Liquid Assets: Plasma Donation Income and Crime*

Sam Kang Brendon McConnell Mariyana Zapryanova

December 10, 2025

Abstract

The United States is one of the few OECD countries to pay individuals to donate blood plasma and is the most generous in terms of remuneration. The opening of a local blood plasma center represents a positive, prospective income shock for would-be donors. Using detailed data on the location of blood plasma centers in the US and two complementary difference-in-differences research designs, we study the impact of these centers on crime outcomes. Our findings indicate that the opening of a plasma center in a city leads to a 12% drop in the crime rate, an effect driven primarily by property and drug-related offenses. A within-city design confirms these findings, highlighting large crime drops in neighborhoods close to a newly opened plasma center. To probe the underlying mechanisms, we conduct additional analyses that examine heterogeneity across sociodemographic groups, offender age profiles, and incapacitation using center-level opening times. We find strong support in favor of a financial channel, which operates most strongly in less affluent areas and among younger age groups. We find no support for the idea that incapacitation drives our results.

JEL codes: K42.

Keywords: Blood plasma centers, financial incentives, crime.

*We are grateful to Beatrix Eugster, Olivier Marie, and Lucie Schmidt for their insightful comments and feedback, as well as seminar and conference participants at TWEC 2025, SEA 2025, St. Gallen, and ESEC. Kang: University of St. Gallen [samueljong.kang@unisg.ch]; McConnell: City St George's, University of London [brendon.mcconnell@gmail.com]; Zapryanova: Smith College [mzapryanova@smith.edu].

1 Introduction

Crime imposes a significant burden on society. In the United States, recent estimates suggest crime imposes an annual cost ranging from $2.6 trillion (Miller et al., 2021) to $5.8 trillion (Anderson, 2021). Common strategies to fight crime, such as increased policing or mass incarceration, are costly and can backfire, particularly in the case of juvenile incarceration (Bayer et al., 2009; Aizer and Doyle, 2015). Policies originating in other areas, such as lead remediation (Reyes, 2007; Aizer and Currie, 2019) or the expansion of access to education (Lochner and Moretti, 2004; Fella and Gallipoli, 2014; Bell et al., 2022), can have unintended crime-reducing effects.

In this paper, we study how an additional source of income in the form of plasma donation payments from private blood plasma centers can impact crime in the US. We provide the first evidence on the impact of plasma donation centers on crime outcomes in US cities. We use detailed information on the location and opening dates of plasma centers nationwide, which we combine with two different datasets on crime incidence. The first dataset allows us to document cross-city differences in crime outcomes that are caused by the opening of a plasma center, while the second dataset enables the estimation of within-city, neighborhood level effects.

Our interest in the impact of US-based plasma centers on crime extends beyond the immediate question. In the US, the relatively generous compensation for plasma donations provides regular donors with a consistent income stream of $400 per month (Dooley and Gallagher, 2024a). This monthly income, earned in approximately three hours of plasma donation time per week, places it at the lower end of recent universal basic income (UBI) experiments in the US.[1] Plasma donors in the US are typically young and low-income, paralleling the demographic profile of UBI participants in teh US. This suggests that what we learn from studying multiple, local (natural) experiments in plasma centers openings across the country may be informative of what a more comprehensive roll-out of UBI would look like in the US. To date, evidence has primarily come from small-scale, geographically constrained RCTs (West et al., 2021; Vivalt et al., 2024).

An additional dimension that makes studying plasma centers valuable in connecting to the wider literature on UBI and cash transfer schemes relates to the explicit regulatory safeguards

[1]Three recent schemes respectively paid participants $500 per month (Stockton Economic Empowerment Demonstration, West et al., 2021), $500 per month (The Minneapolis Guaranteed Basic Income Pilot, Goodman-Bacon and Palmer, 2024), and $1,000 per month (The OpenResearch Unconditional Income Study, Miller et al., 2024; Vivalt et al., 2024).

under which plasma centers operate. Donors are subject to drug -use history questionnaires, physical examinations, and eligibility screening and informal monitoring during each session to ensure quality control of the resulting blood plasma. This adds a binding layer of conditionality to the income stream that donors can obtain from regular donation. As we discuss in greater detail in the concluding remarks, this conditionality sets plasma donation income apart from traditional UBI or unconditional cash transfer schemes in dimensions that are both of first-order importance to our findings and directly relevant to the broader discussion of optimal design of cash transfer programs.

We first formalize the key mechanisms of how plasma center proximity may affect crime outcomes, modifying the standard economic model of crime to allow for the role of plasma donation income. A key testable hypothesis derived from this model is that donation income will have a negative effect on criminal engagement. Given the short duration required to donate plasma – 90 minutes per session – it does not seem reasonable to consider an incapacitation effect of donation.

To estimate the impact of a plasma center opening on city-level crime, we construct a balanced panel dataset of city-level crime incidence at the city-by-month-by-year level. We then use a difference-in-differences research design to estimate the impact of a plasma center opening on crime outcomes. Our baseline specification is based on a two-way fixed effects (TWFE) panel design. In order to estimate our target parameter in this setting – the average treatment effect on the treated (ATT) – we require that in absence of the opening of a plasma center, treated and untreated cities would experience parallel trends in potential outcomes. We provide evidence that the parallel trends assumption holds in our setting. Given the staggered nature of plasma openings across cities and time, we provide additional evidence that our baseline TWFE design is valid in this setting.

Having satisfied the key identifying assumption, and provided evidence of the validity of our baseline specification, we start by estimating the static impact of plasma centers on a battery of crime outcomes. Our data allows us to document treatment effects at both the level of broad crime categories (e.g., violence, property crimes, drug possession) and the granular sub-category level. This is important, as it allows us to document precisely how plasma center openings alter crime outcomes. We then estimate dynamic treatment effects, presenting our results in the form of event study graphs. This approach provides evidence of the timing of the effects of plasma centers on crime. We additionally make use of distribution regression to trace out the effects of

plasma centers across the full distribution of crime. Such an approach is useful from a policy-perspective, as it allows us to identify the distributional anatomy of our treatment effects – a policy that changes crime by altering low-level crime areas has different ramifications compared to one that changes crime at the upper end of the distribution of crime.

We next shift our attention to neighborhood-level crime, using a secondary crime dataset, which covers fewer cities but provides detailed geo-coded information of the crime incidents. The ability to document the impact of plasma centers on crime using a different dataset, at a different geographic level, using a different research design, is advantageous. It offers a check on the internal validity of our primary findings and allows us to make deeper, more nuanced insights into the impact of plasma centers on crime outcomes. To implement our empirical approach in this setting, we plot non-parametric distance decay functions within a relatively tight radius for the period just before and just after a plasma center opening. Overlaying these two curves enables us to detect a radius-based treatment zone. We use neighboring areas – which we observe to be unaffected by a plasma center opening – to serve as local controls. This approach has been employed in similar settings (Linden and Rockoff, 2008; Ang, 2021).

Informed by our non-parametric analysis, we specify a second difference-in-differences design at the neighborhood level. We use only cities with a plasma center present, defining treatment status based on proximity to a plasma center (within 3 km) and using those neighborhoods 3-6 km away as control units. For our DD regressions we use a +/-12 month window around an opening, expanding this to +/-18 months for our dynamic DD approach. This allows a more comprehensive assessment of the parallel trends assumption in this setting, namely that there are no differential trends across treatment and control neighborhoods in the run-up to an opening. Our findings confirm this assumption.

In the final empirical section, we investigate potential mechanisms that drive our key findings. We begin by incorporating a set of economic and sociodemographic variables from the Census measured prior to our study period, which allows us to assess treatment effect heterogeneity by proxies for income. Specifically, we consider how our baseline DD estimates vary with key economic and sociodemographic variables, measured prior to the start of our sample period to avoid contamination by our treatment. We match in city-level controls from the 2000 census, focusing particularly on outcomes that relate to our theoretical prediction of the crime-reducing effect of donation income. Such a mechanism should be more pronounced in less affluent cities. Hence we include information on city-level unemployment rates (which we separate by gender),

poverty rates, and household income. We next extend the analysis to offender age groups, re-estimating our DD specification separately by five-year age bins. This allows us to test whether crime responses vary across the age distribution in a manner consistent with younger individuals being more likely to participate in plasma donation and thus more directly exposed to the income mechanism. Finally, we test for the viability of an incapacitation channel by constructing an hour-by-day variant of our crime data and interacting plasma center openings with center-level operating hours.

Using data from the National Incident-Based Reporting System (NIBRS), we find that the opening of a plasma center leads to a large and statistically significant drop in the total crime rate at the city level, amounting to a drop of 76.3 crimes per 100,000 inhabitants per month. To help interpret the magnitude of these findings, we express our treatment effects in terms of the mean outcome for the control cities. Using this representation, plasma centers lead to an 11.8% reduction in the crime rate. Decomposing the effect across crime types, we find statistically significant declines in violent crime, property crime, and drugs possession offenses. The reduction in violent crime are important, albeit not especially precisely estimated, as violent crime, while accounting for just under 20% of crime incidents in our sample, is the most costly for society. Property crime, accounting for three of every five crimes in our sample, decreases by 12.8%. This latter finding aligns closely with the predictions of our theoretical model, whereas the results for violent crime are somewhat more difficult to interpret within a rational economic model of crime. Drugs possession offenses also drop noticeably once a plasma centers opens – by 14.3%. We posit that this relates to the drug and blood-quality regulations implemented by plasma centers.

The event study evidence we provide next offer two key findings. First, we document that for all of our key outcomes, we cannot reject that the parallel trends assumption is valid in our setting. Second, we find that the opening of a local plasma center leads to an immediate reduction in crime outcomes, with the crime-reducing effects intensifying over time. For total crime rates and drugs possession offenses, we reject that the short-run and medium-run effects are equal. For property crime, while the pattern of point estimates over event time is similar, the estimates are too noisy to rule out equality of treatment effects in the short-run versus medium-run.

The evidence from distribution regression estimates points to plasma centers impacting average crime by reducing crime incidents in the upper tail of the crime distribution. Such an effect

is interesting from a policy perspective, as it suggests that plasma donation centers – which are present in almost all states and most large urban centers – reduce between-city inequality in crime. Given that poorer cities tend to experience higher crime rates (see Figure A3), this means a reduction in crime exposure inequality along a key dimension: income.

We next turn to presenting neighborhood-level evidence based on our second, within-city research design. We document statistically significant drops in total crime following the opening of a plasma center in neighborhoods located near the center. Property crime reductions are the key driver behind the drop in total crime. The magnitude of these effects are somewhat muted compared to those observed from our primary, between-city research design. In a section dedicated to reconciling the findings from the two designs, we suggest this is likely due to a combination of the type of cities the two datasets cover and the fact that our within-city design only considers the short-run crime consequences of a plasma center opening.

Turning to mechanisms, the empirical tests consistently highlight the financial channel as the central driver of the observed decline in crime. First, we document substantial treatment effect heterogeneity across economic dimensions – cities with lower household income or higher poverty rates experience the most pronounced reductions in crime following the opening of a plasma center, while crime in more affluent cities responds considerably less. This finding, which connects our empirical analysis to our theoretical model, suggests that it is indeed the financial element of plasma donations that is driving the concomitant crime drop in areas with a plasma center. Interestingly, drugs possession offenses mirror the pattern observed for property crime, with substantially larger declines in poorer areas. This suggests that donors might alter their substance use behavior in response to drug-related regulatory requirements imposed by plasma centers. Second, the largest declines are observed among offenders aged 18–28, with effect sizes that diminish steadily across older age groups and approach zero for the oldest cohorts. This age gradient is consistent with the interpretation that plasma centers reduce crime by altering the behavior of individuals most likely to participate in plasma donation. Finally, we find no evidence that crime reductions are concentrated during donation hours. If incapacitation were the operative mechanism, declines should be most pronounced when centers are open and donors are temporarily unavailable to offend. Instead, the effects remain constant across both open and closed periods, indicating that incapacitation is unlikely to serve as the primary mechanism explaining the observed declines in crime.

Our study contributes to the literature on financial incentives and criminal behavior. While

previous research has explored the impact of means-tested transfer programs–such as cash benefits and in-kind assistance–on crime, our study expands the understanding of how financial incentives, particularly those tied to voluntary transactions, affect criminal behavior. This distinction is crucial, as voluntary transactions may differ from traditional assistance programs in their effects on financial stability and incentives for criminal behavior.

Research on direct cash payments highlights how welfare payments influence crime rates.[2] Foley (2011) finds that in areas where benefits are distributed monthly, economic hardship intensifies toward the end of the payment cycle, resulting in a rise in property crimes, while violent crime rates remain unchanged. Watson et al. (2020) study Alaska's annual lump-sum Permanent Fund Dividend, documenting large reductions in property crime but no significant changes in violent crime.[3] Deshpande and Mueller-Smith (2022) explore the loss of Supplemental Security Income (SSI) at age 18, demonstrating that this loss results in higher involvement in property crime but not violent offenses.

Studies on in-kind benefits explore whether they serve as deterrents to crime. Tuttle (2019) evaluates the impact of Florida's policy restricting food stamps (SNAP) eligibility for drug offenders, finding increased participation in financially motivated crimes but no rise in violent offenses.[4] Jacob et al. (2015) and Carr and Koppa (2020) investigate the role of housing vouchers in crime rates in Chicago and Houston, respectively, and find no measurable effects. Extending healthcare access, whether within prison (Alsan and Yang, 2025) or outside prison through public programs like Medicaid (Gollu and Zapryanova, 2022; He and Barkowski, 2020; Vogler, 2020; Wen et al., 2017), has been shown to reduce both initial criminal activity and recidivism rates.

Empirical research on the economic and social implications of blood plasma donation remains scarce.[5] Existing studies primarily focus on the ethical debates surrounding donor compensation (Lacetera et al., 2013; Lacetera, 2016; Grabowski and Manning, 2016), as well as the financial incentives associated with plasma donation. Notably, prior work has examined the relationship

[2]While evidence consistently links financial incentives to reductions in property crime, research on their effects on violent crime remains limited and inconclusive (Ludwig and Schnepel, 2025).

[3]Some studies also highlight how the mechanism of financial distribution, not just the amount, can play a crucial role in shaping criminal behavior (Hidrobo et al., 2016; Wright et al., 2017). For example, Wright et al. (2017) investigates the shift from cash-based welfare payments to electronic benefit transfers (EBT) and finds that transition to electronic disbursement reduces street crime, particularly drug-related and property offenses.

[4]Carr and Packham (2021) and Carr and Packham (2019) examine policies smoothing the disbursement of SNAP benefits and report similar results.

[5]Theoretical economic research in this space is also limited, with studies exploring the efficient distribution and allocation of COVID-19 convalescent plasma (CCP) therapy (Kominers et al., 2020) and broader market design considerations in organ donation (Ergin et al., 2017).

between plasma centers and reliance on high-interest payday loans (Dooley and Gallagher, 2024a) and its connections to poverty (Ochoa et al., 2021). However, no study has systematically assessed whether financial incentives tied to plasma donation contribute to criminal activity. Our research fills this gap by providing a novel perspective on the unintended consequences of monetized donation systems.

The paper is organized as follows. Section 2 describes the institutional setting and the data used. Section 3 extends the standard economic model of crime to allow for the role of plasma donation income. Section 4 outlines the empirical strategy we employ. Sections 5 and 6 present our key empirical finding, respectively using a between-city and within-city research design. Section 7 explores the mechanisms that link a plasma center opening to crime reductions. Section 8 concludes.

