The Electoral Return to Patronage*

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Abstract

Why do politicians often rely on patronage, instead of selecting the most qualified applicants for public employment? Does patronage deliver an electoral return? I combine newly digitized personnel records on the selection 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. Linking the results of civil service exams with complete lists of NYPD employees reveals that 21% of patrolmen hired in the period lacked the required test scores. These patronage employees were more likely to be connected to the leaders of Tammany Hall, the city's incumbent Democratic Party organisation at the time. I use a difference-in-differences design around the entrance date of patronage recruits to estimate the electoral return to patronage jobs. Patronage resulted in a 13.9% increase in registered Democratic voters within a 50 meter radius around the employee's address. This electoral response – and complementary results – suggest that patronage employees reciprocated by mobilising the votes of their neighbors. These private electoral benefits of patronage came with public costs: Patronage employees performed considerably worse than their meritocratically selected peers. I provide suggestive evidence that these costs were exacerbated by an electorally motivated pattern of promotions, which sustained the *quid pro quo* relationship.

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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 Richmond), 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 15.7% 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 6.3 voters within a 50 meter radius of the patronage employee's address. This is an increase of 13.9% over the baseline mean of 45.3 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 100 meters 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 evidence against reverse causality. 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 a *reciprocal* response to patronage. Consistent with an electoral return driven by the reciprocity of patronage recipients, I find that the return is larger for patrolmen who received their jobs through a greater deviation from civil service rules. The electoral return tied to patronage employees scales with the size of the gift they received from their political patron.

Can pure reciprocity explain 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 greater turnout of 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

[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 booth level), I use registration as a proxy for actual voting decisions. The terms "voter registration" and "electoral support" are used interchangeably in this paper.

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.

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

[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).

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. 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

[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)

number of studies have documented how politicians around the world share rents and distribute public resources or selectively target government programs to the private 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. I also find an important role for reciprocity in driving the effect of patronage, which resonates with the results of Finan and Schechter (2012) and Lawson and Greene (2014) for vote buying. 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

[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.

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 general 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 Data and Descriptive Statistics

This section introduces the data on applications, civil service exam results, and the employment of patrolmen in the NYPD. I describe how I link the records from these three sources to identify which patrolmen received their jobs through patronage. Lastly, I provide descriptive statistics on the distribution of patronage jobs and the performance of patronage employees.

2.1 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.[16] 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.[17]

Figure 1 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.

[16] 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.

[17] See the Data Appendix for more detail on how I link employees to eligible lists and how I identify the periods during which eligible lists are active.

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".

A potential concern with this approach could be if some of the eligible applicants (i.e. with Rank ≤ 10 in Figure 1) 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 1 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.[18]

Figure 1. 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

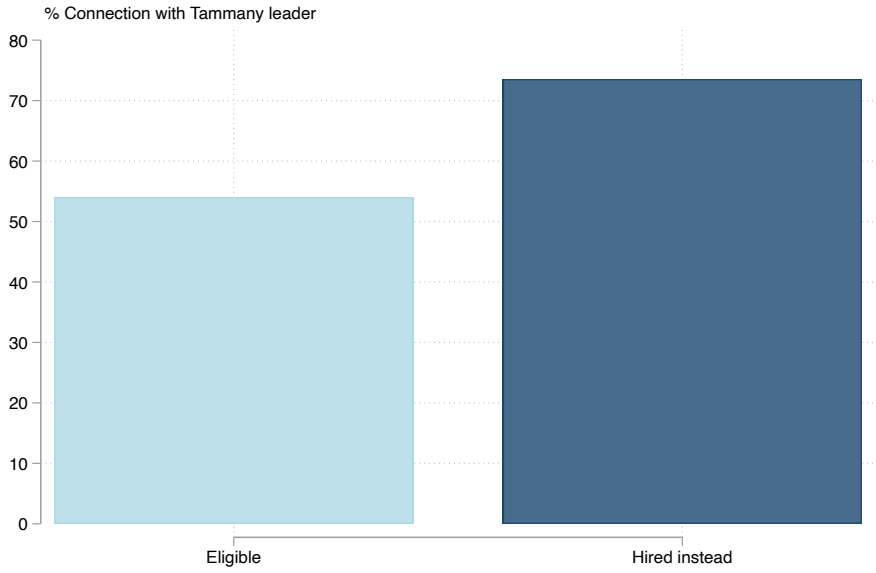
Notes: This figure presents a stylized example of an eligible list with ten appointments (marked with green background). Three employees received the job despite having test scores that placed them outside of the top ten. These patronage employees are marked in bold font. I refer to the other seven names 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".

[18] See Table 1, columns 2 and 3 for a comparison of the electoral return to close and far deviations from merit.

2.2 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. Tammany Hall was a hierarchical organization with a "Boss" at the top and an Executive Committee made up of local leaders from each Assembly District of Manhattan and the Bronx. The boroughs of Brooklyn, Queens, and Staten Island had equivalent patronage organisations that informally 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.

