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Author

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By

Lukas Raoul Clemens Leucht

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Committee in charge:

Professor Ernesto Dal Bó, Co-chair
Professor Guo Xu, Co-chair
Professor Barry Eichengreen
Professor Noam Yuchtman

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Abstract

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This dissertation consists of three studies on political economy and economic history. The first study provides quasi-experimental evidence of electoral rewards for politicians who deviate from meritocracy in the selection of public sector personnel. The second study explores the intergenerational consequences of introducing old-age pensions. The third study, in joint work with Davis Kedrosky and Chiara Motta, investigates the wage effects of spatial competition in a monopsonistic market for Indigenous labor. While each chapter of this dissertation answers a separate research question, all three studies are motivated by a shared interest in the political economy of historical development. Whether studying patronage politics in Progressive Era New York City (Chapter 1), the first old-age pension program of the U.S. federal government during the Second Industrial Revolution (Chapter 2), or competition between colonial companies in Canada's early modern fur trade (Chapter 3), this dissertation aims to shed light on the role of the state and quasi-state organizations in shaping the historical development of North America.

In Chapter 1, I study patronage in personnel selection under the paradigmatic political machine in the U.S. history, Tammany Hall in New York City. Electoral motives are frequently blamed for patronage in public organizations. But quantitative evidence on the behavior of patronage employees and whether they deliver an electoral return remains scarce. Focusing on the New York Police Department (NYPD) during 1900-1916 allows me to overcome the empirical challenges of estimating the electoral return to patronage: I collect new archival data to identify patronage employees, connect them to individual-level electoral responses,

and leverage a difference-in-differences design that compares patronage employees to the control group of applicants who did not receive patronage. Linking NYPD patrolmen to their civil service exam results reveals that 21% of patrolmen in the period entered into police service despite lacking the required test scores. Consistent with historical narratives, I show that these patronage employees were more likely to be connected to the leaders of Tammany Hall, the city's incumbent Democratic Party organization. Estimating a difference-in-differences design around the start of employment for patronage recipients, I find that patronage delivered a 10.3% increase in electoral support within a 50 meter radius around the employee's address (measured in the number of voters registering as Democrats). 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 performed 22.7% worse than their meritocratically selected peers.

In Chapter 2, I study the intergenerational effects of the first federal old-age pension law in the United States. In theory, when the state takes over services like old-age support, this could replace informal care by children who previously provided these services in the parental home, thereby enabling the next generation to move to better economic opportunities. In the context of an industrializing and urbanizing economy this means that state-support, by relaxing location constraints, could contribute to economic modernisation. I test this hypothesis using the 1890 Dependent and Disability Pension Act as a natural experiment. The 1890 Act transformed the Union Army Civil War pension into a federal old-age support program for Union veterans. Using restricted-access full-count census data, I track the sons of Union veterans and match them to their census records in 1870, 1880, 1900, and 1910. I provide difference-in-differences estimates of the effects of pension eligibility, comparing sons of pension-eligible fathers to sons born to ineligible men of the same generation before vs. after the 1890 Act got passed. I find that the pension reform decreased cohabitation between sons and eligible fathers by 1.6%, increased the share of sons settling in urban areas by 8.4%, and shifted affected sons out of farming and into better paying occupations.

In Chapter 3, together with Davis Kedrosky and Chiara Motta, we assemble novel data from the account books of the Hudson's Bay Company (HBC) to study the wage effects of a new entrant into a monopsonistic market for Indigenous labor. We show that in the setting of Canada's early modern fur trade, where Indigenous labor was free and mobile between firms, increased competition improved wages for Indigenous workers. This unique case study allows us to isolate market structure as the main channel of labor market power and to quantify the impact on wages. We find that a 100km decrease in distance to the nearest competitor location was associated with a 1.5% increase in wages.

This dissertation is dedicated to Yuki.
僕のライフパートナーなしではこれは不可能でした。

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Chapter 1

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

1.1 Introduction

Selecting and incentivizing talented employees is 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). Patronage in public employment could allow politicians to prioritize their partisan (or private) goals instead of selecting the best personnel.[1] Political observers have long feared that personnel decisions based on political considerations could turn state officials into "party henchmen" (Eaton, 1885). In theory, a politicised bureaucracy could decrease the quality of the public workforce (Gallego et al., 2020), and 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

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

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 investigations of the partisan benefits and performance costs of patronage remain scarce.[3]

In this paper, I analyse newly digitized personnel records and historical voter registry data to assess the partisan electoral return to patronage and the distortion patronage created in terms of personnel selection and performance. The setting for this empirical investigation is 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 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

[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., based on pure favoritism, or 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, compete in elections, and often win votes as reliably and repetitively as a machine (Scott, 1969).

[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).

the U.S. federal government are appointed at the sole discretion of the president.[6] In 2020, 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 future administrations.[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 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 available reports and link them to panel data on the careers (including promotions) 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.

The starting point of my investigation is to 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 of patronage is in line with contemporaneous reports that alleged frequent deviations from civil service rules on the behest of Tammany Hall.

[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.

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. I validate the party identification information in the voter registry as a measure of electoral support by documenting a strong correlation and highly linear relationship between registration and actual vote shares per polling place.[9] The tight connection between registration and votes is unsurprising given the frequency of registration and the high voter turnout of the time (91.9% of registered voters).

To estimate the causal effect of patronage on electoral support, 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 voter registration of patronage recipients (plus their neighbors) with the registration 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 four 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 (as measured by a likely shared country of origin based on their last names) were significantly more likely to get selected in deviations from the merit system.

[9] Given the results of these validation checks, I employ registration as a proxy for actual voting decisions and use the terms "voter registration" and "electoral support" interchangeably in this paper.

In the second and core empirical part of the paper, I provide an estimate of the causal effect of patronage jobs on electoral support. 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.

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.

Why do patronage employees and their neighbors continue their electoral support for many years after the initial appointment? In the third empirical part of the paper, I shed light on the mechanism underlying the persistent nature of the electoral return to patronage. I leverage the panel-structure of the linked personnel and voter registry data to reveal the importance of electoral support 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 meritocratically selected employees. In contrast, while merit employees get promoted if they perform better, performance does not 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.

Finally, I explore the performance implications of patronage. I document that test scores in civil service exams are strongly correlated with the eventual performance of selected officers, even when comparing patrolmen working in the same police precinct in the same year. This suggests that test scores are valuable signals of potential performance, and deviating from the merit system could come with real costs. Indeed, 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). These performance differences are mainly driven by patronage employees neglecting their official duties. Patronage employees perform even worse than can be explained by their poor exam scores. This pattern is consistent with both selection and incentives contributing to worse performance. Patronage employees are negatively selected and their promotion incentives are tied to mobilizing the votes of their neighbors (instead of rewarding their official activities). The same mechanism that helps drive electoral returns could exacerbate the performance costs of patronage.

If patrolmen get promoted to sergeants not because of their performance but as a reward for mobilising the votes of their neighbors, it is no wonder that Democratic registration increases with a patronage appointment and stays consistently at elevated levels. Career incentives can sustain the *quid pro quo* relationship between patron and client. This electoral logic of patronage in exchange for mobilising votes 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."

1.1.1 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; Dal Bó et al., 2013; Deserranno, 2019) and the selected (Brollo et al., 2017; Colonnelli et al., 2020; Mocanu, 2023; Moreira

[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).

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. 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 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 shed light on the mechanism through which a politicized bureaucracy can affect electoral competition. 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 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 benefit of their supporters or their in-group.[12] This includes work on distributing public jobs as patronage to connected groups

[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)

[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")

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), which models electoral motivations for 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 can have potentially long-lasting costs to overall welfare.

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 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,

[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.

[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.

and I leverage variation from the remaining patronage appointments across neighborhoods within the same city and under the same institutional regime. This paper's quantitative case study of the NYPD under the influence of Tammany Hall 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, at the cost of providing sub-optimal public services.

1.2 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,

[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.

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.

1.3 Data and Descriptive Statistics

This section introduces the data that forms the basis of the empirical analysis in this paper. I also describe how I collected the data and linked the records from multiple sources to construct the main variables of interest. Then I explain how I identify patrolmen who received their jobs through patronage based upon information on applications, entrance exam results, and lists of police employees. Lastly, I provide descriptive statistics on the distribution of patronage jobs.

1.3.1 Data Sources

The main source of data 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 geo-locating 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.

[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.

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 its 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 Hall 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 subsections give more detail on this and other measurement decisions.

1.3.2 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*

[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 the gap between lists is never longer than one year. I start in 1902, because the 1901 issues could not be found.

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).

My analysis focuses on complete *Eligible Lists* for eight 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.

