Release, Detain, or Surveil?

The Effect of Electronic Monitoring on Defendant Outcomes

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Abstract

This paper studies the effect of pretrial electronic monitoring (EM) relative to both pretrial release and pretrial detention (jail). EM often involves a defendant wearing an electronic bracelet, which aims to reduce pretrial misconduct at a low cost. Using the quasi-random assignment of bond court judges, I estimate the effect of EM versus release and EM versus detention on pretrial misconduct, case outcomes, future recidivism, and aggregate total costs. Results indicate that EM reduces overall costs relative to detention. However, EM does not prevent enough high-cost crime to justify its use relative to release.

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1 Introduction

Electronic monitoring (EM) is an increasingly popular tool to surveil presumed innocent criminal defendants instead of releasing them or detaining them in jail. EM generally involves an ankle bracelet to track and restrict the defendant's movement, intending to deter pretrial misconduct. Its popularity rose following recent concerns of jail overcrowding amplified due to COVID-19, the recent pushes to reduce pretrial detention more broadly, and the rise in crime in major U.S. cities. Advocates of EM argue that it offers the best of both worlds: the low cost of pretrial release and the low crime of pretrial detention. Critics fear EM's expansion could lead to worse outcomes, particularly when used on defendants who would otherwise be released. Furthermore, though promoted as a replacement for pretrial detention, EM is a middle option between release and detention, and it allows judges to place would-be released defendants onto EM. In this paper, I provide novel evidence on the effect of pretrial EM relative to both pretrial release and detention.

To assess EM's efficacy as an alternative to pretrial release or detention, we need to examine various outcomes. These include pre- and post-trial misconduct, case outcomes, sentencing, and the direct cost of using EM or jail for defendants. The size and direction of EM's effects compared with each treatment is generally unclear. For example, EM could reduce pretrial misconduct relative to release through deterrence, as a defendant's movement is tracked and restricted. However, it may also increase new charges against defendants due to violations of its strict requirements. Furthermore, compared with detention, it is unclear if EM's deterrent effect will outweigh detention's crime prevention through incapacitating defendants in jail. Regarding case outcomes, there are concerns about EM's potential coerciveness. It might enable prosecutors to pressure defendants into guilty pleas, similar to the current dynamics with detention. After the case concludes, if EM damages employment or social ties, it may increase future criminal activity. Finally, while EM is cheaper than detention in direct costs (around \$15 per day compared with \$150), it is more expensive than release, particularly when cases last multiple months on average.

Despite the expansion of EM in recent years, we have little evidence for its effects or effectiveness in a U.S. or pretrial context. Existing evidence on EM focuses solely on the effects of EM relative to incarceration in contexts outside the U.S. (Di Tella and Schargrodsky (2013), Henneguelle, Monnery, and Kensey (2016), Williams and Weatherburn (2022), Grenet, Grönqvist, and Niknami (2024)), and recidivism is generally the primary outcome of interest. Beyond the significant differences between the U.S. and other criminal justice systems, these studies generally focus on post-trial EM and thus only apply to defendants who are found guilty.[1] However, large shares of pretrial defendants in the U.S. are not found guilty, and post-trial studies cannot speak to the effect of EM compared with release. Furthermore, post-trial recidivism is only one of the many costs that must be considered when evaluating pretrial EM.

Such evidence is difficult to obtain for two main reasons. First, EM's expansion in the U.S. is relatively recent, and criminal proceedings in the U.S. can last many months, if not years. As a result, data with sufficient numbers of defendants on EM is rare. Additionally, data with long time horizons are needed so that essential outcomes, such as case outcomes and post-trial recidivism, can be observed. Second, defendants are not randomly assigned to pretrial 'treatments' (release, EM, or detention), making identifying causal effects difficult. Such identification challenges are compounded by EM's status as a middle treatment, meaning there are multiple treatment effects of interest (i.e., EM vs. release and EM vs. detention) and individual pairwise treatment effects are more difficult to isolate. To overcome the first challenge, I use a novel dataset from Cook County, Illinois, one of the largest bond courts in the U.S. and an early mass adopter of pretrial electronic monitoring in 2013. The data contains a large population of felony defendants on release, EM, and detention, which I can

[1] Grenet, Grönqvist, and Niknami (2024) studies effects of EM relative to prison on labor supply, children's education, and other outcomes in Sweden, in addition to criminal recidivism. Note that Di Tella and Schargrodsky (2013) studies a pretrial usage of EM in Argentina, but this context is significantly different from the pretrial environment in the U.S. as noted in their paper. For example, the average duration on EM is 420 days, around four times the average case duration in this paper, and there is effectively no presumption of innocence with very high guilty rates. In contrast, around 50% of defendants in this paper's environment are found guilty of a felony charge.

follow for multiple years after their cases conclude. Consistent with critics' concerns over EM's application to low-level defendants, judges used EM on both would-be released and would-be detained defendants in Cook County, despite its initial expansion intended to reduce jail overcrowding.

To overcome the identification challenge, I leverage the quasi-random assignment of bond court judges to cases to instrument for a defendant's pretrial treatment. I begin with a traditional instrumental variables approach and recover the two-stage least squares (2SLS) estimates for the effects of EM relative to release and detention. With few exceptions, I cannot reject that treatment effects are constant, suggesting that differences across judges are not producing differential selection on treatment effects. Crucially, this means that 2SLS likely recovers causal estimates of the effects of EM versus release and EM versus detention despite the multiple margins of treatment. Relative to release, 2SLS estimates indicate that EM is an effective deterrent against pretrial misconduct, reducing failures to appear in court and new cases pretrial by 11 and 7.4 percentage points (pp), respectively. However, I find suggestive evidence that EM increases the likelihood of being sentenced to incarceration and the number of new felony cases after the case ends. I also find that the reduction in pretrial misconduct is driven by relatively low-cost offenses, meaning I am unable to reject no effect of EM on pretrial crime costs relative to release. In aggregate and after factoring the larger direct costs of operating EM, I find that EM does not significantly reduce total costs relative to release.

In contrast, I find evidence that EM reduces total costs compared with detention. This is largely driven by EM being around ten times cheaper per day than detention, resulting in thousands of dollars in savings over the pre-trial period. To outweigh the large expense of detention compared with EM and make detention cost-effective, it would have to prevent significant amounts of costly pretrial crime or significantly improve other outcomes. I do find that EM is less effective at preventing pretrial misconduct than detention, as EM increases the likelihood of a failure to appear and a new case pre-trial by 6.1pp and 8.5pp, respectively.

However, this all proves to be driven by low-cost offenses, resulting in small losses through increased misconduct. Additionally, EM has no effect on being sentenced to incarceration relative to detention and a negative, but not statistically significant effect, on post-trial crime costs. After aggregating costs, 2SLS estimates indicate that moving a defendant from detention to EM would save around \$8,200 in marginal costs, or around 20% of the average marginal cost for detained defendants, driven by the large savings in direct costs and small effects on other margins.

Finally, I compare these estimates to the effects of EM's expansion in July of 2013. Using time-series variation, I recover estimates for the total effect of EM being introduced and decompose this change into the effect on the defendants who would have been released and detained if not for EM, under an assumption of stationary average outcomes. Despite being recovered from an entirely different source of variation, these pre-post estimates for defendants moved from release to EM and from detention to EM are consistent with the 2SLS results. In the preferred specification, I find the introduction of EM likely saved between \$250 and \$1,340 per defendant in the sample, and, consistent with the 2SLS results, I find this is driven entirely by savings from moving would-be-detained defendants onto EM of between \$9,170 to \$10,850 per defendant. On net, the expansion of EM likely resulted in net savings, driven primarily by reducing costs from moving would-be detained defendants onto EM and reducing costly time in jail.

This paper contributes to our understanding of the economics of pretrial detention and surveillance. Existing work on EM in economics studies non-U.S. contexts and has focused on the effect of EM relative to incarceration, with recidivism being the primary outcome of interest (Di Tella and Schargrodsky (2013), Henneguelle, Monnery, and Kensey (2016), Williams and Weatherburn (2022), Grenet, Grönqvist, and Niknami (2024)).[2] This paper advances our understanding of how EM influences defendant outcomes relative to release and detention on pretrial misconduct, case outcomes, and recidivism in a large U.S.

[2]Additional work on EM includes Marie (2008), Ouss (2013), and Andersen and Andersen (2014). See Belur et al. (2020) for a review of the interdisciplinary literature on EM.

municipality, informing policy on the impact of using EM instead of release and detention for marginal defendants. This paper also complements the literatures studying criminal justice environments with multiple treatments (Mueller-Smith (2015), Bhuller et al. (2020), Arteaga (2023), Norris, Pecenco, and Weaver (2021), Rose and Shem-Tov (2021), Humphries et al. (2024), Kamat, Norris, and Pecenco (2023)) and pretrial detention and its consequences in the U.S. (Gupta, Hansman, and Frenchman (2016), Leslie and Pope (2017), Dobbie, Goldin, and Yang (2018), Stevenson (2018)).

This work also builds on the economics of modern surveillance technology by studying the rise of individualized surveillance used to deter violations, such as body-worn cameras on police used to deter misconduct (Lum et al. (2020)) or remote-work monitoring technologies (Jensen et al. (2020)). In contrast, much prior work focuses on the economics of mass surveillance technologies (Tirole (2021)). I find that individualized surveillance technology, such as EM, can be a beneficial and effective alternative to more costly policies.

This article proceeds as follows. Section 2 discusses the institutional background, data, and the potential costs and benefits of each treatment. Sections 3 present the empirical strategy, results, and robustness tests for the 2SLS analysis. Section 4 presents additional estimates using a time-series design. Section 5 concludes.

2 Background and Data

2.1 Cook County Bond Court, Bond Types, and Treatments

Following an arrest in Cook County, a defendant is taken into custody and booked based on their arrest charges, generally at the Cook County Jail. Then they are arraigned at bond court, usually within one day of the arrest. Until the resolution of their case, which is referred to as the "pretrial" period (though cases often do not go to trial), the defendant is presumed innocent. The following provides background on the bond court and procedures, while Appendix A.1 provides additional information.

This paper focuses on the central bond court in Cook County, known as Branch 1, and the sample of felony cases which were seen by bond court judges between July of 2013 and May of 2017. Branch 1 operates every day and handles almost all felony cases in Cook County, excluding murder and violent sex offenses, and some non-felony cases misdemeanor cases. At bond court, the sitting bond court judge determines the bond conditions for a defendant, namely bond type and amount.

A single bond court judge handles all cases which pass through Branch 1 on any given day. The judges have an irregular working schedule (Tardy et al. (2014)), which depends on their off days, vacations, and work-day preferences — Figure 1 displays a sample of the calendar from the data. There are relatively few active bond court judges within a given month (≈ 4), and only seven were highly active during the period of study, accounting for $> 95\%$ of observations. Defendants do not have discretion over their assigned judge, and judges cannot choose the cases they see on a given day. Because Branch 1 is always active (including holidays and weekends) and the schedule of each bond court judge is sporadic, there is not a scheduling relationship between bond court judges and prosecutors, public defenders, or trial judges.

During the period of study, judges had three main bond types which they could assign to defendants at their discretion: D-bonds, I-bonds, and IEM bonds. A D-bond is the most common bond (54% of Branch 1 bonds) and conforms to the popular understanding of bonds: the defendant remains in jail until they pay 10% of the bond amount, which can range from below \$50 to over \$200,000. The defendant can pay the amount at any point during their pretrial period and be released from jail, or they are detained in jail until they pay or their case concludes. On the other hand, I-bonds (16%), or "release on recognizance" bonds allow the defendant to be released from jail without posting any money.

Similar to I-bonds, IEM bonds (29%) allow defendants to leave jail at no cost, but they are placed on electronic monitoring (EM). The defendant can be released off of EM if they pay 10% of the bond amount (like a D-bond). IEM bonds were introduced in June 2013 in the wake of a jail-overcrowding crisis and conflict between the court and the sheriff's office.[3]

Despite their introduction to reduce jail overcrowding, IEM bonds were used as a middle option between release and detention and applied to defendants who would otherwise have been released as well as those who would otherwise have been detained. Though no official guidance on EM as the 'middle' option existed when IEM was introduced, the 2016 "Decision Making Framework" in Cook County explicitly places EM between release and detention (CGL and Appleseed (2022)). Comparing judge behavior during the sample period with their behavior prior to IEM's introduction, it is evident that EM was used as a middle option. Figure 2 shows that with the introduction of the IEM bond, mainly mid-level D-bonds were replaced by IEM bonds. To demonstrate this more directly, I use data from 2009-2012 to predict defendants' probability of being detained based on their observables in the pre- and post-EM periods. Figure B.1 displays the distribution of predicted probability of detention by defendant treatment, with the distribution of EM defendants' predicted detention probabilities clearly between those of released and detained defendants.

While electronic monitoring (EM) can refer to various technologies, all operate as electronic individualized surveillance systems with a similar mechanism and purpose. In this paper, I study the EM program run by the Cook County Sheriff's Office between 2013 and 2015: defendants always wear a radio-frequency ankle bracelet. EM was used to ensure defendants did not leave their homes except at pre-approved dates and times (e.g., a specific work or education schedule) or for a small set of "one-time movement" conditions. This system is often referred to as EM coupled with house arrest, and it is common across the United States (Weisburd et al. (2021)). The ankle bracelet communicates with a monitoring unit installed in the defendant's home, which informs the sheriff's office of out-of-bounds movement and tampering.

Defendants on EM agree to warrantless searches of their residence while on EM, and the

[3]EM can be coupled with a D-bond (D-EM) such that defendants were in jail until they paid 10% of the bond amount and were released onto EM. Judges also can but rarely do deny bond (1.5%) if they determine the defendant is a flight risk or a threat to the public.

possession of firearms, drugs, or contraband is a violation of EM bond conditions (Rogers (2022)). As stated by the sheriff's office's EM information sheet, violations or noncompliance with the terms carry the "risk of criminal prosecution and re-incarceration," and damaged or missing equipment can be charged as felony theft (CCSO (2020a)). Similar to other bail conditions of release (e.g., a condition of all bond releases is that the defendant appears in court), violations of the EM bond requirements can lead to re-incarceration in jail as well as additional charges.

I map these bond types and release statuses into three pretrial treatments. At the lowest level, defendants can be "released" if they fully exit the sheriff's office's custody through an I-bond, or because they were given an IEM or D-bond but paid the bond amount and were released within 7 days. The middle level is being on "EM", meaning the defendant was assigned an IEM bond but was not recorded as being released from it (e.g., paid the 10% amount) within 7 days. At the highest level, the defendant can be "detained" if they are given a D-bond and are not released from jail within 7 days. For the main results, the cutoff is 7 days, which is the 44th percentile for the duration in jail/EM, but 75% of releases happen within 3 days for D- and IEM bonds. This cutoff is largely arbitrary, and I construct robustness checks using alternative cutoffs. In particular, using the common 3-day cutoff yields similar results. Notably, around 29% of defendants classified as 'detained' are eventually released before their case ends, though this number is comparable to that of other work, such as Dobbie, Goldin, and Yang (2018). Figure B.2 displays the flows from bond types to treatments.

After the bond hearing, all defendants have the opportunity to plead guilty or proceed with the case (which can involve a future plea). Prior to the case ending, through a trial, dismissal, plea, or being dropped, defendants can be rearrested or charged with new crimes and fail to appear in court. Lengthy cases are common, and trials are rare, with 94% of cases with a guilty outcome involving a guilty plea. If guilty, the defendant can be sentenced to pay a fine, time served, or incarceration in prison or in Cook County Jail. Following the case outcome and subsequent incarceration period, defendants can be rearrested or have a new case against them (a new case post-trial).

2.2 Data and Summary Statistics

2.2.1 Data

The main data for this study comes from the Circuit Court of Cook County and Cook County Jail. The court data contains information on defendants, cases, charge counts, and outcomes for almost all Cook County court cases between 1984 and 2019. This allows me to follow cases from inception to bond court, individual motions and case events, through to final case outcomes (e.g., a defendant demanding a trial, a guilty plea on a specific charge but not others, and a sentence to time served). I connect defendants across cases, allowing me to observe extensive criminal and case histories across tens of thousands of individuals over more than three decades, including past and future cases, arrests, guilty verdicts, charges, sentencing, and pretrial misconduct. I link the court data to data from the Cook County Jail, maintained by the Cook County Sheriff's Office. This jail data, spanning from 2000 to 2017, contains information on individuals' detention spells, intake, and release. I also link this data to Chicago Police Department arrest data. See Rivera (2024) for replication data and code.

This study focuses on the nearly four years between July 2013 to May 2017 in which EM was one of the most common pretrial treatments for defendants. I focus on adult cases with felony charges that went through Branch 1. The data is summarized at the booking and defendant level — which I will refer to as a "case" for simplicity. Linking these cases to jail spells leaves some cases with missing jail information. In the main analysis, I code missing releases for I-bonds as immediate releases, missing EM bonds as EM, and drop missing D-bonds, but I test the robustness of my results to these codings in Appendix A.3. I restrict the sample to cases that do not involve murder or felony sexual assault charges (in the current case) and have categorizable pretrial treatments. Notably, the filters remove misdemeanor-only cases (43,949), cases with missing release status (4,707), and cases assigned

D-EM bonds (6,722), which are utilized in Appendix A.3. The final felony sample contains 65,430 defendant-bookings.[4] See Appendix A.2 for more details on the data construction and filtering.

2.2.2 Summary Statistics

Table 1 summarizes characteristics and outcomes for felony defendants within the sample. The majority of defendants are non-white, with 75% Black and 12% Hispanic, despite Cook County being over 40% white. Defendants are also overwhelmingly (85%) male and are 34 years old on average. Most defendants have some form of drug charge, 52% for possession and 19% have a charge for delivery, while 16% have property charges, 10% have weapon charges, and 7% have violent charges — charges by their 'class' (X being most serious followed by 1 through 4) are also displayed. The average defendant has had around 2.7 past cases in Branch 1 but averages 0.63 prior guilty felony charges. There is clear heterogeneity in defendant characteristics across treatments, with more severe pretrial treatments corresponding to higher shares of Black, male, and younger defendants, who are more likely to be charged with serious felonies (e.g., for violent crimes and Class X or 1) and have worse case histories in the form of more past guilty felonies and prior cases.

