Financial Incentives and Public Safety: The Role of Blood Plasma Donation Centers in Crime Reduction

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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. The crime-reducing effects of plasma donation income are particularly pronounced in less affluent areas, underscoring the financial channel as the primary mechanism behind these results. This study further posits that the perceived severity of plasma center sanctions against substance use, combined with the financial channel, significantly contributes to the observed decline in drug possession incidents.

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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, as are typical enrollees to recent UBI schemes in the 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 regulatory environment in which plasma centers operate. Donors are subject to drug screening and informal monitoring during each session to ensure quality control of the resulting blood plasma. This adds a layer

[1] Two recent schemes paid participants $500 per month (Stockton Economic Empowerment Demonstration, West et al., 2021) and $1,000 per month (The OpenResearch Unconditional Income Study, Miller et al., 2024; Vivalt et al., 2024).

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 work, and relevant to the broader discussion of optimal design of cash transfer schemes.

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 operationalize 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.

We conclude the paper by investigating potential mechanisms that likely mediate the causal impact of plasma centers on crime outcomes. We do so by assessing treatment effect heterogeneity. We consider how our baseline DD estimates vary with key economic and sociodemographic variables. We measure these prior to the start of our sample period to avoid these variables being contaminated 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.

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 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.

In the final empirical section, we explore the mechanisms that drive our key findings. By incorporating a battery of economic and sociodemographic variables from the Census measured prior to our sample period begins, 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 alter their substance use behavior in response to drug-related regulatory requirements imposed by plasma centers. We further discuss the broader implications of these findings in the concluding section of the paper.

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)

[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.

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 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.,

[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).

2021). Plasma is essential in the production of a range of life-saving medicines and therapies, making it a highly valuable commodity in the global medical supply chain.

Plasma donation involves extracting the pale-yellow liquid component of blood that contains vital proteins, antibodies, and clotting factors. This plasma is then processed and used to produce medications for conditions such as hemophilia, immune disorders, and liver disease. The majority of plasma collected in the US comes from commercial centers that compensate donors, using a process called apheresis, which separates plasma from blood and returns the remaining components to the donor. 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. The reason for this difference lies in the apheresis process: while whole blood donations remove red blood cells, which require time to replenish, plasma donation returns all other components to the donor, allowing plasma to be naturally restored within 48 hours.

Commercial plasma centers actively recruit donors through various incentive programs, including referral bonuses, loyalty rewards, and increasing payments based on donation frequency. Compensation can vary by location and promotional campaigns, but generally, first-time or frequent donors can earn a substantial amount through repeated donations. Donors are financially compensated for their time by prepaid debit or ATM cards, typically receiving between $30 and $70 per session, for up to $400 a month (Dooley and Gallagher, 2024a).

The US Food and Drug Administration (FDA) plays a pivotal role in regulating plasma collection to protect both donors and recipients. Key regulations include mandatory testing for transfusion-transmitted infections, such as HIV, as well as rules governing donation frequency and the allowable volume of plasma donated (U.S. Food and Drug Administration, 2025b). Prospective donors undergo thorough initial screenings, including a physical examination, drug use assessment, and completion of a detailed medical and personal history questionnaire (U.S. Food and Drug Administration, 2025a). These screenings play a crucial role in evaluating donor suitability, and might result in either a temporary or permanent deferral. Temporary deferrals, often lasting 6–12 months, apply to conditions such as recent illness, medication use, incarceration lasting over 72 hours, or tattoos and piercings. Permanent deferrals are enforced for high-risk behaviors like intravenous drug use, chronic illnesses, or infectious diseases such as HIV or hepatitis. Individuals who appear under the influence of drugs or alcohol or who may be unable to provide an accurate medical history might be temporarily or permanently banned from donating 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 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).[6] 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

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

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 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. [7]

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. WWe 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.

