Jobs for Votes: Patronage and Performance in Tammany Hall's NYPD*

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Abstract

Do politicians select public sector employees via patronage to win votes while sacrificing performance? I combine newly digitized personnel records on the selection, careers, and performance of 5,795 New York City Police Department (NYPD) officers with geolocated information on all voters and election results in the city for 1900-1916. The linked data reveals that 21% of the police officers were appointed in a deviation from civil service rules. These patronage employees were more likely to be connected to leaders of Tammany Hall, the city's incumbent Democratic Party organization. I use a difference-in-differences design to show that patronage appointments increased Democratic registration by 10.3% within the 50-meter radius around the employee's residential address. This electoral response – and complementary results on promotions tied to electoral support – suggest that patronage employees are incentivized to mobilize the votes of their neighbors. The electoral logic of patronage jobs in exchange for votes has important implications for performance: Patronage employees perform considerably worse than their meritocratically selected peers.

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1 Introduction

The quality and motivations of workers are fundamental to the performance of any organisation. Employees of public organisations have historically been selected and promoted at the discretion of political leaders (Grindle, 2012). Political observers commonly fear that by making employment and advancement conditional on political support, incumbents can turn state officials into "party henchmen" (Eaton, 1885). Control over patronage might incentivize politicians to prioritize their re-election chances over recruiting and motivating talented bureaucrats (Gallego et al., 2020).[1] In theory, a politicised bureaucracy could distort electoral competition to advantage the incumbent (Medina and Stokes, 2002), to undermine accountability (Leight et al., 2020; Menes, 1999; Stokes, 2005), and to depress the provision of public goods (Bardhan and Mookherjee, 2018; Robinson and Verdier, 2013).

Most countries have introduced civil service systems that limit discretion by politicians and require bureaucrats to act in a non-partisan and impartial manner (World Bank, 2000). The landmark civil service law for the U.S. federal government, the 1883 Pendleton Act, explicitly states that no public employee needs "to render any political service, and that he will not be removed or otherwise prejudiced."[2] While recent work has evaluated whether these reforms made the state more effective (Aneja and Xu, 2023; Moreira and Pérez, 2021), we know comparatively little about the political consequences of the state's personnel policies. Does patronage result in bureaucrats delivering political services for their patrons? What, if any, is the electoral return for politicians to deviate from meritocratic selection? Despite plenty of descriptions of patronage systems by social scientists and historians, quantitative evidence on the electoral returns to patronage remains scarce.[3]

In this paper, I provide an estimate of the electoral return to patronage from an infamous era of clientelistic governance in U.S. history — New York City (NYC) under the control of the Tammany Hall political machine.[4] Tammany Hall was the

[1] I follow the definition of "patronage" as discretionary appointments of individuals to governmental or political positions (Webster's II New College Dictionary 1995).

[2] Full text of the bill available here.

[3] The effect of patronage on votes is theoretically ambiguous and therefore of empirical interest. In theory, if patronage goes to loyal supporters it might not affect electoral behavior at all. Patronage could also be distributed for non-electoral reasons, e.g., to hold bureaucrats politically accountable (Toral, 2023) or ideologically aligned and motivated (Spenkuch et al., 2023). Key (1964) and Sigman (2022) point to patronage as a contributor to within-party cohesion and a source of party financing.

[4] Political machines are hierarchical organisations that distribute particularistic benefits to compete in elections, and often winning votes as reliably and repetitively as a machine (Scott, 1969).

city's main Democratic Party organization, which wielded outsized influence on the nomination and election of Democratic politicians in municipal, state-wide, and even national contests during the Gilded Age (1870-1900) and Progressive Era (1890-1929). Historians credit Tammany's use of patronage as a crucial source of its power. The organization's leaders certainly thought patronage paid off at the ballot box. Caro (1975) quotes a district leader proclaiming that "[t]his is how we make Democrats," when describing Tammany's interventions in public hiring. Yet, to this date there has been no systematic evaluation of these claims.

All the while, Tammany's New York has served as a common point of comparison for scholars of modern patronage systems and clientelistic politics.[5] Former U.S. President Barack Obama has described Brazil's President Lula da Silva as "having the scruples of a Tammany Hall boss," and the Russian President Vladimir Putin reminded him "of the sorts of men who had once run the Chicago machine or Tammany Hall" (Obama, 2020). NYC at the turn of the 19th century bears many similarities with the societies in which patronage thrives today: Tammany Hall operated in an environment where inequality was high and politicians were powerful enough to deviate from *de jure* civil service rules. Even today, patronage is not purely a developing country phenomenon. More than 8,000 jobs in the U.S. federal government are appointed at the sole discretion of the president.[6] In 2020, then President Donald Trump passed an executive order to remove civil service protections from an estimated 50,000 additional bureaucrats.[7] In response, the House of Representatives in 2021 passed the "Preventing a Patronage System Act" to limit the executive power of future administration.[8]

Identifying the electoral return to patronage is difficult in any setting. The empirical challenges are starkest when patronage arrangements are informal and individual votes are secret, as is the case for most modern settings and Progressive Era New York. The ideal research design combines information on the recipients of patronage jobs with data on voting decisions which they could plausibly influence (e.g., their

[5]See, for example, in Latin America (Hidalgo and Nichter, 2016; Szwarcberg, 2015), Southeast Asia (Chandra, 2004; Scott, 1969), and the Middle East (Corstange, 2016).

[6]See the list of "United States Government Policy and Supporting Positions", also known as the "Plum Book" (GAO-13-299R, March 1 2013).

[7]Executive Order 13957 created Schedule F in the excepted service, and ordered currently protected positions to be classified. President Joe Biden revoked the Executive Order before it could be implemented. But it has been reported that ex-staffers of Trump's administration are planning to re-instate the order under the next Republican president, and that they identified 50,000 employees to terminate after exemption.

[8]More information on this bill is available here.

own, and those of their family members or neighbors). Even with the ideal data, we cannot simply interpret any correlation between patronage and votes as the causal effect of patronage. Patronage jobs are not randomly assigned and instead reflect the strategic decisions of political actors.

To overcome these empirical challenges, I combine newly digitized personnel records of the New York City Police Department (NYPD) with geo-referenced voter registry information of all voters in the city for 1900-1916. The police department is close to the ideal organization to study the electoral return to patronage. The NYPD was the largest city department at the time, with a footprint in all five boroughs (Manhattan, Brooklyn, Queens, Bronx, and Staten Island), and its officers were in frequent contact with potential voters. Focusing on police officers also allows me to construct an individual-level measure of performance, which is rare to observe in any bureaucratic organisation. Reports of the police administration include all complaints against individual officers and whether they were fined as a result of the complaint. I digitized all of the reports and link them to panel data on the careers of patrolmen hired in 1900-1916. The amount of fines they receive per year serves as a proxy for the (mis-)performance of each police employee.

First, I identify who received patronage jobs. Municipal civil service rules stipulated that all patrolmen (entry-level police officers) had to be selected through standardized exams. I collect data on the applicants and their exam results. Linking this information to complete lists of NYPD employees reveals that 21% of the 5,795 patrolmen hired in 1900-1916 did not have the required test scores. This pattern is in line with contemporaneous reports that alleged frequent deviations from civil service rules on the behest of Tammany Hall.

The voter registry data serves as a proxy for individual voting decisions. The archival records I digitized include the full name, residential address, and party identification of all registered voters in NYC. One feature of New York's election law at the time makes these records especially valuable: NYC voters had to renew their voter registration and party identification a few weeks before each election. With various contests for municipal, state-wide, and federal offices on different electoral cycles, this gives us a yearly measure of individual voting intentions.

To estimate the causal effect of patronage, I employ a difference-in-differences strategy. A rare feature of my data facilitates the evaluation of patronage decisions: I track the voter registry information for all types of applicants. This includes other ineligible and unsuccessful applicants — the most likely counterfactual recipients of

patronage. Other applicants to the same job opening with equally bad exam results serve as a natural control group to patronage recipients. But simply comparing the voter registration outcomes of the two groups likely underestimates the electoral return to patronage. Patronage could plausibly influence the voting intentions of the direct recipients *and* members of their social network. To account for such spillover effects, I geo-locate all registered voters in NYC and count the number of registered Democrats at each applicant's address and in their immediate neighborhood. The difference-in-differences approach compares the post-employment voting intentions of patronage recipients (plus their neighbors) with the intentions of unsuccessful applicants (and those closest to them) in the same years. This research design addresses any time-invariant sources of endogeneity, such as differences in the neighborhoods or personal characteristics of patronage employees and other applicants. It also removes any shared time-trends (e.g., if the Democratic Party becomes more popular over time).

The empirical analysis proceeds in two steps. First, I document how the patronage system operated in the selection of entry-level officers in the NYPD. Of the 5,795 patrolmen hired in the period, 21% got the job without achieving the required test score on the civil service exam. In turn, some applicants with better exam results were passed over. Applicants with a connection to their local Tammany Hall district leader were significantly more likely to get selected in deviations from the merit system. Test scores are strongly correlated with actual performance of selected officers, even when comparing patrolmen working in the same police precinct in the same year. Directly comparing patronage and merit employees in the same position confirms that patronage employees perform 22.7% worse on the job (as measured by the amount of fines they receive). Together, these empirical facts suggest that the selection of patronage recipients was politically motivated, and came with significant costs to public service delivery.

In the second and core empirical part of the paper, I provide an estimate of the causal effect of patronage jobs. My main finding is that patronage appointments delivered an electoral return. In years after the appointment, the number of registered Democrats increased by 3.0 voters within a 50 meter radius of the patronage employee's address. This is an increase of 10.3% over the baseline mean of 28.7 Democrats in control neighborhoods. There are no pre-trends, the increase in registered Democrats immediately follows the patronage employee's entry into police

service, and the increased electoral support lasts for at least 6 years.[9]

While the estimated electoral return is robust to choosing a slightly smaller or bigger radius around the applicant's residence, the effect is strongest at the exact home address and dies out with distance. Voters who live further than 140 meters from the recipient are unaffected in their electoral behavior. This strongly localized pattern alleviates common concerns with ecological inference, as the electoral return is directly tied to the recipients of patronage.

The lack of pre-trends is also indicative of the mechanism underlying the electoral return to patronage. Electoral support follows the receipt of patronage, and not the other way around. In theory, patronage could generate an electoral return by motivating applicants to support the incumbent in the hope of receiving patronage as a *reward*. In contrast, the empirical pattern I document is more in line with the electoral return as a *response* to patronage. What explains why patronage employees and their neighbors continue their electoral support for many years after the initial appointment?

In the last part of the paper, I shed light on the mechanism underlying the persistent nature of the electoral return to patronage. First, I show that patronage jobs cause an increase in the number of registered Democrats without a decrease in Republican registration. This suggests that the effect is driven by mobilising Democratic voters, and not by persuading voters to change their affiliation.

Next, I leverage the panel-structure of the linked personnel and voter registry data to reveal the importance of Democratic turnout for the careers of patronage employees. I show that the likelihood of promotion for patronage employees increases with the number of registered Democrats among their neighbors. There is no such relationship between voter behavior and the career progression of merit employees. In contrast, merit employees get promoted if they perform better, but performance does not seem to matter for the promotion chances of patronage employees. These empirical patterns suggest that patronage employees work under an incentive scheme which values their political services, while allowing them to neglect their official duties. The performance data is consistent with such incentives, as patronage employees perform even worse than predicted by their poor exam results.

[9]Strictly speaking, I measure voter registration. The voting decisions of individual voters are unobserved. Because registration had to be renewed every year, and since party registration and votes are highly correlated (at the polling place level), I use registration as a proxy for actual voting decisions. The terms "voter registration" and "electoral support" are used interchangeably in this paper.

If patrolmen can become sergeants by mobilising the votes of their neighbors, it is no wonder that Democratic registration increases with a patronage appointment and stays consistently at elevated levels. The voter mobilisation mechanism of the electoral return to patronage is consistent with historical narratives. Some accounts highlight explicitly how mobilising the votes of neighbors was valued by Tammany Hall. George Washington Plunkitt, a notorious leader of Tammany Hall, recounts how he made his start in politics:

> "Two young men in the flat next to mine were school friends—I went to them, just as I went to Tommy, and they agreed to stand by me. Then I had a followin' of three voters and I began to get a bit chesty. Whenever I dropped into district head-quarters, everybody shook hands with me [...]" (Riordon, 1905)

This anecdote illuminates how party loyalists thought about electoral politics in Tammany Hall's NYC. The empirical results presented in this paper suggest that patronage delivered an electoral return by making bureaucrats behave just like these "party henchmen."

1.1 Related Literature

This paper contributes to several strands of related literature. First, this paper speaks to the literature on selection and incentives in public organisations (Finan et al., 2017). A growing branch of the literature investigates the impact of discretion (including patronage, nepotism, or other forms of favoritism) or more impartial and merit-based personnel practises in bureaucracies.[10] Much of the recent work has focused on the consequences of these policies on the qualities of applicants (Ashraf et al., 2020; Deserranno, 2019; Dal Bó et al., 2013) and the selected (Brollo et al., 2017; Colonnelli et al., 2020; Mocanu, 2023; Moreira and Pérez, 2022; Weaver, 2021), or on their performance (Aneja and Xu, 2023; Estrada, 2019; Moreira and Pérez, 2021; Otero and Munoz, 2022; Riaño, 2023; Toral, 2023; Voth and Xu, 2022; Xu, 2018). We know less about the political economy effects of the personnel policies adopted by the state.

[10] While this paper focuses on discretion in public organizations, discretion and deviations from merit-based processes are also common in the personnel decisions of for-profit companies (Bertrand, 2009; Colonnelli et al., 2022; Hoffman et al., 2018). Business owners and managers use their discretionary power for political purposes (Frye et al., 2014; Robinson and Baland, 2008), including in the European Union (Mares and Young, 2019) and the United States (Hertel-Fernandez, 2017).