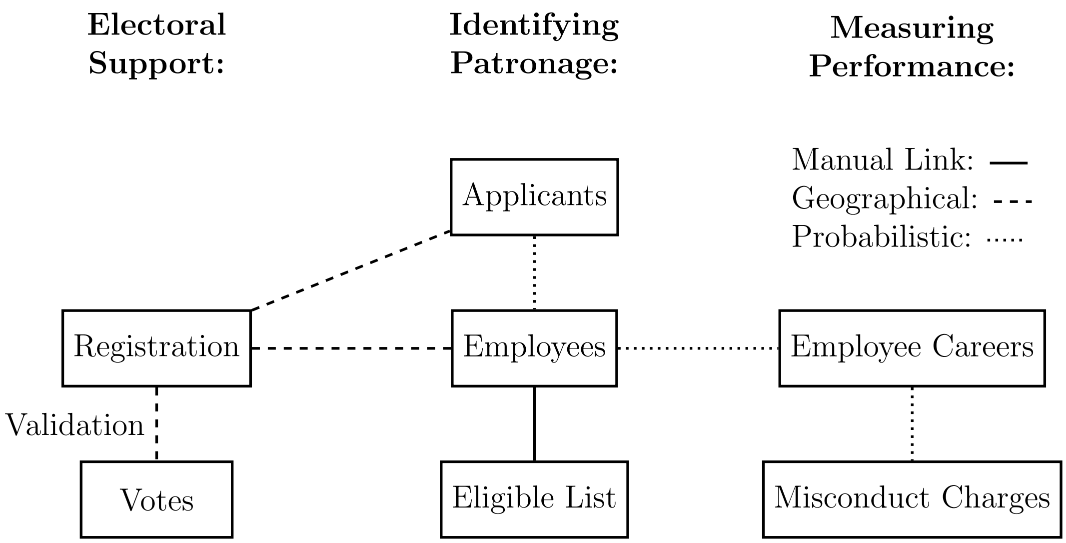
Linking of Personnel Records: Figure 1.1 illustrates how I link the various data sets derived from reports in the *City Record* to measure the concepts at the core of this paper: The patronage status of patrolmen, changes in electoral support, and performance. To identify patronage status (Figure 1.1, 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 test scores and entrance date for employees) facilitates the matching. We can therefore conclude with some certainty that employees not matched to an *Eligible List* did not receive a score of 70% or higher on the civil service exam. See Section 1.3.3 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.

[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.

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 hiring periods, I match on full names and residential addresses. Section 1.4 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]

Figure 1.1: Illustration of Record Linkage and Measurement



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

[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.

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 1.5.3 and 1.6 employ the panel data to analyse the 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 to 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 1.3.1) 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 1.1, 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

[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

and patronage in Sections 1.4 and 1.5, 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 Democrats in close proximity to patronage employees (compared to unsuccessful applicants).[24] I focus on Democratic registration because during 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 1.5.1 validates voter registration as a measure of electoral support by demonstrating a strong linear relationship between the share of 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.

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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.

[24] See Section 1.5 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).

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 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.

1.3.3 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 code jobs given to individuals who did not have the required exam results as patronage appointments. 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.

Figure 1.2 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

[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.

identify 21% of them as patronage employees. I refer to the remaining patrolmen as "merit employees".

Figure 1.2: 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 1.2) 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 1.2 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]

1.3.4 Patterns in the Distribution of Patronage Jobs

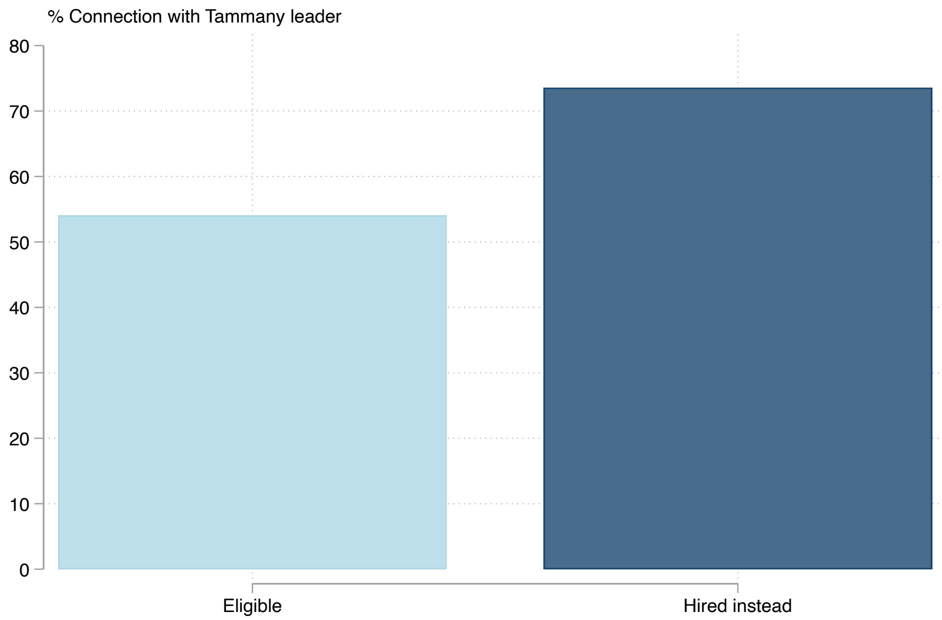
Patronage jobs are unlikely to be randomly distributed. This section explores the main determinant of patronage highlighted by the historical literature: Connections between office-seekers and their local Tammany Hall district leaders.

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 1.3 compares the share of connected applicants within two groups: Those eligible for patrolman jobs but passed over (left bar), and those 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. Of course, a correlation in the characteristics of patronage employees and political leaders could also be due to other factors, such as shared social networks, higher levels of trust for members of the in-group, or even taste-based ethnic favoritism. Substantiating an electoral logic of patronage requires further evidence on the effects of patronage appointments. The next section investigates whether distributing patronage jobs resulted in electoral returns.

[28] See Table 1.1, columns 4 and 5 for a comparison of the electoral return to close and far deviations from merit.

Figure 1.3: Connections Between Applicants and Local Tammany Hall Leaders



Notes: This figure presents 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.

1.4 Estimating the Electoral Return to Patronage

1.4.1 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 applicants, who were unsuccessful and ineligible, and 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 1.2), but Donovan 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 Donovan and his neighbors after September 1905 to changes in the behavior of Brown 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. For example, for Donovan (or Brown) 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 Donovan and Brown applied during the same period and Donovan 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

[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 1.4.3 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).

[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.

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:

$$\mathrm{y}_{j(i)t} = \beta \mathrm{patronage}_i \times \mathrm{post}_t + \eta_i + \lambda_t + \mu \mathrm{X}_{it} + \epsilon_{it} \tag{1.1}$$

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 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

[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.

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 1.1:

$$\mathrm{y}_{j(i)t} = \sum_{k \neq -1} \beta_k \text{patronage}_i \times \lambda_k + \eta_i + \lambda_t + \mu \mathrm{X}_{it} + \epsilon_{it} \tag{1.2}$$

where I sum over the interaction between the 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 1.1. 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 violations. 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 of electoral support.

1.4.2 The Effect of Patronage on Electoral Support

Table 1.1 presents the main results from Equation 1.1 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 over a control mean of 29; an electoral return of 6.8-10.4%.

Column 1 of Table 1.1 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. 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 1.1, 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.

Table 1.1: Patronage Jobs and Democratic Registration

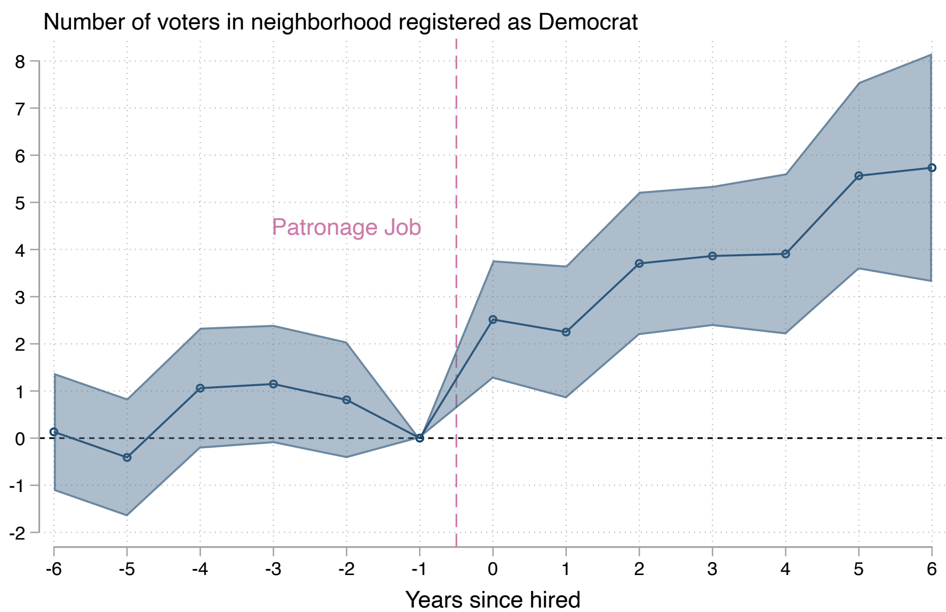
	(1) All	(2) All	(3) All	(4) Close	(5) Far
Patronage Appointment	2.02***	1.95***	2.95***	2.99***	2.93***
	(0.57)	(0.58)	(0.57)	(0.68)	(0.86)
Outcome Mean	28.67	28.67	28.67	28.67	28.67
R-squared	0.84	0.84	0.85	0.84	0.85
Observations	72492	72492	72492	70296	67320
Patronage Employees	614	614	614	431	183
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 1.1). 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 equal to 1 starting in the year that the applicant receives their patronage job, and 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

[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 A.1 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.