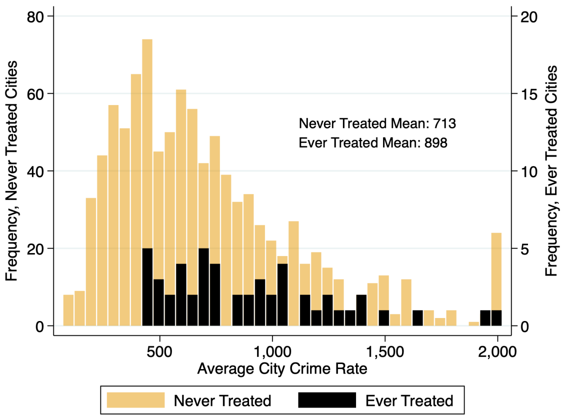
Due to the sample selection restrictions we impose on our NIBRS sample–ensuring the inclusion of only agencies with consistently high-quality reporting–our final working sample includes 1,190 cities, 54 of which are ever-treated. While the ever-treated have higher crime rates prior to experiencing a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

We next 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.

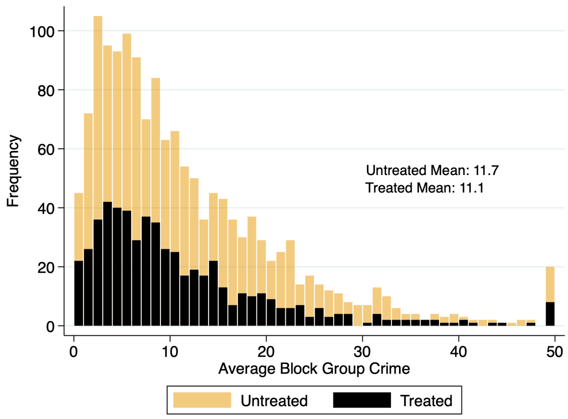
We complement the map of plasma center locations with information on the type of area in which plasma centers are located. We consider two spatial levels: the city level, which affords a

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

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.

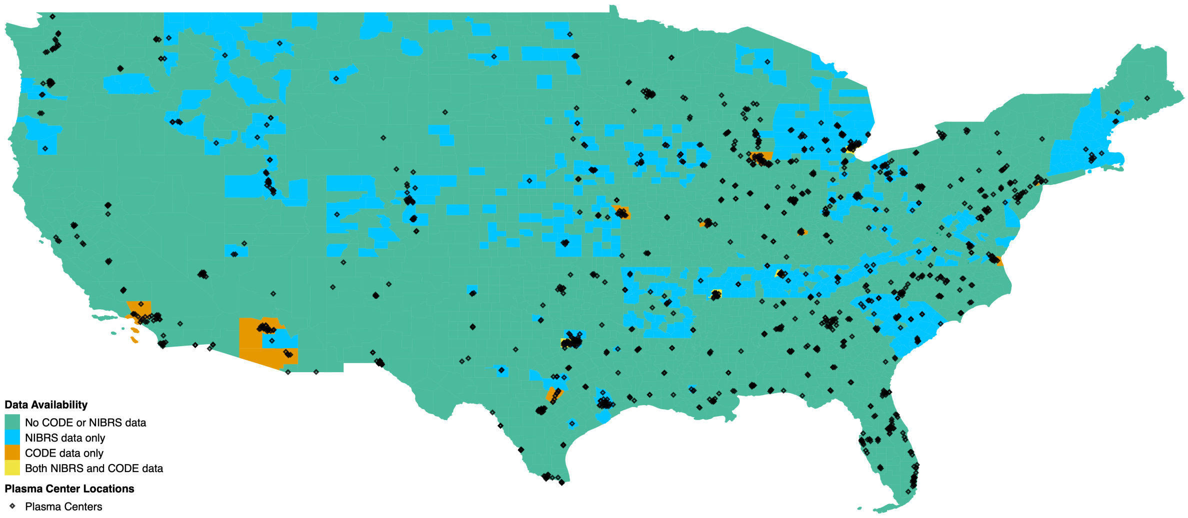
cross-city comparison, and the block group level, which enables a within-city comparison. Key area characteristics are presented in Table 1, with all metrics reported in percentile rank form on a 0–100 scale to ensure straightforward comparisons across characteristics.