2 Institutional Setting and Data

2.1 Plasma Centers

The United States is the largest producer of human blood plasma in the world, with an estimated 3.1 million adult donors supplying approximately 65% of the global plasma supply (Pant et al., 2021).[6] In 2019, over 53 million plasma donations were collected in the United States (Dooley and Gallagher, 2024a). This volume suggests that as few as 500,000 individuals could have provided all donations if each donated twice weekly, while survey data provided by (Dooley and Gallagher, 2024a), indicating an average of twice monthly, point to closer to 2.2 million annual donors.

The majority of plasma collected in the US comes from commercial centers that compensate donors. Unlike whole blood donation, which is limited to once every eight weeks due to iron depletion, plasma donation can occur much more frequently, up to twice a week.[7] Using survey results, Dooley and Gallagher (2024a) find that the average donation frequency is approximately twice per month, and 10% of donors reported donating 10 times or more in the last 6 months.

Individuals who donate plasma in the US are predominantly younger adults with limited

[6]Plasma donation involves the extraction of plasma, the pale-yellow liquid part of blood that contains essential proteins, immunoglobulins, and clotting factors. It's collected through a process called apheresis, which separates plasma from the rest of the blood and returns the red blood cells and other components back to the donor. Plasma is essential in the production of a range of life-saving drugs and therapies, making it a highly valuable commodity in the global medical supply chain.

[7]The difference stems from apheresis: whole blood donations remove red cells that take time to replenish, while plasma donations return other components, allowing plasma to restore naturally within 48 hours.

financial resources and restricted access to traditional credit markets. Dooley and Gallagher (2024a) use survey data and find that plasma donors are disproportionately male, under the age of 35, more likely to be Black and from low-income households. Many face liquidity constraints, earning less than $20,000 annually, holding little in savings, and possessing poor credit scores. Further, among surveyed donors, the majority cite day-to-day expenses and emergency needs as their primary reasons for donating. While 58% donate to support basic consumption and 6% to cover sudden shocks, 19% do so to finance nonessential purchases, such as entertainment or leisure goods (Dooley and Gallagher, 2024a).

Donors are financially compensated with prepaid debit or ATM cards, typically receiving between $30 and $70 per session, for up to $400 a month (Dooley and Gallagher, 2024a).[8] Each donation session lasts 90 minutes to two hours and may also involve travel and wait times. Second, despite the fact that plasma donation is widely considered safe for healthy individuals, there are some common transient physical side effects such as fatigue, dizziness, bruising at the needle site, and in rarer cases, infection or fainting.

Plasma centers tend to open in economically disadvantaged areas, specifically in Census tracts with lower income, higher minority populations, and lower rates of home ownership (Dooley and Gallagher, 2024a; James and Mustard, 2004; Ochoa et al., 2021). Commercial plasma centers actively recruit donors through various incentive programs, including referral bonuses, loyalty rewards, and increasing payments based on donation frequency. Because donors may attempt to "cross-donate" at multiple centers to maximize earnings, the Food and Drug Administration (FDA) mandates that donors must present valid identification, typically a government-issued photo ID (U.S. Food and Drug Administration, 2001). In addition to identity verification, plasma centers maintain detailed donor records that feed into national databases such as the National Donor Deferral Registry (NDDR), ensuring donor traceability, monitoring frequency, preventing unsafe cross-donation, and allowing plasma units to be tracked back to the donor in case of adverse events (U.S. Food and Drug Administration, 2001). [9] Centers also verify donor residency within a designated Donor Recruitment Area (DRA), which ties donors to a specific geographic catchment and strengthens compliance with cross-center safeguards outlined in FDA regulations (International Quality Plasma Program, 2020).[10]

[8]Because donors are compensated for their time and biological material, this payment is treated as "other income" and must be reported to the IRS.

[9]This system of traceability, mandated under FDA regulations (21 CFR Part 630), also requires that all collected plasma undergo testing for transfusion-transmitted infections (HIV, Hepatitis B, Hepatitis C, Syphilis, and Human T-cell Lymphotropic Virus) before release for therapeutic use.

[10]A DRA varies by center and region, typically defined by postal codes, municipal boundaries, or a mapped

Beyond the operational safeguards implemented by plasma centers, the broader regulatory framework established by the FDA governs donor eligibility. Prospective donors must be between 18 and 64 years of age and weigh at least 110 pounds (U.S. Food and Drug Administration, 1997). Donors must maintain at least 6.0 g/dL total protein, verified by a quick blood test before donation. They are required to undergo thorough initial screenings, including a physical examination, drug use assessment, completion of a detailed medical and personal history questionnaire and interview (U.S. Food and Drug Administration, 2025). These screenings play a crucial role in evaluating donor suitability, and might result in either a temporary or permanent deferrals from donating plasma. Temporary deferrals, often lasting 6–12 months, apply to certain medication use, pregnancy, incarceration lasting more than 72 hours, or recent tattoos and piercings. Permanent deferrals are enforced for high-risk behaviors like intravenous drug use, chronic illnesses, or infectious diseases.[11] Individuals who appear under the influence of drugs or alcohol or who may be unable to provide an accurate medical history might ineligible to donate blood plasma (U.S. Food and Drug Administration, 1997).

2.2 Data

We use two complementary datasets on crime in this work. Our primary dataset is city-level crime data. We supplement this with data on within-city crime outcomes, measured at the block group level, for a small set of cities that release location-level crime data. We merge both of these crime datasets to data on the location and opening dates of plasma centers in the US, which we source from Dooley and Gallagher (2024a). Finally, we use data on area characteristics from IPUMS NHGIS (Manson et al., 2024) to better understand where plasma centers are located. Below, we detail the key datasets we employ in this work.

2.2.1 City-Level Crime Data

Our primary crime data comes from the National Incident-Based Reporting System (NIBRS), compiled by the Federal Bureau of Investigation (FBI). We construct a panel at the city-by-month-by-year level. Distinct law enforcement agencies, whose jurisdictions might cover city, county, or state-level areas report data voluntarily to NIBRS. As a result, studies typically focus on a specific level of aggregation, often combining multiple agency levels. For our analysis, we

radius around the plasma center, though the exact boundaries are not publicly disclosed (International Quality Plasma Program, 2020).

[11] FDA regulations require the blood plasma center to check donor's arms and forearms for any punctures or scarring that could suggest intravenous drug use.

aggregate crimes reported by city police agencies to the aggregate city-level. We make this choice for several distinct reasons. First, we select cities as our spatial unit of interest due to plasma centers' recruitment area restrictions, which establish residence-based criteria for potential donors and are relatively small in size, making cities the most appropriate spatial unit for analyzing the impact of plasma centers on crime (International Quality Plasma Program, 2020). Second, cities are the unit of local taxation variation. Among other public goods, local taxes fund local law enforcement, generating meaningful variation in crime at the city rather than county level. Lastly, we exclude crimes reported by county or state agencies because they cannot be accurately attributed across cities.

We move from the raw NIBRS data to our final sample by implementing the following sample selection decisions. First, we restrict our attention to city police agencies, the modal agency category in the data (64% of our raw sample).[12] Second, in order to avoid both inconsistent agency reporting over the sample period and to exclude agencies with other reporting issues, we require police agencies to report in all months for our sample period of 2005-2019 inclusive. This decision imposes a strict balance on our panel of cities; however, it also represents the most demanding sample selection criterion, leading to the exclusion of 61.6% of the remaining city-month observations associated with partially reporting agencies. Third, we remove agencies with extreme year-to-year variations in total crime, resulting in the exclusion of five additional cities. Finally, we exclude always-treated cities – for our setting that means cities with a plasma center that opened pre-January 2005. This results in the exclusion of 91 cities.

2.2.2 Location-Level Crime Data

We use a secondary data source, the Crime Open Database (CODE), which contains location-level crime records (Ashby, 2018). This dataset covers 21 distinct cities, spanning the period of 2009-2019, though its temporal coverage is unbalanced and varies by city. The dataset provides incident-level geo-coded and time-stamped crime data from these cities,allowing a more spatially-refined view of how plasma centers impact crime patterns *within* cities. Due to its limited geographic coverage, this dataset necessarily plays a secondary role in our analysis.

To prepare the data for analysis, we first create a within-city balanced panel of crime outcomes at the block group-month-year. While balanced within-city, the block group panels are not balanced across cities. For example, in Seattle, the panel spans January 2009 to December

[12]This means we exclude county, university, and state police agencies.

2019, whereas in Detroit, it covers January 2017 to December 2019. We then match the block group data to plasma centers based on precise location of plasma centers and centroids of block groups, keeping all block groups within a 10 km radius of a plasma center. Finally, we restrict the temporal dimension of our crime data to a range of 12 months on either side of the plasma center opening.

2.2.3 Plasma Center Data

We source our data on the location and opening times of plasma centers from the comprehensive database meticulously compiled by Dooley and Gallagher (2024a). The authors initially rely on the Blood Establishment Registration database, produced by the Food and Drug Administration (FDA). The BER database only records information for the most recent registration information for each establishment, limiting its ability to provide detailed opening dates for older centers. The authors address this limitation by (i) using the precise location of each establishment coupled with Google Streetview to date the opening of a plasma center and (ii) using both current and archived listings (via the Wayback Internet Archive) from pharmaceutical companies that operate plasma centers. To validate their approach, the authors also incorporate records purchased from Infogroup on plasma collectors and blood banks. [13]

2.3 Summary Statistics

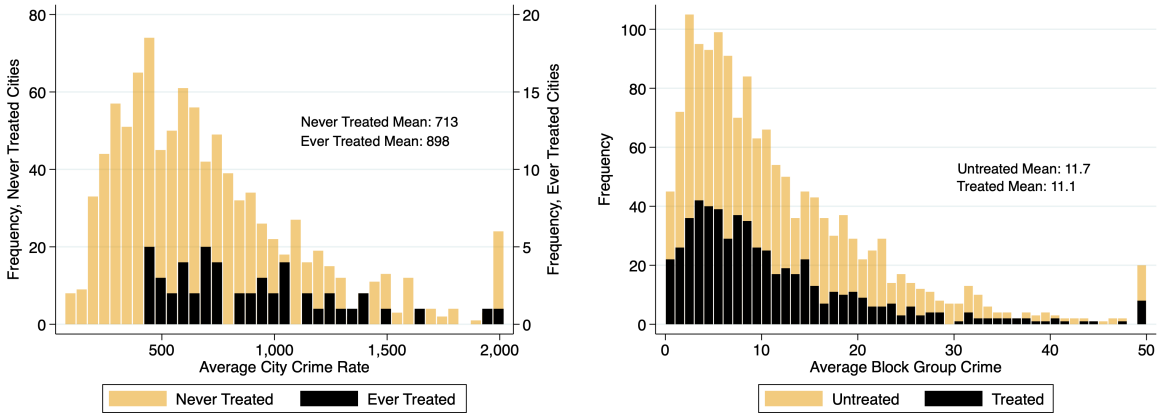
We present the distribution of crime in both of our datasets in Figure 1. For our NIBRS sample, we present average crime rates at the city level for the period January 2006 to March 2008. We choose this time frame as the first plasma center in our NIBRS sample opens in April 2008, allowing us to observe city crime statistics prior to any treatment. For our CODE sample, we present average crime at the block group level for the 12 months prior to a plasma center opening. For this second sample, our definition of treated and untreated block groups is explained in detail in Section 6.1.

Due to the sample selection restrictions we impose on our NIBRS sample–ensuring the inclusion of only agencies with consistently high-quality reporting–our final working sample includes 1,190 cities, 54 of which are ever-treated. While the ever-treated have higher crime rates prior to experiencing a plasma center opening, we observe in Figure 1(a) that there is common support

[13] For more information on the creation of this database, see Appendix D of Dooley and Gallagher (2024a). The authors have made the plasma center database publicly available at https://doi.org/10.7910/DVN/W9W713 (Dooley and Gallagher, 2024b).

in crime rates when comparing our ever-treated to their never-treated counterparts. Similarly, for our CODE sample, Figure 1(b) illustrates common support at the block level for crime outcomes. We also find that the average crime rates across treated and untreated block groups are very similar.

Figure 1: The Distribution of Crime Prior to a Plasma Center Opening



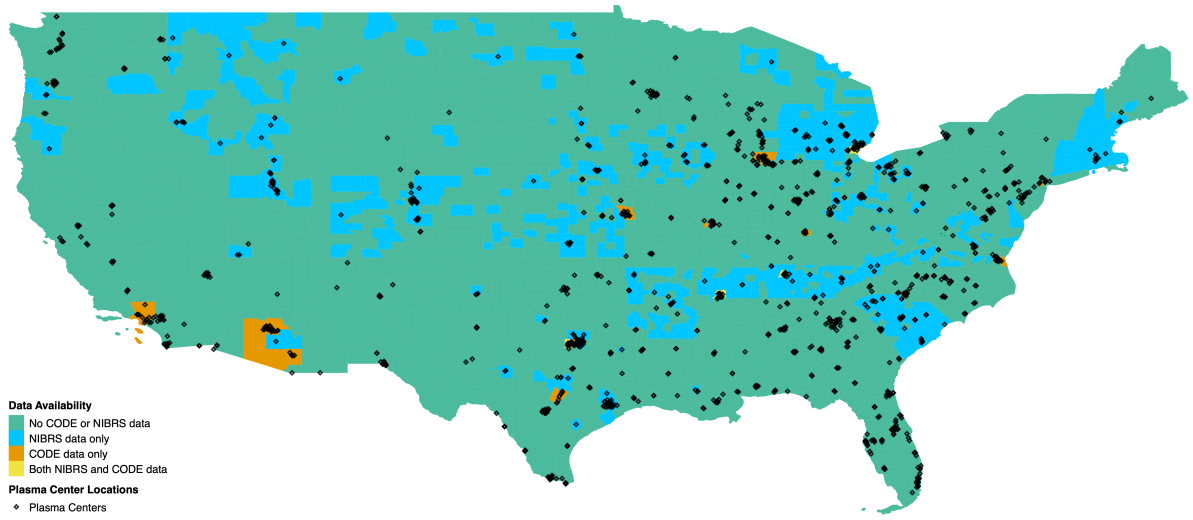
(a) NIBRS (b) CODE

Notes: In panel (a), average city crime rates are presented for our sample of NIBRS cities for the period of January 2006 to March 2008 – the period in our data where all ever-treated cities are still pre-treatment. In panel (b), average block group level crime counts are presented for our sample of CODE neighborhoods for the 12 months prior to a plasma center opening.

Next, we examine the location of plasma centers, mapping these against the backdrop of crime data availability in Figure 2. This figure conveys several key aspects of the spatial location of plasma centers and the data used in the study. First, plasma centers are located in the vast majority of US states, and typically cluster in large cities and metropolitan areas. Second, given the constraints we impose to ensure data reporting quality in our NIBRS sample, we exclude the majority of these plasma centers. Later in this section, we present a set of statistics on the location of plasma centers, both between- and within-city. Third, the map confirms an aspect of the CODE data discussed above – the limited geographic coverage of the data. Finally, the overlap between our NIBRS and CODE samples is minimal, which we view as an advantage since it improves the combined datasets' overall coverage in this analysis.

We complement the map of plasma center locations with information on the type of area in which plasma centers are located. We consider two spatial levels: the city level, which affords a cross-city comparison, and the block group level, which enables a within-city comparison. Key area characteristics are presented in Table 1, with all metrics reported in percentile rank form

Figure 2: Data Availability and the Location of Plasma Centers



Notes: To facilitate ease of viewing, we link our cities of interest to their respective county and then map our data at the county level. Areas corresponding to NIBRS data availability are those areas for which the NIBRS data passes the various sample selection criteria we discuss in Section 2.2.1. The CODE data availability corresponds to cities in the CODE data that pass the sample selection criteria we detail in Section 2.2.2. The plasma centers presented in the map are all plasma centers listed in the Dooley and Gallagher (2024a) database.

on a 0–100 scale to ensure straightforward comparisons across characteristics.