A causal interpretation of the estimates in Table 1.1 requires that electoral support in neighborhoods with and without patronage recipients would have followed common trends in the absence of any patronage. Figure 1.4 provides event-study evidence from an estimation of Equation 1.2 with the fixed effects structure of the specification in Table 1.1, 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 1.4 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 1.4: 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 1.2). The outcome is the number of registered Democrats within a 50 meter neighborhood. See the notes of Table 1.1, column 3, for details on the specification.

1.4.3 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 1.3.3, 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 1.1 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.

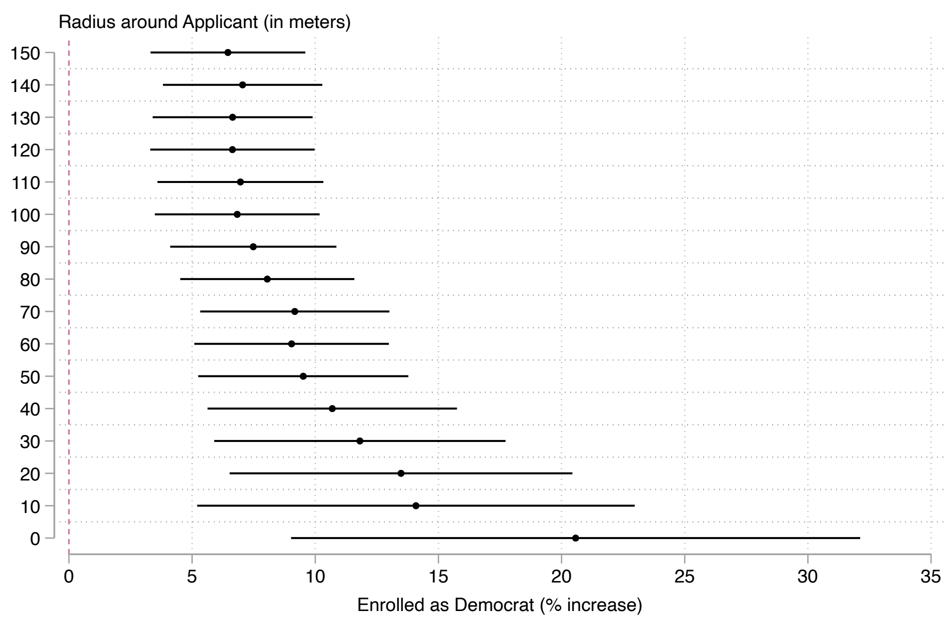
The baseline estimate in Table 1.1 reports the effect of patronage on Democratic registration in a 50 meter radius around applicants. This choice of radius is somewhat arbitrary. Figure 1.5 demonstrates that the estimated electoral return is robust to different choices for the neighborhoods around patronage recipients and control applicants. The figure plots the coefficients from a difference-in-differences specification as in Table 1.1, 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 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 A.1, for example, replicates the event study of Figure 1.4 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

[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 1.7 and Table 1.1, column 3, are slightly different. The sample for Table 1.1 only avoided overlap in a radius of 50 meters when trimming the sample.

research design of estimating Equation 1.1 and 1.2 via OLS. Appendix Figure A.2 compares event study estimates from the baseline model as in 1.4 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.

Figure 1.5: 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 1.1, column 3, for details on the underlying regression.

1.5 Drivers of the Electoral Return to Patronage

The results in section 1.4 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.

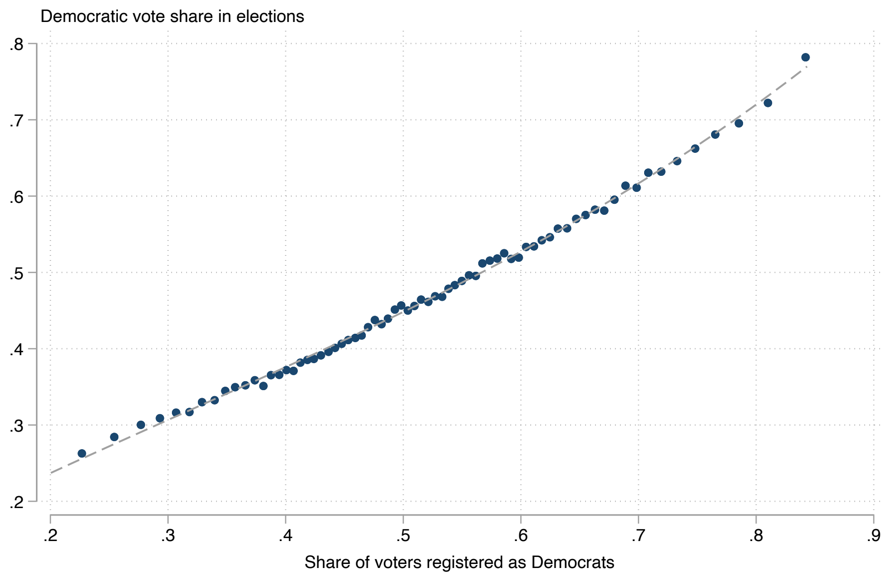
1.5.1 Voter Mobilisation

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 A.2 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 1.4 and Table 1.1, 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 1.6 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 Appendix Table A.3 for regression results and details on the underlying specification). High voter turnout in this setting, further alleviates concerns over relying on registration data. The median turnout among registered voters across polling places was 91.9%.

[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.

Figure 1.6: Binned Scatter Plot of Election Results and Voter Registration

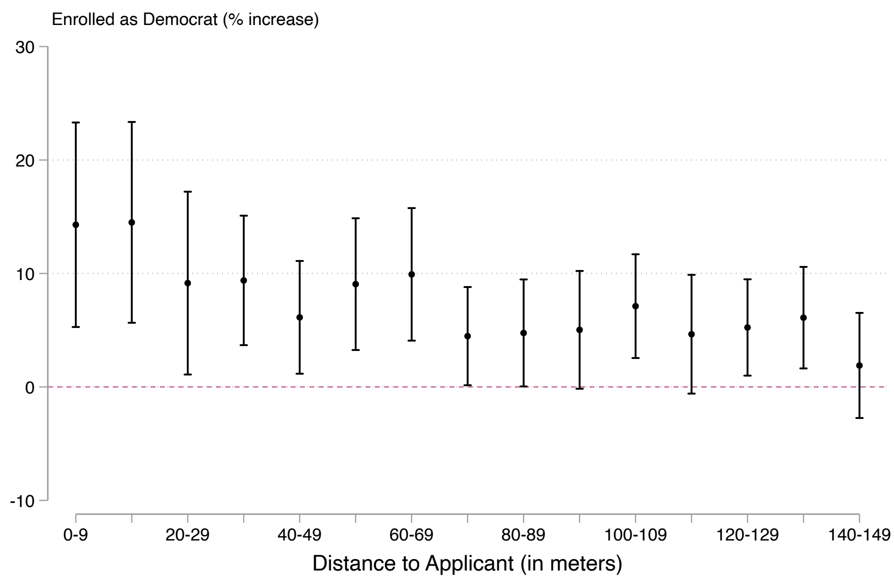


Notes: This figure presents the relationship between the Democratic vote share in elections and the share of Democrats among registered voters by polling place, year, and election as a binned scatter plot. The line of best fit was estimated flexibly up to third order polynomials. The underlying regression 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 A.3, 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 1.7 plots coefficients of treatment effects from difference-in-difference specifications as outlined in Equation 1.1 and estimated in Table 1.1, 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 1.7 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 fades out with distance until at a distance of 140-149 meters, where close to zero additional voters register as Democrats.[35]

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

Figure 1.7: 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 1.1, 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 NYPD policy to not allocate patrolmen to beats that included their home, but this is exactly where patronage generated the greatest electoral return.[37]

[36]The evidence of heterogeneity by borough, presented in Appendix Table A.1 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 their home, but when allocating them within the precinct they should not patrol beats that include their home address.

1.5.2 Electoral Response to Patronage

After showing in the preceding section that patronage mobilised the votes of the closest neighbors of recipients, this section 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 1.4 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?