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.[8] 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).[9] 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).

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

[9] 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.

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.

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 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[10] 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.

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

[10]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.

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

Notes: All location charactertistics 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.

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.[11] 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.

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.[12]

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:

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

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

[12]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.

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 differential 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.[13] 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, 2025a). 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 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 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.

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

5.2 Are TWFE Estimates Valid in Our Setting?

In settings with staggered treatment adoption, TWFE estimates a weighted sum of treatment effects.[14] However, when treatment effects are heterogeneous, these weighted sums are not easily interpretable and may not even represent a causal parameter.[15] 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.

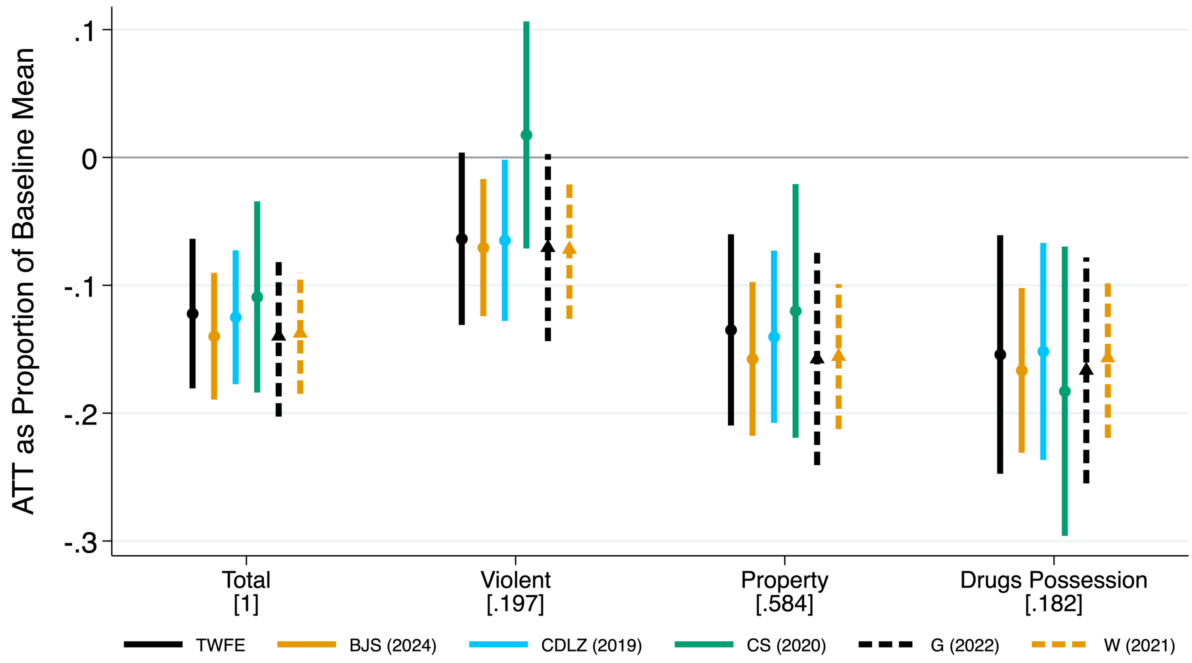
Given the temporally staggered nature of plasma centers across cities, concerns may arise regarding the use of TWFE estimates in this context. To address these concerns, we provide two pieces of evidence. First, we implement the Goodman-Bacon (2021a) decomposition for a simplified version of our baseline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

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

[15] 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.

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.

