

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 deter pretrial misconduct. 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 from two-stage least square, marginal treatment effect, and time-series analyses 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 not clear. For example, EM could reduce pretrial misconduct relative to release through deterrence, as a defendant's movement is being 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. 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 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

¹Grenet, 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.

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. 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, but has no sizable effect on the case outcomes or post-trial recidivism. In aggregate, EM does not significantly reduce total costs relative to release.

Compared with detention, EM allows for more low-level pretrial misconduct, but not more costly pretrial misconduct. Furthermore, EM actually has a small negative effect on post-trial crime compared with detention. Direct costs prove to be a significant factor: EM results in thousands of dollars in savings per defendant compared with detention. After aggregating costs under the preferred specification, 2SLS estimates indicate that moving a defendant from detention to EM would save around \$10,000 in marginal costs, or around 25% of the average marginal cost for detained defendants.

With few exceptions, I cannot reject that treatment effects are constant, indicating that 2SLS likely recovers causal estimates. Nevertheless, to explore policy counterfactuals, I explore treatment effect heterogeneity on both margins using a marginal treatment effects (MTE) framework with ordered treatments. With this, I construct policy counterfactuals for the expansion of EM to replace release and the expansion of EM to replace detention, equivalent to removing the release or detention options from judges: the average treatment effect of EM on the released ("ATR") and average treatment effect of EM on the detained ("ATD"). Consistent with constant treatment effects, the ATR estimates for the marginal total cost savings from moving released defendants to EM would be close to zero, economically smaller than the 2SLS estimates. Similarly, the ATD estimates suggest that expanding EM to replace detention would reduce marginal costs by over \$10,000 per defendant, economically

larger than the 2SLS estimates. Adjusting for changes in the probability of detection of crime in the pretrial period when on EM produces even larger suggested savings.

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 would have been 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 and MTE results, further reinforcing the primary findings. On net, the expansion of EM likely resulted in a net savings, driven primarily by cost savings 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 Schargrodsy (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. municipality by recovering 2SLS and marginal treatment effect estimates, informing policy on the impact of expanding EM on either margin.³

Much like Rose and Shem-Tov (2021), I build on Heckman, Urzua, and Vytlacil (2006) by identifying marginal treatment response (MTR) functions in ordered treatment environments using variation in the probability of adjacent treatments, but I focus on point-identification and estimation of MTRs over common support using continuous instrument. By providing a

²Additional 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.

³More generally, this builds on the literature on alternatives to incarceration. For work on the effect of probation, parole, and alternatives to detention, see Hawken and Kleiman (2009), Kilmer et al. (2013), Kuziemko (2013), Rose (2021), LaForest (2021), and Arbour and Marchand (2022).

method for estimating MTEs in an ordered treatment environment, this paper contributes to the literature on marginal treatment effects and identification with multiple treatments (Kirkeboen, Leuven, and Mogstad (2016), Kline and Walters (2016), Mountjoy (2022)).⁴ More broadly, I exploit the quasi-random variation in judge assignments in a criminal justice environment with multiple treatments to study selection patterns and the degree of treatment effect heterogeneity, and I find generally constant treatment effects.⁵

Finally, this work 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)).⁶ 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 and 4 present the empirical strategy, results, and robustness tests for the 2SLS and MTE analyses, respectively. Section 5 present additional estimates using a time-series design. Section 6 concludes.

⁴Rose and Shem-Tov (2021) develops a similar method, but their focus is on bounding MTRs (building on Mogstad, Santos, and Torgovitsky (2018)). See also Björklund and Moffitt (1987), Heckman and Vytlacil (1999), Dahl (2002), Heckman and Vytlacil (2005), Heckman, Urzua, and Vytlacil (2008), Moffitt (2008), Carneiro and Lee (2009), Brinch, Mogstad, and Wiswall (2017), Lee and Salanié (2018), Mogstad and Torgovitsky (2018), Mogstad, Torgovitsky, and Walters (2021), Cornelissen et al. (2018), and Bhuller and Sigstad (2022). See also Cornelissen et al. (2016) and Andresen (2018).

⁵See Mueller-Smith (2015), Bhuller et al. (2020), Arteaga (2021), Norris, Pecenco, and Weaver (2021), Humphries et al. (2023), and Kamat, Norris, and Pecenco (2023) for work in the criminal justice system with multiple treatments. See Gelbach and Bushway (2010), Gupta, Hansman, and Frenchman (2016), Leslie and Pope (2017), Dobbie, Goldin, and Yang (2018), Stevenson (2018), Arnold, Dobbie, and Yang (2018), Agan, Doleac, and Harvey (2021), and Arnold, Dobbie, and Hull (2022) for ‘judge’ designs in pretrial context. Exploiting ‘judge’ random assignment is a common identification strategy: Kling (2006), Doyle Jr. (2008), Aizer and Doyle (2015), Dobbie and Song (2015), Jordan, Karger, and Neal (2022), Goncalves and Mello (2022), Gross and Baron (2022), and Frandsen, Lefgren, and Leslie (2023). See Dobbie and Yang (2021) for a discussion of the U.S. pretrial system. See Myers (1981) for an early analysis of the economics of pretrial detention. See Chyn, Frandsen, and Leslie (2023) for a review of the ‘judge’ design literature.

⁶See also Beraja, Yang, and Yuchtman (2020), Beraja et al. (2021), and Acemoglu (2021) on AI and mass surveillance. Barbaro et al. (2022) discusses the post-COVID rise of surveillance and remote-work.

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 (CCJ). 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.

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 handles almost all felony cases in Cook County and operates every day, excluding murder and violent sex offenses.⁷ At bond court, the sitting bond court judge determines the bond conditions for a defendant, namely bond type and amount.⁸

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 7 were active during the period of study.⁹ 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 no scheduling relationship between bond court

⁷On 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.

⁸See here for a schedule of the bond courts in Cook County. Defendants are generally processed at CCJ after they have their bond hearing at the court.

⁹Active 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 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 the 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.¹⁰

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 — see Appendix A.1 for more details on bond types and background on IEM’s introduction.¹¹

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

¹⁰However, as with all bond types discussed, 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 (2022)), 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. (2021)).

¹¹EM 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. There are two additional bond types in the data A and C bonds, but they account for a minute share of bonds.

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.4 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.¹² This system is often referred to as EM coupled with house arrest, and it is common across the United States (Weisbord 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.¹³

Defendants on EM agree to warrantless searches of their residence while on EM, and the 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 (Office (2020)). Similar to other bail conditions of release (e.g., a condition of all bond releases is that the defendant appears in

¹²See this information sheet for the rules and information sheet for the EM program in 2020. While the domain of EM monitoring is generally one's home, exceptions can be made in advance for work, school, or other reasons (Federation (2020)), it but requires 2 days prior approval. The Sheriff's EM program is by far the most common form of EM (Green (2016), Federation (2020)). See Appendix A.1 for a discussion of other EM programs. 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 (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 2013-2015 system.

¹³While I do not observe whether or when the defendant has the EM system set up in their home, 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. I test the robustness of my results to modifying the treatment definitions using these disposition codes in Appendix A.4.

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 ($S \in \{1, 2, 3\}$). At the lowest level, defendants can be “released” ($S = 1$) 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” ($S = 2$), 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” ($S = 3$) 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 30% 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.1 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).¹⁴ 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 93% 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 (Illinois Department of Corrections) 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).

¹⁴The 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 B.2 and B.3, respectively.

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.

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.4. 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 (5,318), and cases assigned D-EM bonds (6,111), which are utilized in Appendix A.4.¹⁵ The final felony sample contains

¹⁵Though misdemeanor arrests are an important part of the criminal justice system (Kohler-Hausmann (2013), Mayson and Stevenson (2020)), EM was primarily used on felony cases. The main felony sample also excludes D-EM cases.

65,430 defendant-bookings. 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 35 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 23 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 at a department of corrections, this is much higher for detained defendants (62%) compared with EM (31%), while it is rare for release (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

To evaluate the relative costs and benefits of EM versus release and detention, I quantify their effects on multiple dimensions and then aggregate them to provide a more complete comparison.

Direct Costs Direct costs amount to operating each treatment type. Jails cost about \$100-\$200 per detainee per day to maintain.¹⁶ 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)).¹⁷ 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.

Crime Costs Crime costs amount to the social cost of pre- and post-trial crime and misconduct. Pretrial detention should reduce pretrial crime costs by incapacitating defendants in jail compared with EM and release. EM should prevent pretrial misconduct through deterrence, as it increases the likelihood that a crime or violation will be detected and punished compared with a released defendant. Additionally, the house-arrest aspect of EM implies a partial incapacitation effect in addition to deterrence.

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 damaging social ties, economic opportunities, and health outcomes and increasing housing insecurity

¹⁶For example, in 2011, CCJ cost about \$90 per defendant-day, while in 2021 it cost \$223 per defendant-day (Institute (2022)).

¹⁷Marginal 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.

(CGL and Appleseed (2022)).¹⁸

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).¹⁹ 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 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 in Heckman et al. (2010).²⁰ Importantly, new cases only account for crimes that are detected, so to recover the effect on total crime, not just detected crimes, I account for the fact that detection depends on the probability of detection, which is higher for EM compared with release, in Section 4.2.4. In addition to crime costs, failures to appear in court are also costly, and I 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 result from defendants being incarcerated due to a detected violation or being prosecuted or fined for pretrial misconduct. Direct costs account for the first-order punishment costs of detention and EM. However, because a defendant can be rearrested and placed in jail if released or on EM, release can increase punishment costs if pretrial misconduct is detected and penalized. Whether EM reduces punishment costs

¹⁸Alternatively, EM may reduce future crime through an individual deterrent effect (increasing defendants' expectations of punishment) — though the research on EM versus detention (which should have a similar effect) has not found evidence consistent with such a mechanism (Williams and Weatherburn (2022)).

¹⁹I 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.

²⁰For 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.

compared with release depends on whether being placed on EM increases or decreases the likelihood of receiving a new case pretrial. While EM aims to reduce pretrial crime through deterrence and partial incapacitation, it may result in higher punishment costs through two mechanisms. First, on EM, a larger set of activities can be penalized as crimes (e.g., leaving one's house, removing the bracelet).²¹ Second, because EM raises the probability of any misconduct being detected through surveillance and warrantless searches, if defendants do not change their behavior enough in response to the increase in detection, then observed pretrial misconduct, and thus punishment costs, will increase on EM relative to release. Overall, if the rules of EM are too stringent, unforgiving, or unreasonable such that defendants are punished for non-criminal activity (Weisburd et al. (2021)), then EM's punishment cost may be high.

Furthermore, treatments may vary in punishment costs due to being differentially coercive. Because almost all convictions are done through plea deals, more severe pretrial treatments may increase the likelihood of being found guilty and sentenced to incarceration.²² All else equal, post-trial incarceration increases punishment costs as convicts are expensive to imprison — around \$33,000 per inmate in Illinois in 2015 (Mai and Subramanian (2017)).

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.

Aggregating Costs To aggregate these costs into a single measure of the total change in social costs from placing a defendant on release, EM, or in detention, I use the following formula to compute the marginal cost of using release or detention relative to EM:

²¹Because EM does not allow for perfect detection of defendants' misconduct, it applies sanctions to complementary but non-criminal activity (e.g., leaving one's home without permission), broadly consistent with principal-agent models of multitasking (Holmstrom and Milgrom (1991)). Any increase in new cases from violations can be seen as a cost of monitoring the agent (defendant) beyond the direct costs.

²²See Leslie and Pope (2017), Stevenson (2018), and Dobbie, Goldin, and Yang (2018). Bargaining power is a significant factor in case outcomes (Silveira (2017)).

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

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. Also, the portion of punishment costs due to punishment for detected pretrial misconduct is included in pretrial crime costs. Both pre- and post-trial crime costs can be adjusted for the different detection probabilities, as done in Section 4.2.4. While I cannot quantify the personal or financial costs, they are likely larger for higher levels of treatment, i.e., $\Delta \text{Personal/Financial Costs}^{EM,R} \gg 0$ and $\Delta \text{Personal/Financial Costs}^{EM,D} \ll 0$.

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

additional assumptions discussed below, judge assignment can be used as a valid instrument for pretrial treatment. The primary specification used in this section is:

$$Y_{ijc} = \beta_R Release_{ijc} + \beta_D Detain_{ijc} + \theta X_{ijc} + \epsilon_{ijc} \quad (2)$$

$$\begin{aligned} Release_{ijc} &= \alpha_R^R z_{j(i)}^R + \alpha_D^D z_{j(i)}^D + \lambda_R X_{ijc} + e_{R,ijc} \\ Detain_{ijc} &= \alpha_D^R z_{j(i)}^R + \alpha_D^D z_{j(i)}^D + \lambda_D X_{ijc} + e_{D,ijc} \end{aligned} \quad (3)$$

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 release or detention, respectively. $z_{j(i)}^R$ and $z_{j(i)}^D$ are measures of judge j 's propensity to assign the defendant to release and detention, respectively, and are used to instrument for the endogenous treatments. β_R and β_D are the causal effects of being assigned to release or detention relative to being assigned to EM, and I use two-stage least squares (2SLS) to recover estimates of both parameters.

For the principal 2SLS analysis, I construct $z_{j(i)}^R$ and $z_{j(i)}^D$ using the cluster-jackknife instrumental variables estimator (CJIVE) method introduced by Frandsen, Leslie, and McIntyre (2023), which extends Kolesár (2013)'s UJIVE method to environments where treatment is determined in clusters — in this case, the cluster is the date a defendant is in bond court which determines which judge they see. Beyond constructing judge-specific propensities for releasing and detaining defendants, CJIVE avoids the constructed instruments for defendant i being correlated with the outcome and treatment for defendants seen on the same day as defendant i , conditional on controls, X_{ijc} . In the main specification, X_{ijc} includes year-quarter fixed effects, the log number of prior Branch 1 cases (plus 1 to ensure finite values), and an indicator for whether the defendant had any previous felony cases resulting in a guilty finding.

3.1.1 Exogeneity

To be a valid instrument, the judge's propensity to assign a defendant to a specific treatment must 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 judge propensities, and the relationship between observables and judges can be empirically tested.

Table 2 displays the results for this test by regressing the defendant's treatment status (whether they were released in Column (1), on EM in Column (3), or detained in Column (5)) and their judge's leave-out propensity for release and detention (Columns (2), (4), and (6)) on the defendant and case characteristics, conditional on year-quarter fixed effects. While Columns (1), (3), and (5) show significant evidence for selection on observables into treatments, Columns (2) and (6) show that defendant and case observables have no significant relationship with the judge propensities for release or detention. The joint F-tests for whether defendant and case characteristics are related to judge propensities all fail to reject that there is no relationship at conventional levels ($p=0.33$ for $z_{j(i)}^R$ and $p=0.37$ for $z_{j(i)}^D$).²³ However, the F-test does reject for $z_{j(i)}^{EM}$, with an F-statistic of 3.5, driven by economically small but statistically significant coefficients on weapon and drug delivery charges. Nevertheless, the joint F-test on the stacked regression of judge propensities on observables fails to reject a jointly significant relationship between observables and judge propensities ($p=0.27$).

Additionally, if the judge influences defendant outcomes through means other than their pretrial treatment, then this violates the exclusion restriction. Fortunately, Branch 1 operates

²³I also conduct an additional test for the validity of the calendar system: if judges do not select into seeing specific case types, I would expect that time fixed effects (i.e., year-quarter effects) explain some of the variation in which judge a defendant sees. In contrast, defendant and case observables would explain none of the variation. I test this by estimating a multinomial logistic regression model with judges as the outcome and using defendant observables as one set of regressors and time fixed effects as a different set of regressors. Consistent with random assignment, defendant characteristics explain none of the variation in judge assignment, while time effects explain a significant portion (Table B.2).

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.²⁴ 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

Figure 3 visualizes the support of both (residualized) leave-out propensities, which range from -0.12 to 0.15 for $z_{j(i)}^R$ and from -0.22 to 0.17 for $z_{j(i)}^D$, and the nonparametric (local linear) relationship between the relevant instrument and the probability of receiving that treatment. Moving across the support of $z_{j(i)}^R$ induces a 22pp change in probability of receiving release and moving across the support of $z_{j(i)}^D$ induces a 37pp change in probability of receiving detention, consistent with strong first stage relationships and highly relevant instruments.

The first rows in Panels A and B of Table 3 display the first stage coefficients on the endogenous variables for the pooled sample and across five subsamples. In all subsamples, the relevant instrument has a strong relationship with the relevant endogenous variable (e.g., $z_{j(i)}^R$ for *Release* = 1). Furthermore, Sanderson and Windmeijer (2016) test for weak instruments using judge indicators as instruments produce sufficiently large conditional F-statistics of 110.8 and 134.2 for release and detain, respectively.

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_R and β_D can be interpreted as the causal effect of release compared with EM and detention compared with EM. In Section 3.3, I test the validity of these assumptions and provide additional estimates.

²⁴In the sample, only 0.1% of defendants saw their bond court judge in some capacity later in their case. However, 15% of observations see the same bond court judge in a future case (though this 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.

3.2 Results

Table 4 displays estimates of the effect of being assigned to EM versus release ($-\beta^R$) and the effect of being assigned to EM compared with detention ($-\beta^D$). Panels correspond to different specifications, beginning with simple OLS (equation (2)) in Panel A and 2SLS results with a first stage using equation (3) and the leave-out propensity instruments in Panel B. 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 (2023)), and Figure B.5 displays results using alternate clusters.

3.2.1 EM vs. Release

Compared with release, EM decreases the likelihood of a failure to appear in court (FTA) by -12.4pp ($p<0.01$) and the likelihood of a new case pre-trial by -10.8pp ($p<0.05$), and it has an imprecise negative effect on cases for violations (-2.8pp ($p=0.08$)). The OLS estimates understate these effects on FTAs and new cases (-5.8 ($p<0.01$) and 0.5 ($p>0.1$)), consistent with positive selection into release. 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 -\$3,544 ($p>0.1$) but is highly 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 similarly sized but much more precise (-\$3,911 ($p<0.05$))).

For case outcomes, while OLS results suggest EM significantly increases the likelihood of being sentenced to post-trial incarceration by 10.8pp ($p<0.01$), 2SLS results are small and noisy (2.9pp ($p>0.1$)). Because release is significantly cheaper than EM, 2SLS estimates indicate that using EM costs about \$628 ($p>0.1$) per defendant in direct costs. Post-trial, EM produces a small positive effect on total new felony cases within three years (0.17

($p=0.08$)), but a noisy negative effect on total post-trial new case costs within three years using full murder costs (-\$44,034 ($p>0.1$)) and virtually no effect using low murder costs (-\$765 ($p>0.1$)).

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, \$8,247 ($p=0.06$) per defendant, 2SLS estimates indicate that EM reduces costs compared with EM though estimates are imprecise and cannot reject no savings (-\$44,918 (95% CI=[-149,639 , 59,804])). Using the low murder cost, the magnitude of EM's savings is much smaller but still highly imprecise (-\$3,644 (95% CI=[-13,230 , 5,941])). Collectively, these results suggest that while release allows for more pretrial misconduct than EM, aggregating total costs indicates that EM does not significantly reduce total costs relative to EM. 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 4.9pp ($p<0.05$) and the likelihood of a new case pre-trial by 6.1pp ($p<0.05$) compared with detention, and this includes a 3.7pp ($p<0.01$) increase in the likelihood of a new case for a violation. However, the increase in misconduct is on the extensive rather than intensive margin, as EM has noisy negative effects on new case cost using full and low murder costs (-\$10,411 ($p>0.1$) and -\$1,086 ($p>0.1$)).

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 (-21.8pp ($p<0.01$)). Primarily due to detention's significantly higher costs, 2SLS estimates indicate that EM reduces direct costs by -\$14,044 ($p<0.01$) per

defendant compared with detention. While EM appears to have a positive effect on post-trial crimes, increasing the number of felony cases over three years post-trial by 0.05 ($p>0.1$), this is consistent with a small incapacitation effect of higher post-trial incarceration due to detention. 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 (-\$52,777 ($p=0.12$) and -\$3,146 ($p>0.1$)).

Aggregating costs using (1), I find that EM significantly reduces total costs compared with detention using both full, -\$66,739 ($p=0.06$), and low murder costs, -\$9,552 ($p<0.01$). The smaller and more precise low murder results reject total marginal costs savings of less than -\$3,400 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 intensive margin of pre- and post-trial crime, the sizable direct cost of detention, and a small negative effect on post-trial incarceration costs.

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, Pencenco, 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 4. Additionally, the figure contains results using only judge fixed effect instruments or constructing UJIVE rather than CJIVE judge-propensities, with highly similar results. Furthermore, as shown in Blandhol et al. (2022), the inclusion of controls in 2SLS complicate interpretation of results, however the 2SLS results are nearly identical when judge fixed effects are used as the instruments and no control variables are included.

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. (2023)). As shown in Figure 4 and Panel C of Table 4 which also contains the coefficients for bond amounts, 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 B.6. 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 groups’ 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 B.6). This indicates the duration cutoff used is not driving the results. Overall, these tests suggest that exclusion restriction violations are not likely to be significantly biasing the results such that the overall conclusions are affected.

3.3.2 Constant Treatment Effects

I test for violations of the constant treatment effects assumption using the Sargan-Hansen overidentification test (Sargan (1958), Hansen (1982)). As shown in Table B.3, 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 3 out of 12 outcomes (failure to appear, new case pretrial, and sentenced to incarceration).

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} .²⁵ I also control for the direct influence of W_{ijc} in both the first and second stages to ensure that the instruments induce variation only through disagreements across judges.

Consistent with a degree of 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 4 (“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.

3.3.3 Monotonicity and Compliers

Finally, without constant treatment effects, interpreting the 2SLS results as estimates of the causal effect of EM vs. release and detention is complicated. In particular, stronger monotonicity assumptions are required, specifying how defendants (would) move between treatments in response to different values of the judge instruments. Monotonicity assumptions have been widely studied and criticized, with most work focusing on the case of a single endogenous variable and ‘IA’ monotonicity (Imbens and Angrist (1994)). However, the 2SLS

²⁵Defendant 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, as used in Table 2, in addition to indicators for misdemeanor charge types: drug possession, domestic violence, property, and other misdemeanors. Note that W_{ijc} are not fully saturated interactions due to the large number of observables. 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. Note that this instrument also satisfies relevance, with stronger first-stages, and I reject that they are weak instruments using the Sanderson and Windmeijer (2016) test. Conditional F-statistics are 20.8 and 34.7 for release and detain, with just under 150 instruments.

results can be interpreted as the (properly) weighted average of causal treatment effects across defendants who are induced into the respective treatment due to the instrument ('compliers'), or the local average treatment effect (LATE), if monotonicity assumptions are satisfied even if effects are heterogeneous.

Using methods developed for single endogenous variable settings (Frandsen, Lefgren, and Leslie (2023)), I recover information on the composition of complier groups. The second to last rows in Panels A and B of Table 3 display the relative frequency of a specific characteristic (e.g., Black defendants) among compliers divided by its frequency in the sample for release and detention. For release, complier compositions do not significantly differ from the average defendant, with defendants with more serious charges (Class X or Class 1 felonies) being slightly underrepresented while those with non-violent charges are slightly overrepresented. For detention, Black defendants are overrepresented (112%), defendants with more serious charges (Class X or 1) are significantly overrepresented (171%) among compliers, and female defendants are significantly underrepresented (57%).

Humphries et al. (2023), Bhuller and Sigstad (2023), and Kamat, Norris, and Pecenco (2023) provide monotonicity assumptions in environments with multiple endogenous variables, under which I can apply a LATE interpretation to the 2SLS results.²⁶ A consistent result in these works is that if treatment effects are constant and there are no defiers — or if defiers are less common than compliers (Chaisemartin (2017)) — then 2SLS recovers the causal effects of interest. However, if treatment effects are heterogeneous, an additional assumption is required for a LATE interpretation to apply, such as unordered partial monotonicity (UPM) (Mountjoy (2022), Humphries et al. (2023)) or latent monotonicity (LM) (Kamat, Norris, and Pecenco (2023)).²⁷

²⁶I discuss Bhuller and Sigstad (2023) in Appendix A.3. Also note that even though judge propensities to release are negatively correlated with their propensity to detain defendants, monotonicity does not require that judge's who are more 'harsh' on the release - EM margin are also more or less 'harsh' on the EM - detention margin.

²⁷Kamat, Norris, and Pecenco (2023) shows that under LM a wider range of compliance types (flows between treatments in response to different judges) are allowed. They provide a test for LM, which adapts the Frandsen, Lefgren, and Leslie (2023) monotonicity and exclusion test to testing for violations of the LM assumptions, in addition to other forms of monotonicity. Under LM, treatment effects are not point identified

In Humphries et al. (2023), the UPM assumption restricts the flows of compliers across treatment 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.²⁸ 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.²⁹ The results are shown in Figure 4 (“HOSSvD Thresholds”), and, in general, they are consistent with the main results. Overall, using this alternative instrument construction produces highly similar conclusions.

These results collectively indicate that the unordered partial monotonicity assumption is unlikely to hold, given the usage of judge propensity instruments. However, treatment effects do not appear to be sufficiently heterogeneous among the population of defendants that could be influenced into different treatments through judge assignment such that this seriously biases the results and influences the main interpretations. This would also be consistent with the Sargan-Hansen overidentification test generally failing to reject and a lack of significantly different effects under the robustness checks for exclusion restriction violations or using different instrument constructions.

but can be partially identified.

²⁸Humphries et al. (2023) provide a test for UPM based on predicted characteristics of individuals moving between treatments, and I reject UPM using this test.

²⁹Specifically, 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 $\zeta_j^{Detain} = \log(\overline{Detain}_j) - \log(1 - \overline{EM}_j - \overline{Detain}_j)$.

4 Effects of Expanding EM

In this section, I explore the policy-relevant counterfactuals of EM being expanded to replace release and expanded to replace detention for a large portion of defendants. This requires going beyond 2SLS and estimating heterogeneous treatment effects across the distribution of defendants' unobservable types and constructing estimates incorporating observable and unobservable differences across defendants. For this, I turn to a marginal treatment effects (MTE) framework, which allows for the construction of policy counterfactuals at the price of much stronger modeling assumptions. In this section, I build on Heckman, Urzua, and Vytlacil (2006)'s generalized ordered choice Roy (1951)-style model to identify MTEs under weaker assumptions and flexibly estimate said MTEs. Then, I construct relevant treatment parameters for policy counterfactuals. For expositional expedience, this section focuses on the case of 3 ordered treatments and the most pertinent assumptions, while Appendix C provides a more detailed discussion of the general identification result, proofs, and estimation method.

4.1 Empirical Strategy

4.1.1 Model

Selection into Treatment The level of pretrial treatment a defendant receives ($S \in \{1, 2, 3\}$) is determined by a randomly assigned judge, and treatments are ordered by their intensity: release ($S = 1$), EM ($S = 2$), and detention ($S = 3$). The judge decides based on the defendant's observables X , the judge's specific preferences over those observables (the instrument), $Z = \text{Judge} \times X$, and a single latent factor, U , which is a defendant's unobserved (to the econometrician, but observed by the judge) resistance to higher levels of treatment.³⁰ Treatment is determined by the single index U crossing multiple thresholds, which are functions of observable (to the econometrician) factors Z, X ($\pi_s(Z, X)$):

³⁰The practical implications of interacting Judge effects and defendant observables to produce the instrument are discussed in Appendix A.5.

$$S = \begin{cases} 1 (\text{Release}), & \text{if } U \geq \pi_1(Z, X) \\ 2 (\text{EM}), & \text{if } \pi_1(Z, X) > U \geq \pi_2(Z, X) \\ 3 (\text{Detain}), & \text{if } \pi_2(Z, X) > U \end{cases}$$

Effectively, this model assumes that conditional on observables X , all judges see a defendant's resistance u , drawn from $U \sim \text{Unif}[0, 1]$, and they would all agree on the defendant's relative severity — meaning all judges would agree on the ranking of defendants in terms of who should receive relatively higher and relatively lower treatments. However, each judge sets different cutoffs for assigning a defendant to release, EM, or detention, which can be a function of X . Essentially, the defendant is assigned to a treatment higher than s if the observable factors pushing them higher, as measured $\pi_s(Z, X)$, are larger than their unobserved resistance to treatment, as measured by U . Note that assumptions on U is similar to the monotonicity assumptions in Imbens and Angrist (1994) and Vytlacil (2002), but in an ordered environment (Vytlacil (2006)), and are stronger than the UPM assumption referenced in Section 3.3.3.

Potential Outcomes For any outcome of interest, Y_s is the defendant's treatment (s)-specific potential outcome — their outcome if they were placed in treatment level s (Holland (1986)) — which is a function of observables (X) and unobservable factors $\omega_s \forall s$. The observed outcome of a defendant is their potential outcome for the treatment they receive: $Y = \sum_{s=1}^3 Y_s \times 1[S = s]$. I assume full independence of observables and unobservables (Carneiro, Heckman, and Vytlacil (2011)), $(X, Z) \perp (\omega_1, \omega_2, \omega_3, U)$, and that Y_s is linear in observables: $Y_s = \beta_s X + \omega_s$, meaning the way observables influence potential outcomes is unrelated to the way unobservables do. This implies that $\mathbb{E}[Y_s | X, U] = \beta_s X + \mathbb{E}[\omega_s | U]$, which significantly simplifies estimation.³¹

³¹Note that the full independence assumption means that observables have heterogeneous effects on treatment assignment across judges but do not have heterogeneous treatment effects across defendants with

4.1.2 Treatment Effects and Estimation

Treatment Effects With this set up, I construct a variety of treatment parameters of interest (Heckman and Vytlacil (2005)), specifically the average treatment effects of expanding EM to replace release and to replace detention. This is equivalent to the average treatment effect of moving a random would-be released (detained) defendant from release (detention) to EM, and given the ordered structure, these treatment parameters capture the average effect of removing the option of releasing or detaining for these defendants from judges. I refer to as the average treatment effect on the released (detained) or ‘ATR’ (‘ATD’):

$$\begin{aligned} ATR &= \int_0^1 MTE_{EM,R}(u, x) W_R(u, x) du, \\ ATD &= \int_0^1 MTE_{EM,D}(u, x) W_D(u, x) du \end{aligned}$$

where $MTE_{s',s}(X = x, U = u) = \mathbb{E}[Y_{s'} - Y_s | X = x, U = u]$ are marginal treatment effects for defendants with observables x and unobserved resistance to higher treatment u . Because all judges agree on the ranking of defendants in terms of U but disagree on where to set cutoffs as a function of observables, two nearly identical defendants can be assigned to different treatments solely due to their quasi-randomly assigned judge, which allows for the estimation of MTEs across values of x and u .³²

The ATR (ATD) integrates over the relevant MTE and weighs the effect of the observables

different U ’s. As a robustness test, I provide estimates with this assumption relaxed and flexibly estimate MTRs with interacted X and U components, discussed in Section A.4.

