The Price of Corrupt Justice:

Evidence from Progressive Era Chicago

Roman Rivera*

McCourt School of Public Policy

Georgetown University

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Abstract

Does judicial independence have spillover effects? I study the 1905 Municipal Court Act (MCA) in Chicago, which replaced patronage-selected justices with elected, salaried judges. Using newly assembled archival data, I show that the MCA professionalized the courts without directly affecting police compensation or staffing. Nevertheless, arrests declined by over 30%, driven by low-level misdemeanor offenses. Precinct-level data reveal this decline was concentrated where police and justices had been known to collude in extractive arrest schemes. I find no increase in property crime and suggestive evidence that murder declined, indicating the MCA significantly improved police performance.

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1 Introduction

In 2015, the US Department of Justice revealed extensive collusion between the municipal court and police of Ferguson, Missouri, resulting in revenue extraction from residents using fines, fees, and tickets (DOJ (2015)). While Ferguson is the most high-profile example, legal scholarship has revealed the underlying institutional features that are common across the lower courts of the United States (Natapoff (2021)). In particular, judges and local police often share social ties and political and economic incentives: they have frequent interactions; fines and fees raise local revenue; both desire to be tough-on-crime; and the same official may control both the police and the local court. Such alignment between bureaucrats of different agencies could improve efficiency (Spenkuch et al. (2023), Bellodi et al. (2025)), but it could also undermine judicial independence, which is a cornerstone of state capacity and economic growth (Besley & Persson (2011), Ferraz & Finan (2025)). Despite rising concerns and the implications for local state capacity, we have limited causal evidence on whether strengthening lower court independence has spillover effects on police behavior and public safety.

In this paper, I show that reforming lower courts can improve the performance of police officers. I study the effects of a pivotal Progressive Era legislation, the 1905 Municipal Court Act in Chicago (MCA), which inspired similar reforms across the country. The MCA established Chicago's Municipal Court system and reformed how judges were selected, and, crucially, it had no direct effect on the police. The MCA abolished the pre-existing system of police courts and 'police justices'. These justices were selected by patronage networks, paid by fines and fees, and were financially and politically tied to Chicago police officers who were embedded in the same local political networks. The new Municipal Court was run by judges who were required to be lawyers and US citizens, selected in city-wide elections, and paid fixed salaries.

To measure the effects of the MCA, I construct a dataset on policing, crime, composition, and expenditures from archival sources, digitized records, and existing datasets. The core data

were obtained by digitizing over a dozen annual police reports from Chicago and other major US cities. Because records vary across sources, I employ multiple empirical strategies: time series regressions, Manski & Pepper (2018) bounds, difference-in-differences, and synthetic difference-in-differences (Arkhangelsky et al. (2021)).

I begin by showing that the historical narrative of judicial professionalization without directly targeting the police is supported by archival city expenditure records, handwritten police officer registries, and micro-census data. After the MCA, expenditures on lower-courts in Chicago doubled, mainly due to salaries, and lawyers and judges in 1910 Chicago were drawn from higher status professions than those in other major cities. Cross-referencing contemporaneous historical documents shows that, while in 1905 many justices were Irish Democrats, after the MCA in 1907 almost all judges were non-Irish Republicans. By contrast, the expenditures per police officer and the observable features of police officers (nativity, prior occupation, age at hire) do not change significantly after the MCA, and police hiring continues along pre-MCA trends. Overall, the MCA did not have a detectable effect on total city finances.

Despite not directly targeting the police, the MCA caused a massive decline in arrests, without increases in crime or evidence of shirking. The MCA caused the number of arrests made by Chicago police officers to decline by over 30%, resulting in at least 25,000 fewer arrests per year. This effect is large, precisely estimated, and robust across specifications. The decline was driven by misdemeanor charges, which declined by over 40%, while felony arrests declined by less than 12%. In particular, charge data shows that the decline was driven by arrests for offenses which would be interpreted today as low-quality: disorderly conduct, prostitution, gambling, miscellaneous misdemeanors, and unspecified ordinance violations.

To assess whether the decline in arrests is driven by city-wide changes in civilian behavior, judicial attitudes, or police protocol, I use precinct-level data on arrests of women to determine where the decline in arrests was concentrated. Women were primarily arrested for low-level

offenses, and the majority of their arrests are from two precincts which contain and border the redlight district. Historical documents describe police and justices in the redlight district colluding in a technically legal racket: officers made large numbers of arrests, and judges and police would split fees and bonds totaling $2 - $6 per arrestee (about $66 - $196 in 2025 dollars). Records also indicate that the MCA ended this practice. Consistent with this narrative, I find arrests of women in the redlight precincts experienced an excess decline of over 25% compared with other precincts. I estimate how much Chicagoans in these precincts saved from the end of this police-justice collusion. The MCA prevented around 9,000 extortive arrests per year, saving Chicagoans at least $36,000 ($1.2 million) in 1907 and $216,000 ($7 million) between 1907 and 1912.

Finally, I estimate the effects of the MCA on property crime and murder. The decline in low-level arrests could signal officers pulling back or staying on their beats rather than being preoccupied. I do not find any evidence for an increase in property crime, and I find suggestive but imprecise evidence that murder declined due to the MCA from cross-city and cross-precinct comparisons. This suggests that the MCA did not increase crime despite a large decline in arrests.

Combined with the effects on low-level misdemeanor arrests, this indicates the MCA substantially improved police performance. In the present, police-court collusion persists across the country where political and economic ties remain strong (Natapoff (2021)) — best exemplified by the case of Ferguson, Missouri (DOJ (2015)) — and there are growing concerns over the scale of low-level enforcement (Stevenson & Mayson (2018)). The effectiveness of the MCA suggests that severing such ties, along with more accountable judges, can generate benefits for performance and public safety.

This paper contributes to research in law and economics and political economy on how legal institutions shape state capacity and accountability. In modern settings, existing work documents how perverse incentives arise from fiscal and political links between police, courts, and local governments (Baicker & Jacobson (2007), Makowsky & Stratmann (2009), Berdejó

& Yuchtman (2013), Makowsky et al. (2019), Ouss (2020)). This paper uses newly assembled historical data to show how reforming one institution can reshape incentives for another. Complementing work on the historical influences and development of American institutions (Derenoncourt (2022), Rubio-Ramos (2022), Aneja & Xu (2022), Moreira & Pérez (2024), Ang & Chinoy (2025), Cunningham & Goodman-Bacon (2025)), I show how a Progressive Era reform which ended patronage selection of local court judges generated positive spillovers by breaking the ties between police and courts.[1] These results illustrate how patronage selection can generate negative spillovers beyond specific agencies. This contributes to the literature on patronage and public-sector personnel systems, which mainly focuses on how selection affects performance within individual agencies (Folke et al. (2011), Xu (2018), Colonnelli et al. (2020), Brassiolo et al. (2020), Voth & Xu (2022), Leucht (2023), Mastrorocco & Teso (2025)), while this paper documents significant spillover effects on related agencies.

Finally, this paper contributes to work on reform bundles and accountability structures (Finan & Mazzocco (2025), Ferraz & Finan (2025)). The MCA bundled multiple changes to judicial selection and compensation by introducing city-wide elections, strengthening eligibility, and replacing fee-based compensation with salaries. Collectively, this weakened the economic and political incentives that had linked judges and police. While a large literature documents how vertical accountability (e.g., elections) influences bureaucratic behavior, this paper shows how increased accountability for one institution results in horizontal accountability for a related institution through mis-aligned ideologies and incentives. Prior work on horizontal accountability often focuses on how audits and anti-corruption monitoring affect performance and economic development (Ferraz & Finan (2008), Bobonis et al. (2016), Avis et al. (2018), Chen & Kung (2019), Colonnelli & Prem (2022), Bobonis et al. (2023)), while Bellodi et al. (2025) shows that ideological misalignment between politicians and bureaucrats can

[1]One mechanism is the end of a pay-per-arrest scheme, which was increasing output (arrests) but reducing the quality of output and overall performance. This is consistent with prior work on performance pay (Muralidharan & Sundararaman (2011), Ashraf et al. (2014), Khan et al. (2016), Leaver et al. (2021), Dal Bó et al. (2021)) and issues generated by incentivizing multi-tasking agents with complex tasks or opportunities for corruption (Becker & Stigler (1974), Kerr (1975), Holmstrom & Milgrom (1991), Finan et al. (2017), Besley et al. (2022))

reduce efficiency. However, I find that ideological misalignment between bureaucrats can help prevent collusion. Severing the connections between police and courts and introducing intrinsically motivated judges were key to improving police performance.[2] This mechanism relates to that of Vannutelli (2022), who shows that switching from politically-appointed auditors to randomly assigned ones in Italy improves outcomes due to severed connections between mayors and auditors.

2 Historical Context

2.1 Pre-Reform Courts and Police

Prior to the establishment of the Municipal Court in 1906, Chicago's criminal justice system operated through a patchwork system of semi-autonomous 'justice shops' administered by justices of the peace (Willrich (2003)). In this system, which was not unusual at the time, justices handled civil and criminal cases, had little to no oversight, and were not required to have any legal training. For their services, justices extracted fines and fees from the individuals brought before them. Contemporaneous accounts note that justice shops could be immensely profitable for their untrained justices, who largely owed their positions to 'pull' rather than qualifications (Tribune (1904), Tribune (1906)). Most relevant to this paper, police justices specialized in criminal matters, and they were appointed by the mayor, with the City Council's consent. As a result, police justices were individuals with local political influence within their wards and were strongly tied to local political powers (Haller (1976), Willrich (2003)).

The Chicago Police Department (CPD), like many departments of its day, was also under

[2]This is consistent with research on intrinsic motivation and horizontal accountability (Prendergast (2007), O'Donnell (1998)). The influence of vertical accountability through elections, term limits, and competition is well studied (Maskin & Tirole (2004), Ferraz & Finan (2008), Bardhan & Mookherjee (2010), Besley et al. (2010), Ferraz & Finan (2011), Dal Bó & Rossi (2011), Acemoglu et al. (2014)). This also intersects with the literature on judicial reforms, primarily in developing countries, which emphasize the importance of judicial independence and efficiency (Chemin (2009), Aney et al. (2021), Mehmood (2022), Liu et al. (2022)).

heavy influence by the local political leaders (Haller (1976), Leucht (2023)). Even after civil service reforms in 1895, Chicago police officers were still embroiled in partisan politics and served as political agents for local party bosses (Lindberg (1998)). Officers were formally compensated with salaries around $1,000 a year, which was a sizable sum at the time (Haller (1976)) — similar to that of a physician or engineer, and more than two times that of other comparable work such as a bartender, sailor, or soldier (Willets (1903)). They were expected to remit some of their salary to the local political forces and dominant party, of which they were integral components (Haller (1976)).

Officers would bring arrests before police justices. In practice, arrests were processed en masse without much interest in legal guilt or innocence and few defense lawyers (Haller (1976)). Fines were a common outcome, and a small percentage went to a superior criminal court or grand jury, which were handled by salaried judges of the county or superior courts (Willrich (2003)).

Demographically and politically, the police and justices were both drawn from the local immigrant or immigrant-adjacent and Democratically-aligned populations which were a part of the local political machines.[3] By the late 1880s, most officers were first- or second-generation immigrants, primarily Irish or German, and officers were closely tied to their immigrant and Democrat-aligned neighborhoods (Mitrani (2013)), and many police justices were also drawn from similar populations. For example, the 1906 Lakeside Annual Directory of the City of Chicago (Company (1906)) lists 24 police justices serving in North, South, and West Chicago. Cross-referencing those with the 1905 Book of Chicagoans (Leonard & Marquis (1905)) reveals that of the 11 profiled police justices, over 60% are noted to be Democrats and most have Irish heritage or are immigrants.

[3]By the 1870s, the Democratic Party in Chicago held sway over the large Irish and German immigrant populations. Mayor Carter Harrison, a long-serving Democratic mayor after 15 years of Republican mayors, was elected in 1879, and he reformed the police (Mitrani (2013)). Importantly, he also dramatically increased hiring of immigrants into the police, whereas the police force had previously been largely native-born Republicans. Irish Democrats took control of the Policeman's Benevolent Association from Republicans by the 1880s. As a result, in their day-to-day work, officers were ethnically and socially tied to the populations they policed (Haller (1976)).

2.2 Corruption

The police justice system gave justices significant discretion, such that the outcome of one's case, the process, and the fines paid would depend more on the justice shop than the jurisdiction (Willrich (2003)). Police and justices were selectively used against the illegal operations of political opponents (Haller (1976)) and to ensure political obedience from local (illicit) business operators (Willrich (2003)). This was particularly true in the redlight district of Chicago, known as the Levees (see Figure A.1), which was located in the First Ward. Notably, both the police justices and police officers within and around the Levees were strongly aligned with the local Democratic Party.[4]

Selective enforcement and police being an integral part of the local political machine would persist long after the 1906 reform. However, before the MCA, the alignment between police officers and police justices produced a corruption which required collusion between police and justices: the 'bailbonds racket' (Lindberg (1998)). Police justices could deny trial at night and retain a fee of $1 for each bailbond issued (Matthews (1898)), and bailbondsmen collect a fee of at least $1 for their service (Illinois Senate (1898), page 21).[5] So, officers would make mass arrests at night and bring the arrestees before police justices. Justices would deny the arrestees trial at night, leaving a bond as the arrestees' only option to leave until morning. The arrestees pay the judge a fee for the bond, and they also pay the bailbondsmen, and these payments are split with the arresting officer(s). The result was mass arrests of civilians, innocent or guilty, to rack up fees and bonds, which would be split among the officers, justices, and bailbondsmen. This scheme was prevalent at "Central Headquarters

[4]For example, one well-known police justice, John Prindiville, was a Democrat and served at the "infamous" Harrison Street station for 10 years (Willrich (2003), pg. 15). Even as late as 1911, the First Ward remained aligned with Harrison's Democratic faction against the Republicans and reform-aligned faction of the Democratic Party (Lindberg (1998), pg. 126-127).

[5]The bond system in Chicago had a number of oddities in this time period in part due to the incentives of justices who could only retain fines from issuing bailbonds and from holding trial (Funk (2025)). Importantly for the bailbonds racket, bailbondsmen were usually wealthy business owners in their own right who made money from being bondsmen. Their essential function was to stand in and find the bonded person if they did not appear for their trial. The bailbondsmen of the redlight district were themselves wealthy from separate business, signed bailbonds as a side business, and were effective at finding people who skipped trial (Tribune (1901)).

and the outlying precinct stations" (Lindberg (1998), pg. 111), such as the Levees district and its infamous Harrison Street station and associated police court.

An Illinois State Senate Report in 1898 attests this practice, suggesting 100 to 200 men and women can be caught up in mass arrests at night and charged at least $2 each, which is split between the justice, bondsman, and officer (Illinois Senate (1898), page 21). A Harper's Weekly article from the period discusses the same operation and explicitly locates it in the redlight district (the Levees), and approximates 250 people being extorted for around $6 each (Matthews (1898)).

2.3 Reform and the Municipal Court Act of 1905

After years of lobbying and pressure from, mainly Republican, progressive reformers, the Municipal Court Act (MCA) was ratified in November 1905.[6] This introduced a centralized court where criminal and civil cases would be heard. It professionalized justices and created a rift between the local political forces and the justices. Rather than reform the justice system as a whole, end police corruption, or destroy the local political machines, the MCA accomplished its main purpose: "to eliminate profiteering in judicial administration" (Willrich (2003), pg. 47).

Under the MCA, judges were to be "at least thirty years of age and a citizen of the United States" (Illinois (1905), pg. 163), and they were required to be lawyers and elected via city-wide elections. Furthermore, currently serving police justices were barred from being on the ballot of the first election. These eligibility requirements favored local elites and progressive legal reformers who were politically and ideologically opposed to the local party control and patronage systems favored by Democrats.

The first judge elections were held in November 1906. The Republican ticket dominated

[6]Republicans stood against the emerging Democratic political machine, and many immigrants in general, with progressive reform slogans such as "divorce politics from the police" (Lindberg (1998), page 35). The state of Illinois, which was Republican controlled and had Republican governors for all but four years between 1856 and 1913, had attempted to clamp down on corrupt practices of justices but these laws went largely unenforced in Chicago (Willrich (2003)).

the elections, winning all but one of the over twenty seats. The first Chief Justice, Harry Olson, was not only a protege of the Republican Illinois Governor but also an outspoken progressive critic of the political control permeating the pre-reform system (Willrich (2003)). On December 3rd, 1906, the Municipal Court opened with centralized administration and with proceedings being held in multiple branches around the city (Motley (1964), Willrich (2003)). The Municipal Court instituted an emphasis on internal oversight and accountability through publishing annual reports, all of which contained an official audit of the court's finances and extensive discussion of operations (Municipal Court of Chicago (1907-1912)).

Following the MCA, historical discussions of judicial involvement in police corruption (e.g., the bailbonds racket) end abruptly (Lindberg (1998), Haller (1976)), suggesting the MCA did succeed in ending 'profiteering' of the lower courts directly through fines and fees and indirectly through interactions and collusion with police. However, the ties between police and their political backers would continue and resist future reforms throughout the progressive era (Fogelson (1977)). Police continued to shield crime bosses and professional criminals from prosecution based on political connections (Haller (1970)), and police and bailbondsmen participated in extortion rackets of brothels, but without the involvement of justices and at a smaller price (Lindberg (1998)).[7]

3 Data

To measure the effects of the MCA, I extracted data from over a dozen historical and archival sources and combined this with multiple pre-existing datasets. The archival sources include: annual police reports from Chicago (1901 - 1913), Boston (1902 - 1910), Cincinnati (1905 - 1908), St. Louis (1905 - 1911), and Buffalo (1913); the handwritten CPD registry (1904 - 1910); annual reports from Chicago's Municipal Court (1907 - 1912); Missouri Birth and

[7]Efforts to reform the police in Chicago, as in other cities in the US during the Progressive Era, were resisted by both police and politicians. For example, in 1912, a push to break the connections between political wards and police precincts was successfully defeated by the police and their political backers (Fogelson (1977)).

Death Records Database Pre-1910; US Census Bureau's Financial Statistics of Cities reports (1903 - 1908); US Census Reports on Manufactures of 1910. Most of these documents were accessed through online library scans, while others were only available in person at the New York Public Library and the Chicago History Museum's Abakanowicz Research Center. Individual records and statistical tables from these sources were extracted and combined into usable data.

Pre-existing datasets used include: Monkkonen (1984), which contains annual populations, arrests, and police force sizes for multiple U.S. cities during the time period; Bienen (2012)'s database of over 10,000 digitized homicide records in Chicago; case-level homicides in San Francisco (1849-2003) and Los Angeles (1830-1974) were obtained from Mullen (2005) and Monkkonen (2005); two samples from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. (2024), Ruggles et al. (2025)).

The core data were extracted from historical records contained in annual police reports. Each report contains dozens of tables and statistics for the year of the report as well as some information on prior years. Not all data exist in each report, though all analyses will use data for which the information exists in at least the 1906 and 1907 reports from Chicago. In Chicago, reports contain personnel records including total officer counts, the number of officers assigned to each precinct within the city, and lists of officers injured on duty. With respect to police performance, the reports contain annual and monthly data on arrest counts, annual arrest counts disaggregated by gender of arrestee, charge type, and case outcome, among other information. The reports also contain a 'Chief Matron's Report' with the counts of arrests made of women (around 15% of all arrests) disaggregated by precinct for a subset of precincts. Reports also contain the total value of property reported stolen at the annual and monthly level, which I use as a proxy for property crime.