1.5.3 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 prospects in the force. 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.

1.5.3.1 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:

$$\text{promotion}_{it} = \beta\text{patronage}_i \times \Delta\text{votes}_{it} + \eta\text{patronage}_i + \lambda\Delta\text{votes}_{it} + \mu\text{X}_{it} + \epsilon_{it} \tag{1.3}$$

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 officers 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 1.4. 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 1.3 for each police officers i in year t:

$$\text{promotion}_{it} = \beta\text{patronage}_i \times \text{perform}_{it} + \eta\text{patronage}_i + \lambda\text{perform}_{it} + \mu\text{X}_{it} + \epsilon_{it} \tag{1.4}$$

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 1.3.

1.5.3.2 Results on Electoral Support, Performance, and Promotions

Figure 1.8 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

[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.

force on their own merit. These results are compatible with promotion incentives driving the electoral return to patronage. Panel (b) of Figure 1.8 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 1.9 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 1.9a).[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 1.9b).

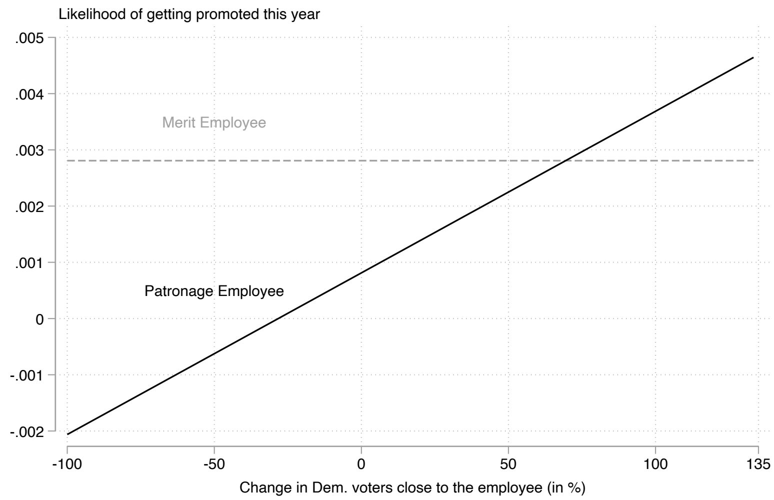
Reverse causality could be a potential concern when interpreting promotions and performance (or electoral support) in the same year. Promotions might be associated with better performance, for example, if sergeants receive fewer fines than patrolmen. Appendix Figure A.3 assuages such concerns by replicating the same patterns as Figure 1.9 for the relationship between promotion and electoral support (Panel a) and performance (Panel b) in the *previous* year.

[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.

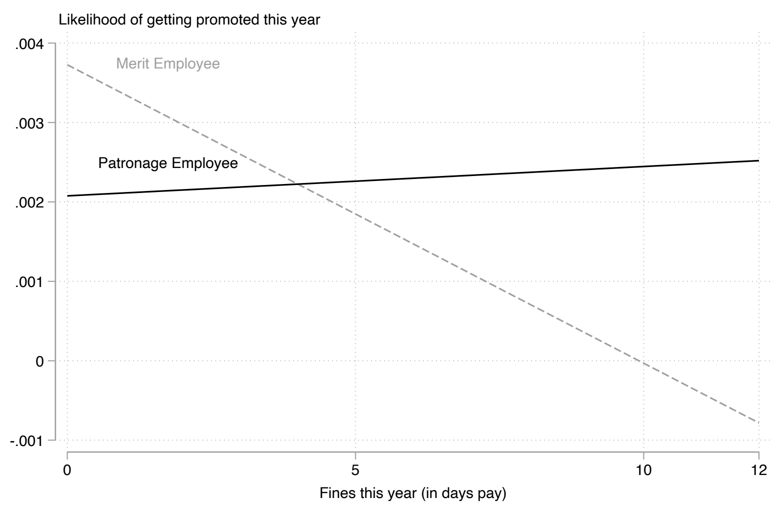
[40]Promotion rates of patronage employees are higher than for merit employees if Democratic registration increases by more than 75%, but the difference is not significant at the 5% level.

Figure 1.8: Determinants of Promotions for Patronage vs. Merit Employees

(a) Promotions and Democratic Registration, Predictive Margins



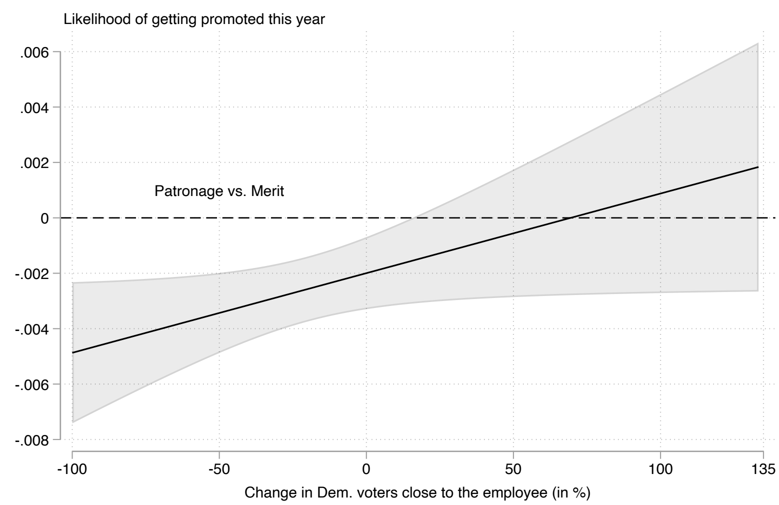
(b) Promotions and Performance, Predictive Margins



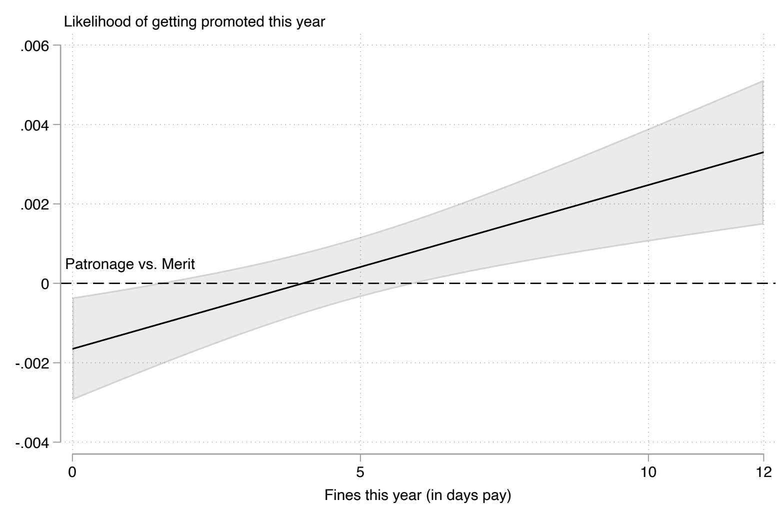
Notes: This figure presents the predicted likelihood for patronage and merit patrolmen to get promoted to sergeants, conditional on the change in electoral support in their neighborhood (Panel a) or their performance (Panel b). Predictive margins are estimated from Equations 1.3 and 1.4. Performance 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 1.5.3.1 for details.

Figure 1.9: 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 1.8 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 A.3 repeats the same exercise as this figure but with last year's performance and electoral support as predictors of promotions.

1.6 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 absence of performance information for applicants who never receive the job. Instead, I perform two closely related empirical exercises: First, I investigate whether test scores in civil service entrance exams predict the performance of appointed patrolmen. Second, 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.

1.6.1 Civil Service Exam Results and Performance

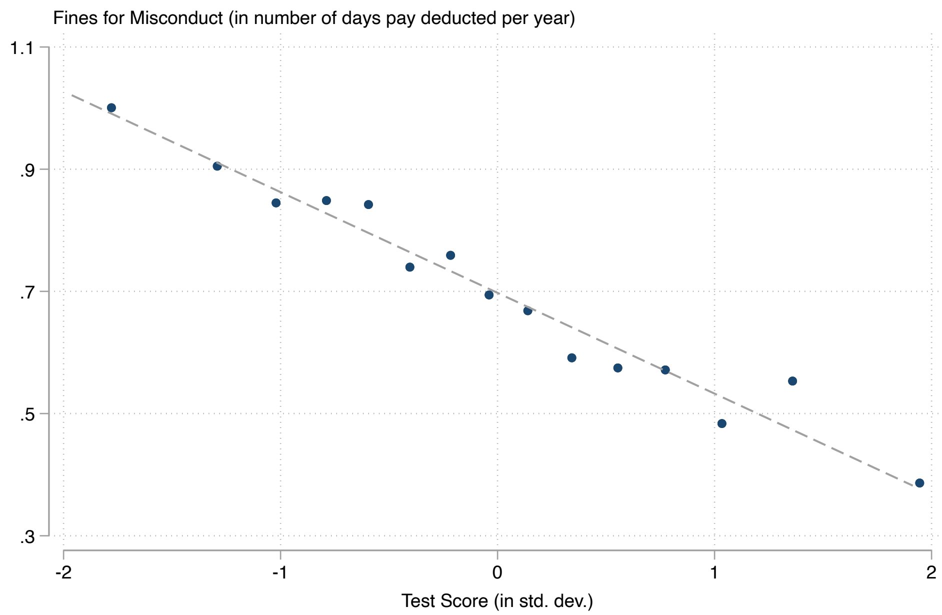
To investigate if exam results predict performance, I estimate the following equation for each police officer i in year t:

$$\text{perform}_{it} = \beta \text{scores}_i + \mu \text{X}_{it} + \epsilon_{it} \tag{1.5}$$

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 1.10 reports the relationship between performance and test scores from Equation 1.5 as a binned scatter plot. There is a strong linear relationship between test scores and actual performance. Appendix Table A.4 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 A.4. 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 1.10: 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 A.4, column 5, for regression output from the underlying specification of this figure. The relationship between test scores and performance is estimated following Equation 1.5 and includes fixed effects for police precinct and year interactions, as well as fixed effects for the period in which patrolmen were hired.