Figure 2. 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 (left bar), and among the applicants who actually received the jobs (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.

Motivated by this institutional knowledge, 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 2 compares the share of connected applicants within two groups: Those eligible for patrolman jobs (left bar), and those who actually receive the jobs (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.

2.3 Patronage and Performance

If the distribution of patronage is electorally motivated, political leaders might trade off performance costs for electoral returns. Alternative explanations for the distribution 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 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.

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 conduct of individual officers and decided whether they should get fined for each complaint. 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, the official publication on the NYC municipal government. 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 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, misconduct, and abuse. Close to 90% of the complaints are concerned with negligence (e.g., not showing up for patrol, or leaving the post early). Misconduct and abuse each make up around 5% of the complaints. The most common form of misconduct was patrolmen getting drunk on the job, and the most frequently alleged abuse was violence against civilians.

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

$$\text{perform}_{it} = \beta \text{scores}_i + \mu X_{it} + \epsilon_{it} \tag{1}$$

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 number of days pay officer i got deducted 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 3 reports the relationship between performance and test scores from Equation 1 as a binned scatter plot. There is a strong linear relationship between test scores and actual 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.

To directly compare the performance of patronage and merit employees, I repeat

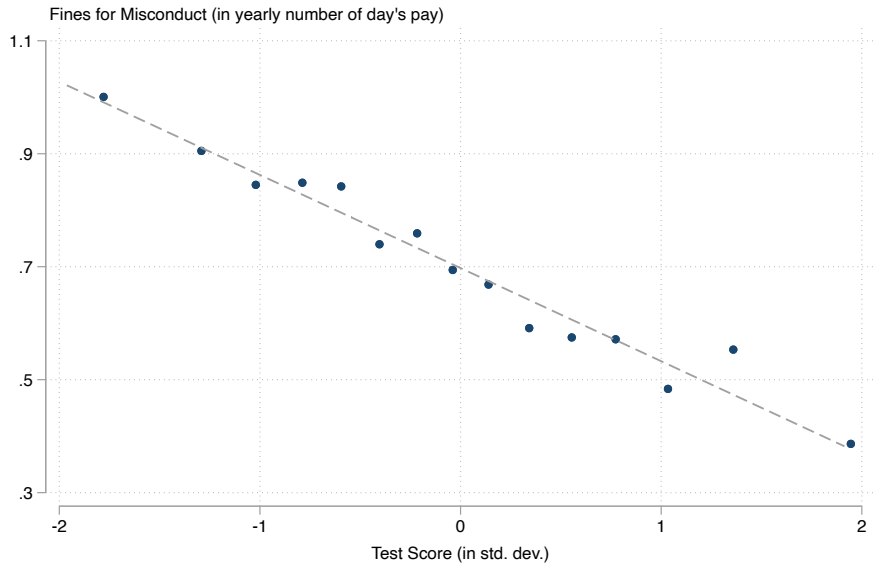
a similar exercise and estimate the following equation each police officer i in year t:

$$\text{perform}_{it} = \beta \text{patronage}_i + \mu X_{it} + \epsilon_{it} \tag{2}$$

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 1.

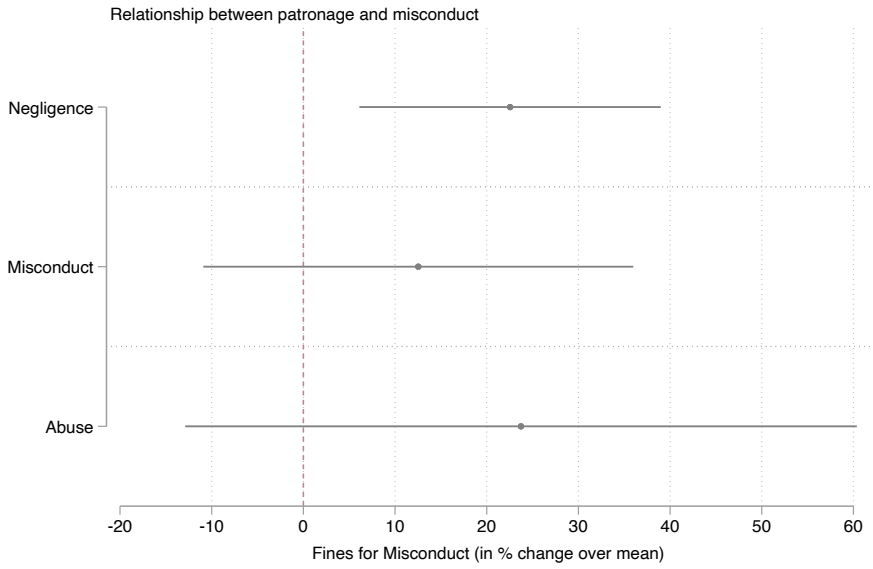
Figure 4 reports the relationship between patronage and performance from estimating Equation 2 separately for each type of complaint that officers can be fined with. Patronage employees are more than 20% more likely than merit employees to receive fines for negligence. Coefficients for misconduct and abuse are of comparable size but not significant (at the 5% level), likely due to the low baseline rates for these types of complaints. These results on the performance of patronage employees, together with the evidence on their connections to local party leaders in the preceding section, speaks against performance motives and in favor of partisan considerations behind the distribution of patronage jobs. The next section formally investigates whether distributing patronage jobs resulted in electoral returns.