Much of the existing work focuses on macro-phenomena.[11] Economists and political scientists have established relationships between civil service laws and the incumbency advantage (Folke et al., 2011), state spending patterns (Ujhelyi, 2014), or the prevalence of partisan newspapers (Aneja and Xu, 2023). This paper complements existing work and fills the gap between personnel policies and political economy outcomes by providing individual-level evidence of the electoral return to patronage. Quantifying the electoral return helps us understand why politicians frequently interfere with public organizations, even if political interference undermines public services. By connecting the behavior of voters to the selection and promotion incentives of individual bureaucrats, I am able to shed light on the mechanism through which a politicized bureaucracy can distort electoral competition in favor of the incumbent. My results suggest that public employees who owe their job to the discretion of party leaders work for the party of their political patron, while performing worse in their official duties.

A closely related literature emphasizes the importance of bureaucrats for state capacity. Much of the work in this area investigates the role of bureaucrats for the capabilities and effectiveness of state institutions (Ash and MacLeod, 2023; Best et al., 2023; Dahis et al., 2023; Fenizia, 2022; Rasul and Rogger, 2018; Rauch and Evans, 2000; Limodio, 2021; Mehmood, 2022; Ornaghi, 2019), or the positive contributions of state capacity for economic development (Besley et al., 2022; Cornell et al., 2020; Dincecco and Katz, 2016; Dell et al., 2018; Evans and Rauch, 1999; Rauch, 1995). In contrast, the findings of this paper highlight how discretion in hiring and promotions allows incumbents to use the human capital of the state for partisan goals. This finding shares a theme with recent work by Heldring (2021, 2023), who similarly documents how a capable bureaucracy is a powerful tool for politicians independent of their objectives.

This paper also contributes to the literatures on vote buying (Mares and Young, 2016), clientelism (Bardhan and Mookherjee, 2020; Hicken and Nathan, 2020; Hicken, 2011), and distributive politics (Golden and Min, 2013; Stokes et al., 2013). A large number of studies have documented how politicians around the world share rents and distribute public resources or selectively target government programs to the private

[11] Another recent branch of the literature draws on survey experiments with bureaucrats to tie discretion in recruitment (Oliveros and Schuster, 2018) or transfers (Brierley, 2020) to perceptions of corrupt bureaucratic behavior. This builds on work documenting a negative correlation between meritocratic practices and corruption across countries or regions (Charron et al., 2017; Dahlström et al., 2012; Rauch and Evans, 2000; Meyer-Sahling and Mikkelsen, 2016)

benefit of their supporters or their in-group.[12] This includes work on distributing public jobs as patronage to connected groups in a similar pattern as in the spoils politics of Tammany Hall's New York (Brierley et al., 2023; Hassan et al., 2023). While there are rich qualitative reports (Chubb, 1981; Oliveros, 2021a,b) and important theoretical work (Robinson and Verdier, 2013; Stokes, 2005) models the electoral motivations of patronage, we lack credible estimates of the electoral return to patronage jobs.[13] At the same time, economists and political scientists have estimated the electoral return to other transfers in cash or kind (Cantú, 2019; Cruz et al., 2018), and the cost per vote of campaign expenditures (Bombardini and Trebbi, 2011; Levitt, 1994) and government subsidies (Slattery, 2023).

This paper connects patronage more closely to the literature on vote buying by estimating the electoral return to patronage jobs. The results highlight important parallels and key differences between patronage and other forms of vote-buying. Consistent with the argument of Nichter (2008) that vote buying can focus on buying turnout of likely supporters, my evidence suggests that patronage mobilizes more than it persuades. In contrast to vote-buying with one-time transfers, patronage employees stay in their job for many years. While this could make patronage an expensive tool, the continued costs to the government's budget are offset by a persistent electoral return. Performance costs are another feature unique to patronage. By selecting and promoting worse employees, patronage has potentially longer-lasting welfare costs than one-time transfers.

Lastly, I contribute to our understanding of American economic history and American political development. The study of patronage in the U.S. has a long tradition (Key, 1936; Wilson, 1961). Weber (1922) described the Republicans and Democrats of his time as "[e]xamples of pure parties of patronage in the modern state". Modern empirical work on the effects of patronage in U.S. history has focused on evaluating federal reforms (Aneja and Xu, 2023; Moreira and Pérez, 2021), or relied on cross-sectional variation across states (Folke et al., 2011; Ujhelyi, 2014) or cities (Menes, 1999; Ornaghi, 2019; Rauch, 1995; Trounstine, 2008). Studying the governance of cities is crucial to understand the development of American state capacity in gen-

[12]Much of the literature surveyed here adopts a broad definition of "patronage" as a catch-all term for any selective transfer from patron to client. In contrast, this paper is exclusively concerned with patronage jobs in the public sector (sometimes referred to as "political patronage")

[13]I build on work by Calvo and Murillo (2004), who document a correlation between public employment and electoral support for Peronists across Argentina's 24 provinces. Wantchekon (2003) provides experimental evidence on voter reactions to campaign *promises* of patronage jobs, which is complementary to my focus on the electoral return to *distributing* patronage jobs.

eral and the economics of patronage in particular.[14] By assembling and analyzing individual-level data for America's biggest city, I give a detailed account of how patronage operated and affected the behavior of bureaucrats and voters. Instead of evaluating a reform, I document how the existing civil service rules were imperfectly enforced, and I leverage the remaining variation in the selection of patronage employees. This paper's quantitative case study of the NYPD under Tammany Hall's influence confirms historical narratives on patronage as a socially wasteful vote-buying tool (Banfield and Wilson, 1965).[15] My findings of private political benefits and public performance costs go against a more benevolent view of machine politics advanced by the defenders of "honest graft" (Riordon, 1905). Without dismissing the work of historians who emphasize the benefits urban machines delivered to poor immigrant communities (Golway, 2014; Link and McCormick, 1983; Scott, 1977), this paper highlights how distributing patronage enabled politicians to win votes, while providing sub-optimal public services.

[14]Cities used to be the level of government with the greatest state capacity, with local governments accounting for 72% of all government debt and 56% of all revenues in 1913 (Wallis, 2000). Brown and Halaby (1987) document that many U.S. cities were dominated by political machines like New York's Tammany Hall in 1870-1945.

[15]This stands in contrast to earlier case studies of political machines in New Haven (Johnston, 1979) and rural Pennsylvania Sorauf (1956), which argued that patronage had limited electoral effects.

2 Institutional Context

New York in the Progressive Era (1890-1930): With 3.4 million residents in 1900, the five boroughs of New York City (Manhattan, Bronx, Brooklyn, Queens, and Staten Island) contained 4.5% of the entire US population (compared to 2.6% in 2021). The city's size still underestimates its national political importance. New York State with its 36 electoral votes used to be an important swing state in an era when the Democratic and Republican parties were relatively evenly matched at the federal level and in NY State. In 1884, Democrats only won the presidency because Grover Cleveland carried NY State by fewer than 1,200 votes. Harrison defeated Cleveland in 1888 by 13,000 votes (a margin of less than 0.1%).

Slim margins at the state level resulted from a stark divide between solidly Republican rural areas and cities dominated by the Democratic party. New York City (NYC) was governed by Democratic mayors for more than 30 out of 40 years in 1890-1930. Democratic politicians could only win state-wide offices if they carried the NYC vote by wide margins. The Democratic Party remained competitive by catering to the city's large immigrant communities from Ireland, Germany, Italy, and Eastern Europe. Progressive Era NYC was a deeply unequal society, with garment workers and other precariously employed tenement dwellers living in the same city as the Rockefellers, Vanderbilts, Carnegies, Morgans, and other "robber barons".

Tammany Hall: The main faction of the Democratic party of NYC used to be better known as Tammany Hall, named after the former headquarter of the party on 14th Street in lower Manhattan. Tammany Hall is infamous for patronage, for corruption, and for its strong grip over Democratic nominations for elections in NYC and beyond. William "Boss" Tweed, Tammany Hall's leader in 1858-1871, coined the phrase "to the victor belongs the spoils," to describe the organisation's practice of distributing government resources, including public sector jobs, to it's supporters and party insiders. This spoils system has been credited by historians for Tammany Hall's sustained electoral success for close to a century and up to the 1930s.

Tammany Hall was a highly hierarchical and geographically organized political machine with a "Boss" at the top, who presided over an Executive Committee consisting of local leaders from each Assembly District of Manhattan and the Bronx. The boroughs of Brooklyn, Queens, and Staten Island had similar political machines which largely followed the lead of Tammany Hall. District leaders were responsible for the distribution of patronage and oversaw the mobilisation of voters for primaries

and general elections in their districts. Contemporary accounts suggest that the political fortune of district leaders was closely linked to their support among the local immigrant networks.

Tammany Hall could count on the loyal votes of large numbers of poor and middle class Irish Americans and other recent immigrants. The story of Al Smith, NY Governor (1921-1928) and Democratic candidate for President (1928), is illustrative. Smith grew up in a poor immigrant community in lower Manhattan and received his first city job through the connections built in his local Tammany Hall club. Like other Tammany Hall supporters, Smith took pride in being a"regular", meaning he turned out for all elections and voted straight Tammany Hall tickets in primaries and the Democratic ticket in general elections.

3 Data and Descriptive Statistics

This section introduces the data sources that form the basis of the empirical analysis in this paper. I also describe how I link the records from these sources and construct the main variables of interest. Then I go into more detail on how linked information on applications, entrance exam results, and lists of police employees allows me to identify which patrolmen received their jobs through patronage. Lastly, I provide descriptive statistics on the distribution of patronage jobs.

3.1 Data Sources

The main source underlying the data collection for this paper is the *City Record*, the official journal of New York City. The *City Record* is a daily gazette of the NYC municipal government which since its founding in 1873 and to this day publishes a comprehensive array of announcements, reports, and legal notices concerning the city's government agencies. The publication was created as part of a reform package in reaction to corruption by Tammany Hall and its former leader William "Boss" Tweed, who was convicted of embezzling an estimated 30-200 million dollars of public funds (in 1877 dollars). New York's City Charter, enacted by the New York State legislature, requires the journal's existence and continued documentation of city business to satisfy state transparency requirements.

Importantly, for the purposes of this paper, the *City Record* in the early 20th century published twice yearly lists of all city employees, information on applicants and the results of civil service exams for various positions (including patrolmen positions in the police department), weekly reports on the internal disciplinary proceedings of the police department, yearly voter registration records, and highly disaggregated results of all elections within the city's boundaries (including state and federal elections). Despite the richness of the information contained in the millions of pages published by the *City Record* throughout it's 150 year history, this source has largely been ignored by modern social scientists.

To prepare the information from *City Record* reports for econometric analysis, I had to overcome a series of challenges. This process involved original archival work, locating thousands of individual reports, scanning tens of thousands of pages, transcribing millions of rows to a machine readable format (combining optical character recognition and manual entry), linking these observations across data sets, and geolocating hundreds of thousands of locations to their exact address in the city. This

data collection builds on "The City Record Project" (TCRP) by Jonathan Soffer and collaborators, who provided an invaluable public good by scanning most issues of the *City Record* for 1873-1947 and making them available as searchable PDFs. Many of the necessary reports for this paper could be found among their scans. To fill the remaining gaps, I visited the research division of the New York Public Library and the New York City Municipal Archives in Manhattan and Brooklyn, retrieved the archival records, and made original scans.[16] Most importantly, TCRP did not provide scans of the *Enrollment Books*, my source for the party identification of individual voters.[17] With a total of over 20,000 newly scanned pages, which yielded more than 9,118,000 voter-year observations, preparing the *Enrollment Books* for statistical analysis is a major component of the data contribution in this paper.

Additional Sources: I complement the data derived from the *City Record* with information from three additional sources, the archive of The New York Times (NYT) articles since 1858 (available online through the "TimesMachine" service), scans of The Tammany Times (TTT) publications (available for 1893-1912 via Hathi Trust), and restricted access full count data from the Decennial Census of 1900 (accessed through the Demography Lab at UC Berkeley). The NYT regularly reported critically on the internal governance of Tammany Hall and changes in it's leadership. The TTT was published by Tammany Hall as a partisan newspaper and contained a mix of propaganda, local political news, and updates on Tammany internal matters. I read through all articles in either publication in 1900-1916 that contained any of the following keywords to manually assemble a panel data set on Tammany Hall district leaders in Manhattan and the Bronx: "District leader", "Tammany leader", or "Democratic leader." Individual-level information, including full names, from the Decennial Census allows me to predict the likely country of origin for all district leaders and for applicants to patrolmen positions in the New York Police Department (NYPD). The following subsection gives more detail on this and other measurement decisions.

[16] Gaps in TCRP scans arise either because some issues (or individual pages or longer supplements like the *Enrollment Books*) were not included or, in rare cases, the included scan was too blurry for transcription. A research assistant for this paper looked through every page provided by TCRP for 1900-1916, downloaded all pages with relevant reports, and made notes on any gaps and missing reports. I then went through physical copies of the *City Record* in the archives with a focus on filling gaps in TCRP scans and finding missing reports. Any remaining gaps are likely due to the loss of some records during the last 107-123 years.

[17] TCRP does provide closely related reports, *Registry Lists*, which list all registered voters but do not include their party identification.

3.2 Data Construction, Record Linkage, and Measurement

I combine three sets of personnel records on the selection and employment of policemen in New York City to identify patronage employees and unsuccessful applicants who could have received patronage jobs, and to track the subsequent career outcomes of new recruits in the police department. The *City Record* publishes twice annual lists of all city employees (called the *Civil List*), individual-level results of civil service exams (in *Eligible Lists*), and lists of all applicants for some city positions (including for patrolmen in the police department).

Civil Lists: I collect and transcribe at least one *Civil List* each year for 1902-1916.[18] The resulting *Civil List* data set consists of yearly cross-sections of all police employees, with variables on their full name, residential address, exact entrance date into police service, the day they left the service (if they left since the last *Civil List*), their rank (e.g., patrolman, sergeant, captain), their annual salary, any changes in the salary since the last list, and the police precinct they work in.