³²The judge only influences outcomes by influencing treatment, not potential outcomes. While X is observed and affects treatment and outcomes, ω_s is unobserved. Because U captures all unobserved factors determining treatment, I can identify the average potential outcome under treatment s given observables $X = x$ and resistance $U = u$, known as the marginal treatment response (MTR) function (Mogstad, Santos, and Torgovitsky (2018)): $m_s(x, u) = \mathbb{E}[Y_s | X = x, U = u] = \beta_s x + \mathbb{E}[\omega_s | u]$. And the difference between MTRs of different treatments is the marginal treatment effect (MTE), the average treatment effect of moving a defendant with observables $X = x$ and unobservable resistance $U = u$ from one treatment s to another s' as: $MTE_{s',s}(X = x, U = u) = \mathbb{E}[Y_{s'} - Y_s | X = x, U = u] = m_s(x, u) - m_{s'}(x, u)$. So, I will use the variation in judges to study how defendants across the distribution of U respond to different treatments. Because U ranks defendants by how likely they are to be assigned to a higher or lower treatment, different treatment effects across different values of U inform how outcomes will change if, for example, EM is expanded to defendants who are likely to be detained (low U , all else equal) or released (high U , all else equal). Note that I can only make such comparisons where there is common support, meaning that judges must have sufficiently different cutoffs such that I observe individuals with $U = u$ in different treatments.

and unobservables of individuals more likely to be released (detained) more heavily, as captured by W_R (W_D) which integrate to 1.³³ In contrast, an average treatment effect of EM vs. release ($ATE_{EM,R}$) captures the average effect of taking a random defendant and comparing their outcomes on EM vs. on release, applying equal weights to all observations, and similarly for $ATE_{EM,D}$.

Estimation I estimate MTEs by first recovering values of π_1 and π_2 using probits and predicting treatment probabilities as functions of the judge and defendant observables. Then to recover MTEs, I first regress treatment-specific outcomes ($Y \times 1\{S = s\} \forall s$) on a function of observables and polynomial of the difference between predicted treatment probabilities ($\hat{\pi}_1, \hat{\pi}_2$); then I take the derivative of this fitted function with respect to the predicted treatment probability for all relevant treatments at each potential probability of treatment within the support to recover the expected potential outcome given $X = x, U = u$ ($\hat{E}[Y_s|X = x, U = u]$); finally, the difference between these fitted potential outcomes to recover MTEs ($MTE_{s+1,s}(x, u) = \hat{E}[Y_{s+1}|X = x, U = u] - \hat{E}[Y_s|X = x, U = u]$) over the common support of u for given values of X . More details on estimation are in Appendix C.5.

In the main specification, MTEs are effectively estimated using 2nd degree polynomial of predicted treatment probabilities. 95% confidence intervals are based on bootstrapped estimates (blocked at the bond court date level) with 400 runs producing non-symmetric confidence bands. As MTEs can only be identified where common support exists between relevant treatments, the estimates are restricted to $\pi_1, \pi_2 \in [0.24, 0.76]$ (see Figure C.3), so the main results focus on EM versus release and EM versus detention $u \in [0.24, 0.76]$, corresponding to the mid-50% defendants in terms of U .

Constructing Treatment Parameters I follow Cornelissen et al. (2016) in the construction of these treatment effects (see Appendix C.6 for details), and, due to a lack of common support, I recover common support equivalents of the parameters of interest: CATEs, CATR, CATD (i.e., common support ATEs, ATR, ATD) (Carneiro, Heckman, and Vytlacil (2011),

³³They weigh the effects of unobservables more heavily for individuals who are unobservably more likely to be released (detained), equating to higher weights on high (low) values of $U = \pi_1$ ($U = \pi_2$).

Bhuller et al. (2020)). See Figure B.7 for weights.

Testing Monotonicity To more directly test the monotonicity assumption, I perform the Frandsen, Lefgren, and Leslie (2023) test for monotonicity and exclusion violations (designed for single endogenous variable environments) on both margins separately using judge fixed effect instruments and controlling for year-quarter fixed effects (see Table B.3). On both the detained ($S > 2$) and released ($S > 1$) margins, both tests strongly reject the nulls ($p < 0.01$) for 6 and 7 out of 12 outcomes, though not always on the same outcomes. Surprisingly, the test is not strongly rejected for all outcomes considering the strength of the strict monotonicity assumption, it being run using only judge fixed effects (not accounting for interactions on observables), and the test rejecting for exclusion violations. Nevertheless, MTE results should be interpreted cautiously and primarily as a supplemental analysis, given that the test generally strongly rejects for most outcomes on at least one margin.

4.2 Results

Table 5 presents the estimates for treatment parameters of interest, with Columns (1) and (3) displaying the CATEs for EM vs. release and EM vs. Detention and Column (2) displays the CATR estimates and Column (4) displays the CATD estimates. Figure 5 displays 2SLS, CATE, CATD, and CATR estimates for comparison, Figures 6 and 7 display the MTE estimates for the main outcomes as well, and MTRs are displayed in Figure B.10. I discuss robustness checks in Appendix A.4.

4.2.1 Expanding EM to Replace Release

CATR estimates in Column (2) tells us the average effect of expanding EM to otherwise released defendants in the region of common support. For the average released defendant, replacing release with EM reduces pretrial misconduct. It reduces the likelihood of an FTA by -10.8pp ($p < 0.01$), but it has a small and not statistically significant effect on the likelihood of a new case pretrial (-2.1pp ($p > 0.1$)) and total pretrial new case costs using full and low

murder costs (-\$1,016 ($p>0.1$) and -\$719 ($p>0.1$)), all of which are smaller than the 2SLS estimates.

Furthermore, EM increases the likelihood of defendants receiving a new case for a violation by 1.5pp ($p<0.01$). EM also increases the likelihood of being sentenced to incarceration (2.12pp ($p=0.12$)) and a small positive effect on detention/EM costs (\$761 ($p<0.01$)), both similar to the 2SLS estimates though more precise. CATR estimates for post-trial cases and case costs and total costs using both full and low murder costs are economically small and not statistically significant. Overall, these results are consistent with the 2SLS estimates, but they suggest even smaller gains of using EM instead of release. Overall, they indicate that the expansion of EM to replace release is not likely to reduce overall costs.

4.2.2 Expanding EM to Replace Detention

The CATD estimates in Column (4) tell us the average effect of expanding EM to otherwise detained defendants in the region of common support. Moving detained defendants to EM allows for slightly more pretrial misconduct with increases in the likelihoods of an FTA (3.68pp ($p<0.01$)), a new case pretrial (7.53pp ($p<0.01$)), and a new case for a violation (4.65pp ($p<0.01$)), all within the 2SLS estimates' 95% confidence intervals. Effects on new case pretrial costs, direct jail/EM costs, new cases post-trial, and new case post-trial costs are also highly similar to the 2SLS estimates. Notably, CATD estimates suggest that detention is more coercive than EM as the likelihood of being sentenced to incarceration on EM compared with detention declines by -21.92pp ($p<0.01$), much larger than the 2SLS estimate. Overall, using full murder costs, the estimated savings of replacing detention with EM are similar to the 2SLS estimates but more precise (-\$57,000 ($p<0.01$)), while the low murder cost estimates are larger and more precise than the 2SLS estimates (-\$15,200 ($p<0.01$)), though still within the 2SLS 95% confidence interval. These results suggest that replacing detention with EM would yield significant savings, consistent with the 2SLS results.

4.2.3 Selection on Gains and Treatment Effect Heterogeneity

The similarity between the 2SLS and respective CATR and CATD estimates for most outcomes is consistent with relatively constant treatment effects suggested by the result in Section 3.3. However, the differences in magnitudes of some effects implies some degree of heterogeneity that can inform how judges make pretrial treatment decisions. The slopes of the MTE curves in Figures 6 and 7 inform the degree of heterogeneous treatment effects across the unobservable dimension of defendants, which influences their likelihood of being assigned to release, EM, and detention. I conduct more formal tests for heterogeneous treatment effects inspired by Heckman, Schmiederer, and Urzua (2010) and Carneiro, Heckman, and Vytlacil (2011), as shown in Table 6. I test whether the slope of the MTE curve is constant (Columns (1)-(3) and (5)-(7)) and for the equality of the CATE and CATR/CATD (Columns (4) and (8)). Based on these tests, I cannot generally reject constant slopes or equivalent CATE and CATR/CATD for a majority of outcomes — in particular CATEs are not significantly different from CATR and CATD for total costs using either full or low murder costs.

For EM vs. release, the slope of MTE curves for pretrial misconduct are suggestive of selection on gains by judges: defendants who are unobservably most likely to be released (high U) experience the smallest reductions in misconduct on EM. While I cannot reject that these slopes are constant, I can generally reject that the CATE and CATR are equal. However, the jail/EM cost and the likelihood of being sentenced to incarceration MTE curves are non-constant and downward sloping with significantly different CATR and CATE estimates, suggesting that the defendants who are most likely to be placed on EM relative to release have more expensive (longer) cases and their case outcomes are most influenced by being placed on EM. For post-trial outcomes and total costs, the MTEs are often non-constant but relatively parabolic, resulting in similar CATE and CATR estimates. Nevertheless, these results are consistent with judges selecting defendants into higher treatment on the EM vs. release margin based on gains of lower pretrial misconduct and higher guilty findings at the expense more costly (longer) cases.

For EM vs. detention, there is stronger evidence for non-constant treatment effects. Detention reduces a new case pretrial and failures to appear more for those who are least likely to be detained, but new case pretrial costs are constant. Additionally, jail/EM cost and sentenced to incarceration MTEs are both non-constant, with defendants highly likely to be detained experiencing smaller reductions in the likelihood of incarceration if placed in detention relative to EM, but they also have much more expensive cases based on jail/EM costs if placed in detention.³⁴

The differential selection behavior on the two margins is consistent with the differences between 2SLS and CATR/CATD results. Because judges are selecting defendants into EM on gains for some outcomes, compared with release, expanding EM to would-be released defendants does not produce significant savings. However, because judges are not selecting gains when assigning defendants to EM relative to detention, expanding EM to replace detention can produce large savings. Nevertheless, treatment effects are often either constant or non-constant but parabolic, which reduces the effective heterogeneity in treatment effects. These results further support the consistency between the 2SLS and CATR/CATD estimates and ease concerns over biased 2SLS estimates, as treatment effects are not highly heterogeneous.

4.2.4 Correcting for Changes in the Probability of Detection

EM's core function is to deter pretrial misconduct by increasing the probability an individual's misconduct will be detected, either through knowledge of where a defendant is at a given time or when they exited or entered their home or through the ability for authorities to search home without announcement or a warrant. So, the probability of a crime being detected (p) on EM is greater than on release and detention (if released from detention at some point pretrial), $p^{EM} > p^R, p^D$. Yet, crimes are not observed (c). Rather only new cases against

³⁴This pattern may also be the result of judges assigning defendants whose detention status is near certain due to other systemic factors (e.g., immigration, other pending cases, etc.) being ‘assigned’ detention but having relatively ‘low’ unobservables, resulting in them contributing to the outcomes of low U defendants.

defendants or detected crimes are observed ($a = c \times p$). So, the change in new cases pretrial on EM vs. release and detention will not be proportional to the change in crime and may be understated, if the difference in p is not accounted for: $c^s - c^{s'} = \frac{a^s}{p^s} - \frac{a^{s'}}{p^{s'}}$.

Given that the change in crime (not just arrests) is paramount for assessing costs and benefits, I provide adjusted estimates for the change in crime costs by making assumptions for values of p^{EM} , p^R , p^D . Specifically, I use $p^{R,D} = 0.375$ and a detection rate under EM of $p^{EM} = 0.625$, and I apply a fixed probability of detection to post-trial crime at $p = 0.375$. Then I compute corrected crime costs by scaling the observed crime costs by the probability of detection.

Table 7 displays the adjusted results, including the costs associated with FTAs, jail/EM duration, and being sentenced to incarceration using the main estimates applying the cost formula (1) to the main estimates CATE/CATR/CATD, which were used before in constructing the total cost and total cost (low murder) dependent variables in the prior sections. While the adjustments do not alter the main takeaway that EM is cost-saving relative to detention but not clearly so relative to release, adjusting for changes in the probability of detection suggests larger gains from EM relative to detention — with adjusted total savings being over twice as large as the unadjusted ones.³⁵ This is consistent with EM's higher probability of detection resulting in understated gains by only comparing observed crime.

5 Effect of the Introduction of IEM

As a final analysis, 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 will provide complementary estimates to the main 2SLS and CATR/CATD estimates though it relies on an entirely

³⁵ Appendix A.6 performs additional analyses using this setup, including bounding the amount of crime committed on each treatment and defendants' elasticity of crime with respect to the probability of detection.

different source of variation and does not leverage judge assignments at all.

5.1 Empirical Strategy

Exploiting the introduction of EM in Cook County to estimate the effect moving defendants onto EM faces two main issues. First, court data on other municipalities 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, a before-and-after comparison will not inform the effects of EM vs. release and retention, but rather the effect of EM vs. no EM. Given this, the results in this section must be taken primarily as a descriptive analysis and external check on the 2SLS and MTE results.

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. The sample of data is constructed similarly as the main sample, other than including data prior to June 2013 and dropping 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). 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.

Let there be four mutually exclusive types of defendants indexed by K : $k = R \rightarrow R$ are ‘always’ released defendants who are released even if EM is available; similarly $k = D \rightarrow D$ are ‘always’ detained defendants who are detained even if EM is available; $k = R \rightarrow EM$ are release to EM compliers who are released if EM is not available but on EM if it is available; similarly $k = D \rightarrow EM$ are detain to EM compliers who are detained if EM is not available but on EM if it is available. Similarly, I assume there are no release to detain or detain to release ‘compliers’ who switch between release and detention upon EM’s introduction. Let

$Y^s(k)$ refer to a defendant of type k 's potential outcome if assigned to treatment s .³⁶

I am interested in recovering the effect of EM on $R \rightarrow EM$ compliers relative to release ($E[Y^2(k = R \rightarrow EM) - Y^1(k = R \rightarrow EM)]$), and the effect of EM on $D \rightarrow EM$ compliers relative to detain ($E[Y^2(k = D \rightarrow EM) - Y^3(k = D \rightarrow EM)]$). Prior to June 2013, the average outcome for released (detained) defendants is a weighted sum of the outcomes for $R \rightarrow R$ ($D \rightarrow D$) and $R \rightarrow EM$ ($D \rightarrow EM$) defendants on release (detention). After June 2013, the average outcome for released (detained) defendants is solely the outcome for $R \rightarrow R$ ($D \rightarrow D$) defendants, and the average outcome for EM defendants is a weighted sum of the outcomes for would-be released and would-be detained defendants. The following exposition will focus on the EM vs. Release effect, and EM vs. Detain reflects the same process.

Let t denote the time period, where $t = 1$ refers to after the introduction of IEM and $t = 0$ refers to before the introduction. Focusing on the effect of EM vs. release first: in the data we observe $E[Y|s = 1, t = 0]$, the average outcome for defendants assigned to release prior to IEM, as well as $E[Y|s = 1, t = 1]$, the average outcome for defendants assigned to release after to IEM, and $E[Y|s = 2, t = 1]$, the average outcome for defendants assigned to EM after to IEM.

Prior to IEM, $E[Y|s = 1, t = 0] = \omega_1 E[Y^1(k = R \rightarrow R)] + (1 - \omega_1) E[Y^1(k = R \rightarrow EM)]$, where $\omega_1 \in [0, 1]$ is the share of $R \rightarrow R$ defendants in the sample of those assigned to release without EM (constant across time periods). After EM, $E[Y|s = 1, t = 1] = E[Y^1(k = R \rightarrow R)]$. Under the assumption that there is no change in composition over time, such that $E[Y|s = 1, t = 1] = E[Y^1(k = R \rightarrow R)]$:

$$\begin{aligned} E[Y|s = 1, t = 0] &= \omega_1 E[Y^1(k = R \rightarrow R)] + (1 - \omega_1) E[Y^1(k = R \rightarrow EM)] \\ &= \omega_1 E[Y|s = 1, t = 1] + (1 - \omega_1) E[Y^1(k = R \rightarrow EM)] \end{aligned}$$

³⁶While the expansion of EM following June 2013 influenced the $R \rightarrow EM$ and $D \rightarrow EM$ defendants, the ATR and ATD parameters discussed above capture the effect of moving $R \rightarrow R$ and $D \rightarrow D$ defendants onto EM, respectively.

As a result,

$$E[Y^1(k = R \rightarrow EM)] = \frac{1}{(1 - \omega_1)} [E[Y|s = 1, t = 0] - \omega_1 E[Y|s = 1, t = 1]]$$

where ω_1 is the share of $k = R \rightarrow R$ defendants and can be computed as $\omega_1 = \frac{\frac{N(s=1,t=1)}{N(t=1)}}{\frac{N(s=1,t=0)}{N(t=0)}}$,

in order to account for any change in the total number of defendants.

To recover $E[Y^2(k = R \rightarrow EM)] - E[Y^1(k = R \rightarrow EM)]$, we also require an estimate of $E[Y^2(k = R \rightarrow EM)]$. However we observe $E[Y|s = 2, t = 1]$ which is composed of a weighted sum of *Release* \rightarrow *EM* and *Detain* \rightarrow *EM* compliers: $E[Y|s = 2, t = 1] = \omega_2 E[Y^2(k = R \rightarrow EM)] + (1 - \omega_2) E[Y^2(k = D \rightarrow EM)]$. To compute estimates of $E[Y^2(k = R \rightarrow EM)]$, I use data from before IEM was introduced to compute a predicted likelihood of the defendant being assigned to detain (vs. release), $P(D = 1|X)$, using a probit model and defendant observables. I construct an estimate of $E[Y^2(k = R \rightarrow EM)]$ as the weighted average of $E[Y|s = 2, t = 1]$ where the weights correspond to $P(R = 1|X) = 1 - P(D = 1|X)$:

$$E[Y^2(k = R \rightarrow EM)] \approx \frac{1}{N(s = 2, t = 1)} \frac{1}{\hat{P}(R = 1|X = x_i, s = 2)} \sum_i^{N(t=1)} 1\{s = 2\} \times Y \times \hat{P}(R = 1|X = x_i)$$

A similar derivation of effects applies for *EM* vs. *Detention* and the $k = D \rightarrow EM$ defendants.

5.2 Results

Table 8 displays the results for the effect of *EM* on *Release* \rightarrow *EM* and *Detain* \rightarrow *EM* as well as estimates of $E[Y^1(k = R \rightarrow EM)]$ and $E[Y^3(k = D \rightarrow EM)]$ for 3, 6, and 9 month windows for computing average outcomes, while Figure 8 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 and CATR/CATD estimates for comparison. Table 9 contains the number of observations pre- and post-IEM in each sample along with values of ω_1 and ω_3 .

As shown in Table 8, the net effect of introducing *EM* is a total savings of between \$200

and \$1,400 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. 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.

In general, the 3, 6, and 9 month results for main outcomes are similar to the 2SLS or CATR/CATD estimates or between them, with few 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 → EM* defendants and increases them by 2pp for *Detain → EM* defendants. For total costs using low-murder, EM increases costs by \$706 for *Release → EM* defendants and decreases costs by -\$10,845 for *Detain → EM* defendants. Each of these estimates are well within either their respective 2SLS or CATR/CATD 95% confidence interval.

The main difference in total costs using full murder costs is that of EM vs. detention which are near zero for 6 and 9 month windows and positive for the 3 month window (whereas 2SLS and ATD 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 → EM* and *D → EM* defendants, and the 2SLS estimates (the effect on the marginal defendant between EM and release and EM and detention given EM exists) and the CATR/CATD estimates (the effect of expanding EM to the *R → R* and *D → D* defendants) is consistent with the relatively constant treatment effects found in the main analyses.

6 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 and use both two-stage least squares and marginal treatment effects methods to estimate the effect of EM vs. release and detention. Both sets of estimates yield similar conclusions: 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 it reduces costly pretrial and post-trial crime and improves case outcomes at a significantly lower direct cost. 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 results.

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 (1)	Release (2)	EM (3)	Detain (4)
Defendant				
Black	0.75	0.63	0.78	0.8
Hispanic	0.12	0.17	0.1	0.1
White	0.13	0.19	0.11	0.09
Male	0.85	0.8	0.82	0.92
Age	35	35	37	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 sentenced to incarceration, either in the Illinois or Cook County Department of Corrections.

Table 2: Tests for Violations of Exogeneity

	Release	Judge Leave-Out Release Propensity	EM	Judge Leave-Out EM Propensity	Detain	Judge Leave-Out Detain Propensity	Joint Judge Leave-Out Propensities
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black	-0.1*** (0.00444)	0.00094 (0.00071)	0.063*** (0.00457)	0.00047 (0.0006)	0.039*** (0.00408)	-0.0014 (0.00107)	-
Female	0.069*** (0.00571)	-0.00015 (0.00076)	0.054*** (0.0066)	0.00058 (0.00063)	-0.12*** (0.00459)	-0.00043 (0.00114)	-
Age ≥ 30	0.045*** (0.00401)	-0.00059 (0.00061)	0.028*** (0.00444)	-0.00017 (0.00051)	-0.073*** (0.00386)	0.00076 (0.00092)	-
Felony Violent	-0.2*** (0.00817)	-0.00008 (0.00143)	-0.22*** (0.00853)	0.0012 (0.00127)	0.42*** (0.00897)	-0.0011 (0.00216)	-
Felony Drug Delivery	-0.076*** (0.00676)	0.00004 (0.00117)	0.033*** (0.00763)	-0.0022** (0.00108)	0.043*** (0.00775)	0.0022 (0.00185)	-
Felony Drug Possession	0.024*** (0.00659)	-0.001 (0.00105)	0.051*** (0.00684)	-0.00012 (0.00095)	-0.075*** (0.00718)	0.0011 (0.00163)	-
Felony Weapon	-0.12*** (0.0083)	0.0011 (0.00128)	-0.3*** (0.00732)	-0.0045*** (0.00111)	0.42*** (0.00875)	0.0034* (0.00196)	-
Felony Property	-0.072*** (0.00773)	0.00077 (0.00128)	-0.011 (0.00905)	0.00024 (0.00108)	0.083*** (0.00836)	-0.001 (0.0019)	-
Felony Other	-0.066*** (0.00896)	-0.00085 (0.00196)	-0.11*** (0.00917)	-0.0012 (0.00148)	0.17*** (0.01017)	0.0021 (0.00295)	-
N Past Cases [1,4]	-0.12*** (0.0057)	-0.00019 (0.00091)	0.0016 (0.00583)	-0.00036 (0.00074)	0.12*** (0.00481)	0.00055 (0.00138)	-
N Past Cases [5,12]	-0.22*** (0.00683)	-0.00008 (0.00109)	0.016** (0.00758)	0.00015 (0.00088)	0.21*** (0.00603)	-0.00007 (0.00167)	-
N Past Cases > 12	-0.26*** (0.00826)	0.00023 (0.00133)	0.031*** (0.00986)	0.00018 (0.00113)	0.23*** (0.00786)	-0.00041 (0.00203)	-
N Past FTAs [1,2]	-0.031*** (0.00441)	-0.0003 (0.00071)	0.0032 (0.00467)	-0.00098* (0.00059)	0.028*** (0.00436)	0.0013 (0.00108)	-
N Past FTAs > 2	-0.028*** (0.00573)	-0.0007 (0.0009)	0.0042 (0.0066)	-0.0014 (0.00084)	0.023*** (0.00624)	0.0021 (0.00141)	-
N Charges [2,3]	-0.0015 (0.00401)	0.0011* (0.00061)	-0.019*** (0.00427)	0.00015 (0.00053)	0.021*** (0.00392)	-0.0012 (0.00092)	-
N Charges > 3	0.033*** (0.00596)	0.0012 (0.00097)	-0.052*** (0.0059)	-0.00075 (0.00077)	0.02*** (0.00579)	-0.0004 (0.00146)	-
F-Statistic	476.84	1.11	379.1	3.5	1138.68	1.08	1.11
p-value	<0.01	0.33	<0.01	<0.01	<0.01	0.37	0.273
N	65430	65430	65430	65430	65430	65430	196290

Note: Table displays results of regressing endogenous treatments variables (Columns (1), (3), (5)) and judge leave-out propensities (Columns (2), (4), and (6)) on defendant and case characteristics, with year-quarter fixed effects. Column (7) provides the results from stacking the specifications from Columns (2), (4), and (6), testing the joint relationship between all three judge leave-out propensities and defendant observables. F-statistic refers to an F-test of joint significance of defendant and case observables on the dependent variable, conditional on year-quarter fixed effects. Standard errors in parentheses 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 Leave-Out Propensities

	All	Race=Black	Gender=Female	No Violent Felony Charge	Charge Felony Class X or 1	Charge Felony Class 2, 3, or 4
	(1)	(2)	(3)	(4)	(5)	(6)
Endogenous Variable = Release						
$z_{j(i)}^R$	0.95*** (0.079)	0.94*** (0.073)	1.21*** (0.165)	1*** (0.082)	0.79*** (0.106)	0.96*** (0.086)
$z_{j(i)}^D$	-0.02 (0.048)	0.03 (0.045)	-0.18* (0.103)	-0.04 (0.05)	0.07 (0.067)	-0.05 (0.053)
Complier Share	1	1.03	1.04	1.06	0.94	0.99
N Obs.	65430	48816	9678	60908	11667	53344
Endogenous Variable = Detain						
$z_{j(i)}^R$	-0.01 (0.065)	0.04 (0.076)	-0.07 (0.112)	0.01 (0.066)	-0.06 (0.14)	0.01 (0.066)
$z_{j(i)}^D$	0.98*** (0.043)	1.09*** (0.049)	0.61*** (0.076)	1.01*** (0.043)	1.36*** (0.088)	0.93*** (0.044)
Complier Share	1	1.12	0.57	1.04	1.71	0.93
N Obs.	65430	48816	9678	60908	11667	53344

Note: Table displays the first stage of judge leave-out propensities for release ($z_{j(i)}^R$) and detain ($z_{j(i)}^D$) on the endogenous variables, release=1 and detain=1, from equation (3). Columns (2)-(6) restrict the sample and perform computes the complier-weighted characteristic share relative to the overall frequency in the sample. Controls include year-quarter fixed effects, logged past Branch 1 cases (plus 1), and an indicator for a prior guilty felony charge. Standard errors in parentheses are clustered at the branch 1 date level. ***p < 0.01; **p < 0.05; *p < 0.1

Table 4: Effects of EM compared with Release and Detention

	Pretrial				Case Outcomes				Posttrial				Total Marginal Cost	
	Failure to Appear	New Case Pretrial	New Case Violation	New Case Cost (\$1,000)	New Case Cost (Low Murder) (\$1,000)	Sentenced to Incarceration	Detention/ EM Cost (\$1,000)	Total New Cases Posttrial (3 Years)	Total New Case Cost (3 Years) (\$1,000)	Total New Case Cost (3 Years) (\$1,000)	Total New Case Cost (Low Murder) (\$1,000)	Total Cost (\$1,000)	Total Cost (\$1,000)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)		
Panel A: OLS with Main Controls														
Release	-0.058*** (0.00331)	0.0051 (0.00409)	0.028*** (0.00167)	-0.13 (1.64)	-0.49*** (0.172)	0.11*** (0.00434)	1.1*** (0.0551)	0.096*** (0.008)	5.8 (4.15)	3*** (0.367)	8.2* (4.46)	5.1*** (0.419)		
Detain	0.052*** (0.00224)	0.1*** (0.0035)	0.044*** (0.00164)	-1.8 (1.73)	1.1*** (0.163)	-0.22*** (0.00425)	-20*** (0.197)	0.037*** (0.00796)	-19*** (4.57)	-1** (0.393)	-32*** (4.84)	-11*** (0.424)		
Panel B: 2SLS with Leave-Out Judge Propensity Instruments														
Release	-0.12*** (0.0298)	-0.11** (0.0422)	-0.028* (0.0162)	-3.5 (20.3)	-3.9** (1.97)	0.029 (0.0477)	0.63 (1.79)	0.17* (0.0949)	-44 (50.6)	-0.76 (4.41)	-45 (53.4)	-3.6 (4.89)		
Detain	0.049** (0.0192)	0.061** (0.0275)	0.037*** (0.0107)	-10 (13.9)	-1.1 (1.28)	-0.0081 (0.0311)	-14*** (1.14)	0.054 (0.061)	-53 (33.7)	-3.1 (2.91)	-67* (35.4)	-9.6*** (3.16)		
Panel C: 2SLS with Instrumented Bond Amounts														
Release	-0.096*** (0.0323)	-0.037 (0.0456)	-0.011 (0.0181)	3.4 (22.2)	-1.7 (2.09)	0.0038 (0.0507)	1.4 (1.9)	0.16 (0.1)	-72 (53.6)	-3.7 (4.71)	-65 (56.5)	-4.8 (5.2)		
Detain	0.08*** (0.0237)	0.14*** (0.0334)	0.056*** (0.0133)	-2.4 (16.5)	1.5 (1.54)	-0.038 (0.0368)	-13*** (1.38)	0.04 (0.0724)	-85** (40.5)	-6.5* (3.47)	-90** (42.6)	-11*** (3.83)		
Z(EM Price)	-0.00011 (0.000262)	0.00027 (0.000349)	0.0003** (0.000142)	0.013 (0.146)	0.019 (0.0152)	0.00051 (0.000407)	0.027* (0.0138)	0.001 (0.000652)	0.11 (0.399)	0.016 (0.034)	0.12 (0.425)	0.048 (0.0393)		
Z(D Price)	-0.00014 (0.000144)	-0.00053*** (0.000192)	-0.0002** (0.0000794)	-0.048 (0.0875)	-0.02** (0.00874)	-0.0000035 (0.000217)	-0.013* (0.00786)	-0.000025 (0.000368)	0.14 (0.225)	0.013 (0.0189)	0.09 (0.241)	-0.0082 (0.022)		
N. Obs	65412	65430	65430	65430	65430	65430	65192	65430	65430	65430	65174	65174		
Mean Dep Var. 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 Dep Var. 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 Dep Var. 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 and 2SLS 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 detain, respectively. Panels display results using alternative specifications as described in the main text. Controls include year-quarter fixed effects, logged past Branch 1 cases, and an indicator for a prior guilty felony charge. 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. ***p < 0.01; **p < 0.05; *p < 0.1