In Column 1, we present we present the average percentile rank of a set of area characteristics for the plasma centers we consider in our NIBRS-based analysis. This allows us to gain a general sense of where these centers are located.[14] We present respective p-values from two comparisons of means – the first compares our NIBRS plasma center locations to non-plasma locations, and the second compares them to plasma center locations excluded from our NIBRS analysis. Two broad patterns emerge from these combined statistics. First, our NIBRS-based plasma centers are located in cities of slightly above-average affluence (Column 1, Panel a).[15] Second, when shifting attention to within-city locations, NIBRS-based plasma centers appear to be located in neighborhoods similar to the average block group (Column 2, Panel b), as well as to the average non-matched plasma location (Column 3, Panel b).

Our CODE-based plasma centers are located in both more affluent cities (Columns 4-7, Panel a) compared to the average, as well as in more affluent neighborhoods within treated cities than

[14]Given the percentile rank averages to 50 for the full sample, we conserve column space and present only statistics for our points of interest.

[15]Our NIBRS-based plasma centers are also located in more affluent cities compared to the average location of plasma centers – to conserve space we do not present the means, but we present p-values from a test of equality of means and note that for each characteristic, our NIBRS-based plasma locations rank more favorably than non-NIBRS-based plasma locations.

Table 1: Characterizing the Location of Plasma Centers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	NIBRS-Based Plasma Centers			**CODE-Based Plasma Centers**			
	Locations with an Open Plasma Center	*p*-Value: NIBRS Plasma Location = Non-Plasma Location	*p*-Value: NIBRS Plasma Location = Non-NIBRS Plasma Location	Locations with an Open Plasma Center	*p*-Value: CODE Plasma Location = Non-Plasma Location	*p*-Value: CODE Plasma Location = Non-CODE Plasma Location	*p*-Value: CODE Plasma Location = NIBRS Plasma Location
Number of Plasma Centers	61			35			
(a) City-Level Location Characterization							
Percentile Rank of:							
Occupancy Rate	68.1	[0.000]	[0.009]	77.3	[0.000]	[0.000]	[0.022]
	(20.6)			(19.2)			
Ownership Rate	29.4	[0.000]	[0.072]	46.4	[0.466]	[0.000]	[0.013]
	(23.8)			(35.7)			
Unemployment Rate	50.1	[0.821]	[0.000]	43.2	[0.090]	[0.000]	[0.098]
	(19.2)			(22.7)			
Median HH Income	62.1	[0.000]	[0.017]	73.9	[0.000]	[0.000]	[0.004]
	(21.2)			(17.4)			
Poverty Rate	47.9	[0.519]	[0.001]	34.7	[0.000]	[0.000]	[0.007]
	(24.2)			(23.8)			
(b) Block Group-Level Location Characterization (Treated Cities Only)							
Percentile Rank of:							
Distance from City Center	24.7	[0.000]	[0.009]	47.2	[0.637]	[0.015]	[0.001]
	(20.9)			(36.9)			
Occupancy Rate	46.6	[0.288]	[0.330]	65.6	[0.000]	[0.000]	[0.000]
	(24.8)			(23.3)			
Ownership Rate	44.5	[0.124]	[0.241]	57.2	[0.166]	[0.001]	[0.039]
	(27.9)			(28.7)			
Unemployment Rate	47.1	[0.359]	[0.126]	41.3	[0.050]	[0.017]	[0.274]
	(22.4)			(24.6)			
Median HH Income	43.6	[0.051]	[0.165]	59.3	[0.024]	[0.000]	[0.001]
	(25.6)			(22.9)			
Poverty Rate	49.0	[0.820]	[0.035]	39.9	[0.034]	[0.000]	[0.095]
	(27.7)			(26.7)			

Notes: All location characertistics are represented as percentile ranks, on a 0-100 scale, to facilitate comparision across characteristics. Means and standard deviations in parentheses are presented. *p*-values from regression-based equality of means are presented. In these regressions, we specify Eicker-Huber-White standard errors. The set of plasma centers we consider are all plasma centers open pre-2020 in the mainland US states.

average (Columns 4-7, Panel b). These findings hold true if we compare our CODE cities to non-plasma locations, non-CODE-based cities with a plasma center, or to our NIBRS-based cities. This characterization of CODE-based plasma locations is worth bearing in mind as we proceed throughout the paper. To preview our key findings, (i) we find more pronounced effects using our NIBRS sample than our CODE sample[16] and (ii) we document crime-reducing impacts of plasma centers of considerably greater in magnitude in less affluent cities. Against the backdrop of these two points, we can reconcile the differences in treatment effects across our two samples by noting that our CODE-based sample of cities are more affluent than our NIBRS-based cities.

[16]The identification strategy may be a contributing factor here, as while we use a difference-in-differences design in both cases, our approach using the CODE sample takes a within-city approach, whereas we use a between-city approach with the NIBRS sample.

3 A Theoretical Model of Plasma Donations and Crime

In this section, we briefly review the economic model of crime, in order to get a sense of how a legal-sector, non-labor income shock may impact crime. We consider the workhorse, Becker-Ehrlich model of crime here (Becker, 1968; Ehrlich, 1973). Given that (i) this model takes a rational agent approach to criminal engagement and (ii) the key shock we are considering here is an income shock, the model is most useful when considering property crime or proactive aggression (Miller and Lynam, 2006). This model is more limited in considering the impact of a positive income shock on violence.

3.1 The Becker-Ehrlich Model

We follow the approach of Draca et al. (2018) in applying the standard economic model of crime à la Becker (1968) or Ehrlich (1973) to an aggregated setting. We start to consider an individual, who will commit crime if the expected value of crime exceeds that of engaging in the legal sector:

$$E(V_C) > E(V_L). \tag{1}$$

The expected value of crime is a weighted average of the benefits of crime (P) and the costs of being caught ($-S$), which occur with probability π:

$$E(V_C) = (1-\pi)P - \pi S. \tag{2}$$

Similarly, the expected value of engaging in the legal labor market is a weighted average of obtaining wage W when employed, benefits B when not employed, as well as income from plasma donations, D, which can be obtained in either employment state. Unemployment occurs with probability u:

$$E(V_L) = D + (1-u)W + uB. \tag{3}$$

Following the approach of Draca et al. (2018), we rewrite $\pi = \kappa_1 C + \kappa_2 O + \kappa_3$ where $\kappa_1 > 0$, $\kappa_2 > 0$, O is the strength of the police force and C the quantity of crime. We add the term κ_3 to allow for the fact that individuals may be exposed to different apprehension or detection technologies in different cities or states, and assume $\kappa_3 > 0$. We write down an equation for the

equilibrium of crime as follows:

$$(1 - \kappa_1 C - \kappa_2 O - \kappa_3)P - (\kappa_1 C + \kappa_2 O + \kappa_3)S = D + (1 - u)W + uB. \tag{4}$$

Rearranging yields the following:

$$C = \frac{P - D - (1 - u)W - uB - (P + S)(\kappa_2 O + \kappa_3)}{\kappa_1 (P + S)}. \tag{5}$$

Our leading hypothesis is that the income that may be derived from donating plasma at a nearby center will lead to a drop in financially motivated crime. To see this in our model, we partially differentiate Equation 5 with respect to D and multiply by D/C to obtain the crime-donation income elasticity:

$$\frac{\partial C}{\partial D}\frac{D}{C} = \frac{-1}{\kappa_1 (P + S)}\frac{D}{C} < 0. \tag{6}$$

Equation (6) makes clear the prediction of the Becker-Ehrlich model in our setting. The positive shock to expected income due to plasma donation payments (D) should reduce crime: individuals no longer need to risk incarceration and the associated costs of a prison sentence in order to increase their legal income. Instead, they can donate plasma. We can aggregate the supply of crime at the local level to obtain a city-level measure for the supply of crime.

We also consider a secondary hypothesis: that donating plasma leads to lower levels of crime due to post-donation fatigue. We do not consider this mechanism to play a primary role – given that much of crime is committed by younger individuals, it seems unlikely that feeling fatigued would lead to a first-order shift in crime propensities.[17] That noted, we present this hypothesis for completeness. There are two different ways to model this. One would be to note that the physiological effects of plasma donation would lead to lower expected returns from crime, and therefore impact P in Equation (2). A second way to model the fatigue effect of plasma donation would be to do so by including an additional term in the detection probability π, also in Equation (2). The rationale for such an approach would be grounded in the observation that fatigue would lead to an increased probability of being caught, conditional on committing a crime. [18]

[17] This is some, albeit limited, evidence that health factors can influence criminal engagement (Chalfin et al., 2019).

[18] By the selfsame logic, plasma donations will likely lead to an increase in the *demand* for crime via the physiological channel – post-donation, local residents may make easier targets for certain types of crime. We don't view this as a key concern, as this demand-side effect will counteract our primary mechanism, leading us to estimate a lower-bound on the crime-reducing effect of plasma donations.

The demand for crime will depend on local factors that relate to the gains from crime. This may involve local wages, house prices, levels of conspicuous consumption, levels of risk aversion, demographic composition, and many other factors. We may be able to proxy for a subset of these factors, but it is unrealistic to account for all relevant demand factors. To the extent that many of these factors will remain fixed at a low spatial level, we argue that city fixed effects along with state time effects will adequately subsume all relevant demand-side factors. The key assumption we make here is that donation income, D, does not impact the demand for crime.[19]

4 Empirical Approach

To estimate the effect of plasma centers on city-level crime, we start with a panel TWFE design. Our baseline specification takes the form:

$$C_{cst} = \beta D_{ct} + \gamma_c + \theta_{s\times t} + \epsilon_{cst}\,, \tag{7}$$

where C_{cst} is the crime rate in city c in state s in period t. We included city fixed effects to account for time-invariant unobservables at the city level. We incorporate state-by-year-by-month fixed effects to allow for differential, state-specific shocks to crime. These spatiotemporal fixed effects can account for potential threats to identification such as conflating our DD term with contemporaneous shocks to the local labor market (this is a first-order concern, given that labor market conditions play a key role in the Becker model of crime (Agan and Makowsky, 2023; Bignon et al., 2017; Britto et al., 2022; Hjalmarsson et al., 2024)), as well as time-varying shifts in state-level sentencing policy. We cluster our standard errors at the city level, consistent with the level at which treatment is determined in our setting.

Identification

When estimating our panel TWFE specifications, our target parameter is the ATT. To identify the ATT with our specification, we require the parallel trends assumption (PTA) to hold. We provide two interrelated pieces of evidence below to support the PTA. First, we present event study graphs in Figure 4, which allow for a graphical inspection to assess the presence of dif-

[19] If this assumption is incorrect, then when we estimate a crime equation of the form outlined in Section 4, the coefficient related to plasma centers will represent a lower (absolute) bound for the impact of a plasma donation income, D, on the supply of crime. This is because if D has an impact on crime demand, via gains from crime, then a plasma donation-induced increase in income, D, will lead to an *increased* demand for crime.

ferential pre-trends. To supplement this visual analysis, each event studies graphs also includes the p-value from a test of joint significance of the lagged terms displayed in the graphs. The evidence we present here suggests that the parallel trends assumption is satisfied in our setting.

A recent literature has highlighted that a TWFE DD design may be biased when treatment is staggered over time, as in our context. If treatment effects are heterogeneous over time, TWFE will no longer recover the ATT. We address these concerns directly in Section 5.2, where we provide evidence supporting the validity of employing a TWFE DD design to estimate the impact of plasma centers on crime.

Event Study Specification

To explore both dynamic treatment effects and to shed light on the validity of our key identifying assumption – the parallel trends assumption – we additionally consider an event study variant of Equation (7). This specification takes the form:

$$C_{cst} = \sum_{\substack{e=-7, \\ e\neq -1}}^{7} \beta_e D_{ce} + \gamma_c + \theta_{s\times t} + \nu_{cst} \tag{8}$$

where we define event time, e in years. We do this as we are interested in tracing out both the short- and medium-term impacts of plasma centers on crime, and a monthly specification in event-time is both very noisy, and adds little informational content to what we can glean from the annual event study specification in Equation (8). As before, the term γ_c is a city fixed-effect, $\theta_{s\times t}$ is a state-by-month-by-year fixed effect, and standard errors are clustered at the city-level.

5 Primary Results From City-Level Panel Data on Crime Incidents

In this section, we present our primary evidence on the causal impact of plasma centers on city-level crime. We start by presenting our DD estimates, which will give us a sense of the overall effect of plasma centers on crime. We then assess the validity of our baseline TWFE design in this context, concluding that the design is appropriate for our analysis. Next, we move to considering dynamic treatment effects, documenting the impact of plasma centers in both the short- and medium-run. Finally, we present evidence on distributional treatment effects, allowing us to better understand how plasma centers affect city-level crime.

5.1 DD Estimates

We display the DD estimates for total crime rates, as well as core crime categories in Table 1. In Column 1, we present the results for total crime rates. Here we see that a plasma center in the city leads to a large and statistically significant drop in the crime rate – post plasma center opening, the crime rate is 11.8% lower. Columns 2-6 enable us to see where this crime drop originates. Here, we find that declines in property crime (a 12.8% drop, accounting for 58% of total crime) and drugs possession offenses (a 14.3% drop, accounting for 18% of total crime) are the key drivers of the drop in total crime. Violent crime rates drop by 7.2% in response to a plasma center opening, , although this finding is borderline statistically significant. We note this nonetheless, given the large societal costs of violent crime.

The pronounced decline in property crime aligns with our primary hypothesis: that plasma center openings lead to crime reductions via a financial channel. Individuals on the margin of crime no longer rely on illegal activities to supplement income; instead, plasma centers provide them with a legal, alternative income source. We further break down the source of these crime drops by estimating our baseline specification for property crime sub-categories in Appendix Table A1. In this table, we find that theft, which accounts for over three fifths of property crime, drops by 13.4%. We also document small absolute, but large proportional, drops in car theft.

A secondary notable reduction in crime following the establishment of plasma centers is observed in column 5 of Table 1, with drug possession incidents dropping by over 14%. This decline is likely influenced by two countervailing forces. On one hand, increased access to cash from plasma donations may encourage drug use in treated cities.[20] On the other hand, donors could be either temporarily or permanently barred from donating if plasma centers identify signs of drug use during physical examinations, such as visible injection marks or signs of intoxication, or if donors disclose intravenous drug use on medical history questionnaires (U.S. Food and Drug Administration, 2025). These strict regulatory measures might serve as a deterrent, discouraging individuals who use drugs from attempting to donate due to the risk of detection and subsequent temporary or permanent disqualification. Appendix Table A2 sheds additional light on this finding. Marijuana possession falls by 15% and methamphetamine possession by 39%, while we document no significant changes for hard drug categories of cocaine (including crack cocaine) or

[20] This is what Watson et al. (2020) find – substance use spikes in the days after individuals receive a lump-sum universal basic income payment.

heroin.