1.6.2 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:

$$\text{perform}_{it} = \beta \text{patronage}_i + \mu \text{X}_{it} + \epsilon_{it} \tag{1.6}$$

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 1.5.

Table 1.2 reports regression results on the relationship between patronage and performance from estimating Equation 1.6. 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 1.2, 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 A.4, col. 5).

Table 1.2: 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 1.6. 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 A.4 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).

1.6.3 Selection and Incentives

Given the positive relationship between entrance exam scores and performance (see Figure 1.10) 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 1.5.3 suggests that patronage and merit employees are on different career tracks and that patronage employees face weaker performance incentives.

Table 1.3: 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 1.6. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 1.3 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 1.2 and A.4 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.

1.7 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

[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 1.3, column 1, is stronger than in Table 1.2, column 4. Table 1.3 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.

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 further 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 does not compare personnel practices across regimes, and instead leverages variation in patronage appointments within a regime of constant 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. The 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.

Chapter 2

Replacing the Ties that Bind: Modernizing Effects of The First Federal U.S. Pension Program

2.1 Introduction

During the late 19th and early 20th century, the economies of Western Europe and North America modernized and their welfare states expanded. To what extent did the expansion of the welfare state contribute to economic modernization? In theory, when the state takes over services like child care or old-age support, this could free up family members that would have had to provide these services. Children that expect to care for their parents in old age are unlikely to leave their hometown, even if this means forgoing better occupational opportunities in other regions, such as expanding metropolitan areas. If the state provides old-age support, can this spur urbanization and industrialization by enabling the next generation to move to new regions and new economic sectors?

To empirically assess the predicted link between state provision of old-age support and the migration and occupational choices of future generations, I need variation in pension eligibility that newly covers some families but not other, comparable families. I also need to be able to link individuals across generations and to observe their long-term life outcomes. I study the effects of the 1890 Dependent and Disability Pension Act (or just the "1890 Act" for short). The 1890 Act, for the first time, provided pensions for all veterans of the Union Army, regardless whether they were injured during their Civil War service or not. Importantly, Confederate veterans were not offered pensions. Access to Full Count census data for 1870-1910 and modern record linkage techniques enable me to observe how the sons of eligible and ineligible fathers reacted. I provide difference-in-differences estimates for the effect of fathers' pension eligibility on geographic and occupational mobility.

I find that pension eligibility decreases the likelihood that sons of eligible men stay in their parental home by 1.6% and increases the share of them that move to urban areas by 8.4%. Introducing old age pensions also shifted sons of eligible men out of farming and into higher paid occupations, thereby increasing their average occupational income by 3%. The research design ensures that these effects are not driven by other time-invariant differences between families of eligible and ineligible men. Neither are the effects driven by time-trends or regional shocks, since I keep the comparison between individuals who grew up in the same county and I flexibly control for county-specific time trends. In robustness checks, I address potential concerns that some long-run consequences of military service could confound the effect of pensions for Union veterans. Using a triple difference design, comparing the sons of eligible Union veterans to those of ineligible Confederate veterans growing up in the same state, I show that the effects survive if I separate pension eligibility from veteran status. Taken together, these findings suggest that state-provided old-age support can substitute for informal care, thereby empowering the next generation to participate in economic modernisation and improve their economic standing.

This study contributes to various economic literatures. There is a growing literature on US structural change (Eckert and Peters, 2022), and the country's 19th to 20th century experience of urbanization and industrialization (see Herrendorf et al. (2013) for a recent literature review). To my knowledge, there is no prior study providing causal estimates of the welfare state's contribution to these modernization processes.

Economists have studied how state-provided insurance schemes can crowd out informal insurance provided by the family or other community members (Di Tella and MacCulloch, 2002; Albarran and Attanasio, 2003; Shan and Park, 2023). Old age care by family members is often provided locally and in-kind. Existing work (Chen, 2017; Bau, 2021) highlights that pension reform can therefore reduce inter-generational cohabitation and Fetter et al. (2024) finds that lifting this "location constraint" for the children of pension recipients improves their long-run economic standing. The unique contribution of my paper is to show empirically that introducing old age pensions enabled sons of recipients to move to urban areas and out of farming, thereby contributing to economic modernization.[1]

Lastly, I contribute to the economic history literature on Union Army veterans and their pensions. Economic historians have long studied records of Union Army veterans to learn about the evolution of retirement in the United States (Costa, 1998a,b). Most of the existing

[1]This finding resonates with counterfactual exercises by Munshi and Rosenzweig (2016), which suggest that expanding national insurance programs in India could reduce misallocation by crowding out local informal insurance and increasing rural-urban migration.

work evaluating the Union Army pension focuses on the health and labor market effects for the direct recipients (Costa, 1995; Eli, 2015), or on the consequences for widows of veterans (Salisbury, 2017). The closest paper in this literature is Costa (1997), who presents evidence that by 1910 Union Army veterans where less likely to reside with their children than other elderly men who did not serve and therefore did not receive pension benefits. Costa (1997) interprets this finding as the result of an income effect of pensions that raises the demand of the elderly for separate living arrangements. In contrast to the focus of Costa (1997) on elderly veterans, this paper focuses on the residential and occupational decisions of their sons. Newly linked census data allows me to track sons of veterans before and after the 1890 Act, and I show that they move to urban areas, out of farming, and to better paying jobs.

2.2 Historical Background

Before the 1890 Act, pensions were only granted to Union Army veterans that were disabled during the Civil War (Glasson, 1900). There were no comparable old-age pension or disability payments to those that did not fight in the war of rebellion, or those that fought on the Confederate side. In 1885, only 244,201 individuals or 16.85% of all surviving Union veterans were enrolled as pensioners due to their war time disabilities (Skocpol, 1993). Before the 1890 Act, the pension roll was seen by some commentators as an honor roll naming those veterans that were wounded during their service for the nation (Glasson, 1900). The 1890 Act broke with this principle and massively expanded the number of eligible veterans. By 1891, the number of pensioners more than doubled to 39.34% of all surviving Union veterans (Skocpol, 1993). To receive the pension benefits, veterans did not have to experience any active combat. Pensions were paid to everyone who served at least three months as a member of the Union forces, was honorably discharged, and unable to perform manual labor and therefore dependent on others (Glasson, 1900). Pensioners were paid 6 to 12 dollars per month (about 170 to 340 in today's dollars), depending on the severity of their disability to perform manual labor (Glasson, 1900).

In 1904, Theodore Roosevelt issued an Executive Order stipulating that all Union veterans aged 62 or older would be eligible for pensions. The Executive order officially classified old age as disability to perform manual labor. But the transformation of the Civil War disability pension into old-age support already started before 1900. With old age in effect satisfying the disability condition, the pension rolls again almost doubled to 74.13% of all 1 million union veterans in 1900 (Skocpol, 1993). At the time of its passage, the 1890 act was the most expensive appropriation in US history. By 1894, over a third of the federal budget was spent on pension payments (Glasson, 1988).

The US started to urbanize and industrialize during the late 19th century. Urbanisation rates increased from 26% in 1870 to 40% in 1900 (Census Bureau). But in contrast to the experiences of other countries, it was not the urban working class that first successfully pushed for old-age and disability support. Skocpol (1993) documents how political pressure from Civil War veterans as an organized voting block led to the 1890 Act. Both parties wanted to be seen as catering to the former soldiers and win their vote. The 1890 transformation of a military pension into old-age support for a select group therefore serves as a fitting case study to explore the effect of welfare state expansion on economic modernization.