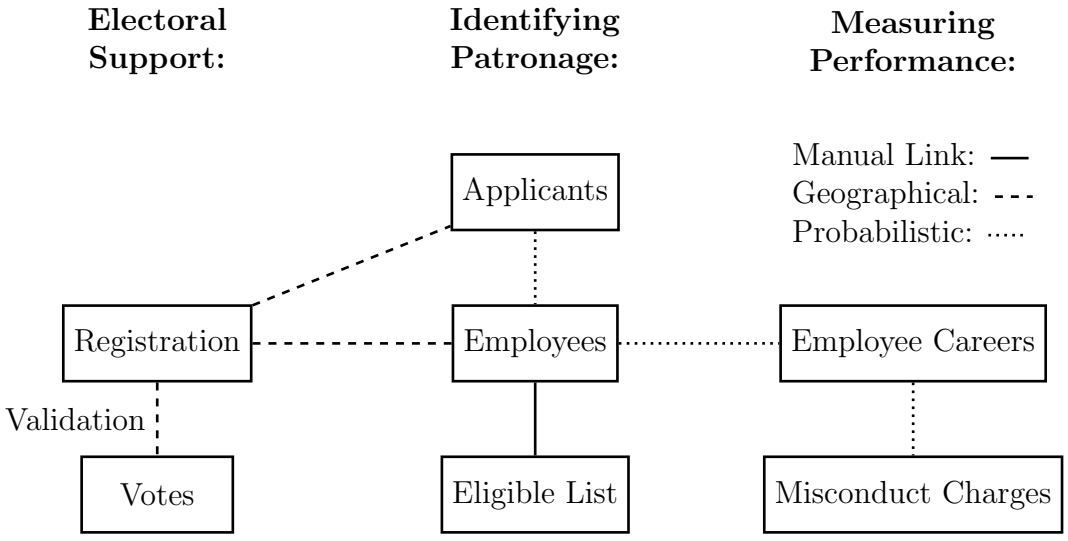
Applications and Eligible Lists: Reports on applications and *Eligible Lists* are hard to find since they could be published at any given day and on random pages of the *City Record*. Lists of applicants are spread over hundreds of reports, often listing small batches of applicants at a time. Given my comprehensive search of the *City Record*, I am confident that my data collection uncovered close to all surviving reports on the recruitment of patrolmen in 1900-1916.[19] Each *Eligible List* is a ranked list of applicants according to their composite score in the civil service exam (where 100% is the best score). Applicants who score less than 70% in the examination are not included in the *Eligible List*. Therefore all of the reports on applications are necessary to reconstruct the entire applicant pool. Applications and *Eligible Lists* include the full names of applicants and their residential address. Applications additionally list the date of submission and the occupation of applicants at that date. Each *Eligible List* includes the date it was published, the rank of applicants, and their test score (in percent).

[18] The *Civil Lists* for 1900-1916 are published on the last day of January and of July each year. But not all of them are still available. I collect the July issues for 1902-1904, the January issues for 1905-1910, both issues for 1911, and the July issues for 1912-1916 to ensure that there is never more than a year gap between lists. I start in 1902, because the 1901 issues could not be found.

[19] See footnote 16 for details on the general process. Applications in most years also include running ID numbers, which further facilitate tracking the completeness of the data collection.

My analysis focuses on complete *Eligible Lists* for 8 distinct "hiring periods" between 1901 and 1913, which can be linked to a comprehensive set of 22,761 applications received in 1900-1912 (and collected from hundreds of separate reports). For each eligible list, I define the hiring period as the time period between the days that the first and the last patrolmen selected from that list enters the police department. The *Civil List* data reveals that the police department recruited 5,795 new patrolmen during these hiring periods.

Figure 1. Illustration of Record Linkage and Measurement



Linking of Personnel Records: Figure 1 illustrates how I link the various data sets derived from reports in the *City Record* to measure the concepts at the core of this paper: The patronage status of patrolmen, changes in electoral support, and performance. To identify patronage status (Figure 1, middle column), I first link all newly employed patrolmen in the *Civil List* to the *Eligible List* that corresponds to their hiring period. High quality matches are especially important in this step. An incorrect match of a high-scoring patrolman to a low test score, for example, might falsely label him as a patronage employee. I therefore manually create matches between the 5,795 new patrolmen and applicants on the *Eligible Lists*. Unusually rich information in both sets of records on full names, exact residential addresses, and on the time period (date of publication for the list and entrance date for employees) facilitates the matching. We can therefore conclude with some certainty that employ-

ees not matched to an *Eligible List* did not receive a score of 70% or higher on the civil service exam. See Section 3.3 for details on how I use the linked data to identify patronage status.

Next, I use probabilistic matching to link individuals on the application lists to the newly employed patrolmen. These links allow me to observe which applicants do not get employed in the police department, and if some patrolmen are employed without formally applying. To be considered as a candidate for a match to an employee, applicants have to apply after the preceding *Eligible Lists* was published and within a one-year window before the start of the hiring period during which the employee enters the police department. Within those blocks of applicants and employees assigned to the same hiring period, I match on full names and residential addresses. Section 4 describes how the pool of unsuccessful (i.e. unmatched) applicants constructed for each hiring period via this match serves as the control group for patronage employees selected during that period.[20]

Lastly, I link all police employees across all waves of the *Civil List* to construct a panel on the careers of the 5,795 patrolmen recruited during the selected hiring periods.[21] The matching procedure uses information on names, addresses, entrance dates, ranks, and precincts of employees. In an iterative procedure, I start by requiring at least 99% match on name strings and exact matches on entrance date, rank, and precinct (breaking ties in name similarity by choosing the closest match in geographic address distance). The requirements are then relaxed step by step for unmatched employees. The last matches still require 99% similarity in names. I first link each *Civil List* to the subsequent issue of the list in the next year. The resulting links are then chained to recover links across multiple years. For all employees that remain unmatched after chaining, I repeat the iterative matching procedure but link each *Civil List* to the issue of the list published two years later. The additional links from this step allow me to fill in gaps and extend the coverage of the chained links.

All employees that are still not matched after the full procedure are coded as exiting the police department. The detailed panel data on the careers of patrolmen derived from linking the *Civil Lists* allows me to track them as they achieve higher ranks or change precincts. Sections 5.3 and 6 employ the panel data to analyse the

[20] I also match applicants to *Eligible Lists* on names and addresses within blocks defined by hiring periods. This ensures that applicants on eligible lists are not double counted when constructing the pool of unsuccessful applicants for each hiring period.

[21] I need to link all employees, and not just the set of new patrolmen, to avoid matching the new patrolmen to records that are a better match for another employee. I also cannot limit to patrolmen in later waves because I want to track career progression.

promotion incentives of patronage employees and to explore the relationship between patronage and performance.

Electoral Support: One of the main challenges with estimating the electoral return to patronage is to measure changes in electoral support that could be affected by the distribution of patronage. This requires a measure of electoral support which can be observed for potential recipients of patronage and other voters who could potentially be influenced. The measure cannot be too aggregate to not risk conflating the effect of patronage and those of other policies or shocks, and to not commit ecological fallacies. Lastly, the measure needs to be observable at a relatively high frequency to investigate its dynamics and rule out reverse causality. Individual voter registration records with party identification, as found in the *Enrollment Books* of the City Record, fulfill all these requirements.

The *Enrollment Books* that I collect and transcribe for this paper (as described in Section 3.1) yield over 9,118,000 voter-year observations in 1900-1916.[22] For each observation, the data includes the full name of the registered voter, their address, the party they register for (or the absence of party registration), and the election district (ED) and assembly district (AD) within which their address is located in. To connect registered voters to applicants and employed patrolmen, I geo-locate the addresses of the three groups (Figure 1, left column). This involves parsing and standardizing millions of observation to more than 300,000 unique address strings with the correct format for locating their geographic coordinates. Most registered voters, applicants, and employees can be located in their exact building (69.8%, 46.5%, and 77.8%).[23] When analysing the relationship between voter registration outcomes and patronage in Sections 4 and 5, I always restrict attention to those individuals that can be exactly located.

My main measure of electoral support is the number of voters who register as

[22]The *Enrollment Books* cover 1900, 1903-1914, and 1916. To the best of my knowledge, no complete *Enrollment Books* for 1901, 1902, and 1915 have survived. *Enrollment Books* were first published in 1898, but 1900 is the earliest extant book that I could find.

[23]There are multiple reasons why some addresses cannot be located at this level of accuracy. For example, transcription errors, errors in standardizing the address, vague descriptions of some addresses (e.g. "corner of Sullivan St. and Houston St."), and changes in the names of streets. A larger share of employees than of unsuccessful applicants are located in their exact building. This is partly explained by the greater availability of information for employees. There are up to three observations for the addresses of employees, when they enter the police department (from their application, their *Eligible List*, and their first *Civil List*). I assign them the coordinates that are located with the highest level of confidence, while prioritizing earlier addresses (i.e. at the time of application) in the case of ties.

Democrats in close proximity to patronage employees (compared to unsuccessful applicants).[24] I focus on Democratic registration because during for most of the time period (1904-1913) NYC was governed by Democratic mayors, who appointed Democratic police commissioners, who in turn were influenced in their selection of patrolmen by Democratic (i.e., Tammany Hall) district leaders. If there is a relationship between patronage jobs and electoral support, it should primarily show up as support for the Democratic party. For robustness, I also consider the number of Republican voters in the close proximity of patronage recipients.

For voter registration to serve as a valid measure of electoral support it needs to closely track actual voting behavior. To validate the relationship between registration and votes, I collected detailed data on election results for 1900-1916 from the *Official Canvass* of votes published in the *City Record*.[25] The *Official Canvass* reports election results for each election in NYC (including municipal, state, and federal contests) by election district (ED), contest (e.g., elected office or referendum), candidate, and year. The median ED contains 374 voters, corresponds to exactly one polling place, and covers one or two city blocks. Unfortunately the *Official Canvass* does not list the party of candidates. Instead, I searched the New York Times archive for articles on the nomination of candidates by Tammany Hall and the results of primary contests within the Democratic Party. Through this effort, I identified the Democratic candidates for each contest to compute the Democratic vote share by ED, contest, and year.[26] Section 5.1 validates voter registration as a measure of electoral support by demonstrating a strong linear relationship between the share Democrats among registered voters and the Democratic vote share by ED and year. This is unsurprising since the election law of New York State required all voters in large cities to renew their registration before each election. Voter registration therefore closely tracked voting intentions.

Performance: Measuring the performance of individual employees is challenging in any organisation. A unique feature of the NYPD in this time period facilitates the task: The police commissioners held weekly meetings to hear complaints on the

[24] See Section 5 for details on the research design.

[25] This does not include the *Official Canvass* for 1901 and 1910, which could not be found.

[26] In some contests, anti-Tammany forces in the Democratic party ran on "fusion" tickets with Republicans or independents. I do not count votes for these tickets as part of the Democratic vote share and instead track only the votes received by the Tammany Hall candidates. To keep vote shares comparable across different elected offices, I drop contests for multiple seats in the same district (e.g., most judicial races or Sheriff elections).

conduct of individual officers and decided for each complaint whether officers should get fined and how harshly. Complaints could get filed by anyone, including ordinary citizens, peers, or supervisors of the employee. All complaints and fines were published in the *City Record*. I searched through all of the volumes of The City Record in 1900-1916 to collect and digitize the information on complaints and fines. I then linked them to the employee records from the *Civil List* based on their name, their rank, and their police precinct at the time of the complaint.

Using this linked data, I measure the yearly performance of each employee as the number of days pay deducted in fines. More fines suggest worse performance. The text of the complaints and study of the internal NYPD rule book of the time allow me to classify the complaints into three broad categories: Negligence, misbehavior, and abuse.

3.3 Identifying Patronage Appointments

According to the municipal civil service rules of the time, patrolman positions should only go to the top performers in standardized exams. I therefore define patronage appointments as jobs given to individuals who did not have the required exam results. To identify these deviations from the civil service rules, I manually linked all patrolmen who started their employment with the NYPD in 1900-1916 to their exam results. Linking is facilitated by the fact that archival records on exam results and employed policemen both include complete names of individuals, their residential address, and the exam date or employment begin respectively.

Applicants for patrolman positions are ranked accord to their exam results, and the resulting rankings are published as "eligible lists". Civil service rules specify that no position should be filled with anyone not on the current eligible list, and that offers have to be made in order of the ranking on the list. Applicants with a composite score of less than 70% are not included in the list and not eligible for appointment. When the list is exhausted, the police department needs to ask the civil service commission to advertise for a new set of job openings, to hold new exams, and to create a new eligible list.[27] Motivated by these rules, I therefore identify patrolmen as patronage employees if their rank on the eligible list at the time of their recruitment was worse than the number of appointments made during the time this eligible list was active.

[27]The number of yearly patrolman appointments is decided by the budget passed at the beginning of each fiscal year. The exam results cut-off above which patrolmen should get hired according to the civil service rules is therefore not fixed. Instead, the cut-off is jointly determined by the number of patrolmen demanded by the budget and the quality of the applicant pool.

Figure 2 illustrates this process of identifying patronage employees in a stylized example of a linked eligible list. In this example, ten appointments are made during the time the depicted list is active (marked with green background). But three of these jobs went to applicants with test scores outside of the top ten: Andrew K. Dllon, Timothy Donovan, and Frank B. Zabriskie (all marked with bold font). They are coded as patronage employees. I repeat this exercise for all 5,795 patrolmen that enter police service in 1900-1916, and I identify 21% of them as patronage employees. I refer to the remaining patrolmen as "merit employees".

Figure 2. How to Identify Patronage Appointments, Stylized Example

Applicant:	Score:	Rank:
William F. Gill	90.51%	1
James H. Kearns	87.61%	2
John A. McCarthy	86.78%	3
Cornelius B. Corcoran	85.42%	4
Walter P. Robertson	83.15%	5
William D. I. Waters	82.86%	6
Augustin F. Sexton	81.83%	7
John F. Byrne	80.38%	8
Samuel W. Noble	77.29%	9
Daniel J. Foley	76.20%	10
Grover C. Brown	75.98%	11
Frederick C. Struss	72.24%	12
Andrew J. Dillon	71.87%	13
Timothy Donovan	70.28%	14
John F. Cook	70.15%	15
Frank B. Zabriskie	70.04%	16

Notes: This figure presents a stylized example of an eligible list with ten appointments (marked with green background). Three employees received the job despite having test scores that placed them outside of the top ten. These patronage employees are marked in bold font. I refer to the other seven names as "merit employees". Other applicants had the necessary scores (i.e. Rank ≤ 10) to be appointed but did not receive jobs (white background), and I refer to them as being "passed over".