Table 2: The Impact of Blood Plasma Centers on City-Level Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Crime	Violent Crime	Property Crime	Drug Sales	Drug Possession	Other Crime
Plasma Center in City	–76.3***	–9.11*	–48.3***	-.567	–17**	–1.25**
	(21.3)	(5.29)	(15.5)	(1.16)	(7.09)	(.485)
Proportion of Total Crime	[1.000]	[0.197]	[0.584]	[0.022]	[0.182]	[0.015]
$\overline{Y}_{NT}$	648	127	377	14.7	119	9.34
Plasma Center/ $\overline{Y}_{NT}$	-.118***	-.0715*	-.128***	-.0386	-.143**	-.134**
	(.0329)	(.0415)	(.0411)	(.0791)	(.0594)	(.0519)
Number Treated Cities	53	53	53	53	53	53
Number Cities	1,185	1,185	1,185	1,185	1,185	1,185
Observations	198,744	198,744	198,744	198,744	198,744	198,744
Adjusted R^2	.718	.678	.707	.274	.498	.247

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the city level. The outcome variables crime rates – monthly counts of city-level crime per 100,000 city-level population. Staggered DD estimates for our main specifications for the effect of having ever opened a plasma donation clinic on city crime rates per 100,000 population. The estimates are obtained by including city and state-by-month-by-year fixed effects. We present both raw and scaled DD estimates – the latter are scaled by $\overline{Y}_{NT}$ – the mean crime rate in never-treated cities, which allows for a proportional interpretation. Applying the decomposition of Goodman-Bacon (2021a) to a simplified setting of city and year fixed effects, we find clean comparisons comprise 98.0% of the weight used to form our baseline DD estimates. The estimating sample is defined as the cities matched between the NIBRS data and plasma clinic data for which the local police agencies report in all periods from January 2006 and ending in December 2019. Cities which have opened a plasma donation clinic prior to January 2006 are excluded from the sample.

5.2 Are TWFE Estimates Valid in Our Setting?

In settings with staggered treatment adoption, TWFE estimates a weighted sum of treatment effects.[21] However, when treatment effects are heterogeneous, these weighted sums are not easily interpretable and may not even represent a causal parameter.[22] A collection of studies instead suggest alternative estimators that leverage only valid causal comparisons between pairs of treated and untreated observations in order to produce an appropriately weighted estimate that is consistent for the ATT (Callaway and Sant'Anna, 2021; Borusyak et al., 2024; Cengiz et al., 2019; Wooldridge, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Gardner, 2022). Since each estimator varies in its choice of comparison group, we evaluate the robustness of our main findings across a diverse set of alternative estimators.

Given the temporally staggered nature of plasma centers across cities, concerns may arise regarding the use of TWFE estimates in this context. To address these concerns, we provide two pieces of evidence. First, we implement the Goodman-Bacon (2021a) decomposition for a

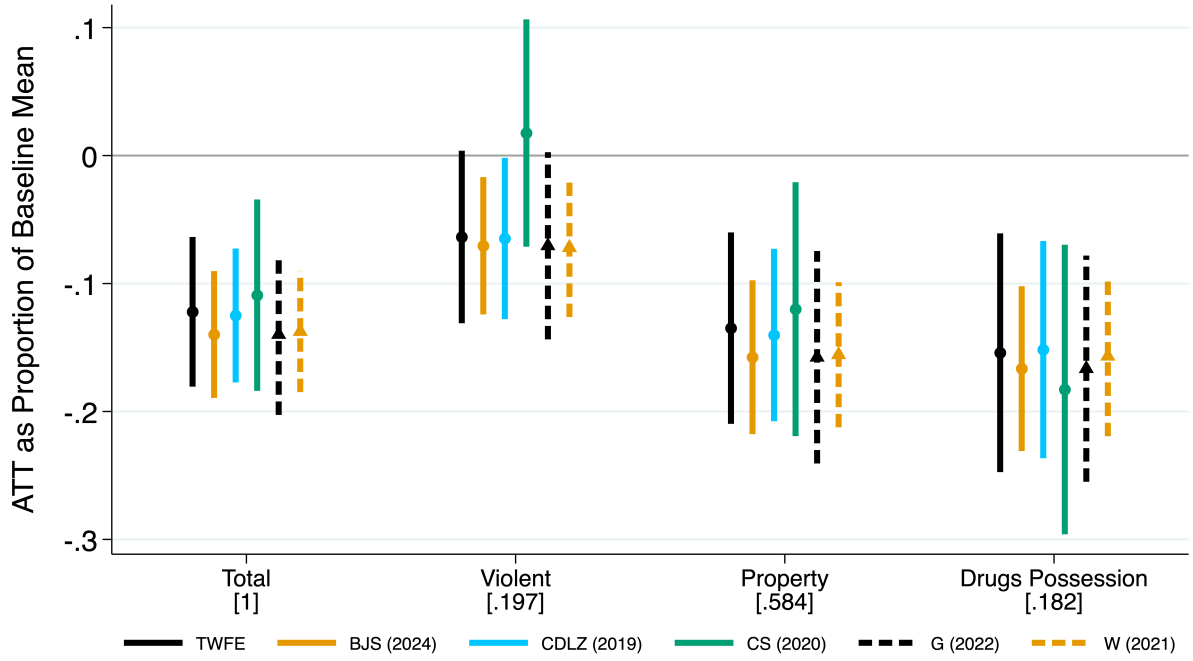
[21] Please refer to Roth et al. (2023) and Baker et al. (2025) for recent reviews of related studies.

[22] Following the notation of Blandhol et al. (2022), a parameter holds a weakly causal interpretation if it can be represented as a positively weighted average of treatment effects. Borusyak et al. (2024) show that the weights in the TWFE estimand can take negative values, particularly when the share of early-treated units is low.

simplified version of our baseline model (time fixed effects instead of state-by-time). In this specification, we find that 98% of the weight used to form the simplified TWFE DD estimates come from clean comparisons – never treated cities. This statistic, reported in Table 2, suggests that our TWFE estimates are unlikely to be first-order impacted by "negative weighting" issues, as clean comparisons comprise almost the entirety of the weight used to form our DD estimates.

Second, we provide in support of the validity of using TWFE estimates in our setting are presented in Figure 3 below. For each of our key crime outcomes, we present our baseline estimates alongside several alternative estimators which are robust to potential biases induced in a staggered setting – those from Borusyak et al. (2024), Cengiz et al. (2019), Callaway and Sant'Anna (2021), Gardner (2022), and Wooldridge (2021). Our TWFE estimates are in line with these more recently developed estimators, which suggests that our baseline specification is appropriate in this setting. For these reasons and for brevity, we focus on TWFE estimates in the subsequent sections.

Figure 3: Alternative DD Estimators



Notes: We present point estimates and 90% confidence intervals for our DD estimates from a variety of different estimators. These include: our baseline estimator [TWFE], and the estimators from Borusyak et al. (2024) [BJS (2024)], Cengiz et al. (2019) [CDLZ (2019)], Callaway and Sant'Anna (2021) [CS (2021)], Gardner (2022) [G (2022)], and Wooldridge (2021) [W (2021)]. In square brackets under each category label, we present the proportion of total crime accounted for by each crime category. Sample period: January 2006-December 2019. Data source: NIBRS.

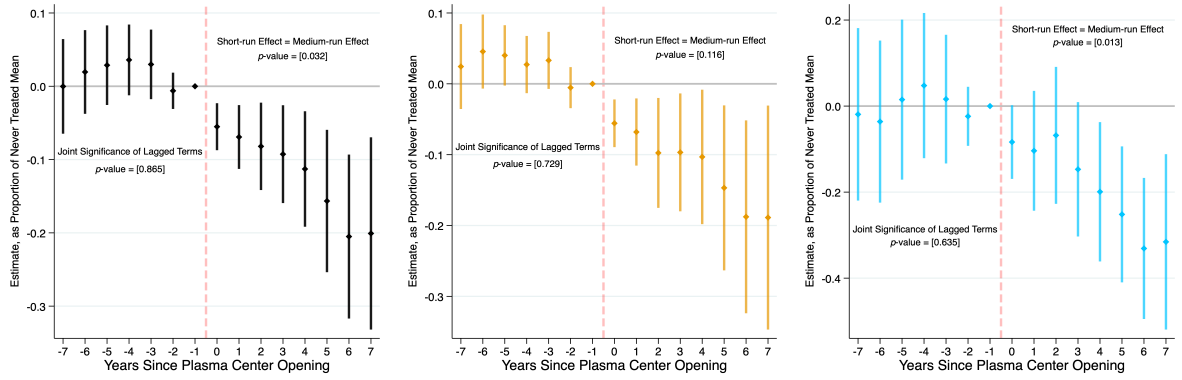
5.3 Dynamic Effects

We now consider the dynamic treatment effects of a plasma center opening on crime. We present the resulting estimates from Equation (8) in the form of event study graphs. In Figure 4 we present results for total crime rates, as well as the two crime categories with the largest response to a plasma center opening – property crime and drugs possession.

The event study graphs highlight two key findings. First, there are no statistically significant or economically meaningful pre-trends in any of our outcomes. This is of first-order interest to us, as it provides strong evidence for our identifying assumption: parallel trends between treated and untreated cities. We additionally test for the joint significance of the pre-treatment/lagged terms, providing the p-value from this test in the south-west quadrant of each graph. In all cases, we cannot reject the null that the lagged treatment effects are jointly equal to zero.

Second, the graphs illustrate the temporal pattern of treatment effects. Crime rates drop immediately after a plasma center opens, with the effect intensifying over time. Seven years post-plasma center opening, property crime rates are 20% lower, and drugs possession rates 30% lower than immediately before the plasma center opened. In Section 7, we revisit this in greater detail.

Figure 4: Event Study Estimates of Plasma Center Opening on City Crime Outcomes



(a) Total Crime (b) Property Crime (c) Drug Possession

Notes: We present point estimates and 90% confidence intervals for dynamic treatment effects, estimated using the specification outlined in Equation (8). Sample period: January 2006-December 2019. Data source: NIBRS.

5.4 Distributional Effects

As a final exercise in this section, we consider the distributional effects of plasma centers on crime. We present the results of our distributional analysis in two different forms. In Figure 5,

we present the results from our distributional DD regression in the form of an inverse cumulative distribution function (CDF) representation (hereafter distribution regression). In Appendix Figure A2 we present the estimates in unconditional quantile partial effect (UQPE) form.[23]

To apply the distribution regression approach, we estimate our standard DD model, as in Equation (7), but replace the dependent variable with a series of dummies indicating if the crime rate is greater than a given quantile, Q_τ, of crime rates in cities that we never observe with a plasma center, for $\tau = [1, \ldots, 99]$, that is $y_{cst} = \mathbb{1}[\text{crime}_{cst} > Q_\tau]$. This gives rise to 99 distribution regressions, allowing us to trace the effect of plasma centers along the full distribution of crime rates.[24]

The distributional DD results are informative regarding the distributional source of our baseline estimates – plasma centers reduce city crime rates at the mean by reducing crime at the upper end of the distribution. Such a finding is of policy-relevance, and not merely limited to the roll-out of future plasma centers. What emerges is that the crime-reducing elements that comprise the treatment effect associated with plasma centers – including but not limited to legal income in return for a few hours of time per week plus regular blood testing – are particularly important in higher crime areas.

It is of note to consider these findings with reference to the distribution of crime in our ever-treated cities, relative to their never-treated counterparts (Figure 1). There is a large region of common support across the two groups of cities. Hence, our distribution regression findings are not some mechanical artifact. Rather, the finding indicates that plasma centers reduce crime on average by reducing crime in the upper tails of the crime distribution. This finding is particularly noteworthy in the context of between-city inequality. The expansion of plasma centers may unintentionally contribute to a reduction in crime incidence inequality across cities, further enhancing their societal impact.

6 Additional Evidence From a Within-City Design

We supplement our primary city-level evidence on the impact of plasma centers on crime with data from treated cities at the block group by month level. We take this approach for two core reasons. First, the logic of triangulation – we get a better sense of the impact of plasma centers on crime by considering the treatment effect from multiple different angles. Second, the

[23] We discuss the pros and cons of this approach for our setting in Appendix Section A.3.
[24] An example of this approach can be seen in Goodman-Bacon (2021b).

Figure 5: Distributional Effects of Plasma Centers on Crime



Inverse CDF Representation

Notes: We present point estimates and 90% confidence intervals for the impact of plasma centers on crime from a series of distributional DD regressions. The estimates come from a set of regressions where the outcome is $y_{cst} = \mathbb{1}[\text{crime}_{cst} > Q_\tau]$ for $\tau = [1, \ldots, 99]$. Sample period: January 2006-December 2019. Data source: NIBRS.

secondary data is is much more spatially granular, thereby allowing us to explore new questions. For instance, is there a distance decay curve of the impact of plasma centers on crime? If so, what does it look like?

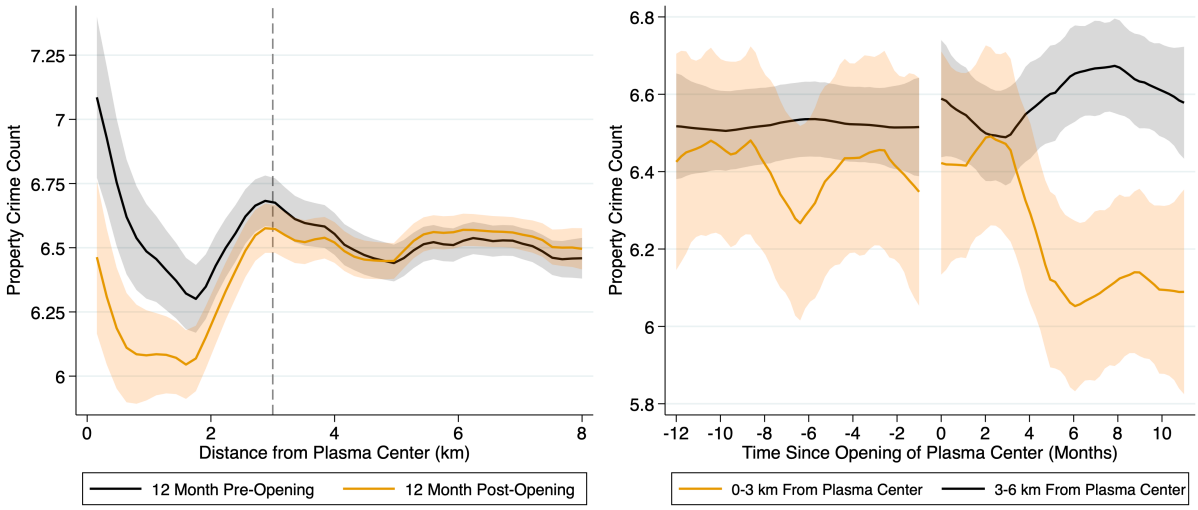
6.1 Identification

We approach identification of a within-city treatment effect in the spirit of work by Linden and Rockoff (2008) and Ang (2021). This entails non-parametrically plotting the spatial evolution of crime with distance from plasma centers for two time periods – the year before the opening of a plasma center and the year after the opening. Next, we overlay the two graphs to look for departures of the crime-distance function post-opening relative to the pre-opening period. This gives us a data-driven estimate of the treatment zone. The typical approach taken in the literature is somewhat ad-hoc in nature. There is no firm consensus on how one should set up the initial non-parametric distance decay setting, nor is the pivot from these graphs to a formal econometric specification clearly defined. Should one consider all relevant outcome variables in this exercise? If only one, which one? Given the resulting estimating equation will typically include fixed effects and potentially other control variables, what is the correct level of residualization for the non-parametric distance-decay graphs?