Figure 3. 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 the discussion of Equation 1 in section 2.3 for details on the underlying regression and controls.

Figure 4. 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, with greater fines suggesting worse performance. The figure plots coefficients for separate regressions of Equation 2, where the outcome for each coefficient is a different dummy variable indicating what type of misconduct the officer got fined for: Negligence, Misconduct, or Abuse. Coefficients are standardized to percentage changes over the baseline probability of receiving fines of this type. Standard errors are clustered at the level of the police precinct, and the figure reports 95% confidence intervals. See the discussion of Equation 2 in section 2.3 for details on the underlying regression and controls.

3 Estimating the Electoral Return to Patronage

3.1 Empirical Strategy

The 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.[19] 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, X and Y applied in May of 1903 and neither of them received the required test scores to be eligible, but X 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 X and his neighbors after September 1903 to changes in the behavior of Y 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 always 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.

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 X and Y applied during the same period and X started work as a patrolman in $s = 1903$, the event time in 1904 is $t = 1$ for both of them. To capture the effects of patronage on behavior in the immediate neighborhood j of applicant i,

[19] The Data Appendix describes in detail how I link applications, eligible lists, and police employee registers to identify hiring periods and who applied to the same set of job openings.

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:

$$\mathrm{y}_{j(i)t} = \beta \mathrm{patronage}_i \times \mathrm{post}_t + \eta_i + \lambda_t + \mu \mathrm{X}_{it} + \epsilon_{it} \tag{3}$$

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. Other control variables X_{it} for time-varying characteristics of applicants i or their neighborhoods are included for robustness checks. 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 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.[20] 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

[20] 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.

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 3:

$$\mathrm{y}_{j(i)t} = \sum_{k \neq -1} \beta_k \mathrm{patronage}_i \times \lambda_k + \eta_i + \lambda_t + \mu \mathrm{X}_{it} + \epsilon_{it} \tag{4}$$

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

3.2 Effect on Electoral Support

Table 1 presents the main results from Equation 3 on the effect of patronage on electoral support. The baseline specification focuses on the effect in a 50 meter radius around the residential address of applicants. On average, distributing patronage delivered an electoral return of 6.3 extra registered Democrats in the neighborhood of the patronage recipient (Table 1, column 1). In comparison to an average of 45.3 registered Democrats in neighborhoods of unsuccessful applicants, Democratic registration increases by 13.9% in elections after patronage employees begin their job as patrolmen.

The estimated electoral return to patronage is robust to different choices for the neighborhoods of patronage recipients and control applicants. Figure 5 plots the coefficients from a difference-in-differences specification as in Table 1, column 1, but with outcomes measured in neighborhoods ranging from 0 meters (i.e. the same address as the applicant) to a 100 meter radius around the applicants' residence. When scaled to the average number of registered Democrats in the control neighborhood, patronage increases electoral support by 8-16%. The size of the effect peaks at a 40

meter radius and then decreases as treatment gets diluted for larger definitions of the neighborhood.

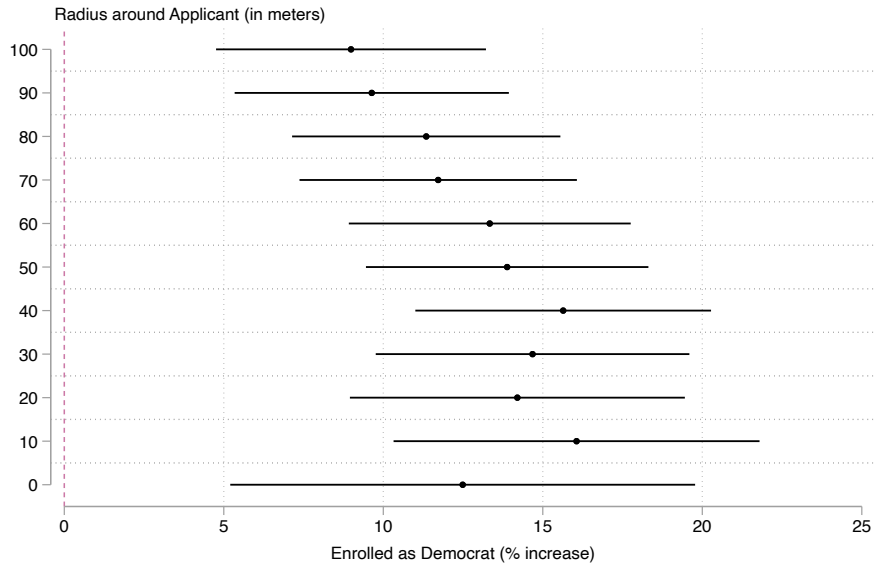
Table 1. Patronage Jobs and Democratic Registration