2.3 Data Construction

Full Count Census data for 1870 to 1910 allows me to track veterans and their sons throughout the period after the Civil War period and to study their reactions to the 1890 Act.[2] The 1910 census is the first census which included questions on the veteran status of respondents. I identify about 210,000 individuals as Union veterans and 90,000 as Confederate veterans in 1910. I also identify other men that are would have been between 15 and 45 years old in 1861 (or 64-94 years in 1910) to construct a sample of similarly aged non-veterans that can serve as a control group.

To assemble panel data on the decisions of their dependents, I first match the veterans and other men of their age cohort in 1910 to their younger selves in the 1870 census. I apply modern probabilistic record linkage techniques as outlined in Abramitzky et al. (2021). To reduce the risk of false positives, I require records to match exactly on birth place (i.e. US state or foreign country) and the initials of first and last name. I only consider records within a 5-year age window as potential matches, and then match on Jaro-Winkler string distance of first and last names. To be conservative, I only keep records that match at least 90% on string distance and have sufficiently unique names (i.e. there are no other potential matches with the same age that match to at least 90% on names).

Next, I identify sons of the matched sample in 1870. I focus on sons because I need to match on names to observe them in later census waves, and women are more likely to change their last names when they marry. I focus on sons that are 15 years or younger in 1870. To keep the control group comparable to the sons of Union veterans, I restrict the sample to families that in 1870 lived in the Northern United States. I then match the sons to their older selves in the 1880, 1900, and 1910 censuses.[3] Observing them in 1870 and 1880 allows me to observe pre-trends in the outcomes for sons of Union veterans and sons of control families

[2] I accesses the IPUMS Full Count Data through an agreement of the Minnesota Population Center with the UC Berkeley Department of Demography.

[3] The 1890 census records unfortunately were lost in a fire and are therefore not available for analysis.

before the treatment of the 1890 Act. Matching to 1900 and 1910 reveals information on post-treatment outcomes. I restrict the sample to sons who can be matched to at least one of the periods after the 1890 Act. Since I see all of them in 1870, this sample restriction ensures that each son is observed at least once before and after the pension reform.

The resulting sample is an unbalanced panel of 226,197 sons. Of those, 143,459 are also observed in 1880, and 166,016 and 162,484 are linked to 1900 and 1910 respectively. This implies a very high match rate between census waves. This is in parts due to conditioning the sample on having at least one post-1890 match. The unconditional match-rate is in line with the usual range reported in the literature on census linking.

To study economic modernization at the individual level, I construct outcome variables that aim to capture participation in processes of urbanization and industrialization. For urbanization, I observe in the census data if sons of veterans and similarly aged men live in an urban or a rural county (as classified by IPUMS). I also track whether individuals work in farming (owners, tenants, or farm managers) or one of four other occupational groups: low-skilled (service workers or laborers), semi-skilled (sales or operatives), high-skilled (professional, technical, manager, craftsmen, officials, and proprietors), or no formal occupation. This classification follows Dupraz and Ferrara (2023).

In addition to these outcomes, I also measure whether sons stay in their parental home. Cohabitation is measured based on census variables indicating the familial relationships between members of each household. Cohabitation is an important dimension of individual residential choices, but it also proxies for potential in-person services and informal labor provided by sons for their parents. Lastly, I use occupational income scores as a proxy for the economic implications of the 1890 Act for the sons of eligible veterans. Occupational income is based on the median income (in 1950 dollars) earned by persons with that occupation in 1950. These scores are commonly used as measures of incomes since the census did not ask about personal income before 1940.

2.4 Results

2.4.1 Empirical Design

I estimate the following equation to recover the difference-in-differences estimate for the effect of the 1890 Act on the sons of eligible Civil War veterans:

$$y_{it} = \beta Vet_i \times post_t + \eta Vet_i + \lambda post_t + \mu \mathbf{X_{it}} + \epsilon_{it} \tag{2.1}$$

The variable y_{it} captures the main outcomes of interest: whether son i resides in an urban county, and whether i cohabitates with their father at time t. In supplementary results (cf. Section 2.4.4), outcomes y_{it} are indicators for the type of occupation (including farming) that i is working in at time t and the income they receive from these occupations. Vet_i is a dummy variable that takes value equal to 1 if the father of i is a Union veteran. The dummy $post_t$ is equal to 1 in decades t after the 1890 Act. I include a vector of control variables X_{it}. In particular, I present specifications, where I additionally include individual fixed effects, decade fixed effects, and county-decade time trends. Standard errors ϵ_{it} are clustered at the level of the 1870 county throughout.

The main coefficient of interest is β. The main assumption that needs to be satisfied for β to identify the effect of the 1890 Act on cohabitation choices and participation in urbanization, is that sons with eligible and ineligible fathers would have had parallel trends on these outcomes in the absence of the 1890 Act. The parallel trends assumption is not directly testable. Instead we can test whether sons of eligible and illegible fathers had parallel trends in the period before treatment.

Estimating the following equation allows us to compare trends for sons with eligible and ineligible fathers, and to observe the dynamic effects of the 1890 Act:

$$y_{it} = \sum_{s=1870}^{1910} \beta_s \lambda_s \times Vet_i + \theta_{c(i)t} + \eta_i + \epsilon_{it} \tag{2.2}$$

Now the coefficients of interest, β_s, on the interaction of the treatment group indicator Vet_i and time fixed effects λ_s are allowed to vary by decade s. With 1880 as the baseline period, the coefficients β_s return the difference in outcomes between sons of eligible and ineligible fathers in comparison to the 1880 pre-period. Including individual fixed effects η_i and county-decade time trends $\theta_{c(i)t}$ (based on the county c of son i in 1870) eliminates time-invariant confounders and limits the comparison to sons of ineligible fathers who resided in the same county.

Note that the estimates for the coefficients of interest, β and β_s, should be interpreted as Intention-to-treat (ITT) estimates of becoming eligible for monetary old-age support in 1890. I currently do not have access to pension claims and disability status of Civil War veterans and therefore do not observe which veterans actually received the pension that they became eligible for.[4] By interpreting the estimate as ITT, I assume that all Union veterans - irregardless of disability status in 1890 - can expect to claim benefits eventually.

[4]Note that I also do not observe which veterans already were eligible for some support before 1890, e.g. because of Civil War injuries. But since veterans were allowed to claim pension benefits under multiple laws, any unobserved prior claims would, if anything, lead me to underestimate the effect of the 1890 Act (Glasson, 1900).

2.4.2 Discussion of Results

Table 2.1 presents the results from Equation 2.1 on the difference-in-differences effect of the 1890 Act on the residential choices of sons of Union veterans. The dependent variable for Panel A of Table 2.1 is a dummy for whether sons of Union veterans cohabitate with their father, and the dependent variable for Panel B is a dummy for whether the sons reside in an urban area. The columns vary the specification to test the robustness of the results. To summarize, after the passage of the 1890 Act, sons of Union veterans became 1.3-1.5 percentage points (p.p.) less likely to live with their father and 1.6-2.3 p.p. more likely to reside in urban areas than sons of ineligible fathers. These effect sizes imply that the 1890 Act decreased cohabitation by an extra 1.6-1.9% and increased urbanisation by 8.4-12.0% more than the time trend over the same period (a 80.3 p.p. drop in cohabitation and 19.1 p.p. increase in urban residence for sons of ineligible fathers).

Column 1 of Table 2.1 reports results from the baseline specification, where I only include indicator variables for the post-1890 time trend and the Union veteran status of fathers as control variables. Cohabitation of Union veteran sons falls an extra 1.5 p.p. and their participation in urbanization increases an additional 2.3 p.p. after passage of the 1890 Act. Columns 2 and 3 subsequently add decade and individual-specific fixed effects to allow for flexible time trends and to hold fixed any time-invariant characteristics of sons that might affect their residential decisions. Lastly, Column 4 adds a full set of county fixed effects (based on the earliest available pre-1890 county of sons) and their interaction with decadal time trends to allow for county-specific shocks and to keep the comparison strictly between sons of Union veterans and sons of ineligible fathers growing up in the same county. The estimated effect of the 1890 Act remains robust and of comparable magnitude across all four specifications. According to the preferred specification, in Column 4, the 1890 Act decreased cohabitation by 1.3 p.p. and increased urbanization by 1.6 p.p. among sons of eligible Veterans.

The vast majority (80.3%) of sons in the control group also moved out of their parental home after 1890, but relatively few (19.1%) chose to settle in urban areas. In context of these trends, the modest cohabitation effect (1.6% less than the baseline trend) is accompanied with a sizeable increase in urbanization of 8.4%.