A potential concern with this approach could be if some of the eligible applicants (i.e. with Rank ≤ 10 in Figure 2) that are not appointed were not truly "passed over", but instead received and rejected the offer of employment. For example, if Robertson, Sexton, and Byrne on the list of Figure 2 rejected offers of employment, then Andrew J. Dillon at Rank 13 should not be considered a patronage employee. It is therefore comforting to know that the main results of this paper remain virtually unchanged

when restricting attention to the most severe cases of patronage: Patrolmen who should not have received jobs, because their test scores were below 70% or who never formally applied at all.[28]

3.4 Patterns in the Distribution of Patronage Jobs

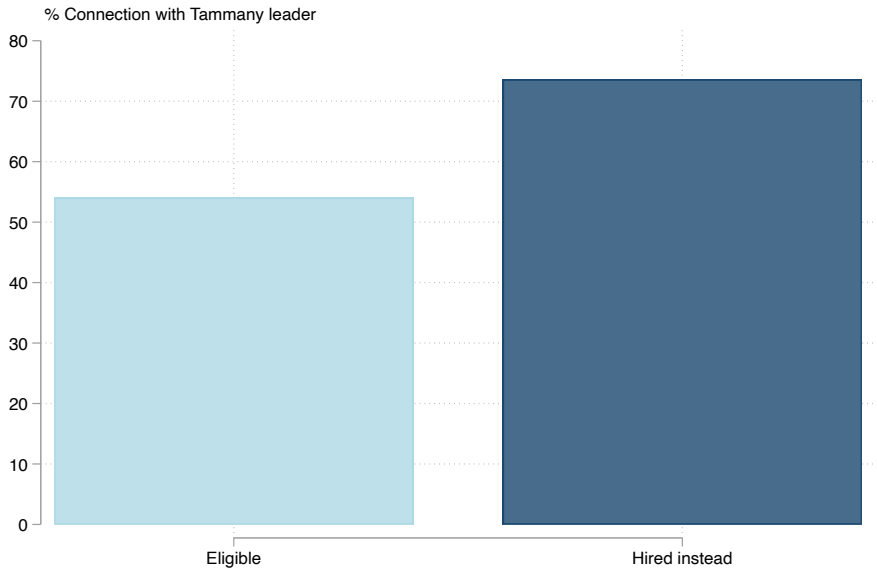
Patronage jobs are unlikely to be randomly distributed. This section explores the main determinant of patronage highlighted by the historical literature: Connections between office-seekers and their local Tammany Hall district leaders.

I investigate whether a shared immigration origin between applicants and their local district leader predicts the distribution of patronage jobs. I hand-collected information on all Tammany Hall leaders during 1900-1916 from contemporary New York Times articles and assembled a panel data set on their identities and times in office. For many of the district leaders, newspaper articles also include biographical information including their country of origin, or the origin of their parents if leaders are U.S. born. For the remaining district leaders, I predict their origin based on their last names. Predictions are based on the most common country of birth for immigrants with the same last name in the Decennial Census of 1900. I follow the same procedure to predict the country of origin for all applicants to patrolmen positions. The resulting predictions confirm historical reports that the majority of Tammany Hall leaders at the time were of Irish or German origin.

Sharing a country of origin is a rough proxy for connections between applicants and their local Tammany Hall leaders. Figure 3 compares the share of connected applicants within two groups: Those eligible for patrolman jobs but passed over (left bar), and those who who receive the jobs instead (right bar). Hired patrolmen are around 20 percentage points more likely to be connected to their local Tammany leaders than the eligible applicant pool. This pattern is compatible with the interpretation that the distribution of patronage jobs followed a political logic. The next section formally investigates whether distributing patronage jobs resulted in electoral returns.

[28] See Table 1, columns 4 and 5 for a comparison of the electoral return to close and far deviations from merit.

Figure 3. Connections Between Applicants and Local Tammany Hall Leaders



Notes: This figure presents bar graphs for the share of connected applicants among those who were eligible to receive patrolman jobs but were passed over (left bar), and among the applicants who actually received the jobs instead (right bar). Connections are measured as sharing a (predicted) country of origin with the Tammany Hall leader of their local Assembly District. The country of origin is predicted as the most common country of birth for immigrants with the same last name in the 1900 Decennial Census.

4 Estimating the Electoral Return to Patronage

4.1 Empirical Strategy

The main objective of this paper is to analyse how the distribution of patronage jobs affects voter behavior. To this end, I implement an event study design around the date at which recipients of patronage begin their service as patrolmen in the NYPD. In this research design, each calendar year in which patrolmen receive patronage jobs contributes a sub-experiment. Each sub-experiment compares voters in the immediate neighborhood (e.g., within 50 meters) of patronage recipients to the neighbors of unsuccessful and ineligible applicants, who applied to the same set of job openings as the patronage recipient. For each sub-experiment, I then estimate the difference-in-differences in the voting behavior of applicants and neighbors in elections before and after the patronage employees start their duties. The combined event study estimate is the average treatment effect of all these sub-experiments.

For example, assume that Timothy Donovan and Grover Brown applied in May of 1905 and neither of them received the required test scores to be eligible (as in Figure 2), but Timothy enters police service in September of the same year as a patronage recipient. The difference-in-difference of this sub-experiment then compares any change in voting behavior of Timothy and his neighbors after September 1905 to changes in the behavior of Grover and his neighbors over the same time period. This comparison is then repeated thousands of times to cover all applicants and recipients of patronage in 1900-1916.

Unsuccessful and ineligible applicants are a natural control group since they are the counterfactual choice set of individuals who could have received patronage. By fixing the comparison to applicants who applied in the same period and are never hired (or "never-treated"), this research design avoids common econometric issues with traditional "two-way" fixed effects approaches for time-varying treatments as pointed out by Goodman-Bacon (2021).[29]

Importantly, I limit the sample to recipients of patronage and control applicants who do not have any overlap in neighborhoods with any other patronage recipients.

[29]By focusing on "clean" control units, the research design described in this section is closest in spirit to the stacked event study approach as in Cengiz et al. (2019). In contrast to standard stacked designs, in this setting there is an institutionally justified connection between treated and control units and each neighborhood-year observation only enters the sample once. For robustness, I show in Section 4.3 that results are robust to using other models tailored to settings with time-varying treatments (Borusyak et al., 2023; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2023).

For example, for Timothy (or Grover) to be included in the sample when estimating the electoral return within a 50 meter radius, there cannot be anyone who receives a patronage job at any point in 1900-1916 within 100 meters of his residential address. This sample definition ensures that the neighborhoods of control applicants are truly "never treated" and that estimates from treated neighborhoods are not contaminated by multiple correlated treatments.[30]

Let $s = \{1900, 1901, ...1916\}$ denote the year in which applicants start patronage jobs, and compute the event time t relative to the start year s. The event time t takes negative values for applicants i in years before patronage recipients from their application period begin their employment, and positive values afterwards. For example, since Timothy and Grover applied during the same period and Timothy started work as a patrolman in $s = 1905$, the event time in 1906 is $t = 1$ for both of them. To capture the effects of patronage on behavior in the immediate neighborhood j of applicant i, I aggregate their voting outcomes and the outcomes of voters within a small radius (e.g., 50 meters) of their residential address at the time of application. For each neighborhood $j(i)$ in event time t, I then estimate the following equation:

$$\mathrm{y}_{j(i)t} = \beta \mathrm{patronage}_i \times \mathrm{post}_t + \eta_i + \lambda_t + \mu \mathrm{X}_{it} + \epsilon_{it} \tag{1}$$

where $\mathrm{patronage}_i = 1$ if applicant i receives a patronage job, and $\mathrm{patronage}_i = 0$ if the applicant is unsuccessful. The post_t variable is defined as an indicator function $\mathbb{1}(t \geq 0)$, with values switching from zero to one in the year that individuals in the applicant's cohort receive patronage jobs. I control for individual-specific fixed effects η_i and a full set of event year fixed effects λ_t. Control variables X_{it} for time-varying characteristics of applicants i or their neighborhoods are included in some specifications. For example, I include fixed effects for the application period interacted with event-time fixed effects in X_{it} to focus the comparison on neighborhoods of patronage recipients and control applicants who were considered for the same set of job openings. The key parameter of interest β captures the effect on electoral outcomes $\mathrm{y}_{j(i)t}$ in neighborhoods when applicant i in neighborhood j receives a patronage jobs in comparison to neighborhoods of applicants that go without patronage. The main electoral outcome $\mathrm{y}_{j(i)t}$ I focus on is the number of registered Democrats in the neighborhood. Standard errors ϵ_{it} are clustered by neighborhoods $j(i)$, since this is the

[30] I also limit the sample to the time period of 1903-1914 to have a balanced panel, because I do not have voter registration data for 1901-1902 or 1915. The results are robust in the unbalanced panel of 1900-1916.

level at which the patronage treatment is assigned.

For β to identify the causal effect of patronage on electoral support, it is necessary to assume that support in neighborhoods with and without patronage recipients would have followed parallel trends in the absence of patronage. The main concern with the identification assumption is that patronage is not randomly assigned. Applicants that receive patronage differ from those that do not. Aggregating the electoral support to neighborhoods and including neighborhood fixed effects helps alleviate concerns over level differences between applicants. But estimates of treatment effects could still be confounded by differences in trends between neighborhoods with and without patronage recipients.[31] For example, if patronage jobs go to applicants from neighborhoods that recently received some public improvements (e.g., street lights, sewerage, paved streets) and are therefore increasingly supporting their Democratic incumbents, a simple difference-in-differences estimate would mistakenly attribute this trend to the effect of patronage.

To address this concern, I leverage the yearly frequency of the voter registration data and investigate the dynamics of the estimated treatment effect. The year-by-year estimates of the event study approach would reveal any confounders or trends that start before the applicants receive patronage jobs or that develop slower than the yearly changes in voter registration. For each neighborhood $j(i)$ in event time t, I estimate the following dynamic version of Equation 1:

$$\mathrm{y}_{j(i)t} = \sum_{k \neq -1} \beta_k \mathrm{patronage}_i \times \lambda_k + \eta_i + \lambda_t + \mu \mathrm{X}_{it} + \epsilon_{it} \tag{2}$$

where I sum over the interaction between the $\mathrm{patronage}_i$ dummy and individual event year fixed effects λ_k. Each λ_k variable is defined as an indicator function $\mathbb{1}(t = k)$ for event year k. All other variables are identical to Equation 1. With the first pre-period $k = -1$ as the leave-out category, the coefficients β_k on the interaction can be interpreted as the year-by-year effects of patronage. While it is impossible to directly test the identification assumption of common trends in the absence of patronage, the coefficients β_k in the pre-periods $k \leq -1$ can shed light on likely valuations. I check for parallel trends in the pre-period as an indirect test for

[31] A related concern would be any shock or policy that coincides with the distribution of patronage jobs. For such shocks to confound the estimated treatment effects, they would have to hit the same neighborhoods in the same year. Since patronage jobs are distributed throughout the entire study period and the focus is on highly local effects (e.g., within a 50 meter radius), it is hard to think of any shock that qualifies.

confounders, such as recent public improvements, that put neighborhoods with and without patronage on different trajectories in electoral support.

4.2 The Effect of Patronage on Electoral Support

Table 1 presents the main results from Equation 1 on the effect of patronage on electoral support. The dependent variable across all columns is the number of voters registered as Democrats in the 50 meter neighborhood around the residential address of patronage recipients and control applicants. The columns vary the specification and samples to probe the robustness of the results. Overall, neighborhood in which applicants receive patronage jobs experience an increase in Democratic registration of 2-3 extra voters; an electoral return of 6.8-10.4% over the baseline mean.

Column 1 of Table 1 reports estimates from the simplest specification which only includes fixed effects for individual neighborhoods and the event year. Democratic registration increases by 2.0 extra voters in neighborhoods with patronage appointments. column 1). In comparison to an average of 28.7 registered Democrats in neighborhoods of unsuccessful applicants, two extra voters means that Democratic registration increases by 7.0% in elections after patronage employees begin their job as patrolmen. The specification in column 2 additionally includes fixed effects for the period in which patronage recipients and individuals in control neighborhoods applied for the patrolmen positions and interacts these fixed effects with the event year. This empirical approach ensures that treatment effects are computed from a direct comparison of neighborhoods where treatment and control individuals applied for the same set of positions. The estimated electoral return to patronage in column 2 remains virtually unchanged. The preferred specification, column 3 of Table 1, adds time-varying fixed effects for the borough (i.e. Manhattan, Brooklyn, Bronx, Queens, or Staten Island) of the neighborhoods. When controlling for such flexible time-trends, neighborhoods with patronage appointments experience an increase of 3.0 extra registered Democrats than neighborhoods around unsuccessful applicants who live in the same borough and applied for the same positions.[32] This is an increase of 10.3% over the baseline mean.

[32]Including borough by year fixed effects likely matters for the magnitude of the effect because of variation in the dynamics of treatment effects across boroughs. Appendix Table A1 shows that effects are large in the more populous boroughs of Manhattan and Brooklyn (with 4.3 and 2.3 extra registered Democrats) and zero effects in the three smaller boroughs.