For comparison with Chicago, I extracted similar data from annual reports from the other cities (Boston, Cincinnati, St. Louis, Buffalo). The data from these cities do not perfectly overlap with those of Chicago, so I employ them in different analyses depending on

what data are available. I use Boston, Cincinnati, St. Louis, and Buffalo as comparisons for stolen property measurements, St. Louis for annual charge type comparisons, and Cincinnati for monthly arrest counts. For a broader comparison group of arrests, I use the data from Monkkonen (1984), and I take the sample of Chicago and 15 other major U.S. cities with complete data between 1898 and 1912.

To measure murder rates in Chicago, I use the detailed database on Chicago homicides of Bienen (2012). I count murders as the number of homicides which are reported to be either intentional or felony murder. The murder data also contain the police precinct of the murder for roughly 75% of observations during the relevant period. Outside of Chicago, I collected murder rates for St. Louis, Cincinnati, Los Angeles, and San Francisco. I extracted St. Louis death records from the time period from the Missouri Birth and Death Records Database Pre-1910. Cincinnati records also include the number of arrests for murder each month, which I use as a metric for murders in Cincinnati. Case-level homicides in San Francisco (1849-2003) and Los Angeles (1830-1974) were obtained from Mullen (2005) and Monkkonen (2005).[8]

I measure bureaucratic composition across multiple cities using Census data and within the CPD using CPD registries, allowing me to explore compositional changes in the CPD specifically. I measure the composition of Chicago's legal bureaucracy from two samples from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al. (2024), Ruggles et al. (2025)). The first sample contains demographic and socioeconomic information for individuals employed as judges/lawyers and policemen/detectives in Chicago, Philadelphia, St. Louis, and New York between 1870 and 1910 (excluding 1890). The second sample subsets to records of individuals linked between 1900 and 1910, such that past professions of lawyers and police in 1910 can be measured. To obtain more detailed information on CPD recruitment before and after the MCA, I extract the names, birth places, birth dates, prior professions,

[8]Data provided by Monkkonen (1984) contain a wide breadth of policing and crime data across many decades and cities. However, the data for many interesting variables are incomplete around this time period, making much of it unusable for this analysis. Data from Monkkonen (2001) provides homicide rates in New York, Chicago, and some other areas over the same time period, but does not distinguish between murder and homicides. 'Death Registry' data has been used in other studies, such as Owens (2011), but it does not break out intentional murders and is at the annual level.

appointment dates, and termination dates from a sample of the handwritten CPD registry from 1904 - 1910 including 787 CPD officers from the Chicago Police Department collection at the Chicago History Museum (Chicago (Ill.). Police Department. (1969)).[9]

Finally, I employ data on governmental financials and private sector performance from the US Census to understand city-level changes following the MCA. I measure the financial performance of Chicago's government compared with other cities using court and police-related expenditure data and total expenditures and receipts in the top 15 largest US cities between 1903 to 1908 extracted from Bureau (2025). To measure private-sector performance of Chicago compared with other major US cities, I use city-level manufacturing production and employment data for 1899, 1904, and 1909 from the 1910 Census (Census (1912)).

4 Empirical Strategies

The main goal of this paper is to understand the effects of the MCA on bureaucratic performance and composition in Chicago. The direct effects of the MCA were on the courts, not police, though the relationship between police officers and police justices suggests significant spillovers.

4.1 Single-Unit Time Series

For time series data, when only Chicago-level data are available, I employ two designs. First, I use an interrupted time series regression to recover the effect of the MCA (δ^{ITS}):

$$y_t = \delta^{ITS} 1\{t > t^*\} + \beta_0 + \beta_1 t + \beta_2((t - t^*) \times 1\{t > t^*\}) + \beta_3 X_t + \epsilon_t \tag{1}$$

where t denotes the time period (e.g., the month, quarter, or year), t^* denotes the period right before the MCA reform went into effect (e.g., 1906 for years, November 1906 for months,

[9]The records, stored on microfilm, are only accessible in person at the Chicago History Museum's Abakanowicz Research Center. However, visiting hours and staff are restricted due to budget cuts, and so I was only able to scan 94 pages of the registries.

and quarter 4 of 1906 for quarters). In X_t, I control for police force size when possible and I include season-fixed effects when using sub-annual data (e.g., monthly or quarterly). To account for autocorrelation and heteroskedasticity, I use Newey-West standard errors with lag automatically determined based on Newey & West (1987).

Identification in the time series model requires that, absent the MCA, outcomes would have trended similarly conditional on controls. This assumption is violated if there are confounding shocks or policy changes or anticipatory responses.

For inferring the statistical significance of the effect of MCA, I also employ the partial identification strategy of Manski & Pepper (2018) (MP) to place bounds on a null effect. I assume bounded time variation: if the MCA had no effect, we expect that the magnitude of the immediate change in outcomes should be bounded by the observed variation in the time series in the pre-MCA period. I set these bounds based on value $\kappa > 0$. Following a conservative approach, with at least 10 pre-MCA observations, κ is the 90th percentile of pre-MCA period-to-period absolute changes, and, with fewer than 10 pre-MCA observations, κ is the maximum pre-MCA period-to-period absolute change. Then the MP bounds for a null effect are $Y_{t=t^*} \pm \kappa$, and if the observed post-reform outcome ($Y_{t=t^*+1}$) is outside of these bounds, it suggests that there was a significant effect of the reform.

4.2 Multi-Unit Panels

When two or more units are available (e.g., multiple cities or police precincts), I employ a traditional difference-in-differences (DiD) design estimated using a two-way fixed effects (TWFE) regression:

$$Y_{i,t} = \delta^{TWFE} 1\{i \in \mathcal{T}, t > t^*\} + \beta X_{i,t} + \gamma_i + \theta_t + \epsilon_{i,t} \tag{2}$$

where i denotes the unit, $\mathcal{T}$ is the set of units treated by the MCA, t^* is the MCA's start date, γ_i are unit-specific fixed effects, and θ_t are time-specific fixed effects. $X_{i,t}$ includes

time-varying controls when applicable, such as police force size. I estimate this with OLS because there are not multiple treatments or timings, and I denote the coefficient as TWFE (two-way fixed effects). I employ Newey-West standard errors with the lag automatically determined based on Newey & West (1994).

This design relies on the parallel trends assumption: without the MCA, the unobserved differences between the treated and control units would have trended similarly. One way to test for violations of this is by examining pre-trends in outcomes. However, this assumption is violated if there are similarly timed confounding shocks or policy changes that also influence outcomes. Additionally, if agents respond to the treatment before the treatment period (anticipation) or control units are influenced by the treatment, then estimates will be biased.

Finally, for panel data with more than two units, I also employ a synthetic difference-in-differences (SDID) design of Arkhangelsky et al. (2021), which has advantages over both synthetic control (Abadie et al. (2010)) and traditional DiD designs. SDID constructs a 'synthetic' unit composed of the control units based on the pre-treatment (pre-MCA) relationship between the trends in the treated unit's outcomes and those of the control units. Based on pre-treatment trends, the method constructs unit-specific weights ω such that the aggregate pre-treatment trends in the synthetic control outcome coincides with the trends of the treated unit. Time weights (λ_t) are also constructed to balance pre- and post-treatment time periods. The estimated weights are used to estimate the effect of the treatment on the treated unit:

$$\hat{\delta}^{SDID}, \hat{\mu}, \hat{\gamma}, \hat{\theta} = \arg \min_{\delta^{SDID}, \mu, \gamma, \theta} \{\sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \gamma_i - \theta_t - W_{it}\delta^{SDID})^2 \hat{\omega}_i \hat{\lambda}_t\} \tag{3}$$

where γ_i and θ_t are unit and time period fixed effects, μ is an intercept term, $W_{i,t}$ is an indicator for treatment status (e.g., if i=Chicago and $t > t^*$ in the multi-city data), and δ^{SDID} is the average causal effect of the MCA. Inference on $\hat{\delta}^{SDID}$ is conducted using the placebo method, which is usable even with one treated unit and relatively small panels but assumes homoskedasticity.

SDID is doubly-robust by combining the strengths of difference-in-differences and synthetic control, and it utilizes unit and time weights to construct a synthetic control unit which satisfies the parallel trends assumption. However, SDID estimates are still sensitive to anticipation, confounding treatments, and spillover concerns, and relies on sufficient data and stability for proper weights to be established.

Note that δ^{ITS} captures the immediate change in the outcome following the MCA, while δ^{TWFE} and δ^{SDID} capture the average effect over the post-MCA period.

5 Effects on Finances and Bureaucratic Composition

Using a subset of these data sources, I validate the historical account of the MCA's effects on municipal finances and composition. Based on the historical account, the MCA should have increased direct spending on the courts and on judge salaries and raised observable 'quality' of judges. However, it should not have had any effect on the police department through funding or composition.

Figure 1 compares city expenditures on municipal and police courts, expenditures on police, and total governmental receipts and payments across 10 major US cities between 1903 and 1908, along with Manski and Pepper bounds for Chicago in 1907 centered at the 1906 value. Table 1 displays corresponding TWFE, which includes city-specific linear trends, and SDID effects of the MCA.

Panels A and B of Figure 1 show that after the MCA in 1906, Chicago experiences a sizable and persistent increase in total expenditures on courts from around \$0.1 per person to \$0.25 per person, which is primarily driven by increased spending on salaries of court officials. These increases occur only in Chicago and are far outside of the MP bounds, and regression estimates are all highly statistically significant and correspond to well over a 100% increase. Notably, this increase moves Chicago to the second highest per-capita expenditures on courts, second only to Boston. Boston had replaced its Police Court with a Municipal Court system

in 1866 and had replaced fee-based payment with salaries (Davis (1900)).

Despite higher expenditures on the courts, Chicago's overall finances experienced smaller changes (Panels C and D) because the court comprises a relatively small share of total receipts and payments (less than \$0.3 versus over \$30 per person). In Chicago, both receipts and payments were trending upwards, so while the 1907 values are just outside of the MP bounds, the regression estimates are not statistically significant. Consistent with strong oversight and fiscal responsibility, the municipal court maintained a running surplus following its creation based on numbers from its annual audit reports (Figure A.3). Furthermore, the manufacturing production and employment data suggest Chicago experienced similar growth in value and employment as other US cities (Figure A.5).

Panels E and F focus on police. The number of police officers in Chicago rose between 1906 and 1907, but this was driven by pre-MCA trends beginning in 1905 when the CPD began hiring, and the 1907 value is well within the MP bounds. The hiring was in response to Chicago, despite being the 2nd largest city in the US, having relatively few police per capita in the early 1900s. Expenditures per officer (largely salary) also rise between 1906 and 1907, but this is returning it to its previous level following a dip in 1906, and is within the MP bounds. No regression estimates are statistically significant. Overall, while the MCA caused a discontinuous increase in expenditures on the Municipal Court in Chicago, there was no effect on expenditures for policing or effects on overall city finances.

Turning to composition, Figure 2 shows that the lawyers and judges in Chicago in 1910 tended to have higher quality professions in 1900 compared with those in St. Louis, Philadelphia, and New York City — specifically, they were disproportionately likely to have been lawyers/judges in 1900, consistent with the new requirements of the MCA. Politically and demographically, there was also a shift: as noted in Section 2, Irish Democrats were well-represented among the pre-MCA justices, while the first set of Municipal Court judges was overwhelmingly non-Irish Republicans.

By contrast, police and detectives in 1910 Chicago did not hold noticeably different

or higher quality professions in 1900 compared with other cities. Figure A.4 displays trends of observables of lawyers/judges and police/detectives in census data, but there are no distinguishable differences. I also use CPD registry data to determine if there were disproportionate exits after the MCA or whether the composition of CPD recruits changed after the MCA. Figure A.6 shows there was no discontinuous increase in CPD exits or hiring following the MCA, beyond pre-existing trends. Additionally, those who were hired before and after the MCA were similar in birthplace (Panel C) and prior profession (Panel D) — Chi-squared tests fail to reject that the pre-/post-MCA compositions are significantly different. People hired after the MCA are older than those hired before (Panel B). While this difference is statistically significant based on a t-test, the difference is economically small as pre-MCA hires averaged around 29 years old while those hired after average 30 years.

In sum, these results are consistent with the professionalization of the lower court in Chicago, with Municipal Court judges required to be lawyers and paid fixed salaries rather than making money from extracting payments in fees or in-kind. Importantly, there were no corresponding changes to finances or composition of the Chicago Police Department, and the overall economy and governmental finances of Chicago were not impacted.

6 Effects on Officer Behavior

I measure officer behavior using arrests. Arrests are both a primary metric of performance and activity in law enforcement and a key mechanism through which justice-police collusion was operating.

6.1 Total Arrests

Despite the MCA having no direct effect on police in Chicago, arrests declined substantially after the MCA. Panel A of Figure 3 shows that between 1902 and 1906, arrests rose in Chicago. After the MCA, there is a massive decline of around 30,000 arrests in 1907, nearly

1,500 arrests per 100,000 population (per 100K) — over a 30% decline compared with the 1906 counts. Arrests resume their upward trend but do not reach their 1906 highs even by 1912. These year-over-year changes are well outside of the Manski & Pepper (2018) (MP) bounds denoted by the error bars computed using bounds of time series variation based on the prior years.

The more granular time series of monthly arrests per 100K is in Panel B. The decline in arrests was sudden, occurring over the course of a few months between October 1906 and February 1907. The first noticeable decline is between October and November of 1906, when the MCA elections took place, and then another large drop between November and December of 1906, when the Municipal Court began operations. There are additional declines between December of 1906 and January of 1907, though within the MP bounds, with monthly arrests reaching their lowest point in February of 1907. Arrests remain extremely low until May of 1907, when there is a notable increase in arrests, though still below their pre-MCA levels — this coincides with a Republican mayor taking office in April of 1907 for the first time since 1879. By contrast, the passage and ratification of the MCA in November of 1905 has no visible difference from the points around it. The effects first appearing after the election are consistent with an anticipation effect before the actual opening of the Municipal Court, but it is inconsistent with the results being driven by any sort of change in record keeping or data recording.

Columns (1)-(4) of Table 2 display the ITS estimates of the effect of the MCA on arrests at both the annual and monthly level. Controlling for police force size produces similar estimates in proportion to the pre-MCA means. At the annual level, the MCA reduced annual arrests by 37,890 arrests per year ($p < 0.01$). Relative to the pre-treatment mean of Chicago, this equates to -50% decline. Using the log of arrests per 100K, the MCA caused a -38% decline ($p < 0.01$). At the monthly level, the MCA in December of 1906 reduced monthly arrests per capita by -36% ($p < 0.01$). This estimate may be attenuated due to anticipation effects, and a donut ITS specification that removes the months of October 1906

to February 1907 results in an even larger estimate of a -41% decline ($p < 0.01$).

Comparing Chicago with other major US cities at the time reinforces these results. Panel A of Figure 4 displays the trends in log-transformed arrests per 100K for Chicago, eleven control cities, and the Synthetic Chicago used to estimate the SDID coefficients, and Panel B compares monthly arrests in Chicago and Cincinnati.[10] Before the MCA, Chicago and Synthetic Chicago trend very similarly, however following 1906 there is a large decline in Chicago only. The other cities and the Synthetic Chicago display no such decline in 1907 and continue their upward trend. At the monthly level, before the MCA, Chicago and Cincinnati trended similarly, though Chicago's arrests grow at a slightly higher rate. After the MCA, Chicago experiences a massive decline, while there is no detectable change in Cincinnati.

As shown in columns (5)-(9) of Table 2, the SDID and TWFE point estimates are similar to those of the ITS results. At the annual level, SDID and TWFE results are similar. In the short-run, using a post-treatment period of 1907, the MCA caused -37% and -35% declines ($p < 0.01$) in arrests using TWFE and SDID. Using a long-run post-treatment period of 1907-1912, the effect is still large, with a TWFE estimate of -33% ($p < 0.01$) and a SDID estimate of -31% ($p > 0.1$). At the monthly level comparing Chicago and Cincinnati (Column (7)), TWFE results indicate the MCA caused an -39% decline in arrests ($p < 0.01$) in Chicago.

6.2 Arrest Composition

Using annual charge counts data, I separately estimate the time series regression on arrests for felonies (14% of pre-MCA arrests) and misdemeanors (86%). The decline was mainly driven by misdemeanors, which not only declined relatively more than felonies compared with their frequency but also comprised the vast majority of arrests. Panel A of Figure 5 displays annual felony and misdemeanor arrests per 100K from 1899 to 1910. While felony arrests decline throughout the pre-MCA period, there is only a small additional decline post-MCA,

[10]Not all control cities are displayed in Figure 4 for readability. There are 14 control cities which contribute to the short-run SDID estimate and 11 control cities that contribute to the long-run SDID estimates because SDID requires complete data for over the full period. The TWFE regressions drop cities which do not have patrol officer counts, while SDID does not.

whereas misdemeanor arrests are trending upward pre- and post-MCA and experience a significant drop following the MCA. The change in misdemeanors is well outside of the MP bounds while the felony arrests are well within the bounds.

Table 3 displays regression estimates of these changes. Time series regressions show that a -42% ($p < 0.01$) decline in misdemeanors following the MCA drove the vast majority of the total decline while felonies declined by only -11% ($p < 0.05$). A TWFE regression comparing misdemeanors with felonies, and including arrest type-linear time trends, returns an excess decline of -31% ($p < 0.01$) for misdemeanors relative to felonies within Chicago. A cross-city comparison of misdemeanors in Chicago with those in St. Louis indicates misdemeanors in Chicago experienced an excess decline of -48% ($p < 0.01$). This suggests that officers did not reduce their pursuit of more serious crimes in response to the MCA compared with the change in their low-level enforcement.

Decomposing the arrest declines by specific charge types shows that the decline was driven by low-level and highly discretionary misdemeanor charges which are consistent with redlight district raids. Panel B of Figure 5 compares the changes in specific charge types between 1906 and 1907. Among misdemeanors, the substantial decline was driven by low-level offenses. The least serious but most common misdemeanors declined by large amounts: resisting an officer, gambling/gaming or houses of "ill fame", "street walkers" (prostitution), all declined by at least 40%. The two most common misdemeanors, comprising the majority of all arrests in 1906, are miscellaneous charges often not related to any harmful criminal activity: disorderly conduct (54%) and unspecified misdemeanors / ordinance violations (9.5%). Post-MCA in 1907, disorderly conduct and unnamed misdemeanor arrests declined by 29% and 46% respectively. Arrests relating to Gaming Houses, which comprised 7.5% of arrests in 1906, fell by 72% in 1907. Additional low-level offenses include those related to houses that are 'Disorderly' or 'of Ill Fame', Opium dens, gambling (gaming), resisting an officer, and prostitution ('street walkers'). By contrast, felony arrests in the most common charge types declined by far less, with the most common charge being larceny for which

arrests only declined by 1%, though the next most common, burglary and robbery, declined by 21-30% though they only comprised a small share of arrests in 1906.

Panel C of Figure 5 shows that the change in arrests between 1906 and 1907 was driven by a decline in arrests which would have been disposed of in police court (now the municipal court), further suggesting that the change in officer behavior was in response to the change in the judges. Fine rates do not change substantially, receiving only a 9% increase. Figure A.7 shows that changes in fines were not nearly as large or persistent as arrests, and, while the overall decline in low-level arrests likely resulted in fewer total fines, the focus on relatively more serious arrest resulted in a short-run increase in fines per arrest.