Outcomes also differ across treatments. While the median case lasts around four months, the median detained defendant's case is about six months, and similarly detained defendants spend around half that time in jail (the median being 84 days), compared with 24 days for EM and 0 days for release defendants. Release defendants fail to appear in court (14%) significantly more than EM (8%) or detained defendants (3%). However, EM defendants are most likely to receive a new case pretrial (19%) compared with release (18%) and detain (11%). While only around 37% of defendants are sentenced to incarceration, this is much higher for detained defendants (62%) compared with EM (31%), while it is rare for release

[4] D-EM bonds are removed from the main sample because the jail data does not clearly distinguish between being released from jail (detention) and being released from the sheriff's custody. With IEM and D bonds, the end of one's time in jail can be easily mapped to an exit from EM or detention, while with D-EM bonds it is less clear, and more assumptions are made to map defendants with D-EM bonds to outcomes.

(14%). However, EM and detained defendants receive a new felony case within three years post-trial at similar rates (32% and 29%), while release defendants are much lower at 21%.

2.3 Framing Costs and Benefits of EM

This section constructs estimates for the total social costs associated with defendant outcomes by aggregating costs across the many outcomes observed. This includes both direct costs (e.g., operating expenses for detention or EM) and indirect costs (e.g., crime and misconduct, punishment, and post-trial recidivism). This approach allows us to study the effect of EM relative to release and detention on total costs and help us determine whether EM is a cost-savings pre-trial treatment in a more comprehensive comparison.

To quantify total costs, I use the cost equation below ((1)) which sums over multiple dimensions of defendant outcomes: the direct costs of each treatment type, the costs of pretrial misconduct and crime, the costs of post-trial incarceration, and the costs of post-trial criminal behavior. Additionally, while difficult to measure, personal and financial costs borne by defendants are also considered. The change in marginal total costs from moving a defendant from release or detention to EM is:

$$
\begin{aligned}
\Delta \text{Total Cost}^{EM,R/D} = \text{Total Cost}^{EM} - \text{Total Cost}^{R/D} = \\
\underbrace{\Delta[\text{Direct Cost per Day} \times \text{Duration}]}_{\text{Cost per Day=\$150}(Jail)\text{,\$15}(EM)\text{,\$0}(Release)\times\text{40\%(Marginal Cost)}} \\
+ \underbrace{\Delta\text{Failure to Appear}}_{\text{\$1,000 per FTA}} \\
+ \Delta\text{Cost of Crime Committed Pre-Trial} \\
+ \quad \underbrace{\Delta\text{Cost of Punishment (Sentencing)}}_{\text{\$20,000 per Incarceration (about 60\% of 1 Year in Prison)}} \\
+ \Delta\text{Cost of Crime Committed Post-Trial} \\
+ \Delta\text{Personal/Financial Costs}
\end{aligned}
\tag{1}
$$

Direct Costs Direct costs amount to operating each treatment type. Jails cost about \$100-\$200 per detainee per day to maintain.[5] While pretrial EM is significantly cheaper than detention, monitoring defendants is still costly: about \$15 per defendant-day in Cook County in 2021 (CGL and Appleseed (2022)). By comparison, release is almost costless. While these are average costs, I consider marginal costs likely around 20% and 60% of average costs (Wilson and Lemoine (2022)).[6] As the average defendant is in jail or on EM for over 100 days, the marginal direct cost of jail is between \$3,000 and \$8,000 greater than EM.

Pretrial Crime and Misconduct During the pretrial defendant may commit a crime, which incurs social costs depending on its type (e.g., petty theft is less costly than murder), and they may commit low-level misconduct such as failing to appear in court. Pretrial detention should reduce pretrial crime costs by incapacitating defendants in jail compared with EM and release. EM should prevent pretrial misconduct through deterrence (the defendant is being surveilled) and partial incapacitation when they are at home.

I estimate changes in crime cost as the effect of EM versus release and detention on the incidence cost of new cases and misconduct (e.g., failure to appear in court) defendants are charged with after bond court. In order to quantify this intensive margin of defendants' recidivism and avoid common issues with binary measures of recidivism (Rosenfeld and Grigg (2022)), I quantify the social costs of different crimes by supplementing my data with the crime cost information from Miller et al. (2021).[7] This measurement captures the intensive margin of recidivism in both the number of new cases as well as the intensity (social costs) of the associated crimes. However, because the cost of a single murder is so high (almost \$8 million dollars, which is equivalent to about 200 police-reported robberies), I also conduct

[5]For example, in 2011, Cook County Jail cost about \$90 per defendant-day, while in 2021 it cost \$223 per defendant-day (Vera Institute (2022)).

[6]Marginal costs are more difficult to calculate. Wilson and Lemoine (2022) find that incarceration's short-run and long-run marginal costs are often around 20% and 60% of average costs. Thus, per defendant-day in jail, the average cost is around \$150, while the short-run and long-run marginal costs are around \$30 and \$90.

[7]I use Table 5 from Miller et al. (2021), which contains the total tangible and quality of life costs associated with different crimes, to guide the construction crime costs for charges against defendants in the data as well as imputing costs for some unlisted crimes. A similar method is used in Mello (2019) to compute cost-weighted crimes per capita. As the crime codes in this data do not match those of Miller et al. (2021) perfectly, Table B.1 displays how coded charge types were mapped to incidence costs.

analyses using a "low" murder cost which is the preferred specification, making it comparable to a police-reported rape (about $400,000) to determine if outlier defendants accused of murder in future cases are driving the results, inspired by a similar procedure in Heckman et al. (2010).[8] I also value a case with any failure to appear at $1,000, which is in line with the cost used in Dobbie, Goldin, and Yang (2018) ($1,185).

Punishment Costs Punishment costs are the costs associated with defendants being found guilty and incarcerated in the post-trial period. Because almost all convictions are done through plea deals, more severe pretrial treatments can be 'coercive' and increase the likelihood of being found guilty and sentenced to incarceration.[9] While EM is likely less coercive than detention, it is likely more coercive than release. However, EM also has the potential defendants being rearrested due to violations of their conditions, which may increase the likelihood of a guilty plea on the original charges. For punishment costs, I assume a marginal cost of $20,000 per defendant sentenced to incarceration, which is conservative as that is around 60% of the average cost of a single year in prison (around $33,000 per inmate in Illinois in 2015 (Mai and Subramanian (2017))).

Post-Trial Crime Costs Post-trial criminal behavior can be influenced by pretrial treatments through multiple channels. If more stringent treatments (i.e., EM vs. Release and Detention vs. EM) are more coercive, then they will lead to more incarceration. This will incapacitate the defendant post-trial, resulting in an incapacitation effect and less post-trial crime. However, pretrial treatments may be differentially criminogenic (increasing the likelihood of criminal activity), resulting in changes in post-trial crime costs. For example, spending time in jail may reduce future economic opportunity and raise criminal capital (Bayer, Hjalmarsson, and Pozen (2009), Stevenson (2017)), and detention and higher bail have been shown to be criminogenic relative to release (Leslie and Pope (2017), Gupta, Hansman, and Frenchman (2016)). EM may be criminogenic as well, as interviews with participants note

[8] For reference, only 31 defendants have pretrial crime costs higher than 1 million dollars (almost entirely driven by murder charges) and 179 defendants in the 3-year post-trial period.

[9] See Leslie and Pope (2017), Stevenson (2018), and Dobbie, Goldin, and Yang (2018). Bargaining power is a significant factor in case outcomes (Silveira (2017)).

damaging social ties, economic opportunities, and health outcomes and increasing housing insecurity (CGL and Appleseed (2022)). Alternatively, receiving a more severe treatment may reduce future crime through an individual deterrent effect (increasing defendants' expectations of pretrial punishment) — though the research on EM versus incarceration has not found evidence consistent with such a mechanism (Williams and Weatherburn (2022)). Which of these effects dominates is not clear ex-ante. To assess these costs, I apply the same crime-cost weights discussed above to charges of new cases within three years post-trail.

Personal/Financial Costs Finally, there are indirect costs on the defendant, including personal suffering, damaged employment and future earnings, and social and health damages, which I cannot measure. These costs are likely to be large, as Dobbie, Goldin, and Yang (2018) estimates the marginal defendant (between release and detention) loses around $30,000 in lifetime earnings if detained pretrial. While I cannot quantify the personal or financial costs, they are likely larger for higher levels of treatment, i.e., $\Delta\text{Personal/Financial Costs}^{EM,R} >> 0$ and $\Delta\text{Personal/Financial Costs}^{EM,D} << 0$. As these costs are not included in total cost calculations, the comparisons are likely biased in favor of more stringent treatments.

3 Effect of EM versus Release and Detention

3.1 Empirical Strategy

To estimate the causal effect of EM relative to pretrial release and detention, I instrument for endogenous pretrial treatments using the quasi-random assignment of bond court judges to cases. Because judges are assigned to cases based on the bond court schedule and the date of a defendant's bond court hearing cannot be manipulated, the specific judge for a case is exogenous with respect to the defendant's observable and unobservable characteristics. Under these and additional assumptions discussed below, judge assignment can be used as a valid instrument for pretrial treatment. The primary specification used in this section is:

$$Y_{ijc} = \beta^R Release_{ijc} + \beta^D Detain_{ijc} + \theta X_{ijc} + \epsilon_{ijc} \tag{2}$$

$$\begin{aligned} Release_{ijc} &= \sum_j \alpha_j^R 1\{\text{Judge} = j\} + \lambda^R X_{ijc} + e_{ijc}^R \\ Detain_{ijc} &= \sum_j \alpha_j^D 1\{\text{Judge} = j\} + \lambda^D X_{ijc} + e_{ijc}^D \end{aligned} \tag{3}$$

where Y_{ijc} is the outcome of interest for defendant i who is assigned to judge j in case c, X_{ijc} are a vector of controls, and $Release_{ijc}$ and $Detain_{ijc}$ are indicators for the defendant receiving endogenous treatments EM or detention, respectively. α_j^R and α_j^D are the judge-specific effects (judge fixed effects) for placing a defendant on release and detention, respectively. β^R and β^D are the causal effects of being assigned to release or detention relative to EM. I use two-stage least squares (2SLS) to recover estimates of both parameters. For the principal 2SLS analysis, X_{ijc} includes year-quarter and day-of-week fixed effects.

3.1.1 Exogeneity and Exclusion

To be a valid instrument, the assigned judge should be unrelated to the defendant's unobservable characteristics, which would influence their outcomes. This exogeneity assumption is satisfied if judges are effectively randomly assigned to cases, which is likely given the calendar system judges operate on and their inability to choose which cases they see (see Figure 1). This assumption implies that defendant observables and unobservables should be uncorrelated with the assigned judge, and the relationship between observables and judges can be empirically tested.

I begin by regressing defendant treatment on their observables, controlling for year-quarter and day-of-week fixed effects. In Table 2, Columns (1)-(3) show clear evidence of selection on observables, as defendant characteristics have economically large and statistically significant relationships with which treatment they are assigned to.

Next, I test for whether specific judges are systematically seeing different types of cases by regressing a defendant's characteristic on judge fixed effects (controlling for year-quarter and day-of-week fixed effects), then testing the joint nullity of the judge effects with a Wald test. Columns (4) and (5) show the results, with all tests failing to reject the null of joint zero judge effects across every characteristic, indicating judges are not seeing different types of cases, consistent with the exogeneity assumption. Note that Column (4) (and Columns (1)-(3)) use the sample of felony cases for which treatments can be defined (i.e., include D-EM bonds) and Column (5) repeats the exercise in Column (4) using the entire felony case sample, even cases for which treatments cannot be defined, in order to avoid filtering cases based on post-bond court factors which judges may influence.[10]

Next, if the judge influences defendant outcomes through means other than their pretrial treatment, then this violates the exclusion restriction. Fortunately, Branch 1 operates on an entirely separate schedule from the other elements of the Cook County court system, and bond court judges do not play a role in future portions of the case.[11] While bond court judges may not directly influence the case at a later stage, the discretization of treatment assignments obscures the fact that judge's make a multidimensional decision, assigning both bond type and bond amount, which may violate the exclusion restriction. Section 3.3.1 provides additional analyses to address these concerns.

3.1.2 First Stage

To identify the causal effects of EM relative to release and detention, judges must have a sufficiently strong influence over the treatment of their assigned defendants, satisfying 2SLS's relevance assumption. I apply two primary tests for this. First, I estimate judge-specific effects for each assignment in equation (3) (α_j^R, α_j^D) and compute the variance of the fixed effects. To ensure the variance is not overstated due to measurement error, I apply the correction from Kline, Saggio, and Sølvsten (2020), though I find the bias is relatively small,

[10] While the main analysis sample excludes D-EM bonds due to difficulty making a direct mapping between court and jail data to pretrial treatment, their inclusion in the sample, as shown in Section 3.3, does not significantly change the main results.

[11] In the sample, 17% of defendants see the same bond court judge in a future Branch 1 case, which is expected given high recidivism and few judges, though whether or not a judge remembers an individual they saw among dozens a year ago is not clear.

consistent with the large amount of observations per judge. As shown in Panel A of Table 3, 1 standard deviation in judge effects increases the likelihood of being assigned to release and detention by 7.1pp and 10pp, respectively, equivalent to 24.5% and 29% of their respective means. Variance across judges constitutes 2.4% and 4.8% of the variation in assignments to release and detention. Second, a Wald test strongly rejects the null hypothesis of zero effect across judges for both treatments.

Because there are two endogenous variables, we must also ensure judge effects are not actually weak instruments when applied to both treatments simultaneously. For this, I apply the Sanderson and Windmeijer (2016) weak instruments test for multiple endogenous variables, which builds on builds on the Angrist-Pischke partial F-statistic (Angrist and Pischke (2009)). As shown in Panel B of Table 3, both the Sanderson-Windmeijer and Angrist-Pischke F-statistics are large (> 40) and the tests strongly reject that judge fixed effects are weak instruments for release and detention.

I begin by assuming that treatment effects are constant, and I make no assumption on the ordered nature of the treatments. Under these assumptions, the 2SLS estimates of β^{EM} and β^{D} can be interpreted as the causal effect of EM compared with release and detention compared with release. In Section 3.3, I test the validity of these assumptions and provide additional estimates.

3.2 Results

Table 4 displays estimates of the effect of being assigned to EM relative to release ($-\beta^{R}$) and the effect of being assigned to detention relative to EM ($-\beta^{D}$). Panel A displays results for simple OLS (equation (2)), and Panel B displays 2SLS results using judge fixed effects in the first stage (equation (3)). Columns correspond to different outcomes. Because judge assignment is determined by the day a defendant is in bond court, I cluster standard errors in the main specifications at the bond court date level (Abadie et al. (2022), Chyn, Frandsen, and Leslie (2024)), and Figure B.3 displays results using alternate clusters.

3.2.1 EM vs. Release

Based on the 2SLS estimates, compared to release, EM decreases the likelihood of a failure to appear in court (FTA) by -10.6pp ($p<0.01$) and the likelihood of a new case pre-trial by -7.4pp ($p=0.06$), and it has an imprecise negative effect on cases for violations (-2.1pp ($p>0.1$)). The OLS estimates understate these effects on FTAs and new cases (-5.7 ($p<0.01$) and 1.8 ($p<0.01$)), consistent with positive selection into release relative to EM. EM's prevention of failures to appear is potentially due to reminder effects, consistent with the effect of reminder text messages (Fishbane, Ouss, and Shah (2020)). Weighting new cases by incidence costs, the 2SLS results suggest that EM reduces pre-trial crime costs by -\$6,776 (95% CI=[-45,415 , 31,862]) but is very imprecise. Part of this imprecision is due to the very high cost of murder, 20x more costly than the next highest crime. Using the low murder cost produces estimates that are one-third the size and much more precise (-\$2,880 (95% CI=[-6,514 , 754])). Overall, the primary effect of EM relative to release is to reduce pretrial misconduct on the intensive margin — whether there is any detected misconduct — rather than on the extensive margin — the amount and intensity of the misconduct. Additionally, the results for crime costs are biased towards EM being more costly since it may increase the likelihood defendants' offenses are detected compared with release.

For case outcomes, while OLS results suggest EM significantly increases the likelihood of being sentenced to post-trial incarceration by 18.3pp ($p<0.01$), 2SLS results are much smaller though still positive (9.6pp ($p=0.08$)). Because release is significantly cheaper than EM, 2SLS estimates indicate that using EM costs about \$1,120 ($p>0.1$) per defendant in direct costs, though not statistically significant. Post-trial, EM produces a small positive effect on total new felony cases within three years (0.15 ($p=0.09$)), but a noisy negative effect on total post-trial new case costs within three years using full murder costs (-\$41,039 ($p>0.1$)) and virtually no effect using low murder costs (-\$1,207 ($p>0.1$)).

Using the cost weights in (1), I aggregate costs to determine if EM reduces total marginal costs compared with release. While OLS suggests that EM results in significantly higher total costs, \$9,259 ($p<0.05$) per defendant, 2SLS estimates indicate that EM reduces costs compared with release though estimates are imprecise and cannot reject no savings (-\$42,628 (95% CI=[-140,254 , 54,998])). Using the low murder cost, the magnitude of EM's savings is much smaller but still highly imprecise (-\$1,510 (95% CI=[-10,345 , 7,325])). Collectively, these results suggest that while release allows for more pretrial misconduct than EM, the effects are simply not large enough to make EM a clearly superior option after accounting for its effects on other outcomes. As a result, aggregating total costs indicates that EM does not significantly reduce total costs relative to release. However, OLS results significantly overstate release's benefits because of positive selection of defendants into release.

3.2.2 EM vs. Detention

The second row in each panel contains estimates of moving a defendant from detention to EM ($-\beta^D$). In the pretrial period, EM allows for more pretrial misconduct than detention, with 2SLS estimates indicating that EM increases the likelihood of an FTA by 6.1pp ($p<0.01$) and the likelihood of a new case pre-trial by 8.5pp ($p<0.01$) compared with detention, and this includes a 4.1pp ($p<0.01$) increase in the likelihood of a new case for a violation. These effects are actually quite close to those recovered from OLS. The increase in misconduct is, however, on the extensive rather than intensive margin, as EM has noisy and very small negative effects on new case cost using full and low murder costs (-\$12,706 ($p>0.1$) and -\$316 ($p>0.1$)).

For case outcomes, EM does not significantly reduce the likelihood of being sentenced to incarceration compared with detention, though negative selection into detention produces a large negative OLS estimate (-30.3pp ($p<0.01$). Primarily due to detention's significantly higher costs, 2SLS estimates indicate that EM reduces direct costs by -\$13,790 ($p<0.01$) per defendant compared with detention. EM appears to have a positive effect on post-trial crimes, increasing the number of felony cases over three years post-trial by 0.04 ($p>0.1$). However, using the cost of post-trial crime indicates that EM slightly reduces the intensity of recidivism, suggesting detention has a criminogenic effect compared to EM, though estimates are noisy using both full and low murder costs (-\$51,141 ($p=0.12$) and -\$3,517 ($p>0.1$)).