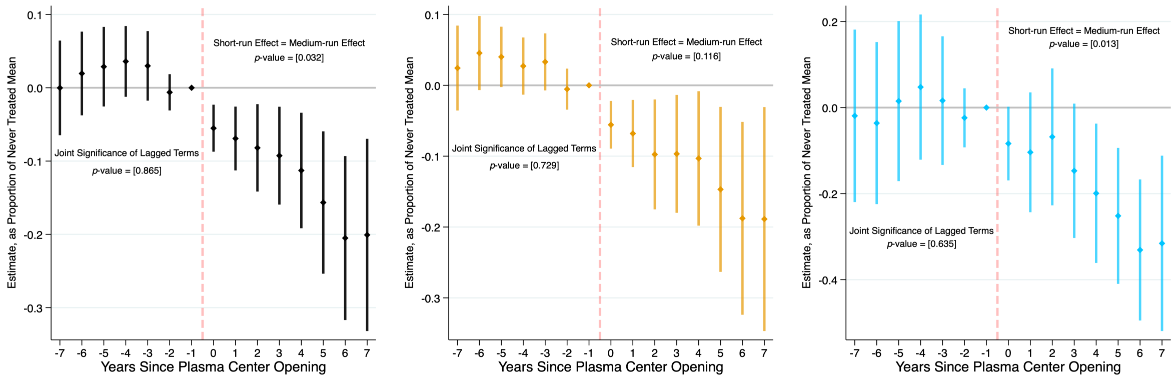
5.3 Dynamic Effects

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 A1 we present the estimates in unconditional quantile partial effect (UQPE) form.[16]

To operationalize 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.[17]

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

[16] We discuss the pros and cons of this approach for our setting in Appendix Section A.2.
[17] An example of this approach can be seen in Goodman-Bacon (2021b).

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.

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.

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 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?

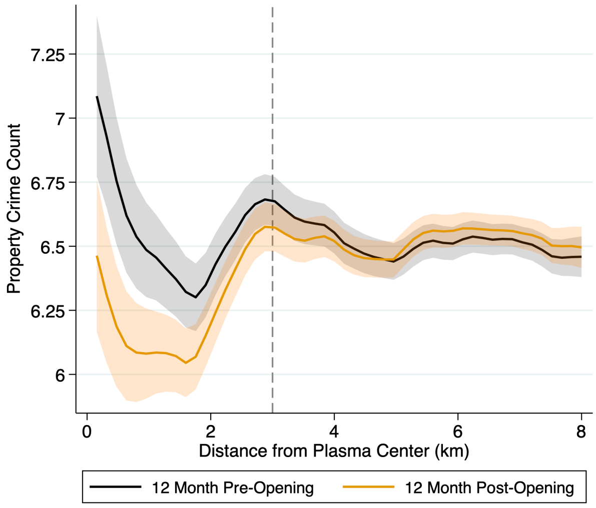
6.1 Identification

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?

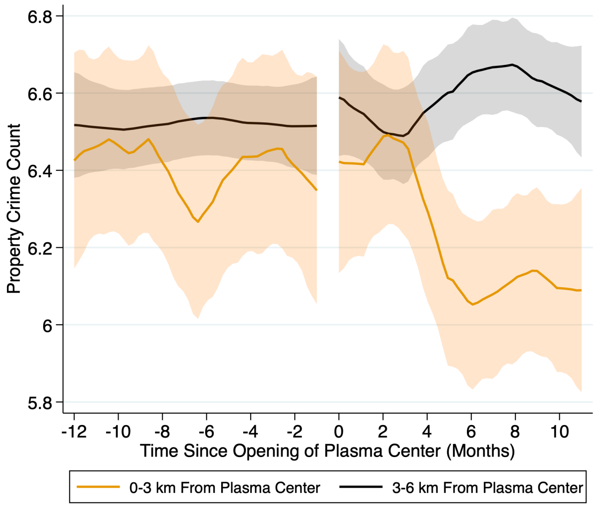
In our setting, we consider property crime as the key outcome, given the results from the previous section. We residualize crime by city-month-year and census tract fixed effects to account for city-specific time effects in crime as well as broad spatial patterns in crime.[18] We present the resulting 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 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.