	(1) All	(2) Close to Eligible	(3) Far from Eligible
Patronage Job	6.29***	6.16***	6.66***
	(1.02)	(1.19)	(1.60)
Mean	45.29	45.29	45.29
Observations	28212	26436	25176
Treated	401	253	148
Control	1950	1950	1950

Notes: This table reports difference-in-difference estimates of the effect of patronage (i.e. coefficient β of Equation 3). The outcome for all columns is the number of registered Democrats within a 50 meter neighborhood around the applicant. Observations are at the neighborhood-year level. *Patronage Job* 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. Column 1 includes all patronage recipients, while columns 2 focuses on patronage recipients with test scores close to the eligibility cut-off, and column 3 only includes recipients of patronage that are far from the cut-off or did not apply at all. Standard errors in parenthesis are clustered at the level of applicants' neighborhoods. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

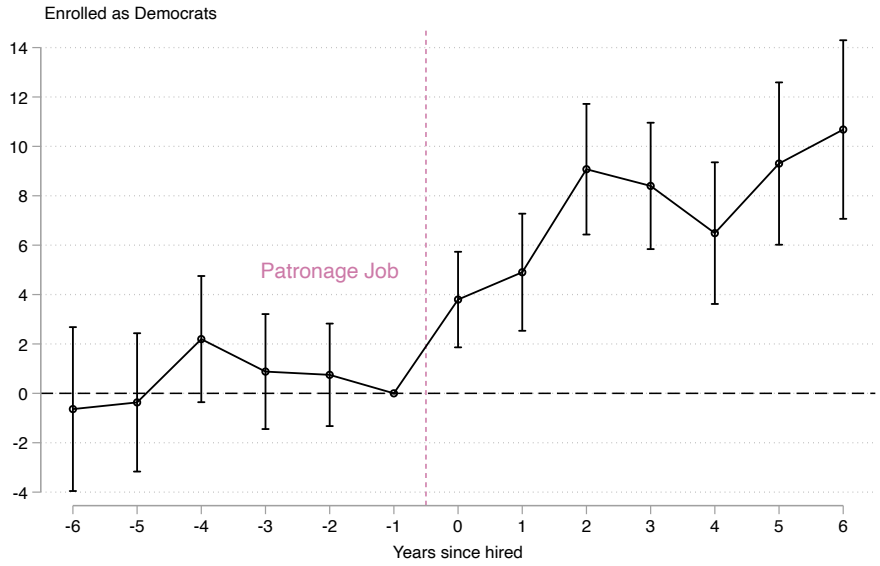
A causal interpretation of the estimates in Table 3 requires that electoral support in neighborhoods with and without patronage recipients would have followed common trends in the absence of any patronage. Figure 6 provides event-study evidence from an estimation of Equation 4 that treated and control neighborhoods were on parallel trends before applicants received their patronage jobs. While no defnitive 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 6 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 the subsequent two years and then stays relatively stable 6-10 extra registered Democrats for each of the four years after that.

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 100 meters. See the notes of Table 1, column 1, for details on the underlying regression.

Figure 6. 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 4). The outcome is the number of registered Democrats within a 50 meter neighborhood. See the notes of Table 1, column 1, for details on the specification.

4 Drivers of the Electoral Return to Patronage

The results in section 3 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.

4.1 Voter Mobilisation

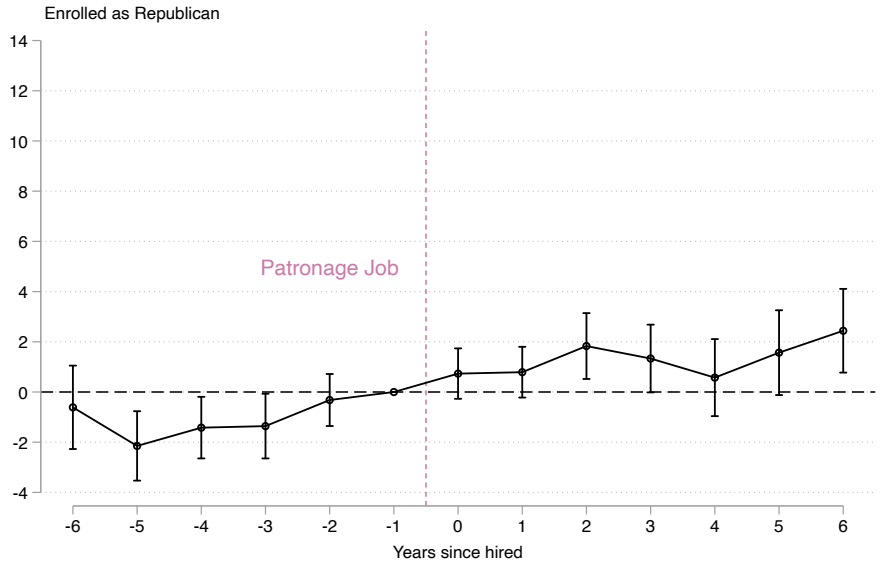
First, I investigate whether the electoral response in neighborhoods of patronage recipients is driven by persuasion or mobilization. Patronage could work through persuasion, for example, if neighbors of patronage recipients positively update about the incumbent party because they now think the party 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). Figure 7 presents event study evidence from estimating Equation 4, but with the number of registered Republicans as the outcome variable. This exercise demonstrates that patronage did not decrease Republican registration. If anything, Republican registration increased slightly. Together with the baseline effects on increased Democratic registration in Figure 6 and Table 1, these results suggest that instead of persuading voters to change their support, patronage mobilized additional supporters of the incumbent party to register.

