jfe/form/SV_2fOdxQEsvmAO5U2`.
The The QR Code in Figure A20 also links to the survey.

in this study," were thanked for their time and the survey ended. Participants who clicked "Continue," moved forward to the Background Section of the Survey.

The next set of questions asked the participants about their jail history. We asked participants whether they have ever been booked into jail. If the participant answered "No," they were redirected to the Friend Family Jail 1 section. For participants who clicked "Yes," they continued to the Jail History 1 section and were asked about the month and year they were last booked and released.

Participants who selected a release date that was before 2016 were redirected to the Friend Family Jail 1 section. Participants whose release data was after 2016 continued to the Jail History 2 section and Jail Experience section which inquired about the participant's recent experience in a jail and their activities following release.

Participants redirected to the Friend Family Jail 1 section were further asked whether the participant had a friend or family member who has ever spent time in a local jail. Participants who answered, "No one I know has ever spent time in a local jail," were redirected to the Opinions section. Those who clicked "Yes" to having a friend or family member spending time in jail continued to the Friend Family Jail 2 section, which inquired about the month and year the friend or family member were last booked and released. During the first wave of the survey (October-November 2023) participants who provided a release date prior to 2016 were redirected to the Opinions section.

Participants who provided a friend or family member's jail release date on or after 2016 continue the Friend Family Jail 2 section, which inquire about the jail and post-release experience for the friend or family member.

Those who clicked "Yes" to having a friend or family member spending time in jail continued to the Friend Family Jail 2 section, which inquired about the month and year the friend or family member were last booked and released. Initially, participants who provided a release date prior to 2016 were redirected to the Opinions section.

In the middle of surveying a few changes were introduced to the survey instrument, which we distinguish participants who completed the survey before and after the changes by survey wave. The following changes were made between survey wave 1 (October/November 2023) and survey wave 2 (December 2023).

Respondents who had no personal jail experience but had a family or friend with jail experience prior to 2016 were now allowed to share jail length and answer additional questions beyond their view on law enforcement, as it related to their family or friend. Failure to do so in the first wave explains our drop-off from 101 participants to 87 participants when we interact positive view of law enforcement with jail length.

Respondents who had personal experience with jail prior to 2016 and had no family or friend with jail experience on or after 2016 were now allowed to answer additional questions beyond views on law enforcement, as it related to their own experience. Previously leaving out this group from the remaining questions explains the drop-off from 87 to 62 between law enforcement and other outcomes explored in Table 6 and listed below.

In the Opinion section, all participants who consented to the survey were asked, "To what extent do you disagree or agree with the following statement: Law enforcement looks out for me and my community." And continued to the Demographics section before uncovering a $25 Walmart gift card link for completion of the survey.

Variable definition for Table 6 include the following:

- *Positive views 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."
- *Positive Activities* is an indicator for whether the formerly incarcerated as "Looking for work," "Taking classes," "Working for pay that does not involve crime," and/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?"

C Econometric Appendix

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

Equations (2) and (3) follow by substituting the causal model (1) in to 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 in each line follow from the facts of $Z_i \perp\!\!\!\perp (P_i, Y_i(0), \gamma_i, \beta_i)$ and $M_i^I = M_i^J \times P_i$. Let $M_i^J(z)$ denote 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

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

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 $\omega_i^{Pre} = \frac{M_i^J(1) - M_i^J(0)}{E[M_i^J(1) - M_i^J(0) \mid P_i = 0]}$ and $\omega_i^{Post} = \frac{M_i^I(1) - M_i^I(0)}{E[M_i^I(1) - M_i^I(0) \mid P_i = 1]}$.

C.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., $\mathbf{1}[M_i^I(1) \geq m] - \mathbf{1}[M_i^I(0) \geq m] \geq 0$.

C.3 Identification of ACR Weights and Complier Characteristics

As in Angrist and Imbens (1995), the difference-in-IVs estimand in Appendix C.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 $E[\beta_i(m) \mid M_i^I(1) \geq m > M_i^I(0), P_i = 1]$, the complier-average effect for margin m, are 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. Leting 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.