[18] 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.

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.

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

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

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.

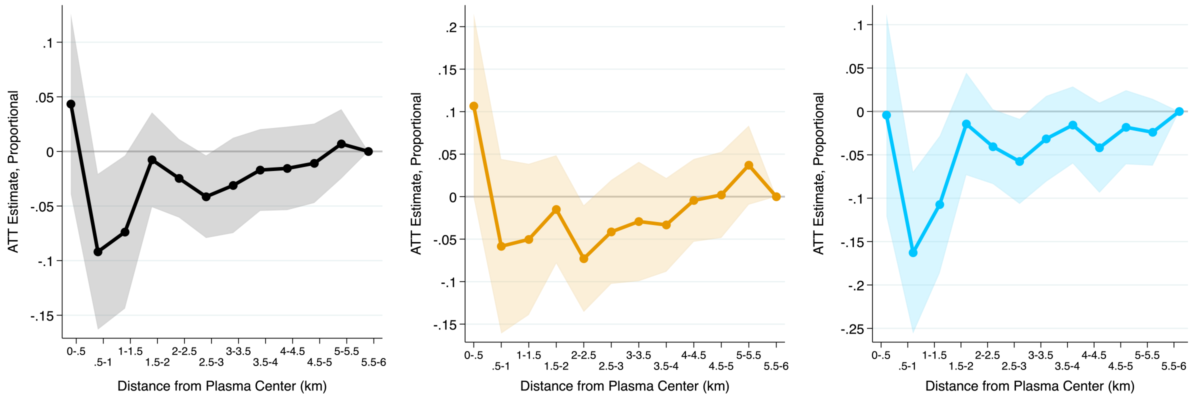
Table 3: The Impact of the Opening of a Nearby Blood Plasma Center on Block-Group Crime Counts

	(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.

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 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

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

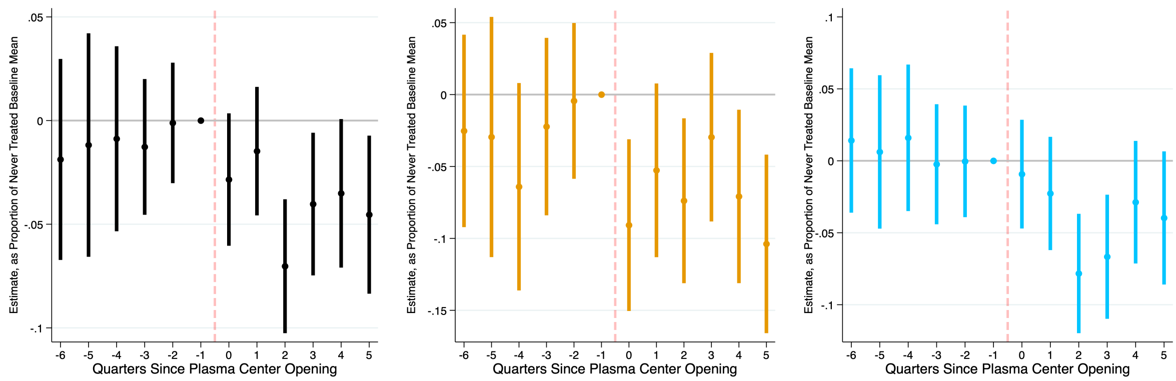


(a) Total Crime (b) Violent Crime (c) Property Crime

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.

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 underlying mechanisms driving the results, 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 test the predictions that arise from our model.