Next, I explore which voters are mobilized by patronage. Figure 8 plots coefficients of treatment effects from difference-in-difference specifications as outlined in Equation 3 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 8 demonstrates that voters at distances of 1-10 meters from the residential address of patronage recipients show the strongest reaction, with Democratic registration increasing by close to 20% over the baseline mean for that ring. 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 of patronage on electoral support dies out at a distance of 81-90 meters, where close to zero additional voters register as Democrats.

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. 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.[21]

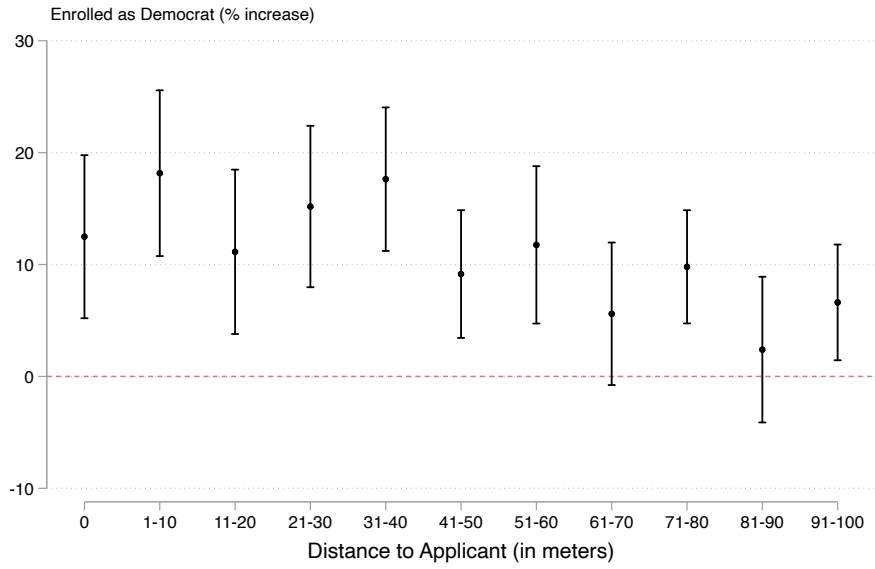
Figure 7. Event Study of Republican Registration Around Receipt of Patronage



Notes: This figure presents the dynamic treatment effect of patronage on Republican registration with 95% confidence intervals (i.e. the event-study coefficients β_k of Equation 4). This replicates the same specification as Figure 6, but with the number of registered Republicans (instead of Democrats) within a 50 meter neighborhood. See the notes of Table 1, column 1, for details on the specification.

[21]The NYPD made an effort to allow patrolmen to work for police precincts relatively close to their home, but when allocating them within the precinct they should not patrol beats that include their home address.

Figure 8. 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; by increasing distance to patronage recipient from 0 (i.e. same address) to 91-100 meters. See the notes of Table 1, column 1, for details on the underlying regression.

4.2 Reciprocity

After showing in the preceding section that patronage mobilised the votes of the closest neighbors of recipients, this section tests reciprocity as one potential explanation for this pattern. Patronage employees and those closest to them 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. If patronage recipients are reciprocal, they might return the favor by mobilising their closest neighbors to support the party of their patron (i.e. the Democrats).

The timing of the effect documented in the event study of Figure 4 already tells us that the mobilisation of votes does not precede the distribution of patronage. If the electoral return to patronage is sustained through 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.

As a test for reciprocity as a driver of this pattern, I check whether the electoral

return to patronage increases with greater deviations from meritocratic recruitment. Columns 2 and 3 of Table 1 repeat the difference-in-difference specification of column 1, but only for the sub-samples of patronage recipient that benefited from a small deviation (col. 1) or a large deviation (col. 2) from merit. A small deviation means that the patrolman would have been eligible for the job if every "passed-over" applicant rejected the offer, while applicants that benefited from a large deviation have even worse test scores or none at all. I find that electoral support increases slightly more for large deviations than small deviations. This is compatible with reciprocity as a partial driver of the electoral return to patronage.