6.3 Localized Police-Justice Collusion and the Bailbonds Racket

To explore the mechanisms behind the arrest decline, I estimate the effect of the MCA on specific precincts in Chicago where judicial corruption and extortive arrests, such as the bailbonds racket, were known to take place. If the MCA's effect was through changing norms or behaviors across the city or general changes in the policing environment (different judges, centralization, etc.), we would not expect a differential large effect in these areas of the city. I use data on women arrested in the Matron's Report, which provides annual counts of women arrested for a subset of precincts, accounting for around 84% of women arrested in the city. Importantly, while women were only around 13% of pre-MCA arrests, they were disproportionately arrested for low-level offenses (86% of women versus 77% for men in 1906).

As shown in Figure 6, the majority of women arrested were in two precincts, 2 and 27. Precinct 2 corresponds to the redlight district of the time, the 'Levees', explicitly mentioned as a place where the scheme was taking place; Precinct 27 is just across the river from Precinct 2's north-west corner, close to the site of at least two highly active Levees areas (see Figure A.1). The other precincts comprised smaller numbers of arrested women pre-MCA, and the number of women arrested was mainly from these two precincts: in 1906, Precincts 2 and 27

accounted for 62% of arrests among the eleven precincts in the Matron's data, though they only account for 23% of patrolmen across the eleven precincts. Between 1906 and 1907, total arrests of women fell by 41% in the pooled Precincts 2 and 27 while they declined by 15% in the pooled other precincts.

Table 4 displays regression estimates confirming Precincts 2 and 27 experienced excess declines in arrests across a number of specifications. Panel A focuses on long-run (1898-1912) effects while Panel B focuses on short-run (1898-1907) effects. Columns (1)-(3) display TWFE results, controlling for precinct and year fixed effects and the treatment effect being whether the precinct is 2 or 27 after 1906 and precinct-specific police force size. Column (1) displays effects on total arrests, while Columns (2) and (3) use logged arrests. Because precincts did experience differential trends before the MCA, Column (3) includes controls for precinct-specific linear time trends. Column (4) and (5) display SDID estimates using total arrests and logged arrests as the outcomes. Across all specifications, the Precincts 2 and 27 display economically significant excess declines in women arrests following the MCA. The base TWFE specification indicates -52% ($p < 0.01$) and -48% ($p < 0.1$) declines in the long- and short-run. The SDID estimates in the long- and short-run generate -49% ($p < 0.1$) and -31% ($p > 0.1$) excess declines in women arrests.

These estimates indicate large excess declines in arrests of women in the areas where judges were likely engaging in corrupt schemes with police. Even though these estimates are attenuated if other precincts were also influenced by the MCA, the magnitudes likely align with the overall effects on arrests in Chicago for two reasons. First, discretionary arrests comprise a large portion of total counts. Second, these arrests were concentrated in areas near Central Headquarters like Precincts 2 and 27.

6.4 Quantifying Cost Savings

The decline in arrests concentrated in redlight or adjacent precincts for low-level charges is consistent with the historical record on the end of the bailbonds racket. This suggests that

transfers from Chicagoans to law enforcement declined, which we can approximate given the prior estimates and historical accounts. The total amount of extractive corruption can be expressed as the sum of judicial-related and non-judicial related extractions, as a function of the MCA being in place. Non-judicial corruption includes police actions such as charging protection money from brothel owners, arrests on legitimate charges to extract concessions by selective enforcement, and other police actions which do not rely on justices being 'in on' the corruption. While the level or change in such actions are difficult to measure, the historical record does not note any particular change in such occurrences. For example, practices such as extorting prostitutes and brothel owners for weekly "stipends" of $3 or paying $1-$2 bribes to officers continued after the MCA which involved officers and bondsmen (Lindberg (1998), pg. 128), but they did not involve judges and the courts. Based on the Senate report and Harper's Weekly article, the amount extorted was at least $2 and likely around $6 per person — I use the midpoint of $4 per arrest. For context, $1 in 1913 (the earliest year for the CPI Inflation Calculator) is around $33 in 2025.

This section estimates the change in judicial-involved extortion under modest assumptions. First, the MCA ended judicial corruption only occurring in the Precincts 2 and 27. Second, only arrests not made due to the MCA would have been extortive.[11] Third, I assume low-level arrests are randomly distributed between police court and non-police court, and extortion only occurred for low-level arrests (based on charges) in police courts in Precincts 2 and 27.

To approximate the number of judicial corruption-involved arrests, I begin by focusing on the set of low-level charges and cases brought to police courts. Arrests of women were almost entirely composed of low-level charges (86% in 1906), and these charges, if involved in corruption, were likely to be brought to police courts (89% in 1906). As a more conservative estimate, I use the estimated effect of the MCA on women arrests in Precincts 2 and 27, where the bailbonds racket was likely taking place, compared with the other precincts. The smallest short-run estimate is an excess decline of 25% off of a pre-MCA count of 6,258

[11] Only 'complier' arrests involved extortion, while arrests that would be made with- and without the MCA would not involve extortion pre-MCA.

arrests of women in Precincts 2 and 27.

I multiply the pre-MCA count (6,258) by the excess decline (0.25) and by the shares in police courts (0.89) and shares of low-level charges (0.84) — even though there was likely high overlap between these categories. This results in 1,199 extortive arrests of women prevented in 1907.[12] Applying the extortion price midpoint, in just arrests of women in Precincts 2 and 27 in 1907, the MCA saved Chicagoans around $4,800 or $158,396 in 2025 dollars.

While no precinct-level arrest data for men exists, simple assumptions allow us to back out a similar estimate for men. So, I assume a similar proportion of the 79,485 male arrests occurred in Precincts 2 and 27 as the proportion of women arrested ($0.52 = \frac{6{,}258}{12{,}007}$), and I also multiply this number by the overall rates of male arrests being low-level (76%) and in police court (99%) in 1906. The result suggests that the MCA prevented 7,817 extortive arrests of men in 1907 in Precincts 2 and 27, which implies the MCA saved male Chicagoans around $31,268 or $1,031,847 in 2025 dollars.[13] A total of 9,017 arrests avoided in 1907 is consistent with the Senate Report and Harper's Weekly accounts of potentially over 200 people being arrested in a night over the course of less than 50 nights.

In total, the MCA likely saved Chicagoans at least $36,068 or $1,190,243 in 2025 dollars in prevented extortive arrests. Expanding this number forward in the following years while using the smaller short-run estimate and ignoring population growth, this suggests savings of over $216,408 or $7,141,460 in 2025 dollars between 1907 and 1912.

While a form of corruption, the end of corruption was a loss of income for officers. Assuming, as before, these practices were concentrated among officers in Precincts 2 and 27, together totaling 319 total officers in 1907, then the MCA resulted in an average of around $42 in lost income per officer in 1907, assuming the officer earned $1.5 on average per arrest. This is equivalent to a 4.2% decline in income for these officers, and likely more per affected

[12] $1{,}199 = \underbrace{0.25}_{\text{Excess Female Decline in Precincts 2 \& 27}} \times \underbrace{0.887}_{\text{Share Female in Police Court}} \times \underbrace{0.864}_{\text{Share Female Low-Level}} \times \underbrace{6{,}258}_{\text{Female Arrests in Precincts 2 \& 27}}$

[13] $7{,}817 = \underbrace{0.25}_{\text{Excess Female Decline in Precincts 2 \& 27}} \times \underbrace{0.99}_{\text{Share Male in Police Court}} \times \underbrace{0.76}_{\text{Share Male Low-Level}} \times \underbrace{\frac{6{,}258}{12{,}007}}_{\text{Share of All Female in Precincts 2 \& 27}} \times \underbrace{79{,}485}_{\text{Total Men Arrests}}$

officer if the practice was actually concentrated in a smaller number of patrolmen.

7 Effects on Crime

While arrests are the primary metric of police activity and performance, crime is the primary outcome for criminal justice policy, and the effects of the MCA on crime also provide suggestive evidence for mechanisms. For example, if the MCA led to a decline in arrests but rise in crime, then it suggests that judicial corruption may be beneficial as a motivator for policing.

7.1 Property Reported Stolen

Crime reports are rare during this time period separate from arrest reports. To measure property crime, I use the value of property reported stolen, which appears in both Chicago as well as other cities' reports. As shown in Panel A of Figure A.8, Chicago exhibits no differential pattern in property crime compared with other cities at the annual level, specifically Boston (1899 - 1910), Buffalo (1904 - 1912), and St. Louis (1905 - 1909). If anything, Chicago's property crime rate appears to rise slower following the MCA than St. Louis and Buffalo. Panel B focuses on Chicago-only monthly and quarterly time series of stolen property, and there is no evidence of a discontinuous change in property crime rate with the first post-MCA points being well within the MP bounds. I confirm these null effects with regression analysis. As displayed in Table 5, none of the ITS estimates and only one of the TWFE estimates is statistically significant, a -23% decline ($p < 0.01$). Taken together, the MCA likely did not lead to changes in property crime in Chicago.

7.2 Murder

As with property crime, reports of violent crimes, separate from violent crime arrests, are difficult to obtain. Murder, which is the most socially costly crime, is also more commonly recorded separate from arrests. Importantly, I focus on murder rather than homicides by

subsetting the Bienen (2012) data to homicides classified as "Intentional murder", "Other murder, felony murder", or "Intentional manslaughter". This avoids pooling murders with accidents, abortions, and suicides, which historic 'homicide' data generally does not distinguish between.[14]

Panel A of Figure A.9 displays cross-city comparisons of logged murder rates between 1890 and 1912 from Los Angeles, San Francisco, and Chicago, as well as Cincinnati (1890 - 1908).[15] Cincinnati and San Francisco trend similarly to Chicago, though while Chicago levels off after 1905, they continue to rise. Panel B shows quarterly murder rates in Chicago, Cincinnati, and St. Louis, with a clear decline between Q4 of 1906 and Q1 of 1907 in Chicago that is outside of the MP bounds.[16]

Table 6 displays TWFE and SDID results using different balanced panels of the annual data depending on the timeframe (Cincinnati ends in 1908) and controls (Los Angeles is missing officer counts). The TWFE estimates range from -15% ($p < 0.1$) to -46% ($p < 0.01$). The SDID estimate reports almost no effect (-1% ($p > 0.1$)), though there are very few control cities. Table 7 compares murder rates within Chicago by aggregating murders (and police officers) to the precinct-by-quarter level and studying whether the precincts highly exposed to extortive arrests (Precincts 1, 2, and 27) experienced different changes in murder rates after the MCA. TWFE and SDID results indicate that the highly exposed precincts experienced larger declines (all over -20% of the mean) though none are statistically significant.

Overall, this suggests that the end of judicial corruption and decline in arrests coincided with, at worst, no effect on murders in Chicago. Nevertheless, the point estimates for murder

[14]For example, based on the Bienen (2012) data, in 1907 in Chicago a chemical explosion in a plant killed 12 people was investigated as homicide, but not an intentional murder. These deaths are likely counted in homicide data, such as Monkkonen (2001). Figure A.10 displays a comparison between the Monkkonen (2001) homicide data and murder and homicide rates constructed from the Bienen (2012) data. Appendix Figure A.11 decomposes murder rates by the category of murder and the location of the murder. A plurality of murders are from unknown circumstances, while the declines between 1904 and 1908 are primarily driven by fight-related and 'other' circumstances, and the declines are driven by places of business rather than residences or in the street/non-commercial public places.

[15]Cincinnati numbers are based on a long time series of murder arrests, and so this comparison assumes a constant rate of murder arrests in the city.

[16]Cincinnati counts are based on monthly murder arrests, and the St. Louis counts are incomplete and based on the legible records from the Missouri Birth and Death Records Database Pre-1910.

suggest that the MCA led to a short-run decline in murder, particularly in areas where the MCA had its largest effect on arrest, consistent with improved crime prevention.[17]

8 Robustness

The primary evidence suggests that the MCA and the replacement of police-aligned justices with anti-corruption judges caused a significant decline in low-level arrests, particularly in precincts where the bailbonds racket was taking place. In this section, I explore the robustness of the main results to potential confounders and additional outcomes.

Confounding Events: One concern is that there are additional events which took place around the time of the MCA, and, since the main interpretation relies on the breakdown of the relationship between justices and police, these other mechanisms should not be able to explain the results. First, if social or political pressures caused a shift in policing behavior, rather than the end of the bailbonds racket, then we would expect visible changes around the ratification of the MCA (November 1905) or the election of Republican mayor Fred Busse (elected and sworn in in April 1907). However, we see no such change around these events (see Figure @ref(fig:arrest1a.pdf)). Similar concerns exist over the adoption of technology by the CPD which may have influenced their policing or crime-prevention. Specifically, the creation of the mounted (on horse) police squad in September of 1906 (CPD (2025)) or the adoption of cars, with the first car delivered in July of 1907 (Scott (2004)). Both of these innovations were designed for traffic control in specific areas, were relatively small in number, and were not introduced at the same time as the MCA.

Additional Behavior Metrics: Another test of policing behavior is their injury rate. If officers began to simply avoid performing important crime-preventing or pursuit functions, we expect them to be injured less following the MCA. However, Figure A.13 and Table A.1 show that there is no noticeable decline in injury rates in the department as a whole nor a

[17]Appendix B provides a model which is consistent with this result: while the threat of an arrest does deter crime, pay-per-arrest schemes can increase serious crime by 1. distracting officers with low-level arrests and hauling defendants to court and 2. false arrests reducing the deterrence effect of arrests.

relative change in Precincts 2 and 27 following the MCA.

Ethnically Disparate Effects and Anti-Irish Laws The lack of large effects on other outcomes other than low-level arrests suggests that officers did not shift enforcement to other areas or more strictly enforce different laws. Additionally, there may be a concern that officers changed how they policed which resulted in additional but unobserved costs on Chicagoans. For example, Sunday Drinking laws, specifically targeted at the immigrants, were technically in place but unenforced before the MCA (Willrich (2003)). If the change in control of the courts led to changes in police behavior other than an end in collusive arrests, it is possible different communities bore the brunt of the change. Data on the nativity of arrests shows that arrests of all main nativity groups (Americans, Irish, Germans, and Russians, which comprised almost 80% of arrests in 1906) experienced similar declines in arrests between -21% and -31%, consistent with no new targeting of the primary immigrant groups (see Figure A.12).

Heightened Legal Standards: It is possible the MCA did in fact introduce new legal standards or court procedures that independently shifted officer arrest incentives, and prior work has documented the importance of right to counsel in influencing crime and arrests (Ater et al. (2017)). In the case of Chicago, no such right was established and in practice the introduction of legal standards was "marginal" (Haller (1976)). This is particularly true in the short-run, as the court would go through major reforms and improvements in the following decade under progressive leadership (Willrich (2003)), but these were not immediate whereas the decline in arrests is.

Changing CPD Composition: The MCA and end of justice-police collusion could have produced a compositional change within the CPD, as the loss of a stream of extortive income could have changed who joined and exited the department. Importantly, being a CPD officer was still a plum job in the local patronage system, with salaries similar to those of a physician or engineer while requiring no experience or education. The lack of an effect on recruitment is supported by the results in Section 5 and Figures 2 and A.6. Furthermore,

the majority of the growth of the CPD between 1904 and 1908 occurred in precincts outside of Precincts 1, 2, and 27 even before the MCA, meaning the marginal recruits were less likely to be exposed to the bailbonds racket or impacted by its end (see Figure A.2).

9 Conclusion

This paper shows that reforming lower courts can have positive spillovers onto local police departments. The Municipal Court Act of 1905 ended the system of police courts and patronage selection of justices in Chicago, and it created Chicago's First Municipal Court with salaried judges selected by city-wide elections. This reform broke the social and political ties between the courts and the police. Using historical data collected from a range of archival sources and pre-existing datasets, I show the MCA improved police behavior by reducing the number of low-level and rent-seeking arrests without increasing crime.

Despite being a landmark Progressive Era legislation, the lessons of the Municipal Court Act have not been universally applied in the US. Today, many municipal courts are strongly influenced by or financially tied to their local police departments, influencing millions of Americans through the enforcement of minor criminal and civil infractions. While this paper provides evidence of how breaking those ties can have positive direct and spillover effects, we do not know how pervasive such problems are and which present reforms are most effective at addressing them.