Aggregating costs using (1), I find that EM significantly reduces total costs compared with detention using both full, -\$65,778 ($p=0.05$), and low murder costs, -\$8,234 ($p<0.01$). The smaller and more precise low murder results reject total marginal costs savings of less than $-\$2{,}300$ per defendant for EM vs. detention. Overall, these results indicate that though EM allows for more pretrial misconduct, it is significantly less costly due to the sizable direct cost of detention, in addition to small effects on the intensity of pre-trial and post-trial recidivism. For detention to be less costly than EM, placing a defendant on EM would have to incur thousands of dollars in other costs on average, and I do not find this to be the case.

While large, these effect sizes are consistent with prior literature studying criminal justice environments in the U.S. For example, Dobbie, Goldin, and Yang (2018) find that pretrial detention (relative to release) for the marginal defendant costs between \$26,000 and \$70,000 in social costs and they lose an addition of \$29,000 in lifetime income. Norris, Pecenco, and Weaver (2021) calculates the net cost of post-trial incarceration (including familial spillovers) between \$8,200 and -\$715. Mueller-Smith (2015) calculates that post-trial incarceration for 1 year in prison produces social costs between \$56,000 and \$67,000.

3.3 Interpretation and Robustness

Interpreting the 2SLS results as the causal effect of release and detention relative to EM requires strong assumptions. Under constant treatment effects, 'IA' monotonicity (Imbens and Angrist (1994)), and no exclusion restriction violations, as referenced in Section 3.1, the 2SLS results can be interpreted as the causal effects on all defendants (Kirkeboen, Leuven, and Mogstad (2016), Mountjoy (2022)). As these assumptions may not hold, I test for violations and probe the robustness of the results in this section. Point estimates along with confidence intervals for the following specifications are displayed in Figure 3 while those focusing on exclusion restriction violations are in Figure 4. Additional robustness tests are discussed in Appendix A.3 and displayed in Figures B.5 - B.7.

While the sample contains relatively few judges, the literature with many judges generally employs Kolesár (2013)'s UJIVE method, so I use Frandsen, Leslie, and McIntyre (2023)'s extension with CJIVE judge-propensities replacing judge fixed effects, with almost identical results. As shown in Blandhol et al. (2022), the inclusion of controls in 2SLS complicates interpretation of results, however the 2SLS results are highly similar when judge fixed effects are used as the instruments and no year-quarter or day-of-week controls are included. Results are also similar when adding defendant and case-level controls. I also perform the same analysis including D-EM bonds in the analysis sample.

While D-EM bonds are harder to classify into treatments than the main bond types, their inclusion in the sample does not significantly alter the main interpretation. Some estimates do differ, such as the results suggesting that EM (detention) has more positive effects on pre-trial misconduct compared with release (EM) when including D-EM bonds, but the cost estimates are highly similar to the main sample results. These deviations may be due to misclassification of the D-EM bonds to treatments. Relatedly, Appendix A.3 discusses the results when using different recodings of missing treatments.

3.3.1 Exclusion and Bond Amounts

Because the judge's actual decision in bond court is to choose a bond amount and bond type, while the defendant then decides whether to pay to be released, the treatments as defined may violate the exclusion restriction because bond amounts can directly influence defendant outcomes (Gupta, Hansman, and Frenchman (2016)). Additionally, the duration the defendant spends on EM or detention could influence their outcomes as well. To test the sensitivity of my results to these issues, I perform multiple tests.

First, I control for the judge's bond amount propensity in the first and second stages, which will control for the bond amount dimension of an exclusion violation and will allow judge propensity instruments to recover the effects of EM vs. release and detention if treatment effects are constant for each treatment (Mueller-Smith (2015), Bhuller et al. (2020), Norris, Pecenco, and Weaver (2021), Humphries et al. (2024)). Because these bond propensities are co-linear with judge fixed effects, I use judge propensities for treatments as the instruments. As shown in Figure 4, the estimates differ slightly from the main 2SLS results; for example, EM has a larger positive effect on receiving a failure to appear or new case pretrial compared with detention. However, the overall conclusions are essentially unchanged, with EM still not producing a significant reduction in total costs relative to release but now producing a larger reduction in total costs relative to detention.

Next, I redefine the treatments using bond types and amounts in— 'release' is defined as receiving an I-bond or low D-bond, 'EM' is receiving an EM bond, and 'detain' is receiving a higher D-bond. This ensures that the instrument captures only the judge's choice rather than the defendant's decision to pay. I vary the low/high cutoff for D-bonds: group 1 uses a high cutoff of \$40,000, group 2 uses a moderate cutoff of \$20,000, and group 3 uses a very low cutoff of \$0 (effectively, only I-bond are classified as release). The results are shown in Figure 4. Groups 1 and 2 generally produce similar results for EM vs. release as the main estimates, while group 3's very low cutoff occasionally leads to more variable estimates — consistent with many actually released defendants being classified as detained. Nevertheless, each group's results are generally consistent with the main results.

Finally, I shift the cutoff of release vs. detain and EM from 7 days to 3 days (as used in Dobbie, Goldin, and Yang (2018), Stevenson (2018)) and to 14 days, and the results are highly similar to those using the 7-day cutoff (see Figure 4). This indicates the duration cutoff used is not driving the results. Overall, these tests suggest that exclusion restriction violations are not likely to significantly bias the results such that the overall conclusions are affected.

3.3.2 Monotonicity and Constant Treatment Effects

Given the main assumptions hold, 2SLS recovers the causal effect of EM versus release and detention if treatment effects are constant. This is a consistent result in the literature on 2SLS with multiple treatments (Humphries et al. (2024), Bhuller and Sigstad (2024), and Kamat, Norris, and Pecenco (2023)). However, if treatment effects are heterogeneous, then, at best, 2SLS only recovers a weighted average of causal treatment effects across defendants who are induced into the respective treatment due to their judge ('compliers'), or the local average treatment effect (LATE). This requires additional assumptions on how defendants (would) move between treatments in response to different values of the judge instruments, such as unordered partial monotonicity (UPM) (Mountjoy (2022), Humphries et al. (2024)), average conditional monotonicity and no cross effects (Bhuller and Sigstad (2024)), or latent monotonicity (Kamat, Norris, and Pecenco (2023)).

In Humphries et al. (2024), the UPM assumption restricts the flows of compliers across treatments in response to instruments. However, because judge propensity instruments for different treatments must sum to one — as they are essentially judge-specific probabilities for multiple mutually exclusive treatments — they will generally violate UPM. Consistent with their results, I also find violations of UPM in my environment.[12] They provide an alternative identification approach to overcome the issues of judge propensity instruments. It involves using judge-specific cutoffs rather than propensities as instruments, which are recovered by modeling judges' decisions to assign different treatments. While recovering the latent cutoffs involves alternative assumptions on judge behavior, the method does allow for an alternative set of 2SLS estimates with instruments that do not suffer from the issues associated with judge propensities. Following this, I use a simple method to recover judge-specific thresholds and use them as instruments for release and detention.[13] The results are shown in Figure 3

[12]Humphries et al. (2024) provide a test for UPM based on predicted characteristics of individuals moving between treatments, and I reject UPM using this test, though the results are not shown.

[13]Specifically, I compute each judge's treatment-specific cutoffs (ζ_j^s) using a method analogous to a simplified Berry inversion (Berry (1994)), where $\bar{s}_j$ is judge j's mean rate of assigning treatment s, as $\zeta_j^{EM} = log(\overline{EM}_j) - log(1 - \overline{EM}_j - \overline{Detain}_j)$ and $\zeta_j^{Detain} = log(\overline{Detain}_j) - log(1 - \overline{EM}_j - \overline{Detain}_j)$.

("HOSSvD Thresholds"), and, in general, they are consistent with the main results though less precise.

If treatments are ordered then 2SLS will recover LATEs, though whether judges behave as if treatments are ordered is a testable assumption itself (Bhuller and Sigstad (2024), Humphries et al. (2024)). For example, a UPM assumption is necessary for a strictly ordered model (Humphries et al. (2024)), and so violating this assumption (as above) rejects a strictly ordered model. Bhuller and Sigstad (2024) show that if judges assign defendants to ordered treatments (in this case, release, EM, then detain) based on a threshold-crossing model, then 2SLS recovers LATEs if predicted release is a linear function of predicted detention — where predicted treatments are based on instruments. Similar to their example, I find modest violations of this assumption as shown in Figure B.4, suggesting that 2SLS estimates will be biased if treatment effects are heterogeneous, though the bias will likely be relatively small.

Finally, I test for violations of the constant treatment effects assumption using the Sargan-Hansen over-identification test (Sargan (1958), Hansen (1982)). As shown in Panel C of Table 4, I reject the Sargan-Hansen overidentification test for 4 of 12 outcomes at the 5% level, consistent with different instrument values picking up different treatment effects. However, after adjusting p-values for the multiple hypotheses using the Holm (1979) correction, I reject constant effects at the 5% level for only 2 out of 12 outcomes (failure to appear and any new case pretrial).

To explore the extent of potential heterogeneity, I use an alternative construction of the instruments. This should influence a different set of compliers, and if treatment effects are highly heterogeneous this should result in different estimates. Specifically, I construct judge-preference instruments such that defendants are differentially influenced into treatments based on both their judge and their observables (Mueller-Smith (2015), Leslie and Pope (2017), Stevenson (2018)). I use the interactions between judge dummies and a vector of observables (W_{ijc}) beyond the main controls X_{ijc}.[14] Consistent with a small degree of

[14]Defendant observables contained in W_{ijc} are indicators for the defendant being Black, female, older than 30, past cases, FTA, and charge bins, and charge type indicators, in addition to indicators for misdemeanor

heterogeneous treatment effects, the results differ somewhat from those using judge leave-out propensities, particularly for EM vs. release and for new cases pretrial, as shown in Figure 3 ("Judge x X"). However, the extent of heterogeneity across main outcomes is not generally economically significant, and the primary conclusions are highly similar to those of the main results. Collectively, these results suggest that for treatment effects are not perfectly constant but sufficiently close to constant such that violations of monotonicity are unlikely to significantly change interpretations of the results.

4 Effect of the Introduction of IEM

In this section, I compare the prior estimates of the effect of EM relative to release and detention with the observed changes in outcomes resulting from the introduction of IEM and expansion of EM following June 2013. This time-series analysis provides complementary estimates to the main 2SLS estimates, though it relies on an entirely different source of variation and does not leverage judge assignments.

4.1 Empirical Strategy

Exploiting the introduction of EM in Cook County to estimate the effect of moving defendants onto EM faces two main issues. First, court data on other municipalities that are sufficiently similar to Cook County is difficult to obtain. Second, because the treatment environment goes from two to three treatments, with EM being an alternative to both release and detention,

charge types. To avoid issues with selecting instruments on t-values, I include many observables and do not filter or use machine learning techniques to choose which instruments to include (Angrist and Frandsen (2022)). For the first stage, it becomes:

$$\begin{aligned} Release_{ijc} &= \sum_j \gamma_j^R 1\{J=j\} W_{ijc} + \mu_R W_{ijc} + \lambda_R X_{ijc} + e_{R,ijc} \\ Detain_{ijc} &= \sum_j \gamma_j^D 1\{J=j\} W_{ijc} + \mu_D W_{ijc} + \lambda_D X_{ijc} + e_{D,ijc} \end{aligned} \tag{4}$$

where γ_j^R and γ_j^D are vectors that capture judge-specific propensities for assigning a defendant with observables W_{ijc} to release or detention. I reject that they are weak instruments using the Sanderson and Windmeijer (2016) test. Conditional F-statistics are 21 and 34.1 for release and detain, with over 100 instruments.

comparing aggregate outcomes pre-EM to those post-EM only recovers the effect of EM vs. no EM. However, I show that aggregate outcomes can be decomposed in order to construct the effects of EM vs. release and EM vs. detention.

I decompose the changes in average outcomes of defendants within treatment groups before and after the introduction of IEM bonds to construct the effect of moving defendants from release and detention onto EM. For this, I use data from before and after EM's introduction. The primary assumptions are that the average outcomes of defendants assigned to each pre-EM treatment (release and detention) would have been constant over time if not for the introduction of EM, that the underlying composition of defendants did not change significantly before and after EM, and that the introduction of EM did not affect the outcomes of defendants who were not moved to EM. While these are reasonable in this context, the results in this section must be taken primarily as an external check on the main results.

Let there be four mutually exclusive types of defendants indexed by k: $k = R \to R$ are 'always' released defendants who are released even if EM is available; similarly $k = D \to D$ are 'always' detained defendants who are detained even if EM is available; $k = R \to EM$ are release to EM compliers who are released if EM is not available but on EM if it is available; similarly $k = D \to EM$ are detained to EM compliers who are detained if EM is not available but on EM if it is available. Let $Y_s(k)$ refer to a defendant of type k's potential outcome if assigned to treatment $s \in \{R, EM, D\}$. Let t denote the time period, where $t = 1$ refers to after the introduction of IEM and $t = 0$ refers to before the introduction.

The following exposition will focus on the EM vs. Release effect, and EM vs. Detain reflects the same process — see Appendix A.4 for details. I am interested in the effect of EM relative to release on $R \to EM$ compliers: $\mathbb{E}[Y_{EM}(k = R \to EM) - Y_R(k = R \to EM)]$. To recover $\mathbb{E}[Y_R(k = R \to EM)$, I use the fact that pre-EM, observed outcomes for released defendant are weighted averages of outcomes for $R \to R$ and $R \to EM$ ($\omega_R \in [0, 1]$ is the share of $R \to R$ defendants), while after EM it is only composed of $R \to R$, and so:

$$\mathbb{E}[Y_R(k = R \to EM) = \frac{1}{(1-\omega_R)}[\mathbb{E}[Y|s = R, t = 0] - \omega_R\mathbb{E}[Y|s = R, t = 1]]$$

To recover $\mathbb{E}[Y_{EM}(k = R \to EM)]$, I use the fact that post-IEM outcomes for those on EM ($\mathbb{E}[Y|s = EM, t = 1]$) is a weighted sum of outcomes for $R \to EM$ and $D \to EM$ defendants. So, I take the weighted mean of $Y|s = EM, t = 1$, where observations are weighted based on their predicted probability of being assigned to release based on defendant observables, and predicted probabilities are constructed using a probit model trained on pre-EM data.

Taking the difference between the constructed estimates of $\mathbb{E}[Y_{EM}(k = R \to EM)]$ and $\mathbb{E}[Y_R(k = R \to EM)]$ provides the effect of being moved from release to EM due to the introduction of EM for those $R \to EM$ compliers.

4.2 Results

Table 5 displays the results for the effect of EM relative to release on $Release \to EM$ and EM relative to detention on $Detain \to EM$, as well as estimates of $\mathbb{E}[Y_R(k = R \to EM)]$ and $\mathbb{E}[Y_D(k = D \to EM)]$ for 3, 6, and 9 month windows for computing average outcomes, while Figure 5 displays the computed effects using different time spans (restricting the time-series before and after to 3 months, 6 months, and 9 months) along with the 2SLS estimates for comparison. Table 6 contains the number of observations pre- and post-IEM in each sample along with values of ω_R and ω_D.

As shown in Table 5, the net effect of introducing EM is a total savings of between $240 and $1,340 per defendant using low-murder costs, while using full murder costs produces savings only for the 9-month window due to high-cost outlier pre-trial new case costs in the first 3 months (visible in Figure 5). Overall, the cost savings are driven by moving would-be detained defendants onto EM, resulting in large savings on the cost of their time in jail and the cost of sentencing them to incarceration.

The 3, 6, and 9 month windows produce results for main outcomes that are often similar to the 2SLS estimates, with some exceptions, despite using a completely different source of identifying variation and assumptions. For example, using the $+/-6$ months specification, EM reduces failures to appear by -8pp for $Release \rightarrow EM$ defendants and increases them by 2pp for $Detain \rightarrow EM$ defendants. For total costs using low-murder, EM increases costs by \$705 for $Release \rightarrow EM$ defendants and decreases costs by $-\$10{,}845$ for $Detain \rightarrow EM$ defendants. Both of these estimates are well within their respective 2SLS 95% confidence interval.

The main difference in total costs using full murder costs is that of EM vs. detention which is near zero for 6 and 9 month windows and positive for the 3 month window (whereas 2SLS estimates are negative), due to large positive estimates for pretrial crime costs using full murder costs. The similarity between the pre-post estimates, which captures the effect of EM on $R \rightarrow EM$ and $D \rightarrow EM$ defendants, and the 2SLS estimates (the effect on the marginal defendant between EM and release and EM and detention given EM exists) is consistent with the relatively constant treatment effects found in the main analyses.

5 Conclusion

This paper explores the effect of pretrial electronic monitoring on defendant outcomes relative to both release and detention in Cook County, Illinois. I leverage the quasi-random assignment of bond court judges to cases to estimate the effect of EM vs. release and detention. I find that EM reduces total costs compared with detention, but not necessarily compared with release. EM, in comparison to detention, leads to more low-level pretrial misconduct, but has a small negative effect on costly pretrial and post-trial crime. However it is significantly cheaper in terms of direct costs, which proves to outweigh other dimensions. When compared with release, EM curbs minor pretrial misconduct but has minimal benefits with respect to overall crime costs at a higher direct cost. A time-series analysis exploiting the introduction of EM also yields similar conclusions.

These results bode well for expanding EM in the U.S. pretrial system as a less costly and harmful replacement for pretrial detention. However, the results caution against its use on defendants who would otherwise be released. In contrast with existing work, which focuses on prison versus EM outside of the U.S., the setting and sample of this study, felony defendants in Cook County, are more representative of and applicable to other major jail systems in the U.S.

While this paper documents the effects of pretrial release, EM, and detention across multiple dimensions, I cannot quantify many of the personal and financial costs of pretrial EM and detention which biases results in favor of higher levels of treatment. With respect to EM, testimonials and research indicate that rigid EM rules with limited flexibility potentially lead to adverse economic, social, and health outcomes (Green (2016), Hager (2020), Weisburd et al. (2021), CGL and Appleseed (2022)). Addressing these concerns could involve milder violation penalties, increased discretion, and leniency, aligning with optimal deterrence models (Becker (1968)) and "swift-and-certain" sanctions programs (Hawken and Kleiman (2009), Kilmer et al. (2013)). Lastly, this paper underscores the need to comprehend the costs and benefits of surveillance technology in criminal justice and broader economic contexts, as this study represents an initial step in evaluating the desirability of such technologies.