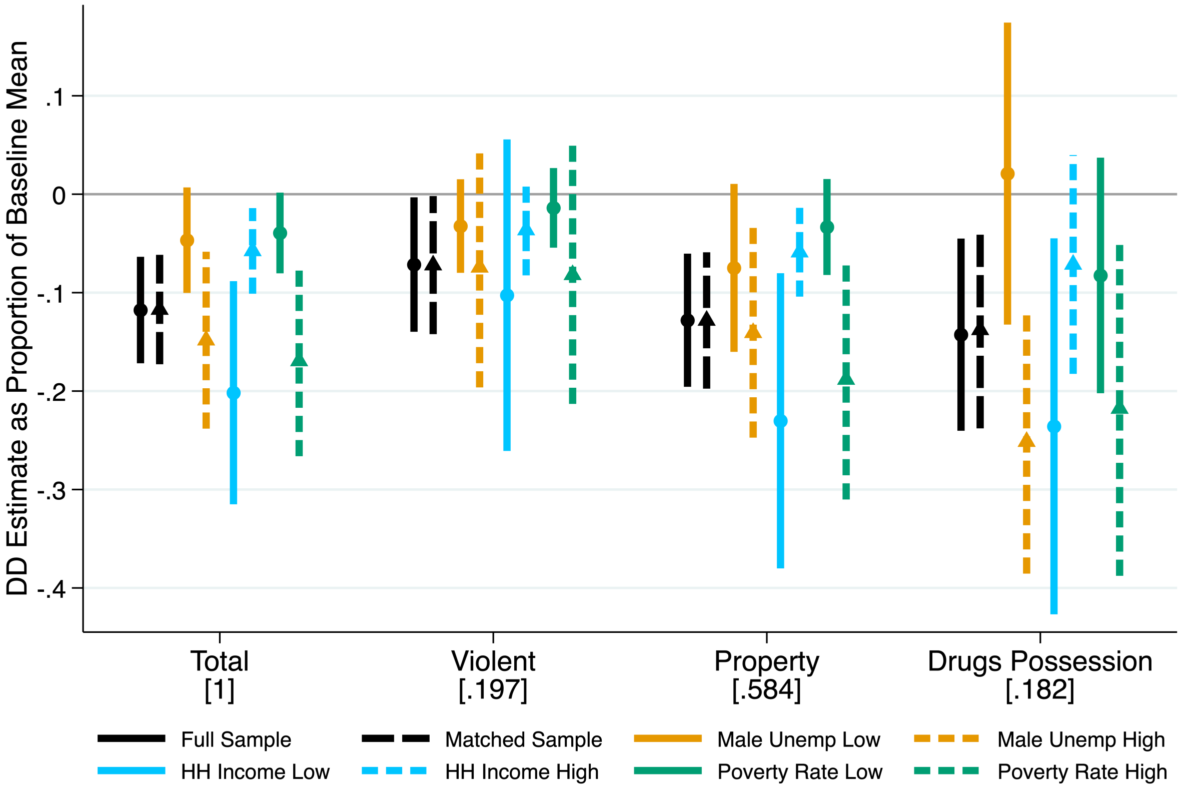
7.1 Treatment Effect Heterogeneity

We start by matching our NIBRS cities to census information from 2000 via census places names.[19] City/place limit definitions appear to differ in the two datasets, as do naming con-

[19]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.

ventions, 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, 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.[20]

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

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

wealthier areas, its proportional impact is substantially lower.

8 Discussion and Concluding Remarks

Plasma donation centers have become a ubiquitous feature of the urban landscape in the US, operating across 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, then 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 not explicit drugs 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 they can 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 the 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, although the work of Dooley and Gallagher (2024a) suggests 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 appears to be central – not only do plasma donor payments (the equivalence of the "cash transfer" element of CCTs) reduce property crime, but they also reduce drugs-related offenses, through an implicit behavioral incentive.

Such an interpretation of the local role of plasma centers across US cities also allows us to consider how our work may contribute to the discussion of the merits of universal basic income (UBI) schemes. Notably, the monthly payment in a recent UBI experiment in Stockton, California (West et al., 2021) ($500) 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 in the shape of a lower 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 (Frankman, 2010), and to Alaska in the early 2000s (Watson et al., 2020).