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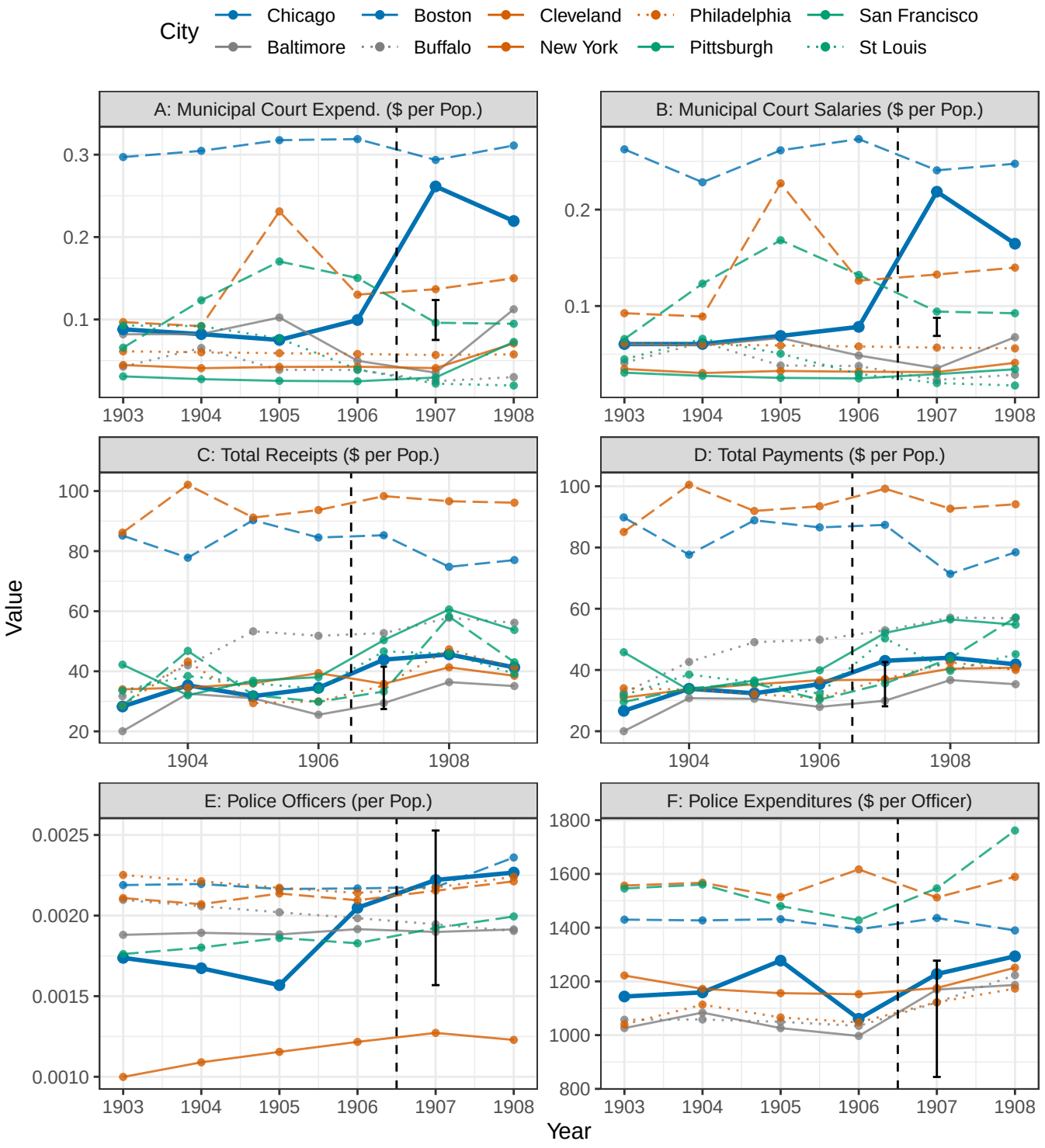
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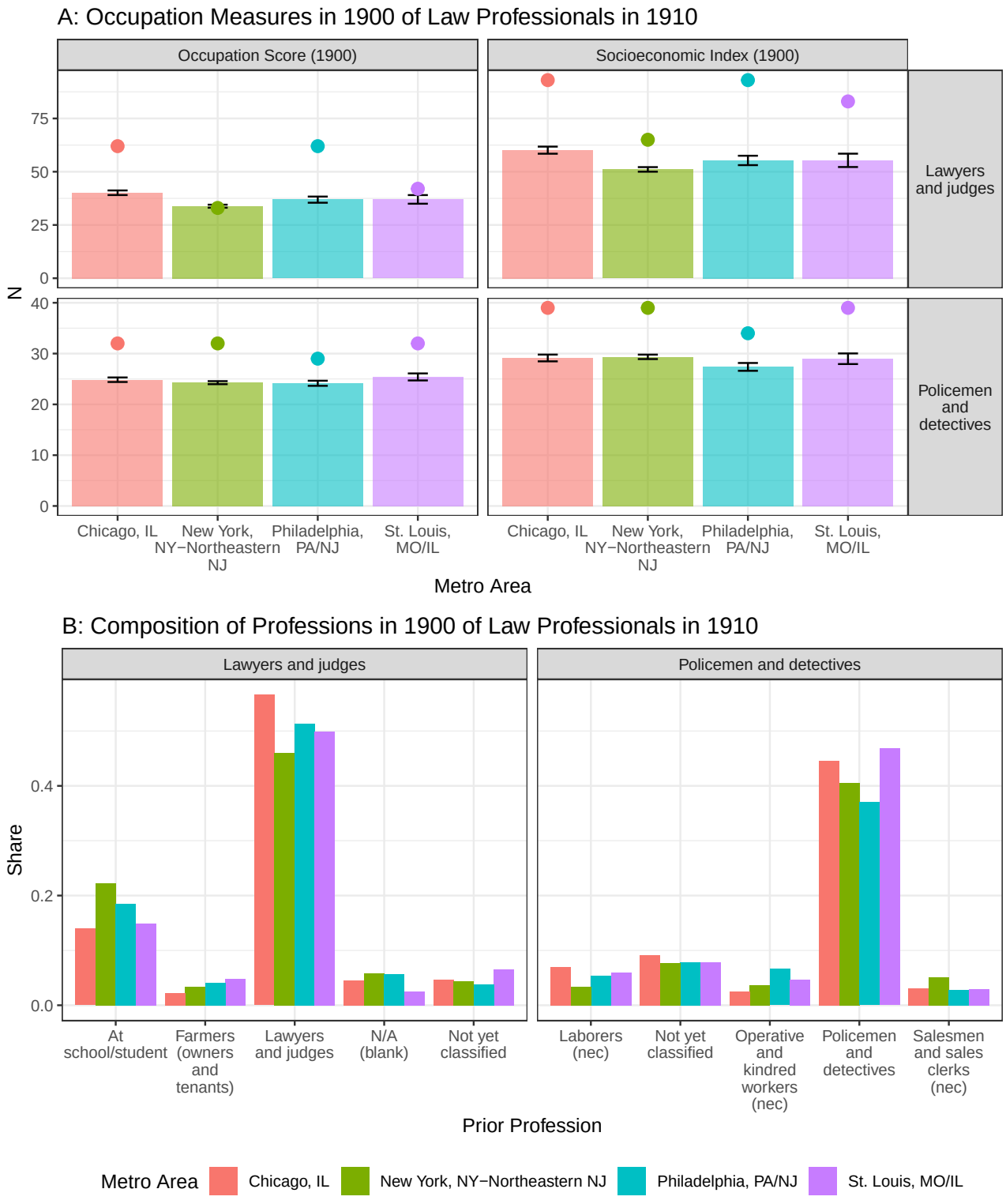
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Figure 1: Expenditures, Salaries, and Police Forces across Cities



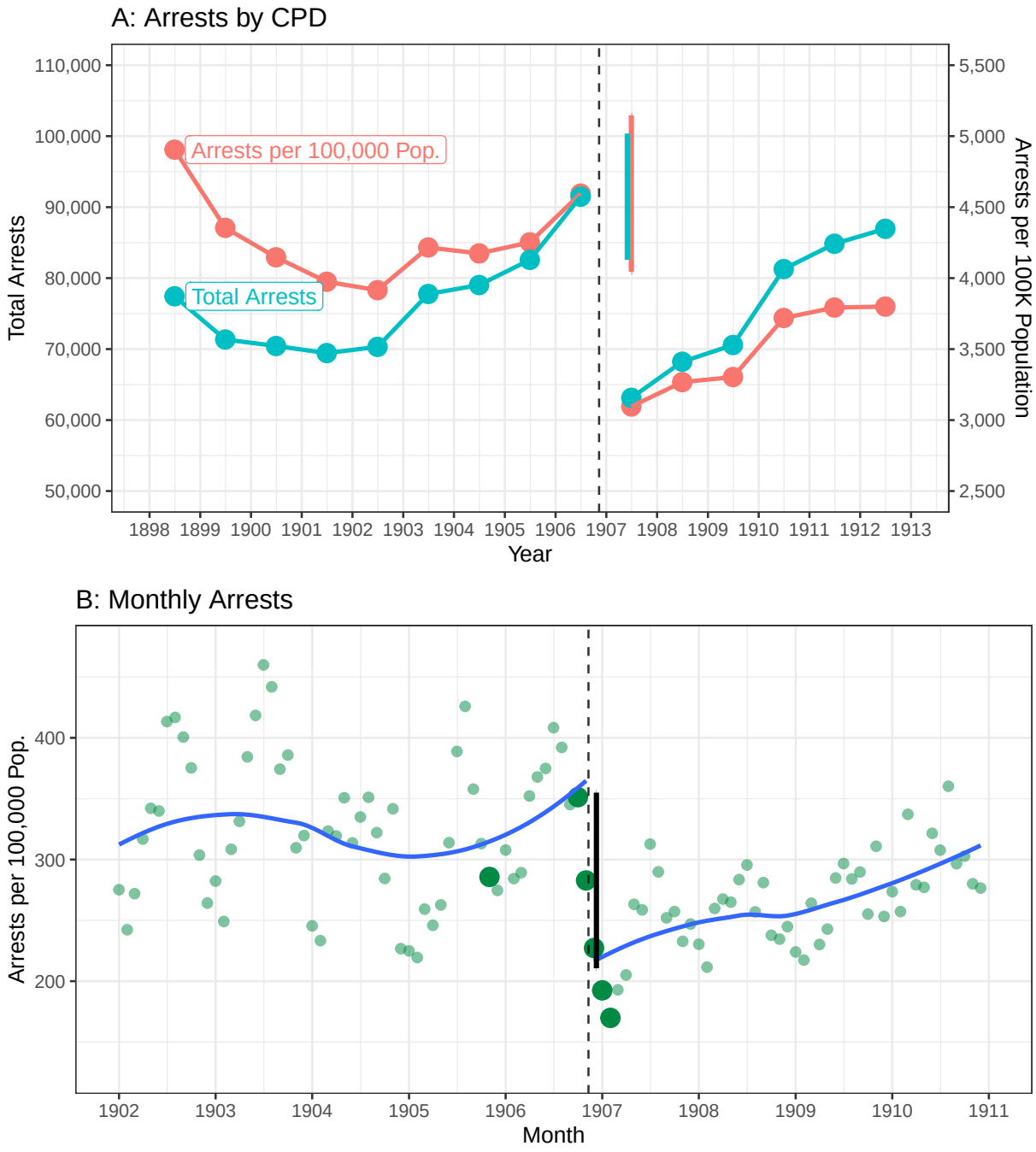
Note: Figure displays comparisons of Chicago's government annual finances compared with other major cities in the US for: total expenditures and salary-related expenditures for municipal/police courts per population, total governmental receipts and payments per population, police officers per population, and total expenditures on police per police officer. Two cities (Los Angeles and Pittsburgh) are removed from the sample for the police-related figures as they do not have police officer counts for the full period. Dashed vertical lines denote the MCA. Black error bars in 1907 denote the Manski and Pepper bounds centered at Chicago's 1906 value.

Figure 2: Prior Professions of Law Professionals in 1910



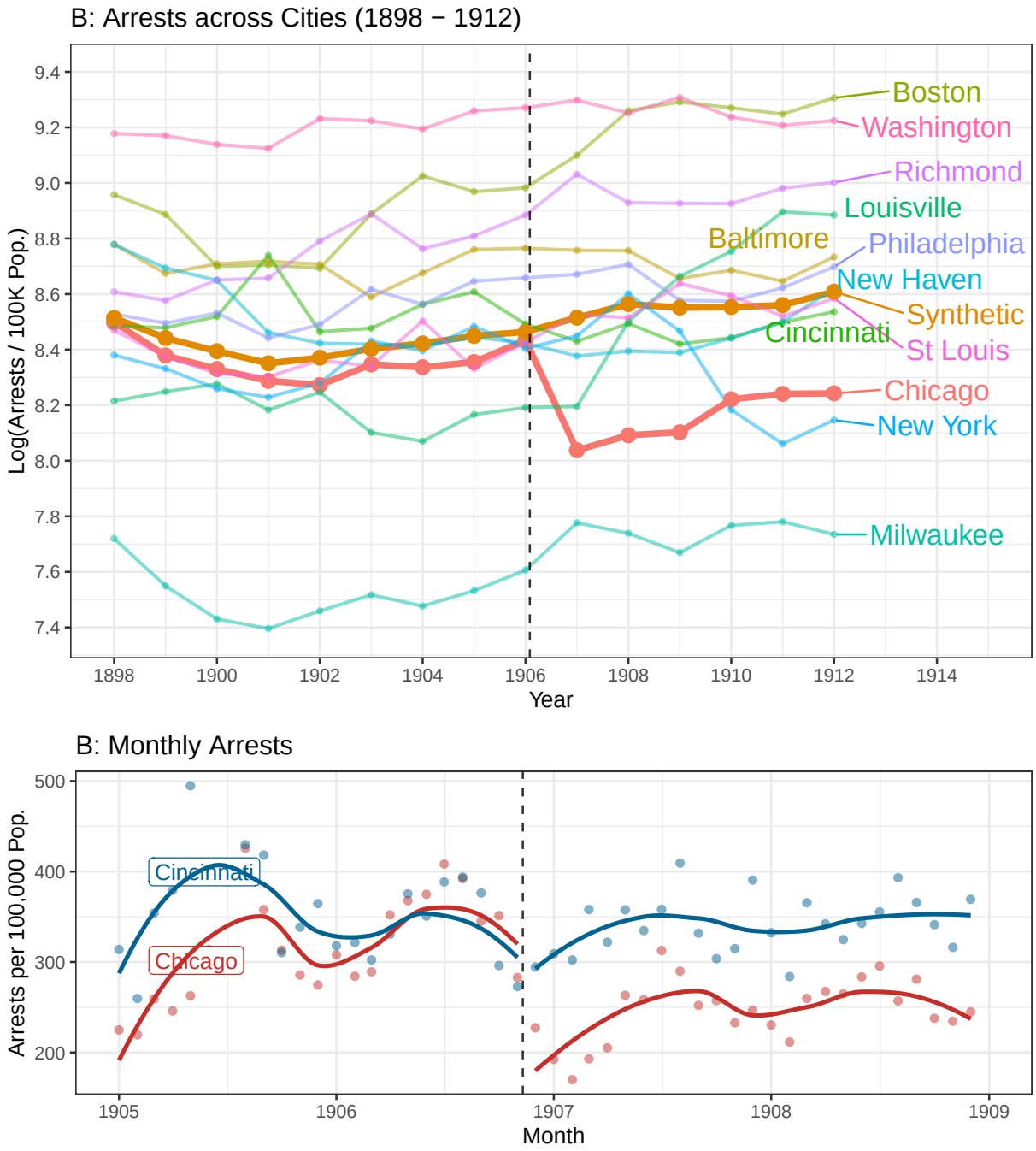
Note: Figure displays the scores and composition of the prior professions (1900) of people who were Lawyers or Judges and Policemen and Detectives in 1910, based on linked census data, for four major metropolitan areas. Bars denote the means, black error bars denote 95% confidence intervals, and points (in panel A) denote medians.

Figure 3: Arrest Rates in Chicago around the MCA



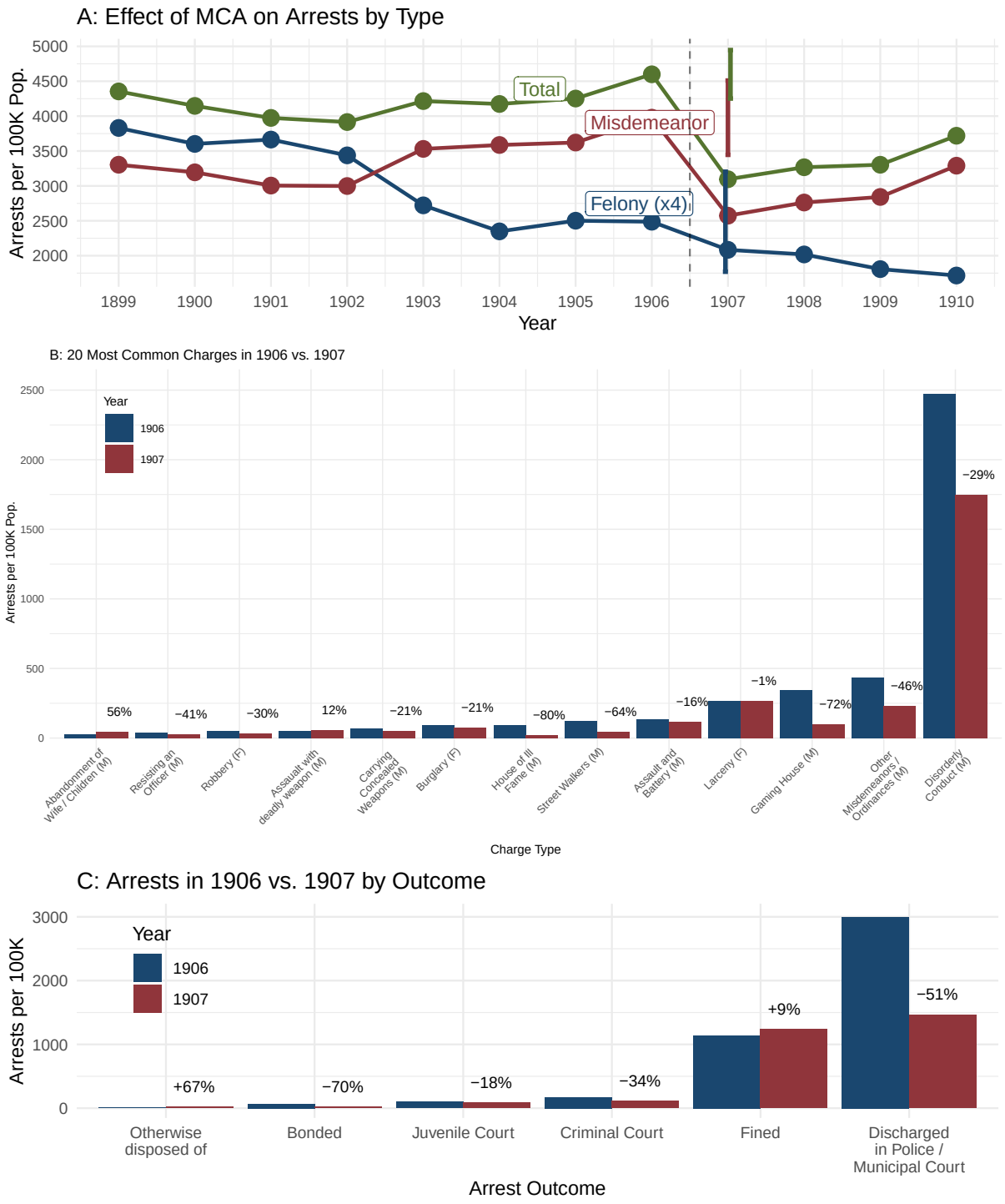
Note: Figure displays trends in arrests in Chicago between 1898 and 1912, with black vertical lines denoting the creation of the MCA in Chicago. Panel A includes total arrests (blue points, left y-axis), arrests per 100,000 population (red points, right y-axis). Panel B shows monthly arrests per 100,000 population are displayed between 1902 and 1910, with large points for the months November 1905 and October 1906 to March 1907, and a nonlinear fit line. Vertical bars in Panel A and B correspond to Manski and Pepper bounds.

Figure 4: Arrest Rates across Cities around the MCA



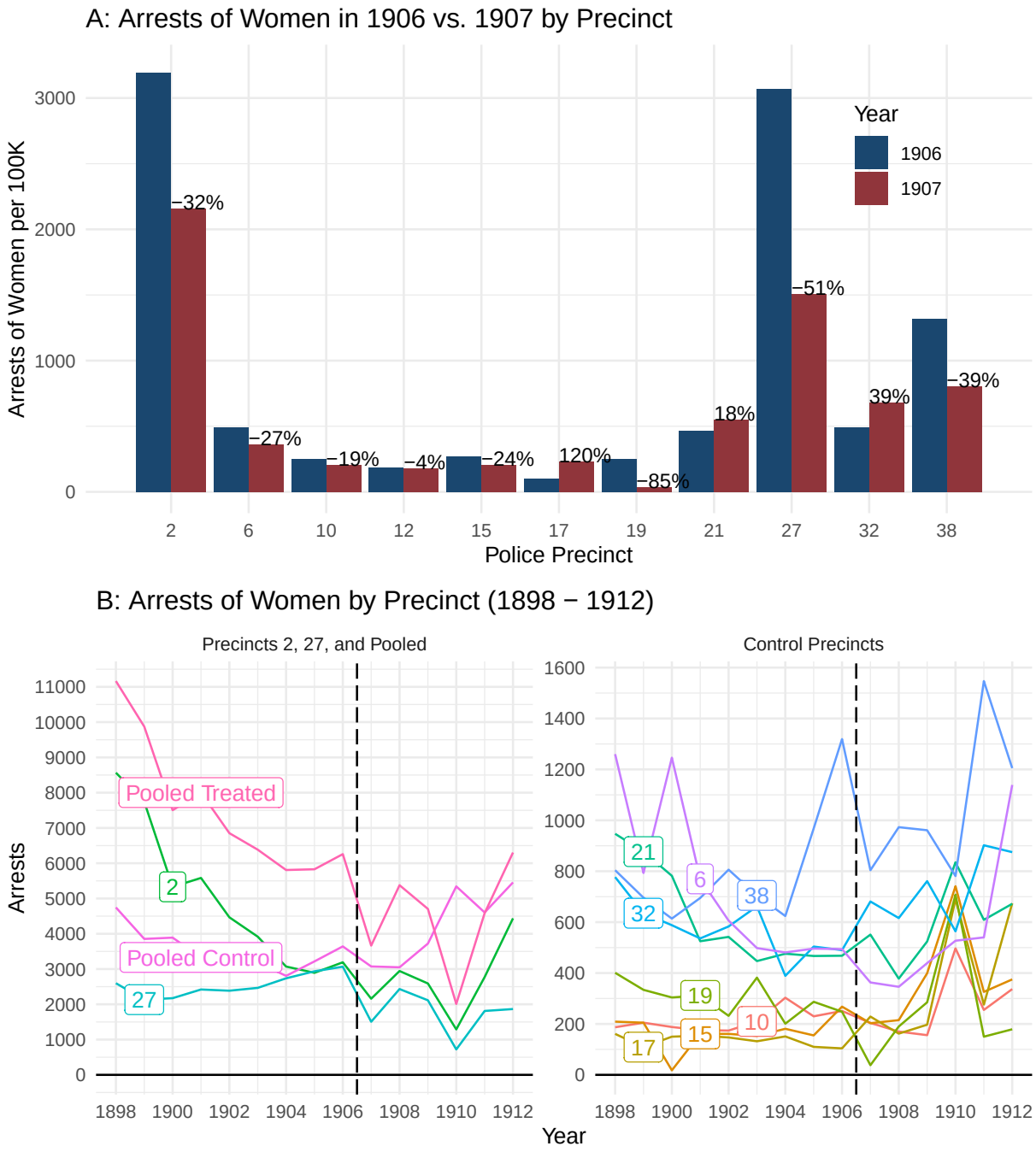
Note: Figure displays trends in logged arrests per 100,000 population in Chicago and other major cities. Panel A displays annual arrests between 1898 and 1912 compared with 11 other major cities, and Panel B displays monthly arrests between 1905 and 1908 compared with Cincinnati, with black vertical line denotes the creation of the MCA in Chicago. Panel A also displays the Synthetic Chicago constructed for the synthetic difference-in-differences analysis (equation (3)) as a weighted average of the arrests in non-Chicago cities. Panel B contains a nonlinear fit for each city's arrests before and after the MCA.