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Table 1: Summary Statistics by Treatment

	All	Release	EM	Detain
	(1)	(2)	(3)	(4)
Defendant				
Black	0.75	0.63	0.78	0.8
Hispanic	0.12	0.17	0.1	0.1
White/Other	0.13	0.2	0.12	0.09
Male	0.85	0.8	0.82	0.92
Age	34	35	36	32
Any Charge by Type				
Felony Violent	0.07	0.03	0.02	0.15
Felony Drug Poss.	0.52	0.63	0.62	0.34
Felony Drug Deliv.	0.19	0.15	0.22	0.19
Felony Property	0.16	0.14	0.16	0.17
Felony Weapon	0.1	0.08	0.01	0.22
Any Charge by Class				
Class X	0.11	0.06	0.1	0.18
Class 1	0.07	0.05	0.06	0.09
Class 2	0.13	0.07	0.1	0.2
Class 3	0.13	0.13	0.09	0.18
Class 4	0.6	0.7	0.67	0.46
Class Unknown	0.07	0.07	0.05	0.08
Case History				
Past B1 Cases	2.7	1.72	3.02	3.2
Case within Year	0.41	0.27	0.42	0.53
Past Guilty Felonies	0.63	0.21	0.63	0.99
Bond Court Outcomes				
Bond Amount	$48,707	$20,167	$31,171	$90,721
Median Days in Jail	23	0	-	84
Median Days on EM	19	0	24	-
Median Case Duration	122	93	74	177
Defendant Outcomes				
Failure to Appear	0.08	0.14	0.08	0.03
New Case Pretrial	0.16	0.18	0.19	0.11
Any Guilty Felony Charge	0.53	0.37	0.49	0.72
Sentenced to Incarceration	0.37	0.14	0.31	0.62
New Felony Case Post-Trial within 3 Years	0.28	0.21	0.32	0.29
N Obs	65,430	19,285	23,314	22,831
Share of Obs	1	0.29	0.36	0.35

Note: Table displays summary statistics by pretrial treatment with one observation per case in the main sample which went through Branch 1 bond court ('B1'). Variables beginning with 'Charge' are binary variables indicating any charge of a specific type. Class U felonies are of unknown class. Case within Year is an indicator for having any case (Branch 1 or not) within the past year. Sentenced to incarceration refers to being sentneced to incarceration, either in the Illinois or Cook County Department of Corrections.

Table 2: Selection on Observables and Tests for Violations of Exogeneity

	Regressing Treatment on Characteristics			Regressing Characteristic on Judge FEs	
	Release	EM	Detain	All Judges Wald P-Value	
	(1)	(2)	(3)	(4)	(5)
Black	-0.071***	0.047***	0.024***	0.076	0.09
Hispanic	0.011*	0.0028	-0.014**	0.723	0.55
Female	0.059***	0.053***	-0.11***	0.836	0.981
Age >= 30	0.043***	0.03***	-0.074***	0.418	0.232
Charge Felony Class X/1	-0.076***	-0.046***	0.12***	0.131	0.146
Charge Felony Class 2-4	0.0093	0.01	-0.02**	0.077	0.211
Charge Class Unknown	0.014	-0.00098	-0.013	0.35	0.331
Felony Violent	-0.1***	-0.21***	0.31***	0.732	0.568
Felony Drug	0.011*	0.045***	-0.057***	0.122	0.109
Felony Weapon	-0.11***	-0.28***	0.39***	0.166	0.079
Felony Property	0.0037	0.0079	-0.012	0.278	0.562
Felony Other	-0.055***	-0.09***	0.15***	0.527	0.533
No Felony Violent or Property	0.044**	0.02	-0.063**	0.226	0.639
N Past Cases [1,4]	-0.075***	-0.0058	0.081***	0.892	0.956
N Past Cases [5,12]	-0.13***	0.0076	0.13***	0.546	0.54
N Past Cases > 12	-0.16***	0.022**	0.13***	0.774	0.868
N Past FTAs [1,2]	-0.027***	-0.00074	0.027***	0.413	0.537
N Past FTAs > 2	-0.029***	-0.0033	0.032***	0.861	0.97
Any Past Guilty	-0.027***	0.023***	0.0044	0.326	0.576
Any Past Guilty Felony	-0.12***	-0.057***	0.17***	0.411	0.668
N Obs	72152	72152	72152	72152	76859
Wald Stat.	190.52	222.27	1182.36	0.953	0.909
Wald P-value	<0.001	<0.001	<0.001	0.641	0.773

Note: Table displays tests for selection on observables in Columns (1)-(3) by regressing each treatment (columns) on a vector of defendant characteristics (rows); Columns (4) and (5) display tests for judges selecting defendant characteristics by regressing each defendant characteristic (rows) on judge fixed effects variables at testing for the joint hypothesis that they are all zero. All regressions include year-quarter and day-of-week fixed effects. Sample in Columns (1)-(4) is all felony cases for which treatments can be assigned, meaning the main sample and cases with D-EM bonds, while Column (5) includes all felony cases even if treatments cannot be assigned. Wald tests in Columns (1)-(3) test the joint null of all characteristic coefficients; Wald tests in Columns (4)-(5) test the joint null of all judge effects across coefficients by stacking regressions. Standard errors (not displayed) are clustered at the branch 1 date level. ***p < 0.01; **p < 0.05; *p < 0.1

Table 3: First Stage of Release and Detain on Judge Fixed Effects

	Release	Detain
	(1)	(2)
A: Single Treatment on Judge Fixed Effects		
Mean Treat.	0.29	0.35
SD(Judge FE)	0.071	0.1
Var(Judge FE)/Var(Treat.)	0.024	0.048
Wald Stat.	75.94	293.12
Wald P-value	<0.001	<0.001
B: Both Treatments on Judge Fixed Effects		
Angrist-Pischke F-Stat.	40.31	102.65
Angrist-Pischke P-value	<0.001	<0.001
Sanderson-Windmeijer F-Stat.	42.05	55.83
Sanderson-Windmeijer P-value	<0.001	<0.001

Note: Table displays tests for the strength of the first stage (equation (3)) relationship between judge fixed effects and being assigned to pretrial release or detention. Panel A shows results for regressing a single treatment on judge fixed effects, with SD(Judge FE) and Var(Judge FE) referring to the standard deviation and variance of treatment-specific judge fixed effects, after being bias corrected using Kline, Saggio, and Solvenston (2020), and the Wald test tests for a joint zero effect across all judges. Panel B shows results using both first stage equations (two treatment variables) in equation (3), with Angrist-Pischke and Sanderson-Windmeijer being tests for weak instruments with multiple endogenous variables. All specification cluster standard errors at the branch 1 date level and include year-quarter and day-of-week fixed effects.

Table 4: Effects of EM compared with Release and Detention

	Pretrial					Case Outcomes		Posttrial (3 Years)			Total Marginal Cost	
	Failure to Appear	New Case Pretrial	New Case Pretrial Violation	New Case Cost ($1,000)	New Case Cost (Low Murder) ($1,000)	Sentenced to Incarceration	Detention/ EM Cost ($1,000)	Total New Cases	New Case Cost ($1,000)	New Case Cost (Low Murder) ($1,000)	Total Cost ($1,000)	Total Cost (Low Murder) ($1,000)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: OLS												
EM vs. Release ($-\beta^R$)	-0.057***	0.018***	0.033***	-0.56	-0.32*	0.18***	1.2***	0.16***	5.7	4.4***	9.3**	8.2***
	(0.0032)	(0.0041)	(0.00174)	(1.54)	(0.166)	(0.00559)	(0.0415)	(0.00811)	(4)	(0.366)	(4.29)	(0.426)
EM vs. Detain ($-\beta^D$)	0.05***	0.087***	0.04***	-1.9	0.8***	-0.3***	-21***	0.055***	-16***	-0.31	-32***	-13***
	(0.0022)	(0.00347)	(0.00157)	(1.69)	(0.158)	(0.0055)	(0.191)	(0.00859)	(4.59)	(0.397)	(4.85)	(0.436)
Panel B: 2SLS with Judge Fixed Effects												
EM vs. Release ($-\beta^R$)	-0.11***	-0.074*	-0.021	-6.8	-2.9	0.096*	1.1	0.15*	-41	-1.2	-43	-1.5
	(0.0274)	(0.0392)	(0.0148)	(19.7)	(1.85)	(0.0546)	(1.64)	(0.0876)	(47)	(4.12)	(49.8)	(4.51)
EM vs. Detain ($-\beta^D$)	0.061***	0.085***	0.041***	-13	-0.32	0.033	-14***	0.041	-51	-3.5	-66*	-8.2***
	(0.0184)	(0.0269)	(0.0103)	(13.8)	(1.25)	(0.0367)	(1.08)	(0.0584)	(32.5)	(2.79)	(34.2)	(3.01)
Panel C: Sargan-Hansen Overidentification Test												
J-Stat	19.25	39.22	7.94	3.65	11.2	13.86	5.59	4.22	5.28	7.09	3.77	4.74
P-value	<0.01	<0.01	0.09	0.46	0.02	<0.01	0.23	0.38	0.26	0.13	0.44	0.32
Adj. P-value	0.01	<0.01	0.75	1	0.22	0.08	1	1	1	0.92	1	1
N. Obs	65412	65430	65430	65430	65430	65430	65192	65430	65430	65430	65174	65174
Mean Y | Release	0.14	0.18	0.01	6.83	4.14	0.14	0.15	0.32	26.12	11.52	35.9	18.59
Mean Y | EM	0.08	0.19	0.05	6.32	3.78	0.31	1.16	0.49	32.5	15.98	45.5	26.43
Mean Y | Detain	0.03	0.11	0.01	8.19	3	0.62	21.95	0.44	48.77	16.32	77.18	39.48

Note: Table displays OLS (Panel A) and 2SLS (Panel B) results for equation (2), displaying estimates of $-\beta_R$ and $-\beta_D$ which are the effect of EM relative to release and EM relative to detention, respectively. Controls include year-quarter and day-of-week fixed effects. Total marginal cost calculations follow equation (1), and 'low murder' refers to using $400,000 as the cost of a murder charge. Standard errors in parentheses are clustered at the branch 1 date level. Panel C displays the results for the Sargan-Hansen overidentification test (clustering at the branch 1 date level) and Holm (1979)-adjusted p-values. ***p $<$ 0.01; **p $<$ 0.05; *p $<$ 0.1

Table 5: Effects of EM Introduction

	Pretrial					Case Outcomes		Posttrial (3 Years)			Total Marginal Cost	
	Failure to Appear	New Case Pretrial	New Case Pretrial Violation	New Case Cost ($1,000)	New Case Cost (Low Murder) ($1,000)	Sentenced to Incarceration	Detention/ EM Cost ($1,000)	Total New Cases	New Case Cost ($1,000)	New Case Cost (Low Murder) ($1,000)	Total Cost ($1,000)	Total Cost (Low Murder) ($1,000)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
+/- 3 Months												
Net Effect of EM	-0.011	0.012	0.006	0.099	0.003	-1.521	0.001	-0.006	5.172	0.224	4.771	-0.248
Effect of Release to EM	-0.060	-0.008	0.009	-20.809	-1.461	0.678	0.048	0.049	4.909	0.157	-14.666	-0.046
Effect of Detain to EM	0.015	0.094	0.025	38.050	1.587	-18.050	-0.225	-0.245	24.747	-0.058	52.046	-9.174
E[Y|k=R-EM]	0.083	0.161	0.028	3.213	3.213	0.795	0.184	0.446	36.176	13.734	43.396	20.937
E[Y|k=D-EM]	0.074	0.195	0.046	3.911	3.911	1.045	0.261	0.546	35.078	16.368	44.610	25.894
+/- 6 Months												
Net Effect of EM	-0.015	-0.001	0.009	0.307	-0.312	-2.241	-0.012	0.017	1.394	0.473	0.622	-0.872
Effect of Release to EM	-0.080	-0.049	0.011	-12.639	-1.896	0.629	0.034	0.078	5.484	1.674	-6.239	0.705
Effect of Detain to EM	0.024	0.044	0.031	17.678	0.302	-16.906	-0.224	-0.069	-0.738	-0.621	6.182	-10.845
E[Y|k=R-EM]	0.075	0.158	0.028	8.760	3.411	0.760	0.182	0.454	28.950	13.744	41.674	21.098
E[Y|k=D-EM]	0.069	0.188	0.044	8.469	3.862	1.010	0.262	0.554	30.665	16.375	44.790	25.878
+/- 9 Months												
Net Effect of EM	-0.012	-0.006	0.011	-2.888	-0.430	-2.582	-0.018	0.019	-0.106	0.357	-4.315	-1.338
Effect of Release to EM	-0.077	-0.075	0.015	-14.489	-2.313	0.668	0.056	0.113	-8.924	1.468	-22.088	0.536
Effect of Detain to EM	0.019	0.029	0.035	-3.728	-0.336	-14.560	-0.205	-0.046	5.482	-0.353	-7.612	-9.968
E[Y|k=R-EM]	0.071	0.164	0.034	7.213	3.398	0.829	0.206	0.469	26.114	14.075	37.780	21.915
E[Y|k=D-EM]	0.064	0.192	0.049	7.137	3.868	1.078	0.287	0.559	27.821	16.546	41.112	26.559

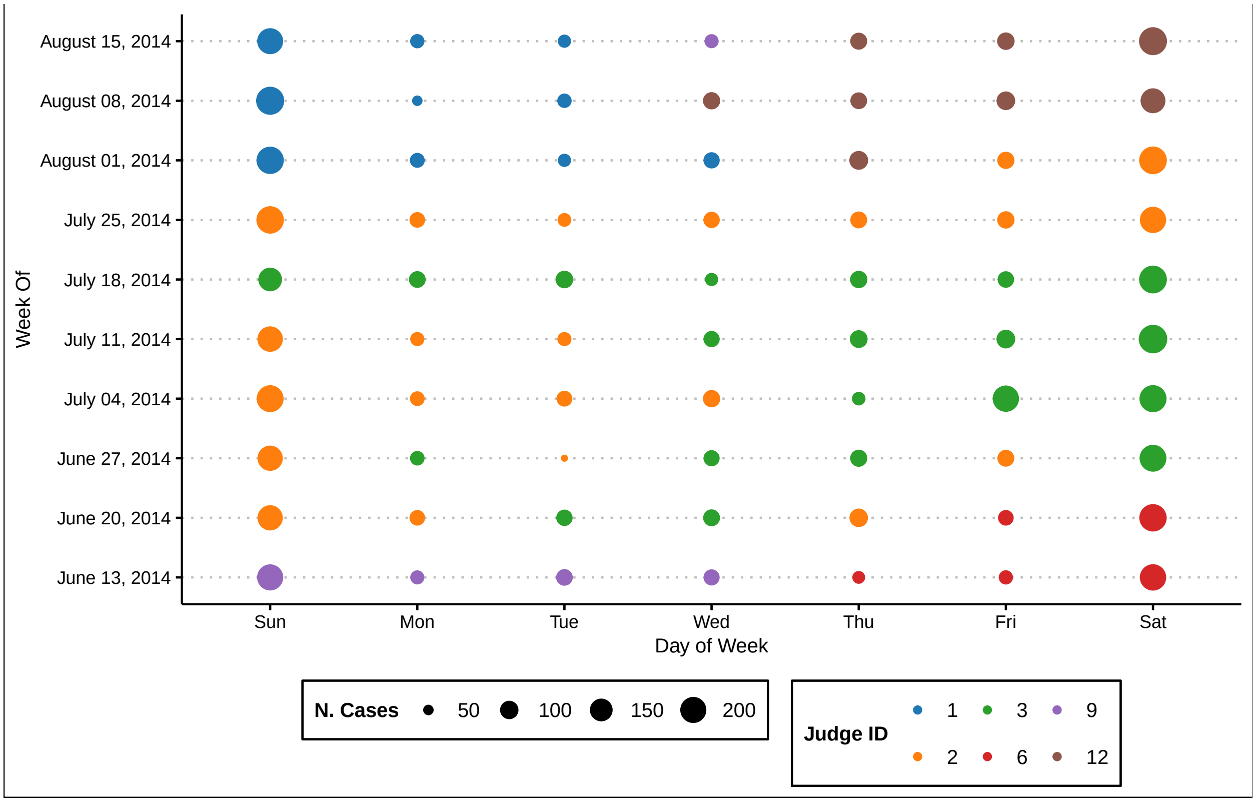
Note: Table displays the effect of the introduction of IEM on outcomes. +/- 3, 6, and 9 months refers to the time span before and after June 2013 that is used to compute the estimates. Net change pre/post EM is computed as the change in outcomes before and after June 2013. Effect of Release/Detain to EM are computed as discussed in Section 4 and Appendix A.4. E[Y|k=R-EM] and E[Y|D-EM] denote the compute average outcomes for defendants who moved from release or detention onto EM used in the treatment effect calcuation. Sample contains felony defendants without missing release types and excluding D-EM bonds.