4.3 Promotion Incentives

Having demonstrated in the preceding section that pure reciprocity is unlikely to be the main driver of the electoral return to patronage, I now turn to analysing 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 selection to patrolman positions. We therefore have reason to believe that patronage could play a role in promotions.

4.3.1 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 of police officers i serving in calendar year t:

$$\text{promotion}_{it} = \beta \text{patronage}_i \times \Delta\text{votes}_{it} + \eta \text{patronage}_i + \lambda \Delta\text{votes}_{it} + \mu X_{it} + \epsilon_{it} \quad (5)$$

where patronage$_i$ is equal to 1 if police officer i received his position as a patrolman through patronage,[22] 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 in the period before police offers start their job.[23] 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. I control for the same set of fixed effects and control variables X$_{it}$ as in the performance specification of Equation 2 in section 2.3. Most importantly, the controls 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 3. 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 work 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 5 for each police officers i in year t:

$$\text{promotion}_{it} = \beta \text{patronage}_i \times \text{perform}_{it} + \eta \text{patronage}_i + \lambda \text{perform}_{it} + \mu X_{it} + \epsilon_{it} \quad (6)$$

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 5. I estimate Equations 5 and 6 as linear probability models.

[22] I estimate this equation in the same panel data set on the careers of officers hired as patrolmen in 1900-1916 as the performance regressions of section 2.3

[23] For officers that start their job early in the study period, I have less than six years of pre-periods, and I instead compute the pre-period average for all periods with available voter registry data.

4.3.2 Results on Electoral Support, Performance, and Promotions

Figure 9 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 9 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.[24]

Figure 10 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 10a).[25] 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 10b).

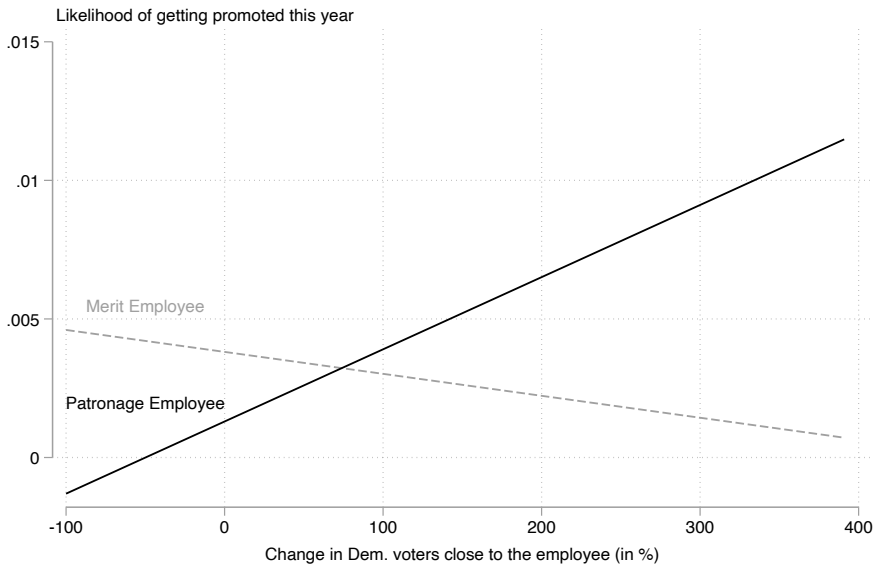
Reverse causality could be a potential concern when interpreting promotions and performance (or electoral support) in the same year. Promotions might be associated with better performance, for example, if sergeants receive fewer fines than patrolmen. Appendix Figure A1 assuages such concerns by replicating the same patterns as Figure 10 for the relationship between promotion and electoral support (Panel a) and performance (Panel b) in the *previous* year.

[24]Note that promotions to the rank of sergeant are very rare. For example, the yearly rate of promotions for merit employees with zero fines is less than 0.4%. This is partly due to the focus of this paper on the first few years of police officers' careers. But 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.