Figure 5: Change in Arrest Composition



Note: Figure displays trends in arrests by type (total = misdemeanor + felony) in Chicago between 1899 and 1910 in Panel A along with Manski and Pepper bounds as colored bars, changes in the 20 most common charges of arrests in 1906 vs 1907 in Panel B, and changes in the outcomes of arrests in 1906 and 1907 in Panel C.

Figure 6: Change in Arrests of Women by Precinct



Note: Figure displays the changes in arrests of women between 1906 and 1907 by Precinct as reported in the Matron's Reports in Panel A, and the trends in arrests of women by precinct between 1898 and 1912 in Panel B. The left side of Panel B displays trends for the 'treated' precincts (2 and 27), their pooled arrest counts, and the pooled arrest counts of the 'control' (all other) precincts, and the precinct-specific values of the control precincts are displayed in the right side.

Table 1: Effect of MCA on Government Finances (1903-1908)

	Municipal Court Expenditures ($ per Pop.)	Municipal Court Expenditures on Salaries ($ per Pop.)	Total Receipts ($ per Pop.)	Total Payments ($ per Pop.)	Police Officers (per 1,000 Pop.)	Police Expenditures ($ per Officer)
	(1)	(2)	(3)	(4)	(5)	(6)
A: TWFE						
MCA	0.18***	0.15***	5.1	1.8	0.22	29
	(0.02)	(0.02)	(3.02)	(3.28)	(0.17)	(89.65)
% Change	178%	169%	10%	4%	11%	2%
Mean(Y)	0.1	0.086	49	49	1.9	1272
N Obs.	60	60	70	70	48	48
N. Cities	10	10	10	10	8	8
Frequency	Annual	Annual	Annual	Annual	Annual	Annual
Range	1903-1908	1903-1908	1903-1908	1903-1908	1903-1908	1903-1908
B: SDID						
MCA	0.15***	0.14***	3.2	2.5	0.34***	53
	(0.03)	(0.02)	(10.78)	(9.82)	(0.08)	(59.42)
% Change	150%	158%	7%	5%	17%	4%
Mean(Y)	0.1	0.086	49	49	1.9	1272
N Obs.	60	60	70	70	48	48
N. Cities	10	10	10	10	8	8
Frequency	Annual	Annual	Annual	Annual	Annual	Annual
Range	1903-1908	1903-1908	1903-1908	1903-1908	1903-1908	1903-1908

Note: Table displays the regression estimates for the effect of the MCA on Chicago's government annual finances compared with other major cities in the US. Panel A displays TWFE estimates (equation (2)) including city-specific linear time trends, and Panel B displays synthetic difference-in-differences estimates (equation (3)). Columns (1) and (2) use total expenditures and salary-related expenditures for municipal/police courts per population; Columns (3) and (4) use total governmental receipts and payments per population; Column (5) uses police officers per 1,000 population; Column (6) uses total expenditures on police per police officer. Two cities (Los Angeles and Pittsburgh) are removed from the sample for the police-related regressions as they do not have police officer counts for the full period. *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Effect of MCA on Arrests

	Total Arrests (1,000)	Log(Arrests / 100K)							
	ITS	ITS			TWFE			SDID	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
MCA	-37.89***	-0.48***	-0.44***	-0.53***	-0.46***	-0.4***	-0.5***	-0.43***	-0.37
	(3.27)	(0.04)	(0.08)	(0.09)	(0.04)	(0.06)	(0.05)	(0.1)	(0.25)
% Change	-50%	-38%	-36%	-41%	-37%	-33%	-39%	-35%	-31%
Mean(Y)	76.33	8.28	5.67	5.68	8.46	8.54	5.73	8.54	8.54
N Obs.	15	15	108	103	130	165	96	150	180
N. Cities	1	1	1	1	13	11	2	15	12
Frequency	Annual	Annual	Monthly	M. Donut	Annual	Annual	Monthly	Annual	Annual
Range	1898-1912	1898-1912	1902-1910	1902-1910	1898-1907	1898-1912	1905-1908	1898-1907	1898-1912

Note: Table displays estimates of the effect of the MCA on arrests. Columns (1)-(4) display time series regression estimates (equation (1)) controlling for patrol officers (transformed in same way as outcome), Column (5)-(7) display difference-in-differences estimates recovered from using two-way fixed effect regressions (equation (2)) controlling for patrol officers (transformed in same way as outcome), Columns (8)-(9) display synthetic difference-in-differences estimates (equation (3)). % Change refers to the point estimate transformed to recover an effect in percent change (if logged) or as a percent of the pre-MCA mean of Chicago (if non-logged); Mean(Y) refers to the pre-MCA mean of the dependent variable for Chicago. Monthly donut uses monthly arrest data but excludes five months around the MCA, from October 1906 to February 1907. Column (7) compares Chicago and Cincinnati at the monthly level. Standard errors are in parenthesis. SDID standard errors are computed with the placebo method, all others are computed using Newey-West standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Effect of MCA on Log(Arrests / 100K Pop.)

	ITS			TWFE	TWFE
	Total	Misdemeanor	Felony	Misdemeanors (vs. Felonies)	Misdemeanor in Chicago (vs. Misdemeanors in St. Louis)
	(1)	(2)	(3)	(4)	(5)
MCA	-0.49***	-0.55***	-0.12**	-0.38***	-0.66***
	(0.03)	(0.03)	(0.04)	(0.06)	(0.04)
% Change	-39%	-42%	-11%	-31%	-48%
Mean(Y)	8.27	8.07	6.47	7.27	8.19
N Obs.	12	12	12	24	10
N. Units	1	1	1	2	2
Range	1899-1910	1899-1910	1899-1910	1899-1910	1904-1908

Note: Table displays estimates of the effect of the MCA on logged arrests per 100,000 population by type. Columns (1)-(3) display time series regression estimates (equation (1)) controlling for logged patrol officers per 100,000 population, Columns (4)-(5) display difference-in-differences estimates recovered from using two-way fixed effect regressions (equation (2)). Column (4) displays the effect of the MCA on misdemeanors in Chicago relative to felonies, and Column (5) displays the effect of the MCA on misdemeanors in Chicago relative to misdemeanors (city offenses) in St. Louis. % Change refers to the point estimate transformed to recover an effect in percent change. Standard errors are in parenthesis and are computed using Newey-West standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effect of MCA on Women's Arrests in Precincts 2 and 27 vs. Others

	TWFE			SDID	
	Total	Logged	Logged (Precinct Trends)	Total	Logged
	(1)	(2)	(3)	(4)	(5)
A: Long Run (1898 - 1912)					
MCA	-112.88***	-0.73***	-0.41	-62.81***	-0.67*
	(30.9)	(0.19)	(0.26)	(5.5)	(0.38)
% Change	-53%	-52%	-34%	-117%	-49%
Mean(Y)	214.36	3.37	3.37	53.69	3.37
N Obs.	150	150	150	150	150
N. Units	10	10	10	10	10
B: Short Run (1898 - 1907)					
MCA	-104.54**	-0.65*	-0.29	-71.77***	-0.37
	(37.27)	(0.34)	(0.35)	(7.6)	(0.72)
% Change	-49%	-48%	-25%	-134%	-31%
Mean(Y)	214.36	3.38	3.38	53.69	3.37
N Obs.	100	100	100	100	100
N. Units	10	10	10	10	10

Note: Table displays estimates of the effect of the MCA in 'treated' precincts on arrests of women using panel data at the precinct-year level. Treated precincts are 2 and 27 and control precincts are all others in the Matron's Reports which have values for all years used (6, 10, 15, 17, 19, 21, 32, 38). Panel A focuses on long-run effects stopping the panel data at 1912, and Panel B focuses on short-run effects stopping the panel data at 1907. Columns (1)-(3) display difference-in-differences estimates recovered from using two-way fixed effect regressions (equation (2)), with Column (3) including precinct-specific linear time trends as a control. Columns (1)-(3) control for reported number of patrol officers in each precinct-year (logged if outcome is logged). Columns (4)-(5) display synthetic difference-in-differences estimates (equation (3)). % Change refers to the point estimate transformed to recover an effect in percent change (if logged) or as a percent of the overall mean (if non-logged). Standard errors are in parenthesis. SDID standard errors are computed with the placebo method, all others are computed using Newey-West standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of MCA on Property Crime

	Log($ of Property Stolen / 100K)				
	TWFE		ITS		
	(1)	(2)	(3)	(4)	(5)
MCA	-0.23***	-0.04	-0.08	0.07	0.06
	(0.07)	(0.09)	(0.12)	(0.1)	(0.09)
% Change	-21%	-4%	-8%	7%	6%
Mean(Y)	10.05	9.94	10.13	7.64	8.75
N Obs.	43	16	16	84	28
N. Cities	4	4	1	1	1
Frequency	Annual	Annual	Annual	Monthly	Quarterly
Range	1898-1912 (Unbalanced)	1905-1908 (Balanced)	1898-1912	1902-1908	1902-1908

Note: Table displays estimates of the effect of the MCA on the amount of stolen property within Chicago and Chicago compared with other cities (Boston, Buffalo, St. Louis). Columns (1)-(2) display difference-in-differences estimates recovered from using two-way fixed effect regressions (equation (2)) controlling for patrol officers (transformed in same way as outcome) using annual panel data, and Columns (3)-(5) display time series regression estimates (equation (1)) controlling for patrol officers (transformed in same way as outcome) at the annual, monthly, and quarterly level. % Change refers to the point estimate transformed to recover an effect in percent change (if logged) or as a percent of the overall mean (if non-logged). Newey-West standard errors are in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effect of MCA on Murder across Cities

	Log(Murders / 100K)					
	TWFE					SDID
	(1)	(2)	(3)	(4)	(5)	(6)
MCA	-0.16*	-0.43***	-0.39***	-0.62***	-0.44***	-0.01
	(0.09)	(0.14)	(0.13)	(0.19)	(0.1)	(0.55)
% Change	-15%	-35%	-32%	-46%	-36%	-1%
Mean(Y)	1.85	2.2	1.79	1.79	2.08	2.08
N Obs.	46	69	57	57	76	76
N. Cities	2	3	3	3	4	4
Range	1890-1912	1890-1912	1890-1908	1890-1908	1890-1908	1890-1908
Controls	Log(POs)	Lin. City	Log(POs)	Log(POs) + Lin. City	Lin. City	

Note: Table displays estimates of the effect of the MCA on murders using city-by-year panel data. Column (1)-(5) display difference-in-differences estimates recovered from using two-way fixed effect regressions (equation (2)), and Column (6) displays synthetic difference-in-differences estimates (equation (3)). The cities included are Chicago, Cincinnati, Los Angeles, and San Francisco. Los Angeles is excluded in any regressions with police officer controls (Columns (1), (3), (4)), and Cincinnati only has murder data between 1890 - 1908 so is excluded from Columns (1)-(2). % Change refers to the point estimate transformed to recover an effect in percent change. Standard errors are in parenthesis. SDID standard errors are computed with the placebo method, all others are computed using Newey-West standard errors. *** p<0.01, ** p<0.05, * p<0.1.

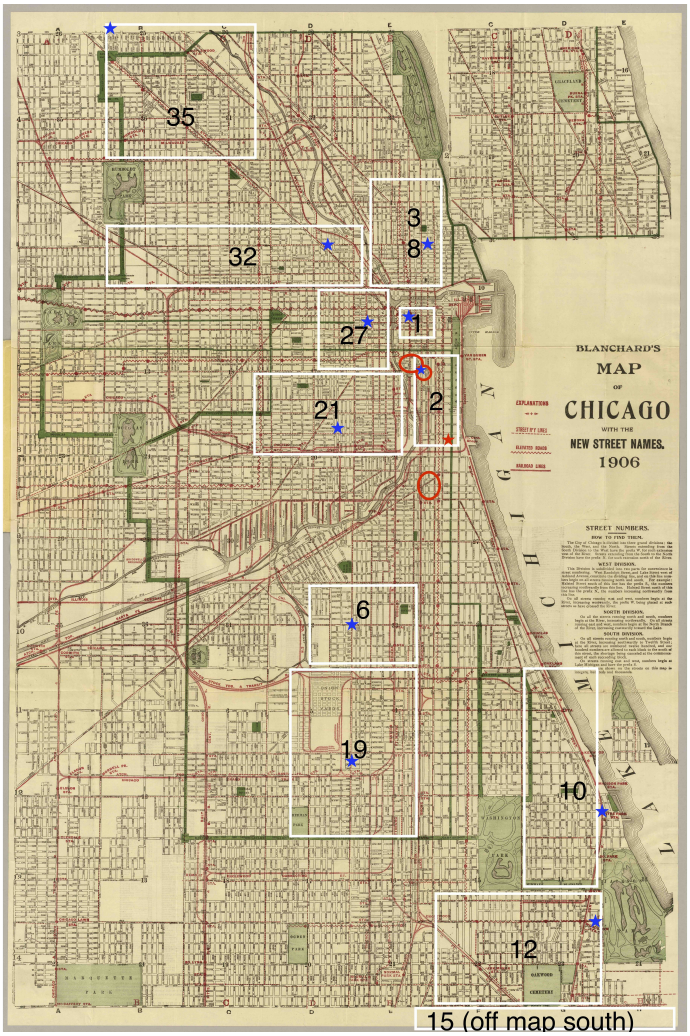
Table 7: Effect of MCA on Murder across Precincts

	Murders				
	TWFE				SDID
	(1)	(2)	(3)	(4)	(5)
MCA	-0.21	-0.55	-0.22	-0.48	-0.12
	(0.27)	(0.34)	(0.41)	(0.5)	(0.17)
% Change	-44%	-117%	-23%	-50%	-28%
Mean(Y)	0.47	0.47	0.95	0.95	0.43
N Obs.	1232	1232	308	308	1344
N. Precincts	44	44	11	11	48
Range	1902-1908	1902-1908	1902-1908	1902-1908	1902-1908
Controls	POs	POs + Lin. Precinct	POs	POs + Lin. Precinct	

Note: Table displays estimates of the effect of the MCA on murders using precinct-by-quarter panel data. Column (1)-(4) display difference-in-differences estimates recovered from using two-way fixed effect regressions (equation (2)) controlling for the total number of patrol officers, and Column (5) displays synthetic difference-in-differences estimates (equation (3)). Columns (2) and (4) include precinct-specific linear time trends. Columns (1)-(2) use precincts with non-missing patrol officer counts and define Precincts 1, 2, and 27 as treated; Columns (3)-(4) use precincts in the Matron's data with non-missing patrol officer counts (2,6,10,12,15,17,19,21,27,32,38) and define Precincts 2 and 27 as treated; Column (5) uses all precincts. % Change refers to the point estimate divided by the mean of the outcome. Standard errors are in parenthesis. SDID standard errors are computed with the placebo method, all others are computed using Newey-West standard errors. *** p<0.01, ** p<0.05, * p<0.1.