Table 6: Additional Information for Effects of EM Introduction

Month Range	ω_R	ω_D	N Obs. Post-IEM	N Obs. Pre-IEM
	(1)	(2)	(3)	(4)
+/- 3 Months	0.829	0.502	5992	4869
+/- 6 Months	0.747	0.457	11111	9798
+/- 9 Months	0.677	0.539	15496	14400

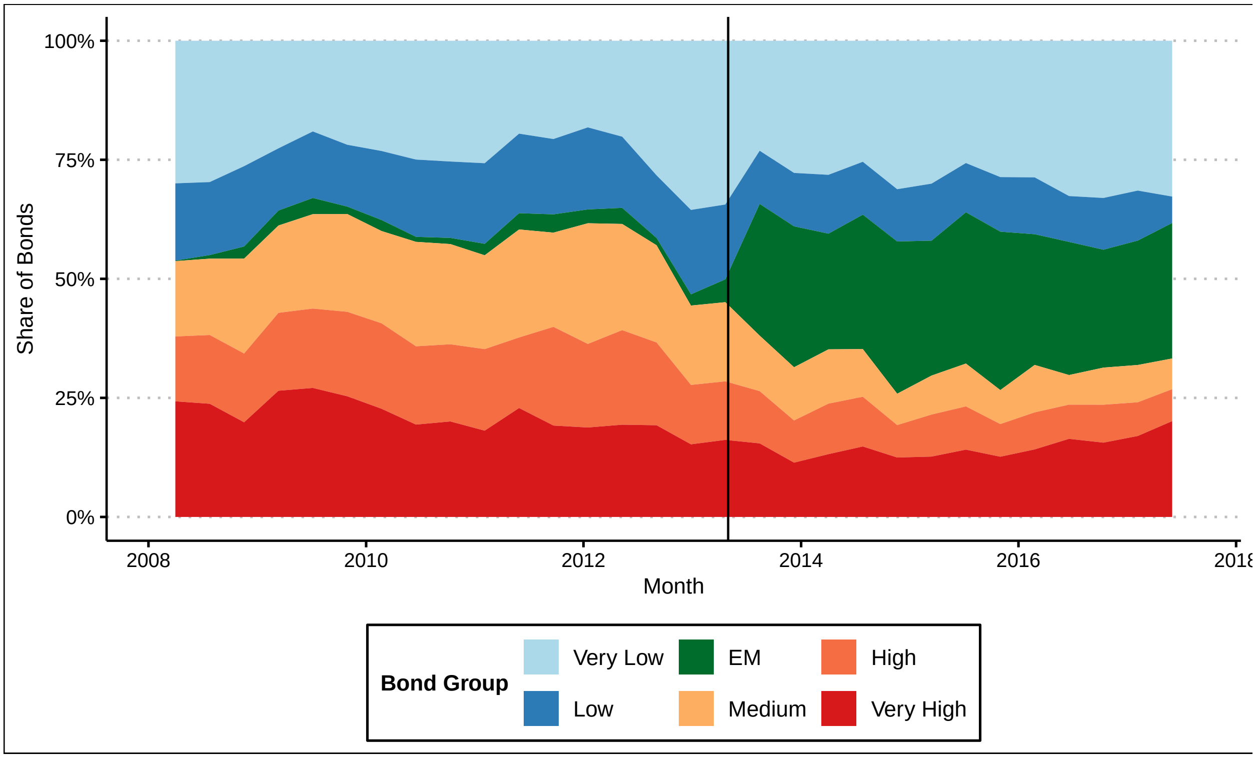
Note: Table displays additional information for the effect of the introduction of IEM on outcomes. +/- 3, 6, and 9 months refers to the time span before and after June 2013 that is used to compute the estimates. ω_R and ω_D (Columns (1) and (2)) refer to the relative shares of defendants who are 'always released' and 'always detained' regardless of whether EM is avaliable compared with those who are released and detained when EM is not avaliable. Columns (3) and (4) contain pre- and post-IEM introduction sample sizes used to compute the estimates. Sample contains felony defendants without missing release types and excluding D-EM bonds.

Figure 1: Example of Bond Court Judge Rotation Calendar



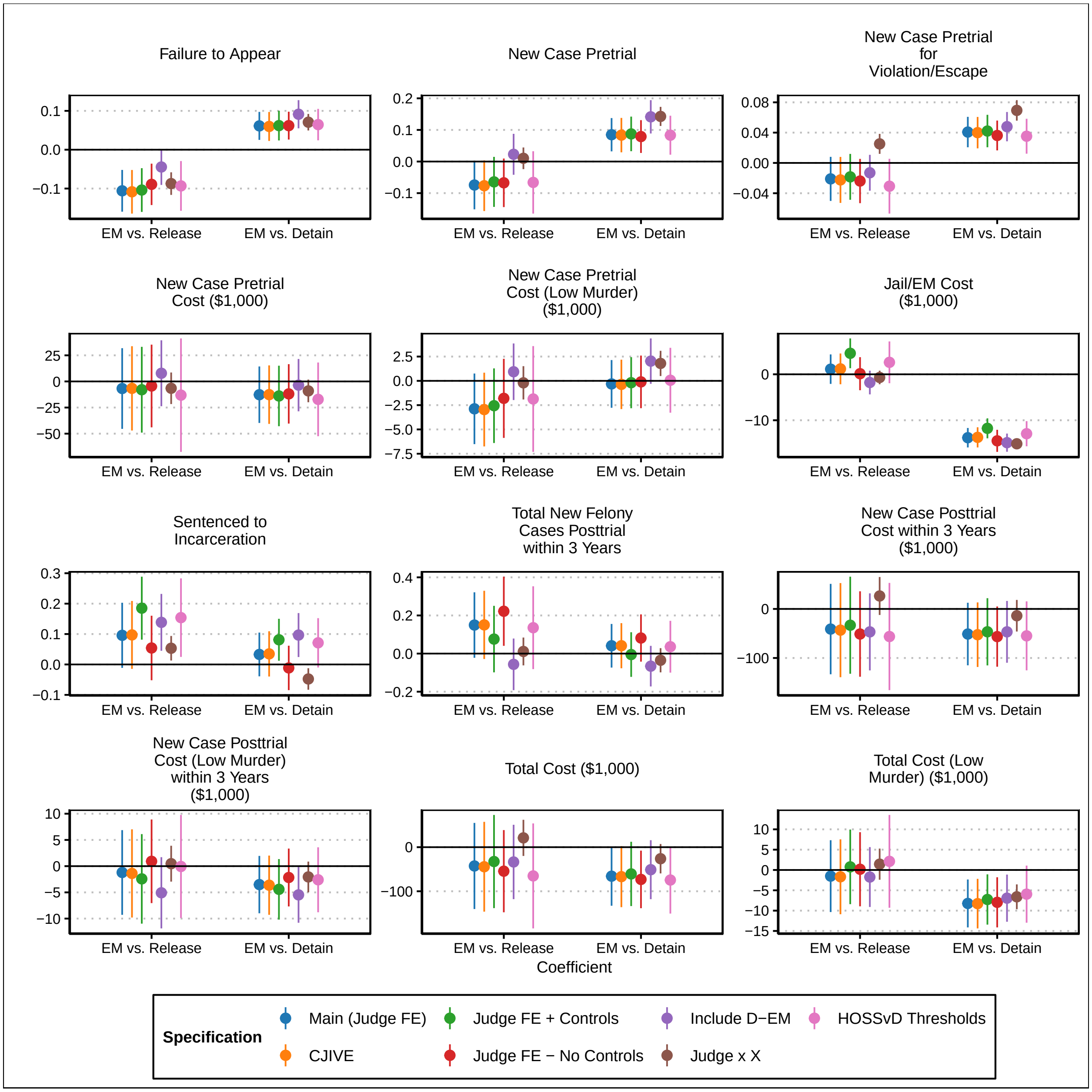
Note: Figure displays the caseloads of active judges in the Cook County Bond Court (Branch 1, Room 100) by week and day of the week between the weeks of June 13, 2014, and August 15, 2014. There were 6 active judges in this period (dot color), while the size of each dot denotes the number of cases they saw that day.

Figure 2: Bond Types Over Time



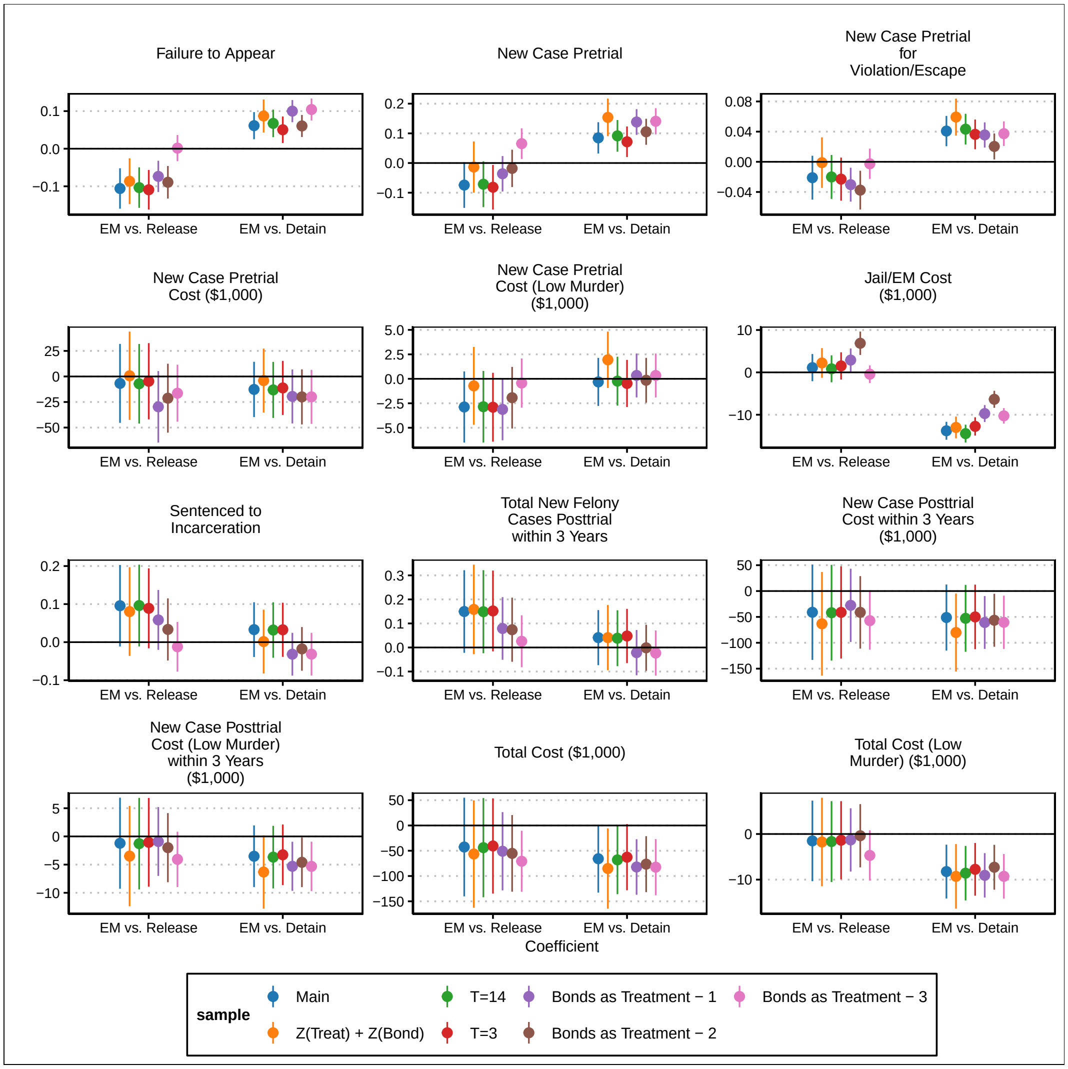
Note: Figure displays the composition of bond types within the sample between March 2008 and 2015 aggregated by the year-month of bond date. Bond group EM denotes IEM bonds, and other bond groups are determined by the bond price required for release (i.e., the bond price for any D-bond, \$0 for I-bonds, and +\$$\infty$ for bond denial). Very low contains bonds with amounts between \$0 and \$7,500; low contains bonds with amounts between \$7,500 to \$20,000; medium contains bond with amounts between \$20,000 to \$40,000; high contains bonds with amounts between \$40,000 to \$60,000; and very high contains all bonds with amounts above \$60,000.

Figure 3: Comparison of 2SLS Estimates across Specifications



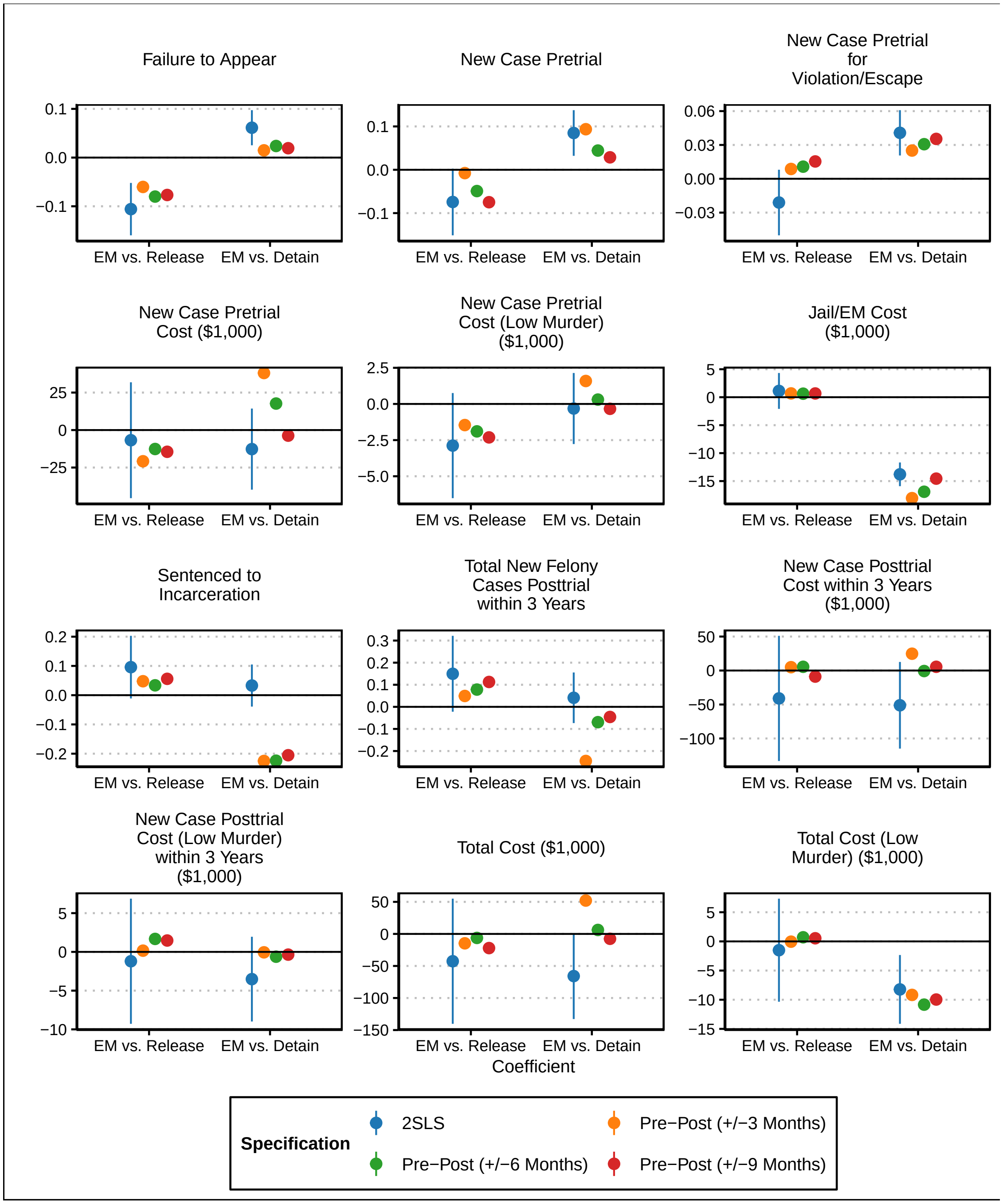
Note: Figure displays 2SLS coefficients and 95% confidence intervals using alternative specifications, with standard errors clustered at the branch 1 date level. Main estimates computed judge fixed effects as instruments, and 'CJIVE' refers to performing CJIVE at the bond court date-level to construct judge-propensity instruments; 'Judge FE + Controls' adds defendant level controls for characteristics, charge types, and past case histories; 'Judge FE - No Controls' uses no controls (year-quarter and day-of-week fixed effects), only judge fixed effects as instruments and the endogenous variables; 'Judge x X' uses judge fixed effects interacted with a vector of defendant characteristics as instruments (see Section 3.3.2); 'HOSSvD Thresholds' uses judge thresholds rather than judge propensities as instruments, following Humphries et al. (2023) (see Section 3.3.2); 'Include D-EM' includes D-EM bonds in the analysis sample..

Figure 4: 2SLS Robustness for Exclusion Restriction



Note: Figure the 2SLS estimates using judge fixed effects as instruments for the effect of EM vs. release and EM vs. detain under alternative treatment definitions and specifications to test the robustness of the main estimates against exclusion restriction violations for a subset of main outcomes. 'Z(Treat) + Z(Bond)' modifies the main specification by adding judges' residualized average bond amounts for IEM and D-bonds as controls and uses judge propensities (predicted judge fixed effects) for treatments as the main instruments; $T = 3$ and $T = 14$ change the day cutoff for classifying release, EM, and detention; bonds as treatments 1-3 uses bond types as proxies for treatments with EM bonds being classified as EM but I bonds and lower D-bonds being classified as release and higher D-bonds being classified as detain, with 1-3 corresponding to using increasingly low D-bond amount cutoffs (at $40,000, $20,000, and $0). All specifications include year-quarter and day-of-week fixed effects. 95% confidence intervals are displayed around the point estimates using standard errors clustered at the branch 1 date level.

Figure 5: Effect of EM's Introduction



Note: Figure displays the resulting estimates of the effect of EM versus release and detention using the June 2013 introduction of IEM bonds (see Section 5 for details) in comparison with the 2SLS (Panel B of Table 4) estimates. +/− 3, 6, and 9 months refers to the time span before and after June 2013 that is used to compute the estimates.

A Appendix A: Additional Analyses and Background

A.1 Background

Branch 1 operates every day, though other bond courts in Cook County do not. On weekdays and non-holidays, non-felony cases (e.g., misdemeanor, traffic, and municipal code violations that require the setting of bail) are generally handled in the bond courts determined by the location of arrest (Branch 1 is the bond court for Chicago arrests) or specifically designated courts — for example, murder and violent sex offenses are handled in their own courts. On holidays and weekends, however, all such cases are handled by Branch 1. See Aid (n.d.) for a schedule of the bond courts in Cook County. After their hearing in bond court, defendants are generally processed at Cook County Jail.

I focus on a small sample of highly active judges. Active is defined as having at least 500 cases within a year, excluding days where a judge saw fewer than 40 cases. In the full data, this filter removes 3% of observations but 90% of unique judges, indicating that the vast majority of unique judges were either sporadically working as bond court judges as substitutes but are most likely miscodings or erroneous entries. Between 2010 and 2016, two active judges are recorded working in bond court on the same date on less than 10 days out of over 2,500, about 0.3% of observations.