Table 5: ATE, ATR, and ATD Results for Common Support

	EM vs. Release		EM vs. Detention	
	$CATE_{EM,R}$	CATR	$CATE_{EM,D}$	CATD
	(1)	(2)	(3)	(4)
Pretrial				
Failure to Appear	-0.15*** [-0.18, -0.11]	-0.11*** [-0.14, -0.081]	0.055*** [0.039, 0.07]	0.037*** [0.02, 0.052]
New Case Pretrial	-0.049*** [-0.091, -0.015]	-0.021 [-0.052, 0.0063]	0.082*** [0.047, 0.11]	0.075*** [0.041, 0.1]
New Case Pretrial for Violation/Escape	0.023*** [0.011, 0.034]	0.015*** [0.0053, 0.024]	0.036*** [0.026, 0.044]	0.046*** [0.034, 0.06]
New Case Pretrial Cost (\$1,000)	-5.9 [-20, 4.7]	-1 [-14, 9.3]	-3.5 [-19, 11]	-8.4 [-22, 3.1]
New Case Pretrial Cost (Low Murder) (\$1,000)	-2** [-3.6, -0.51]	-0.72 [-2.1, 0.48]	0.075 [-1.6, 1.5]	-0.27 [-1.8, 0.89]
Case Outcomes				
Sentenced to Incarceration	0.13*** [0.098, 0.16]	0.021 [-0.0074, 0.051]	-0.25*** [-0.29, -0.21]	-0.22*** [-0.26, -0.19]
Jail/EM Cost (\$1,000)	1.2*** [1.1, 1.3]	0.76*** [0.66, 0.85]	-12*** [-14, -10]	-20*** [-22, -18]
Posttrial				
Total New Felony Cases Posttrial within 3 Years	0.0027 [-0.049, 0.065]	0.01 [-0.043, 0.068]	0.017 [-0.043, 0.08]	0.018 [-0.038, 0.069]
New Case Posttrial Cost within 3 Years (\$1,000)	3.8 [-26, 31]	9.6 [-13, 32]	-41* [-85, 4.1]	-34** [-65, -1.8]
New Case Posttrial Cost (Low Murder) within 3 Years (\$1,000)	-0.85 [-3.8, 1.8]	0.013 [-2.5, 2.4]	-4* [-7.2, -0.049]	-3.2** [-6, -0.71]
Total Marginal Costs				
Total Cost (\$1,000)	1 [-30, 27]	9.2 [-13, 34]	-55** [-99, -14]	-57*** [-89, -26]
Total Cost (Low Murder) (\$1,000)	0.26 [-3.1, 3.2]	-0.026 [-2.9, 2.8]	-14*** [-18, -10]	-15*** [-18, -13]

Note: Table displays the common support average treatment effect (CATE) and average treatment effect on the released and detained (CATR, CATD) estimates averaging over the MTEs. 95 percent CIs (in brackets) and p-values are computed through 400 block bootstrap runs at the bond court date level. P-values are measured by the two times the share of bootstrap runs in which the value crosses zero compared with the main estimate. MTEs are recovered using the main estimation method using a 3rd degree polynomial for Φ_s . Total marginal cost calculations follow equation (1), and 'low murder' refers to using \$400,000 as the cost of a murder charge. ***p < 0.01; **p < 0.05; *p < 0.1

Table 6: Tests for Heterogenous Treatment Effects

	EM vs. Release				EM vs. Detain			
	Constant Slope of MTE in U			$ATE_{EM,R=ATR}$	Constant Slope of MTE in U			$ATE_{EM,D=ATD}$
	$\phi_2^1 - \phi_1^1 = 0$	$\phi_2^2 - \phi_1^2 = 0$	$\phi_2^1 - \phi_1^1 = \phi_2^2 - \phi_1^2 = 0$	Pr. Equal	$\phi_2^1 - \phi_3^1 = 0$	$\phi_2^2 - \phi_3^2 = 0$	$\phi_2^1 - \phi_3^1 = \phi_2^2 - \phi_3^2 = 0$	Pr. Equal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Failure to Appear	0.34	0.51	0.34	<0.01	0.08	0.55	0.08	0.01
New Case Pretrial	0.58	0.80	0.58	<0.01	0.92	0.61	0.00	0.58
New Case Pretrial for Violation/Escape	0.86	0.90	0.80	0.04	0.02	0.07	0.03	0.02
New Case Pretrial Cost (\$1,000)	0.60	0.81	0.60	0.04	0.72	0.81	0.70	0.38
New Case Pretrial Cost (Low Murder) (\$1,000)	0.50	0.78	0.50	<0.01	0.47	0.38	0.35	0.56
Jail/EM Cost (\$1,000)	0.70	0.03	0.03	<0.01	0.05	0.00	0.00	<0.01
Sentenced to Incarceration	0.01	0.32	0.01	<0.01	0.00	0.00	0.00	0.1
Total New Felony Cases Posttrial within 3 Years	0.31	0.24	0.23	0.7	0.45	0.20	0.20	0.98
New Case Posttrial Cost within 3 Years (\$1,000)	0.73	0.63	0.59	0.48	0.88	0.92	0.03	0.77
New Case Posttrial Cost (Low Murder) within 3 Years (\$1,000)	0.10	0.03	0.03	0.25	0.70	0.80	0.68	0.69
Total Cost (\$1,000)	0.78	0.66	0.64	0.33	0.73	0.92	0.70	0.84
Total Cost (Low Murder) (\$1,000)	0.11	0.06	0.06	0.86	0.00	0.02	0.00	0.34

Note: Table displays results from tests for constant treatment effects across unobservables and observables. Columns (1)-(4) display tests for EM vs. Release, and the analogous tests are displayed in Columns (5)-(8) for EM vs. Detain. Columns (1) and (2) contain tests for whether the first or second term in the MTE polynomial is statistically different from zero, as measured by the two times the share of bootstrap runs in which the value crosses zero compared with the main estimate. Column (3) contains the joint test, the two times the share of bootstrap runs in which both values cross zero compared with the main estimates. Column (4) tests if the CATE and CATR/CATD are statistically different, as measured by two times the share of bootstrap runs in which their difference crosses zero compared with the main estimates.

Table 7: Breakdown of Relative Counterfactual Marginal Costs with Adjusted Probabilities of Detection (\$1,000)

	EM vs. Release		EM vs. Detention	
	$CATE_{EM,R}$	CATR	$CATE_{EM,D}$	CATD
	(1)	(2)	(3)	(4)
Failure to Appear	-0.15 [-0.18 , -0.11]	-0.11 [-0.14 , -0.08]	0.055 [0.04 , 0.07]	0.037 [0.02 , 0.05]
New Case Pretrial Cost	-29 [-76 , 5.8]	-17 [-56 , 13]	-21 [-71 , 25]	-33 [-77 , 4.7]
New Case Pretrial Cost (Low Murder)	-13 [-18 , -8.2]	-8.7 [-13 , -5]	-6.2 [-12 , -1.8]	-7.5 [-12 , -4]
Jail/EM Cost	0.48 [0.43 , 0.51]	0.3 [0.27 , 0.34]	-4.8 [-5.8 , -4]	-8 [-8.6 , -7.4]
Sentenced to Incarceration	2.7 [2 , 3.3]	0.42 [-0.15 , 1]	-4.9 [-5.8 , -4.2]	-4.4 [-5.2 , -3.7]
New Case Posttrial Cost within 3 Years	13 [-87 , 100]	32 [-42 , 110]	-140 [-280 , 14]	-110 [-220 , -6]
New Case Posttrial Cost (Low Murder) within 3 Years	-2.8 [-13 , 5.9]	0.042 [-8.3 , 8.2]	-13 [-24 , -0.16]	-11 [-20 , -2.4]
Total	-14 [-160 , 110]	16 [-98 , 120]	-170 [-370 , 31]	-160 [-310 , -12]
Total (Low Murder)	-13 [-29 , 1.4]	-8.1 [-21 , 4.5]	-29 [-47 , -10]	-30 [-45 , -17]

Note: Table displays the relative marginal costs (positive) and savings (negative) per defendant under counterfactual policies, where the average defendant is moved from release to EM or detention to EM (ATE), where the average released or detained defendant is moved to EM (ATR or ATD). Estimates are constructed using common support analogs of MTRs with costs or scaling applied and weighted by the relevant treatment parameter weights. I apply equation (1) to construct costs. I use average costs of \$15 per defendant-day for EM and \$150 defendant-day for jail and apply a 40% marginal cost. FTAs are valued at \$1,000. Crime costs are based on aggregated arrest costs assuming a 37.5% cost-weighted detection rate under release, post-detention release, or post-trial, and a 62.5% cost-weighted detection rate under EM. Pretrial crime costs include punishment costs. Sentencing costs are assumed to be \$20,000 per defendant sentenced to incarceration, based on Mai and Subramanian (2017) estimates of average prison cost per defendant in Illinois in 2015 of \$33,507 and applying a 60% marginal cost discount. This also assumes defendants sentenced to incarceration are serving one year on average, which may be an underestimate (over 75% of individuals sentenced to prison or jail are sentenced to prison). 95% CIs are displayed below the estimate and are computed through 400 block bootstrap runs at the branch 1 date level.

Table 8: Effects of EM Introduction

		Pretrial				Case Outcomes			Posttrial			Total Marginal Cost	
		New Case Failure to Appear	New Case Pretrial Violation	New Case Cost (\$1,000)	New Case Cost (\$1,000)	Detention/ (Low Murder)	Sentenced EM Cost (\$1,000)	Total New Cases Posttrial (3 Years)	Total New Case Cost (\$1,000)	Total New Case Cost (\$1,000)	Total Cost (\$1,000)	Total Cost (\$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	4.910	0.158	-14.664	-0.046
Effect of Detain to EM		0.015	0.094	0.025	38.050	1.587	-18.050	-0.225	-0.245	24.746	-0.058	52.045	-9.174
E[Y k=2]		0.083	0.161	0.028	3.213	3.213	0.795	0.184	0.446	36.178	13.734	43.398	20.937
E[Y k=3]		0.074	0.195	0.046	3.911	3.911	1.045	0.261	0.546	35.077	16.368	44.608	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	5.485	1.674	-6.238	0.706
Effect of Detain to EM		0.024	0.044	0.031	17.678	0.302	-16.906	-0.224	-0.069	-0.739	-0.621	6.181	-10.845
E[Y k=2]		0.075	0.158	0.028	8.760	3.411	0.760	0.182	0.454	28.951	13.745	41.675	21.099
E[Y k=3]		0.069	0.188	0.044	8.468	3.862	1.010	0.262	0.554	30.664	16.375	44.789	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	-14.488	-2.313	0.668	0.056	0.113	-8.923	1.468	-22.087	0.537
Effect of Detain to EM		0.019	0.029	0.035	-3.728	-0.336	-14.560	-0.205	-0.046	5.481	-0.353	-7.613	-9.968
E[Y k=2]		0.071	0.164	0.034	7.213	3.399	0.829	0.206	0.469	26.115	14.076	37.781	21.915
E[Y k=3]		0.064	0.192	0.049	7.137	3.868	1.078	0.287	0.559	27.821	16.546	41.112	26.559

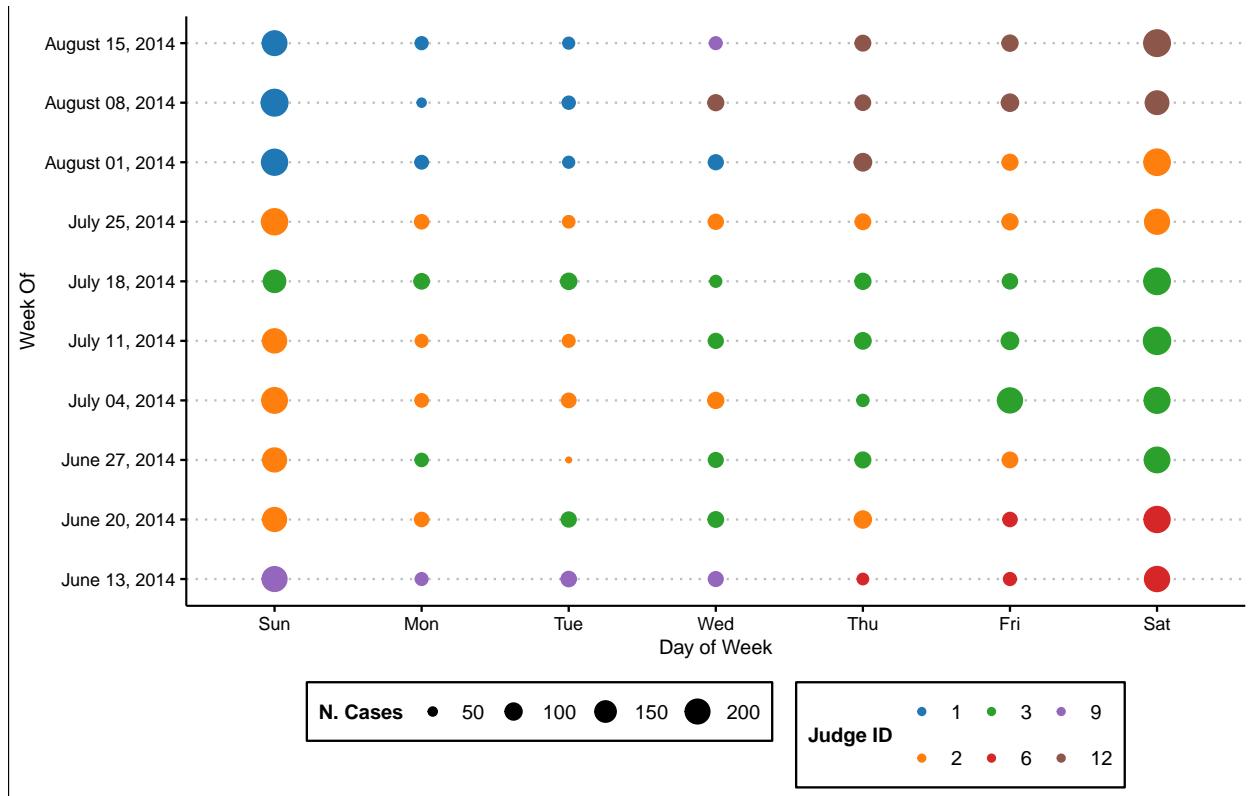
Note: Table displays the effect of the introduction of IEM on outcomes. +/- 3, 6, and 9 months refers to the time span before and after June 2013 that is used to compute the estimates. Net change pre/post EM is computed as the change in outcomes before and after June 2013. Effect of Release/Detain to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

Table 9: Additional Information for Effects of EM Introduction

Month Range	ω_1	ω_3	N Obs. Post-IEM	N Obs. Pre-IEM
	(1)	(2)	(3)	(4)
+/- 3 Months	0.502	0.829	5992	4869
+/- 6 Months	0.457	0.747	11111	9798
+/- 9 Months	0.539	0.677	15496	14400

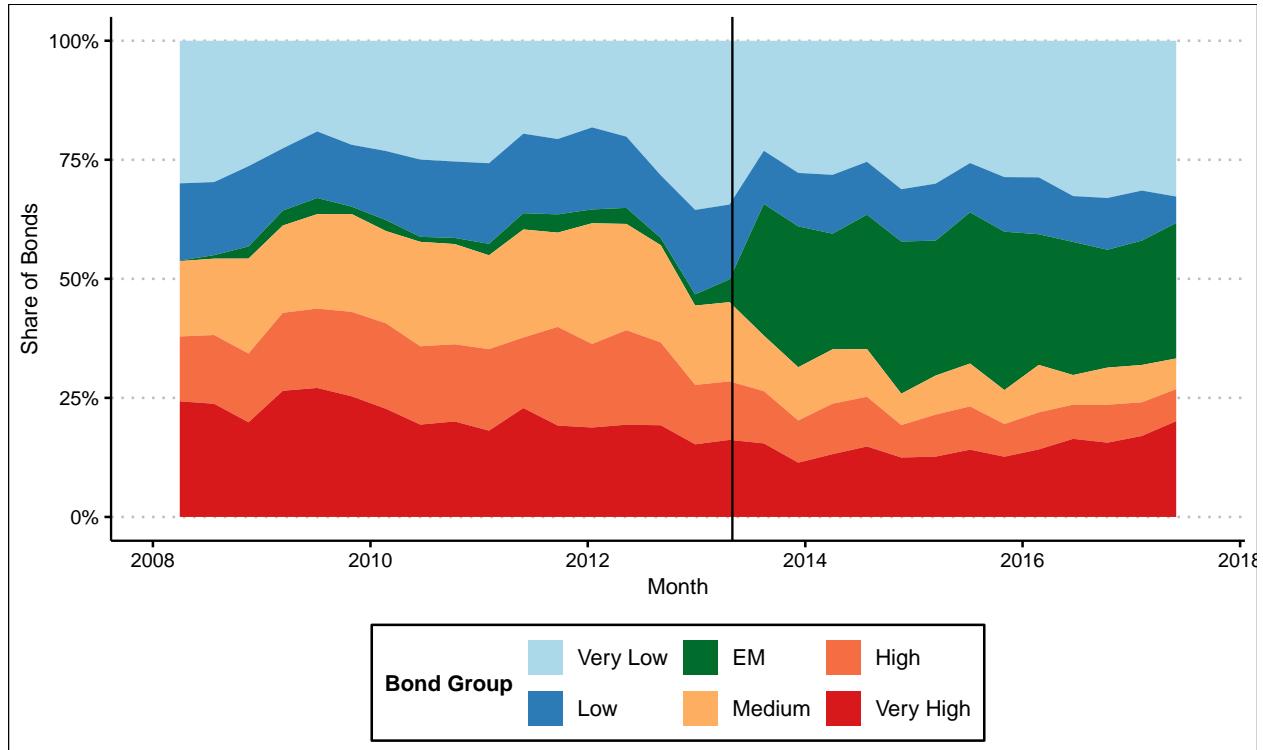
Note: Table displays additional information for the effect of the introduction of IEM on outcomes. +/- 3, 6, and 9 months refers to the time span before and after June 2013 that is used to compute the estimates. ω_1 and ω_3 (Columns (1) and (2)) refer to the relative shares of defendants who are 'always released' and 'always detained' regardless of whether EM is available compared with those who are released and detained when EM is not available. 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



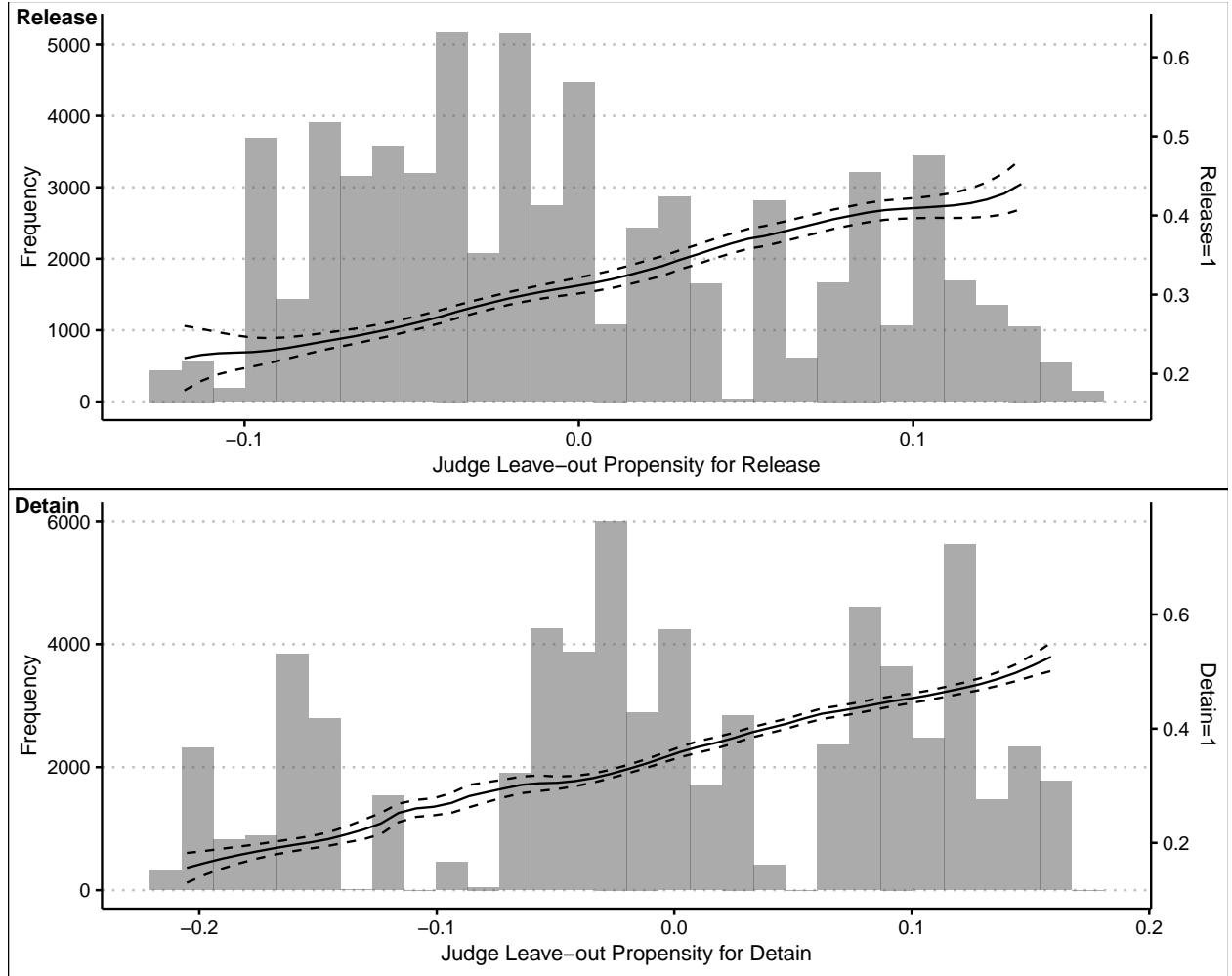
Note: Figure displays the caseloads of active judges in the Cook County Bond Court (Branch 1, Room 100) by week and day of the week between the weeks of June 13, 2014, and August 15, 2014. There were 6 active judges in this period (dot color), while the size of each dot denotes the number of cases they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

Figure 2: Bond Types Over Time



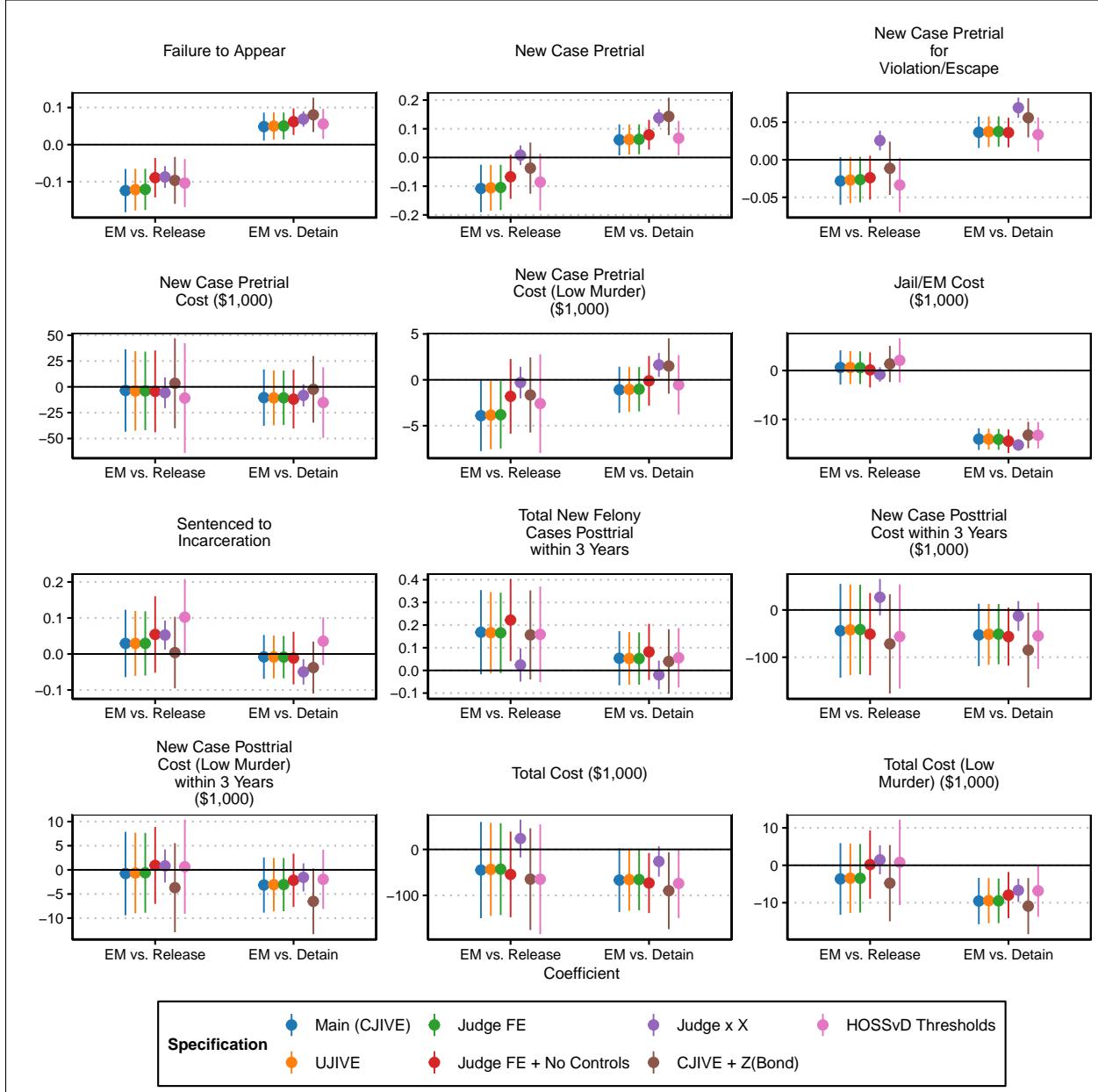
Note: Figure displays the composition of bond types within the sample between March 2008 and 2015 aggregated by the year-month of bond date. Bond group EM denotes IEM bonds, and other bond groups are determined by the bond price required for release (i.e., the bond price for any D-bond, \$0 for I-bonds, 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: First Stage and Support of Judge Instruments



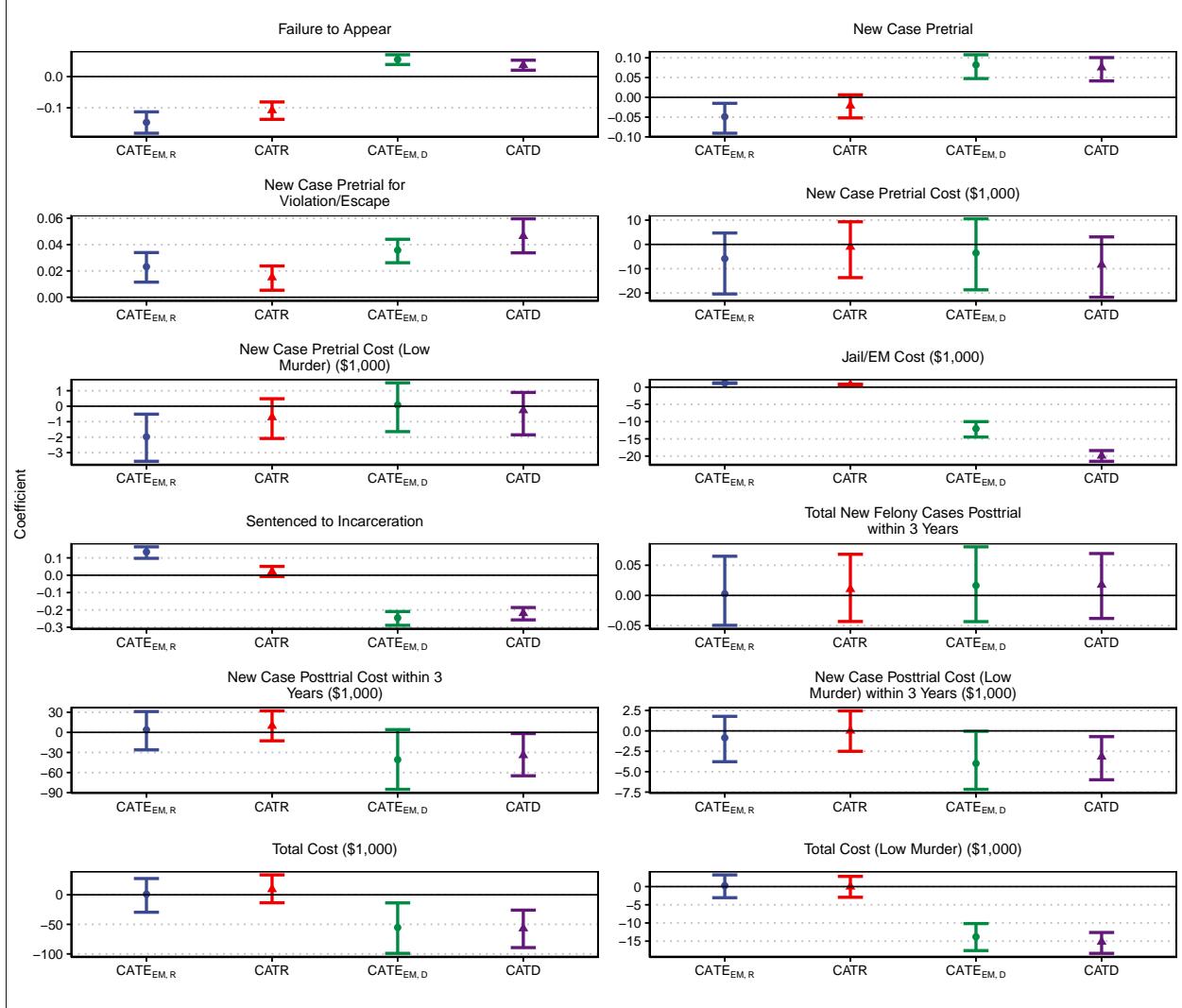
Note: Figure displays the support of the judge-leave out propensity instrument for release (top) and detain (bottom) and the local linear fit between the instrumented propensity and the observed likelihood of release and detain — instrumented propensities and observed likelihood are residualized using main controls and year-quarter fixed effects. The x-axis is the value of the residualized instrument; the left y-axis is the frequency of the instrument value; the right y-axis is the residualized likelihood of the defendant being placed on the treatment of interest. 95% confidence intervals are computed clustered at the branch 1 date level.