The work of Watson et al. (2020) offers an additional finding on substance-use incidents, which spike upwards in the days after a cash payment. This finding is particularly interesting in contrast to our own results, given that we observe a decline in drug possession offenses following plasma center openings. This contrast underscores the importance of incorporating conditionality into cash transfers – whether implicitly perceived, as in our setting, or formally structured, as could be done by modifying UBI schemes. Such modifications could ensure that cash transfer programs not only provide financial support to households and reduce property crime but also mitigate the unintended consequence of increased substance use following 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). What we learn from our study of plasma centers is a different model (a “carrot”-based approach) encompassing conditional cash transfers – where the conditionality may be linked to abstaining from drug use – may lead to desistance from both financially motivated and drugs crime in ways that are more effective than the current “stick” of high levels of supervision with the threat of return to prison. In ongoing work, we are investigating the interplay of these two systems, studying how plasma centers alter recidivism patterns of those released from prison.

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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

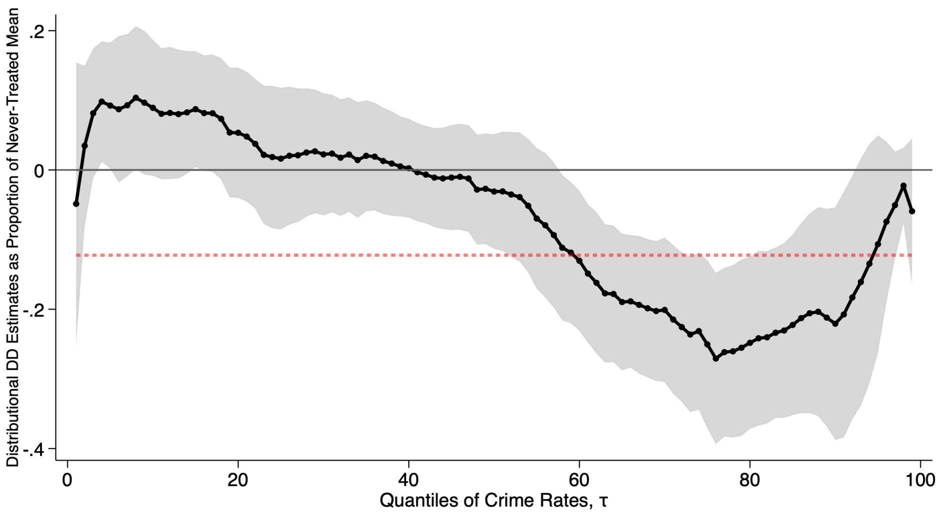
Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Standard errors are clustered at the city level. The outcome variables crime rates – monthly counts of city-level crime per 100,000 city-level population. Staggered DD estimates for Equation (7) for the effect of having ever opened a plasma 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 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 A1 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.