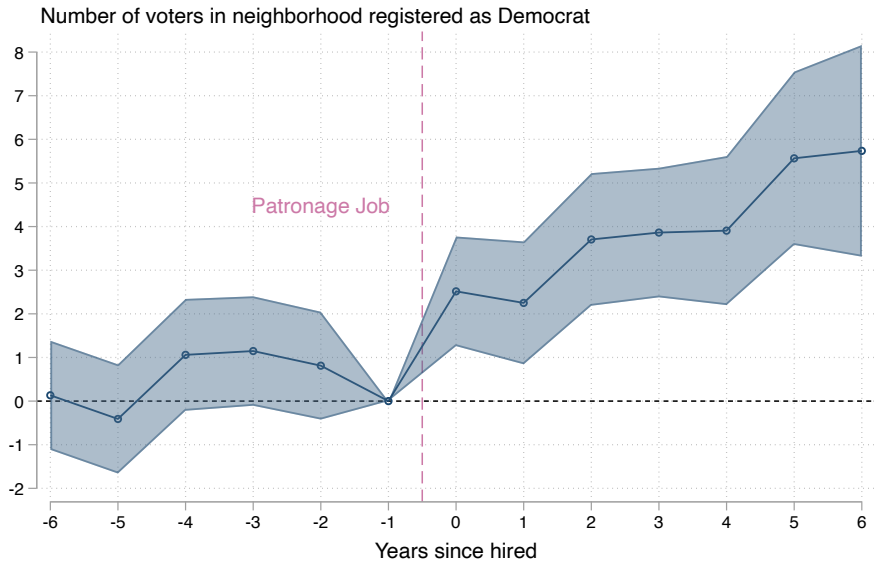
Table 1. Patronage Jobs and Democratic Registration

	(1) All	(2) All	(3) All	(4) Close	(5) Far
Patronage Appointment	2.02***	1.95***	2.95***	2.99***	2.93***
	(0.57)	(0.58)	(0.57)	(0.68)	(0.86)
Outcome Mean	28.67	28.67	28.67	28.67	28.67
R-squared	0.84	0.84	0.85	0.84	0.85
Observations	72492	72492	72492	70296	67320
Patronage Employees	614	614	614	431	183
Control Applicants	5427	5427	5427	5427	5427
Individual FE	Yes	Yes	Yes	Yes	Yes
Event Year FE	Yes	Yes	Yes	Yes	Yes
Application Period x Year FE	No	Yes	Yes	Yes	Yes
Borough x Year FE	No	No	Yes	Yes	Yes

Notes: This table reports difference-in-difference estimates of the effect of patronage (i.e. coefficient β of Equation 1). The outcome for all columns is the number of registered Democrats within a 50 meter neighborhood around the applicant. I winsorize the outcome at 1%. Observations are at the neighborhood-year level. *Patronage Appointment* is a dummy variable that switches on in the year that the applicant receives their patronage job, and the dummy is equal to zero before and for all control applicants. Columns 1-3 include all patronage recipients, while column 4 focuses on patronage recipients with test scores close to the eligibility cut-off, and column 5 only includes recipients of patronage who are far from the cut-off or did not apply at all. Starting in column 3, I include fixed effects for the application period interacted with event-year dummies. In column 4, I additionally include fixed effects for borough by year time trends. Standard errors in parenthesis are clustered at the level of applicants' neighborhoods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

A causal interpretation of the estimates in Table 1 requires that electoral support in neighborhoods with and without patronage recipients would have followed common trends in the absence of any patronage. Figure 4 provides event-study evidence from an estimation of Equation 2 with the fixed effects structure of the specification in Table 1, column 3. This figure demonstrates that treated and control neighborhoods were on parallel trends before applicants received their patronage jobs. While no definitive proof is possible, similar trajectories in Democratic registration for earlier elections support the assumption that these trends would have continued without the distribution of patronage. Figure 4 shows that electoral support increases in neighborhoods where applicants receive patronage exactly in the first election after patronage employees begin their service as patrolmen. The effect increases in subsequent years and reaches around 6 extra registered Democrats per year after six years.

Figure 4. Event Study of Democratic Registration Around Receipt of Patronage



Notes: This figure presents the dynamic treatment effect of patronage on electoral support with 95% confidence intervals (i.e. the event-study coefficients β_k of Equation 2). The outcome is the number of registered Democrats within a 50 meter neighborhood. See the notes of Table 1, column 3, for details on the specification.

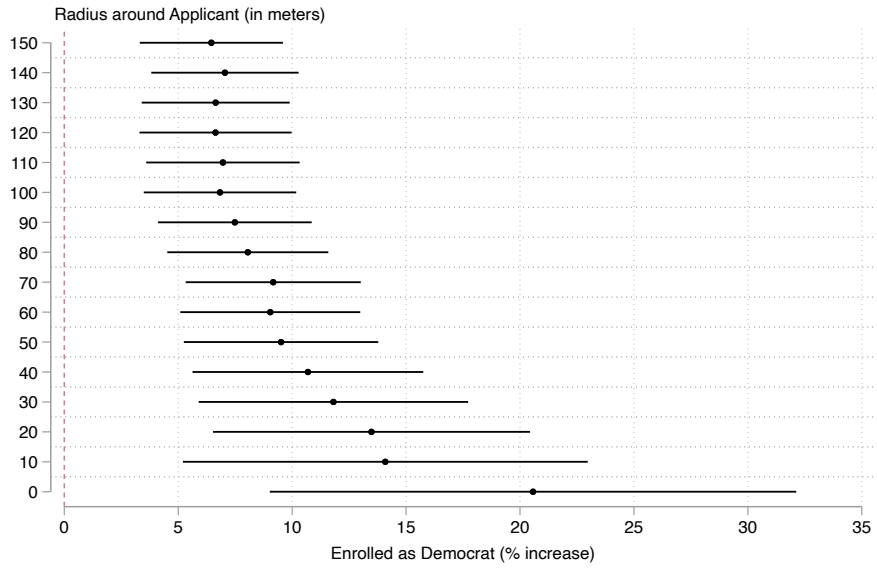
4.3 Robustness Checks

This section probes the robustness of the baseline estimate for the electoral return to patronage by varying the sample, the measure of electoral return, and choice of specification.

One potential concern, as mentioned in section 3.3, could arise if I incorrectly classify some appointments as patronage which go to applicants that merited the job. This could be the case for some applicants, who are just below the eligibility cut-off, but who would be eligible if some of the eligible applicants that I classify as "passed-over" were in fact offered the job and rejected that offer. To alleviate this concern, I directly estimate the electoral return separately for appointments of patrolmen, who were close to eligible (i.e. could have merited the job if all "passed over" applicants rejected the offer), and for appointments of patrolmen, who were far from eligible. Individuals in the far from eligible group can be classified as patronage with high certainty since they test scores were either too low (i.e. often less than 70%) or did not formally apply and therefore should not get jobs according to the civil service rules. Columns 4 and 5 of Table 1 present the estimates for these two

groups. I find that the effect of patronage on electoral support does not depend on these classification choices and the coefficient is virtually the same as in the baseline specification of column 3.

Figure 5. Electoral Return to Patronage, by Size of Neighborhood Around Applicants



Notes: This figure presents coefficients and 95% confidence intervals for the effect of patronage on electoral support; with choices for the neighborhood around applicants between 0 (i.e. same address) and 150 meters. See the notes of Table 1, column 3, for details on the underlying regression.

The baseline estimate in Table 1 reports the effect of patronage on Democratic registration in a 50 meter radius around applicants. This choice of radius is somewhat arbitrary. Figure 5 demonstrates that the estimated electoral return is robust to different choices for the neighborhoods around patronage recipients and control applicants. The figure plots the coefficients from a difference-in-differences specification as in Table 1, column 3, but with outcomes measured in neighborhoods ranging from 0 meters (i.e. at the exact address of the applicant) to a 150 meter radius around the applicants' residence.[33] When scaled to the average number of registered Democrats in the control neighborhood, patronage increases electoral support by 6.5-20.6%. The

[33]To keep the estimates comparable, I consistently trim the sample to avoid overlap with patronage recipients in neighborhoods of a 150 meter radius around their address. This explains why the estimates of the electoral return in a 50 meter neighborhood in Figure 7 and Table 1, column 3, are slightly different. The sample for Table 1 only avoided overlap in a radius of 50 meters when trimming the sample.

effect is largest at the exact residential address of patronage recipients and then decreases as treatment gets diluted for larger definitions of the neighborhood.

Similarly, the timing of the treatment effect and the absence of pre-trends is robust to different choices for the neighborhood around applicants and patronage recipients. Appendix Figure A1, for example, replicates the event study of Figure 4 but with a focus on Democratic registration at the exact residence of treated and control individuals as the outcome. Lastly, the pattern documented in this section does not depend on the chosen research design of estimating Equation 1 and 2 via OLS. Appendix Figure A2 compares event study estimates from the baseline model as in 4 with the results from alternative models for estimating treatment effects when units are treated in different time periods and treatment effects are allowed to be heterogeneous (Borusyak et al., 2023; Sun and Abraham, 2021; de Chaisemartin and D'Haultfœuille, 2023). The pattern is broadly comparable across models.

5 Drivers of the Electoral Return to Patronage

The results in section 4 provide evidence that distributing patronage jobs increased electoral support for the incumbent political party in the neighborhoods of patronage recipients. Having established that patronage jobs deliver an electoral return, I now turn to probing the mechanism driving this effect. To this end, I leverage the granularity of the voter registration data combined with panel data on the performance and careers of police officers. In sum, the collective body of evidence suggests that patronage delivers an electoral return because patronage recipients are motivated to mobilize the votes of their neighbors.

5.1 Voter Mobilisation

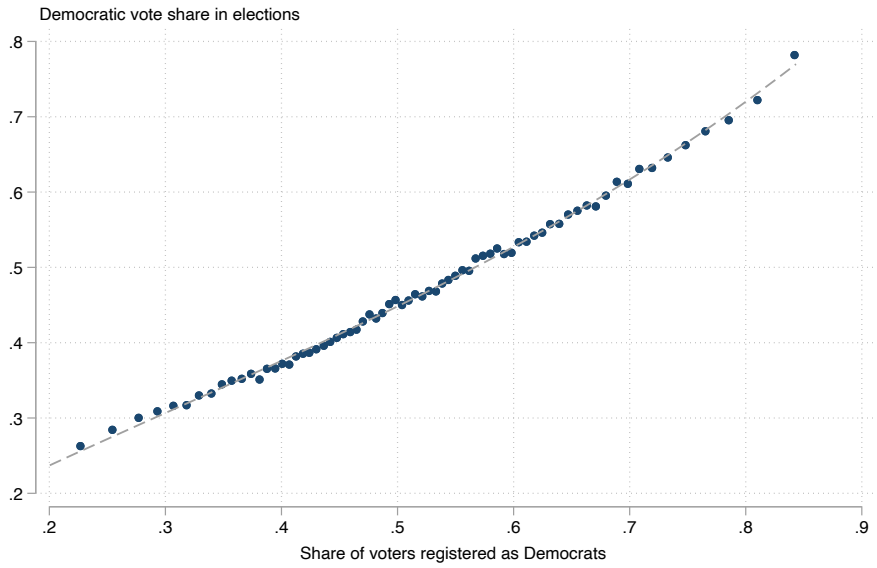
First, I investigate whether the electoral response in neighborhoods of patronage recipients is driven by persuasion or mobilization. Patronage could work through persuasion, for example, if neighbors of patronage recipients positively update about the incumbent party because they now think the party cares about their neighborhood. If voters change their mind in this way, we would expect some of them to switch their support from the party of the challenger (i.e. Republicans) to the incumbent's party (i.e. Democrats). Appendix Table A2 directly compares the effect of patronage on Democratic versus Republican registration. This exercise demonstrates that patronage did not decrease Republican registration (col. 2). Instead, Republican registration also increased, although by a smaller amount. Column 3 shows that the vote margin still increases in favor of the Democratic party. Together with the baseline effects on increased Democratic registration in Figure 4 and Table 1, this result suggest that instead of persuading voters to change their support, patronage mobilized additional supporters of the incumbent party to register.

To interpret these estimates as the effect of patronage on mobilizing actual *votes*, instead of just voter registration, we need evidence that registration proxies for voting behavior. Figure 6 demonstrates that Democratic registration is a strong predictor of votes for Democratic politicians in elections. The figure presents a binned scatter plot on the relationship between the Democratic vote share in elections and the share of Democrats among registered voters by polling place, year, and elected office.[34] The relationship is close to linear with a coefficient of 0.80 and an R-squared of 0.73 (see

[34]Polling places are equivalent to election districts (EDs), which are the most detailed level at which election results are reported. The median number of registered voters per ED is 374.

Appendix Table A3 for regression results and details on the underlying specification). High voter turnout in this setting, further alleviates concerns over relying on voting registration data. The median turnout of votes among registered voters across polling places was 91.9%.

Figure 6. Binned Scatter Plot of Election Results and Voter Registration

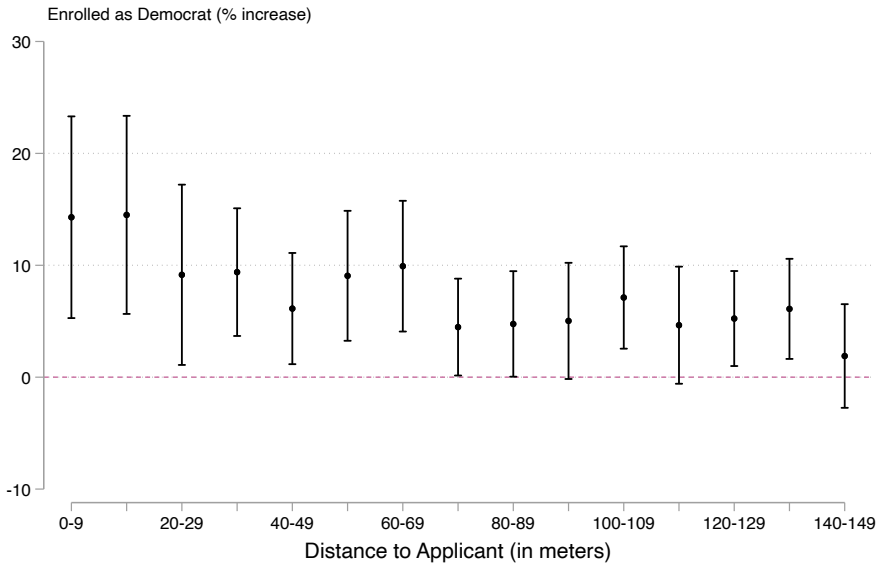


Notes: This figure presents the relationship between the Democratic vote share in elections and the share of Democrats among registered voters by polling place, year, and election as a binned scatter plot. The line of best fit was estimated flexibly up to third order polynomials. The underlying regression includes fixed effects for the borough, the elected office (e.g. mayor or city councillor), and flexible office by year time trends. See Appendix Table A3, column 5, for regression output on this specification and the remaining columns for details on alternative specifications.