Figure 4: Comparison of 2SLS Estimates across Specifications



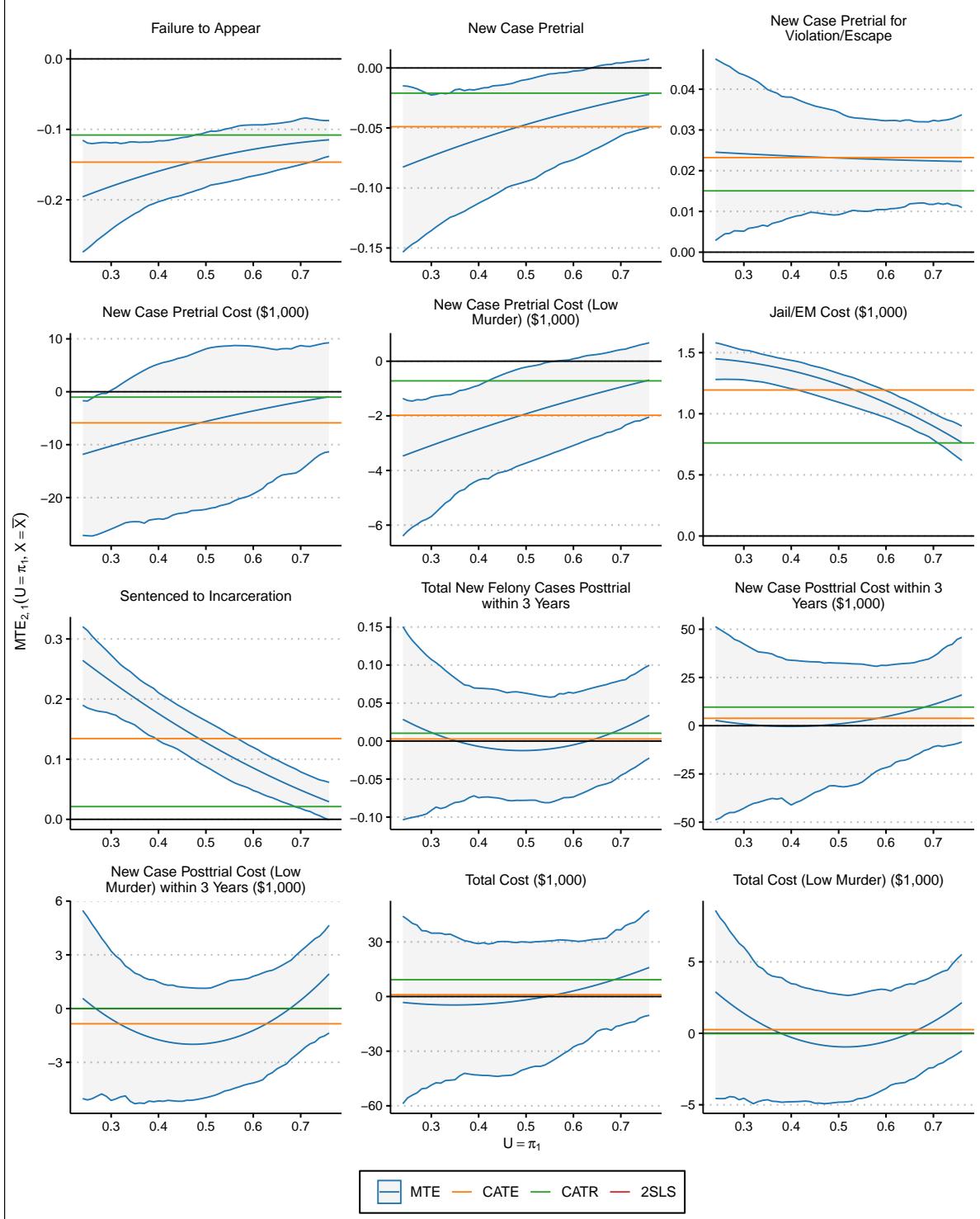
Note: Figure displays 2SLS coefficients and 95% confidence intervals using alternative specifications, with standard errors clustered at the branch 1 date level. Main estimates computed using CJIVE instruments at the branch 1 date level, and ‘UJIVE’ refers to performing CJIVE at the bond court date; ‘Judge FE’ uses judge fixed effects as instruments, and ‘Judge FE + No Controls’ uses no controls, 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); ‘CJIVE + Z(Bond)’ modifies the main specification by adding judges’ residualized average bond amounts for IEM and D-bonds as controls (see Section 3.3.1 and Panel C of Table 4); ‘HOSSvD Thresholds’ uses judge thresholds rather than judge propensities as instruments, following Humphries et al. (2023) (see Section 3.3.3).

Figure 5: Comparison of CATE, CATD, CATR, and 2SLS Estimates



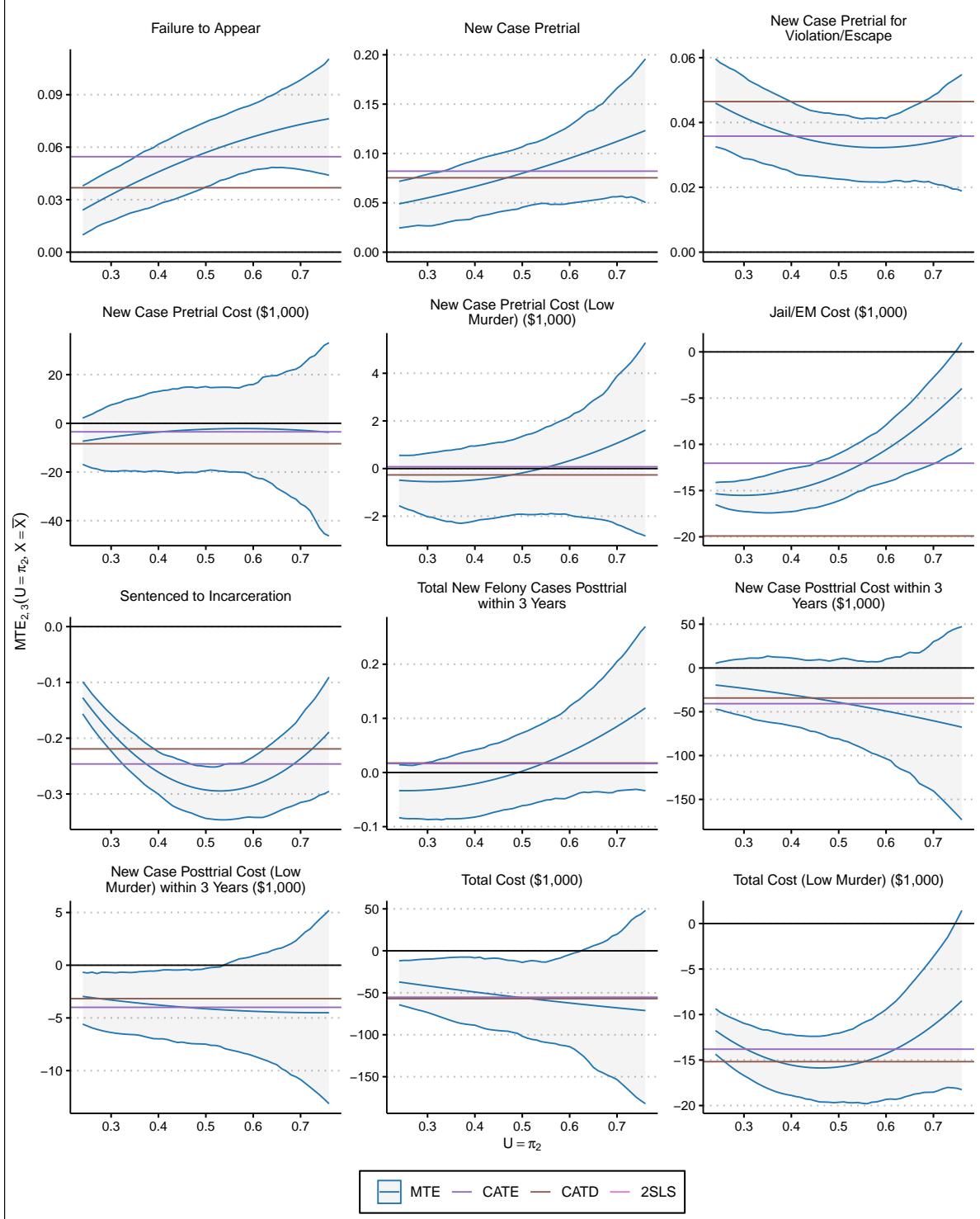
Note: Figure displays the common support ATE, ATR, and ATD estimates for EM vs. release and EM vs. detain along with 2SLS estimates for visual comparison for main outcomes. 2SLS estimates' 95% confidence intervals are computed using standard errors clustered at the branch 1 date level, while other 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level.

Figure 6: MTEs for EM vs. Release



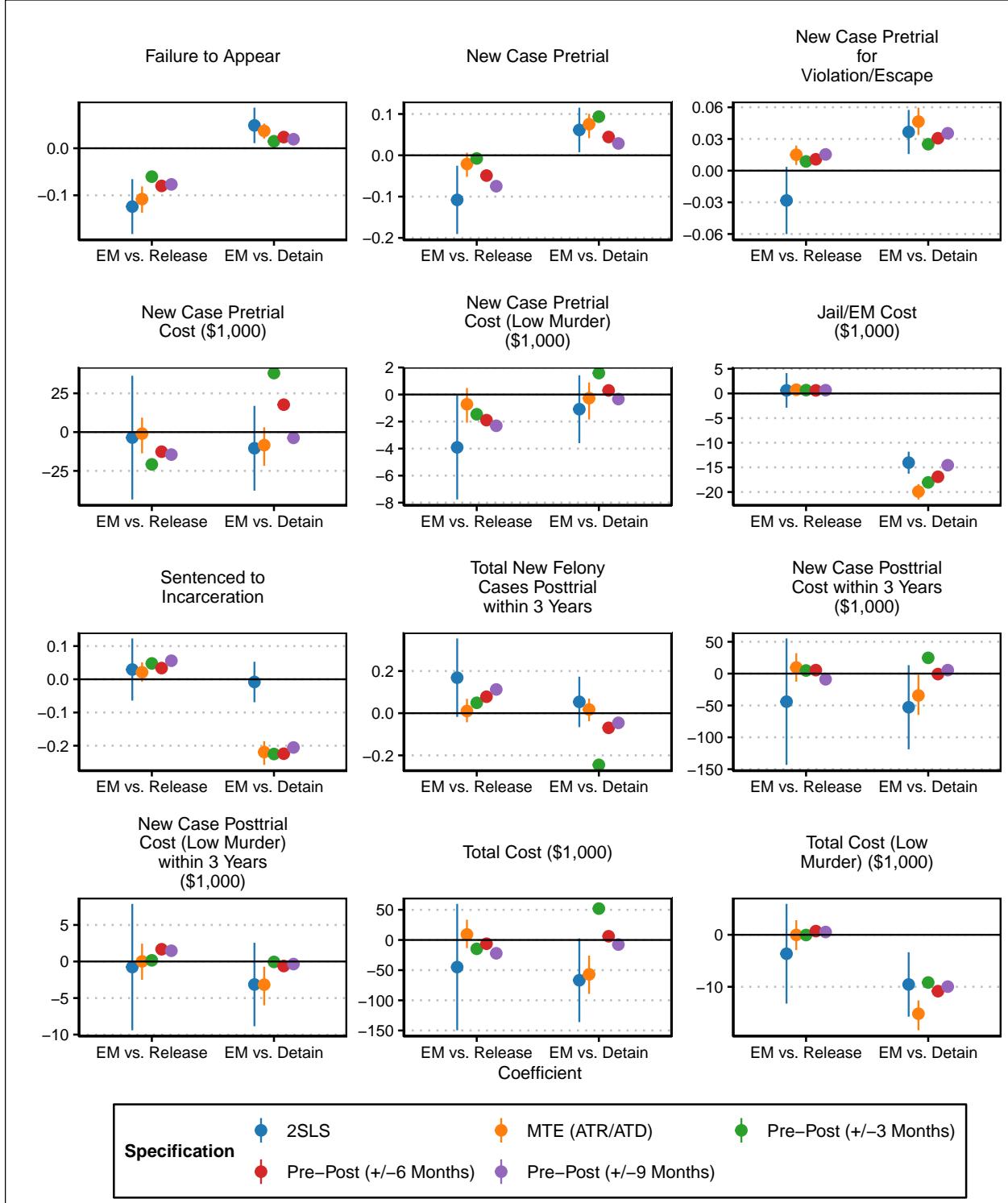
Note: Figure displays the marginal treatment effects (MTEs) across the distribution of defendant types (higher U means more unobservably resistant to higher treatment) for the effect of EM relative to release. MTEs are recovered using the main semiparametric estimation method with 3rd degree polynomial for Φ_s . 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level. Horizontal lines correspond to the CATE and CATR estimates and the 2SLS estimate for the effect of EM vs. release.

Figure 7: MTEs for EM vs. Detaign



Note: Figure displays the marginal treatment effects (MTEs) across the distribution of defendant types (higher U means more unobservably resistant to higher treatment) for the effect of EM relative to detain MTEs are recovered using the main semiparametric estimation method with 3rd degree polynomial for Φ_s . 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level. Horizontal lines correspond to the CATE and CATD estimates and the 2SLS estimate for the effect of EM vs. detain.

Figure 8: 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) and MTE (ATR/ATD over common support, Table 5) estimates. $+/-$ 3, 6, and 9 months refers to the time span before and after June 2013 that is used to compute the estimates.

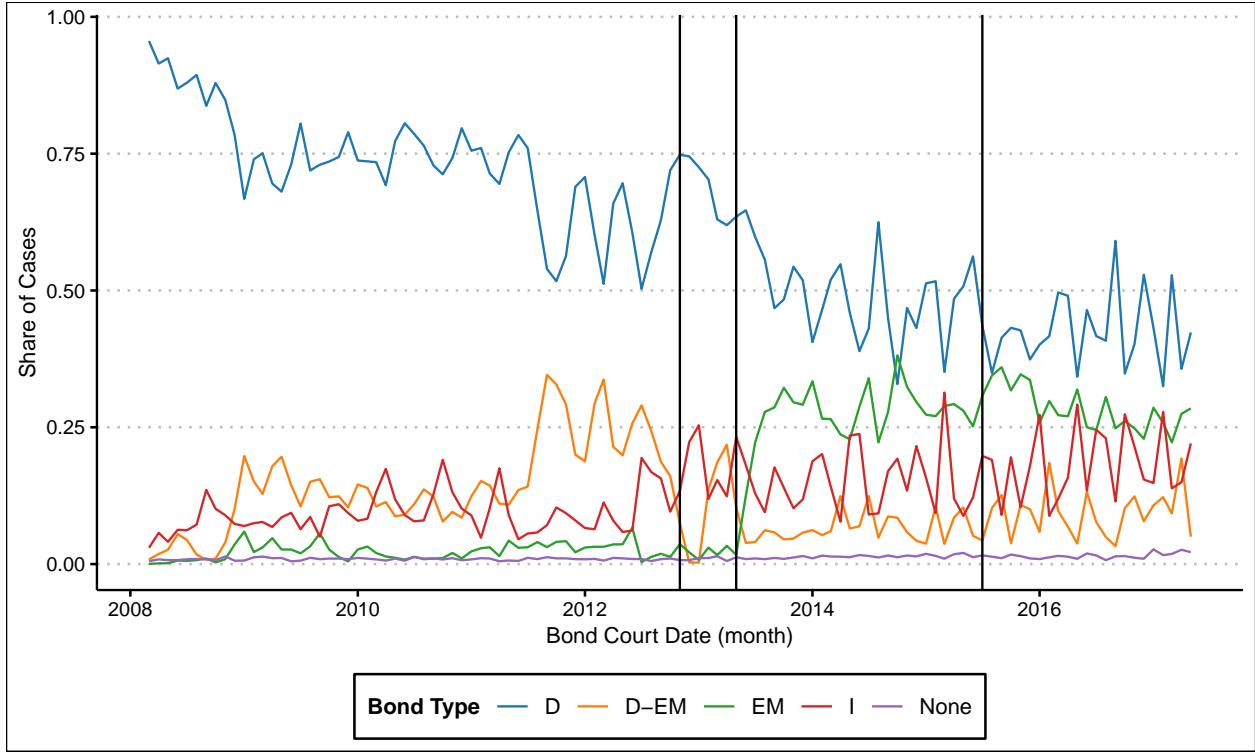
A Appendix A: Additional Analyses and Background

A.1 Background

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 (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 (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 (Federation (2017)). A report by the Civic Federation, using different data, also indicates that in September 2013, IEM was about 25% of dispositions (Federation (2017)).

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.

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 (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 (Sheriff (2020)). Yet, this system has run into technical issues due to false alarms (Daston (2022)).

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 — 83.69% can be linked to a specific arrest.

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.4. 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.

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.4, 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 (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 Bhuller and Sigstad (2023)

Bhuller and Sigstad (2023) show that 2SLS with multiple endogenous variables will assign proper weights to recover the weighted average of heterogeneous treatment effects across compliers if the instruments satisfy the (1) average conditional monotonicity and the (2)

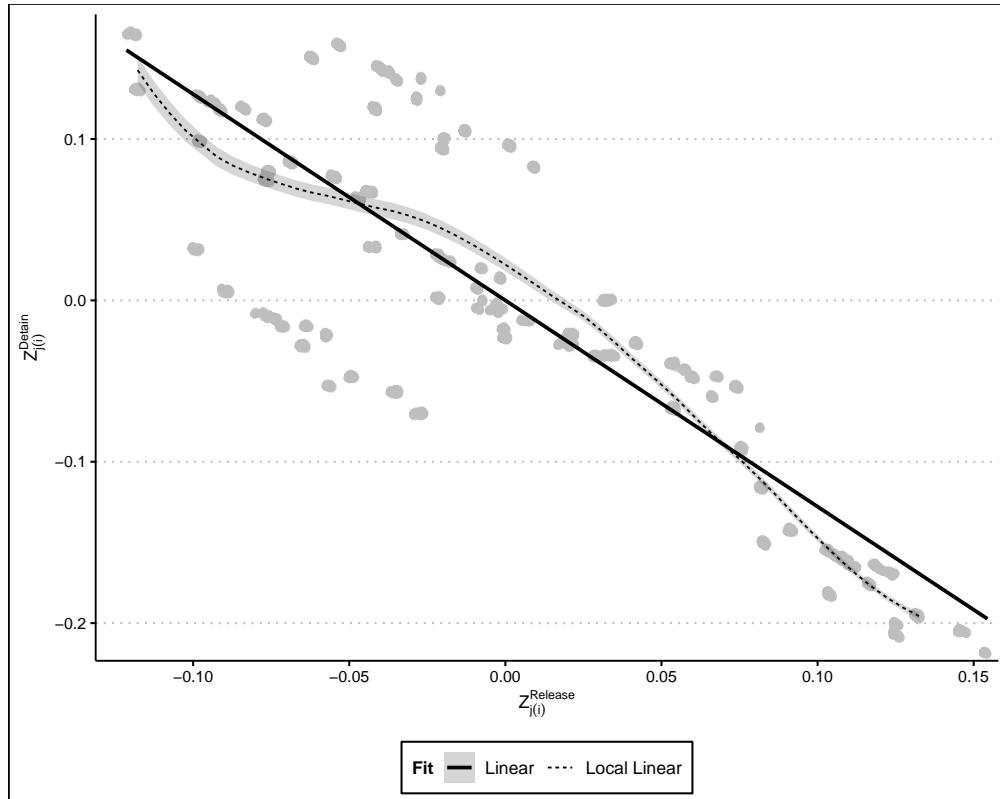
no cross effects assumptions. Assumption (1) requires that conditional on $z_{j(i)}^D$ ($z_{j(i)}^R$), the partial correlation between $z_{j(i)}^R$ ($z_{j(i)}^D$) and the defendant's uptake of release (detain) is non-negative for all defendant types — meaning it need not be non-negative for every value of the instrument, only on average across them.³⁷ Assumption (2) requires that conditional on $z_{j(i)}^D$ ($z_{j(i)}^R$), the partial correlation between $z_{j(i)}^R$ ($z_{j(i)}^D$) and the defendant's uptake of release (detain) is zero for all defendant types. This ensures that, beyond the relevant instrument, the off-instrument does not influence uptake of the treatment. In our setting, these assumptions imply that the first stage coefficients $\alpha_R^R > 0$ and $\alpha_D^D > 0$, while the cross coefficients should be zero, $\alpha_R^D = 0$ and $\alpha_D^R = 0$, in all subsamples.

The first two rows of Table 3 provides results for this test using five subsamples. In these subsamples, there are no violations of average conditional monotonicity, as α_R^R and α_D^D are positive and statistically significant, and there are no clear violations of the no cross effects assumption as I cannot reject the nulls that $\alpha_R^D = 0$ or $\alpha_D^R = 0$ at conventional levels. Going beyond these subsamples, I construct 42 subsamples using 21 defendant observables and run both first stage regressions on each — totaling 84 first stage regressions and 168 coefficients. After adjusting p-values for multiple hypotheses using the Holm (1979) correction, I find positive and statistically significant coefficients on α_R^R and α_D^D in all but 1 specification, and, similarly, I fail to reject the null that $\alpha_R^D = 0$ or $\alpha_D^R = 0$ in only 4 subsample. Both tests fail for the subsample of 10% defendants who have felony weapon charges. Overall, this suggests that neither assumption holds perfectly, implying that 2SLS using the leave-out propensities will not recover properly weighted LATEs if treatment effects are heterogeneous, though the extent of the violation is minor. The exclusion of cases with felony weapon charges does not influence main results (see Figure B.6, though it does indicate that EM has a smaller deterrent effect on pretrial crime and a positive effect on being sentenced to incarceration compared with release. They also provide a test for expected behavior in the case of ordered treatment, underwhich the expected values of each instrument should be a linear function of

³⁷A defendant's type refers to their set of potential responses in response to different instrument values.

the other. As shown in Figure A.2, the local linear fit does not consistently diverge from the linear fit except at the right tail of the instruments.

Figure A.2: Testing for Linearity Between Predicted Treatments



Note: Figure displays scatter plot of observations by values of leave-out judge propensities for Release and Detention as well as their relationship with a linear fit (solid line) and nonlinear fit (dashed line), for both full (top) and main felony (bottom) samples. Nonlinear fit is computed using local linear regression with bandwidth=0.025.

A.4 Additional Robustness

I compare my results against a variety of alternate samples and recompute the MTEs and treatment parameters and 2SLS estimates for each altered sample, as shown in Figure B.13. I begin by excluding felony weapon offenses, due to concerns over monotonicity violations (see Appendix A.3). I also construct samples: excluding unclassified felony types ('Class Unknown'); adding in D-EM bonds (where the defendant pays the bond amount to be released

from jail to EM); and dropping 2013 cases to ensure judges had sufficient time to adapt to the changes. Finally, I reconstruct π_1 and π_2 using a sample including misdemeanor defendants then restrict the analysis sample to the main sample. Overall, none of these alternate samples produce significantly different conclusions compared with the main sample.

I probe the robustness of my results to alternative codings of the treatments in Figures B.14 and B.15. First, defendants who receive EM bonds may not actually be admitted into the EM system. 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.14 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 (other filters applied) and the entire analysis is redone.³⁸ 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.15. Most results are similar, though the sample in which all IEM bonds with missing days in jail are coded as release produces the most extreme and inconsistent estimates for 2SLS but not for the CATR and CATD.

I also test the sensitivity of my results to alternative samples and specifications, as displayed in Figures B.11 - B.15, which include 2SLS estimates in addition to CATE/CATR/CATD estimates for relevant tests. First, I perform the robustness checks for exclusion restriction violations as discussed in Section 3.3.1, shown in Figure B.11. I change the cutoff from $T = 7$ to $T = 3$ and $T = 14$, which produces highly similar results. Additionally, I recode treatments using bond types and bond amount cutoffs, and the main conclusions are unchanged.

³⁸To reduce miscoding, I remove all D-EM cases prior to re-estimating the π 's as well.

Additionally, I probe the robustness of the results to the MTE specification. This includes altering the estimation of Φ to use a 5th-degree polynomial and b-splines of degree 3 and 4 with 4 and 6 degrees of freedom. Then, I add an interaction between judge fixed effects with time fixed effects in the construction of π_1 and π_2 , inspired by Goldsmith-Pinkham, Hull, and Kolesár (2022)'s method for avoiding contamination bias in the first stage. None of these produce significantly different the main results. Finally, to ensure the restriction to common support does not drive the results, I provide full support estimates ($u \in [0.01, 0.99]$) recovered using 1. the extrapolated values of the 3rd degree polynomial of the main method and 2. the assumption of jointly normal unobservables used in Heckman, Urzua, and Vytlacil (2006) and Cornelissen et al. (2018). Both sets of full support estimates tend to be smaller than the main results, though the conclusions are similar.

A.5 Relaxing Full Independence

Practically, interacting judge and defendant observables to produce the instrument Z serves two main purposes. First, it significantly increases the power and support of the judge-based instrument. As Figure B.8 shows, the range of predicted π_1 and π_2 for a given defendant (fixing X) across different judges is large.³⁹ Second, interacting judges and observables reduces, but does not resolve, concerns over the strict monotonicity assumption: rather than each judge having to agree on the ordering of all defendants, the interaction means judges agree on the ordering after removing their judge-specific preferences for that set of observables. This is useful if judges have heterogeneous preferences over defendant observables, which I find they do, as shown in Figure B.9, particularly over the charge types against defendants. Nevertheless, it is entirely plausible that judges still disagree on ordering U conditional on X and that monotonicity is violated.

One issue with this is that I do not, in the main specification, allow for MTEs to vary

³⁹The figure displays the level of inter-judge variation in treatment probabilities given observables — i.e., the distribution of how π_1 and π_2 within defendants (fixing X) across judges — showing that for the median defendant moving from the ‘most’ to ‘least’ stringent judge for their observables changes π_1 and π_2 by over 20 pp.

by observables (the full independence assumption) even though judge preferences can. I relax the full independence assumption, such that $Z \perp (\omega_1, \omega_2, \omega_3, U) | X$, such that the expectation of ω_s can depend on defendant observables. Specifically, I use a fully saturated vector of observables binning defendants into 4 groups, captured by X^* , and these bins broadly correspond to groups over which judges display heterogeneous preferences in treatment assignment. As in the main method, I estimate MTRs using polynomials of the estimates of π_1, π_2 (recomputed using X^*) interacted with X^* . Finally, ATR and ATD estimates are constructed by integrating over the common support within each bin then by taking a weighted sum over bin-specific ATR and ATD estimates with respect to predicted treatment probabilities.

By relaxing the full independence assumption in the MTE analysis to conditional independence, $Z \perp (\omega_1, \omega_2, \omega_3, U) | X^*$, where X^* is a fully saturated vector of a subset of the main observables. Specifically, defendants are grouped sequentially as follows: a. if they have a felony weapon charge, b. if they had no drug charges, c. if they had drug charges and over 5 past cases, d. if they had drug charges and at most 4 past cases. These four groupings are relatively large and judges display heterogeneous preferences over them for treatment assignment, preserving some of the variation used for estimation.

I estimate MTRs as a function of X , $\mathbb{E}[Y_s | X, U] = \mathbb{E}[\omega_s | X, U]$. The key difference with the main estimation for the full independence case is that I re-estimate π_1 and π_2 as functions of the judge and X^* , then $\mathbb{E}[\omega_s | X = x, U]$ is $\Lambda_s^x(\pi_{s-1}) - \Lambda_s^x(\pi_s)$ such that Λ_s^x are independent across values of X^* . Finally, I approximate $\mathbb{E}[\omega_s | X, U]$ using polynomials as before but interacted with X^* , $\phi_s^x[\Phi_s^x(\pi_{s-1}) - \Phi_s^x(\pi_s)]$. Finally, I recover ATD and ATR estimates over the common support by first recovering them for each value of X^* , i.e., $ATR(x)$ and $ATD(x)$, then I compute ATR (ATD) for the sample by summing over $ATR(x)$ ($ATD(x)$) weighted toward defendants who would be released (detained) based on their X^* and judge using $1 - \pi_1$ (π_2).

While much more flexibly estimated, the results are unsurprisingly noisy compared to

the main specification with much larger standard errors. Nevertheless, the point estimates are generally in line with the main results, as displayed in Figure B.16.