In our setting, we consider property crime as the key outcome, given the results from the

previous section. We residualize crime by city-month-year and census tract fixed effects to account for city-specific time effects in crime as well as broad spatial patterns in crime.[25] We present the resulting non-parametric crime-distance curves in Figure 6. Additionally, we provide a second graph illustrating temporal effects for block groups (i) near to and (ii) farther from a plasma center. In the 12 months following a plasma center opening, Figure 6(a) highlights a

Figure 6: Establishing a Distance-Based Cutoff



(a) The Crime-Distance Decay Curve

(b) Temporal Differences by Distance

Notes: Local polynomial estimates and 90% confidence intervals are presented in both panels. We use Epanechnikov kernels, rule-of-thumb (ROT) bandwidths, and pilot bandwidths equal to 1.5 times the ROT bandwidths for standard error calculations. Data source: CODE.

statistically significant departure from the pre-opening crime-distance pattern – crime is lower closer to plasma centers. This crime differential dissipates with distance, and by 3 km, there is no statistical difference between the two curves. We use this graphical evidence to inform a distance-based cutoff for our empirical design. We specify a treatment indicator taking value 1 for census blocks within 3 km from a plasma center, and set a value of 0 for those census blocks 3-6km from a plasma opening. Figure 6(b) serves as both (i) an internal validity check – there are differences post-opening by distance-based treatment status – and (ii) a preview of the event study graph we present later.

Informed by the patterns we observe in Figure 6, our within-city empirical specification takes the form:

$$C_{gct} = \beta D_{gt} + \gamma_g + \theta_{c \times t} + \epsilon_{gct}\,, \tag{9}$$

[25] In our empirical specification for this section, we apply fixed effects at a finer spatial level—census block group.. At this exploratory stage, however, our aim is to condition on broader spatial units to better explore local spatial differences.

where C_{gct} is the crime count in census block group g in city c in period t. D_{gt} takes value 1 for areas within a 3 km radius of a plasma center once it opens, and 0 otherwise. Block group fixed effects absorb low-level spatial differences in crime within the city, which is particularly crucial given the non-random placement of plasma centers within cities. Given the narrow time-window we consider in this setting (+/- 12 months of an opening), these fixed effects will capture both time-invariant unobservables and slow-moving-in-time time-variant unobservables (e.g., changing demographics within a city, migration patterns, shifting land-use within a city). Additionally, we included city-by-month-by-year fixed effects. Not only will these be important to capture differential crime shocks across cities over time, but these city-specific time fixed effects will account for any changes in city-level crime recording practices over time. We can thus be confident that we are not conflating our estimation of treatment effects with changing crime recording practices. Finally, given that some cities have more than one plasma center, we may observe a block group within 6 km of two different plasma centers at two points in time. We thus cluster our standard errors at the block group-by-plasma center level, as it is this level that we observe treatment status.

6.2 Within-City Results

We present our DD results in Table 3. We find that neighborhoods close to a plasma center experience lower crime than areas farther away once the center opens. Total crime falls by 2.7%, violent crime by 3.9%, and property crime by 4%. We do not find any statistically significant impact on drug-related crime.

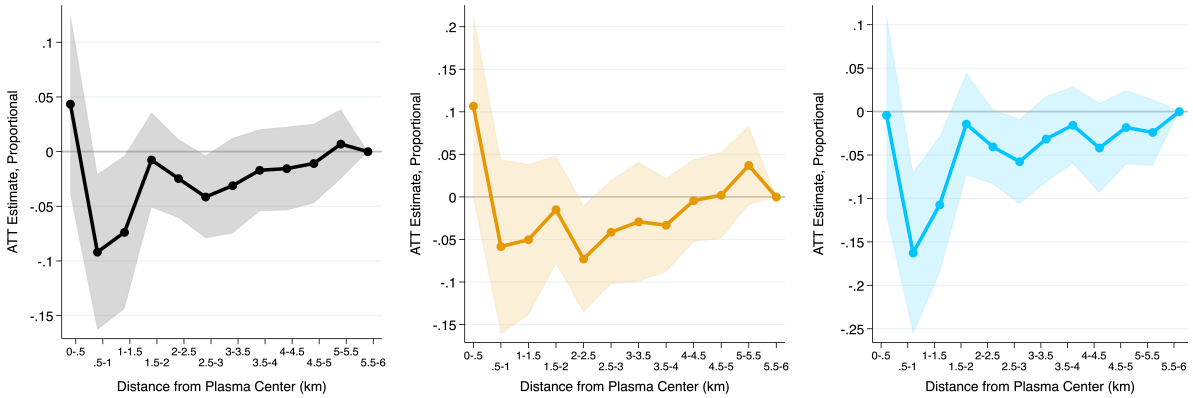
We present a more spatially nuanced version of the key crime outcomes affected by a plasma center opening in Figure 7. For this exercise, we estimate a variant of Equation (9), where the base spatial category is not 3-6 km, but rather 5.5-6 km, from the nearest plasma center. Additionally, we allow the treatment effect to vary across concentric 500m bands surrounding the plasma center. In Figure 7(a) we see that our DD estimate in Column 1 of Table 3 is driven primarily by neighborhoods in the closest proximity to a plasma center, a finding echoed by the patterns we document for property crime in Figure 7(c). As a final consideration of how neighborhood-level crime responds to a plasma center opening, we present dynamic DD estimates in the form of a series of event study plots in Figure 8. Once again, the purpose of

Table 3: The Impact of the Opening of a Nearby Blood Plasma Center on Block-Group Crime Counts

	(1)	(2)	(3)	(4)	(5)
	Total Crime	Violent Crime	Property Crime	Drug Crime	Other Crime
Near Plasma Center × Open	-.318**	-.107**	-.26**	.0185	.0309
	(.153)	(.054)	(.105)	(.0268)	(.039)
Proportion of Total Crime	[1.000]	[0.238]	[0.567]	[0.062]	[0.133]
$\overline{Y}_{NT,pre}$	11.7	2.76	6.57	.8	1.61
DD/ $\overline{Y}_{NT,pre}$	-.027**	-.0387**	-.0396**	.0232	.0192
	(.013)	(.0195)	(.016)	(.0335)	(.0242)
Observations	52,440	52,440	52,440	52,440	52,440
Adjusted R^2	.805	.63	.747	.502	.588

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the city level. The outcome variables here at crime counts at the census block-group by month level. The coefficient of interest here relates to our DD term, Near Plasma Center × Open. Near Plasma Center is defined as the crime-weighted centroid of the block-group being located within 3km radius of a plasma center. Open takes the value 0 in the 12 month prio to an opening, and 1 for the 12 month after an opening. The estimates are obtained by including block-group and city-by-month-by-year fixed effects. We present both raw and scaled DD estimates – the latter are scaled by $\overline{Y}_{NT,pre}$ – the mean crime count in non-treated block-groups in the 12 months prior to a plasma center opening, which allows for a proportional interpretation.

Figure 7: The Effect of Plasma Centers on Crime by Distance

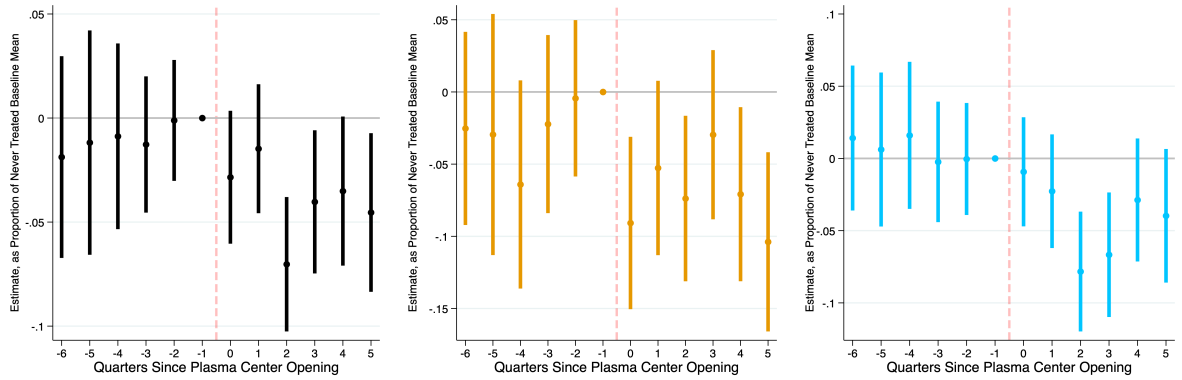


(a) Total Crime (b) Violent Crime (c) Property Crime

Notes: We present point estimates and 90% confidence intervals for a spatial extension of our baseline DD design, using the outer ring of 5.5-6 km as the control, and allowing all inner rings a separated DD-based treatment effect. Sample period: February 2009-September 2019. Data source: CODE.

these is twofold. First, all graphs consistently show that there are no differential trends between treatment and control neighborhoods in the two years prior to the opening of a local plasma center. Second, the timing of crime reductions following an opening varies by crime type – for property crime, the effect only materializes after six months, whereas for violent crime, the impact is immediate.

Figure 8: Event Study Estimates of Plasma Center Opening on Within-City Crime Outcomes



(a) Total Crime (b) Violent Crime (c) Property Crime

Notes: We present point estimates and 90% confidence intervals for dynamic treatment effects, estimated using a dynamic variant of the specification outlined in Equation (9). Sample period: February 2009-September 2019. Data source: CODE.

6.3 Reconciling our Within- and Between-City Estimates

We complete this section on within-city estimates of the impact of plasma centers on crime by considering how we may reconcile the differing magnitudes of treatment effects estimated using our cross-city design using NIBRS and our within-city design using CODE. We preempted a dimension of this discussion when discussing Table 1 in Section 2.3. There we noted that our CODE plasma centers are located in more affluent cities than our NIBRS centers and in more affluent neighborhoods than our NIBRS centers. Affluence is proxied here by multiple factors, including home-ownership rates, unemployment rates, household income, and poverty rates.

The contrast in plasma center locations is particularly relevant given the sub-sample treatment effect heterogeneity analysis presented in the next section (see Figure 9). To preview these findings, we find treatment effects of considerably larger magnitudes in less affluent cities in our NIBRS sample. Combining (i) the fact that our NIBRS sample of plasma centers are located in less affluent areas with (ii) the stark treatment effect heterogeneity that we present in Figure 9, we can reconcile the difference in magnitude between our two samples of plasma centers.

A final point of reconciliation between the two samples pertains to the differences in the time windows around a plasma center opening considered in the two designs. The NIBRS analysis adopts a medium-run perspective, whereas the CODE analysis focuses on a much shorter time horizon. As we show in Figure 4 – the event study graph for our NIBRS sample – the impact of a plasma center opening on crime is initially modest and builds over time. Our CODE window is limited to 12 months around a center opening for our DD design, which we expand to 18

months for our event study analysis, in order to gain a better sense of both pre-trends and dynamic treatment effects. The 1-year effect of a plasma center opening we document for our NIBRS sample in Figure 4 (property crime: -5.6% is of a similar magnitude to the CODE-based treatment effects we highlight in Table 3 (property crime: -4.0%).

7 Mechanisms

To develop a deeper understanding of the potential underlying mechanisms driving the results, we conduct three additional sets of analyses. First, we merge in characteristics of our treated areas and explore the heterogeneity of treatment effects across key sociodemographic variables. This allows us to connect the theoretical framework introduced in Section 3 with our empirical findings. Recall that our primary hypothesis in Section 3 posited that plasma centers should reduce crime via an income channel. Thus, one of the aims of this section is to empirically test the predictions that arise from our model. Second, we document how our DD estimates differ across age groups of offenders. The purpose of this exercise is twofold. First, it serves as an internal validity check – we should be finding pronounced effects for younger individuals, as donors are more likely to be young. Second, given the returns to experience in the labor market, treatment effect heterogeneity by age can serve as an additional piece of evidence in support of donation income playing the role as the key mediating channel between plasma center donations and crime reduction. Finally, we explore the viability of an incapacitation channel driving our findings. We do so using center-level information on opening times across the week.

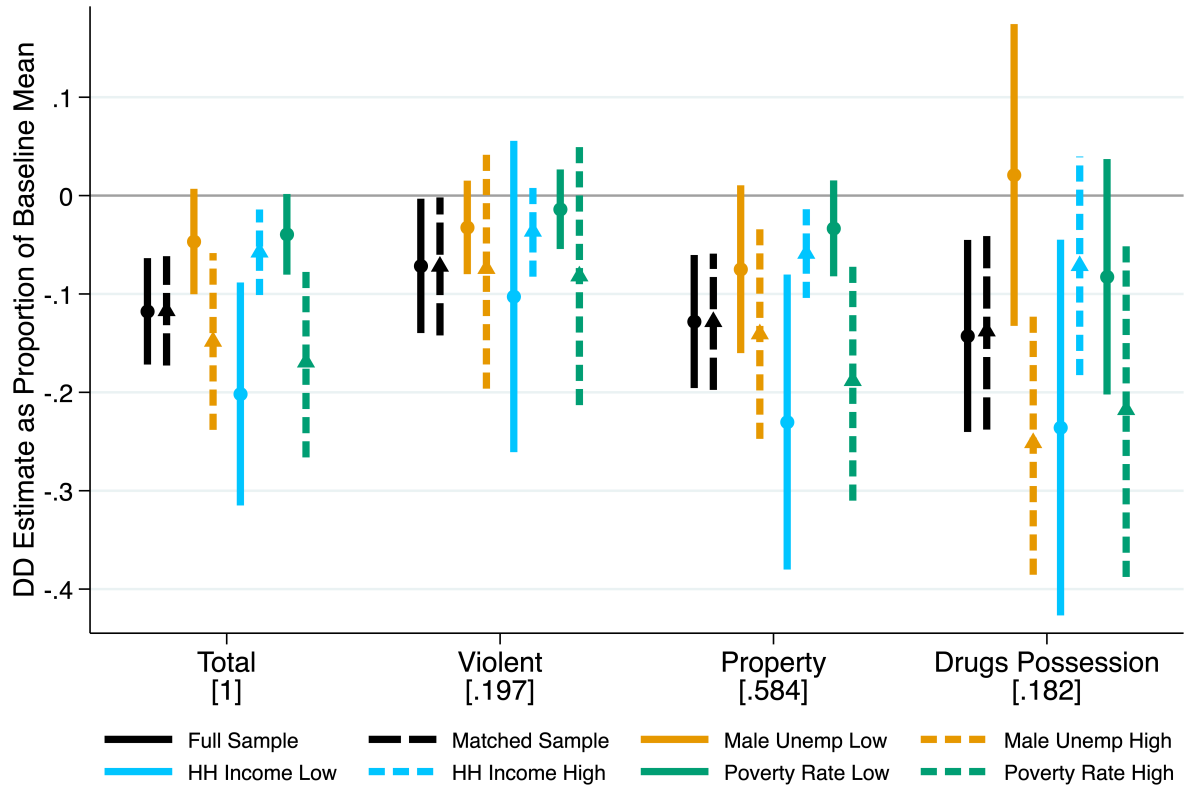
7.1 Treatment Effect Heterogeneity by Income Proxies

We start by matching our NIBRS cities to census information from 2000 via census places names.[26] City/place limit definitions appear to differ in the two datasets, as do naming conventions, resulting in an 85% match rate. As we show, our treatment effect estimates remain effectively unchanged when using either the full sample or the matched sample – missingness does not impact our estimates. Using our NIBRS sample, we estimate our baseline DD equation for a variety of sub-samples. We create sub-samples in a simple fashion, taking the median of each city-level characteristic and assigning cities to high or low status on each dimension based on whether they are above or below the median. We present the results graphically in Figure 9,

[26]Specifically, we use information from the Census 2000 Summary File 1 – which has full population coverage – and Summary File 3 – which samples 1 in 6 households.

where each point is a DD estimate for a given crime type/sub-sample combination.