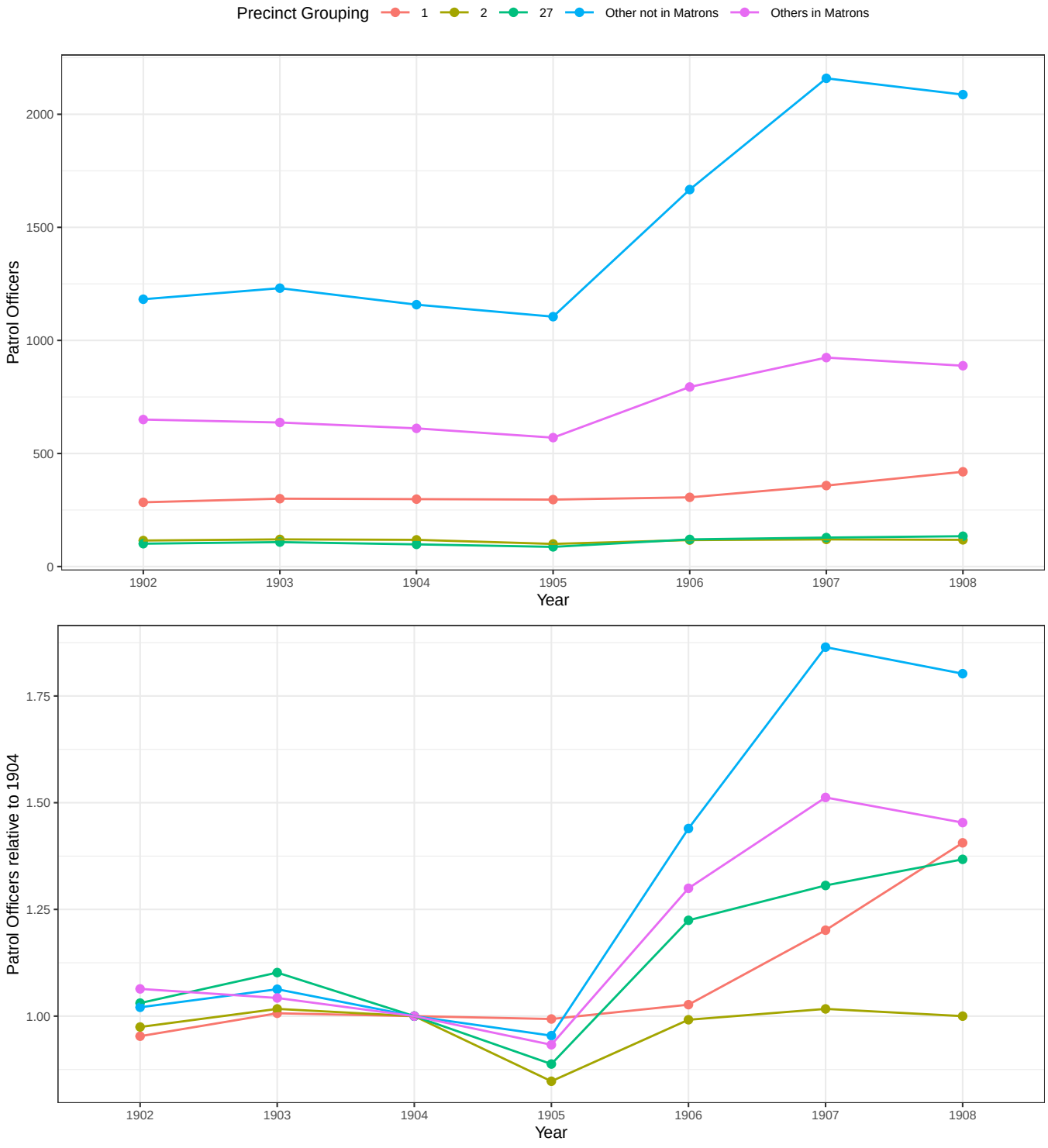
A Additional Figures and Tables

Figure A.1: Edited Map of Blanchard's Map of Chicago (1906)



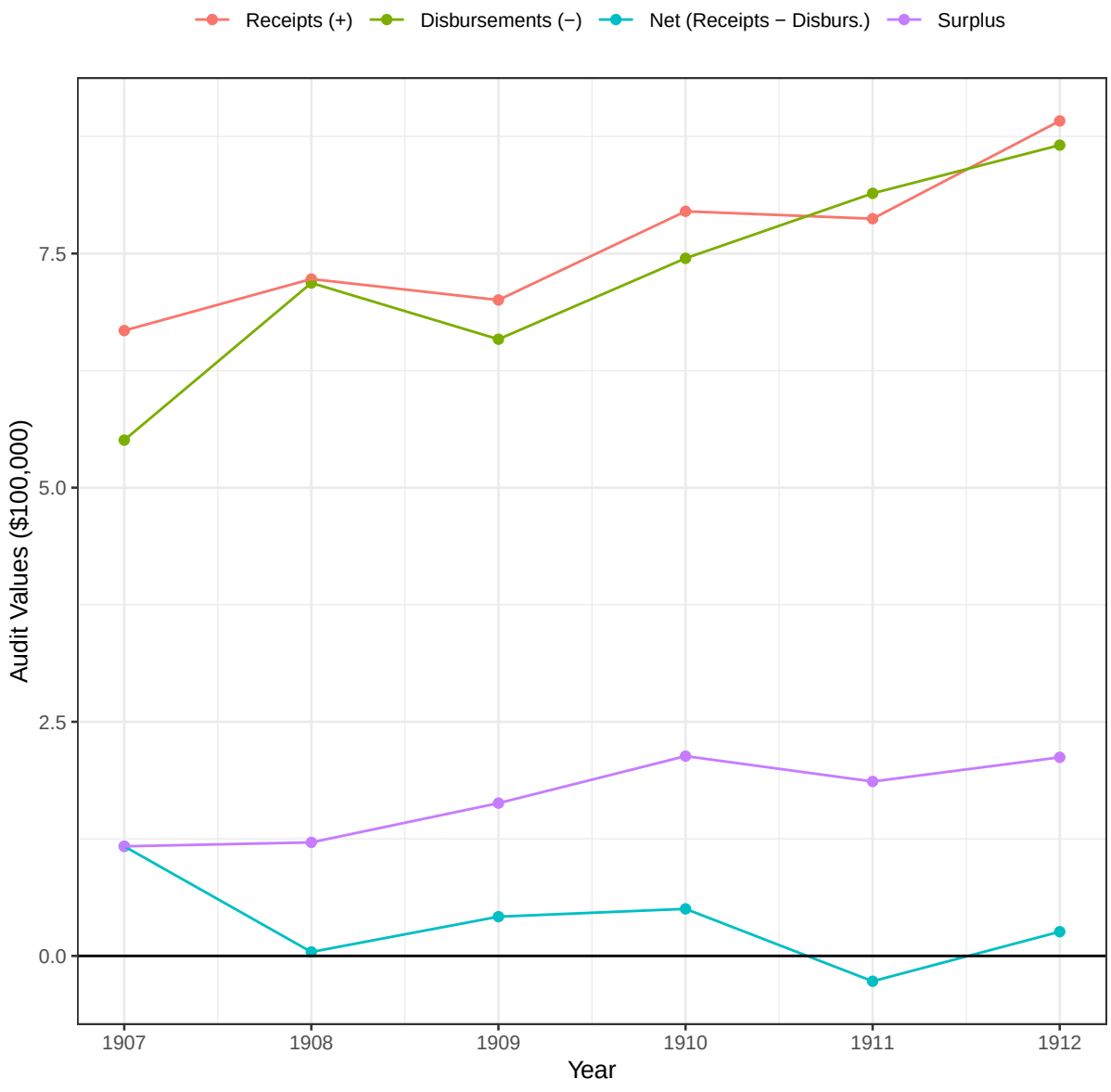
Note: Figure displays an edited version of Blanchard's Map of Chicago (1906). While boxes denote approximate police precinct locations of the 1st precinct and the precincts contained in the Matron's data, based on 1906 police reports which discuss precinct boundaries. Blue stars denote approximate locations of precinct police stations. Red circles denote locations of Levee's activities, with the northern circles referring to pre-1906 activities and the southern circle is related to 1910 activity. The red star refers to the location of the Chicago Coliseum, which hosted the First Ward Ball until 1908 (see Suskin (2010)).

Figure A.2: Precinct-Level Changes in Patrolmen



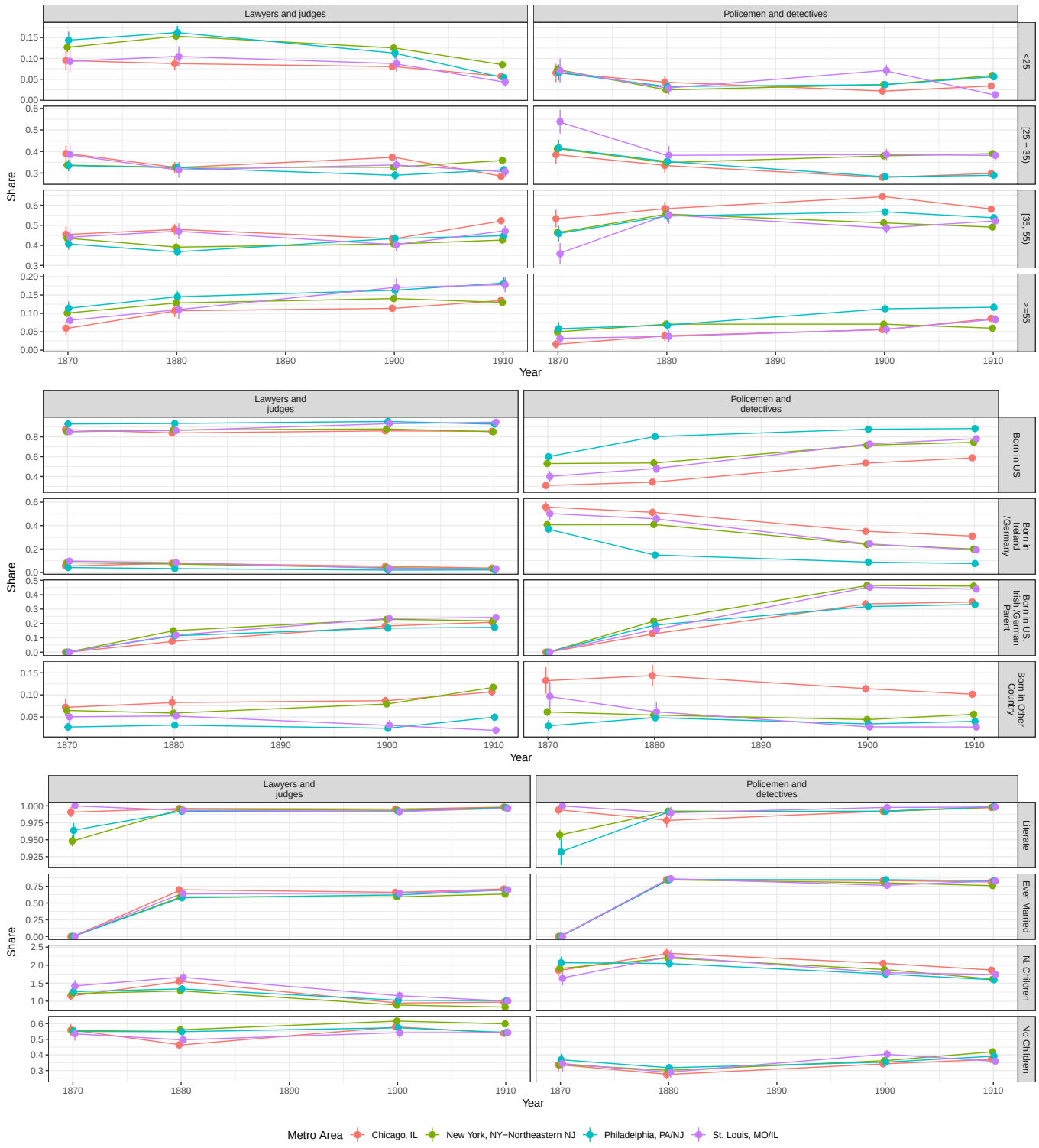
Note: Figure displays groupings of counts of patrol officers in each precinct by year in Panel A, and Panel B displays the number of patrol officers relative to the number in 1904. Groups are Precincts 1, 2, and 27, and all other precincts in the Matron's Data (6, 10, 12, 15, 17, 19, 21, 32, 38), and all other precincts not in the Matron's data (3-5, 7-9, 11, 13, 14, 16, 18 20 22-26, 28-31, 33-37, 39-45.

Figure A.3: Municipal Court Audit Results



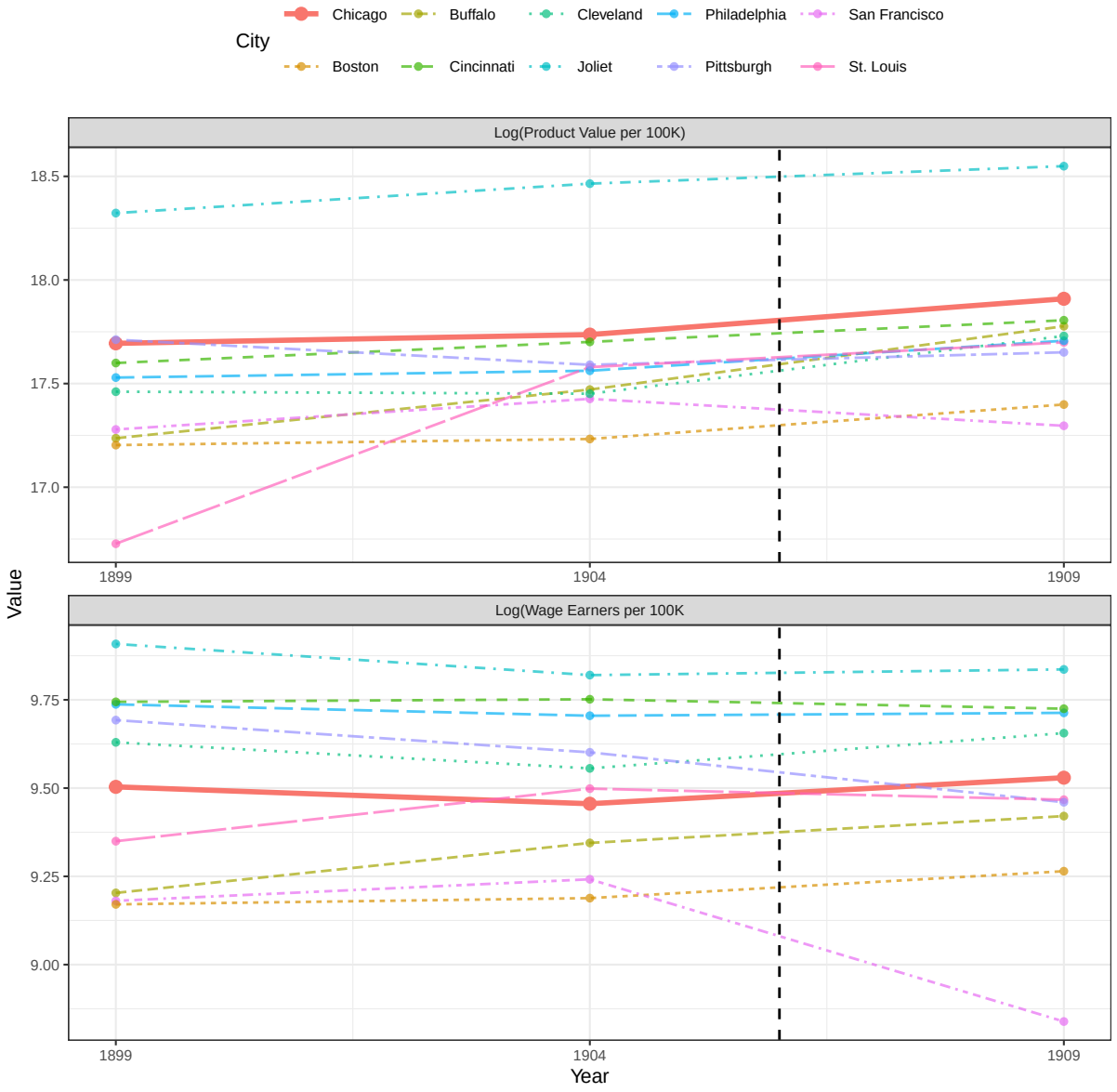
Note: Figure displays audit report results from the Municipal Court of Chicago from the 1907 - 1912 reports.

Figure A.4: Composition of Professions Over Time



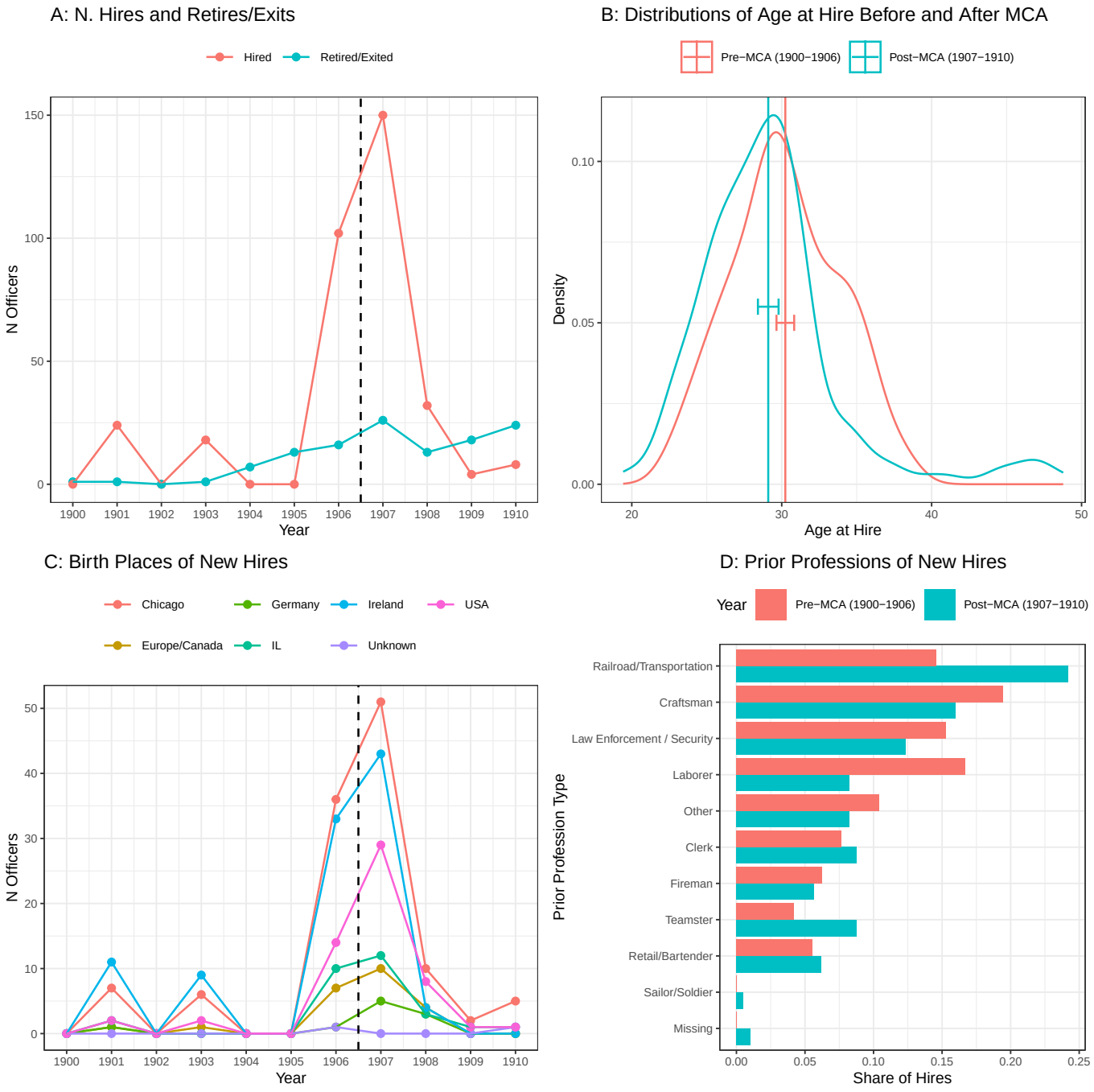
Note: Figure displays the average characteristics (generally shares) of Lawyers and Judges and Policemen and Detectives in Chicago, New York, Philadelphia, and St. Louis drawn from the 1870, 1880, 1900, and 1910 censuses. The top panel shows age composition, middle panel shows immigration and nativity information, and the bottom panel shows literacy and family composition.