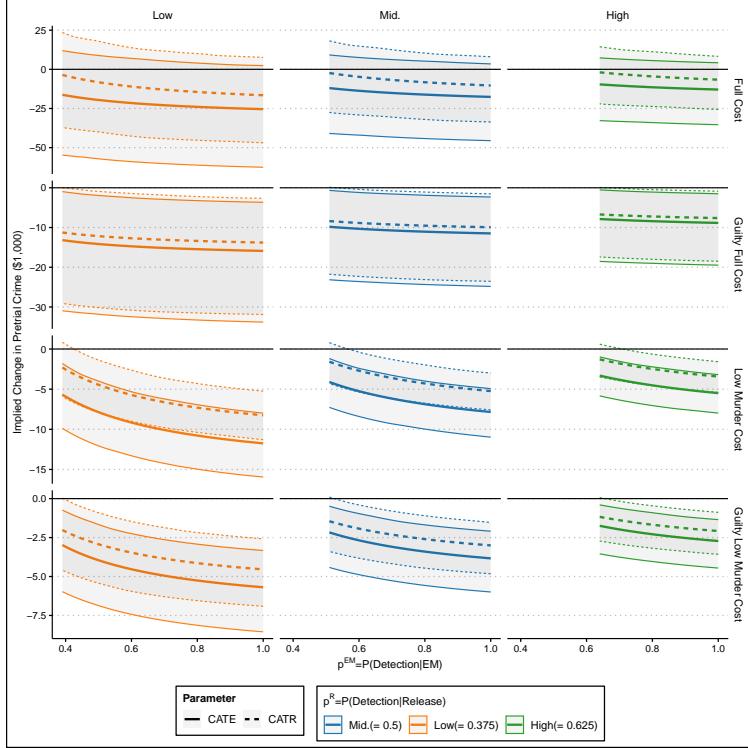
A.6 Bounding Changes in Crime and Elasticities

Then I recover changes in crimes using MTRs as a measure of arrests ($a^s(x, u) = m_s(x, u)$). In practice, I assume all crimes can be aggregated into a single type as weighted by their crime cost, which allows for a single crime-cost-weighted probability of detection. And I recompute the total cost of expanding EM to replace release and detention over the common support using adjusted crime costs to correct for the difference in probabilities of detection. Note that probabilities are relatively high as they are weighted toward higher cost crimes (e.g., murder) which are more likely to be detected compared with low cost crimes (e.g., drug use).⁴⁰

By fixing $p^R = p^D \in [0.375, 0.5, 0.625]$ and using a range of values for p^{EM} , I can bound changes in pretrial crime cost between release, EM, and detention. For EM vs. Release, the results are shown in Figure A.3. Across all probabilities of detection used for release and all $p^{EM} > p^R$, for released defendants I can never reject any significant effect of EM on pretrial crime costs relative to release using the full murder cost. Using low murder cost or subsetting to guilty charges only, I can rule out only small $< \$2,500$ changes in pretrial crime under the strong assumption that EM detects all pretrial crimes. As shown in Figure A.4 with detention (where the probability of detection on release is the post-detention release period pretrial), EM only has a positive effect on pretrial crime costs if I assume, improbably, that the probability of detection under EM is smaller than on release — i.e., if EM makes it easier to commit crime.

⁴⁰Based on FBI statistics, over 40% of violent crimes are cleared, with about 30% of robberies cleared, and almost 20% of property crimes are cleared. I assume slightly higher than average clearance rates for crimes committed by defendants who are released on bail because their location is known and they were in recent contact with the system and are tracked by the court system and the Sheriff (e.g., court attendance). See <https://ucr.fbi.gov/crime-in-the-u-s/2017/crime-in-the-u-s-2017/topic-pages/clearances> for 2017 clearance rates.

Figure A.3: Change in Pretrial Crime Costs (EM vs. Release) Across Different Probabilities of Detection

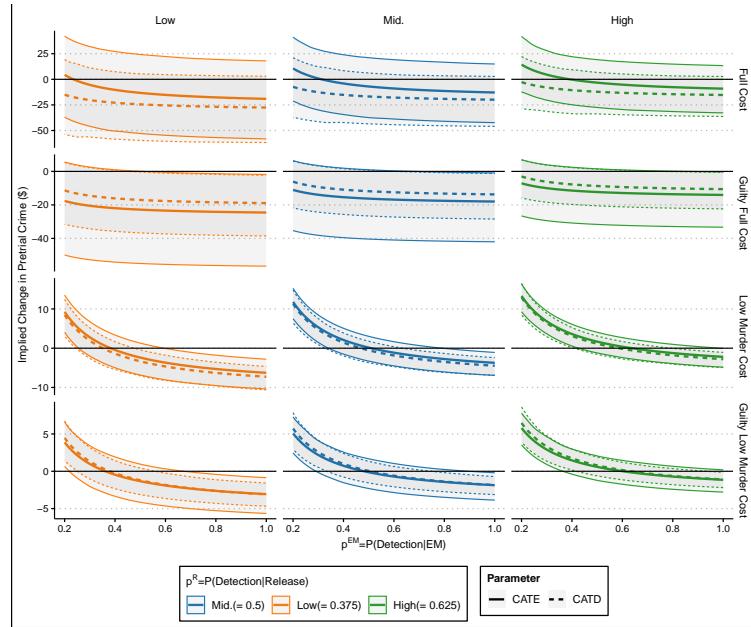


Note: Figure displays implied change in pretrial crime costs using a single value for the initial (release) probability of detection across other potential values for the increased probability of detection under EM. Colors correspond to probabilities of detection under release. Guilty charges refers to counting the crime cost only on charges which has a guilty finding. Low murder refers to using a reduced cost of murder in crime cost computations. Line types correspond to weights being used are for CATE (even weights across defendants and common support) or CATR (higher weights for released defendants across common support). 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level.

I also compute bounds on defendants' elasticity of committing pretrial crime with respect to the probability of detection (ϵ_p^c). If defendants are elastic with respect to probability of detection, $\epsilon_p^c < -1$, then increasing p (e.g., using EM) by 1% will reduce crime by > 1%, resulting in lower crime and lower arrests (despite a higher detection rate), producing double savings as both crime and punishment costs decline. However, if defendants are relatively inelastic ($-1 < \epsilon_p^c < 0$), the increasing p will reduce crime but arrests (and thus punishment costs) will rise. Elasticities for both low-level (e.g., drug, misdemeanor, traffic) and serious (violent, property, and other felonies) new cases pretrial can be bounded using different values of p^{EM}, p^R as discussed above and aggregating over the new case MTRs for new cases pretrial

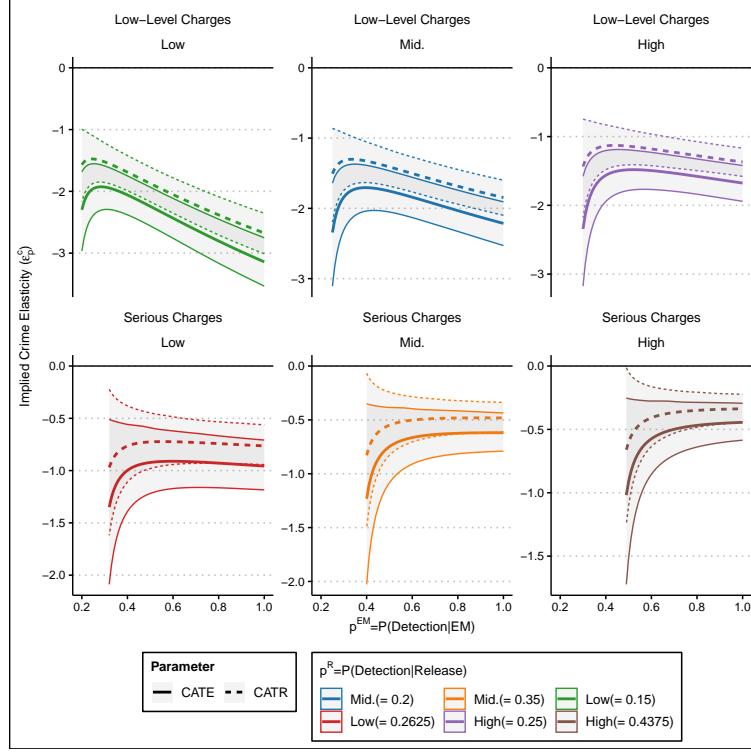
for serious and low-level offenses under release and EM.

Figure A.4: Change in Pretrial Crime Costs (EM vs. Detain) Across Different Probabilities of Detection



Note: Figure displays implied change in pretrial crime costs using a single value for the initial (post-detention release) probability of detection across other potential values for the increased probability of detection under EM. Colors correspond to probabilities of detection under post-detention release. Guilty charges refers to counting the crime cost only on charges which has a guilty finding. Low murder refers to using a reduced cost of murder in crime cost computations. Line types correspond to weights being used are for CATE (even weights across defendants and common support) or CATD (higher weights for detained defendants across common support). 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level.

Figure A.5: Estimated Bounds for Elasticities



Note: Figure displays implied elasticities for different pretrial criminal charge types using a single value for the initial (release) probability of detection across other potential values for the increased probability of detection under EM. Colors correspond to crime type and probabilities of detection under release. Line types correspond to weights being used are for CATE (even weights across defendants and common support) or CATR (higher weights for released defendants across common support). 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level for CATE and CATR.

Since the elasticity of some x with respect to some y is $\epsilon_y^x = \frac{y}{x} \frac{dx}{dy}$, and arrests (detected crime) $a = c \times p$, then $\epsilon_p^a = \frac{p}{a} \frac{da}{dp} = \frac{p}{c \times p} (c + p \frac{dc}{dp}) = 1 + \frac{p}{c} \frac{dc}{dp} = 1 + \epsilon_p^c$. So, $\epsilon_p^a < 0 \iff \epsilon_p^c < -1$. Furthermore, if defendants are elastic $\epsilon_p^c < -1$, then the punishment cost will decline under EM relative to release. I can infer a lower bound for the average defendant's elasticity with respect to the probability of detection, $\epsilon_p^c \approx \frac{\frac{\Delta c}{c^{\text{EM}} + c^R}}{\frac{\Delta p}{p^{\text{EM}} + p^R}}$, where $\Delta x = x^{\text{EM}} - x^R$, using the fact that *arrests* (a) = *crime* (c) $\times p$, to give us an expression for implied elasticity. This is a lower bound on $\epsilon_p^c \geq \hat{\epsilon}_p^c$ because any incapacitation effect (reducing c^{EM}) will be attributed to deterrence.⁴¹

$$^{41} \epsilon_p^c \approx \hat{\epsilon}_p^c = \frac{\frac{\Delta c}{c^{\text{EM}} + c^R}}{\frac{\Delta p}{p^{\text{EM}} + p^R}} = \frac{\frac{\Delta c}{\frac{c^{\text{EM}} + c^R}{2}}}{\frac{\Delta p}{\frac{p^{\text{EM}} + p^R}{2}}} = \frac{\frac{p^{\text{EM}} + p^R}{p^{\text{EM}} - p} \left(\frac{a'}{p'} - \frac{a}{p} \right)}{\frac{a'}{p'} + \frac{a}{p}} = \frac{\frac{2p + \Delta p}{\Delta p} \left(\frac{\Delta a + a}{\Delta p + p} - \frac{a}{p} \right)}{\frac{\Delta a + a}{\Delta p + p} + \frac{a}{p}}$$

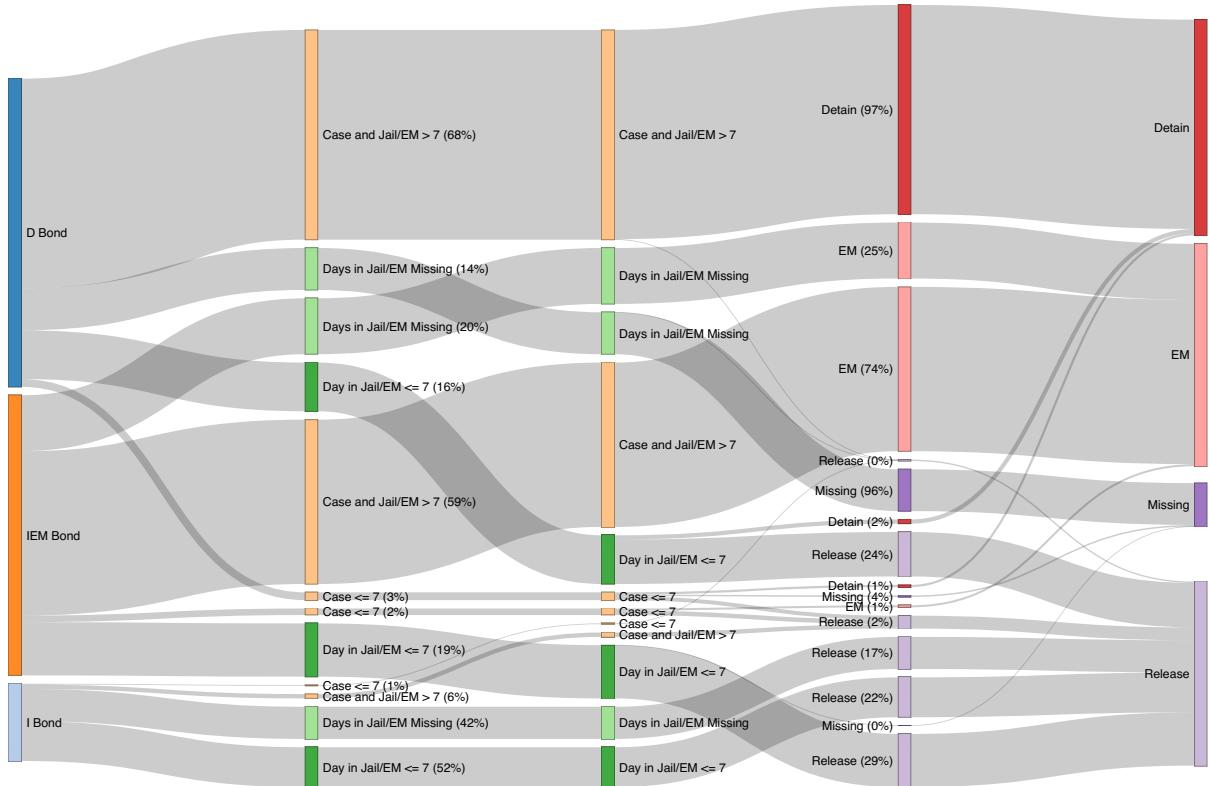
The results are displayed in Figure A.5 for low-level crimes and serious crimes using low, middle, and high values for p^R . The results indicate that low-level crime is likely elastic with respect to surveillance $\epsilon_p^{c,low} < -1$, though defendants more likely to be released are slightly less elastic. I cannot reject the null that serious crime is relatively inelastic with respect to the probability of detection ($\epsilon_p^{c,serious} > -1$) across all estimate types (CATE, CATR). The potential inelasticity of crime with respect to detection reduces the efficacy of surveillance as a social savings policy because even though crime is reduced, socially costly arrests may rise, thereby increasing punishment costs. Furthermore, these are both the lower bounds for elasticities, meaning defendants are likely more *inelastic* than $\hat{\epsilon}_p^c$ suggests. The inelasticity for serious crimes is consistent with the discussions of arrest rates and incarceration (Kleiman (2009), Durlauf and Nagin (2011)).⁴² This raises an important policy issue with respect to surveillance: if surveillance detects more crimes but punishments stay high, and defendants are not sufficiently elastic with respect to detection, then surveillance will result in less crime but more punishment.⁴³

⁴²Even if longer prison sentences deter crime, if potential criminals are relatively inelastic, then the incarcerated population will rise despite higher deterrence.

⁴³Relatedly, in the theoretical literature discussing racial disparities in hit rates, it has been argued that the optimal search rate is not that which produces the highest hit rates but that which deters the most crime (i.e., officers should target the more elastic group) (Persico (2002), Manski (2005)).

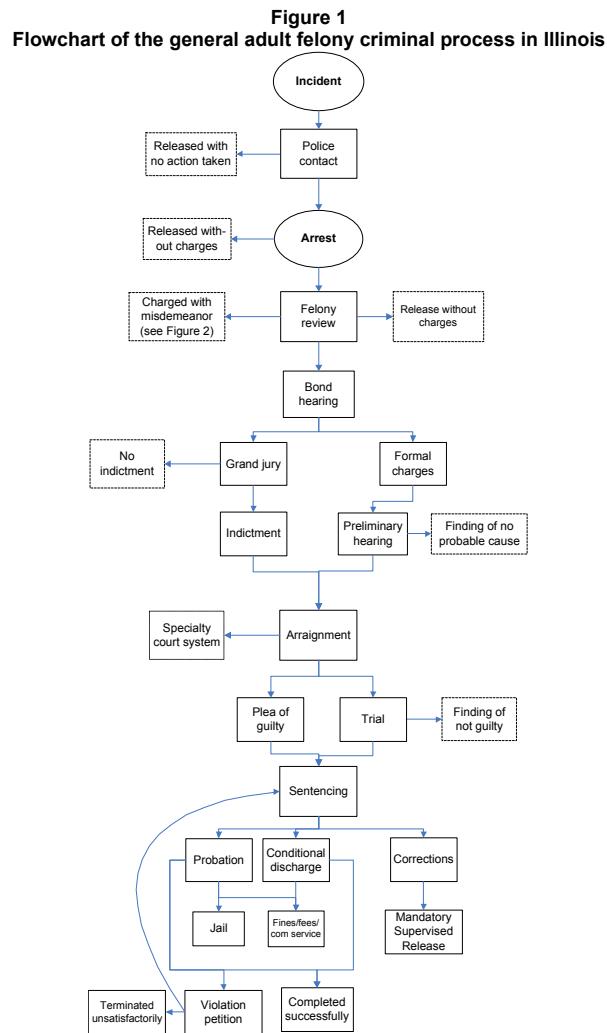
B Appendix B: Additional Figures and Tables

Figure B.1: 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).

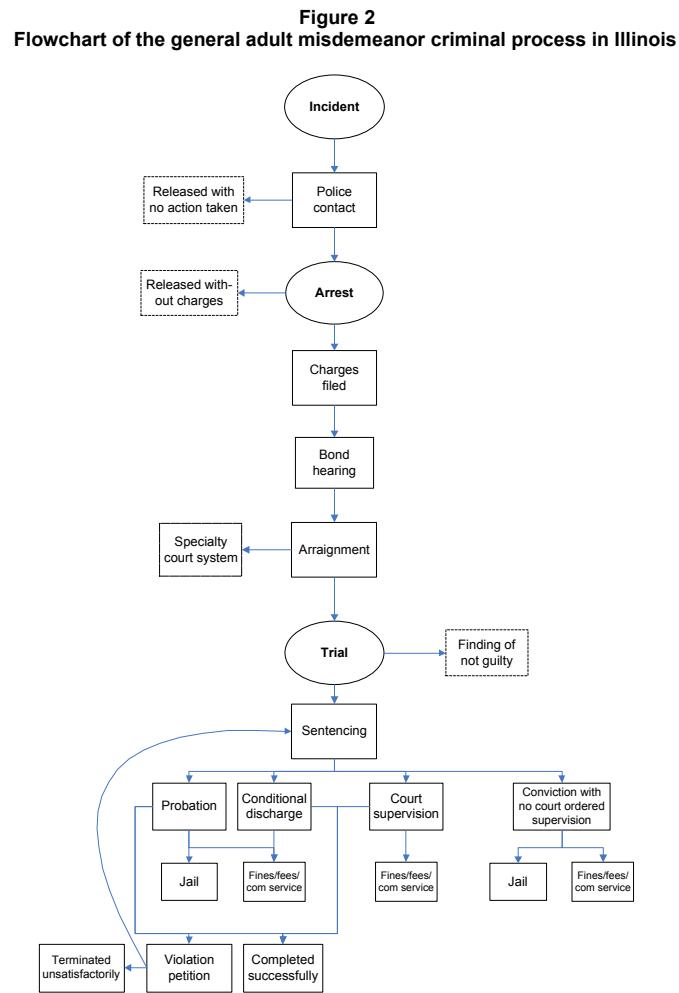
Figure B.2: Felony Case Flow Chart



2

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.

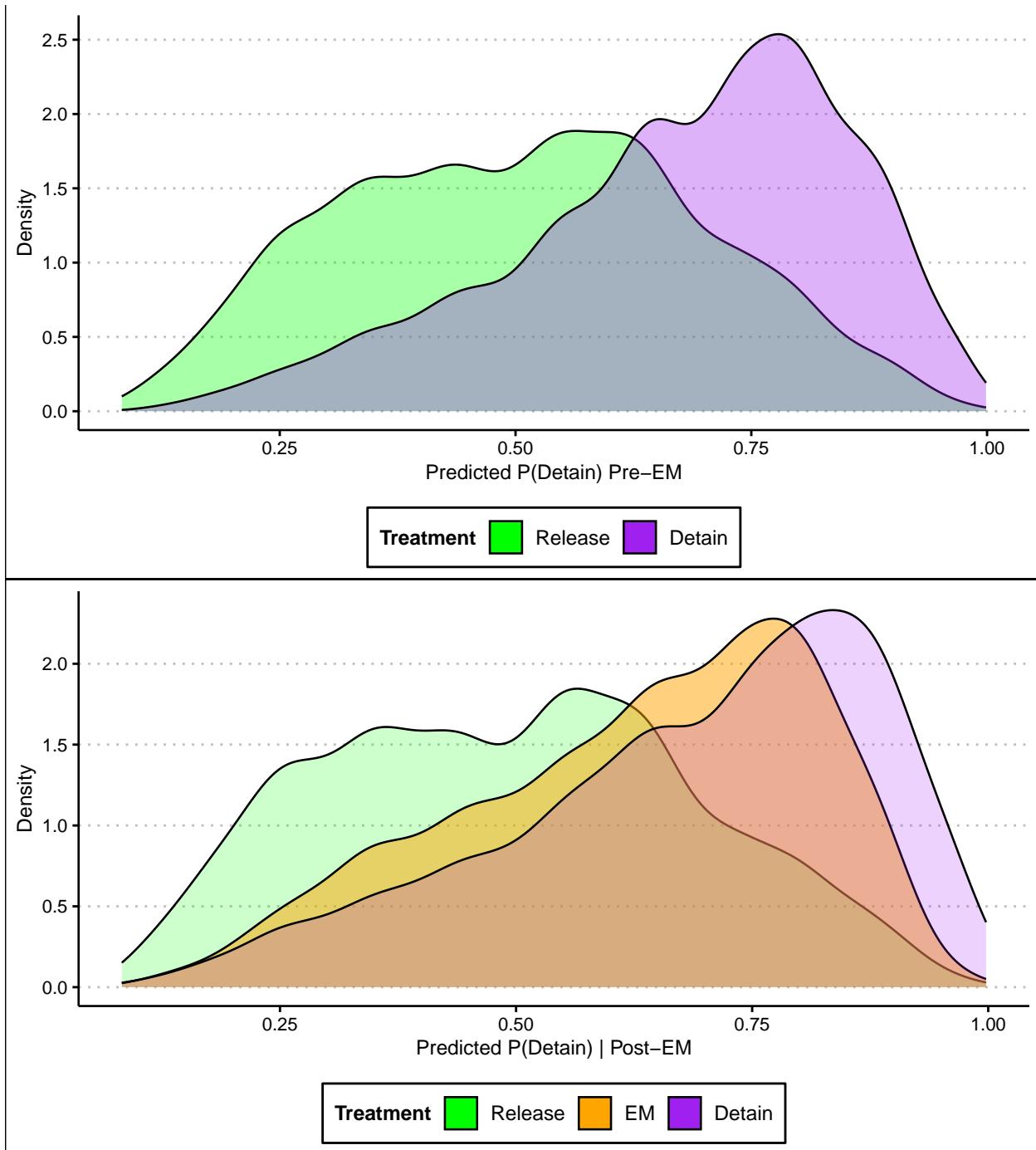
Figure B.3: Misdemeanor Case Flow Chart



3

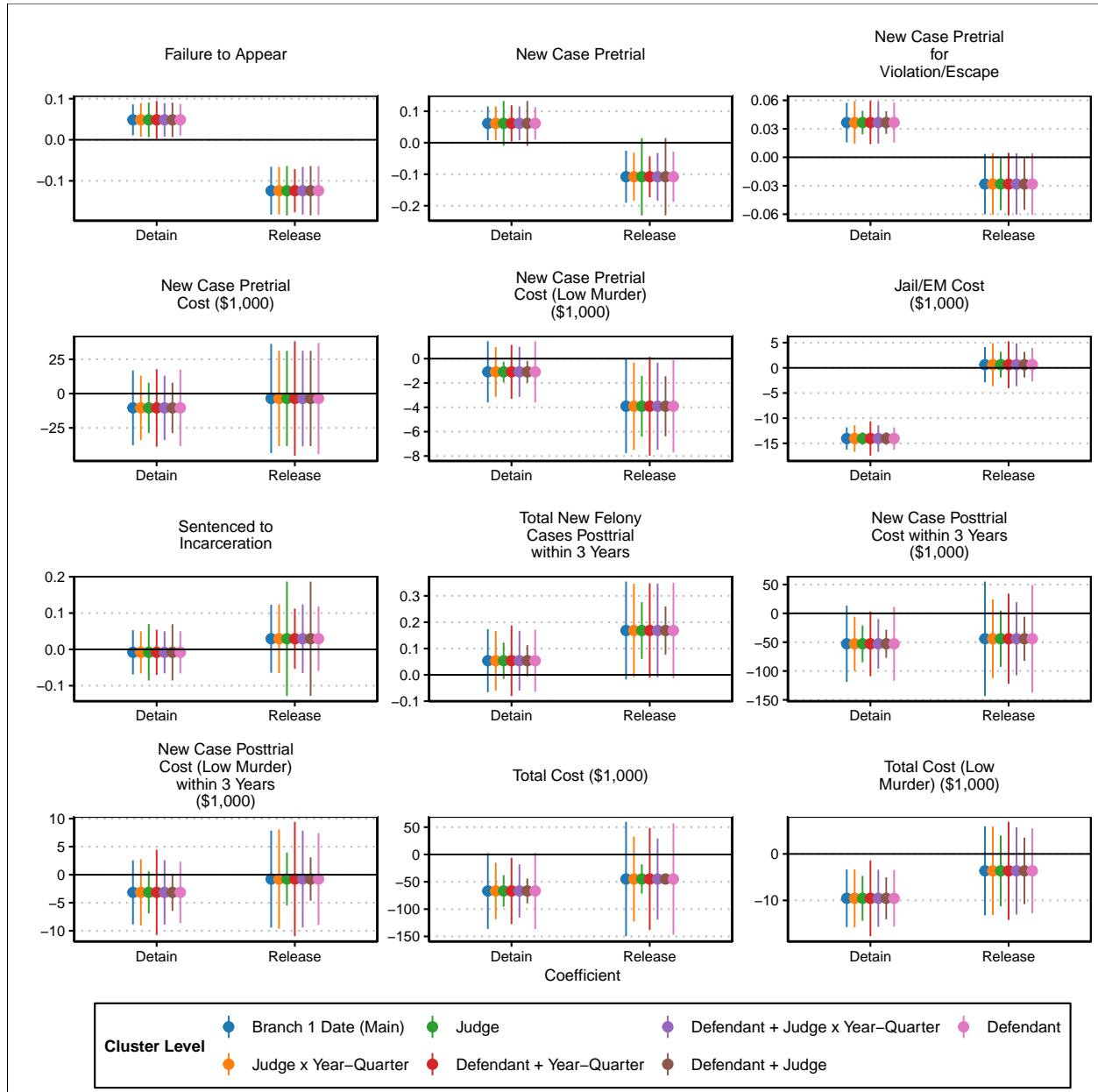
Note: Figure displays sequence of events for misdemeanor cases within Cook County — though document is meant for the entire Illinois criminal justice system more broadly. Source: Afeef et al. (2012), page 3.