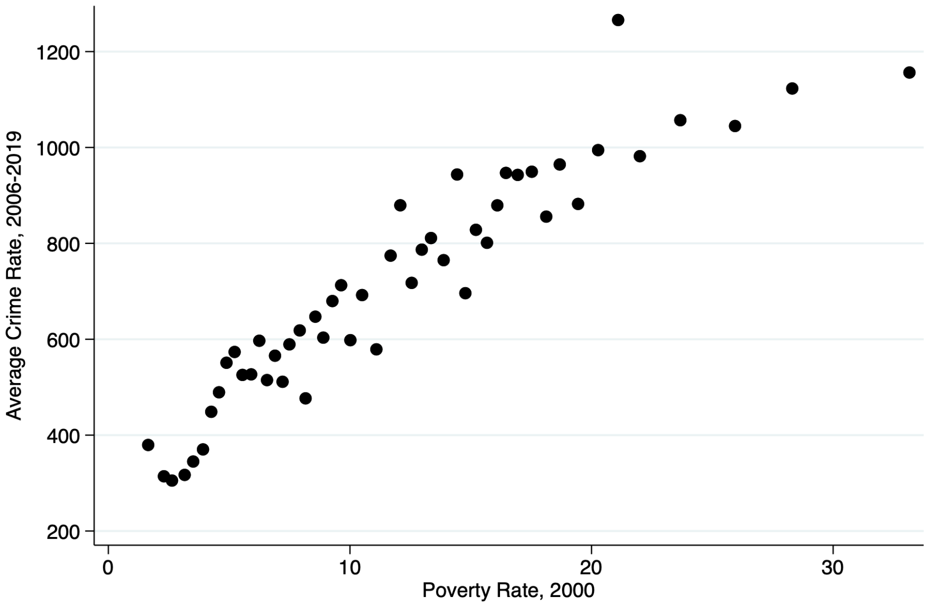
Figure A1: Data Availability and the Location of Plasma Centers



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 A2: The Crime-Poverty Nexus

Figure A3: Data Availability and the Location of Plasma Centers



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

A.3 Placebo DD Estimates for CODE sample

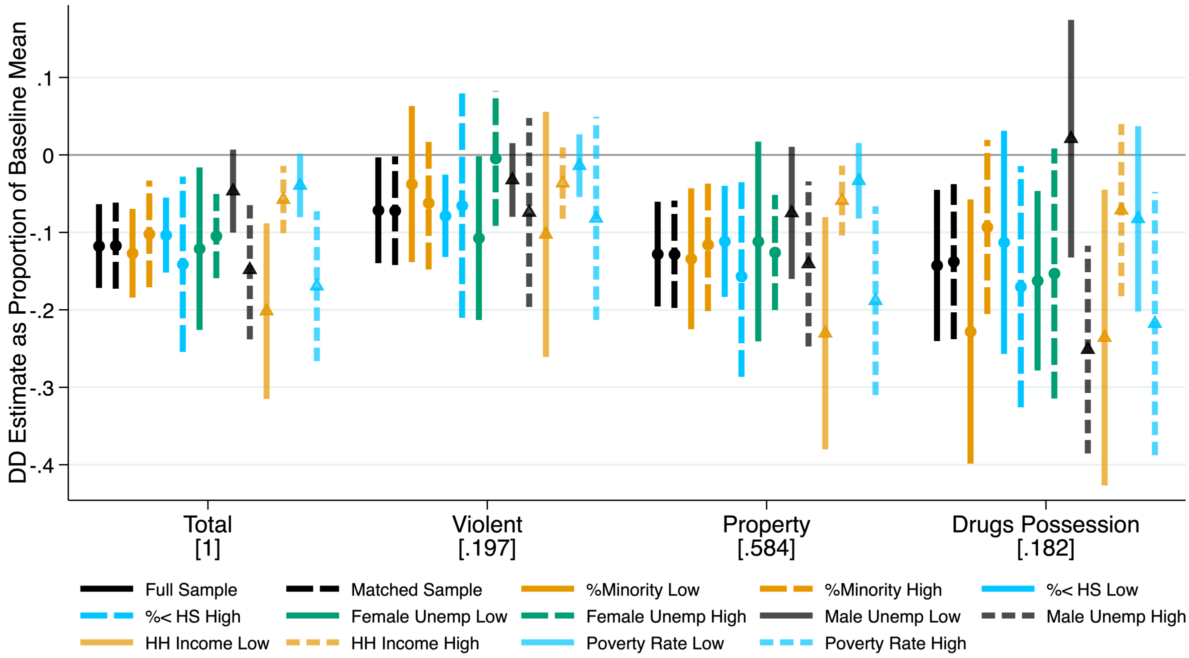
Table A3: The Impact of a Placebo Opening of a Nearby Blood Plasma Center on Block-Group Crime Counts

	(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.4 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.