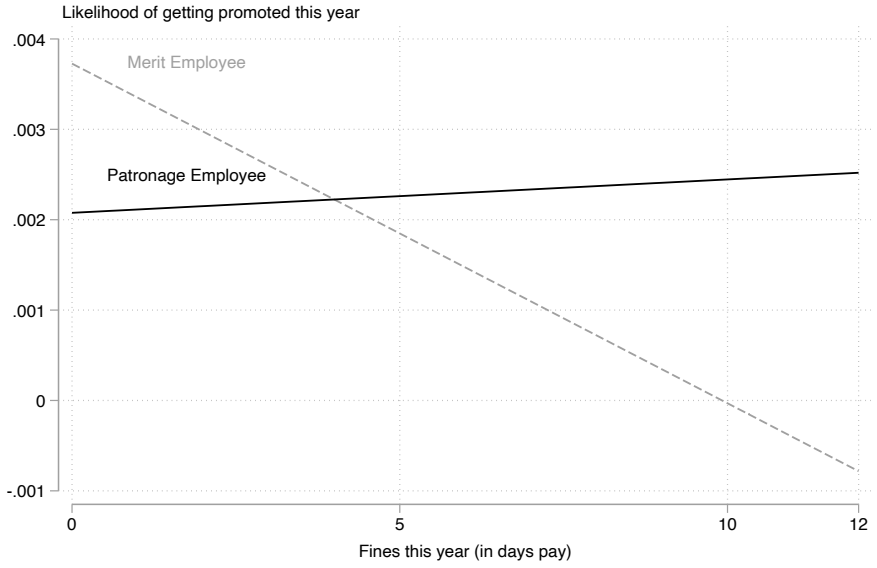
[25]Promotion rates of patronage employees are higher than for merit employees if Democratic registration increases by more than 100%, but the difference is not significant at the 5% level.

Figure 9. Determinants of Promotions for Patronage vs. Merit Employees

(a) Promotions and Democratic Registration, Predictive Margins



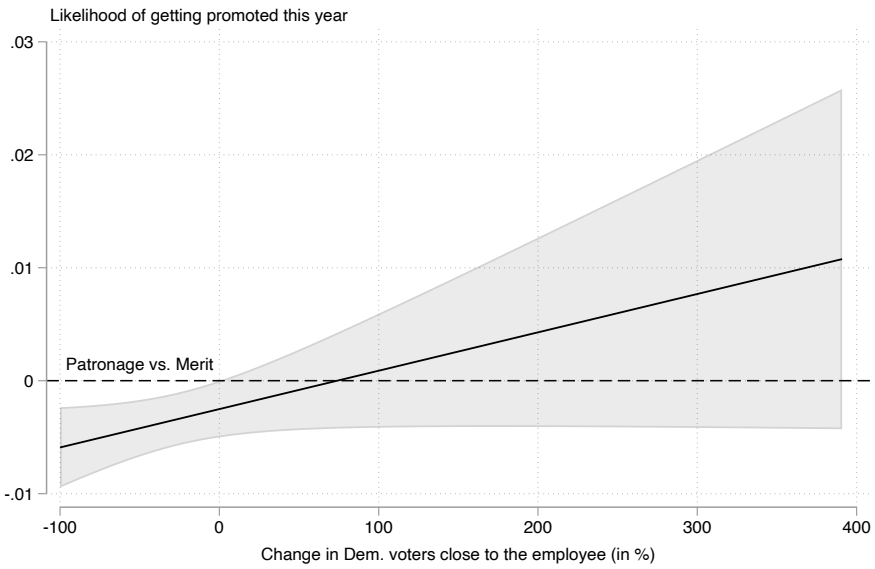
(b) Promotions and Performance, Predictive Margins



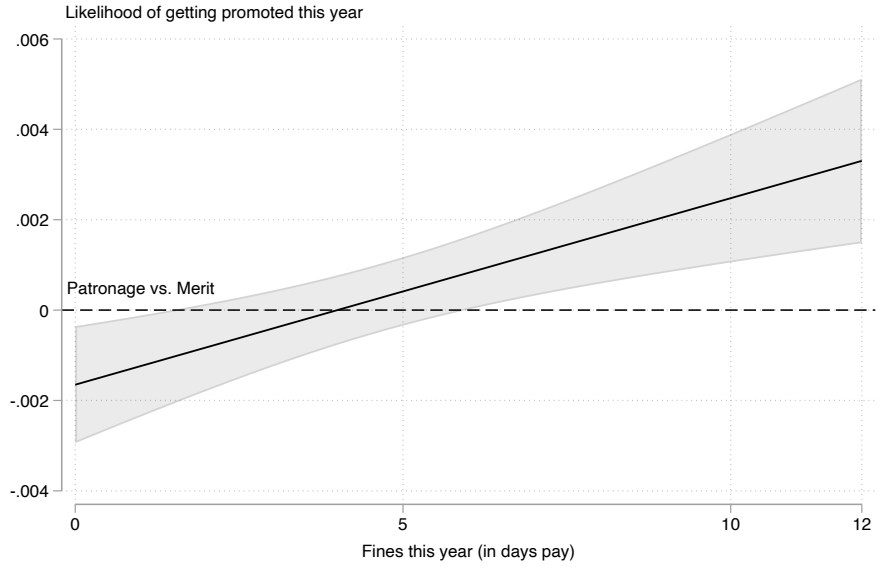
Notes: This figure presents the predicted likelihood for patronage and merit patrolmen to get promoted to sergeants, conditional on the change in electoral support in their neighborhood (Panel a) or their performance (Panel b). The predictive margins were produced from estimating Equations 5 and 6. Performance 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. See section 4.3.1 for details.