Next, I explore which voters are mobilized by patronage. Figure 7 plots coefficients of treatment effects from difference-in-difference specifications as outlined in Equation 1 and estimated in Table 1, except that the outcome variable now focuses on the electoral support of subsets of voters. Instead of counting all registered Democrats within the neighborhood of the patronage recipient, I construct rings of 10 meter width and at increasing distances from their residential address. The pattern presented in Figure 7 demonstrates that voters at less than 20 meters distance from the residential address of patronage recipients show the strongest reaction, with Democratic registration increasing by more than 14% over the baseline means for these first two rings. The effect stays at comparable levels for the next closest voters, but then starts to decrease for voters living in rings that are further than 31-40 meters from the

home of patronage employees. The effect fades out with distance until at a distance of 140-149 meters, where close to zero additional voters register as Democrats.[35]

Figure 7. Electoral Return to Patronage, by Distance to Recipient



Notes: This figure presents coefficients and 95% confidence intervals for the effect of patronage on electoral support in rings of 10 meter width at increasing distances from the address of recipients (from less than 10 meters around the address to 140-149 meters). See the notes of Table 1, column 3, for details on the underlying regression. For comparison, treatment effects are transformed to percentage increases over the baseline mean number of registered Democrats in each ring around the address.

The spatial pattern of increasing electoral support concentrated around the residential address of patronage employees suggests that patronage mobilizes voters who are private acquaintances of the recipient.[36] The evidence presented in this section speaks against mechanisms of patronage that work through the public services performed by patronage employees. If voters respond to the police work of patronage employees, we would instead expect the electoral return to be concentrated around the beat patrolled by the patrolman. It was active NYPD policy to not allocate patrolmen to beats that included their home, but this is exactly where patronage generated the greatest electoral return.[37]

[35]The effect at 140-149 meter distance distance is equal to 1.9% with a p-value of 0.42.

[36]The evidence of heterogeneity by borough, presented in Appendix Table A1 is also compatible with this interpretation. The electoral response is larger in denser neighborhoods (in Manhattan and Brooklyn) and zero in more sparsely populated boroughs (Bronx, Queens, and Staten Island).

[37]The NYPD made an effort to allow patrolmen to work for police precincts relatively close to

5.2 Electoral Response to Patronage

After showing in the preceding section that patronage mobilised the votes of the closest neighbors of recipients, this section discusses argues that such behavior is best described as an electoral *response* by patronage employees. An alternative explanation of patronage jobs as the *reward* for past political services is less consistent with the evidence. The timing of the effect documented in the event study of Figure 2 already tells us that the mobilisation of votes does not precede the distribution of patronage. If the electoral return to patronage is driven by a clientelistic *quid pro quo* relationship between applicants as clients and party leaders as patrons, it does not seem to work through applicants mobilizing votes in the hope of receiving patronage as a reward. Instead, electoral support comes after jobs are distributed.

Patronage employees likely realize that they owe their job to the discretion of party leaders. The results of the civil service exams are widely publicised, which makes it easy for patrolmen to learn their status as patronage or merit employees. They might understand that their appointment is part of a reciprocal relationship and that the response expected of them is to mobilise votes for the party of their patron (i.e. the Democrats). The remaining question left to answer is why patronage employees would comply with such demands after they start their job as patrolmen. What sustains the *quid pro quo* relationship?

5.3 Promotion Incentives

In this section, I analyse the incentive structure for patrolmen in the NYPD. Patronage employees could be incentivized to mobilise the votes of their neighbors if this improves their personnel outcomes. If promotions to higher ranks in the police force are granted as a reward for electoral support, it would explain why neighborhoods become more Democratic *after* patronage employees start their job and why support *stays* at elevated levels for many years. Promotion incentives only kick in with the entry into police service, and exits from the force are relatively rare.

In this section, I investigate the relationship between electoral support and promotions. I focus on promotions to the rank of sergeant, the rank immediately above patrolmen and the first step on the supervisory career track. Promotion from patrolman to sergeant were *de jure* governed by civil service rules, but political leaders could *de facto* use discretion to influence the decisions — just like in the initial se-

their home, but when allocating them within the precinct they should not patrol beats that include their home address.

lection to patrolman positions. We therefore have reason to believe that patronage could play a role in promotions.

5.3.1 Empirical Strategy: Predictors of Promotions

To formally test whether the mobilisation of Democratic voters predicts promotions of patronage employees, I estimate the following equation in the panel data on the careers of police officers i serving in calendar year t as a linear probability model:

$$\text{promotion}_{it} = \beta \text{patronage}_i \times \Delta\text{votes}_{it} + \eta \text{patronage}_i + \lambda \Delta\text{votes}_{it} + \mu \text{X}_{it} + \epsilon_{it} \quad (3)$$

where patronage_i is equal to 1 if police officer i received his position as a patrolman through patronage, and zero otherwise. The variable Δvotes_{it} measures the change in electoral support in the 50 meter neighborhood around the residential address of officer i in year t. The change in electoral support is computed as the percentage change in the number of registered Democrats in the neighborhood between year t and the 6-year average before police offers start their job.[38] The outcome of interest, promotion_{it}, is a dummy variable that takes value one in year t when officer i gets promoted from patrolman to sergeant. The controls X_{it} include precinct-year fixed effects and fixed effects for the hiring period of each employee. These controls ensure that we are comparing employees in the same precinct in the same year, and that we adjust their promotion chances for the time that has elapsed since patrolmen were hired. Standard errors ϵ_{it} are clustered at the level of the police precinct.

There are two motivations to focus on this measure of electoral support. First, it closely approximates the contributions of individual officers to the difference-in-differences estimate of the electoral return to patronage of section 4. Second, such changes in Democratic registration before versus after patrolmen start their job should be easy for local party leaders to monitor. Party leaders can then act on this proxy for the political service of police officers when intervening in promotion decisions.

In a meritocratic organization, performance on the job should be a predictor of promotions. To directly compare individual performance to mobilization of local electoral support, I estimate the following close variation of Equation 3 for each police officers i in year t:

$$\text{promotion}_{it} = \beta \text{patronage}_i \times \text{perform}_{it} + \eta \text{patronage}_i + \lambda \text{perform}_{it} + \mu \text{X}_{it} + \epsilon_{it} \quad (4)$$

[38] For officers with less than six years of pre-periods, and I instead compute the average for all pre-periods with available voter registry data.

where the variable $perform_{it}$ measures the performance of officer i as the number of days pay the officer got deducted in fines for misconduct in year t. Higher fines proxy for worse performance. All other variables are defined as in Equation 3.

5.3.2 Results on Electoral Support, Performance, and Promotions

Figure 8 presents predictive margins of voter mobilisation (Panel a) and performance (Panel b) on the promotion chances of patrolmen separately by patronage or merit status. Patronage employees are more likely to get promoted when more of their neighbors register as Democrats (see Panel a). There is no such pattern for patrolmen that entered the police force on their own merit. These results are compatible with promotion incentives driving the electoral return to patronage. Panel (b) of Figure 8 reveals that the promotion chances of merit employees are increasing in their performance (or decreasing in the number of days pay deducted for misconduct), while performance does not matter for the promotions of patronage employees. Taken together, these results suggest that patrolmen are on a different career track if they entered the police force through patronage.[39]

Figure 9 directly compares the promotion rates of patronage and merit employees at the same level of electoral support (Panel a) or performance (Panel b). When electoral support drops or performance is the highest (i.e. zero days pay deducted), patronage employees are less likely to get promoted than merit employees. But at increasing levels of electoral support, patronage employees catch up and get promoted at the same and potentially higher rates (see Figure 9a).[40] Patronage employees are also protected from the consequences of bad performance. For example, patrolmen that received fines of 10 days pay deducted are more likely to be made sergeants if they are patronage employees (see. Figure 9b).

Reverse causality could be a potential concern when interpreting promotions and performance (or electoral support) in the same year. Promotions might be associated with better performance, for example, if sergeants receive fewer fines than patrolmen. Appendix Figure A3 assuages such concerns by replicating the same patterns as Figure 9 for the relationship between promotion and electoral support (Panel a)

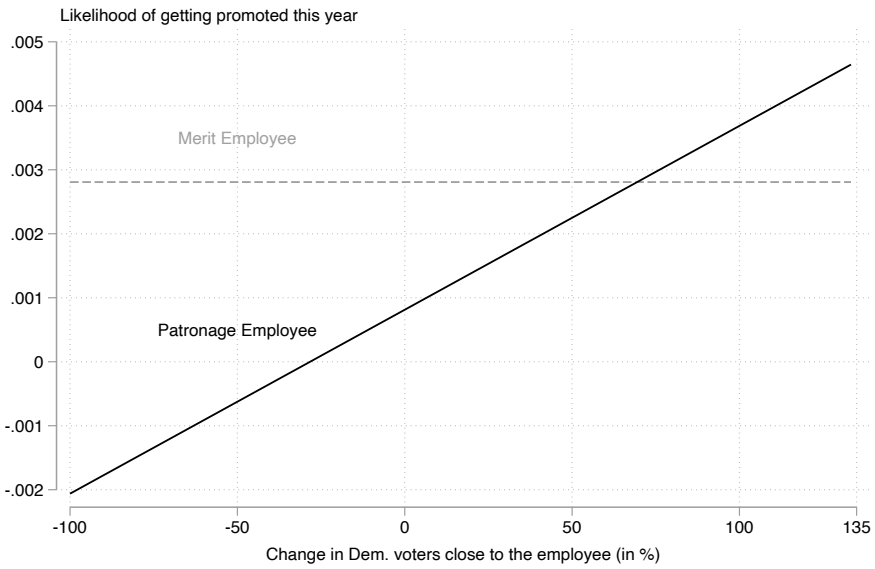
[39]Promotions to the rank of sergeant are very rare. This is partly due to the focus of this paper on the first years of police officers' careers. Even in later years, promotions are not guaranteed and many officers stay at the rank of patrolmen. This suggests that any electorally motivated promotions could have especially pernicious effects on the internal governance of the police department.

[40]Promotion rates of patronage employees are higher than for merit employees if Democratic registration increases by more than 75%, but the difference is not significant at the 5% level.

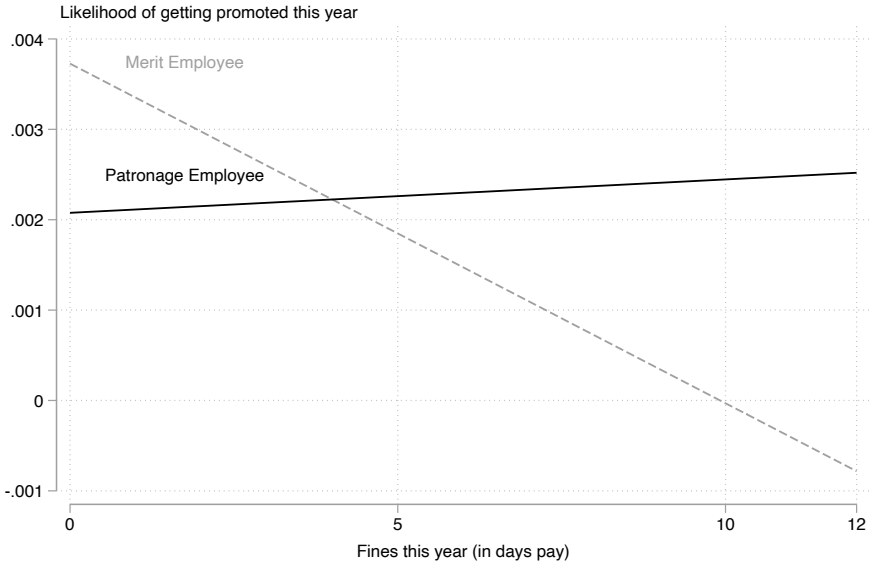
and performance (Panel b) in the *previous* year.

Figure 8. Determinants of Promotions for Patronage vs. Merit Employees

(a) Promotions and Democratic Registration, Predictive Margins



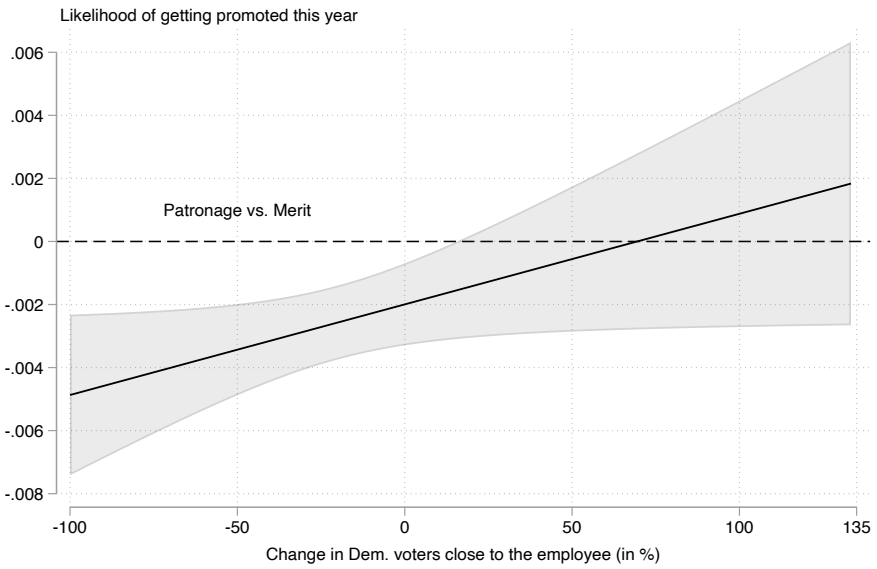
(b) Promotions and Performance, Predictive Margins



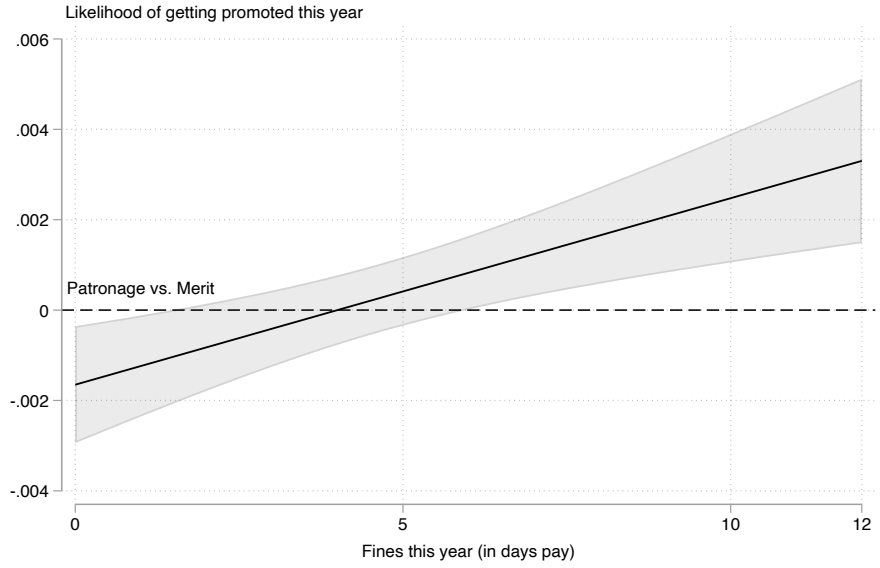
Notes: This figure presents the predicted likelihood for patronage and merit patrolmen to get promoted to sergeants, conditional on the change in electoral support in their neighborhood (Panel a) or their performance (Panel b). Predictive margins are estimated from Equations 3 and 4. Performance is measured in the number of day's pay deducted in fines. More fines proxy for worse performance. Electoral support is measured as the percentage change in the number of registered Democrats in the 50 meter neighborhood of the employee this year in comparison to the average of (up to) six years before the patrolman started their job. The percentage change in registration is winsorized at 5%. See section 5.3.1 for details.