Judges can also link I and D bonds with supervised release requirements, though the main role of the bond is to determine if they can leave the custody of the Sheriff (i.e., exit jail pretrial). In this sense, EM can also be coupled with D-bonds (D-EM), which required the defendant to stay in jail until they paid 10% of the bond amount and were then released onto EM, which accounted for 16% of D-bonds between 2008 and 2012 (Civic Federation (2017)). However, it is unclear how many defendants were actually released onto EM from D-EMs during the period. Prior to 2012, little data on EM usage in Cook County was available (Dizikes and Lightly (2015)). Figure A.1 displays these trends across the sample period.

In 2012, disputes began between the Court and the Sheriff (who runs the jail and most of the EM releases) over the overcrowding of the Cook County Jail and EM usage began. As a result, in November 2012, judges functionally stopped using D-EM bonds which further contributed to jail overcrowding (Civic Federation (2017)), though they were occasionally used during the period of study. This sparked the introduction of the IEM bond discussed in the paper, though IEM bonds are referred to as "Electronic monitoring with D-bonds" in CGL and Appleseed (2022). The IEM bond offered an attractive solution to judges: release defendants from jail and avoid overcrowding but have them monitored by the Sheriff, who bears responsibility for any failures. The initial appeal was increased following reforms in September 2013 which urged a reduction in defendants forced to stay in jail due to lack of money (Civic Federation (2017)). A report by the Civic Federation, using different data, also indicates that in September 2013, IEM was about 25% of dispositions (Civic Federation (2017)).

The Sheriff's EM program is by far the most common form of EM (Green (2016), Civic Federation (2020)). A significantly less common EM program was "Curfew EM" which requires defendants to be in their homes between specified hours, usually 7 pm to 7 am. These programs also co-exist with other monitored release programs by the Chief Judge's office, GPS home confinement, which is primarily used for domestic violence cases (Civic Federation (2020)). Recently, Cook County has adopted GPS monitoring systems instead, though these GPS ankle or wrist bracelets operate in a similar capacity, simply without a home unit, and tracks all of the subject's movements (CCSO (2020b)). Yet, this system has run into technical issues due to false alarms (Daston (2022)).

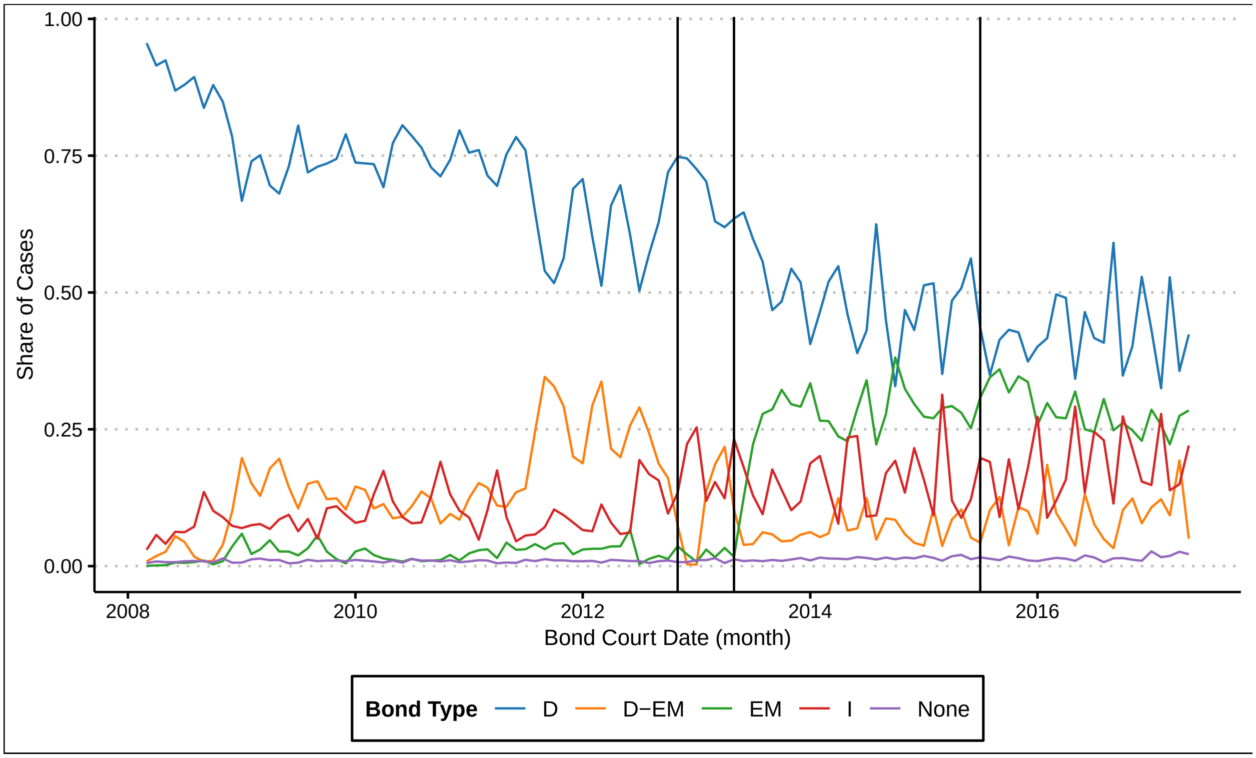
While the domain of EM monitoring is generally one's home, exceptions can be made in advance for work, school, or other reasons (Civic Federation (2020)), it but requires 2 days prior approval. See CCSO (2020a) for the rules and information sheet for the EM program in 2020. Time spent on EM in the Sheriff's program counts as days served in jail and thus can reduce one's time required to be served if found guilty (Civic Federation (2020)). In many jurisdictions, EM can require defendants to pay a fee, but this was not common for pretrial

EM during the period of study based on available sources. See Dizikes and Lightly (2015) for images of the 2013-2017 system.

As with all bond types discussed (IEM, I-bond, D-bond), the defendant is liable to pay the full bond amount if they violate their bond conditions (e.g., they fail to appear in court). While defendants are liable for the bond amount, most defendants cannot pay the large sums, and cash bail has been shown to be ineffective at ensuring court appearances (Ouss and Stevenson (2023)), and the threat of court fines has been shown to be ineffective (Albright (2021)) as the collection of such fines is rare (Pager et al. (2022)).

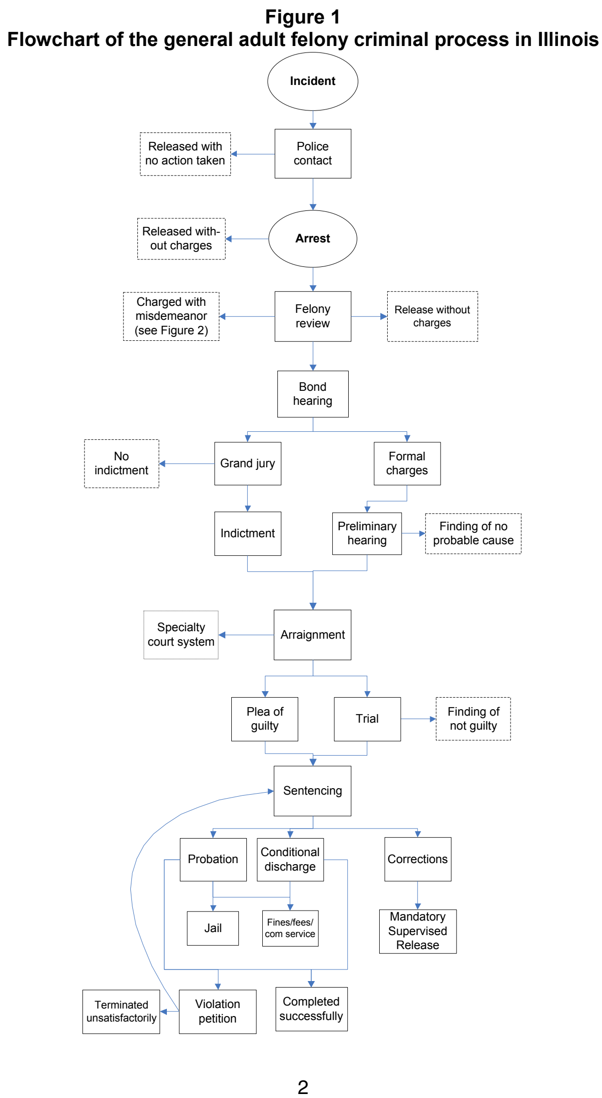
Following the initial plea of guilty or not guilty at the arraignment, the next steps depend on if the case is a misdemeanor or felony case. Felony cases proceed to a hearing which determines if the case can proceed with felony charges (usually a preliminary hearing, grand jury, or an information) and be transferred to the criminal division; otherwise, it is dropped or proceeds with misdemeanor charges. The full evolution of a case involves many events, and a flowchart for felony and misdemeanor cases can be seen in Figures A.2 and A.3, respectively.

Figure A.1: Bond Time Trends in Cook County Court



Note: Figure displays the composition of bond types within the sample between 2008 and 2015 aggregated by year-month of bond date. D-EM bond refers to D-bonds coupled with EM release, and EM refers to I-bonds coupled with EM (IEM). The first vertical line denotes November 2012, when D-EM bonds stopped being issued temporarily, and the second vertical line denotes the introduction of IEM bonds in June 2013.

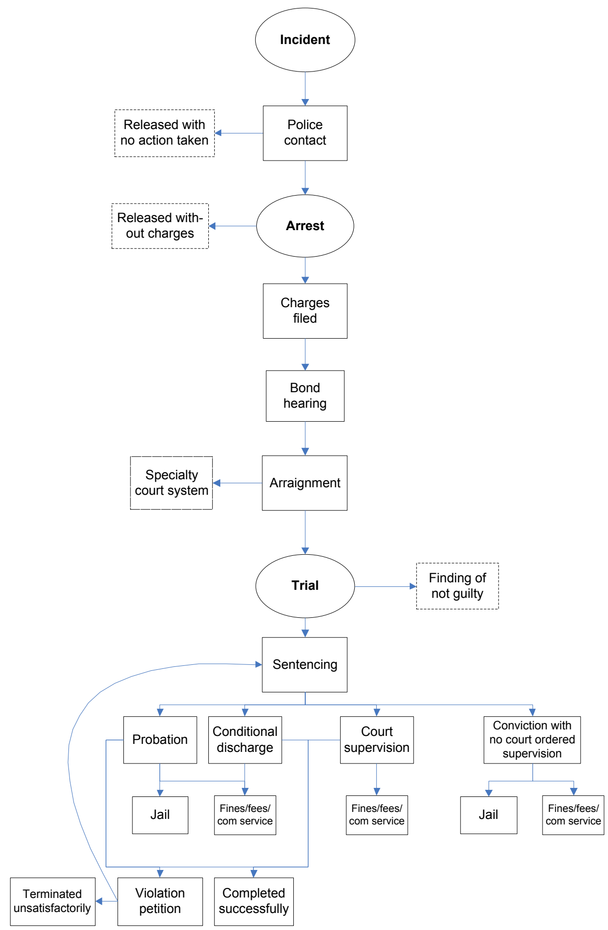
Figure A.2: Felony Case Flow Chart



Note: Figure displays sequence of events for felony cases within Cook County — though document is meant for the entire Illinois criminal justice system more broadly. Source: Afeef et al. (2012), page 2.

Figure A.3: Misdemeanor Case Flow Chart

Figure 2
Flowchart of the general adult misdemeanor criminal process in Illinois



3

Note: Figure displays sequence of events for misdemeanor cases within Cook County — though document is meant for the entire Illinois criminal justice system more broadly. Source: Afeef et al. (2012), page 3.

A.2 Data

The court data has large numbers of cases, first starting in 1984, but also contains sporadic records dating back to the 1930s. Linking is done using individual record numbers as well as personally identifiable characteristics, such as name, birth date, race, gender, and home address. As a single booking can result in multiple cases (generally 2 if it is a felony case), cases can be linked within individuals using central booking numbers common to both cases (RD numbers if CBs are missing). For linking jail data to court data, I connect defendant identities using individual record numbers, identifiable information (names), and case/detention information.

The CB and RD numbers associated with cases are used to connect court/jail profiles to Chicago-specific arrests and reported crimes. While I have additional information for Chicago arrestees, I do not require this filter, though 86.68% of the data are reported to have been arrested by the CPD.

Importantly, not all cases can be linked to a reasonable jail spell, which means that the individual follows a quick timeline of beginning a jail spell (i.e., defendant is reported to have entered jail), having a case opened against them, and proceeding to bond court. This lack of linkage is possible due to some individuals never entering the jail system due to immediately going to bond court and being released or due to a linking error — I test the sensitivity of my results to these unlinked cases in Appendix A.3. Cases with I-bonds or EM-bonds are much more likely to be unlinked to a jail spell (35.19% and 19.58%, respectively) relative to all other bond types (which averages 15.14%), which supports the former case. Interestingly, there is no pattern in missing rates for increasing bond amounts. If this were largely due to immediate release from EM, I would expect higher bonds to imply fewer missing. For instrument construction, I include all cases; for treatment construction in the main specification, non-I-bond and non-EM cases which are unlinked to jail spells are dropped in the main sample, while unlinked I-bond and EM bond cases are kept. A defendant can also be classified as "detained" if they technically exit jail due to a transfer or are sent to alternative detention (e.g., prison).

For final filters, I exclude cases that did not go to Branch 1 within 2 days of being opened. I also remove a small subset of individual-booking observations with irregular case patterns and those which do not have resolutions within the court system. I remove 2.82% if they contain more than 3 unique cases, if there are multiple cases and the difference between the minimum and maximum case initialization date is more than 120 days, if the defendant had more than 60 past cases, and if there were more than 6 individual-bookings corresponding to that defendant within the 2 year period. I drop cases within this time period that are transferred outside of the regular system (2.75%), have short case histories without resolution (2.4%), or end with a warrant being issued (0.31%). I also drop a few attempted murder charges that were not removed by prior filters, and because I focus on IEM, D-bonds, and I-bonds in the paper, I drop two very rare bond types that appear in the data (A and C bonds).

Some cases do not have final disposition dates but end with the case being dropped and contain a guilty, not guilty, stricken, or dropped disposition code (6.48%), I use the last event date as the final disposition date. Lastly, I remove a small number of cases that had murder or felony sex charges or resulted in bond denial, and I drop cases without a categorizable treatment (which includes missing jail spells for defendants without I or EM bonds). In Appendix A.3, I test the sensitivity of the results to alternate classifications of the dropped cases due to missing jail information.

In July 2015, the court introduced a public safety assessment system that guided judges on release decisions using a scoring system (Civic Federation (2020)). However, it is not clear in the data if this influenced judge behavior in any way, though larger reforms occurred in late 2017, so my sample ends in May of 2017.

A.3 Additional Robustness

I compare my results against a variety of alternate samples and recompute the 2SLS estimates for each altered sample, as shown in Figure B.5. I construct samples: excluding unclassified felony types ('No Unclassified Felonies'); dropping 2013 cases to ensure judges had sufficient time to adapt to the changes; and include non-felony (largely misdemeanor) cases in the sample ('Include Misd. Sample). Overall, these alternate samples produce significantly different conclusions compared with the main sample, with the exception being the inclusion of misdemeanor cases which shifts results away from EM being beneficial compared with detention and suggests EM is more costly compared with release, though this does not control for observable difference between felony and misdemeanor defendants.

I probe the robustness of my results to alternative codings of the treatments in Figures B.6 and B.7. First, defendants who receive EM bonds may not actually be admitted into the EM system because I do not observe whether or when the defendant has the EM system set up in their home. However, I do observe disposition codes in a defendant's case which provide more specific information on their EM status, such as explicitly stating a defendant was admitted into the Sheriff's EM program. To determine if this potential miscoding of treatments will influence the results, I reclassify defendants' EM status based on disposition codes observed in their case history. I construct 4 alternative codings which allow for additional conditions under which the defendant is classified as on EM based on disposition codes. The results, shown in Figure B.6 show that the results are generally consistent with the main estimates. Second, because jail data is unmatched to cases for a subset of the sample, a subset of cases are dropped from the sample. I redo the analysis with 4 additional samples in which cases with missing jail data are kept and D-EM bonds are kept (though other filters are applied) and the entire analysis is redone. The 4 cases are combinations of coding all missing (jail data) EM bonds as EM or release and coding all missing D-bonds as detention or release, with results shown in Figure B.7. Most results are similar to the main results.

A.4 Pre-Post IEM

In this section I provide details on the construction of estimates for the pre-post IEM analysis in Section 4. As discussed in the main text, I exploit the introduction of IEM in June 2013. I use follow the same filters for the data as the main sample, however I include data prior to June 2013 (2010 - 2013) and I drop defendants with more than 10 cases in the sample (as opposed to 6 for the main sample, since there is more time covered in the pre-post sample).

I assume there are four mutually exclusive types of defendants indexed by k: $k = R \to R$ are 'always' released defendants who are released even if EM is available; similarly $k = D \to D$ are 'always' detained defendants who are detained even if EM is available; $k = R \to EM$ are release to EM compliers who are released if EM is not available but on EM if it is available; similarly $k = D \to EM$ are detain to EM compliers who are detained if EM is not available but on EM if it is available.

Let $Y_s(k)$ refer to a defendant of type k's potential outcome if assigned to treatment $s \in \{R, EM, D\}$. Let t denote the time period, where $t = 1$ refers to after the introduction of IEM and $t = 0$ refers to before the introduction. I assume that the potential outcomes of defendants in either period is constant in both periods $Y_s \perp t$, that the average outcomes would be the same if not for IEM's introduction, that the composition of defendants did not change, and that IEM's introduction only influenced outcomes through defendants being placed on EM, so $\mathbb{E}[Y_s(k)|t = 0] = \mathbb{E}[Y_s(k)|t = 1]$.

The treatment effects of interest are the effect of EM relative to release on $R \to EM$ compliers:

$$\tau^R = \mathbb{E}[Y_{EM}(k = R \to EM) - Y_R(k = R \to EM)]$$

and the effect of EM relative to detention on $D \to EM$ compliers:

$$\tau^D = \mathbb{E}[Y_{EM}(k = D \to EM) - Y_D(k = D \to EM)]$$

I construct estimates of each piece to compute these effects.