Figure 10. 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 9 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 A1 repeats the same exercise as this figure but with last year's performance and electoral support as predictors of promotions.

Caution is still warranted before causally interpreting the marginal effects of patronage status on promotions (as presented in Figure 10). Neither patronage status nor performance or electoral support are randomly assigned. The patterns documented in this section can merely be suggestive of the incentive structure under which patronage and merit employees operate. But as Appendix Table A1 documents, the performance of patronage employees is compatible with them facing weaker performance incentives (or non-performance incentives such as mobilising voters). Appendix Table A1 presents the results of estimating performance regressions as described in Equation X, except limiting the sample to police employees with test scores and including test scores as a control. I also drop the hiring period fixed effects to leverage the variation in test score cut-offs across the eligible lists. This allows me to compare patronage and merit employees with the same test score. Patronage employees perform worse than merit employees, even after controlling for test scores (Appendix Table A1, column 3). Assuming that test scores capture the selection margin through which patronage affects performance, the remaining performance costs can be interpreted as an incentive effect.

5 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 Tables and Figures

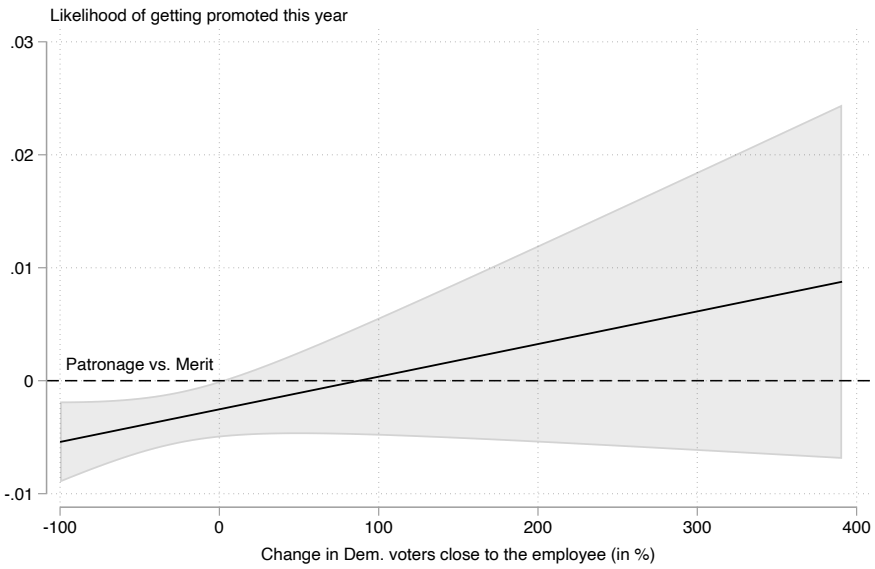
Table A1. Patronage and Performance, Controlling for Test Scores

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

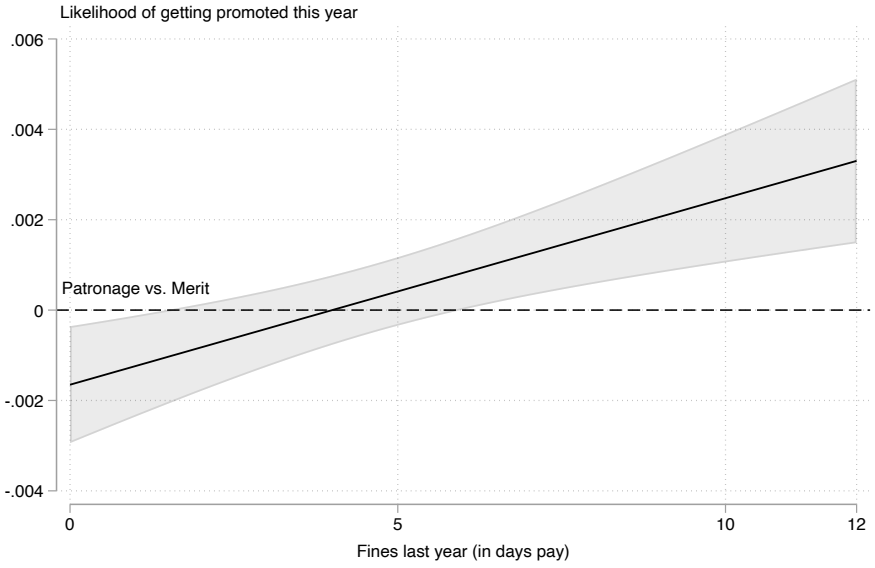
Notes: This table reports regression results from estimating the association of patronage status with performance in the sample of patrolmen with test score information following Equation 2 The outcome for all columns is yearly performance, measured as the number of days pay deducted in fines. Greater 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A1. 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 10, but with last year's electoral support and performance (instead of in the same year as the promotion). See the note to Figure 9 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.