Figure A.5: Manufacturing Production and Employment



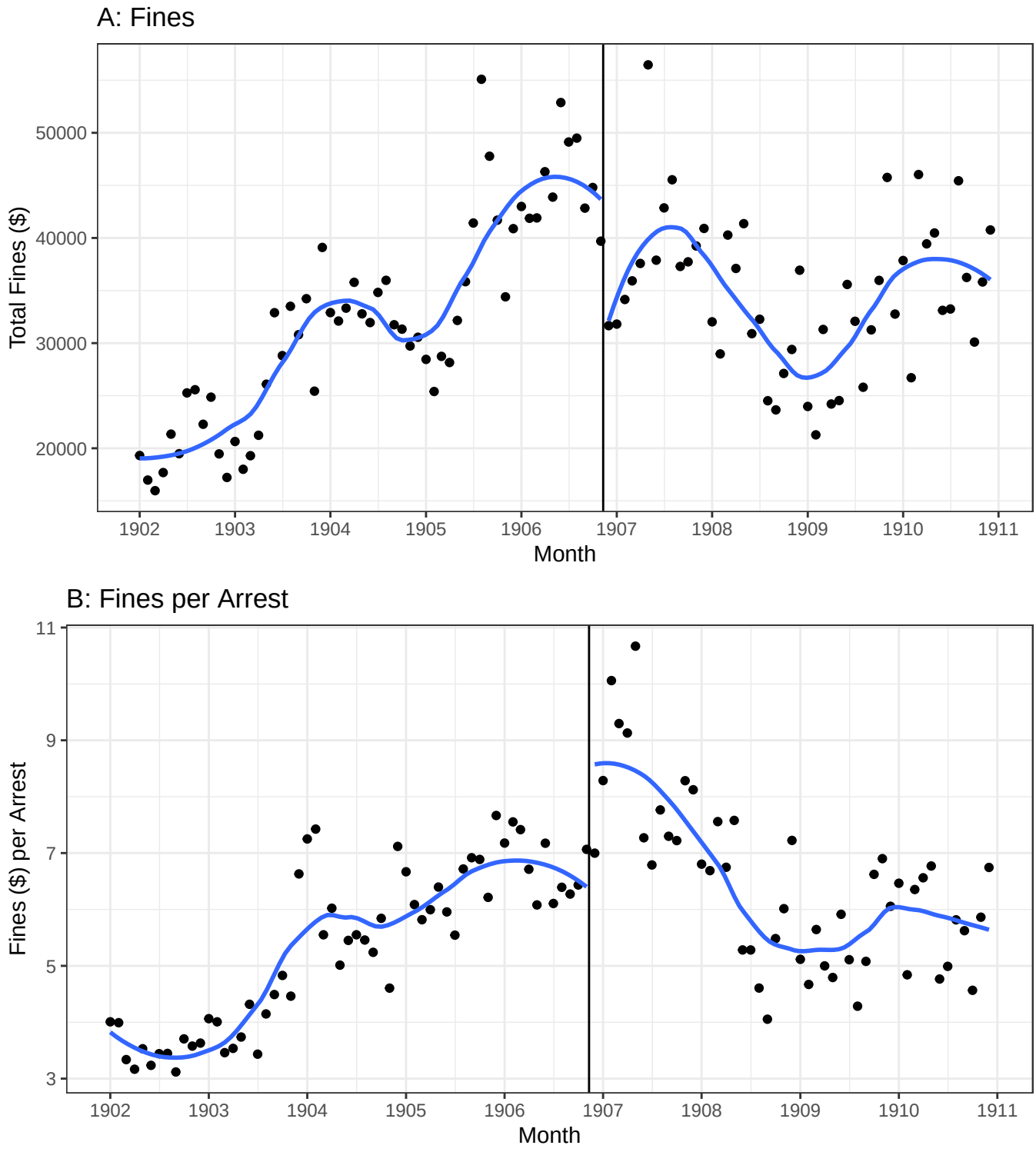
Note: Figure displays logged values of manufacturing product value and number of wage earners per 100,000 population based on the 1910 Census manufacturing chapter for Chicago and other major cities.

Figure A.6: Change in CPD Composition across Outcomes



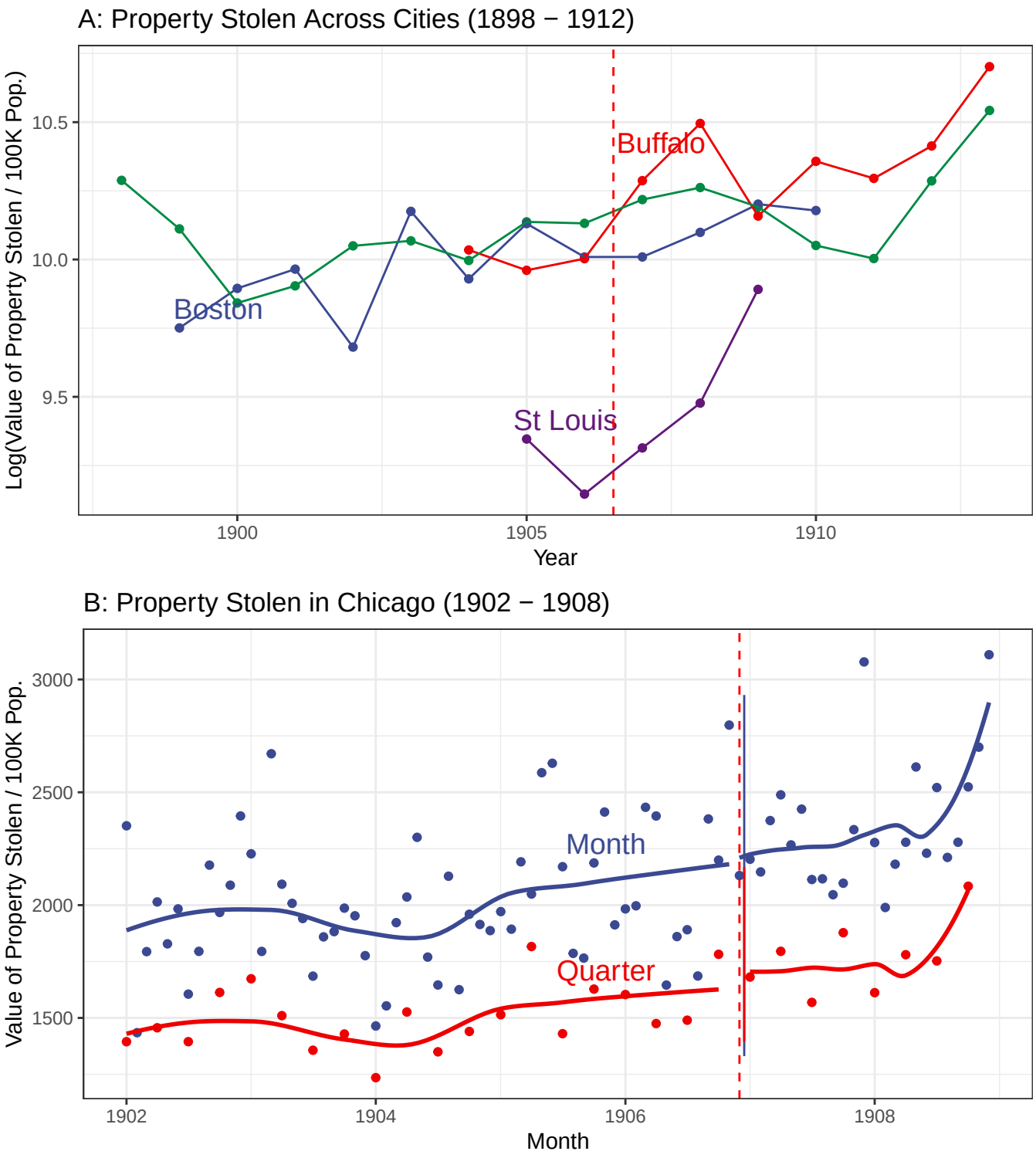
Note: Figure displays changes in Chicago Police Department composition based on registry records, which is composed of an initial sample of 787 CPD officers active between 1904 and 1910. Panel A displays new hires and retirements/exits by year. Panel B displays the distribution of ages at the date of being hired for those hired before and after the MCA, where the vertical lines denote the means and the error bars denote 95% confidence intervals. Panel C displays the counts of new hires by year by place of birth. Panel D displays the share of new hires before and after the MCA by the new hire's prior profession (grouped into 11 categories). Vertical dashed lines denote the MCA.

Figure A.7: Fines in Chicago Over Time



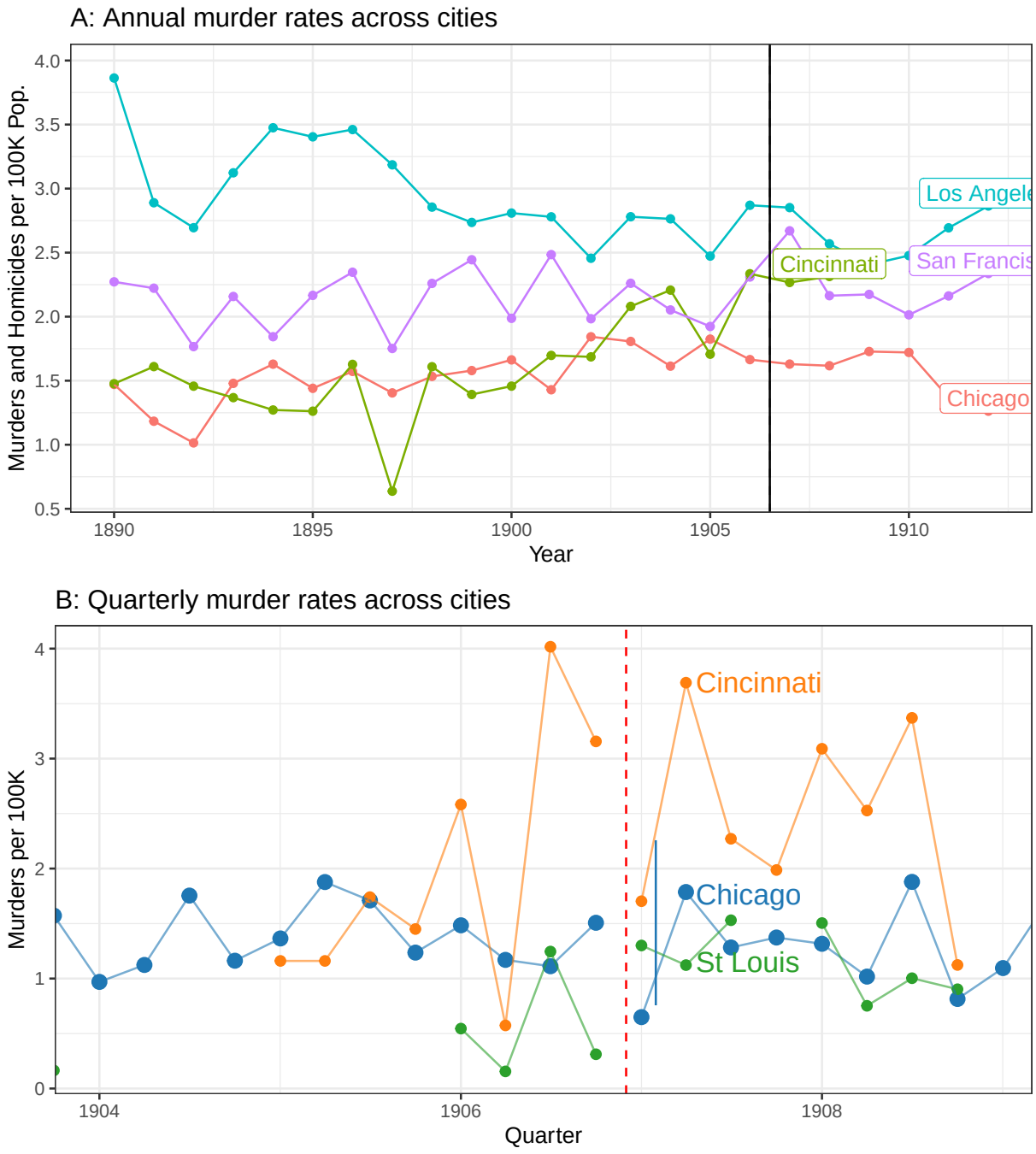
Note: Figure displays total amount of fines (Panel A) and average fine per arrest (Panel B) in Chicago per month between 1902 to 1911, based on annual reports. Blue lines denote nonlinear fits pre- and post-MCA, with black vertical lines denoting the MCA.

Figure A.8: Time Series of Property Crime



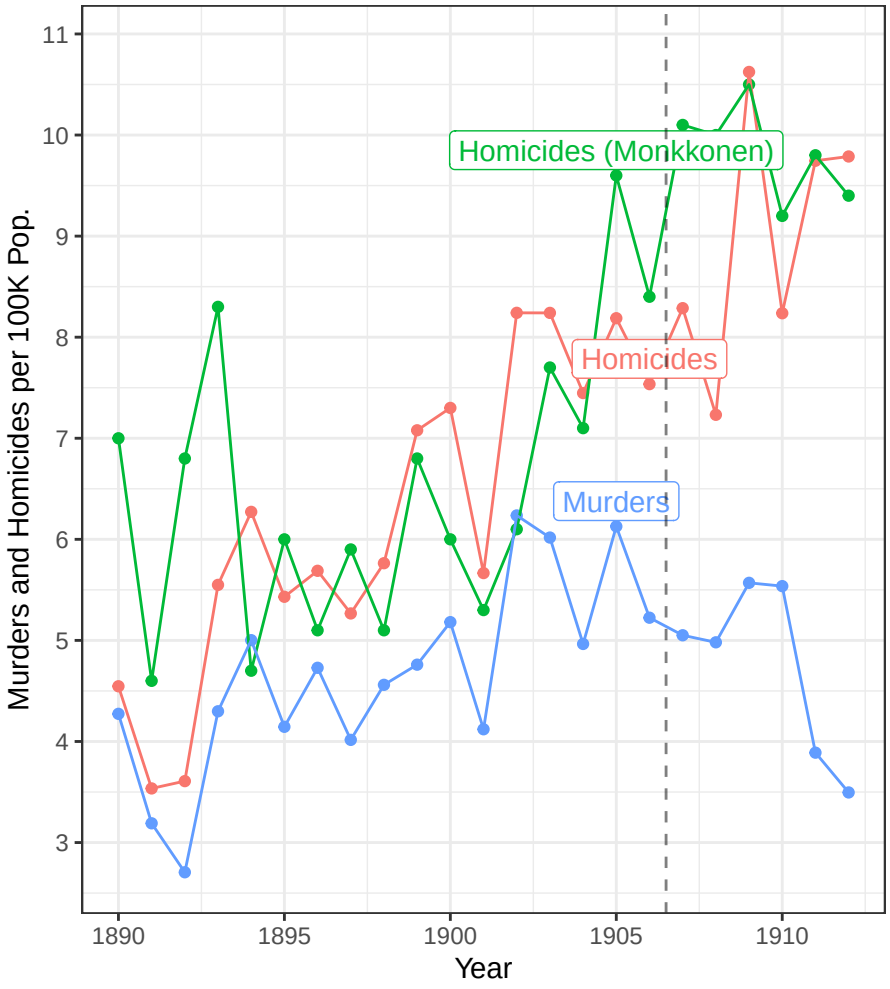
Note: Figure displays trends in the amount of property stolen across cities (Panel A) as recovered from annual police reports and within Chicago at the monthly and quarterly level (Panel B). Panel B contains colored bars denoting Manski and Pepper bounds. Red vertical dashed lines denote the MCA.

Figure A.9: Murder Rates across Cities



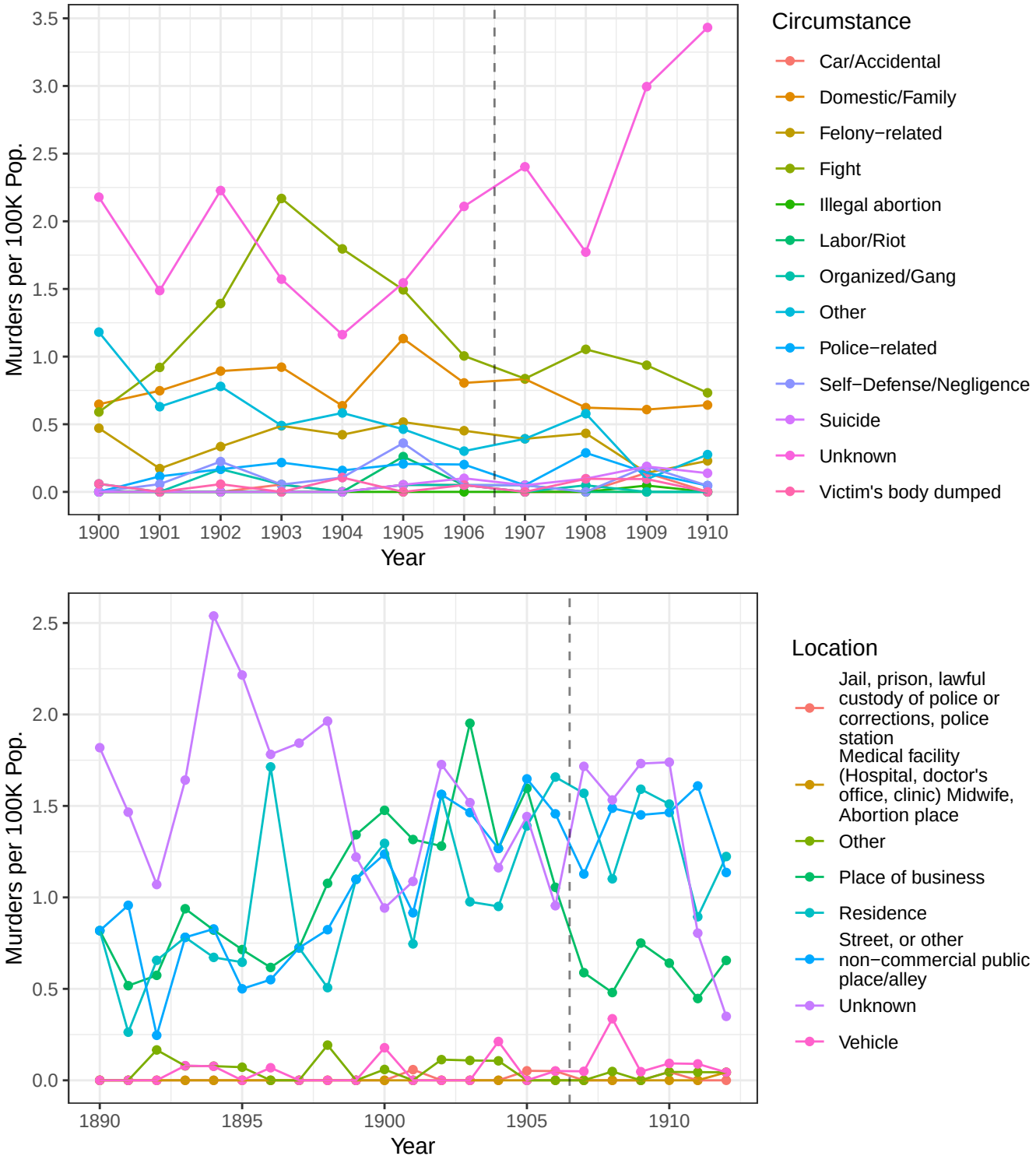
Note: Figure displays murders at the annual (Panel A) and quarterly (Panel B) using different metrics and sources. Panel A compares the Chicago murders (Bienen 2012 data) with Los Angeles murders (Monkkonen 2005), San Francisco (Mullen 2005), and Cincinnati murder arrests (annual reports). Panel B compares quarterly murder rates in Chicago (Bienen 2012 data) with quarterly Cincinnati murder arrests (annual reports) and quarterly St. Louis murders (constructed from scraped Missouri Birth and Death Records Database Pre-1910). Panel B also includes the Manski and Pepper bounds as the colored bar for Chicago.