Figure 9: Heterogeneity Analysis Using Predetermined City Characteristics



Notes: We present point estimates and 90% confidence intervals for our DD estimates from a variety of different sub-samples, as listed. In square brackets under each category label, we present the proportion of total crime accounted for by each crime category. Sample period: January 2006-December 2019. Data source: NIBRS.

For total crime rates, we document pronounced heterogeneity in treatment effects along three, inter-related dimensions, all of which align with the affluence of the city: male unemployment rate, median household income, and poverty rate. Relative to the never-treated mean, cities with above-median baseline income experience a 5% drop in crime due to a plasma center opening. Cities with below baseline incomes experience a drop *four times greater in magnitude*. The effect size ratio remains consistent across other crime measures, and the pattern of treatment effect heterogeneity is highly similar for property crime. In more affluent cities—those with low poverty rates or low male unemployment rates—effect sizes are muted, with confidence intervals overlapping zero. In contrast, less affluent cities exhibit large, statistically significant reductions in crime.

These findings suggest that the primary mechanisms driving crime reduction once a plasma center opens is financial. Recall that donors can earn approximately $400 per month by donating plasma twice a week. As Dooley and Gallagher (2024a) find through survey-based evidence,

donors tend to be young, less educated, low-income, and predominantly male. While we don't have perfect measures of who commits crime, the demographics of plasma donors overlap considerably with demographic data on individuals who are arrested or incarcerated (Lochner and Moretti, 2004; Kearney et al., 2014; Hayes and Barnhorst, 2020). This overlap in who donates plasma and who we observe being arrested or incarcerated establishes both the possibility, and plausibility, of individuals substituting property crime – with its unpredictable payoffs and inherent risks – for the guaranteed income stream from four hours per week at the local blood plasma center.

Two additional findings reinforce the credibility of the financial channel as a the key mechanism linking plasma center openings and crime drops. First, we do not detect such pronounced differences across cities of high and low levels of affluence when observing the treatment effects for violent crime. These effects, as well as being considerably smaller than those for property crime, exhibit less movement across the various measures of affluence. This relative homogeneity of the treatment effects for violence across the various sub-sample suggests financial motives are not the primary channel mediating plasma center presence in a city with reduced violence. Second, we only detect notable treatment effect heterogeneity for male unemployment rates, not female unemployment rates (see Appendix Figure A4). Given that men comprise the majority of both perpetrators of property crime, and those arrested for property crime, this suggests that the financial benefit of plasma centers plays a key role in deterring property crime.[27]

The final crime outcome of interest is drug possession rates. Once again, we find marked differences in treatment effects between less and more affluent areas. Our interpretation of these findings is that once again income is the key driver of these differences. In poorer areas, where the income to be gained from plasma donation accounts for a larger proportional of total income of a potential donor, there is more to be lost from testing positive for drug use and being temporary or permanently barred from donating. While this perceived cost remains present in wealthier areas, its proportional impact is substantially lower.

7.2 Treatment Effect Heterogeneity by Age

Our main analysis shows that the opening of a plasma donation center leads to large and persistent declines in city-level crime. We argue that these effects arise because potential offenders adjust their behavior when they gain access to a reliable, legal income source through plasma

[27]See https://ucr.fbi.gov/nibrs/2013 for statistics by gender for 2013 – the temporal midpoint for our sample period.

donation. In this section, we test this mechanism directly by examining how the crime response varies by the age of offenders. Since Dooley and Gallagher (2024a) document that plasma donors are disproportionately young, evidence of stronger crime reductions among young offenders would support our interpretation.

To characterize the age profile of the marginal offender affected by plasma center entry, we group NIBRS offenses into five-year age bins starting at age 18. We then re-estimate Equation (7) using the age-specific crime rate for each bin as the outcome. Figure 10 reports the estimated effects on total, property, and drug possession crime rates. The largest declines occur among the two youngest offender groups (ages 18–28), where crime falls by roughly 12 offenses per month per 100,000 residents. The magnitude of the effect decreases monotonically with age and approaches zero for the oldest offender bins. The same pattern holds for property crimes and, to a lesser extent, drug possession crimes. This age gradient is precisely what we would expect if plasma center openings reduce crime primarily by altering the behavior of individuals most likely to participate in the plasma donor market.

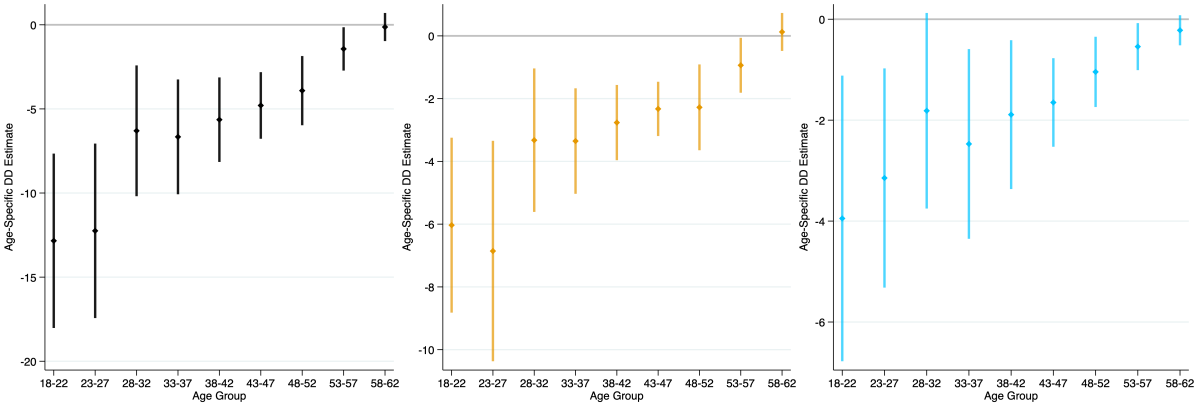
The age of the offender is only reported in roughly half of the offenses contained in the NIBRS data, which can be attributed to the fact that not all incidents contained in NIBRS result in an arrest. This introduces a potential concern that the estimated age-based effects somehow reflects changes in reporting practices of offender information across treated cities. To rule out this concern, we estimate the effect of opening a plasma center on the share of offenses that include the age of the offender using Equation (7). Results for all crimes and individual crime categories are presented in Appendix Figure A1. Across all crime categories, we do not find any evidence that plasma center openings are correlated with changes in reporting patterns.

7.3 Ruling Out Incapacitation as a Mechanism

We next explore the possibility that our findings could be driven by an incapacitation channel, specifically that by donating blood plasma, potential criminals are temporally incapacitated by donation. We construct an hour-by-day variant of our primary NIBRS data, with crime outcomes varying at the city-hour-day-month-year level and estimate a specification of the form:

$$C_{cshdt} = \beta_1 D_{ct} + \beta_2 (D_{ct} \times open_{chdt}) + \gamma_c + \theta_{s\times t} + \tau_{c\times w\times q\times h} + \epsilon_{cshdt}\,, \tag{10}$$

Figure 10: Crime Responses by Offender Age



(a) Total Crime (b) Property Crime (c) Drug Possession

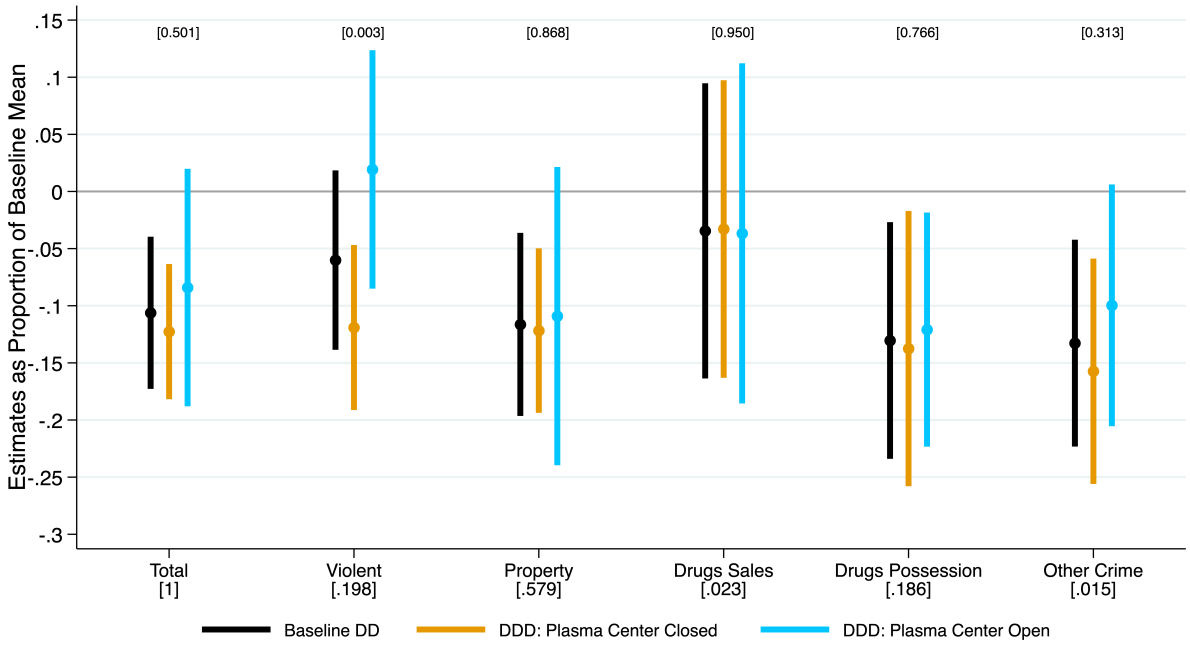
Notes: We present point estimates and 90% confidence intervals for the effect of opening a plasma center on city-level crime rates by the age of the offender reported in the NIBRS data. Effects are estimated using a staggered DiD design described by Equation (7). Sample period: January 2005 - December 2019. Data source: NIBRS.

where h = denotes hour; d = day of the week; w = a dummy for weekend days ; and q = an indicator for season/quarter. Additionally we allow our DD coefficient to vary by whether or not the plasma center in the reference city is opened or closed on that day, at that time. In order to run this specification, we manually search and then code up the opening times of each plasma center in our sample.[28] The high-dimension fixed effect $\tau_{c\times w\times q\times h}$ allows for city-level shocks by weekday/weekend, quarter, and hour. This allows for differential supply- and demand-side crime factors by season and hour (this will encode weather and daylight at the city-level) and differential activity by weekday and weekend at the city-level. The purpose of such high-dimensional fixed effects is to ensure we are truly picking up the impact of plasma centers on high-frequency crime rates and not city-specific temporal shocks. We present (i) our baseline DD estimate re-estimated on the hourly data, and (ii) estimates of β_1 and β_2 from Equation (10) in Figure 11 below. Our findings from this analysis highlight a constancy of the impact of plasma centers on local crime outcomes. We do not find any statistically significant differences in the effect of plasma centers on crime by opening time, with the exception of violence. We note that the violence estimates go in the wrong way for this to be driven by incapacitation.

The evidence we present in this section suggests that incapacitation is not a key mechanism driving our baseline findings. If it were, we would expect to find the lion's share of crime reductions during opening times (when donors are incapacitated), and more muted crime-reduction

[28] Because historical opening and closing times for our study period were unavailable, we rely on current listings. In practice, these hours were obtained by consulting plasma center websites, corporate sites of major providers (e.g., Biomat, Kedplasma), and Google business listings, and then recording the posted schedules.

Figure 11: Heterogeneity Analysis by Opening Time of Plasma Center



Notes: We present point estimates and 90% confidence intervals for our DD estimates from a variety of different sub-samples, as listed. Above the estimates we present p-values for the test of equality between β_1 and β_2. In square brackets under each category label, we present the proportion of total crime accounted for by each crime category. Sample period: January 2006-December 2019. Data source: NIBRS.

effects when the centers are closed. We find no support for the incapacitation mechanism in the data.

8 Discussion and Concluding Remarks

Plasma donation centers have become an ubiquitous feature of the urban landscape in the US, operating in almost all states and typically clustering in urban areas. We approach our study of these plasma centers by considering their income-generating potential, linking this to the canonical economic model of crime, which predicts an increase in legal income opportunities should lead to declines in economically-motivated crime. Using two complementary difference-in-differences research designs, this work examines the effects of plasma center openings on local crime rates.

The evidence we present in this work suggests a clear causal impact of plasma centers on crime – once a plasma center opens we document statistically significant drops in total crime, driven primarily by declines in property crime reductions. That it is property crime – the crime category most closely linked with a financial incentive – suggests that the income-enhancing potential of

the presence of local plasma center has a primarily financial mechanism. We provide additional evidence, documenting that the aggregate effects we find are driven primarily by plasma centers operating in less affluent areas. This study does not decompose the crime reduction into (i) a decline in property crimes committed by those already engaging in crime (the intensive margin) and (ii) an extensive margin effect, where individuals on the brink of committing their first economically motivated crime choose to donate plasma instead. To the extent that there is an extensive margin component to our findings, coupled with the fact that engaging in crime may have some state-dependence, this suggests that for a group of younger individuals on the margin of committing their first crime, plasma centers can have lasting, positive consequences.

An unexpected yet compelling finding in our study is the strong decline in drug possession offenses following the opening of a local plasma center. While there is no explicit drug testing prior to donation, donors must complete an initial screening questionnaire detailing drug use and are checked for visible signs of any drug or alcohol use. Drug use can lead to temporary or permanent exclusion from plasma donation due to safety concerns (U.S. Food and Drug Administration, 1997). Our interpretation of these findings is that plasma donors change drug use patterns once they become donors, as a precautionary behavioral change to ensure a continuous future income stream from plasma donation.

These results suggest that plasma centers may function as a de facto conditional cash transfer (CCT) program for local donors, where the conditionality on drug abstinence operates implicitly rather than through formal enforcement. The parallels, although prominent, are imperfect: there is no explicit targeting of donors, yet survey evidence from Dooley and Gallagher (2024a) indicates that donors are disproportionately young and low income. The conditionality element of CCTs has been widely debated, with mixed evidence on its effectiveness (Attanasio et al., 2015; Benhassine et al., 2015; Bryan et al., 2023). However, in our setting, the conditionality mechanism may play a pivotal role – plasma donor payments (the equivalence of the "cash transfer" in CCTs) not only reduce property crime, but they also curb drugs-related offenses. Plasma donation regulatory safeguards, such as drug-use assessments and physical inspections, function as implicit behavioral incentives, discouraging high-risk behaviors while conditioning access to plasma donor payments.

Such an interpretation of the local role of plasma centers across US cities invites consideration of how our findings contribute to debates on universal basic income (UBI) schemes. Notably, the monthly payment of $500 in two recent UBI experiments in Stockton, California (West et al.,

2021) and Minneapolis, Minnesota (Goodman-Bacon and Palmer, 2024) is comparable to the earnings of a regular plasma donor, who can receive up to $400 per month. Our findings, at least with respect to property crime, suggest that access to regular income streams may generate positive externalities by lowering the incidence of financially motivated crime. Similarly, there is strong congruence in the property crime-reducing effect of UBI schemes, in different settings and different points in time – from Canadian province towns in the 1970s (Calnitsky and Gonalons-Pons, 2020), to Namibia in 2008 (Haarmann et al., 2009), and to Alaska in the early 2000s (Watson et al., 2020).