Beginning with $\mathbb{E}[Y_{EM}(k = R \to EM)]$. Prior to IEM:

$$\mathbb{E}[Y|s = R, t = 0] = \omega_R \mathbb{E}[Y_R(k = R \to R)] + (1 - \omega_R)\mathbb{E}[Y_R(k = R \to EM)]$$

$$\iff \mathbb{E}[Y_R(k = R \to EM)] = \frac{1}{(1 - \omega_R)}[\mathbb{E}[Y|s = R, t = 0] - \omega_R \mathbb{E}[Y_R(k = R \to R)]$$

where $\omega_R \in [0, 1]$ is the share of $R \to R$ defendants in the sample of those assigned to release without EM (constant across time periods). In practice, I compute ω_R as the share of released defendants in $t = 1$ over the share in $t = 0$ in order to account for any change in the total number of defendants: $\omega_R = \frac{N(s=R,t=1)}{N(t=1)} / \frac{N(s=R,t=0)}{N(t=0)}$. After IEM, only $R \to R$ defendants are on release, so $\mathbb{E}[Y_R(k = R \to R)] = \mathbb{E}[Y|s = R, t = 1]$. Then we get an expression in terms of observables:

$$\mathbb{E}[Y_R(k = R \to EM)] = \frac{1}{(1 - \omega_R)}[\mathbb{E}[Y|s = R, t = 0] - \omega_R \mathbb{E}[Y|s = R, t = 1]]$$

Similarly, for $\mathbb{E}[Y_D(k = D \to EM)]$, the same logic returns:

$$\mathbb{E}[Y_D(k = D \to EM)] = \frac{1}{(1 - \omega_D)}[\mathbb{E}[Y|s = D, t = 0] - \omega_D \mathbb{E}[Y|s = D, t = 1]]$$

where $\omega_D \in [0, 1]$ is the share of $D \to D$ defendants in the sample of those assigned to detention without EM (constant across time periods) and is constructed as $\omega_D = \frac{N(s=D,t=1)}{N(t=1)} / \frac{N(s=D,t=0)}{N(t=0)}$.

Next, I construct estimates of $\mathbb{E}[Y_{EM}(k = R \to EM)]$ and $\mathbb{E}[Y_{EM}(k = D \to EM)]$. Post-IEM, the average outcome among defendants on EM is a weighted (by their share of the total $\omega_{EM,R} \in [0, 1]$ and $1 - \omega_{EM,R}$) sum of these two quantities:

$$\mathbb{E}[Y|s=EM,t=1]=\omega_{EM,R}\times\mathbb{E}[Y_{EM}(k=R\rightarrow EM)]+(1-\omega_{EM,R})\times\mathbb{E}[Y_{EM}(k=D\rightarrow EM)]$$

Using pre-IEM data is less useful in this because EM did not exist as a potential treatment. However, the observable equivalent of $\mathbb{E}[Y|s=EM,t=1]$ is composed of defendant, indexed by i, who are either $k=R\rightarrow EM$ or $k=D\rightarrow EM$ and is the sum over their observed outcomes on EM. Given this, I use individual defendant data to construct estimates of $\mathbb{E}[Y_{EM}(k=R\rightarrow EM)]$ ($\mathbb{E}[Y_{EM}(k=D\rightarrow EM)]$) as the weighted mean of defendant outcomes on EM post-IEM with weights on the individual defendant corresponding to their likelihood of being on release (detention) if EM was not an option (i.e., their probability of being $k=R\rightarrow EM$ or $k=D\rightarrow EM$) (and scaled such that weights sum to 1):

$$\mathbb{E}[Y_{EM}(k=R\rightarrow EM)]\approx\frac{1}{N(s=EM)}\sum_{i|s_i=EM}\frac{P_i(k=R\rightarrow EM)}{\sum_{i|s_i=EM}P_i(k=R\rightarrow EM)}\times Y_i$$

$$\mathbb{E}[Y_{EM}(k=D\rightarrow EM)]\approx\frac{1}{N(s=EM)}\sum_{i|s_i=EM}\frac{P_i(k=D\rightarrow EM)}{\sum_{i|s_i=EM}P_i(k=D\rightarrow EM)}\times Y_i$$

I construct estimates of $P_i(D\rightarrow EM|EM)$ and $P_i(R\rightarrow EM|EM)=1-P_i(D\rightarrow EM)$ using defendant characteristics (race, gender, age, charge types, case history) and fitted with a probit model predicting $s_i=D$ and trained on defendants in the pre-IEM period. Then:

$$\mathbb{E}[Y_{EM}(k=R\rightarrow EM)]\approx\frac{1}{N(s=EM)}\sum_{i|s_i=EM}\frac{\hat{P}_i(s_i=R|x_i)}{\sum_{i|s_i=EM}\hat{P}_i(s_i=R|x_i)}\times Y_i$$

$$\mathbb{E}[Y_{EM}(k=D\rightarrow EM)]\approx\frac{1}{N(s=EM)}\sum_{i|s_i=EM}\frac{\hat{P}_i(s_i=D|x_i)}{\sum_{i|s_i=EM}\hat{P}_i(s_i=D|x_i)}\times Y_i$$

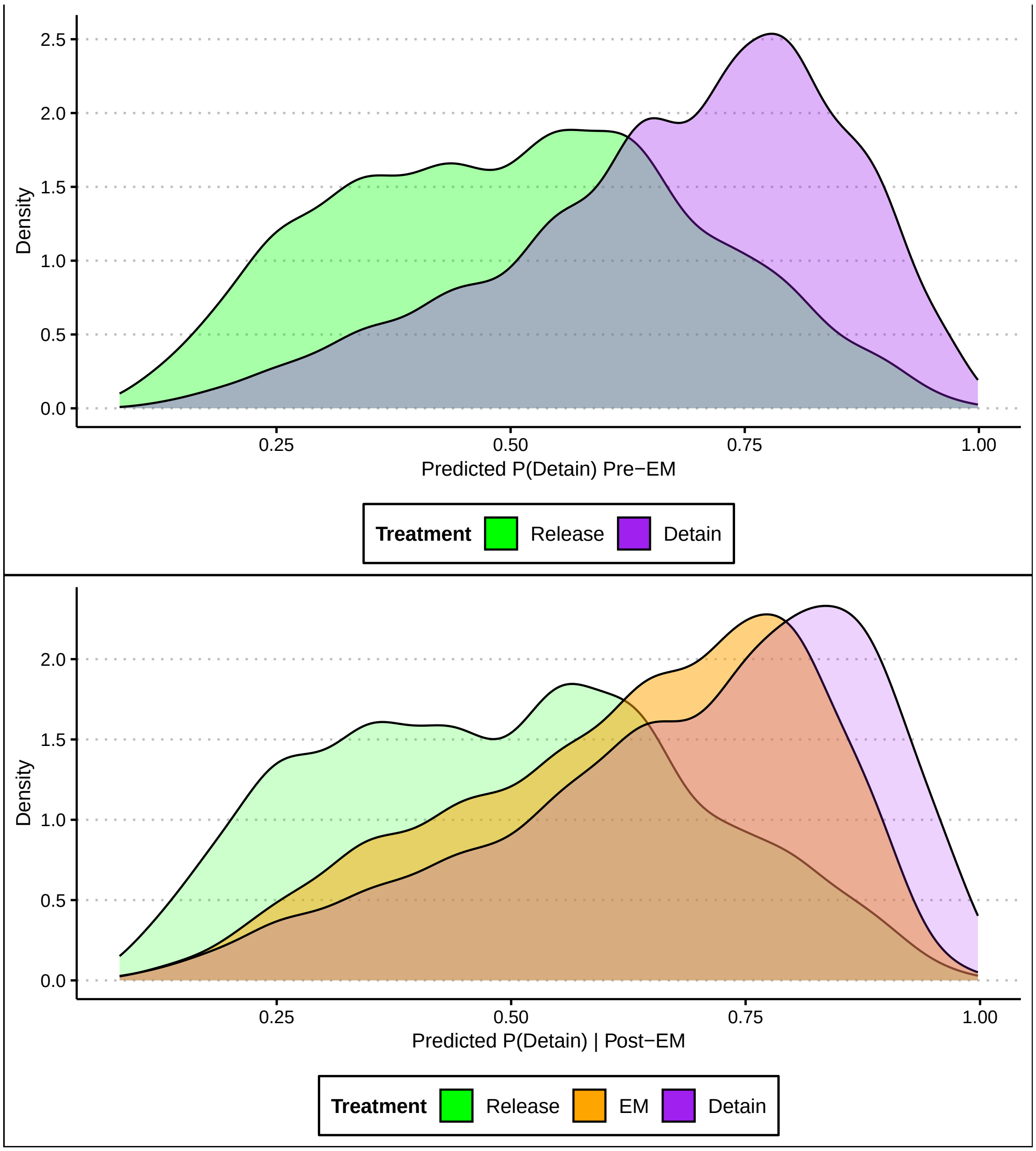
Putting the pieces together:

$$
\begin{aligned}
\tau^R = & \frac{1}{N(s=EM)} \sum_{i|s_i=EM} \frac{\hat{P}_i(s_i=R|x_i)}{\sum_{i|s_i=EM} \hat{P}_i(s_i=R|x_i)} \times Y_i \\
& - \frac{1}{(1-\omega_R)} [\frac{1}{N(s=R,t=0)} \sum_{i|s_i=R,t=0} Y_i - \omega_R \frac{1}{N(s=R,t=1)} \sum_{i|s_i=R,t=1} Y_i]
\end{aligned}
$$

$$
\begin{aligned}
\tau^D = & \frac{1}{N(s=EM)} \sum_{i|s_i=EM} \frac{\hat{P}_i(s_i=D|x_i)}{\sum_{i|s_i=EM} \hat{P}_i(s_i=D|x_i)} \times Y_i \\
& - \frac{1}{(1-\omega_D)} [\frac{1}{N(s=D,t=0)} \sum_{i|s_i=D,t=0} Y_i - \omega_D \frac{1}{N(s=D,t=1)} \sum_{i|s_i=D,t=1} Y_i]
\end{aligned}
$$

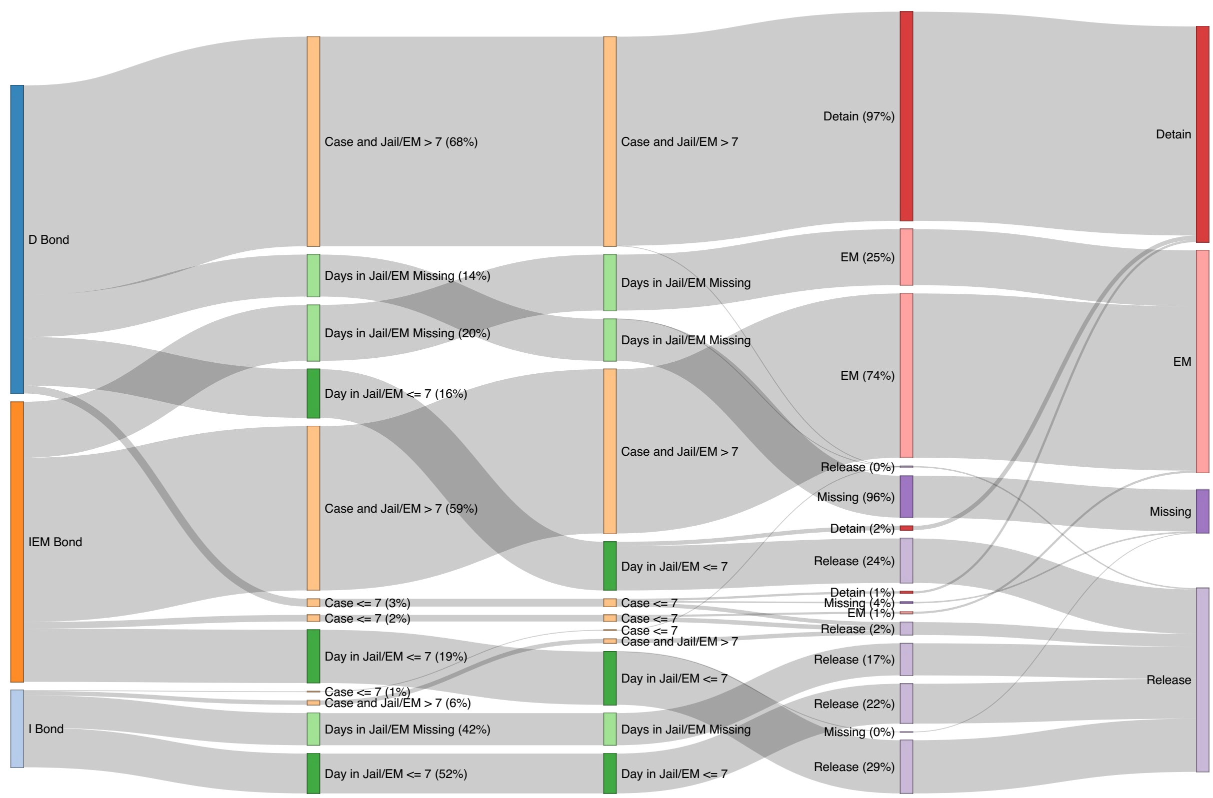
B Appendix B: Additional Figures and Tables

Figure B.1: Distribution of Defendants by Treatment, Pre- and Post-IEM



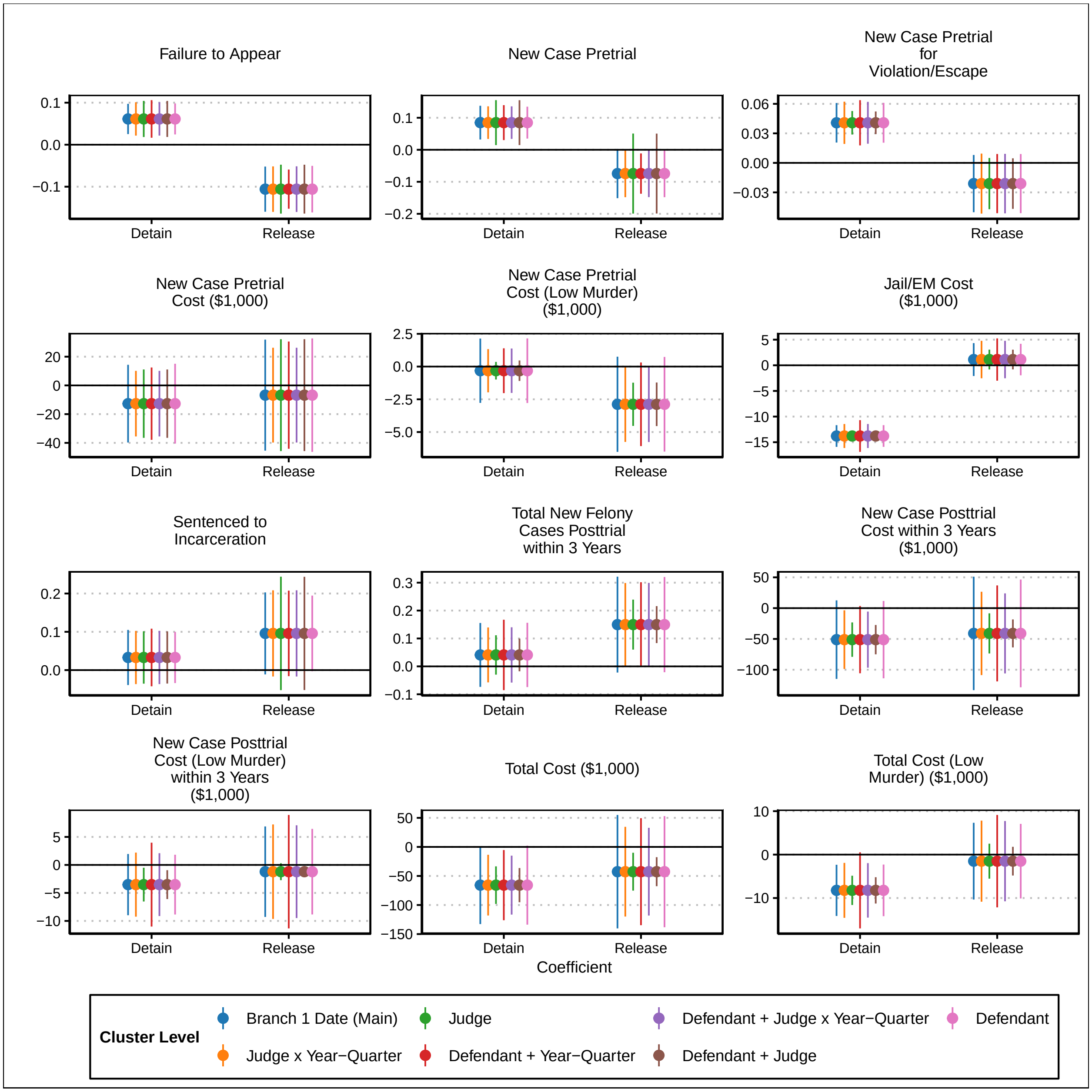
Note: Figures display the distributions of pretrial treatments during the period (Detain, Release) for pre-IEM (2009-2012) and post-IEM (July 2013 - 2015). X-axis is the defendant's predicted likelihood of being detained in the pre-IEM period based on their case observables. Coefficients for predicting likelihood of detention are recovered from regressing detention on defendant observables in the Pre-IEM period, then predicted values are computed using the coefficients on data from the Pre-IEM period (top) and Post-IEM period (bottom).