Figure A.10: Time Series of Murder and Homicide in Chicago



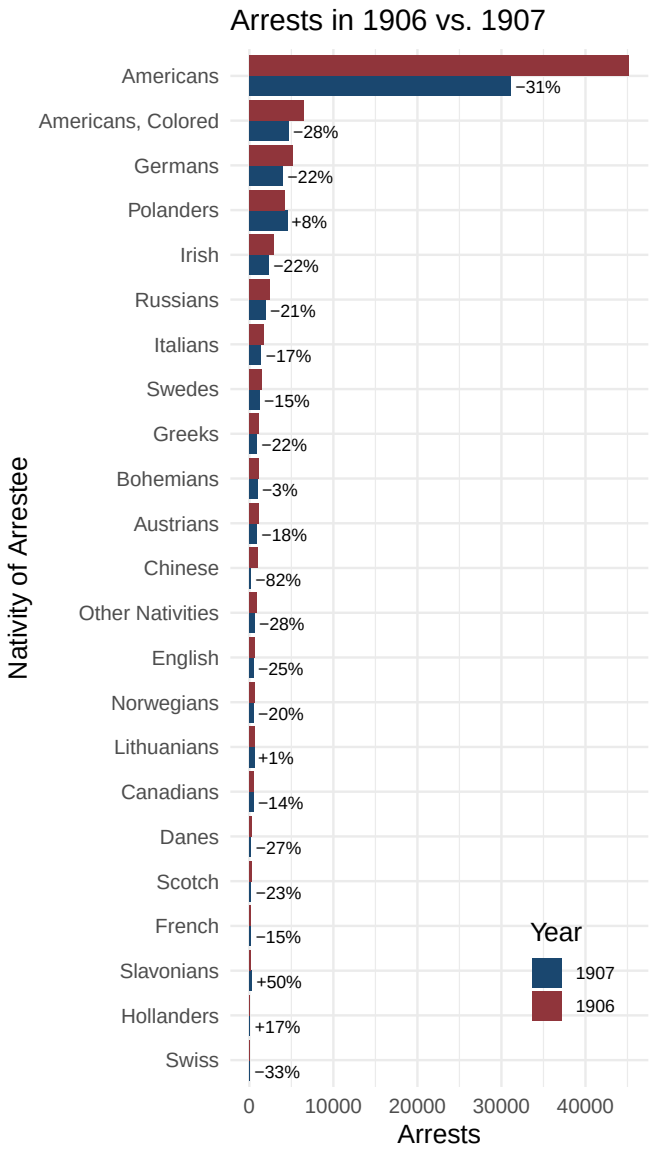
Note: Figure displays trends in homicides and murders in Chicago comparing the Bienen (2012) homicide and murder data with the Monkkonen (2001) homicide data.

Figure A.11: Time Series of Murder in Chicago by Circumstance and Location



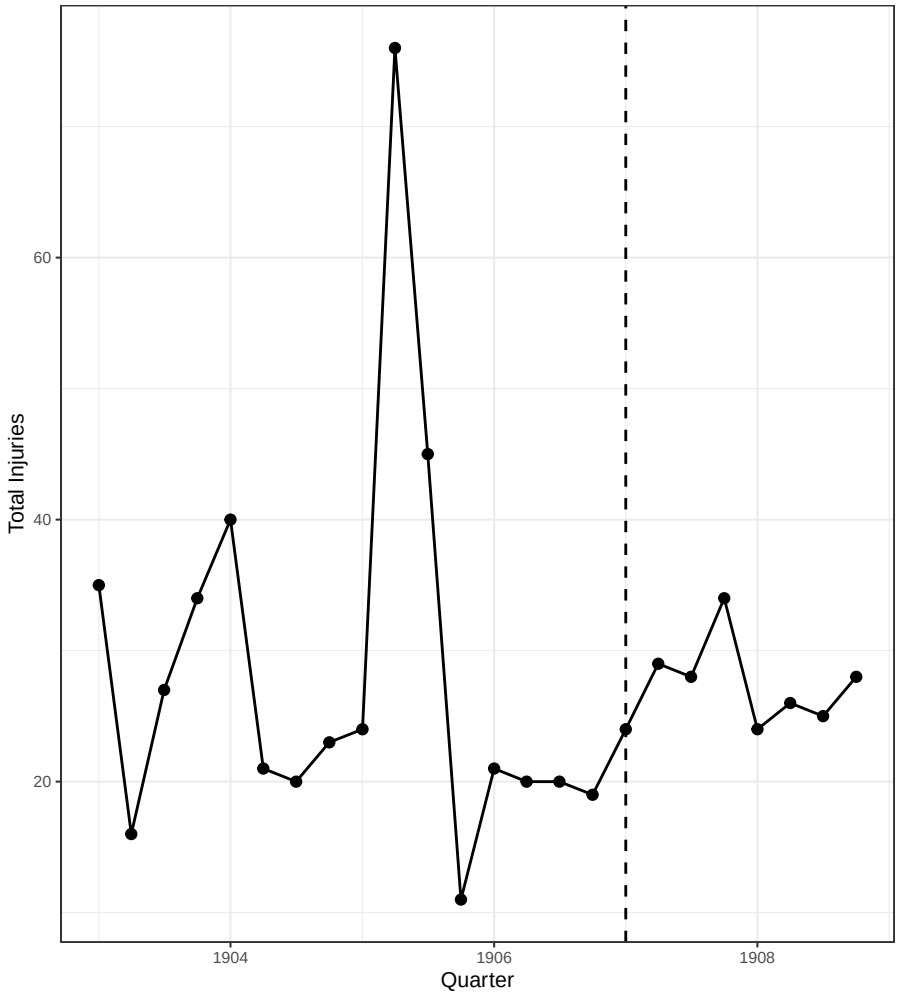
Note: Figure displays trends in murders in Chicago at the quarter-level between 1900 and 1908 classified by the circumstance of death (top panel) and location of the murder (bottom panel) using the Bienen (2012) data.

Figure A.12: Changes in Arrest Composition by Nativity



Note: Figure displays changes between listed 'nativities' in 1906 versus 1907, along with percent changes between 1906 and 1907 relative to 1906.

Figure A.13: Total Injuries of CPD Officers over time



Note: Figure displays the total number of injuries of CPD officers within the precincts in the Matron's data between 1903 and 1909 at the quarterly level. The vertical dashed line denotes the MCA.

Table A.1: Effect of MCA on Injuries

	TWFE	
	Total Injuries	Injuries per Officer
	(1)	(2)
MCA	0.14	0.00084
	(1.13)	(0.01)
% Change	6%	3%
Mean(Y)	2.54	0.03
N Obs.	264	264
N. Precincts	11	11
Frequency	Quarterly	Quarterly
Range	1903-1908	1903-1908

Note: Table displays TWFE regression estimates (equation (2)) comparing total injuries and injuries per officer at the precinct-quarter level comparing Precints 2 and 27 with other precints in the Matron's data (6,10,12,15,17,19,21,32,38), with Precincts 2 and 27 being considered treated after the MCA and the others treated as control. *** p<0.01, ** p<0.05, * p<0.1.

B A Model of Policing and Crime

These empirical results suggest that increasing incentives for arrests result in more high-cost crime, though little effect on lower cost crime. This is difficult to rationalize in a standard model of policing in which incentives for arrests increase the threat of an arrest, and thus deterrence. In this section, I provide a simple dynamic model which incorporates incentives, deterrence, incapacitation, and officer's self-incapacitating by making arrests. The model illustrates trade-offs and the effective multi-tasking between enforcement and deterrence which lead to quality-agnostic arrest incentives (e.g., performance pay) having potentially perverse outcomes.

B.1 Set Up

An officer interacts with a civilian of potential criminal severity $k \geq 0$ who chooses whether or not to commit a crime ($c \in \{0, 1\}$), then the officer chooses whether to make an arrest ($a \in \{0, 1\}$). The officer receives 0 if $a = 0$; if $a = 1, c = 1$, they receive utility depending on performance pay $w \geq 0$, quality-dependent motivation $\beta_m \times m(k)$, where $\beta_m \geq 0,\ m'(k) > 0$ — murder arrests are more valuable than jaywalking ones — and effort cost $e_a > 0$. While in the empirical context w is performance pay, the results apply any quality-agnostic incentive for arrests.

$$U_o(a|c, k) = (\beta_m \times m(k) + w - e_a) \times c \times a$$

The civilian moves first and knows the officer's conditional response. They choose $c \in \{0, 1\}$ to maximize their utility based on gains from crime $g \geq 0$, punishment if arrested $A(k) \geq 0$ ($A'(k) > 0$), and effort cost $e_c > 0$.

$$U_c(c|k) = (g - A(k) \times a - e_c) \times c$$

The outcome of the game is:

$$c^* = \underbrace{1[g > A(k) + e_c] \times 1[\beta_m \times m(k) + w > e_a]}_{\text{Crime, despite arrest}}$$
$$+ \underbrace{1[g > e_c] \times 1[\beta_m \times m(k) + w < e_a]}_{\text{Crime, without arrest}}$$
$$a^* = \underbrace{1[g > A(k) + e_c] \times 1[\beta_m \times m(k) + w > e_a]}_{\text{Crime, then arrest}}$$

B.2 Dynamic Model

I use this set up in a two-period ($t \in \{1, 2\}$) model. In $t = 1$, a measure 1 of civilians are randomly matched with a measure $O_1 \in (0, 1)$ of officers. If a civilian is unmatched (since $O_1 < 1$), they know $a_1^* = 0$. Officers and civilians are myopic. In $t = 1$, civilian types k_1 are drawn from $f_1(k_1)$ and gains g are independently drawn from $f_g(g)$. Upon matching, if an officer chooses $a_1^* = 1$, the civilian and officer are removed from the game in period $t = 2$. As a result, the number of officers available in $t = 2$ is $O_2 = O_1 - a_1^*$. The arrests of civilians distorts the distribution of $k_2 \sim f_2$ by setting all arrested civilians' types to $k_2 = 0$ and removing them from being able to be arrested in $t = 2$ (i.e., the officer arrives to an empty street). If, for example, no one is arrested in $t = 1$ ($a_1^* = 0$), then $f_1 = f_2$ and $O_1 = O_2$.

Let $P_a(k) = 1[\beta_m \times m(k) + w > e_a]$. Civilians can be 'Deterred' from committing crime by increasing $P_a(k)$, which is the threat of an arrest, as in the canonical Becker (1968) model. Clearly, increasing w weakly increases $P_a(k)$ resulting in less crime and more arrests. In $t = 1$, crime is decreasing in w and decreasing in O_1, with arrest incentives and manpower being complementary. The expected amount of crime weighted by the intensity k_1 ('crime cost') in $t = 1$ is:

$$\begin{aligned}\mathbb{E}[c_1^* \times k_1] =& (1 - O_1) \times \mathbb{E}[k_1 \times 1[g > e_c]] + \\ & O_1 \times \mathbb{E}[k_1 \times 1[g > A(k_1) + e_c] + k_1 \times 1[A(k_1) + e_c > g > e_c] \times (1 - P_a(k_1))]\end{aligned}$$

The overall share of arresting officers in $t = 1$ and incapacitated in $t = 2$ is:

$$\mathbb{E}[a_1^*] = O_1 \times \mathbb{E}[P_a(k_1) \times 1[g > A(k_1) + e_c]]$$

In $t = 2$, the expected police force is $O_2 = O_1(1 - \mathbb{E}[P_a(k_1) \times 1[g > A(k_1) + e_c]])$, and the expected distribution of types from which civilians are drawn is:

$$f_2(k_2) = \begin{cases} f_1(k_2) \times (1 - \mathbb{E}[a_1^* | k_1 = k_2]) & \text{for } k_2 > 0 \\ O_1 \times \mathbb{E}[P_a(k_1) \times 1[g > A(k_1) + e_c]] & \text{for } k_2 = 0 \end{cases}$$

Then, crime cost in $t = 2$ is:

$$\begin{aligned} \mathbb{E}[c_2^* \times k] =& (1 - O_2) \times \mathbb{E}[k_2 \times 1[g > e_c]] + \\ & O_2 \times \mathbb{E}[k_2 \times 1[g > A(k_2) + e_c] + k_2 \times 1[A(k_2) + e_c > g > e_c] \times (1 - P_a(k_2))] \end{aligned}$$

The total crime cost ($\mathbb{E}[c_1^* \times k_1] + \mathbb{E}[c_2^* \times k_2]$) is not necessarily decreasing in w due to the interaction of three primary mechanisms. By increasing w: 1. (Deterrence) The likelihood of an arrest if $c = 1$ increases, reducing crime in $t = 1$ and $t = 2$; 2. (Criminal-Incapacitation) Arrests in $t = 1$ increase, reducing the amount of potential crime ($\mathbb{E}[k_2]$) in $t = 2$ because as a more have $k_2 = 0$; 3. (Self-Incapacitation) Arrests in $t = 1$ increase, meaning force size in $t = 2$ (O_2) declines reducing effective Deterrence. Because increasing w produces more arrests of decreasing average quality, Self-Incapacitation eventually dominates Deterrence and Criminal-Incapacitation, resulting in higher crime costs.

B.3 Simulation

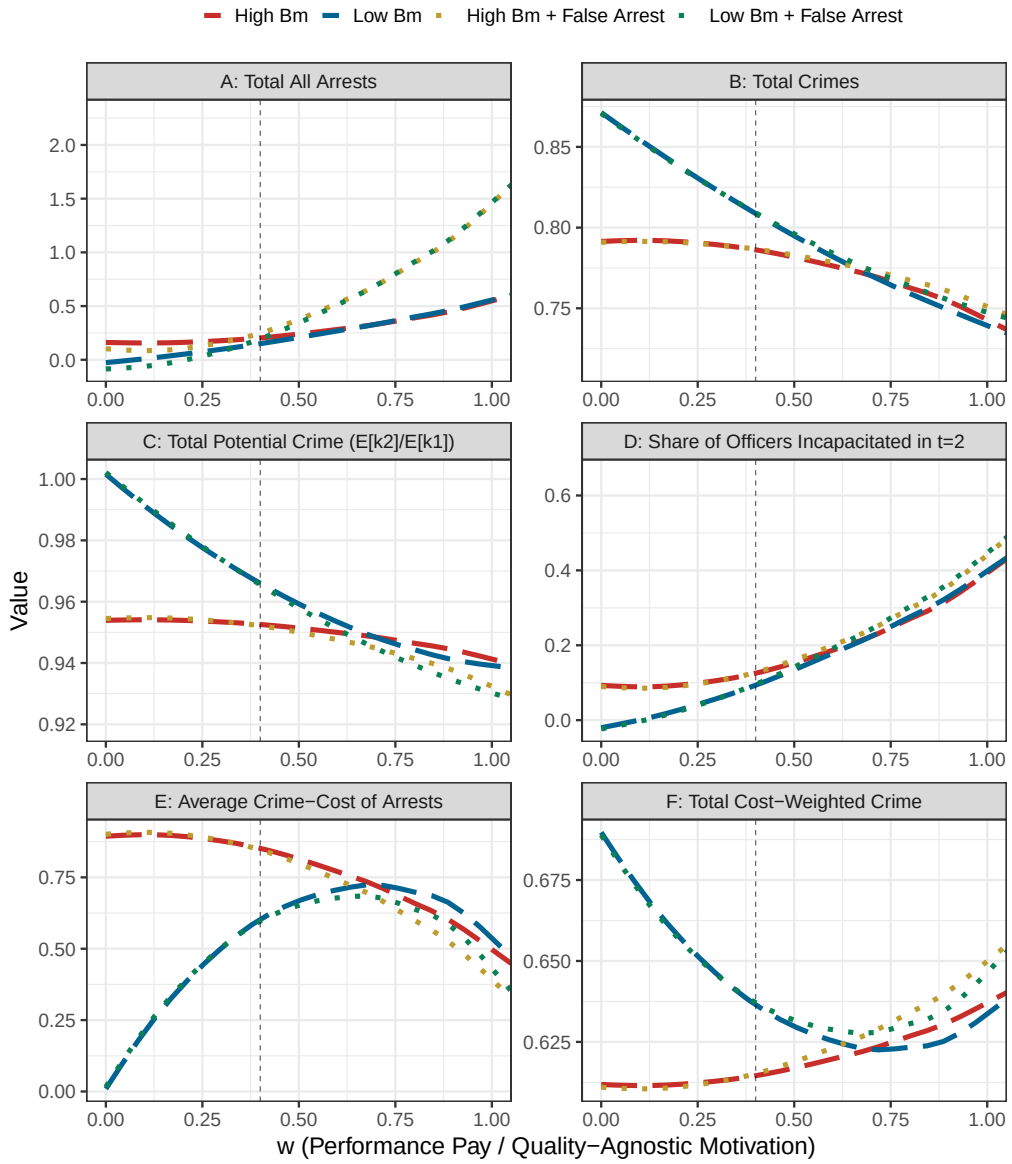
These mechanisms are illustrated in Figure B.1 which displays a simulation exercise across a range of values for w. I include results for two types of officers with high and low quality-dependent motivation ($\beta_m^H > \beta_m^L > 0$). I also include the simulation results for the realistic and simple extension in which officers can make 'false' arrests ($a = 1$ but $c = 0$) and still receive w, though civilians receive $-A(\underline{k})$ if they are falsely arrested.

Focusing on β_m^H, Panels A and B show that increasing w increases total arrests and decreases total crimes, consistent with the Deterrence mechanism. Panels C and D show the expected intensity of potential crime in $t = 2$ ($E[k_2]/E[k_1]$) is slowly decreasing in w due to Criminal-Incapacitation, while the share of officers available in $t = 2$ relative to $t = 1$ ($1 - O_2/O_1$) (Self-Incapacitation) increases dramatically. Finally, Panels E and F show that the average quality of arrests is declining in w and that total crime costs rise considerably as w becomes sufficiently large. Essentially, as w rises, more officers self-incapacitate through unproductive arrests resulting in marginally lower-quality Criminal-Incapacitation but marginally more Self-Incapacitation despite increased Deterrence, which cannot be exercised as officers are incapacitated.

For β_m^L officers by contrast, the lack of initial motivation for arrests means at low w there is no credible threat of an arrest and thus low arrests and high crime. As w increases, Deterrence increases, but the decreasing value of the marginal arrest means Criminal-Incapacitation becomes marginally less important and Self-Incapacitation dominates. So, after a certain value of w, total crime costs begin to rise.

Allowing false arrests makes these patterns more pronounced through two supplemental mechanisms: as w increases officers are more likely to Self-Incapacitate with real and false arrests, but crime also rises because more civilians choose $c = 1$ since the threat of a false arrests reduces their non-crime outside option. This predicts that police misconduct can have crime increasing effects beyond lost civilian cooperation (Ang et al. (2023)).

Figure B.1: Simulated Results of Dynamic Model



Note: Figure displays the results of a specific case of the model, across different values of w and for different values of β_m ($\beta_m^H > \beta_m^L > 0$) and allowing or preventing false arrests. Total Arrests is total number of arrests (of any quality) made by officers in $t = 1$ and $t = 2$, scaled to be out of the total possible arrests; Average Crime-Cost of Arrests divides takes the summation of crime-costs (k) of each arrest made in $t = 1$ and $t = 2$ and divides by total arrests (imputing 0 when no arrest is made or a false arrest is made); Total Cost-Weighted Crime sums over the crime costs (k) of all crimes committed in $t = 1$ and $t = 2$ and scale it by the maximum possible total crime cost. Share of Officers Incapacitated in $t = 2$ is the share of officers who are excluded from $t = 2$ due to making an arrest in $t = 1$ divided by total officers (or $1 - O_2/O_1$). Lines denoted smoothed means across values of w over 100 simulation runs. Implementation includes $A(k) = 3 \times k$, $m(k) = k^2$, $A(\underline{k}) = 0.5$, $f_1(k)$ is a discrete distribution with k taking on three values, 0.1 with $f_1(0.1) = 0.5$, 1 with $f_1(1) = 0.3$, and 3 with $f_1(3) = 0.2$; Eight officers and 20 civilians are used in $t = 1$, meaning $O = 0.4$. Both $e_c, e_a = 0.3$, and $g \sim U[0, 10]$. $\beta_m^L = 0.25$, $\beta_m^H = 0.5$, and the figure displays values of $w \in [0, 0.3]$ rescaled to be between 0 and 1.