Table 2.1: Difference-in-Differences Estimates for the Effect of the 1890 Act

Panel A: Cohabitation

	(1) Baseline	(2) +Decade FE	(3) +Son FE	(4) +County-decade FE
Union vet x post	-0.015***	-0.015***	-0.014***	-0.013***
	(0.003)	(0.003)	(0.003)	(0.003)
Post 1890	-0.803***			
	(0.002)			
Union veteran	0.002**	0.003**		
	(0.001)	(0.001)		
Observations	698156	698156	698156	698156
R^2	0.658	0.668	0.791	0.791

Panel B: Urbanization

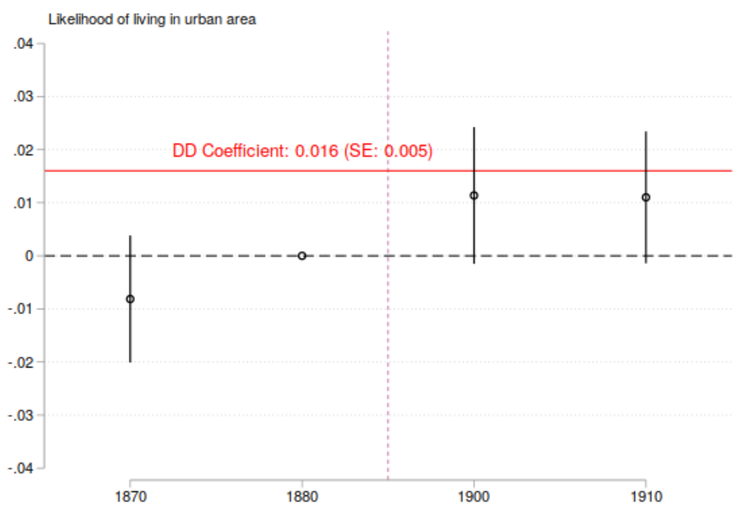
	(1) Baseline	(2) +Decade FE	(3) +Son FE	(4) +County-decade FE
Union vet x post	0.023***	0.023***	0.021***	0.016***
	(0.004)	(0.004)	(0.005)	(0.005)
Post 1890	0.191***			
	(0.010)			
Union veteran	-0.028***	-0.028***		
	(0.007)	(0.007)		
Observations	698156	698156	698156	698156
R^2	0.043	0.044	0.594	0.605

Notes: This table reports difference-in-differences estimates for the effect of the 1890 Act on the decisions of sons of Union veterans to cohabitate with their father (Panel A) or to reside in urban areas (Panel B). The variable *Union vet × post* in the first row of each Panel reports the average treatment effect, β, as estimated following Equation 2.1. The variables *Post 1890* and *Union veteran* correspond to dummy variables for the post-1890 time trend and to indicate whether individuals are sons of Union veterans (cf. $post_t$ and Vet_i in Equation 2.1). Column 1 presents the baseline estimates, where these dummy variables are the only controls. Columns 2-4 subsequently add decade fixed effects (col. 2), individual fixed effects for each son (col. 3), and county-decade specific time trends (col. 4). Observations are at the son-decade level. See Section 2.4.1 for details on the research design and Section 2.4.2 for a discussion of the results. Standard errors in parentheses are clustered at the level of the 1870 household.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 2.1: Event Study on the Effect of the 1890 Act

(a) Cohabitation

(b) Urbanization



Notes: This figure presents the dynamic treatment effect of the 1890 Act on the sons of Union veterans with 95% confidence intervals (i.e. the event-study coefficients β_s of Equation 2.2). The outcome for Panel (a) is whether sons of Union veterans cohabitate with their father, and for Panel (b) the outcome is whether they reside in urban areas. The figure also plots a horizontal red line at the average treatment effect of the corresponding difference-in-differences estimates. See the notes to Table 2.1, Panel B, Column 4, for details on the specification.

A causal interpretation of the estimates presented in Table 2.1 requires that residential decisions of sons with and without Union veteran fathers recipients would have followed common trends without passage of the 1890 Act. Figure 2.1 provides event-study evidence from an estimation of Equation 2.2. Panel A displays pre-1890 trends and dynamic treatment effects of the 1890 Act with cohabitation as the outcome, while Panel B repeats the same exercise with urbanization as the dependent variable instead. Both panels reveal that residential outcomes for sons of eligible and ineligible fathers moved in parallel before 1890.[5] While the identification assumption of parallel trends in the counterfactual without an 1890 Act cannot be tested directly, comparable trajectories in residential outcomes before 1890 support the assumption that these trends would have continued were it not for the new pension law.

The dynamic treatment effects displayed in Figure 2.1 shows that sons of eligible fathers move out in increasing numbers in the immediate decade following the 1890 Act (Panel A) and choose to settle in urban areas (Panel B). These estimates are derived from specifications that include full sets of individual-specific and county-decade fixed effects (just as the preferred specification of Table 2.1 in Column 4). The comparison is therefore between within-person trends of sons with eligible vs. ineligible fathers who grew up in the same county. Taken together, the empirical patterns presented in this section suggests that the 1890 Act made sons of Union veterans, when compared to the sons of their ineligible neighbors, more likely to start their own households and to settle in urban areas.

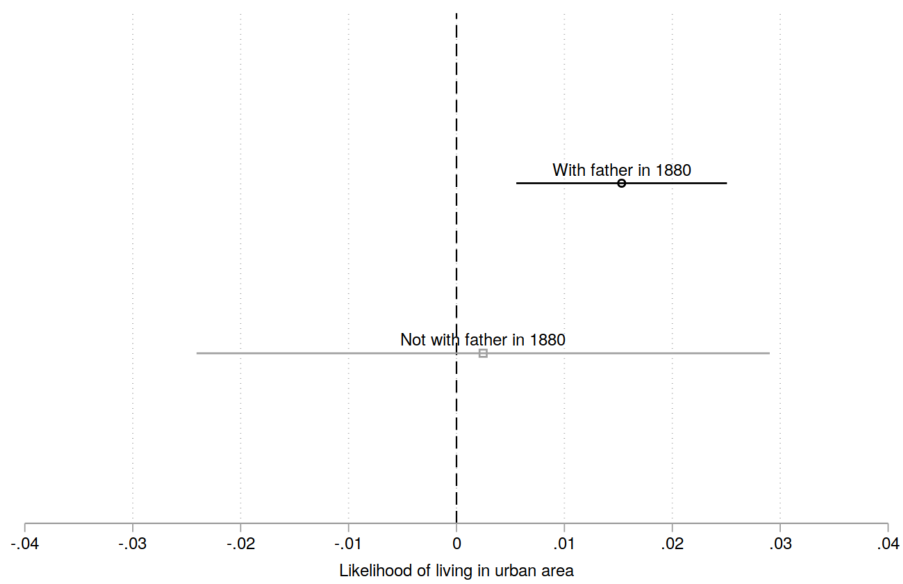
2.4.3 Mechanism of Old Age Support

The results in Section 2.4.2 provide evidence that the sons of Union veterans became more likely to move out of their parental home and to live in urban areas after the 1890 Act became law. This section discusses potential mechanisms driving this effect. I argue that the best explanation for the observed empirical patterns is that eligibility for old age pensions relieved sons of eligible veterans from some of their filial duties and allowed them to move away from their parents to pursue better economic opportunities in rapidly growing urban centers. In this section, I perform two additional empirical exercises to bolster this interpretation. First, I show that the 1890 Act only induces sons of eligible fathers to move to urban areas, if they in fact still lived in their parental home immediately preceding the new law. As expected, for sons who already decided to start their own independent household, it does not matter

[5]For visual evidence of the statistical test of parallel pre-trends, note that the 1870 coefficients in both panels of Figure 2.1 are not significantly different from zero (with 95% confidence intervals). There might still be less severe and insignificant violations of parallel trends. For the cohabitation outcome, however, the patterns in Panel A of Figure 2.1 suggest that any pre-trend violations would likely lead us to underestimate the effect

whether their father became eligible for old age support. Second, I show that the observed effect is not instead driven by some (potentially time-varying) effect of veteran status *per se*. To separately estimate the effects of both pension eligibility and veteran status, I compare sons of Union veterans to sons of Confederate veterans (who did not become eligible for old age support) in a triple difference design. Only sons of eligible (i.e. Union) veterans increasingly reside in urban areas after 1890.

Figure 2.2: Heterogeneity of Effect by Cohabitation Status before 1890 Act



Notes: This figure presents coefficients and 95% confidence intervals for difference-in-difference estimates of the effect of the 1890 Act for two sub-samples: Sons of Union veterans who cohabited with their father in 1880, and those that resided elsewhere. The outcome of interest is whether sons of veterans live in urban areas. See the notes to Table 2.1, Panel B, Column 4, for details on the specification.

Heterogeneity by Cohabitation Status: Figure 2.2 displays the results of estimating Equation 2.1 for two separate sub-samples: Sons who still cohabitated with their fathers preceding the 1890 Act, and sons who already moved out before 1890. The dependent variable in both regressions is a dummy for whether sons live in an urban area. Figure 2.2 reveals that the 1890 Act only increased the likelihood of settling in an urban area for sons of eligible fathers if they resided in their father's household before the pension law. This finding is in line with the theory that pension eligibility for Union veterans allowed their sons to move to urban areas because their were relieved from their filial duties at home. Evaluating the effect of the 1890 Act within the sub-sample of sons that no longer resided with their father serves as a placebo exercise. As predicted by the theory that pension eligibility increases participation in urbanization of sons by replacing the old age care they provide at home, the pension law has no significant effect if father and son already live separately.