The work of Watson et al. (2020) documents an increase in substance-use incidents in the days following cash payments from Alaska's Permanent Fund Dividend, a universal cash transfer scheme. In contrast, our results show a decline in drug possession offenses following plasma center openings. This divergence likely reflects the conditionality attached to plasma donation, explicitly enforced through FDA regulations and donor eligibility screenings related to drug use. FDA regulations mandate drug-use history questionnaires and physical inspections; permanent deferrals apply to high-risk behaviors such as intravenous drug use; and operational safeguards, including identity verification and donor registries, ensure compliance. Although donors may perceive this conditionality only indirectly – since payments are not explicitly tied to complete abstinence – the mechanism itself *is* explicit, enforced, and operates as a genuine behavioral requirement. Designing cash transfer programs with similar safeguards in mind could help ensure that they not only provide financial support and reduce property crime but also avoid unintended increases in substance use that may accompany unconditional, lump-sum payments.

One could also view our setting as informative for new models of behavioral engagement with those released from prison under supervision – either on pre-trial release or those on parole. The current system relies heavily on intensive supervision, often including mandatory drug and alcohol monitoring (Arbour and Marchand, 2022; Georgiou, 2014; LaForest, 2022). By contrast, our study points to a "carrot"-based approach, in which conditional cash transfers – explicitly contingent on abstaining from drug use – may foster desistance from both financially motivated and drug-related crimes. This model may prove more effective than the prevailing "stick" of intensive supervision with the threat of reincarceration. In ongoing work, we are investigating the interplay of these two systems, studying how plasma centers alter recidivism patterns of those released from prison.

References

Agan, A. Y. and M. D. Makowsky (2023): "The Minimum Wage, EITC, and Criminal Recidivism," *Journal of Human Resources*, 58, 1712–1751.

Aizer, A. and J. Currie (2019): "Lead and Juvenile Delinquency: New Evidence from Linked Birth, School, and Juvenile Detention Records," *The Review of Economics and Statistics*, 101, 575–587.

Aizer, A. and J. J. Doyle (2015): "Juvenile Incarceration, Human Capital, and Future Crime: Evidence from Randomly Assigned Judges," *The Quarterly Journal of Economics*, 130, 759–804.

Alsan, M. and C. Yang (2025): "The Hidden Health Care Crisis Behind Bars: A Randomized Trial to Accredit US Jails," *National Bureau of Economic Research*, Working paper No. w33357.

Anderson, D. A. (2021): "The Aggregate Cost of Crime in the United States," *The Journal of Law and Economics*, 64, 857–885.

Ang, D. (2021): "The Effects of Police Violence on Inner-City Students," *The Quarterly Journal of Economics*, 136, 115–168.

Arbour, W. and S. Marchand (2022): *Parole, recidivism, and the role of supervised transition*, SSRN.

Ashby, M. P. J. (2018): "Studying crime and place with the Crime Open Database," .

Attanasio, O. P., V. Oppedisano, and M. Vera-Hernández (2015): "Should Cash Transfers Be Conditional? Conditionality, Preventive Care, and Health Outcomes," *American Economic Journal: Applied Economics*, 7, 35–52.

Baker, A., B. Callaway, S. Cunningham, A. Goodman-Bacon, and P. H. Sant'Anna (2025): "Difference-in-Differences Designs: A Practitioner's Guide," Working paper.

Bayer, P., R. Hjalmarsson, and D. Pozen (2009): "Building Criminal Capital behind Bars: Peer Effects in Juvenile Corrections," *The Quarterly Journal of Economics*, 124, 105–147.

Becker, G. S. (1968): "Crime and Punishment: An Economic Approach," *Journal of Political Economics*, 76, 169–217.

Bell, B., R. Costa, and S. Machin (2022): "Why Does Education Reduce Crime?" *Journal of Political Economy*, 130, 732–765.

Benhassine, N., F. Devoto, E. Duflo, P. Dupas, and V. Pouliquen (2015): "Turning a Shove into a Nudge? A "Labeled Cash Transfer" for Education," *American Economic Journal: Economic Policy*, 7, 86–125.

Bignon, V., E. Caroli, and R. Galbiati (2017): "Stealing to survive? Crime and income shocks in nineteenth century France," *The Economic Journal*, 127, 19–49.

Blandhol, C., J. Bonney, M. Mogstad, and A. Torgovitsky (2022): "When is TSLS Actually LATE?" *R&R at The Review of Economic Studies*.

Borusyak, K., X. Jaravel, and J. Spiess (2024): "Revisiting Event-Study Designs: Robust and Efficient Estimation," *The Review of Economic Studies*, rdae007.

Britto, D. G. C., P. Pinotti, and B. Sampaio (2022): "The Effect of Job Loss and Unemployment Insurance on Crime in Brazil," *Econometrica*, 90, 1393–1423.

Bryan, G., S. Chowdhury, A. M. Mobarak, M. Morten, and J. Smits (2023): "Encouragement and distortionary effects of conditional cash transfers," *Journal of Public Economics*, 228, 105004.

Callaway, B. and P. H. Sant'Anna (2021): "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, 225, 200–230.

Calnitsky, D. and P. Gonalons-Pons (2020): "The Impact of an Experimental Guaranteed Income on Crime and Violence," *Social Problems*, 68, 778–798.

Carr, J. B. and V. Koppa (2020): "Housing vouchers, income shocks and crime: evidence from a lottery," *Journal of Economic Behavior & Organization*, 177, 475–493.

Carr, J. B. and A. Packham (2019): "SNAP benefits and crime: Evidence from changing disbursement schedules," *Review of Economics and Statistics*, 101, 310–325.

——— (2021): "SNAP schedules and domestic violence," *Journal of Policy Analysis and Management*, 40, 412–452.

Cengiz, D., A. Dube, A. Lindner, and B. Zipperer (2019): "The Effect of Minimum Wages on Low-Wage Jobs," *The Quarterly Journal of Economics*, 134, 1405–1454.

Chalfin, A., S. Danagoulian, and M. Deza (2019): "More sneezing, less crime? Health shocks and the market for offenses," *Journal of Health Economics*, 68, 102230.

de Chaisemartin, C. and X. D'Haultfœuille (2020): "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects," *American Economic Review*, 110, 2964–96.

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

Dooley, J. M. and E. A. Gallagher (2024a): "Blood Money: Selling Plasma to Avoid High-Interest Loans," *The Review of Financial Studies*.

——— (2024b): "Dataset for Blood Money: Selling Plasma to Avoid High-Interest Loans," .

Draca, M., T. Koutmeridis, and S. Machin (2018): "The Changing Returns to Crime: Do Criminals Respond to Prices?" *The Review of Economic Studies*, 86, 1228–1257.

Dube, A. (2019): "Minimum Wages and the Distribution of Family Incomes," *American Economic Journal: Applied Economics*, 11, 268–304.

Ehrlich, I. (1973): "Participation in illegitimate activities: A theoretical and empirical investigation," *Journal of political Economy*, 81, 521–565.

Ergin, H., T. Sönmez, and M. U. Ünver (2017): "Dual-donor organ exchange," *Econometrica*, 85, 1645–1671.

Fella, G. and G. Gallipoli (2014): "Education and Crime over the Life Cycle," *The Review of Economic Studies*, 81, 1484–1517.

Foley, C. F. (2011): "Welfare payments and crime," *The review of Economics and Statistics*, 93, 97–112.

Gardner, J. (2022): "Two-stage differences in differences," .

Georgiou, G. (2014): "Does increased post-release supervision of criminal offenders reduce recidivism? Evidence from a statewide quasi-experiment," *International Review of Law and Economics*, 37, 221–243.

Gollu, G. and M. Zapryanova (2022): "The effect of Medicaid on recidivism: Evidence from Medicaid suspension and termination policies," *Southern Economic Journal*, 89, 326–372.

Goodman-Bacon, A. (2021a): "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 225, 254–277, themed Issue: Treatment Effect 1.

——— (2021b): "The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health, and Labor Market Outcomes," *American Economic Review*, 111, 2550–93.

Goodman-Bacon, A. and V. Palmer (2024): "How Do Low-Income Households Respond to Basic Income? Experimental Evidence from Minneapolis," Institute Working Paper 108, Federal Reserve Bank of Minneapolis, Opportunity Inclusive Growth Institute.

Grabowski, H. G. and R. L. Manning (2016): "An economic analysis of global policy proposals to prohibit compensation of blood plasma donors," *International Journal of the Economics of Business*, 23, 149–166.

Haarmann, C., D. Haarmann, H. Jauch, H. Shindondola-Mote, N. Nattrass, I. van Niekerk, and M. Samson (2009): "Making the difference! The BIG in Namibia," *Basic Income Grant Pilot Project. Windhoek: Basic Income Grant Coalition.*

Hayes, T. O. and M. Barnhorst (2020): "Incarceration and poverty in the United States," in *American Action Forum*, vol. 30.

He, Q. and S. Barkowski (2020): "The effect of health insurance on crime: Evidence from the Affordable Care Act Medicaid expansion," *Health economics*, 29, 261–277.

Hidrobo, M., A. Peterman, and L. Heise (2016): "The effect of cash, vouchers, and food transfers on intimate partner violence: evidence from a randomized experiment in Northern Ecuador," *American Economic Journal: Applied Economics*, 8, 284–303.

Hjalmarsson, R., S. Machin, and P. Pinotti (2024): "Chapter 9 - Crime and the labor market," Elsevier, vol. 5 of *Handbook of Labor Economics*, 679–759.

International Quality Plasma Program (2020): "International Quality Plasma Program (IQPP) Community-Based Donor Standard," https://www.donatingplasma.org/images/IQPP_Standards/IQPP_Community-based_Donor_Standard_V5.pdf; Accessed: 2025-04-29.

Jacob, B. A., M. Kapustin, and J. Ludwig (2015): "The impact of housing assistance on child outcomes: Evidence from a randomized housing lottery," *The Quarterly Journal of Economics*, 130, 465–506.

James, R. C. and C. A. Mustard (2004): "Geographic location of commercial plasma donation clinics in the United States, 1980–1995," *American Journal of Public Health*, 94, 1224–1229.

Kearney, M. S., B. H. Harris, E. Jácome, and L. Parker (2014): "Ten economic facts about crime and incarceration in the United States," .

Kominers, S. D., P. A. Pathak, T. Sönmez, and M. U. Ünver (2020): "Paying it backward and forward: Expanding access to convalescent plasma therapy through market design," *National Bureau of Economic Research*, Working paper No. w27143.

Lacetera, N. (2016): "Incentives and ethics in the economics of body parts," *National Bureau of Economic Research*, Working paper No. w22673.

Lacetera, N., M. Macis, and R. Slonim (2013): "Economic rewards to motivate blood donations," *Science*, 340, 927–928.

LaForest, M. (2022): "Parole supervision at the margins," in *2022 APPAM Fall Research Conference. APPAM.*

Linden, L. and J. E. Rockoff (2008): "Estimates of the Impact of Crime Risk on Property Values from Megan's Laws," *American Economic Review*, 98, 1103–27.

Lochner, L. and E. Moretti (2004): "The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports," *American Economic Review*, 94, 155–189.

Ludwig, J. and K. Schnepel (2025): "Does nothing stop a bullet like a job? the effects of income on crime," *Annual Review of Criminology*, 8, 269–289.

Manson, S., J. Schroeder, D. Van Riper, K. Knowles, T. Kugler, F. Roberts, and S. Ruggles (2024): "IPUMS National Historical Geographic Information System: Version 19.0," .

Miller, J. D. and D. R. Lynam (2006): "Reactive and proactive aggression: Similarities and differences," *Personality and Individual Differences*, 41, 1469–1480.

Miller, S., E. Rhodes, A. W. Bartik, D. E. Broockman, P. K. Krause, and E. Vivalt (2024): "Does income affect health? Evidence from a randomized controlled trial of a guaranteed income," Tech. rep., National Bureau of Economic Research.

Miller, T. R., M. A. Cohen, D. I. Swedler, B. Ali, and D. V. Hendrie (2021): "Incidence and Costs of Personal and Property Crimes in the USA, 2017," *Journal of Benefit-Cost Analysis*, 12, 24–54.

Ochoa, A., H. L. Shaefer, and A. Grogan-Kaylor (2021): "The interlinkage between blood plasma donation and poverty in the United States," *J. Soc. & Soc. Welfare*, 48, 56.

Pant, S., R. Bagha, and S. McGill (2021): "International plasma collection practices: project report," *Canadian Journal of Health Technologies*, 1.

Reyes, J. W. (2007): "Environmental policy as social policy? The impact of childhood lead exposure on crime," *The BE Journal of Economic Analysis & Policy*, 7.

Roth, J., P. H. Sant'Anna, A. Bilinski, and J. Poe (2023): "What's trending in difference-in-differences? A synthesis of the recent econometrics literature," *Journal of Econometrics*, 235, 2218–2244.

Sun, L. and S. Abraham (2021): "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of econometrics*, 225, 175–199.

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

U.S. Food and Drug Administration (1997): "Guide to Inspections of Source Plasma Establishments," https://www.fda.gov/inspections-compliance-enforcement-and-criminal-investigations/inspection-guides/section-2. Accessed: 2025-04-28.

——— (2001): "Guide to Inspections of Source Plasma Establishments - Section 2," Editorial revisions to original June 1997 document.

——— (2025): "Electronic Code of Federal Regulations (eCFR) - Title 21, Section 630.10," https://www.ecfr.gov/current/title-21/chapter-I/subchapter-F/part-630/subpart-B/section-630.10; Accessed: 2025-04-28.

Vivalt, E., E. Rhodes, A. W. Bartik, D. E. Broockman, and S. Miller (2024): "The employment effects of a guaranteed income: Experimental evidence from two US states," Tech. rep., National Bureau of Economic Research.

Vogler, J. (2020): "Access to healthcare and criminal behavior: Evidence from the ACA Medicaid expansions," *Journal of Policy Analysis and Management*, 39, 1166–1213.

Watson, B., M. Guettabi, and M. Reimer (2020): "Universal cash and crime," *Review of Economics and Statistics*, 102, 678–689.

Wen, H., J. M. Hockenberry, and J. R. Cummings (2017): "The effect of Medicaid expansion on crime reduction: Evidence from HIFA-waiver expansions," *Journal of Public Economics*, 154, 67–94.

West, S., A. C. Baker, S. Samra, and E. Coltrera (2021): "Preliminary analysis: SEED's first year," *Stockton, CA: Stockton Economic Empowerment Demonstration*, 500.

Wooldridge, J. M. (2021): "Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators," *Available at SSRN 3906345*.

Wright, R., E. Tekin, V. Topalli, C. McClellan, T. Dickinson, and R. Rosenfeld (2017): "Less cash, less crime: Evidence from the electronic benefit transfer program," *The Journal of Law and Economics*, 60, 361–383.

Appendix

A Additional Results

A.1 The Impact of Plasma Centers on Property and Drugs Crime Sub-Categories

Table A1: The Impact of Blood Plasma Centers on City-Level Property Crime Rates

	(1)	(2)	(3)	(4)
	Theft	Car Theft	Fraud	Other Property Crime
Plasma Center in City	–30.9***	–4.05**	–4.33	–8.99*
	(9.71)	(1.63)	(3.81)	(5.19)
Proportion of Property Crime	[0.611]	[0.032]	[0.127]	[0.230]
$\overline{Y}_{NT}$	230	11.7	47.9	87.4
Plasma Center/ $\overline{Y}_{NT}$	-.134***	-.345**	-.0905	-.103*
	(.0421)	(.139)	(.0796)	(.0595)
Number Treated Cities	53	53	53	53
Number Cities	1,185	1,185	1,185	1,185
Observations	198,744	198,744	198,744	198,744
Adjusted R^2	.665	.335	.425	.524

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the city level. The outcome variables crime rates – monthly counts of city-level crime per 100,000 city-level population. Staggered DD estimates for Equation (7) for the effect of having ever opened a plasma donation clinic on city crime rates per 100,000 population. The estimates are obtained by including city and state-by-month-by-year fixed effects. We present both raw and scaled DD estimates – the latter are scaled by $\overline{Y}_{NT}$ – the mean crime rate in never-treated cities, which allows for a proportional interpretation. Applying the decomposition of Goodman-Bacon (2021a) to a simplified setting of city and year fixed effects, we find clean comparisons comprise 98.0% of the weight used to form our baseline DD estimates. The estimating sample is defined as the cities matched between the NIBRS data and plasma clinic data for which the local police agencies report in all periods from January 2006 and ending in December 2019. Cities which have opened a plasma donation clinic prior to January 2006 are excluded from the sample.