Figure B.4: Distribution of Defendants by Treatment, Pre- and Post-IEM



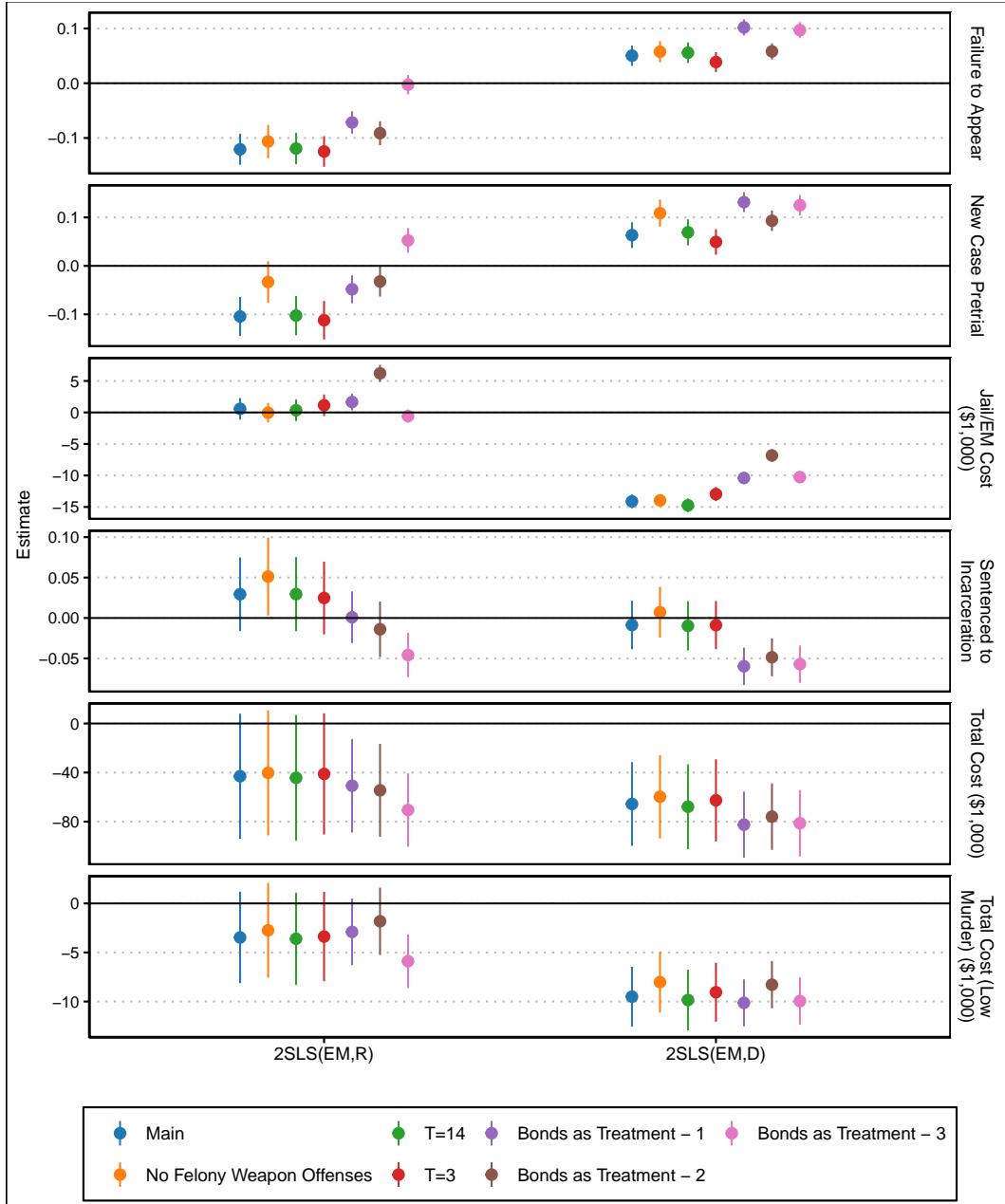
Note: Figures display the distributions of pretrial treatments during the period (Detain, Release) for pre-IEM (2009-2012) and post-IEM (July 2013 - 2015). X-axis is the defendant's predicted likelihood of being detained in the pre-IEM period based on their case observables. Coefficients for predicting 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.5: 95% Confidence Intervals under Alternative Clustering



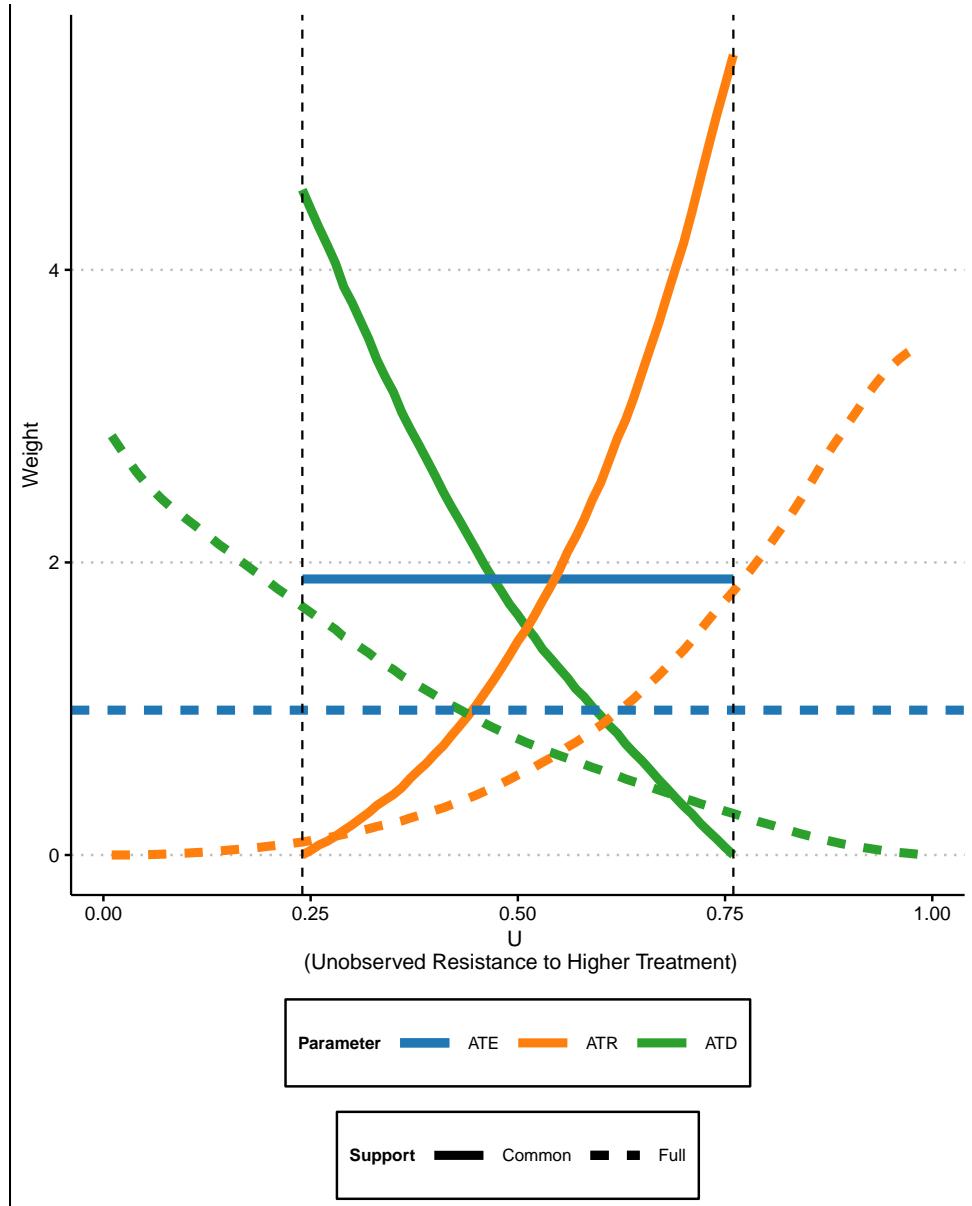
Note: Figure displays the main 2SLS coefficients with 95% confidence intervals using alternative clustering for main outcomes.

Figure B.6: 2SLS Robustness: Exclusion Restriction



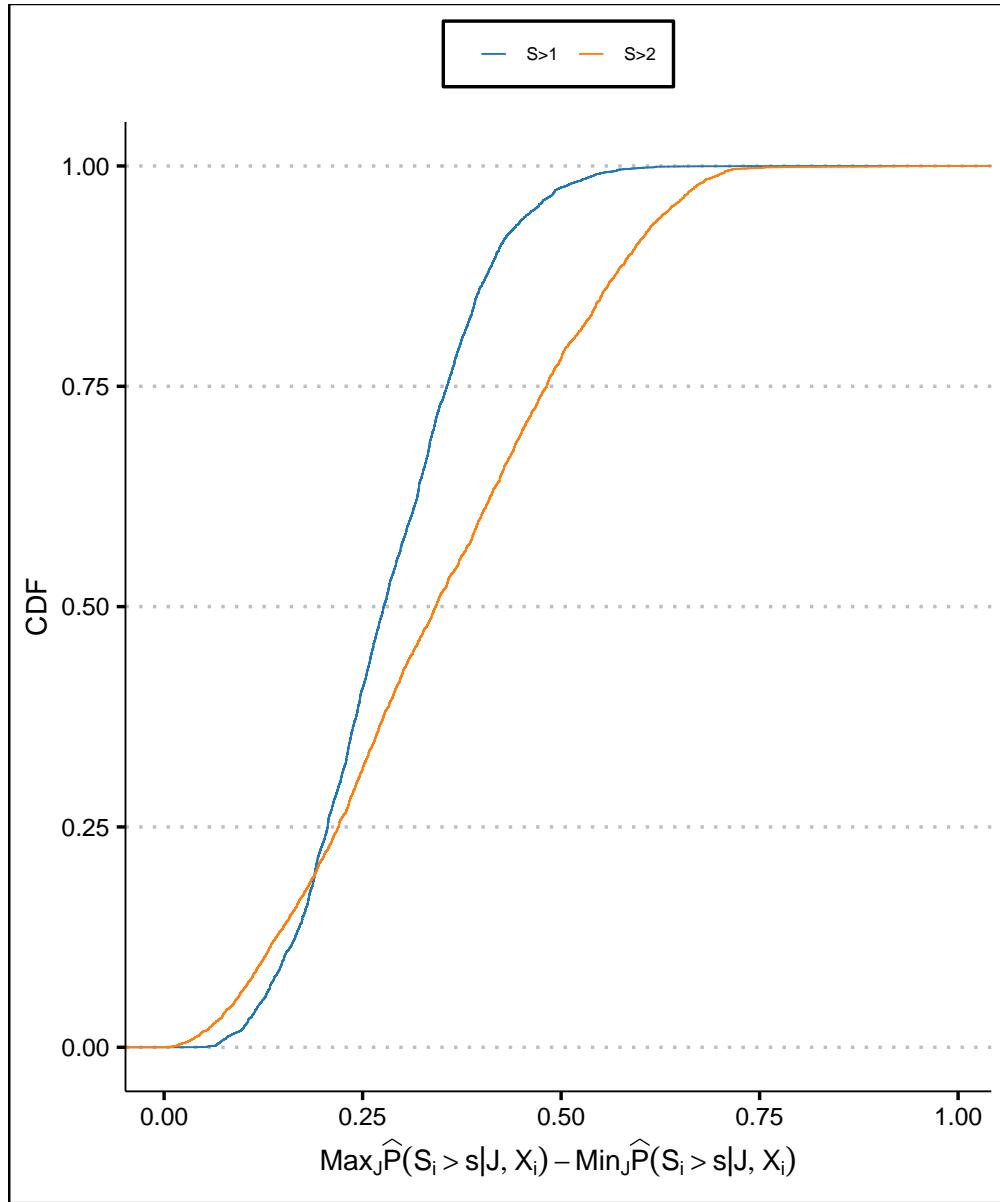
Note: Figure the 2SLS estimates using judge propensity instruments for the effect of EM vs. release and EM vs. detain under alternative treatment definitions and specifications to test the robustness of the main estimates against exclusion restriction violations for a subset of main outcomes. No felony weapon offenses excludes defendants charged with those offenses from the sample; $T = 3$ 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). Main and no felony weapon specifications use the main judge leave-out propensities, while the others use judge fixed effects. All specifications include the main controls and year-quarter fixed effects. 95% confidence intervals are displayed around the point estimates using standard errors clustered at the branch 1 date level.

Figure B.7: Treatment Effect Weights



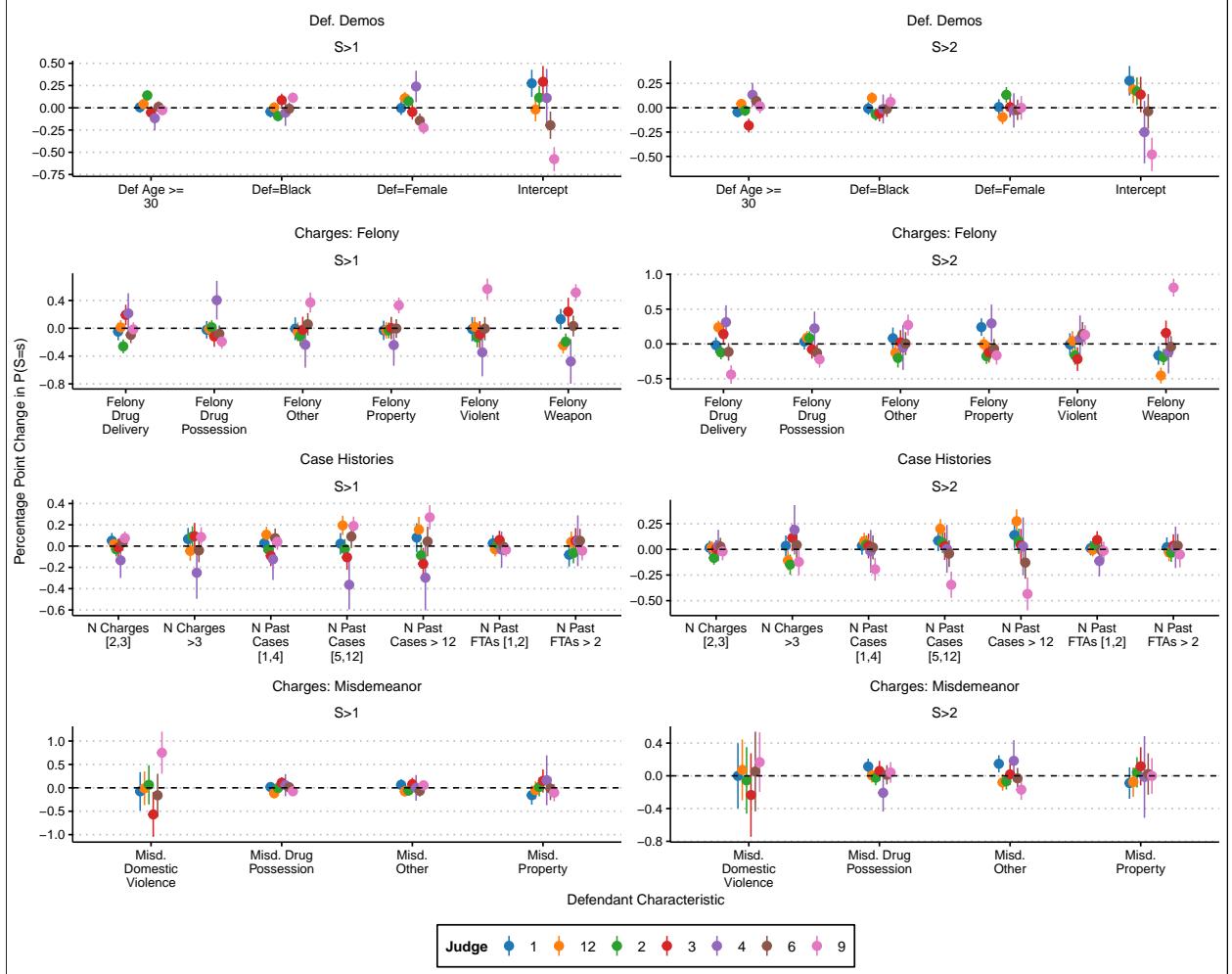
Note: Figure displays the weights used to sum over unobserved resistance to treatment ($U = \pi_1, \pi_2$ depending on the treatment margin) to compute each treatment effect. ATR(D) means average treatment effect within common support on the released (detained) defendants and thus applies a higher weight to higher (lower) resistance to higher treatment. The ATE applies equal weights as u is distributed uniformly.

Figure B.8: Distribution of Range of Across-Judge Treatment Probabilities



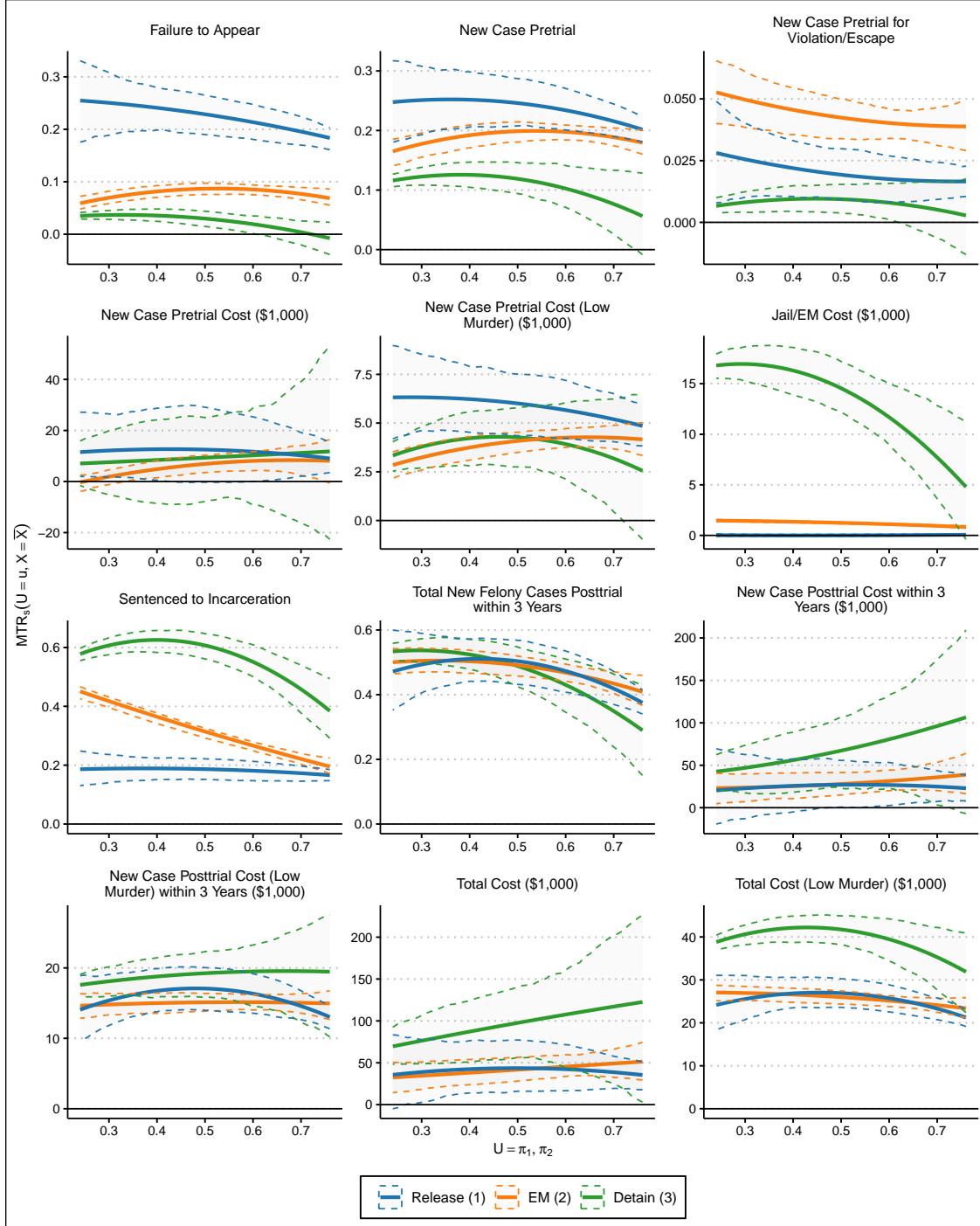
Note: Figure displays the empirical CDFs of defendants' range of treatment probabilities ($S > 1$ for EM or detain and $S > 2$ for detain), where the range is based on predicting their treatment probabilities using equation (5) and a fitted value is produced for each judge given the defendant's observables, then the range is the maximum treatment probability minus the minimum treatment probability across judges for that defendant.

Figure B.9: Judge Preferences over Defendant Observables



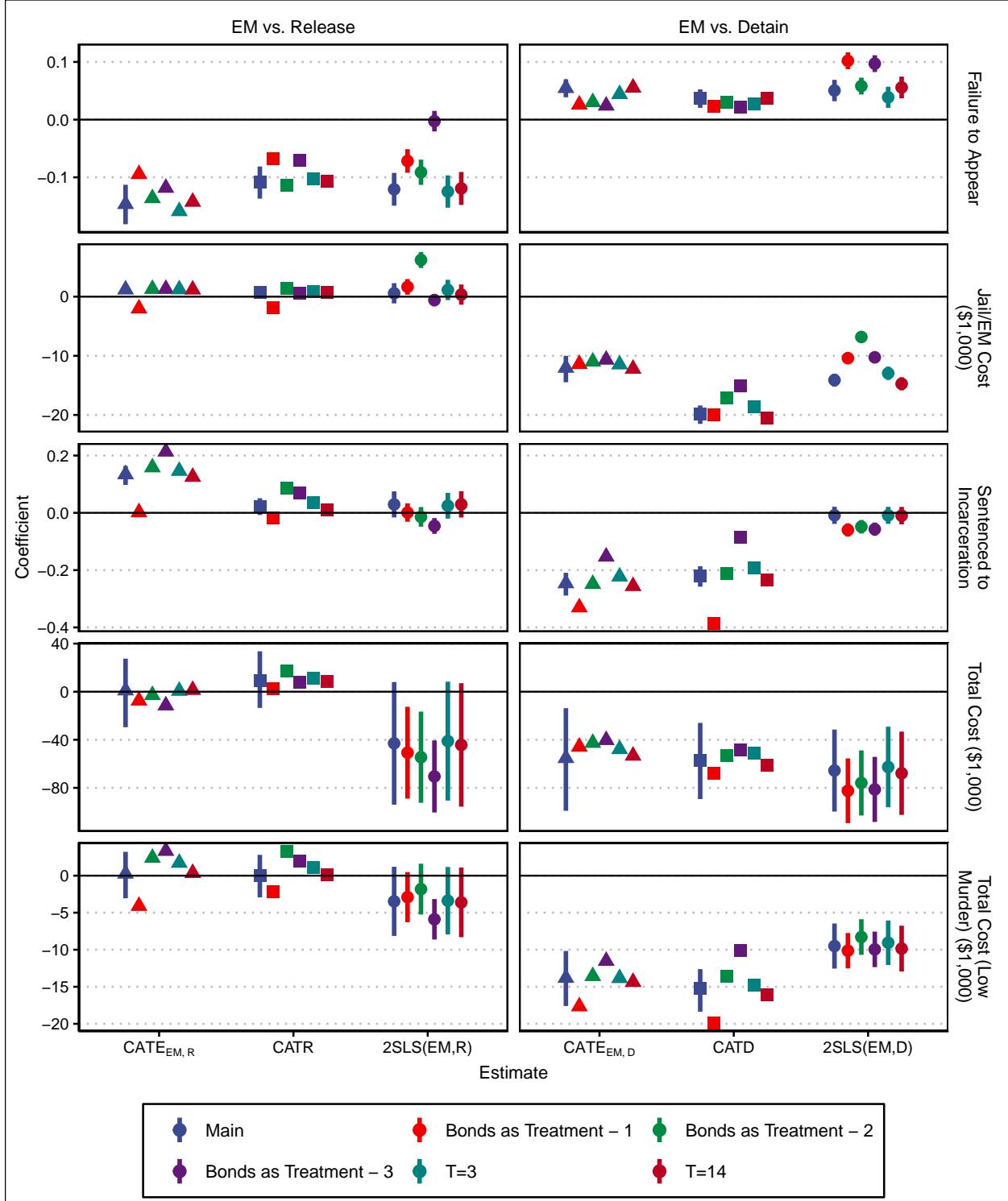
Note: Figure displays probit estimates of judge-specific coefficients (points, with 95% confidence intervals) which are de-meaned within observable type (to focus on across judge variation) for each defendant characteristic (binary variables) recovered by estimating equation (5). The outcome variables are if the defendant receives either EM or detention ($S>1$) or detention ($S>2$), and year-quarter fixed effects are included. Intercept is the baseline probability of the treatment by the judge.

Figure B.10: MTRs for Main Outcomes



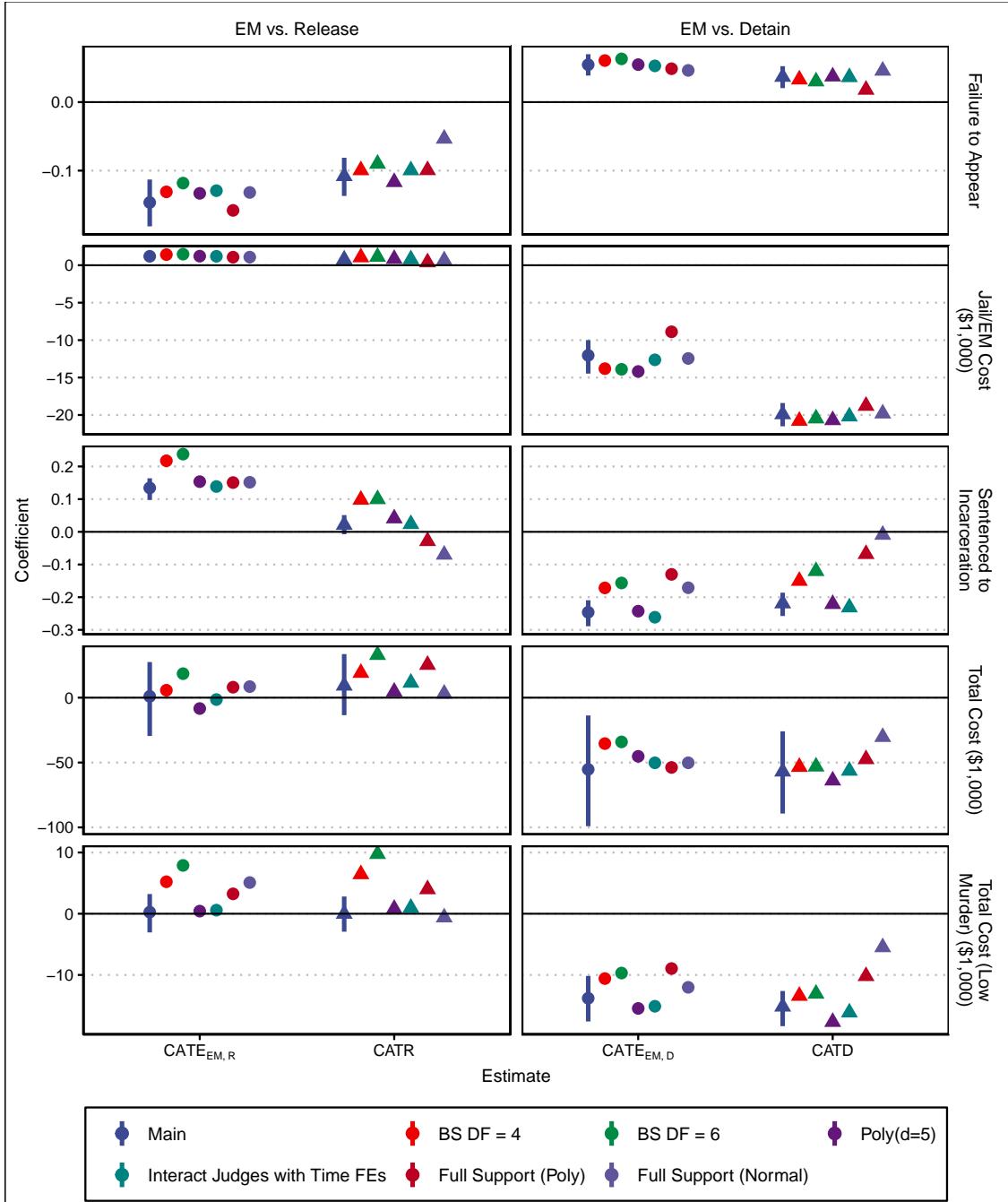
Note: Figure displays the marginal treatment response functions (MTR) across the distribution of defendant types (higher U means more unobservably resistant to higher treatment) for the expected response in each treatment level for main outcomes. MTRs are recovered using the main semiparametric estimation method (equation (6) estimated with 3rd degree polynomial for Φ_s). 95% confidence intervals are computed using 400 block bootstrap runs at the branch 1 date level.

Figure B.11: Treatment Effects for Bonds as Treatments and Changing T



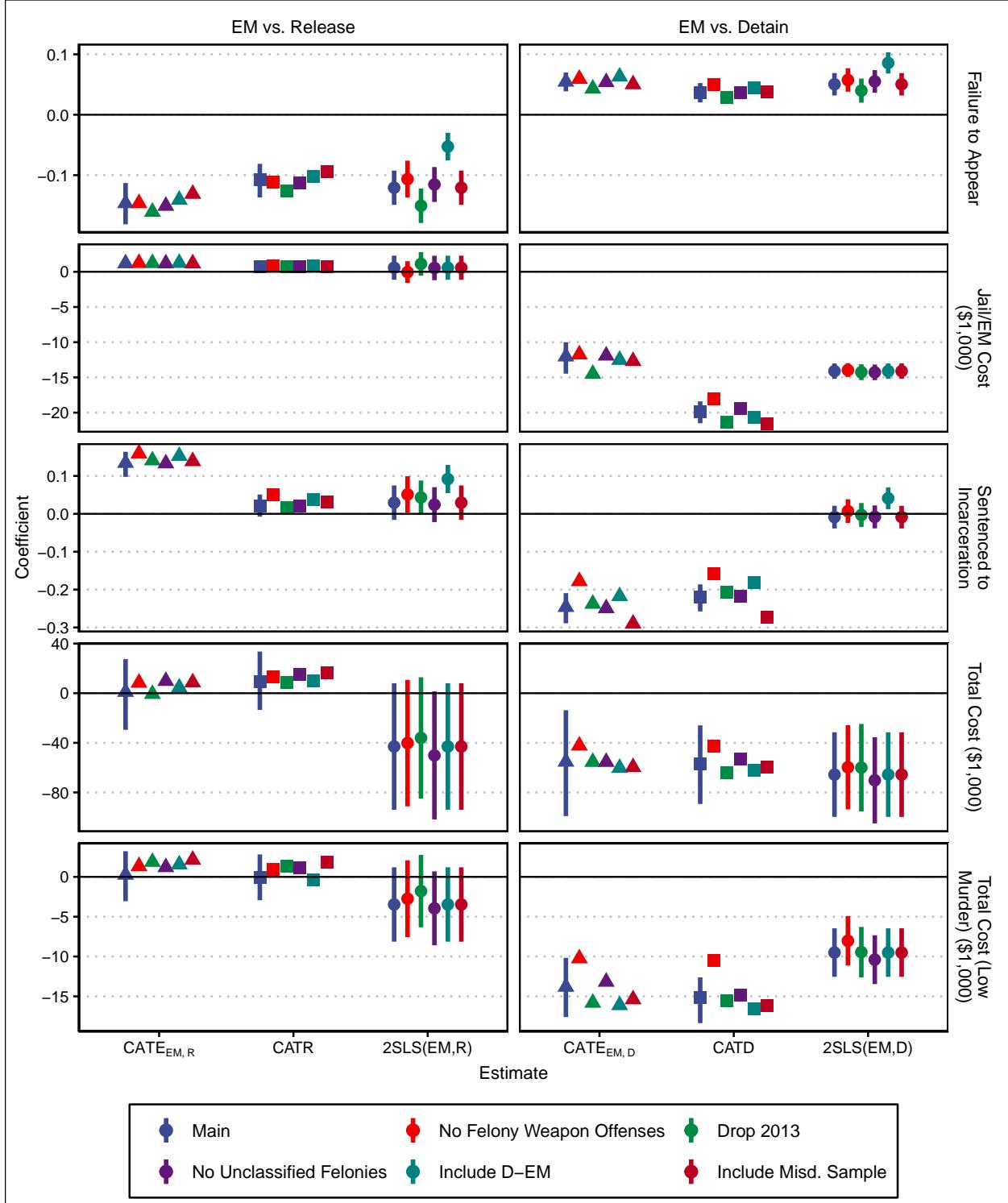
Note: Figure displays the CATE, CATR, CATD, and 2SLS estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right) under various robustness tests discussed in the main text. CATE(R/D) are constructed from MTEs recovered using the main semiparametric estimation method (equation (6) estimated with 3rd degree polynomial for Φ_s), unless otherwise specified. 95% confidence intervals of the estimates are computed using 200 bootstrap runs for non-2SLS, while 2SLS confidence intervals are constructed from standard errors clustered at the branch 1 date level.