Figure 9. Differences in Promotion Chances for Patronage vs. Merit Employees

(a) Promotions and Democratic Registration, Marginal Effects



(b) Promotions and Performance, Marginal Effects



Notes: This figure presents the average marginal effects of patronage vs. merit status of patrolmen on their likelihood of getting promoted to sergeants, conditional on the change in electoral support in their neighborhood (Panel a) or their performance (Panel b). See the note to Figure 8 for details on the variables and how the margins are estimated. Standard errors for the 95% confidence intervals shown here are clustered at the level of the police precinct. Appendix Figure A3 repeats the same exercise as this figure but with last year's performance and electoral support as predictors of promotions.

6 The Performance Implications of Patronage

If the selection and promotion of public employees is electorally motivated, political leaders might trade off performance costs for electoral returns. Alternative explanations of patronage jobs (e.g., the use of private information to identify better applicants (Voth and Xu, 2022), or the selection of ideologically aligned and potentially more motivated applicants (Spenkuch et al., 2023)) would predict that patronage employees perform better than eligible applicants who were passed over. A direct test of this prediction is made impossible by the necessary absence of performance information for applicants who never receive the job. Instead, I explore two closely related empirical exercises: First, I investigate whether test scores in civil service entrance exams predict the performance of appointed patrolmen. In a second test, I compare the performance of patrolmen who received the job through patronage with those who received the job by their own merit. I conclude by considering test scores and patronage status jointly and discuss the potential contributions of selection and incentives to the performance costs of patronage.

6.1 Civil Service Exam Results and Performance

To investigate if exam results predict performance, I estimate the following equation for each police officer i in year t:

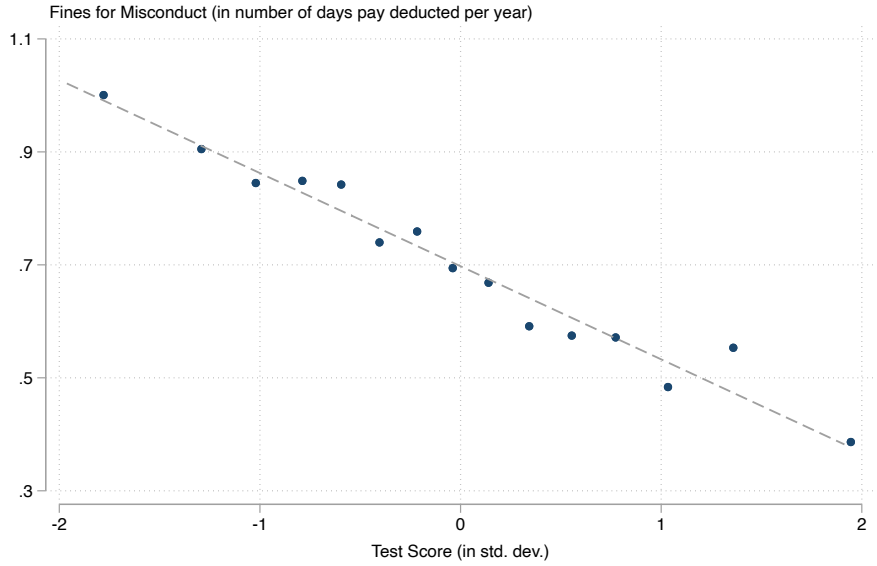
$$\text{perform}_{it} = \beta \text{scores}_i + \mu \text{X}_{it} + \epsilon_{it} \tag{5}$$

where the variable scores_i measures the entrance exam results of officer i, standardized to mean zero and standard deviation 1. The performance outcome is the amount of fines for misconduct (measured in the number of days pay deducted) that officer i received in year t, and β is the coefficient of interest to test the correlation between test scores and performance. To ensure that I am comparing employees in the same precinct in the same year, I include a full set of precinct-year fixed effects in the vector of control variables X_{it}. I also include hiring period fixed effects in X_{it} to adjust for potential variation in the content of the entrance exams across periods. Standard errors ϵ_{it} are clustered at the level of the police precinct.

Figure 10 reports the relationship between performance and test scores from Equation 5 as a binned scatter plot. There is a strong linear relationship between test scores and actual performance. Appendix Table A4 reports the regression results. Each standard deviation reduction in test scores in the entrance exam is associated

with extra fines worth pay deductions of 0.17 days per year (col. 5, Appendix Table A4. In comparison to the mean amount of fines, this is an increase in fines for misconduct equal to a 23.9% reduction in performance. This suggests that entrance exams test for skills or character traits that make for good policemen. Patronage appointments that ignore these results are likely not driven by performance motives.

Figure 10. Binned Scatter Plot of Performance and Test Scores



Notes: This figure presents the relationship between test scores in the civil service entrance exam and the performance of hired police officers as a binned scatter plot. Test scores are standardized to mean zero and standard deviation one. Performance is measured as the number of days pay deducted in fines, with greater fines suggesting worse performance. See Appendix Table A4, column 5, for regression output from the underlying specification of this figure. The relationship between test scores and performance is estimated following Equation 5 and includes fixed effects for police precinct and year interactions, as well as fixed effects for the period in which patrolmen were hired.

6.2 Patronage and Performance

To directly compare the performance of patronage and merit employees, I repeat a similar exercise and estimate the following equation for each police officer i in year t:

$$\text{perform}_{it} = \beta \text{patronage}_i + \mu \text{X}_{it} + \epsilon_{it} \tag{6}$$

where patronage_i is a dummy variable indicating whether officer i was appointed through patronage. All other variables and estimation choices remain the same as in Equation 5.

Table 2 reports regression results on the relationship between patronage and performance from estimating Equation 6. Patronage employees perform notably worse across all specifications. When compared to patrolmen who entered the police force meritocratically during the same hiring period, and work in the same precinct in the same year, patronage employees get 0.16 extra days pay deducted in fines per year (Table 2, col 5.). This amounts to 22.7% worse performance than the average patrolmen, comparable in magnitude to the performance losses associated with one standard deviation lower test scores in the entrance exam (cf. Appendix Table A4, col. 5).

Table 2. Relationship Between Patronage Status and Performance

	(1)	(2)	(3)	(4)	(5)
Patronage	0.174***	0.151***	0.152***	0.147***	0.157***
	(0.055)	(0.053)	(0.052)	(0.054)	(0.053)
Outcome Mean	0.691	0.691	0.691	0.691	0.691
Observations	38439	38438	38438	38364	38364
R-squared	0.000	0.013	0.018	0.052	0.052
Precinct FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Precinct-Year FE	No	No	No	Yes	Yes
Hiring Period FE	No	No	No	No	Yes

Notes: This table reports regression results from estimating the association of patronage status with performance following Equation 6. The outcome for all columns is yearly performance, measured as the number of days pay deducted in fines. Greater fines proxy for worse performance. Police officers are coded as *Patronage* if they received their job without having the required test scores. Columns 2-5 phase in fixed effects for the police precinct (col. 1), the year (col. 2), precinct-year interactions (col. 4), and the period during which the officer got hired (col. 5). Observations are at the police officer-year level. Standard errors in parenthesis are clustered at the level of the police precinct. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

These performance differences are mainly driven by patronage employees neglecting their official duties. Appendix Figure A4 reports the relationship between patronage and performance separately for each type of misconduct (negligence, misbehavior, and abuse) that officers can be fined for. Patronage employees receive 22.6% more fines for negligence than merit employees. Coefficients for misbehavior and abuse are of comparable size but not significant (at the 5% level). More than 85% of all fines for misconduct are due to negligence. Frequent examples of negligence include patrolmen leaving their posts unattended, reporting late for duty, or failing to fulfill their tasks (e.g. making arrests or filing the proper reports).

6.3 Selection and Incentives

Given the positive relationship between entrance exam scores and performance (see Figure 10) it is natural to blame selection for the poor performance of patronage employees. The selection of patronage employees by definition deviates from exam results, which are valuable signals of performance. Incentives could still contribute to the performance differences, either by exacerbating or attenuating the negative effects of selection. The evidence presented in Section 5.3 suggests that patronage and merit employees are on different career tracks and that patronage employees face weaker performance incentives.

Table 3 tests whether there is a relationship between patronage and performance even when comparing employees with similar test scores. This empirical exercise leverages variation in eligibility cut-offs across eligible lists. Some patronage employees from one list would have merited employment with the same test results if they applied during other hiring periods. Columns 1 and 2 replicate the specifications of Column 4 in Tables 2 and A4 with patronage status or test scores as the only independent variable.[41] When including both patronage status and test scores in the same specification (col. 3), patronage employees still perform worse than their peers. Test scores have explanatory power for performance, even conditional on patronage status. The negative association between patronage and performance does not depend on assuming a linear relationship between test scores and performance (col. 4).

This evidence is compatible with both selection and incentives contributing to the performance costs of patronage. Patronage employees are negatively selected and their promotion incentives are tied to mobilizing the votes of their neighbors instead of performing their official duties. The same mechanism that helps sustain the *quid pro quo* relationship and drive electoral returns exacerbates the performance costs of patronage.

[41] To allow comparisons across eligible lists, these specifications do not include fixed effects for the hiring period. The relationship between patronage and performance in Table 3, column 1, is stronger than in Table 2, column 4. Table 3 focuses on the sample of employees with test score information. A large share of the patronage employees without test score information received the job without applying and they perform better than patronage employees with low test scores.

Table 3. Patronage and Performance, Controlling for Test Scores

	(1)	(2)	(3)	(4)
Patronage	0.254***		0.158**	0.165**
	(0.077)		(0.075)	(0.080)
Test Score		-0.097***	-0.076***	-0.074***
		(0.023)	(0.021)	(0.022)
Test Score Squared				-0.006
				(0.015)
Outcome Mean	0.696	0.696	0.696	0.696
Observations	36019	36019	36019	36019
R-squared	0.054	0.054	0.054	0.054
Precinct FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Precinct-Year FE	Yes	Yes	Yes	Yes
Hiring Period FE	No	No	No	No

Notes: This table reports regression results from estimating the association of patronage status with performance in the sample of patrolmen with test score information following Equation 6. The outcome for all columns is yearly performance, measured as the number of days pay deducted in fines. Greater fines proxy for worse performance. Police officers are coded as *Patronage* if they received their job without having the required test scores. *Test Score* are standardized z-scores with mean 0 and standard deviation 1 of the civil service entry exams. All columns include controls for precinct fixed effects, yearly fixed effects, and their interaction. Observations are at the police officer-year level. Standard errors in parenthesis are clustered at the level of the police precinct. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

7 Conclusion

Meritocratic bureaucracies are commonly viewed as important foundations of effective states (Weber, 1922). Despite their importance for state capacity, the public sector personnel of many modern states is still selected via patronage. Conventional wisdom blames electoral motives for such deviations from meritocracy. But quantitative evidence on the electoral returns to patronage remains scarce. In this paper, I studied bureaucratic selection under the paradigmatic political machine in the U.S. history: Tammany Hall in New York City. I show that appointments to patrolmen positions in the NYPD during 1900-1916 frequently did not follow the civil service rules of the time, and that these patronage appointments delivered an electoral return. Leveraging detailed personnel records and individual-level voter registration, I provide evidence suggesting that recipients of patronage jobs are incentivized to mobilise the votes of their neighbors.

Electoral returns to patronage imply that it can be attractive for politicians to undermine meritocratic selection. This likely has negative welfare consequences. I find that patronage employees deliver worse performance, complementing the findings of previous research on the positive effect of civil service reforms on state effectiveness (Aneja and Xu, 2023; Moreira and Pérez, 2021). In addition to the direct performance costs, theory predicts that votes generated through clientelistic transfers can undermine electoral competition to under-provide public goods (Bardhan and Mookherjee, 2018; Robinson and Verdier, 2013).

Much of social science on institutional modernization and state development depicts institutional change as a process in which traditional institutions are replaced by modern ones. Well-identified studies of meritocratic practices often evaluate the impact of important reforms. In contrast, the research design in this paper leverages the remaining variation from patronage appointments which deviate from official but imperfectly enforced civil service rules. The results presented here document how meritocratic selection and performance incentives get undermined but not eliminated in a politicized bureaucracy. Some positions are filled with patronage employees who mobilize electoral support, but most appointments follow the civil service system. Patronage employees perform worse and neglect some of their duties, but they still get fined for their misconduct. Promotions on average go to better performing bureaucrats, but some likely serve as rewards for electoral support. This quantitative case study of patronage in Progressive Era New York City reveals how traditional institutions can coexist alongside modern institutions, interacting with them, and

shaping their function.