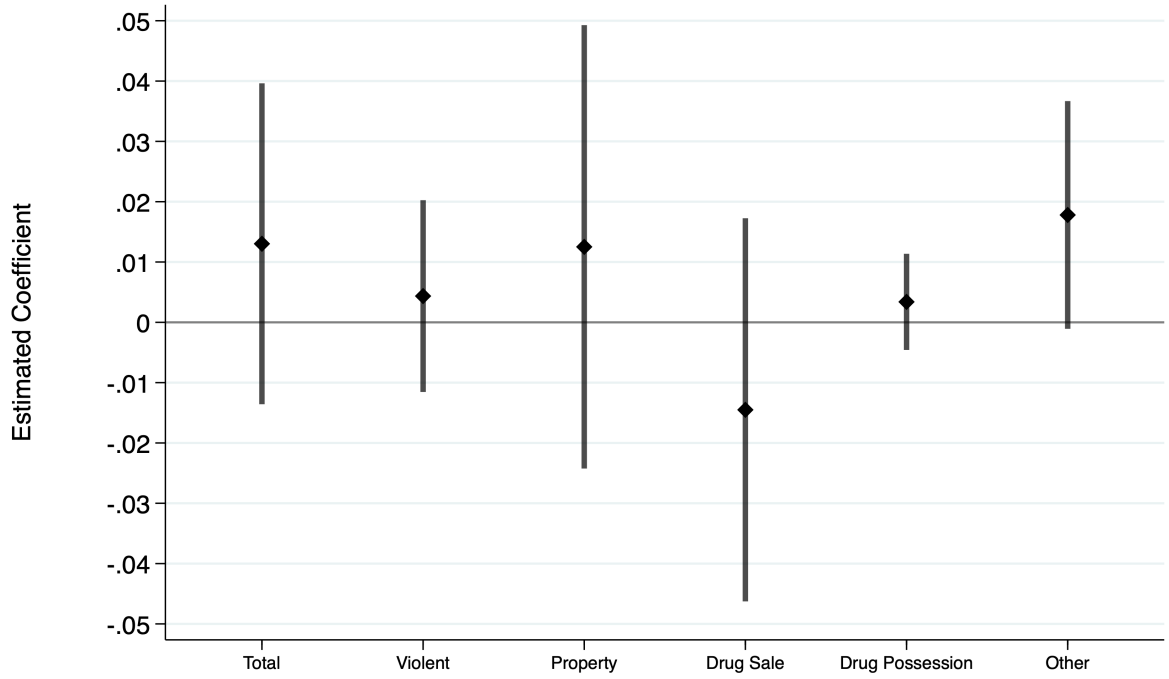
Table A2: The Impact of Blood Plasma Centers on City-Level Property Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Cocaine Possession	Heroin Possession	Marijuana Possession	Methamphetamine Possession	Other Possession	Possession, Drug Unknown
Plasma Center in City	-.6	-.724	–9.72**	–4.67**	.862	–2.19
	(1.25)	(1.52)	(4.5)	(2.3)	(1.66)	(1.65)
Proportion of Drug Possession Crime	[0.053]	[0.039]	[0.546]	[0.100]	[0.106]	[0.156]
$\overline{Y}_{NT}$	6.2	4.66	65.4	11.9	12.8	18.4
Plasma Center/ $\overline{Y}_{NT}$	-.0968	-.155	-.149**	-.392**	.0672	-.119
	(.202)	(.327)	(.0687)	(.193)	(.13)	(.09)
Number Treated Cities	53	53	53	53	53	53
Number Cities	1,185	1,185	1,185	1,185	1,185	1,185
Observations	198,744	198,744	198,744	198,744	198,744	198,744
Adjusted R^2	.166	.187	.408	.254	.338	.275

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the city level. The outcome variables crime rates – monthly counts of city-level crime per 100,000 city-level population. Staggered DD estimates for Equation (7) for the effect of having ever opened a plasma donation clinic on city crime rates per 100,000 population. The estimates are obtained by including city and state-by-month-by-year fixed effects. We present both raw and scaled DD estimates – the latter are scaled by $\overline{Y}_{NT}$ – the mean crime rate in never-treated cities, which allows for a proportional interpretation. Applying the decomposition of Goodman-Bacon (2021a) to a simplified setting of city and year fixed effects, we find clean comparisons comprise 98.0% of the weight used to form our baseline DD estimates. The estimating sample is defined as the cities matched between the NIBRS data and plasma clinic data for which the local police agencies report in all periods from January 2006 and ending in December 2019. Cities which have opened a plasma donation clinic prior to January 2006 are excluded from the sample.

A.2 Effects of Plasma Centers on Reporting Rates of Offender Ages

Figure A1: Effect of Plasma Center Openings on the Share of Offenses that Include the Offender's Age



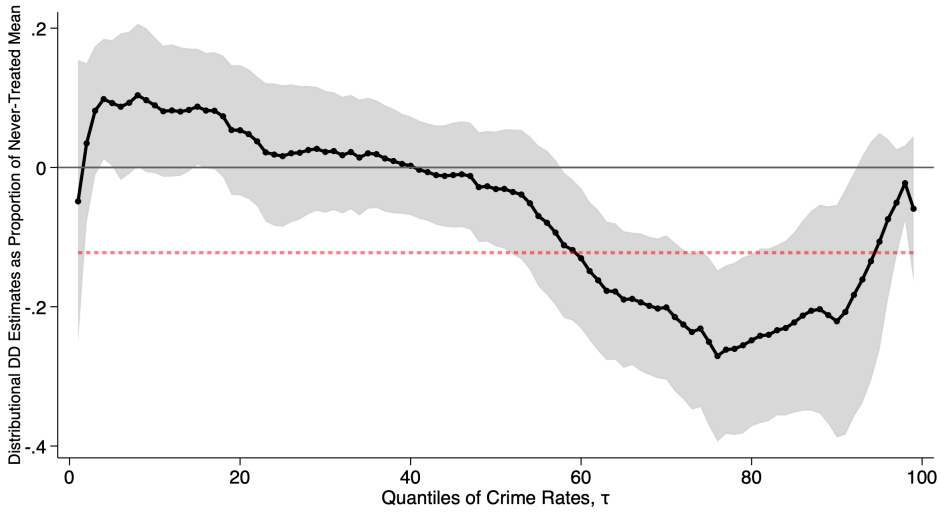
Notes: We present point estimates and 95% confidence intervals for Equation (7) for which the outcome is the share of offenses that report the age of the offender. Estimates are made for all crimes and individual crime categories. Sample period: January 2006-December 2019. Data source: NIBRS.

A.3 Distributional Effects of Plasma Centers on Crime – UQPE Results

The UQPE approach to measuring the distributional effects of plasma centers on crime takes a very similar form to those that we document in Section 5.4. In this case we estimate a DD regression with $y_{cst} = \mathbb{1}[\text{crime}_{cst} < Q_\tau]$. We rescale the resulting quantile-specific DD estimates by the minus one times the density of crime at the τ-th quantile, $f^{NT}(Q_\tau)$, where once again we use the distribution of crime rates in never-treated cities. This gives us a local linear approximation of the UQPE at a given quantile.

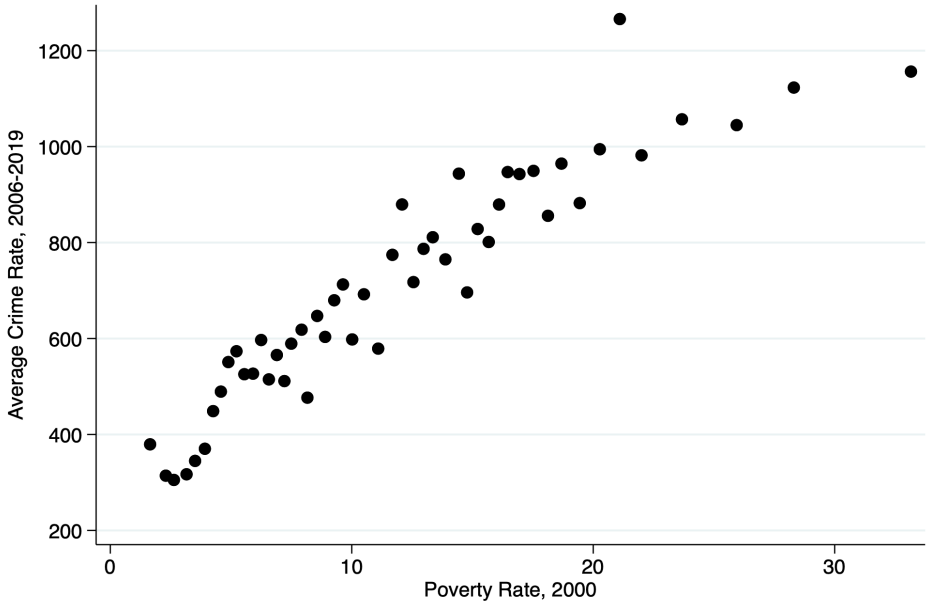
A caveat to the UQPE approximation is that, as noted by Dube (2019), the local linear approximation is best suited for cases where the treatment is continuous and has substantial variation in treatment intensity, and is less well suited for discrete treatments as in our case. We present the results UQPE results in Appendix Figure A2 nonetheless, as the scaling of such estimates allow us a better comparison with our core DD results in proportional form, i.e., Plasma Center/$\overline{Y}_{NT}$ in Table 2. We additionally rescale the estimates by dividing by the quanitle-specific cutoff, $c(\tau) = Q_\tau$ for never-treat cities, in order to facilitate a proportional representation.

Figure A2: Distributional Effects of Plasma Centers on Crime: UQPE



Notes: We present point estimates and 90% confidence intervals for a the impact of plasma centers on crime rates from a series of distributional DD regressions. The estimates come from a set of regressions where the outcome is $y_{cst} = \mathbb{1}[\text{crime}_{cst} > Q_\tau]$ for $\tau = [1, \ldots, 99]$. We apply to scaling factors to the estimates. The first is $1/-f^{NT}(Q_\tau)$. The second is $1/Q_\tau$. This gives the estimates a proportional UQPE representation. The red dotted line in the graph is the baseline (mean) DD estimate, scaled by the mean of crime for never-treated cities, and serves as a reference point for the UQPE estimates.

Figure A3: The Crime-Poverty Nexus



Notes: This binscatter plot graphs average city-level crime rates against poverty rates, as measured at the 2000 Census.

A.4 Placebo DD Estimates for CODE sample

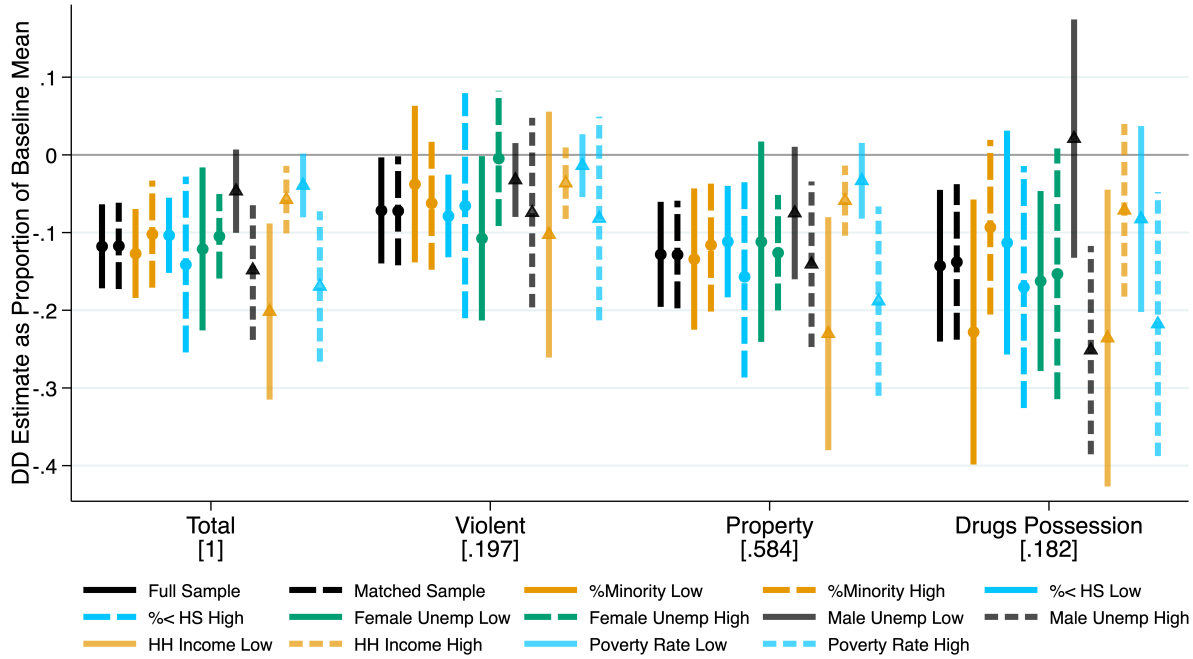
Table A3: The Impact of a Placebo Opening of a Nearby Blood Plasma Center on Block-Group Crime Counts

	(1)	(2)	(3)	(4)	(5)
	Total Crime	Violent Crime	Property Crime	Drug Crime	Other Crime
Near Plasma Center × Placebo Open	.167	.0437	.0206	.0468	.056
	(.201)	(.0596)	(.106)	(.0333)	(.0626)
Proportion of Total Crime	[1.000]	[0.246]	[0.555]	[0.063]	[0.137]
$\overline{Y}_{NT,pre}$	11.5	2.83	6.24	.78	1.61
DD/ $\overline{Y}_{NT,pre}$	.0146	.0154	.00331	.06	.0348
	(.0175)	(.021)	(.017)	(.0427)	(.0389)
Observations	64,440	64,440	64,440	64,440	64,440
Adjusted R^2	.935	.82	.891	.515	.923

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the city level. The outcome variables here at crime counts at the census block-group by month level. The coefficient of interest here relates to our placebo DD term, Near Plasma Center × Placebo Open. Near Plasma Center is defined as the crime-weighted centroid of the block-group being located within 3km radius of a plasma center. Open takes the value 0 in the 13-24 months prior to an opening, and 1 for the 12 proceeding months, all of which are prior to the opening date. The estimates are obtained by including block-group and city-by-month-by-year fixed effects. We present both raw and scaled DD estimates – the latter are scaled by $\overline{Y}_{NT,pre}$ – the mean crime count in non-treated block-groups in the 13-24 months prior to a plasma center opening, which allows for a proportional interpretation.

A.5 Heterogeneous Effects – Extended Results

Figure A4: Heterogeneity Analysis Using Predetermined City Characteristics



Notes: We present point estimates and 90% confidence intervals for our DD estimates from a variety of different subsamples, as listed. In square brackets under each category label, we present the proportion of total crime accounted for by each crime category. Sample period: January 2006-December 2019. Data source: NIBRS.