Figure B.12: Treatment Effects for Alternate Specifications



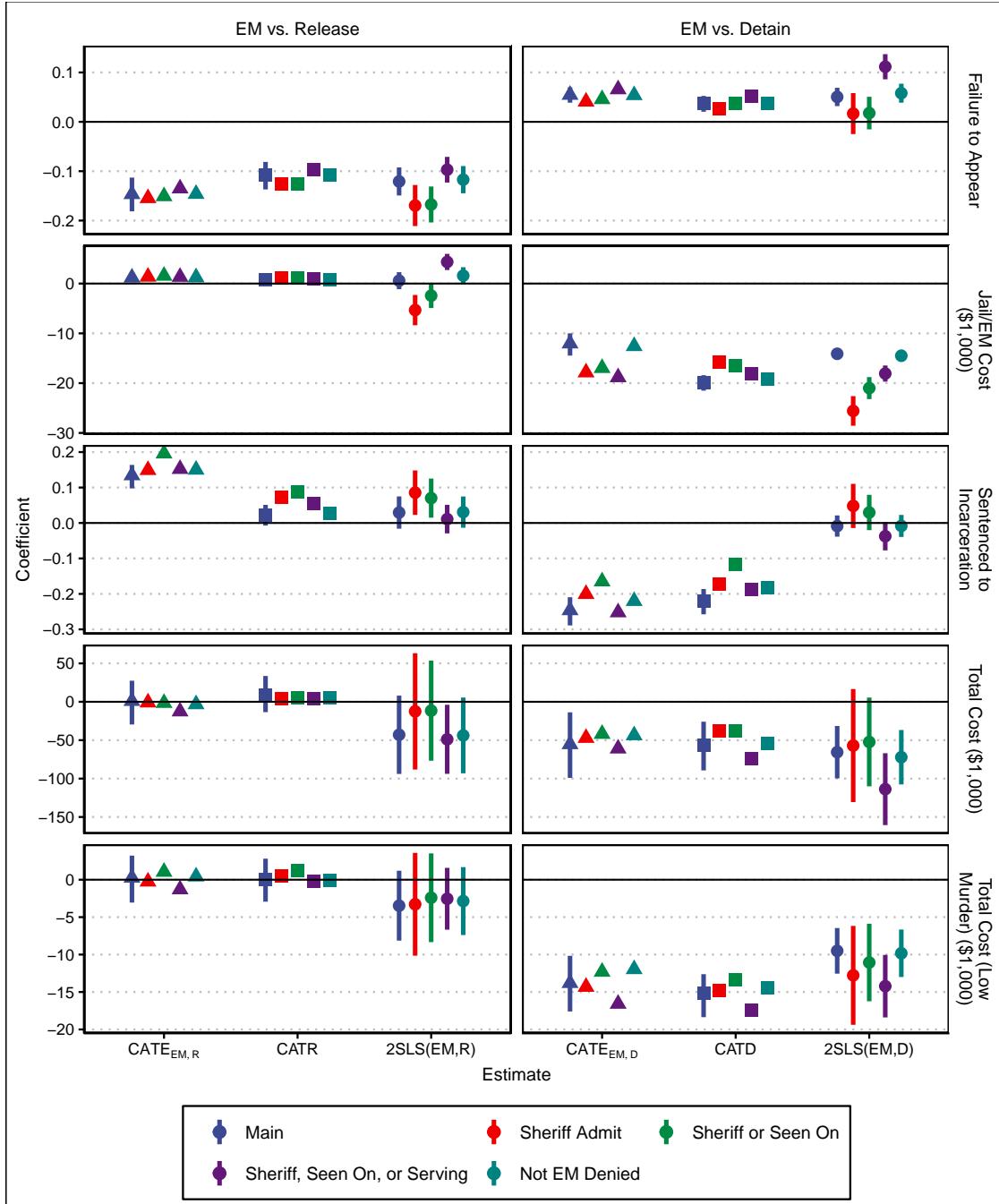
Note: Figure displays the CATE, CATR, and CATD estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right). CATE(R/D) are constructed from MTEs that are estimated semiparametrically with equation (6) where Φ_s are 3rd degree polynomials for all samples unless otherwise specified as discussed in the main text. 95% confidence intervals of the estimates are computed using 400 block bootstrap runs at the branch 1 date level.

Figure B.13: Treatment Effects for Alternate Samples



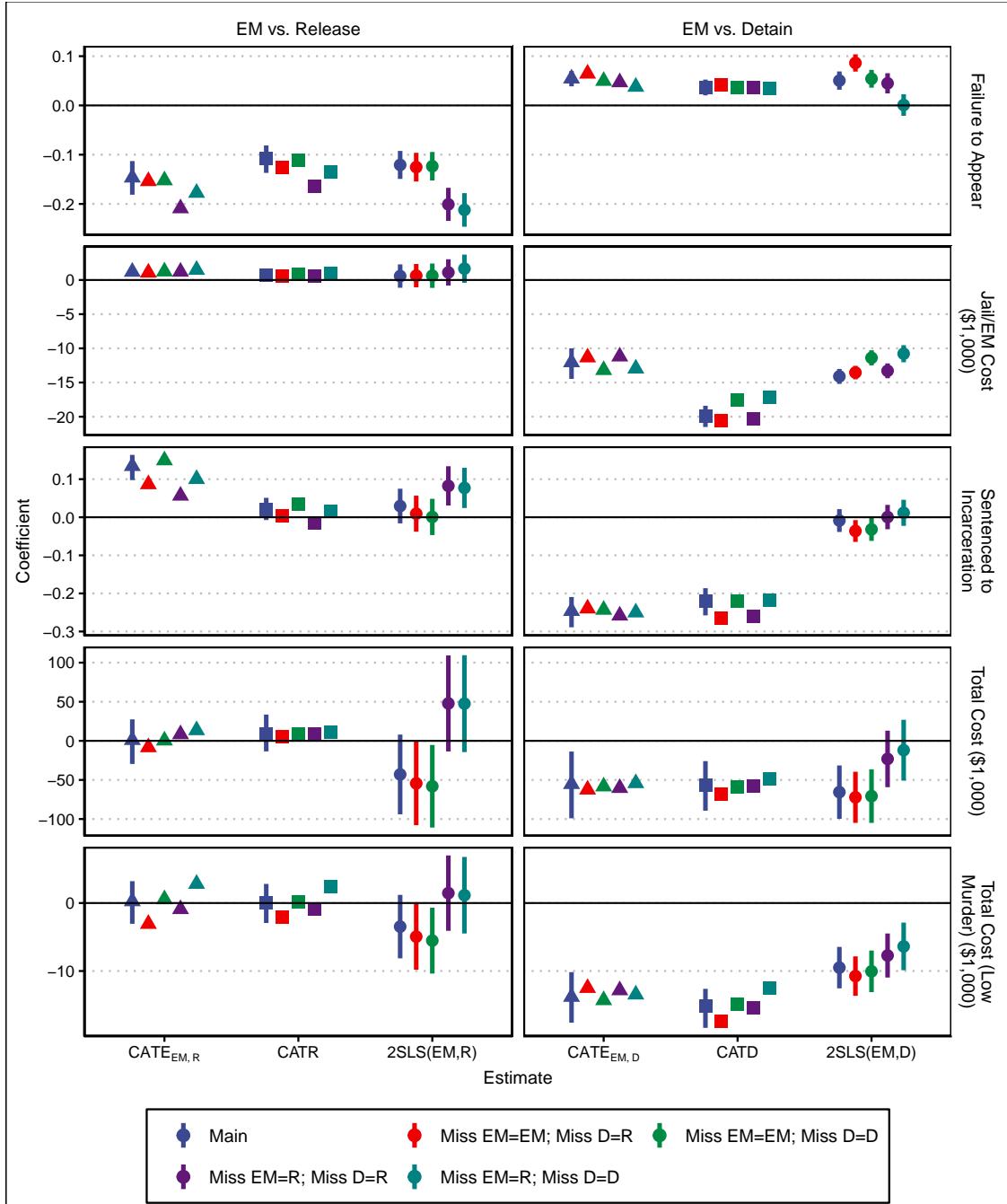
Note: Figure displays the CATE, CATR, CATD, and 2SLS estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right). CATE(R/D) are constructed from MTEs that are estimated semiparametrically with equation (6) where Φ_s are 3rd degree polynomials for all samples. 95% confidence intervals of the estimates are computed using 400 block bootstrap runs at the branch 1 date level for non-2SLS, while 2SLS confidence intervals are constructed from standard errors clustered at the branch 96 date level.

Figure B.14: Treatment Effects for Recoded Treatments using Disposition Codes



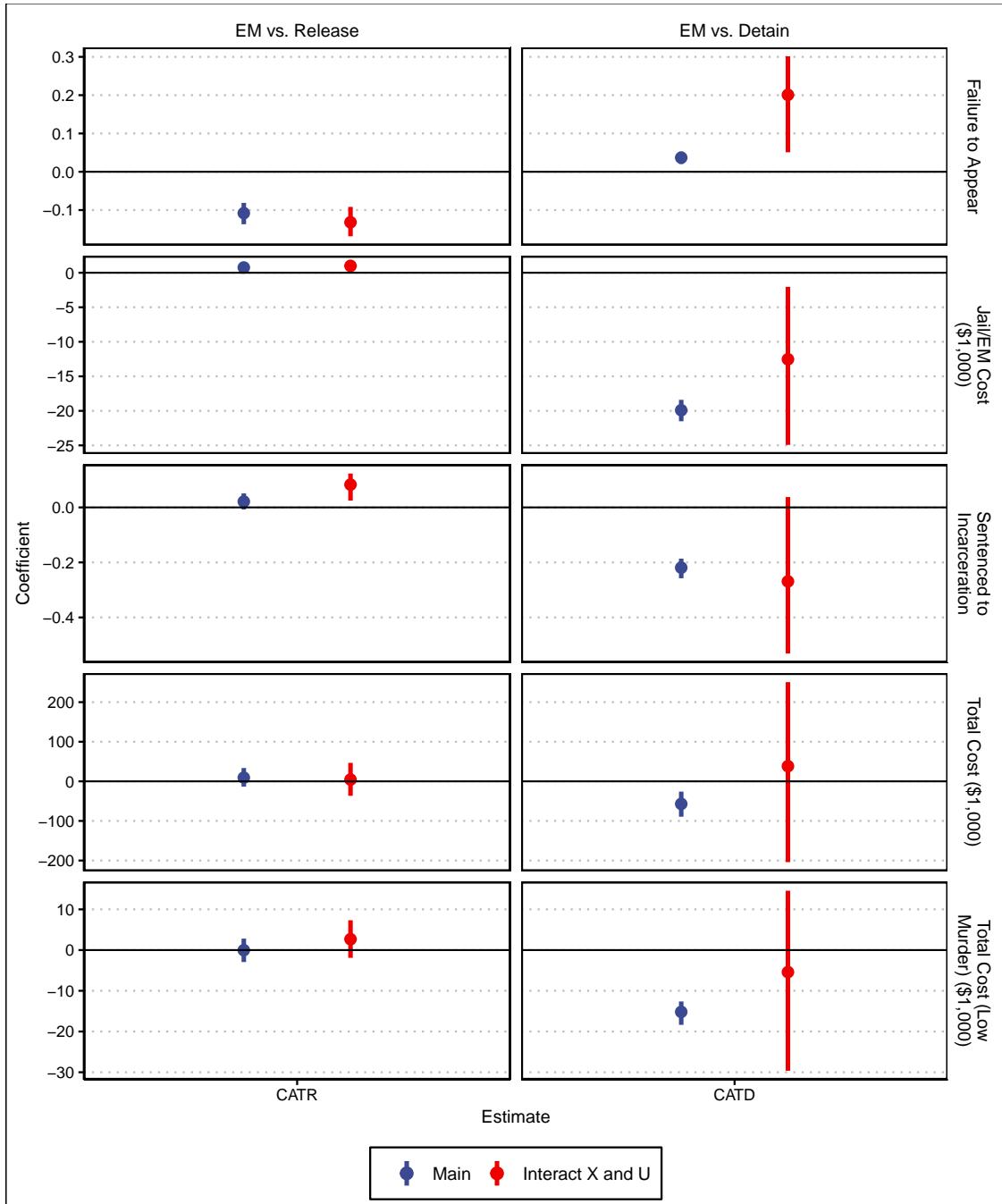
Note: Figure displays the CATE, CATR, CATD, and 2SLS estimates for various robustness checks for the effect of EM relative to Release (left) and EM relative to Detention (right) under different recodings of pretrial treatments using disposition codes to determine if a defendant was assigned to EM. "Sheriff 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). CATE(R/D) are constructed from MTEs recovered using the main semiparametric estimation method (equation (6) estimated with 3rd degree polynomial for Φ_s), unless otherwise specified. 95% confidence intervals of the estimates are computed using 400 block bootstrap runs at the branch 1 date level for non-2SLS, while 2SLS confidence intervals are constructed from standard errors clustered at the branch 1 date level.

Figure B.15: Treatment Effects for Recoded Missing Treatments



Note: Figure displays the CATE, CATR, CATD, and 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. CATE(R/D) are constructed from MTEs recovered using the main semiparametric estimation method (equation (6) estimated with 3rd degree polynomial for Φ_s), unless otherwise specified. 95% confidence intervals of the estimates are computed using 400 block bootstrap runs at the branch 1 date level for non-2SLS, while 2SLS confidence intervals are constructed from standard errors clustered at the branch 1 date level.

Figure B.16: Treatment Effects with Relaxed Full Independence



Note: Figure displays comparison between common support ATR and ATD estimates using the main specification of the MTE analysis (assuming full independence between X and U) and the results of relaxing this assumption by allowing $E[\omega_s|U]$ to depend on the value of X^* . See Section 4.3.2 for more details. 95% confidence intervals are computed using 400 block bootstrap runs 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
			(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(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 available Column (3) contains the crime type used in the form of: [Court data charge] recoded as [crime type].

Table B.2: Testing for Violation of Judge Assignment

Est.	(1)	(2)	(3)
LK / LK(1)	1	0.998	0.849
Pseudo-R2	0	0	0.15
Chi2	1	0	0
Def. Chars	X		
YQ Fes	X		

Note: Table displays results from a multinomial logistic regression with the judge assigned to a specific case as the outcome variable to test if defendant observables are predictive of judge assignment. Column (1) is the base model including no regressors, Column (2) only includes case and defendant level characteristics, Column (3) contains year-quarter (YQ) fixed effects. LK/LK(1) is the ratio of likelihoods of the model to that of Column (1), with smaller values indicating larger divergences from the base model. Pseudo-R2 is McFadden (1974)'s measurement of explained variation in the model. Chi2 is the Chi-squared test for the model in question being the same as the base model with no explanatory variables (Column (1)).

Table B.3: Overidentification and Monotonicity Tests

	Hansen Overidentification			Frandsen et al. (2023) Monotonicity and Exclusion					
	J-Statistic	p-value	Adjusted p-value	Release			Detain		
				(1)	(2)	(3)	(4)	(5)	(6)
Failure to Appear	19.486	<0.01	<0.01	1	<0.01	<0.01	1	<0.01	<0.01
New Case Pretrial	39.332	<0.01	<0.01	0.881	<0.01	<0.01	1	<0.01	<0.01
New Case Pretrial for Violation/Escape	7.447	0.114	0.799	1	<0.01	<0.01	1	<0.01	<0.01
New Case Pretrial Cost (\$1,000)	2.745	0.601	1	1	0.052	0.104	1	0.179	0.358
New Case Pretrial Cost (Low Murder) (\$1,000)	11.472	0.022	0.195	1	<0.01	<0.01	1	<0.01	<0.01
Sentenced to Incarceration	16.714	<0.01	0.022	0.511	<0.01	<0.01	0.65	0.01	0.02
Jail/EM Cost (\$1,000)	5.09	0.278	1	1	<0.01	<0.01	1	0.023	0.046
Total New Felony Cases Posttrial within 3 Years	5.876	0.209	1	1	0.038	0.076	1	<0.01	<0.01
New Case Posttrial Cost within 3 Years (\$1,000)	5.228	0.265	1	1	0.018	0.037	1	0.036	0.072
New Case Posttrial Cost (Low Murder) within 3 Years (\$1,000)	8.732	0.068	0.546	1	0.011	0.021	1	<0.01	<0.01
Total Cost (\$1,000)	3.856	0.426	1	1	0.018	0.037	1	0.104	0.208
Total Cost (Low Murder) (\$1,000)	6.082	0.193	1	1	<0.01	<0.01	1	<0.01	0.017

Note: Table displays the results for the Sargan-Hansen overidentification test (Columns (1) and (2)) (clustering at the branch 1 date level) and Holm (1979)-adjusted p-values (Column (3)), and the p-values of the Frandsen et al. (2023) monotonicity and exclusion test (Columns (4)-(9)), which rejects for violations of monotonicity or the exclusion restriction, using the binary treatments of release and detention, separately, with judge fixed effect instruments and controlling for year-quarter fixed effects.

C Appendix C: General Results for MTEs

Marginal treatment effects (MTE) provide a structure for understanding how treatments affect individuals differently based on their unobservable characteristics. Generally, this involves estimating the effect of a treatment for defendants ranked according to their unobservable ‘resistance’ to treatment. In the binary case, this is straightforward, and there has been significant work on estimating MTEs for binary treatments.

When multiple ordered treatments are introduced, identification becomes more complicated theoretically and more difficult to estimate empirically. Prior work tends to make simplifying assumptions about the joint distribution of unobservables; following Heckman, Urzua, and Vytlacil (2006) (HUV) and Heckman and Vytlacil (2007) (HV), this generally a jointly normal distribution (as in Cornelissen et al. (2018)) which results in MTEs with a linear shape for most values of u . Rose and Shem-Tov (2021) is an exception, as they extend Mogstad, Santos, and Torgovitsky (2018)’s bounding method to the case of ordered treatments by using discrete cutoffs as instruments to construct candidate marginal treatment response functions (MTRs) complying to specific shape restrictions, with the goal being bounding average treatment effects. In contrast, I focus on a general case, extending HUV, to point-identify MTEs and provide a method to estimate MTRs semiparametrically, relying on an interval of common support.

This section will focus on the case of estimating marginal treatment effects for ordered multi-valued treatments. The ordering of the treatments can be either cardinal — such as a dose-response function — or ordinal — where the treatments increase in intensity but have no clear quantitative distance. This section follows the work of HUV and HV closely. Building on their work, I relax one of HUV and HV’s identification assumptions. The main contribution is to provide a tractable method for semiparametric estimation of MTRs.

C.1 Set up

Consider an individual (i , though individual subscripts are suppressed for brevity) either choosing between or being assigned to one of \bar{S} different ‘levels’ of treatment which can be ordered by their intensity and given ranks such that $S \in \mathcal{S} = \{1, \dots, \bar{S}\}$. For example, in the context of this paper, the individual is a defendant and the treatment levels are pretrial release ($S = 1$), EM ($S = 2$), and detention ($S = 3$), so $\bar{S} = 3$, but the levels could correspond to medicine dosage, years of schooling, or intensity of a social service intervention. Each individual has some set of observable (to the econometrician) characteristics X , as well as unobservable features ($\{\omega_1, \dots, \omega_{\bar{S}}, V\}$). The unobservable factor V is observed by the agent who determines treatment.

Potential Outcomes For some outcome of interest, if the individual were assigned to treatment level $S = s$, their *potential* outcome is $Y_s = \mu_s(X, \omega_s)$. μ_s is some treatment-specific function, and X and ω_s is the individual-specific observable and unobservable components which contributes to Y_s . Let $D_s = 1$ if the defendant received treatment s , and $D_s = 0$ otherwise. From this, we know the observed outcome for an individual is:

$$Y = \sum_{s=1}^{\bar{S}} Y_s \times D_s = \sum_{s=1}^{\bar{S}} \mu_s(X, \omega_s) \times D_s.$$

Selection into Treatment The treatment level received by an individual is determined by a single index crossing a series of thresholds. The index, $T(Z, X)$, can be interpreted as the individuals ‘net benefit’ from a higher level of treatment (in the eyes of the agent assigning their treatment, possibly the individual themselves). The individual receives a treatment level higher than $S > s$ (e.g., $s + 1, s + 2, \dots$) if and only if $T(Z, X) > C^s(W)$, where $C^s(W)$ is the cutoff value that is the highest level of $T(Z, X)$ which will result in being assigned to treatment level s . As before, X denotes observables that influence potential outcomes and possibly selection, while Z and W are observable instruments which do not directly influence outcomes but do influence selection either through the index (Z) or cutoffs (W).

From this, an individual is assigned to treatment level s if and only if:

$$D_s = 1(S = s) = 1[C^s(W) \geq T(Z, X) > C^{s-1}(W)]$$

where $C^{\bar{S}} = +\infty$ and $C^0 = -\infty$, because no one can receive a treatment higher than the highest level or lower than the lowest level (1), and $C^{s-1}(W) \leq C^s(W) \forall s$.

I assume that the single index can be decomposed into $T(Z, X) = \tau(Z, X) - V$ where $\tau(Z, X)$ is the individual's 'benefit' from a higher level of treatment and V is the individual's resistance to (or cost of) a higher level of treatment. The single index assumption has multiple implications. First, this separable form implies monotonicity, because for all individuals going from $\tau(Z, X)$ to $\tau(Z', X)$ will shift $T(\cdot, X)$ in the same way (similarly for if Z is fixed and X changes), and thus (weakly) move S in the same direction (HUV). Second, the existence of a single index, V , that determines treatment (conditional on observables) means that all factors can be reduced down to a single dimension in determining which treatment is optimal (to the agent deciding).

With this model, we seek to identify the effect of being assigned to treatment $s + 1$ relative to treatment s for an individual with resistance level v and observables x :

$$MTE_{s+1,s}(x, v) = \mathbb{E}[Y_{s+1} - Y_s | V = v, X = x]$$

C.2 Identification

In order to identify transition-specific marginal treatment effects, I assume the following assumptions from HUV (denoted HUV 1-6, though called OC 1-6 in the original paper):

Assumption 1. *HUV1: $(\omega_s, V) \perp (Z, W)$ for all $s \in \mathcal{S}$ conditional on X .*

Assumption 2. *HUV2: $\tau(Z, X)$ is a non-degenerate random variable conditional on X and W .*

Assumptions 1 and 2 ensure that the instruments are valid and relevant after conditioning

on regressors.

Assumption 3. *HUV3: The distribution of V is absolutely continuous conditional on X .*

From Assumption 3, we can use probability integral transformation to get $U = F_V(V|X = x)$ which is uniformly distributed $U \sim Unif[0, 1]$, conditional on X :

$$\begin{aligned} D_s &= 1[C^s(W) \geq T(Z, X) > C^{s-1}(W)] \\ &= 1[F_V(\tau(Z, X) - C^s(W)) \leq F_V(V) < F_V(\tau(Z, X) - C^{s-1}(W))] \\ &= 1[F_V(\tau(Z, X) - C^s(W)) \leq U < F_V(\tau(Z, X) - C^{s-1}(W))] \end{aligned}$$

Let $\pi_s(Z, X, W) = F_V(C^s(W) - \tau(Z, X)) = \Pr(S > s|Z, X, W)$. By construction, $\pi_0(Z, X, W) = 1$ and $\pi_{\bar{S}}(Z, X, W) = 0$. Then the selection equation becomes: $D_s = 1[\pi_s(Z, X, W) \leq U < \pi_{s-1}(Z, X, W)]$. With this, we can redefine the MTE with the selection unobservable being in terms of U (with a known distribution) rather than V (with an unknown distribution):

$$MTE_{s+1,s}(x, u) = \mathbb{E}[Y_{s+1} - Y_s | U = u, X = x]$$

Assumption 4. *HUV4: $E|Y_s| < \infty \forall s \in S$*

Assumption 5. *HUV5: $0 < \Pr(S = s|X) < 1 \forall s \in S$*

Assumption 6. *HUV6: The distribution of $C^s(W)$ conditional on X and Z and other $C^{s'}$ is non-degenerate and continuous $\forall s \in \{1, \dots, \bar{S} - 1\}$.*

With these assumptions, HUV and HV show that the MTE is identified by taking:

$$\frac{\partial \mathbb{E}[Y|\pi(Z, X, W) = \pi, X = x]}{\partial \pi_s} = MTE_{s+1,s}(u, x) = \mathbb{E}[Y_{s+1} - Y_s | U = u, X = x], \text{ where } \pi = [\pi_1, \dots, \pi_{\bar{S}-1}].$$

However, this introduces complications that make semiparametric or nonparametric estimation of such a model difficult, particularly with more than 3 treatments, for two main reasons. First, Assumption 6 effectively requires variation in the C^s 's conditional on all other $C^{s'} \forall s' \in \{1, \dots, \bar{S} - 1\} \setminus s$ and observables wherever the MTE is to be identified. This means if there are $\bar{S} = 6$ treatment levels, then wherever one wishes to estimate $MTE_{4,3}(u)$, there

must be variation in all 5 π_s 's. This may very well not be the case, for example, at high u comparing $S = 4$ to $S = 3$, π_1 or π_2 may be degenerate (or effectively so in the data). In this scenario, Assumption 6 would not hold.

Second, the form $\frac{\partial \mathbb{E}[Y|\pi(Z,X,W)=\pi]}{\partial \pi_s}$ conditions on vector $\pi = [\pi_1, \dots, \pi_{\bar{S}-1}]$ for estimation and taking a partial derivative of the function $\mathbb{E}[Y|\pi]$ with respect to the specific π_s of interest, and it does not allow us to recover treatment responses only treatment effects. Assuming both ω_s and U are drawn from a jointly normal distribution is a common method for estimation, though this fully parametric assumption leads to effectively linear MTEs for most values of u .

I provide an alternative identification method and a weaker assumption to replace Assumption 6, which improves upon both of these limitations. First, rather than recovering MTEs through a local-IV approach, we can recover MTEs as the difference between marginal treatment response (MTR) functions (Carneiro and Lee (2009), Brinch, Mogstad, and Wiswall (2017), Mogstad, Santos, and Torgovitsky (2018), Rose and Shem-Tov (2021)) at a fixed value of $X = x$ and $U = u$:⁴⁴

$$MTE_{s+1,s}(u, x) = \mathbb{E}[Y_{s+1}|U = u, X = x] - \mathbb{E}[Y_s|U = u, X = x]$$

Identification of an MTR (e.g., $\mathbb{E}[Y_s|U, X]$) can be achieved relying solely on variation in adjacent π_s 's (e.g., π_s and π_{s-1}). Specifically, I provide an weaker alternative to Assumption 6, which both reduces the required variation for identification of MTEs when $\bar{S} > 3$ and allows the identification of MTRs and simpler estimation using the separate approach:

Assumption 7. *For all $s \in \mathcal{S} \setminus \{1, \bar{S}\}$, the joint distribution of $\pi_s(Z, X, W)$ and $\pi_{s-1}(Z, X, W)$ conditional on X is absolutely continuous with respect to the Lebesgue measure on \mathbb{R}^2 . Furthermore, the joint distribution of $\pi_s(Z, X, W)$ and $\pi_{s-1}(Z, X, W)$ conditional on X is non-degenerate in the sense that its support cannot be reduced to a subset on \mathbb{R} .*

Assumption 7 improves upon Assumption 6. First, because it only makes assumptions

⁴⁴The framework stems from the literature on estimating the marginal distributions of potential outcomes in Imbens and Rubin (1997) and Abadie (2003).

on the joint distribution of π_s and π_{s-1} , simply requiring that they are not highly codependent conditional on X — in the language of Assumption 6, for $MTE_{s+1,s}$, C^s only need be non-degenerate and continuous conditional on C^{s+1} , C^{s-1} , and X . Second, this assumption lends itself to a simple semiparametric estimation approach, as will be discussed below, and thus is more feasible for applications in applied work. MTEs are then the difference between MTRs, and MTRs are identified under Assumptions (1)-(5) and (7):

Theorem 1. *Let Assumptions (1)-(5), and (7) hold. Then,*

$$\begin{aligned}\mathbb{E}[Y_1|U=u, X=x] &= -\frac{\partial \mathbb{E}[Y \times D_1 | \pi_1(Z, X, W) = \pi_1, X = x]}{\partial \pi_1} \Big|_{\pi_1=u} \\ \mathbb{E}[Y_{\bar{S}}|U=u, X=x] &= \frac{\partial \mathbb{E}[Y \times D_{\bar{S}} | \pi_{\bar{S}-1}(Z, X, W) = \pi_{\bar{S}-1}, X = x]}{\partial \pi_{\bar{S}-1}} \Big|_{\pi_{\bar{S}-1}=u}\end{aligned}$$

And, for all $s = 2, \dots, \bar{S} - 1$,

$$\begin{aligned}\mathbb{E}[Y_s|U=u, X=x] &= \frac{\partial \mathbb{E}[Y \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_{s-1}} \Big|_{\pi_{s-1}=u} \\ &= -\frac{\partial \mathbb{E}[Y \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_s} \Big|_{\pi_s=u}.\end{aligned}$$

Proof of Theorem 1. Using Assumptions (1)-(5) and (7), write (suppressing W):

$$\begin{aligned}&\mathbb{E}[Y \times D_s | Z = z, X = x, W = w] \\ &= \mathbb{E}[\mu_s(X, \omega_s) \mathbf{1}\{\pi_{s-1}(Z, X, W) > U \geq \pi_s(Z, X, W)\} | Z = z, X = x, W = w] \\ &= \mathbb{E}[\mu_s(X, \omega_s) \mathbf{1}\{\pi_{s-1}(Z, X, W) > U \geq \pi_s(Z, X, W)\} | X = x] \text{ by (1)} \\ &= \int_{\pi_s(z, x, w)}^{\pi_{s-1}(z, x, w)} \mathbb{E}[Y_s | U = u, X = x] du \text{ by (3)}\end{aligned}$$

Then, because

$$\mathbb{E}[Y \times D_s | Z = z, X = x, W = w] = \int_{\pi_s(z, x, w)}^{\pi_{s-1}(z, x, w)} \mathbb{E}[Y_s | U = u, X = x] du,$$

taking the derivative of both sides with respect to π_{s-1} (when $s > 1$) gives:

$$\frac{\partial \mathbb{E}[Y \times D_s | Z = z, X = x, W = w]}{\partial \pi_{s-1}} \Big|_{\pi_{s-1}=u} = \mathbb{E}[Y_s | U = u, X = x],$$

and similarly taking the derivative of both sides with respect to π_s gives:

$$\frac{\partial \mathbb{E}[Y 1(S = s) | Z = z, X = x, W = w]}{\partial \pi_s} \Big|_{\pi_s=u} = -\mathbb{E}[Y_s | U = u, X = x].$$

Because $D_s = 1(S = s) = 1[\pi_{s-1}(Z, X, W) > U \geq \pi_s(Z, X, W)]$ and by Assumption 7, only variation in the adjacent π 's are relevant. So:

$$\begin{aligned} & \frac{\partial \mathbb{E}[Y \times D_s | Z = z, X = x, W = w]}{\partial \pi_{s-1}} \Big|_{\pi_{s-1}=u} \\ &= \frac{\partial \mathbb{E}[Y \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_{s-1}} \Big|_{\pi_{s-1}=u} \end{aligned}$$

and

$$\begin{aligned} & \frac{\partial \mathbb{E}[Y \times D_s | Z = z, X = x, W = w]}{\partial \pi_s} \Big|_{\pi_s=u} \\ &= \frac{\partial \mathbb{E}[Y \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_s} \Big|_{\pi_s=u} \end{aligned}$$

Finally, we have for s with non-degenerate π_s and π_{s-1} :

$$\begin{aligned}\mathbb{E}[Y_s|U = u, X = x] &= \frac{\partial \mathbb{E}[Y \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_{s-1}} \Big|_{\pi_{s-1}=u} \\ &= -\frac{\partial \mathbb{E}[Y \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_s} \Big|_{\pi_s=u}.\end{aligned}$$

□

Assumptions 2, 4, and 5 ensure values in the population are well-defined, while Assumption 7 ensures variation in adjacent π_s 's. With this, we can identify the MTE between levels s and $s + 1$ by identifying the conditional means (MTRs) for s and $s + 1$:

$$MTE_{s+1,s}(x, u) = \mathbb{E}[Y_{s+1} - Y_s | U = u, X = x] = \mathbb{E}[Y_{s+1} | U = u, X = x] - \mathbb{E}[Y_s | U = u, X = x]$$

with

$$\begin{aligned}&\mathbb{E}[Y_s | U = u, X = x] \\ &= \frac{\partial \mathbb{E}[Y_s \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_{s-1}} \Big|_{\pi_{s-1}=u} \\ &= -\frac{\partial \mathbb{E}[Y_s \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]}{\partial \pi_s} \Big|_{\pi_s=u}.\end{aligned}\tag{5}$$

And this equality applies only to $1 < S < \bar{S}$ — so for intermediate treatment levels, $\mathbb{E}[Y_s | U = u, X = x]$ is over-identified.