These findings have important implications for our understanding of how emerging states select and incentivize their bureaucracies. Civil service reforms alone did not eradicate patronage. Neither were the secret ballot or other progressive era reforms enough to eliminate vote buying and political machines. Tammany Hall remained dominant until the 1930s. Similar patronage arrangements still exist today in Latin America, Africa, or Asia even in countries with strict *de jure* civil service rules. How did these rules eventually get enforced in the U.S? American political development can offer lessons on which economic, social, and cultural changes may have relieved governments from the capture by political machines. More research is needed, for example, into the impacts of social policies (e.g., the New Deal reforms in the 1930s) and whether some might have weakened the demand for patronage among voters.

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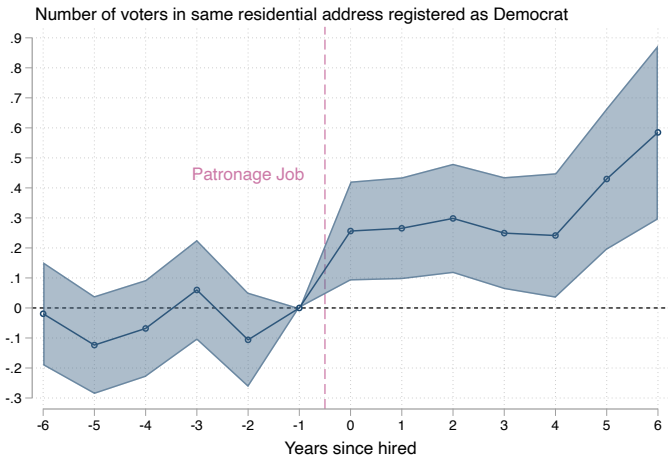
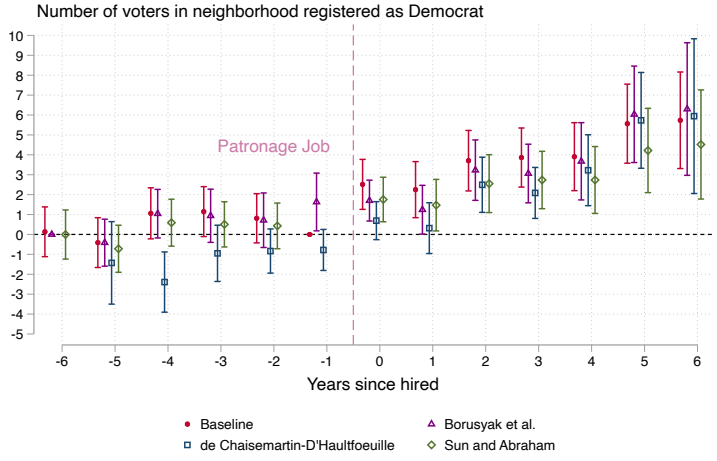
A Appendix Figures and Tables

Figure A1. Event Study of Electoral Return at Address of Patronage Recipients



Notes: This figure presents the dynamic treatment effect of patronage on electoral support at the residential address of patronage recipients (in comparison to the address of control applicants) with 95% confidence intervals. This repeats the empirical exercise of Figure 4, but with outcomes measured at the exact address instead of in 50 meter neighborhoods.

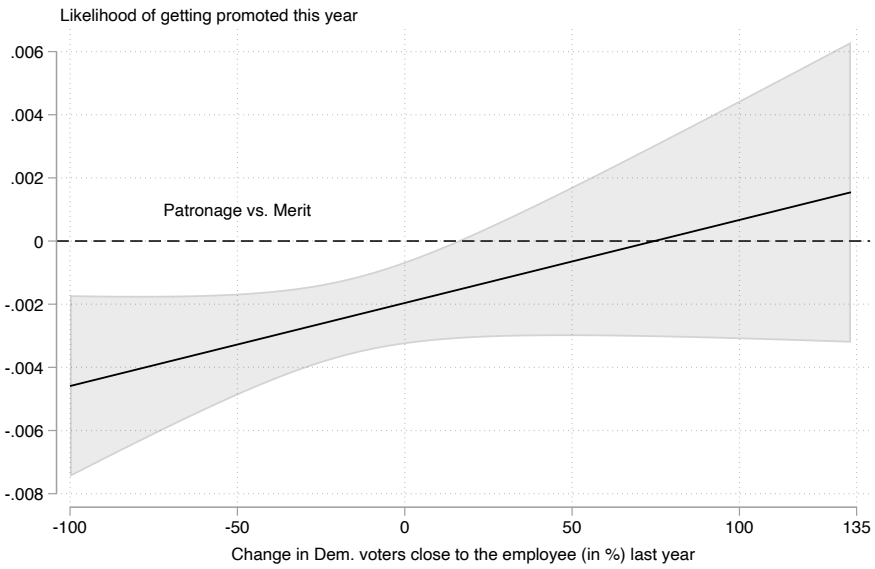
Figure A2. Comparison of Estimates from Alternative Event Study Models



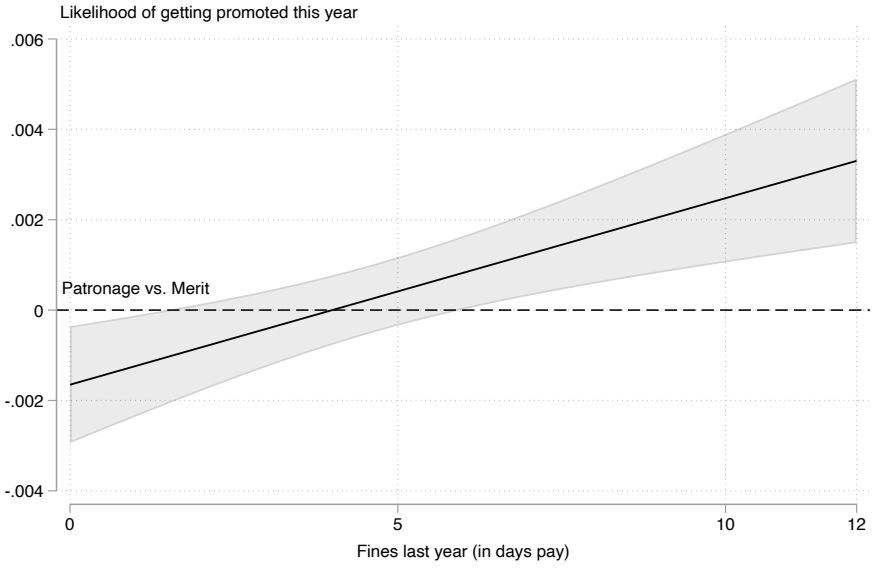
Notes: This figure compares the "Baseline" dynamic treatment effect of patronage on electoral support with 95% confidence intervals (following the specification of Table 1, column 3, and as displayed in Figure 4) with estimates from alternative models as proposed by Borusyak et al. (2023), de Chaisemartin and D'Haultfœuille (2023), and Sun and Abraham (2021). The de Chaisemartin and D'Haultfœuille (2023) model could only be estimated for five pre-periods.

Figure A3. Differences in Promotion Chances for Patronage vs. Merit Employees

(a) Promotions and Democratic Registration, Marginal Effects

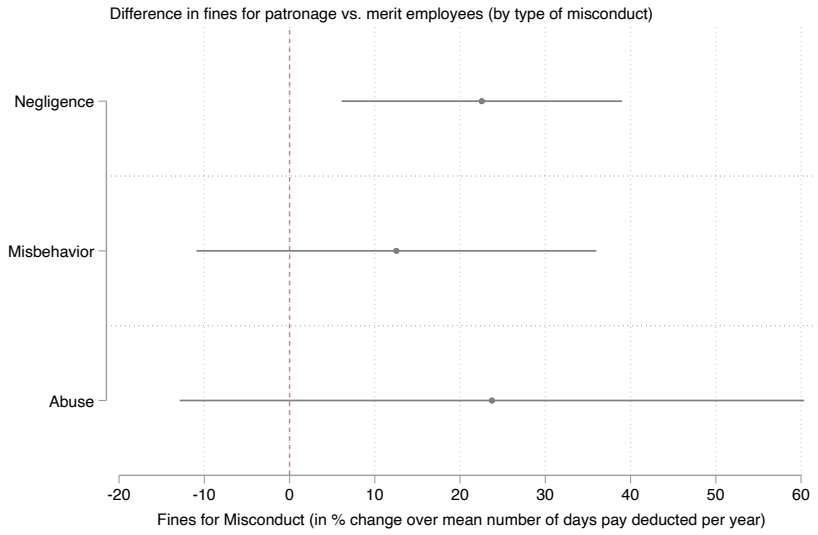


(b) Promotions and Performance, Marginal Effects



Notes: This figure presents the average marginal effects of patronage vs. merit status of patrolmen on their likelihood of getting promoted to sergeants, conditional on the change in electoral support in their neighborhood last year (Panel a) or their performance last year (Panel b). This repeats the same exercise as Figure 9, but with last year's electoral support and performance (instead of in the same year as the promotion). See the note to Figure 8 for details on the variables and how the margins are estimated. Standard errors for the 95% confidence intervals shown here are clustered at the level of the police precinct.

Figure A4. Patronage and Performance, by Type of Misconduct



Notes: This figure presents coefficients on the relationship between patronage status of police officers and their performance. Performance is measured as the number of days pay deducted in fines per year, with greater fines suggesting worse performance. The figure plots coefficients for separate regressions of each type of misconduct (negligence, misbehavior, abuse) and following the specification of Table 2, col. 5. The outcome for each regression is the yearly amount of fines for that type of misconduct. Coefficients are standardized to percentage changes over the average amount of fines police officers receive per year for that type of misconduct. Standard errors are clustered at the level of the police precinct, and the figure reports 95% confidence intervals.

Table A1. Patronage Jobs and Democratic Registration, by Borough

	(1) Manhattan	(2) Brooklyn	(3) Other
Patronage Appointment	4.33**	2.31***	-0.28
	(1.75)	(0.75)	(1.27)
Outcome Mean	44.91	24.57	13.39
R-squared	0.81	0.86	0.75
Observations	19404	41040	12048
Patronage Employees	235	270	109
Control Applicants	1382	3150	895
Individual FE	Yes	Yes	Yes
Event Year FE	Yes	Yes	Yes
Application Period x Year FE	Yes	Yes	Yes

Notes: This table reports difference-in-difference estimates of the effect of patronage (i.e. coefficient β of Equation 1) and following the specification of Table 1, column 2. The outcome for all columns is the number of registered Democrats within a 50 meter neighborhood around the applicant. See the notes to Table 1 for details on the outcome and specification. Column 1 only includes neighborhoods in Manhattan, while col. 2 focuses on Brooklyn, and col. 3 pools the smaller boroughs of Bronx, Queens, and Staten Island. Standard errors in parenthesis are clustered at the level of applicants' neighborhoods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A2. Patronage Jobs and Voter Registration Outcomes, by Political Party

	(1) D Voters	(2) R Voters	(3) D - R Margin
Patronage Appointment	2.95***	1.35***	1.60***
	(0.57)	(0.29)	(0.52)
Outcome Mean	28.67	14.35	14.31
R-squared	0.85	0.79	0.81
Observations	72492	72492	72492
Patronage Employees	614	614	614
Control Applicants	5427	5427	5427
Individual FE	Yes	Yes	Yes
Event Year FE	Yes	Yes	Yes
Application Period x Year FE	Yes	Yes	Yes
Borough x Year FE	Yes	Yes	Yes

Notes: This table reports difference-in-difference estimates of the effect of patronage (i.e. coefficient β of Equation 1) and following the specification of Table 1, column 3. Column 1 replicates Table 1, column 3, and keeps the number of registered Democrats within a 50 meter neighborhood around the applicant as the outcome. The outcome variable for col. 2 is instead the number of registered Republicans, and the outcome for col. 3 is the difference between Democratic and Republican registration. See the notes to Table 1 for details on the specification. Standard errors in parenthesis are clustered at the level of applicants' neighborhoods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A3. Relationship Between Election Results and Voter Registration

	(1)	(2)	(3)	(4)	(5)
Dem. share of registration	0.726***	0.764***	0.796***	0.796***	0.797***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Observations	150039	150039	150039	150039	150039
ED-Year Obs.	17716	17716	17716	17716	17716
R-squared	0.440	0.485	0.565	0.611	0.728
Borough FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Office FE	No	No	No	Yes	Yes
Office-Year FE	No	No	No	No	Yes

Notes: This table reports results from regressions with the Democratic vote share in elections as the outcome and the share of Democrats among registered voters as the main independent variable. Both variables are winsorized at 1%. Observations are at the level of the polling place by year and election. Polling places are equivalent to election districts (EDs). In most years and EDs there are candidates for more than one elected office on the ballot. Columns 2-5 phase in fixed effects for the borough, for the election year, the elected office (e.g. mayor or city councillor), and office by year time trends. Figure 6 presents a binned scatter plot of the relationship from column 5 of this table. Standard errors are clustered at the ED-year level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A4. Relationship Between Performance and Civil Service Exam Test Scores

	(1)	(2)	(3)	(4)	(5)
Test Score	-0.108***	-0.107***	-0.099***	-0.097***	-0.165***
	(0.021)	(0.023)	(0.023)	(0.023)	(0.026)
Outcome Mean	0.691	0.691	0.691	0.691	0.691
Observations	36098	36097	36097	36019	36019
R-squared	0.001	0.014	0.019	0.054	0.056
Precinct FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Precinct-Year FE	No	No	No	Yes	Yes
Hiring Period FE	No	No	No	No	Yes

Notes: This table reports regression results from estimating the association of patronage status with performance in the sample of patrolmen with test score information following Equation 5. The outcome for all columns is yearly performance, measured as the number of days pay deducted in fines. Greater fines proxy for worse performance. *Test Score* is the z-score with mean 0 and standard deviation 1 of the civil service entry exam results. Columns 2-5 phase in fixed effects for the police precinct (col. 1), the year (col. 2), precinct-year interactions (col. 4), and the period during which the officer got hired (col. 5). Observations are at the police officer-year level. Standard errors are clustered at the level of the police precinct. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.