Figure B.2: Flow Chart from Bond Type to Treatment Type



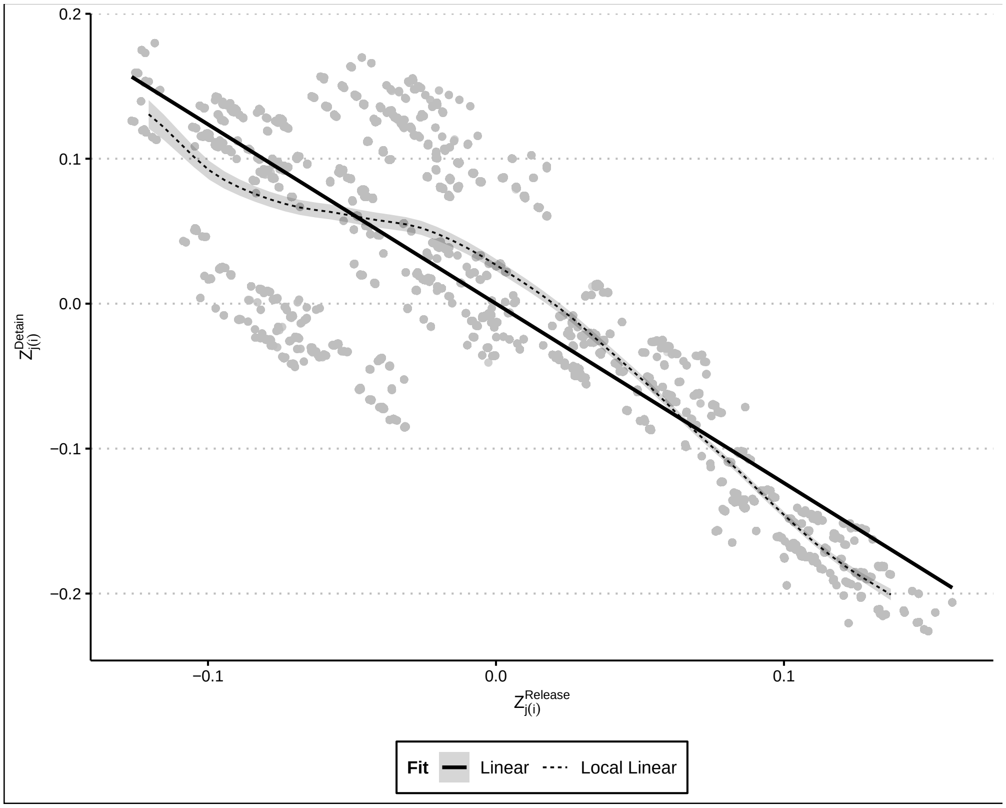
Note: Figure displays classification of bond types to release types using the 7 day cutoff of the main specification for felony cases in the main sample, but including those dropped for having a D-bond and missing days in jail. The left-most nodes are the bond types assigned by the judge and the right-most node contains the treatment classification. The second nodes indicate the cutoff rule which resulted in the bond type being classified as the treatment with the percent of the bond type falling under that classification (e.g., the top node indicates that 69% of D-bond had case durations of jail/EM durations greater than 7 days). The fourth nodes indicate what share of the treatment is contributed by the second node (e.g., 97% of detained defendants are those which had a D-bond and case and jail/EM duration more than 7 days).

Figure B.3: 95% Confidence Intervals under Alternative Clustering



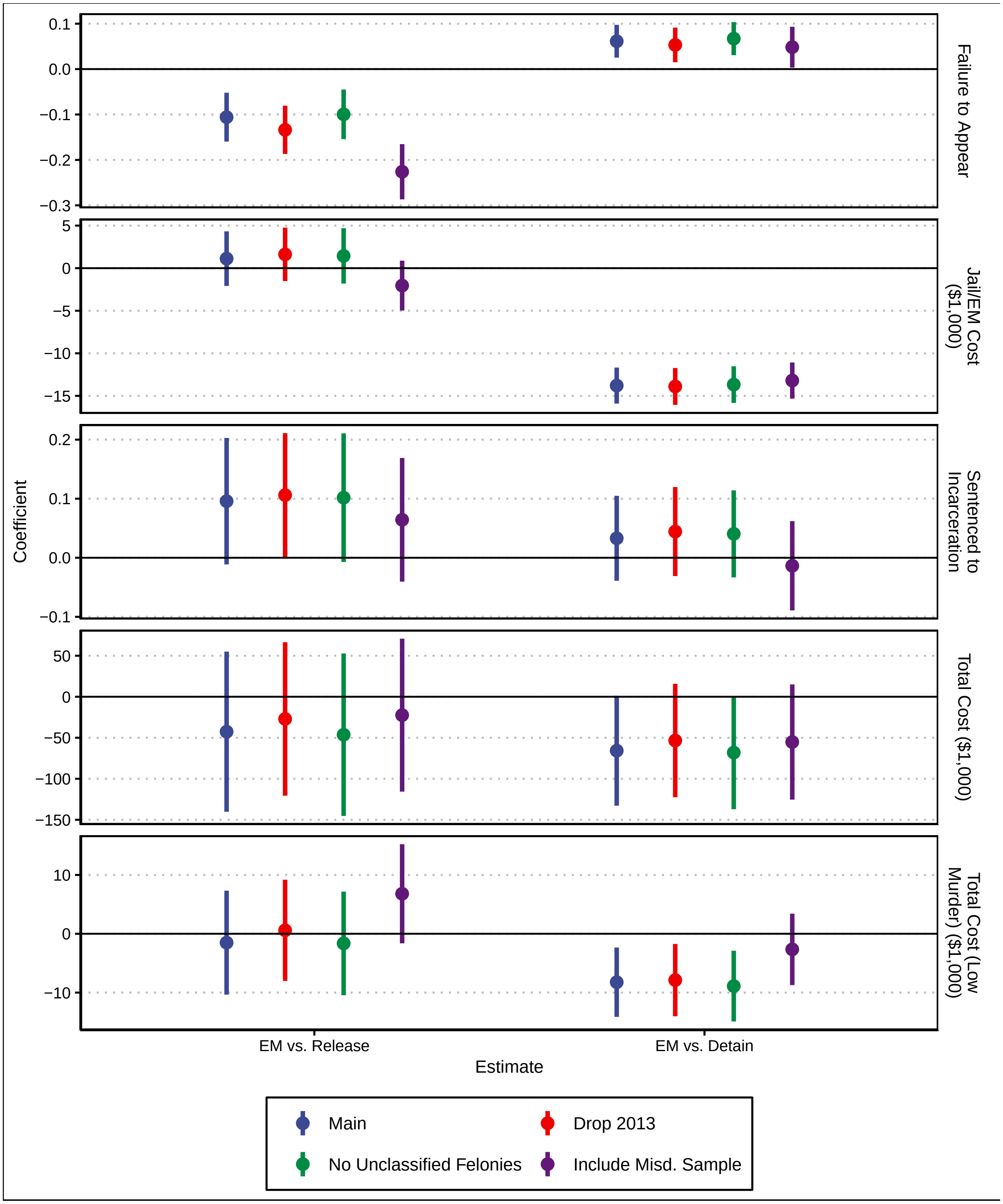
Note: Figure displays the main 2SLS coefficients with 95% confidence intervals using alternative clustering for main outcomes.

Figure B.4: Testing for Linearity Between Predicted Treatments



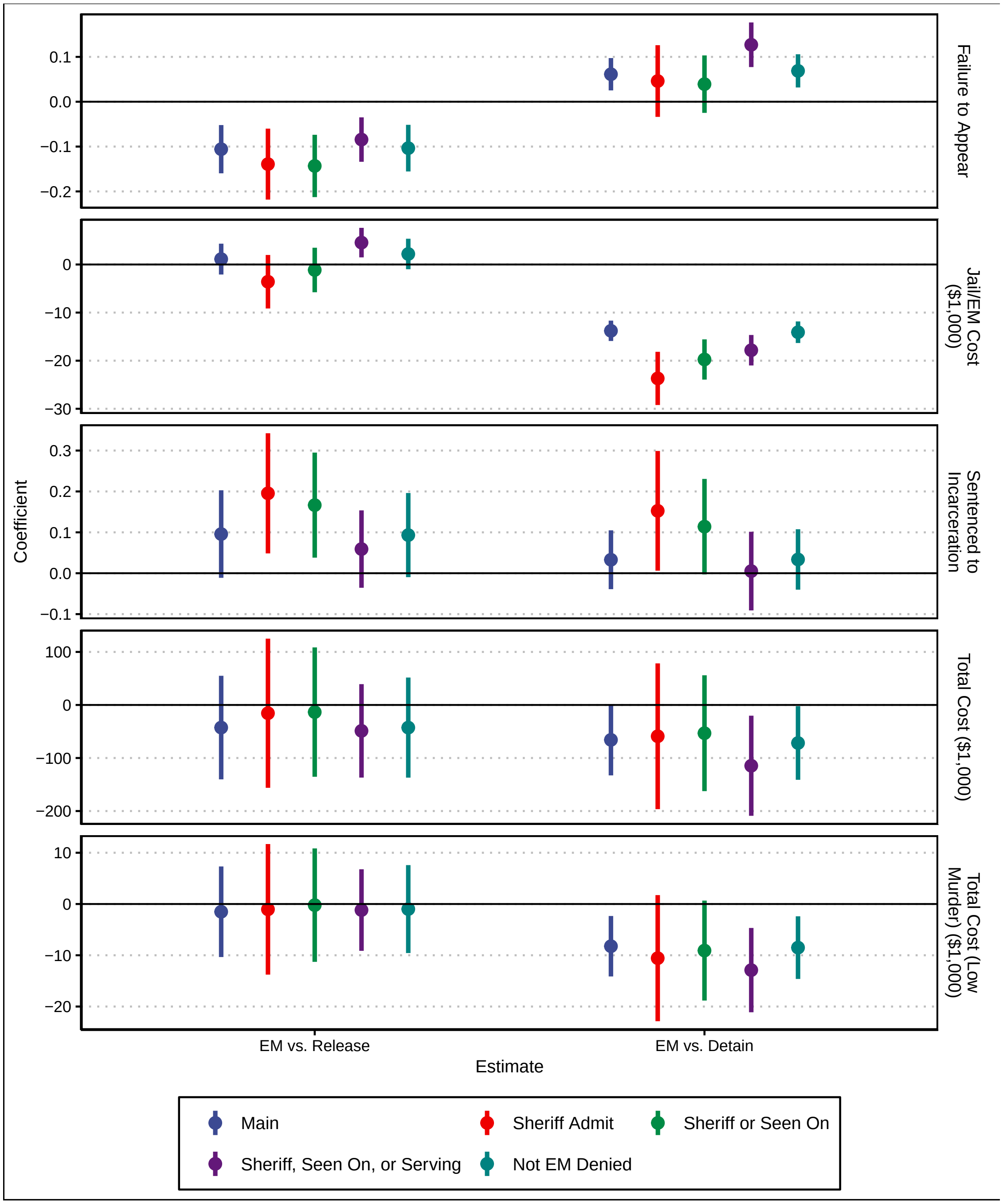
Note: Figure displays scatter plot of observations by values of CJIVE judge propensities for Release and Detention as well as their relationship with a linear fit (solid line) and nonlinear fit (dashed line), Nonlinear fit is computed using local linear regression with bandwidth=0.025.

Figure B.5: Treatment Effects for Alternate Samples



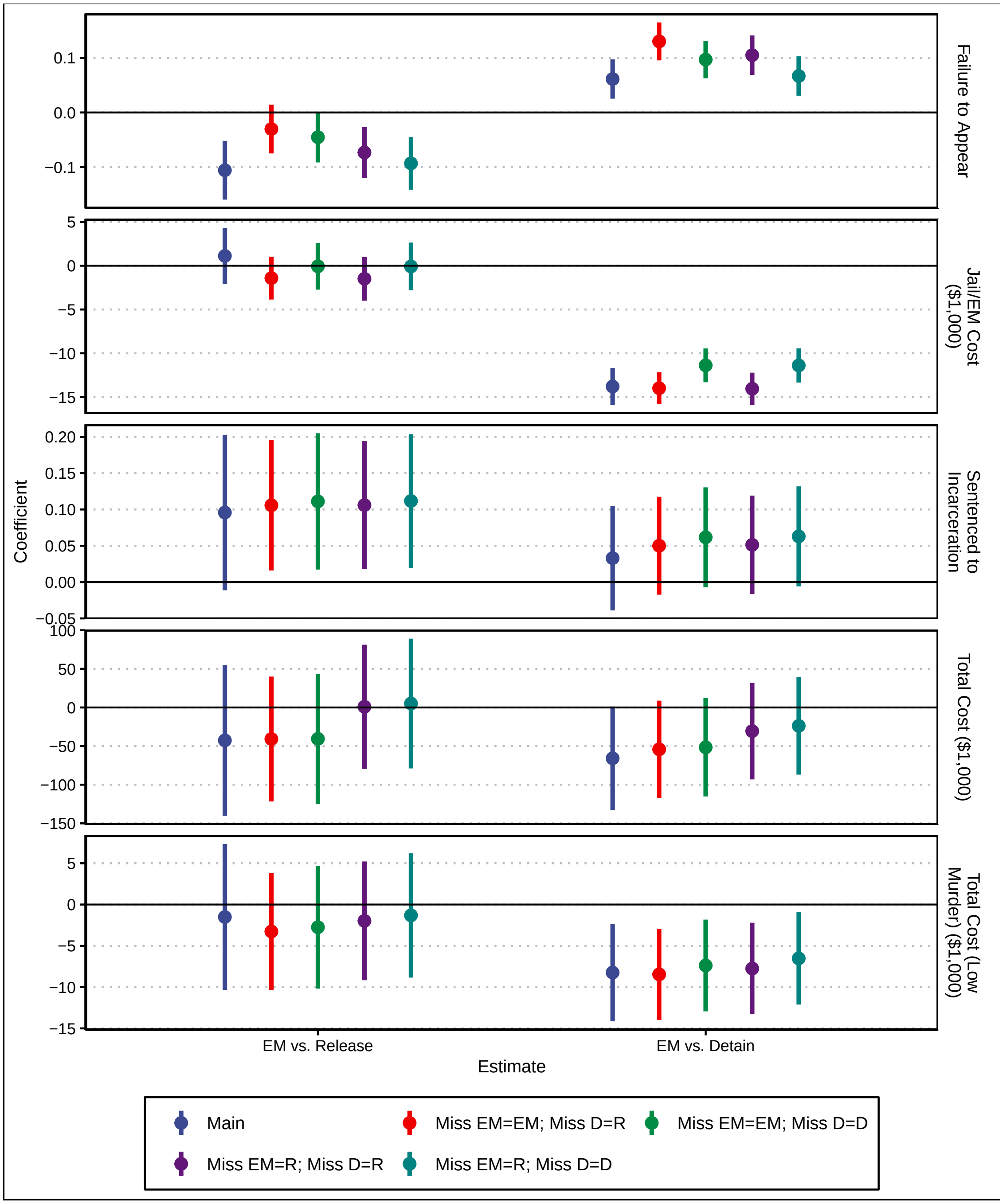
Note: Figure displays the 2SLS estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right). 95% confidence intervals are constructed from standard errors clustered at the branch 1 date level.

Figure B.6: Treatment Effects for Recoded Treatments using Disposition Codes



Note: Figure displays the 2SLS estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right) under different re-codings of pretrial treatments using disposition codes to determine if a defendant was assigned to EM. "Sheriff Admit" means the defendant was explicitly noted to have been admitted into the sheriff's EM program; "Sheriff or Seen On" allows for if the defendant was explicitly noted to be on EM; "Sheriff, Seen On, or Serving" allows for if the defendant was explicitly noted to be serving a monitoring program; and "Not EM Denied" includes "Sheriff, Seen On, or Serving" defendants but excludes any defendant explicitly noted to not be admitted to EM (with bail set to stand). 95% confidence intervals are constructed from standard errors clustered at the branch 1 date level.

Figure B.7: Treatment Effects for Recoded Missing Treatments



Note: Figure displays the 2SLS estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right) under different re-codings of pretrial treatments for cases with missing jail data. 95% confidence intervals are constructed from standard errors clustered at the branch 1 date level.

Table B.1: Incidence Cost from Data to Miller et al. (2021) Types and Costs

Coded Charge Type	Applied Weight	Miller et al. (2021) Crime Type	Incidence Cost ($)									
			Medical	Mental	Productivity	Property	Public Service	Adjudication Sanction	Perpetrator Work Loss	Subtotal Tangible	Quality of Life	Total
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Felony Murder	1.00	Murder	12735	11976	1828638	197	148832	478072	177869	2658319	5150836	7809155
Felony Sexual Assault	0.50	Rape Police-reported	3333	6504	7178	176	901	44660	18409	81161	319632	400793
Felony Sexual Assault	0.50	Other sexual assault	706	1580	1760	68	51	328	135	4627	82507	87134
Felony Violent	0.50	Robbery Police-reported	1959	196	4639	1285	1321	13784	5928	29112	14656	43768
Felony Violent	0.50	Assault Police-reported	2090	403	2292	79	4315	6172	2286	17635	21149	38784
Misdemeanor Domestic Violence	1.00	Intimate partner violence	727	193	1336	65	13	269	207	2810	25440	28251
All Property	0.05	Arson	2647	45	3389	19519	4002	2596	505	33008	6430	39438
All Property	0.25	Burglary Police-reported	0	0	39	2882	582	935	931	5369	0	5369
All Property	0.25	Larceny/theft Police-reported	0	0	31	1052	901	2570	226	4780	0	4780
All Property	0.25	Motor vehicle theft Police-reported	0	0	118	7219	715	1964	767	10783	0	10783
All Property	0.05	Fraud	0	0	57	1854	73	52	16	2053	0	2053
All Property	0.15	Vandalism	0	0	0	390	23	688	248	1349	0	1349
All Weapon	1.00	Weapons carrying	0	0	0	0	79	2573	1073	3725	0	3725
Misdemeanor Other	0.16	Prostitution/pandering	0	0	0	0	79	257	108	444	0	44
All Drug	1.00	Drug possession/sales	0	0	0	0	5046	3599	1502	10147	0	10147
Misdemeanor Other	0.16	Gambling	0	0	0	0	79	257	108	444	0	444
Misdemeanor Other	0.16	Liquor laws	0	0	0	0	79	1228	512	1819	0	1819
Misdemeanor Other	0.16	Drunkenness	0	0	0	0	79	1228	512	1819	0	1819
Misdemeanor Other	0.20	Disorderly conductd	0	0	0	0	79	1228	512	1819	0	1819
Misdemeanor Other	0.16	Vagrancy	0	0	0	0	79	1228	512	1819	0	1819
All Bond Violations	1.00	Curfew/loitering violations	0	0	0	0	79	1228	512	1819	0	1819
All Traffic	0.20	Impaired driving	1208	140	5527	2548	31	1088	107	10649	17355	28004
All Traffic	0.80	OTHER TRAFFIC	0	0	0	0	79	1228	512	1819	0	1819
All Other	1.00	OTHER GENERAL	0	0	0	0	79	1228	512	1819	0	1819
Felony Murder (Low Cost)	1.00	MURDER RECODED AS Rape Police-reported	3333	6504	7178	176	901	44660	18409	81161	319632	400793
All Escape	1.00	ESCAPE RECODED AS Curfew/loitering violations	0	0	0	0	79	1228	512	1819	0	1819
Felony Attempted Murder	1.00	ATT MURDER RECODED AS Other sexual assault	706	1580	1760	68	51	328	135	4627	82507	87134

Note: Table displays the charge types recovered from the court data (Column (1)) and associated crime types (Column (3)) and costs (Columns (4) - (13)) from Miller et al. (2021) Table 5. Column (2) displays the applied weight to the Miller et al. (2021) crime type in order to map multiple crime types to a single recovered charge type that could be recovered from the court data. Not all charge types had perfect mappings to the incidence costs so similar categories / weights were applied — if no category was avaliable Column (3) contains the crime type used in the form of: [Court data charge] recoded as [crime type].