While $\mathbb{E}[Y_s \times D_s | \pi_{s-1}(Z, X, W) = \pi_{s-1}, \pi_s(Z, X, W) = \pi_s, X = x]$ can be estimated nonparametrically, in practice the data requirements make such estimation are rarely feasible, and semiparametric estimation is often the preferred approach in practice in the MTE literature.

For semiparametric estimation, I assume that $\mu_s(X, \omega_s)$ is composed of additively separable functions of X and ω_s , essentially that across all values of covariates, the effect of unobservables works the same and it allows for treatment effects on observables and unobservables separately (Andresen (2018)). This assumption (either directly or as a result of full independence) is common in the literature (Carneiro, Heckman, and Vytlacil (2011), Kline

and Walters (2016), Brinch, Mogstad, and Wiswall (2017), Bhuller et al. (2018), Rose and Shem-Tov (2021)). Specifically, I assume: $Y_s = \beta_s X + \omega_s$ and $\mathbb{E}[Y_s|x, u] = \beta_s x + \mathbb{E}[\omega_s|U = u]$. Then, the marginal treatment effect of moving from one treatment to the next highest one (s to $s + 1$) is:

$$\begin{aligned} MTE_{s+1,s}(U = u, X = x) &= \mathbb{E}[Y_{s+1} - Y_s|U = u, X = x] \\ &= (\beta_{s+1} - \beta_s)x + \mathbb{E}[\omega_{s+1}|U = u] - \mathbb{E}[\omega_s|U = u] \end{aligned}$$

C.3 Semi-Parametric Estimation of MTRs

The following section will provide simple functional form assumptions and an accompanying semiparametric estimation procedure. For notational purposes, I suppress W and allow it to be subsumed by Z, X , as in Cornelissen et al. (2018). In practice, when estimating π_s 's there is no explicit distinction between index instruments (Z) and cutoff instruments (W). Assumptions for estimation are stronger than those for identification above (see Appendix C.3.3).

C.3.1 Estimation Form

With additive separability, we can recover $\mathbb{E}[Y_s|X = x, U = u]$, which we do not observe, by starting with $Y \times D_s$, which we do observe. Specifically:

$$\begin{aligned} \mathbb{E}[Y \times D_s|\pi_{s-1}(Z, X) = \pi_{s-1}, \pi_s(Z, X) = \pi_s, X = x] \\ = \beta_s x(\pi_{s-1} - \pi_s) + \Lambda_s(\pi_{s-1}) - \Lambda_s(\pi_s) \end{aligned} \tag{6}$$

where each $\Lambda_s(k) = \int_0^k \mathbb{E}[\omega_s|U = u]du$. In practice, we can approximate each $\Lambda_s(k)$ with sieves, such that $\Phi_s(k)$ is a vector of basis functions and ϕ_s is a vector of coefficients:

$$\Lambda_s(k) - \Lambda_s(k') \approx \phi'_s[\Phi_s(k) - \Phi_s(k')] = \sum_{j=1}^J \phi_{s,j}(k)[\vartheta_{s,j}(k) - \vartheta_{s,j}(k')].$$

Assume also that we have i.i.d. data $\{(Y_i, S_i, Z_i, X_i) : i = 1, \dots, n\}$.

Then, from equation (5) and using the functional form assumption in equation (6), for all s such that $1 < s < \bar{S}$:

$$\mathbb{E}[Y_s | X = x, U = \pi_s \text{ or } \pi_{s-1}] = -(-\beta_s x - \frac{\partial}{\partial \pi_s} \Lambda_s(\pi_s)) = \beta_s x + \frac{\partial}{\partial \pi_{s-1}} \Lambda_s(\pi_{s-1})$$

and for $s = 1$:

$$\mathbb{E}[Y_1 | X = x, U = \pi_1] = \beta_1 x + \frac{\partial}{\partial \pi_1} \Lambda_1(\pi_1)$$

while for $s = \bar{S}$:

$$\mathbb{E}[Y_{\bar{S}} | X = x, U = \pi_{\bar{S}-1}] = \beta_{\bar{S}} x + \frac{\partial}{\partial \pi_{\bar{S}-1}} \Lambda_{\bar{S}}(\pi_{\bar{S}-1})$$

C.3.2 Estimation Steps

Estimation is based on equation (6). We can approximate Λ_s using B-splines or a polynomial of π_s (and similarly for π_{s-1}). The main estimation procedure in this paper follows five steps:

- 1. Recover estimates of π_s ($\forall s \in \{1, \dots, \bar{S} - 1\}$) as probabilities (i.e., $\hat{\pi}_s \in [0, 1]$) (for example, using separate probit or logistic regressions) by regressing the treatment level being higher than s on instruments and regressors:

$$1\{S_i > s\} = \beta^s [X_i, Z_i, W_i] + \epsilon_i^s$$

Then predict $\hat{\pi}_s \forall s \in \{1, \dots, \bar{S} - 1\}$. In this paper, I use a probit specification regressing $s \in [EM, Detention]$ ($s > 1$) and $s = Detention$ ($s > 2$) on judge fixed effects interacted with observables and time fixed effects to get predicted values for $\hat{\pi}_1$ and $\hat{\pi}_2$, respectively.

- 2. For each $s \in S$, construct $\Phi_s(\pi_s)$ and $\Phi_s(\pi_{s-1})$ either as polynomials or B-splines, each being a vector of basis functions.

- 3. For each $s \in S$, regress

$$Y \times D_s = \beta_s X(\pi_{s-1} - \pi_s) + \phi_s(\Phi_s(\pi_{s-1}) - \Phi_s(\pi_s)).$$

If $s = 1$ then exclude $\Lambda_s(\pi_{s-1})$, and if $s = \bar{S}$ then exclude $\Phi_s(\pi_s)$. ϕ_s is a vector of coefficients with each element corresponding to each basis function. For example, if we are using a 3rd degree polynomial, then $\Phi_s(k) = [k, k^2, k^3]$, so $\phi_s = [\phi_s^1, \phi_s^2, \phi_s^3]$ and $\Phi_s(\pi_{s-1}) - \Phi_s(\pi_s) = [\pi_{s-1} - \pi_s, \pi_{s-1}^2 - \pi_s^2, \pi_{s-1}^3 - \pi_s^3]'$.

- 4. Then compute the estimate for $\mathbb{E}[Y_s|x, u]$ as:

$$\mathbb{E}[\widehat{Y_s|x, u}] = \begin{cases} \text{if } s = 1 & \hat{\beta}_s x + \hat{\phi}_s \Phi'_s(\pi_s), \quad u = \pi_s \\ \text{if } s > 1 & \hat{\beta}_s x + \hat{\phi}_s \Phi'_s(\pi_{s-1}), \quad u = \pi_{s-1} \end{cases}$$

In the $s = 1$ case, $\mathbb{E}[\widehat{Y_s|x, u}] = -\frac{\partial \mathbb{E}[Y(S=s)|x, u=\pi_s]}{\partial \pi_s} = -[-\beta_s x - \hat{\phi}_s \Phi'_s(\pi_s)] = \beta_s x + \hat{\phi}_s \Phi'_s(\pi_s)$.

- 5. For each value of u in the support of both s and $s + 1$ and any value of $X = x$, compute $\widehat{MTE}_{s+1,s}(x, u) = \mathbb{E}[\widehat{Y_{s+1}|x, u}] - \mathbb{E}[\widehat{Y_s|x, u}]$.

C.3.3 Assumptions for Estimation

Assumption 8. *E1: $(\omega_s, U) \perp (Z, X)$ for all $s \in \mathcal{S}$.*

Assumption 9. *E3: The distribution of U is uniform on $[0, 1]$.*

Assumption 10. *E4: $E|Y_s| < \infty \forall s \in S$*

Assumption 11. *E5: $0 < \Pr(S = s) < 1 \forall s \in S$*

Assumption 12. *E6: For all $s \in \mathcal{S} \setminus \{1, \bar{S}\}$, the joint distribution of $\pi_s(Z, X)$ and $\pi_{s-1}(Z, X)$ is absolutely continuous with respect to the Lebesgue measure on \mathbb{R}^2 . Furthermore, the joint distribution of $\pi_s(Z, X)$ and $\pi_{s-1}(Z, X)$ is non-degenerate in the sense that its support cannot be reduced to a subset on \mathbb{R} .*

Assumption 8 replaces V with U and W is subsumed by X, Z and imposes a full independence assumption (rather than conditional independence). From this, the other

assumptions no longer condition on X (due to full independence), W is subsumed into X, Z , and U is used in place of V for expediency removing the need for assumptions on τ .

C.4 Common Support

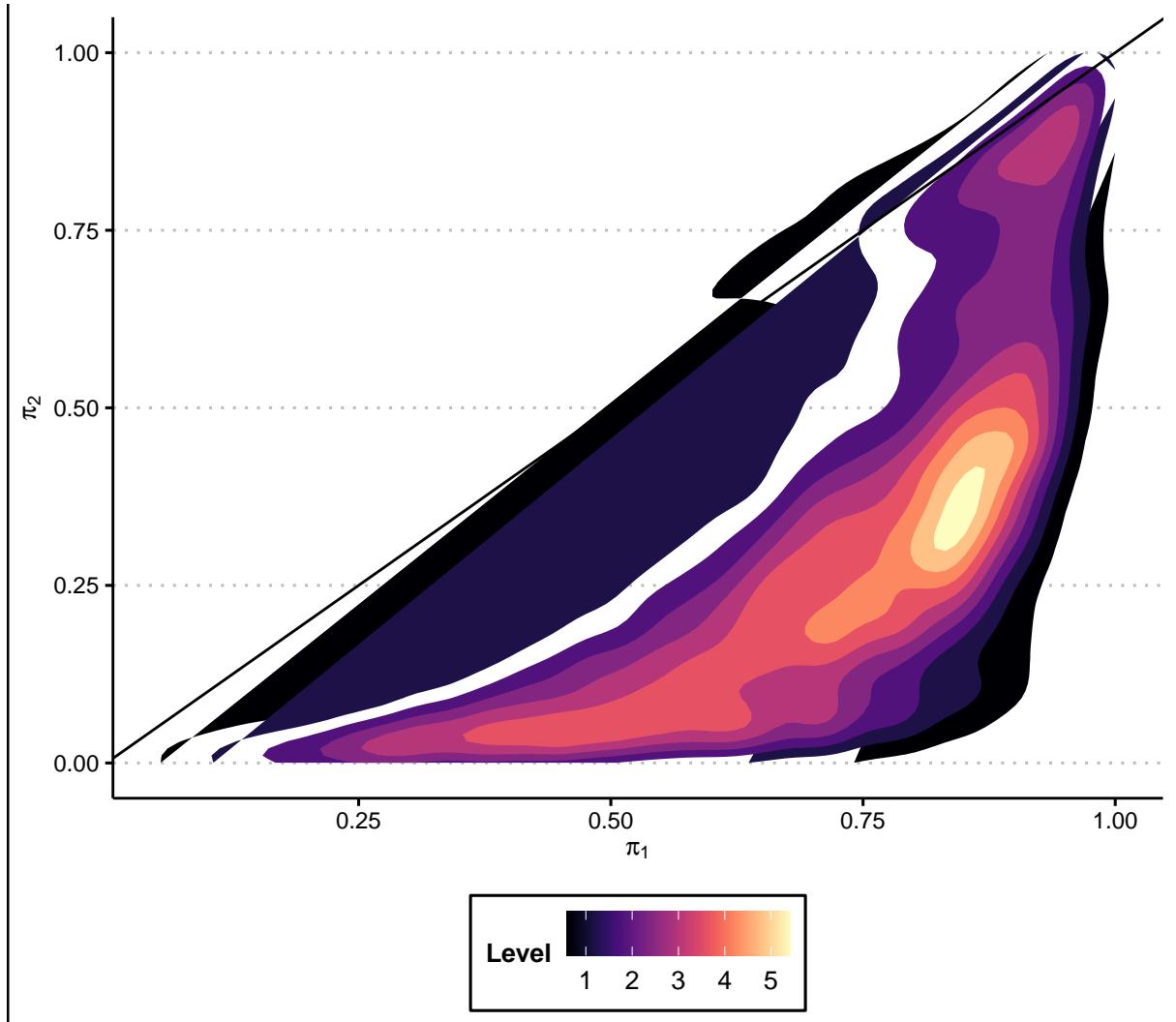
Estimation requires variation in $\pi_{s-1} - \pi_s$, which provides variation in higher terms of $\Lambda_{s-1} - \Lambda_s$. This must be factored into determining where the MTRs are estimable, in addition to ensuring that the MTRs of two adjacent treatments exist. Common support also determines the region over which we are able to produce counterfactuals and treatment effects.

Given that $\pi_0 = 1$ and $\pi_3 = 0$ for all individuals, the variation of π_1 and π_2 are of most concern — though their correlation is high, this is to be expected given that if $s > 2$ then $s > 1$ as well. As shown in Figure C.1, below the 45 degree line (such that $\pi_2 < \pi_1$), there is variation in values of each π excluding very high values of π_1 and very low values of π_2 . The right panel in Figure C.1 displays the same plot for the felony-only sample.

To estimate the MTEs however, we require common support for the π 's and variation within a treatment level for which we will estimate a marginal treatment response function. Figure C.2 displays the distribution of $\pi_{s-1} - \pi_s$ for each treatment level (right panel displays the same for felony-only sample). For $s = 1, 3$, $\pi_{s-1} - \pi_s$ covers the entire unit interval, but for $s = 2$, it falls short, with strong support only between about [0.05, 0.8].

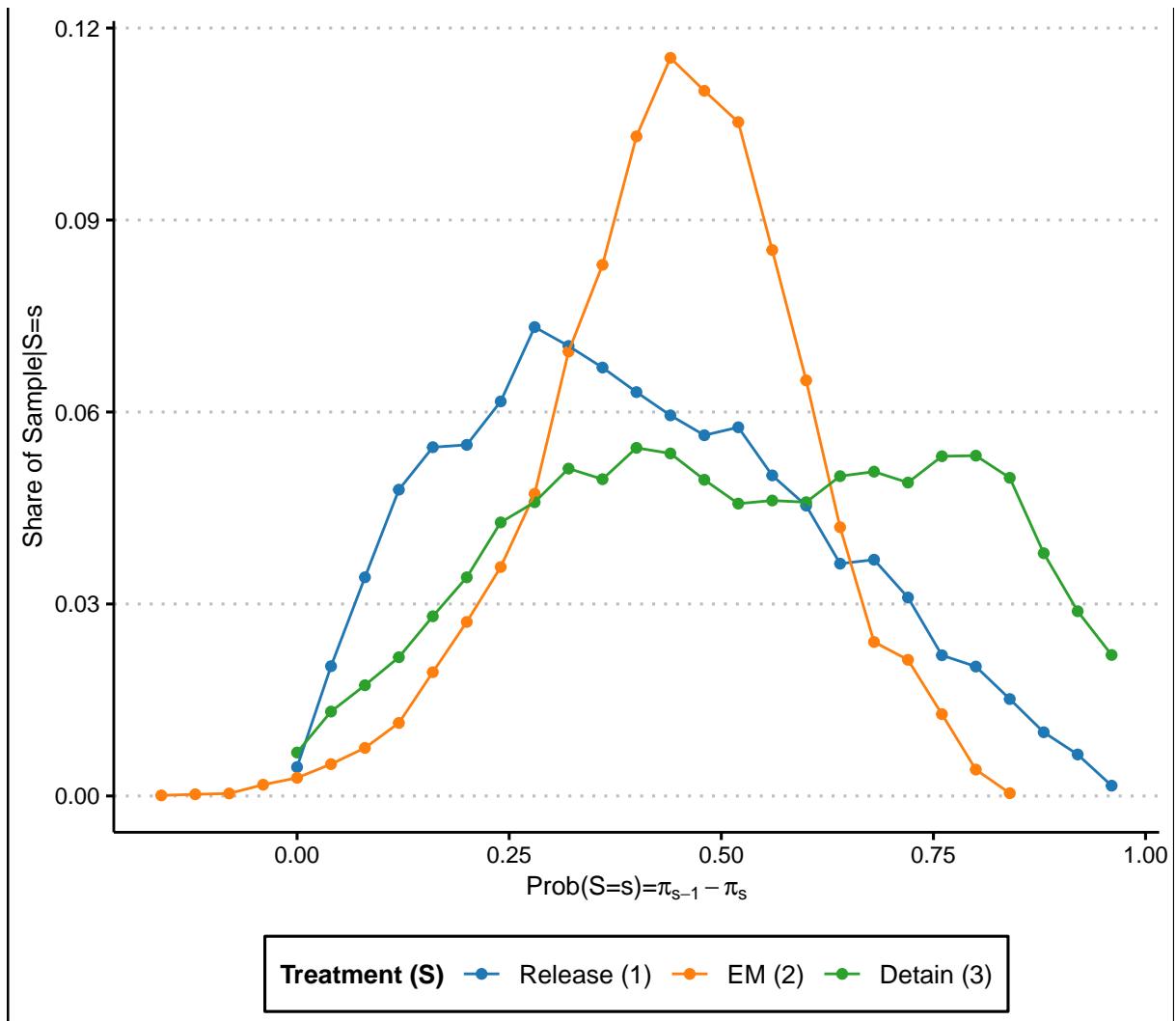
Figure C.3 displays the supports for each relevant π by treatment type (i.e., π_1 for $S = 1, 2$, and π_2 for $S = 2, 3$) for the main sample, along with 1% trim vertical lines denoting the 1st and 99th percentiles within the treatment-specific sample. There is nearly full support for $S = 1, 3$. However, for $S = 2$, the support ranges from about $\pi_1 \in [0.24, 0.98]$ and $\pi_2 \in [0.02, 0.76]$. Since the overlap we require is where there is common support for π_1 and π_2 for $S = 2$, we can only compute the MTR for EM for $\pi_1, \pi_2 \in [0.24, 0.76]$, which limits the range we can compute MTEs for EM versus release and EM versus detention, as EM is the most limited in support.

Figure C.1: Density of π_1 and π_2 by Sample



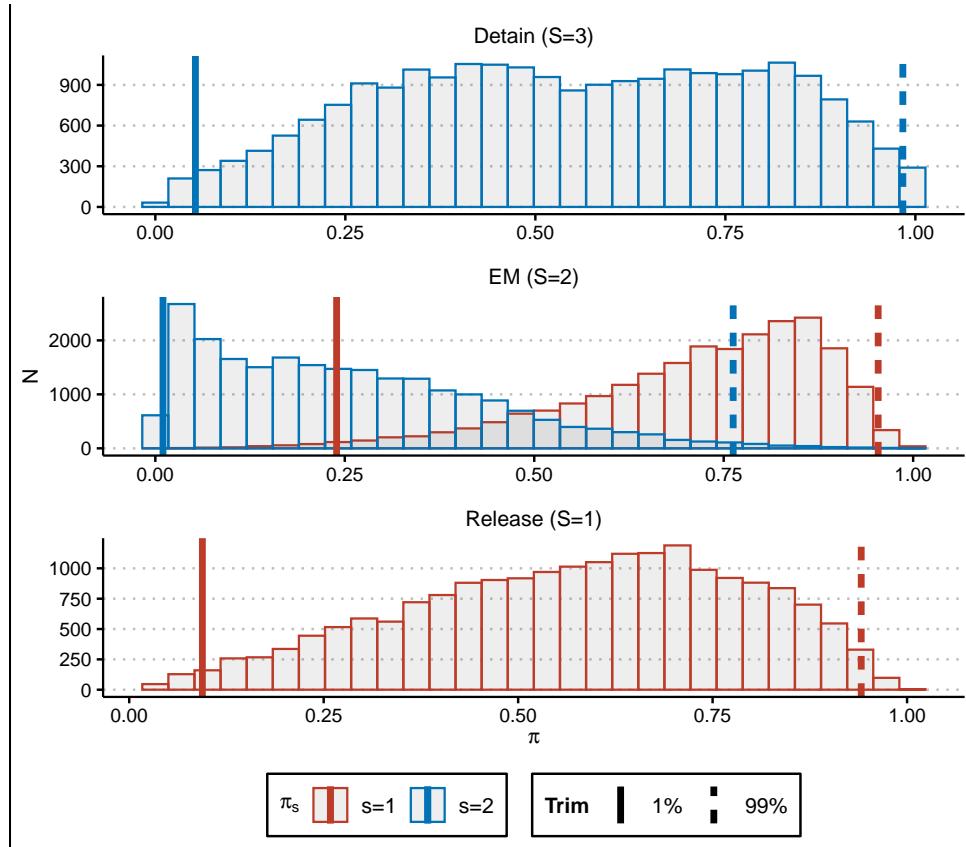
Note: Figure displays density of fitted values for π_1 and π_2 from equation (5).

Figure C.2: Density of $\Pr(S = s) = \pi_{s-1} - \pi_s$



Note: Figure displays densities, within each treatment, of the predicted likelihood the defendant is assigned to that treatment.

Figure C.3: Common Support Across Treatment Levels



Note: Figure displays histograms of relevant π_s and π_{s-1} values for each treatment level (s). Trims indicate the 1st percentile and 99th percentile of the treatment-specific sample for each relevant π value.

C.5 Details on Estimation of MTEs

Estimation of MTEs follows the subsequent steps::

Recovering π π_1 and π_2 can be recovered separately by estimating:

$$1[S_{ijc} > s] = \sum_j \gamma_j^s 1\{J = j\} X_{ijc} + \mu_s X_{ijc} + \kappa_{s,ijc} + e_{s,ijc} \quad (7)$$

with the dependent variables being $1[S > 1]$ and $1[S > 2]$, respectively, using a probit model and predicting treatment probabilities, where $\kappa_{s,ijc}$ are year-quarter fixed effects and X_{ijc} a large vector of the defendant observables over which judges can have heterogeneous preferences.⁴⁵

Recovering MTRs and MTEs Both Y and $D_s = 1[S = s] = 1[\pi_s \leq U < \pi_{s-1}]$ are observed, so I construct $Y \times D_s$: $Y_i \times D_{i,s}$ (0 if $S_i \neq s$ and Y_i if $S_i = s$). Let $\Lambda_s(\pi) = \int_0^\pi \mathbb{E}[\omega_s | U = u] du$. Then, given the assumptions on $\mathbb{E}[Y_s | X = x, U = u]$:

$$\begin{aligned} \mathbb{E}[Y \times D_s | \pi_s, \pi_{s-1}, X] &= \beta_s X \times (\pi_{s-1} - \pi_s) + \mathbb{E}[\omega_s \times D_s | \pi_s, \pi_{s-1}] \\ &= \beta_s X \times (\pi_{s-1} - \pi_s) + \Lambda_s(\pi_{s-1}) - \Lambda_s(\pi_s) \end{aligned}$$

where Λ_s can be approximated using sieves. In the main specification of this paper I use a polynomial, such that $\Lambda_s(\pi_{s-1}) - \Lambda_s(\pi_s) \approx \phi_s[\Phi_s(\pi_{s-1}) - \Phi_s(\pi_s)]$, where $\Phi_s(\pi)$ denote a vector of basis functions and ϕ_s is a vector of coefficients for each s ⁴⁶

Using the approximation of Λ_s and including an error term and time fixed effects, I estimate with OLS:

$$Y \times D_s = \beta_s X(\pi_{s-1} - \pi_s) + \phi_s[\Phi_s(\pi_{s-1}) - \Phi_s(\pi_s)] + \kappa_s + \epsilon \quad (8)$$

⁴⁵These include Black, female, older than 30, past cases, FTA, and charge bins, and charge type indicators (see observables in Table 2) in addition to indicators for misdemeanor charge types, drug possession, domestic violence, property, and other misdemeanors. This allows for all judges' preferences over observables to be heterogeneous across treatment margins, but also allows observables to uniformly affect the likelihood of crossing a treatment threshold. Note that this is similar to, but not equivalent to, estimating an ordered probit with judge-specific thresholds, as it involves additivity across judge \times characteristics by threshold.

⁴⁶For example, with a 3rd polynomial, $\Phi_s(\pi) = [\pi, \pi^2, \pi^3]$ for each s .

Then, I recover the MTR by taking the derivative of the fitted equation (8) with respect to π_s or π_{s-1} to recover $m_s(x, u) = \mathbb{E}[Y_s|x, u] \approx \hat{\beta}_s x + \hat{\phi}_s \Phi'_s(u)$. The difference between MTRs evaluated at the same values of X and U recovers $MTE_{s+1,s}(x, u) \approx (\hat{\beta}_{s+1} - \hat{\beta}_s)x + \hat{\phi}_{s+1}\Phi'_{s+1}(u) - \hat{\phi}_s\Phi'_s(u)$.

Main results use a 3rd degree polynomial for Φ , meaning MTRs and MTEs effectively use a 2nd degree polynomial.

C.6 Construction of Treatment Parameters

Because I assume MTRs are additively separable in u and x , the construction of the observable and unobservable components are done separately, using a uniform grid of 99 points ($u \in \{0.01, 0.02, \dots, 0.99\}$), following the construction of the average treatment effect on the treated (for ATR, ATD) and ATE from Cornelissen et al. (2016).

$$\begin{aligned} ATE_{2,1} &= \frac{1}{N} \sum_{i=1}^N X_i(\beta_2 - \beta_1) + \frac{1}{n_{p_1}} \sum_{u=100 \times \underline{p}_1}^{100 \times \bar{p}_1} \mathbb{E}[\omega_2 - \omega_1 | u = u] \\ ATE_{2,3} &= \frac{1}{N} \sum_{i=1}^N X_i(\beta_2 - \beta_3) + \frac{1}{n_{p_2}} \sum_{u=100 \times \underline{p}_2}^{100 \times \bar{p}_2} \mathbb{E}[\omega_2 - \omega_3 | u = u] \\ ATR &= \frac{1}{N} \sum_{i=1}^N \frac{p_{1,i}}{\hat{p}_1} X_i(\beta_2 - \beta_1) + \frac{1}{n_{p_1}} \sum_{u=100 \times \underline{p}_1}^{100 \times \bar{p}_1} \frac{\Pr(p_1 > 1 - \frac{u}{100})}{\hat{p}_1} \mathbb{E}[\omega_2 - \omega_1 | u = u] \\ ATD &= \frac{1}{N} \sum_{i=1}^N \frac{p_{2,i}}{\hat{p}_2} X_i(\beta_2 - \beta_3) + \frac{1}{n_{p_2}} \sum_{u=100 \times \underline{p}_2}^{\bar{p}_2} \frac{\Pr(p_2 > \frac{u}{100})}{\hat{p}_2} \mathbb{E}[\omega_2 - \omega_3 | u = u] \end{aligned}$$

where N is the number of observations (cases), $p_1 = 1 - \pi_1$, $p_2 = \pi_2$, \bar{x} and \underline{x} refer to the upper and lower limits of common support, n_x is the number of points between the upper and lower limits, and \hat{x} is the average over the range of common support.