Union vs. Confederate Veterans: The empirical strategy behind Equations 2.1 and 2.2 was to compared sons of Union veterans to sons of comparable fathers who did not serve in the Union Army. The main concern with interpreting the treatment effects from this approach is that growing up as the son of a Union veteran father could be a bundled treatment of pension eligibility *and* veteran status. Having a Union veteran has a father means that after 1890 your father becomes eligible for old age pensions. But it also means that you come of age in a household with a veteran, whose war-time experiences might shape you later life choices. To separate the effect of pension eligibility from such potential confounding, I compare sons of Union veterans to a sons of veterans who did not become eligible for any pension in 1890: Confederate veterans. I estimate the following triple difference equation for the effect of the 1890 Act on the decisions of sons to move to urban areas:

$$y_{it} = \beta_e UnionVet_i \times post_t + \beta_v AnyVet_i \times post_t + \theta_{s(i)t} + \eta_i + \epsilon_{it} \tag{2.3}$$

The main coefficient of interest now is β_e on the interaction of the dummy variables for having a father who served in the Union Army, $UnionVet_i$, and for the post-1890 time period, $post_t$. The coefficient β_e captures the additional effect of Union veterans becoming eligible for old age pensions from other effects of veteran status because Equation 2.3 also includes the interaction between post-1890 time trends and the indicator $AnyVet_i$ for being the son of *any* veteran, either Union or Confederate. The coefficient on this interaction, β_v, therefore captures the effect of the 1890 Act for sons of non-Union veterans (i.e. Confederates) in comparison to sons of fathers who did not serve at all. Since only Union veterans became eligible for old age pensions after 1890, β_v serves as a placebo test and is expected to be equal to zero.

The specification includes individual fixed effects, η_i, and a full set of state and state-decade fixed effects $\theta_{s(i)t}$ (based on the state s of individual sons i in 1870). Including these fixed effect ensures that the triple difference estimator compares within-person time trends in the urbanization decisions of sons of Union vs. Confederate veterans who grew up in the same state.[6]

[6]This means that β_e is estimated from Union veteran families who stayed in the North and Confederate

The identifying assumption necessary to causally interpret β_e as the effect of the 1890 Act on the residential decisions of sons of eligible veterans is that the outcomes for sons of Union and Confederate veterans would have followed parallel trends in the absence of the 1890 Act. Following the same logic as for the difference-in-difference design, I test whether both groups were on parallel trends before 1890 as indirect evidence for the identifying assumption. I estimate the following event study specification based on the triple difference estimator of Equation 2.3:

$$y_{it} = \sum_{k=1870}^{1910} \beta_{ek}\lambda_k \times UnionVet_i + \sum_{k=1870}^{1910} \beta_{vk}\lambda_k \times AnyVet_i + \theta_{s(i)t} + \eta_i + \epsilon_{it} \tag{2.4}$$

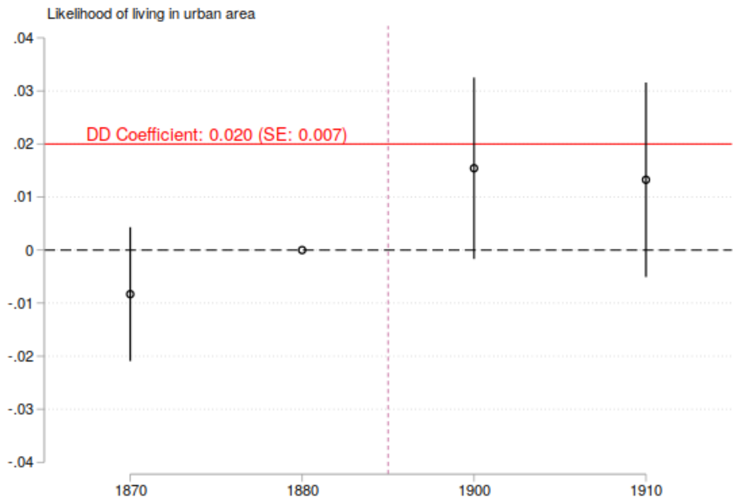
The coefficients of interest, β_{ek}, decompose the treatment effect β_e by decade k and allow us to observe pre-trends and dynamic effects of the 1890 Act for sons of eligible veterans. Coefficients β_{vk} trace out the dynamics of the placebo treatment for sons of ineligible veterans. All other parameters and variables are as defined in Equation 2.3.

Figure 2.3 summarizes the results from estimating Equations 2.3 and 2.4. Panel (a) of Figure 2.3 displays the dynamic effects of the 1890 Act for the urbanization decisions of Union veteran sons in comparison to the sons of Confederate veterans (i.e. coefficients β_{ek}). Before the 1890 Act, there is no significant difference in the trends for both groups, thereby supporting the identifying assumption of the triple difference estimator. After 1890, sons of Union veterans become more likely to move to urban areas. The estimate of the average treatment effect, β_e, implies that pension eligibility increased urban residence by 2.0 p.p. (or by 10.5% on top of the baseline trend). Panel (b) visualizes the dynamics of the placebo exercise, comparing sons of ineligible Confederate veterans to sons of equally ineligible fathers who did not serve in the war. There is no effect of the 1890 Act for sons without eligible fathers. The absence of any effect for ineligible veterans suggests that the effects of the 1890 Act can indeed be attributed to Union veterans becoming eligible for old age pensions.

veterans who moved from the South to the North between the end of the Civil War and 1870. I include state-decade fixed effects instead of county-decade fixed effects (as in Equation 2.2) because there is limited overlap in the 1870 counties of Union and Confederate veteran families.

Figure 2.3: Triple Difference Event Studies for Sons of Union vs. Confederate Veterans

(a) Sons of Union Veterans



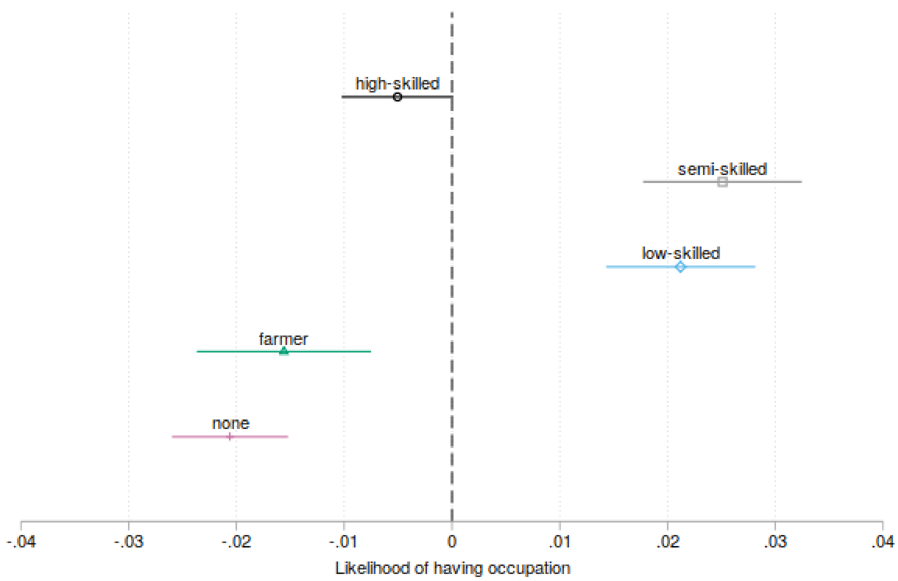
(b) Sons of Confederate Veterans

Notes: This figure presents the dynamic treatment effects of the 1890 Act on the sons of Union vs. Confederate veterans (with 95% confidence intervals) as estimated from the triple difference outlined in Equation 2.3. Panel (a) presents coefficients β_{ek} for each decade k comparing sons of eligible (i.e. Union) veterans to sons of ineligible (i.e. Confederate) veterans, and Panel (b) presents coefficients β_{vk} for the placebo exercise comparing sons of Confederate veterans to sons of fathers who did not serve at all. The outcome of interest is whether sons of veterans reside in urban areas. The figure plots horizontal red lines at the average treatment effect, β_e, in Panel (a), and the placebo effect, β_v, in Panel (b) as estimated from the triple difference design of Equation 2.3. Standard errors are clustered at the level of the 1870 household.

2.4.4 Labor Market Implications

In Sections 2.4.2 and 2.4.3, I documented that the 1890 Act caused sons of eligible Veterans to move to urban areas. If pensions relax the location constraints of sons, does this translate to improvements in their economic standing? In this section, I investigate the implications of pension eligibility for the occupational outcomes of sons. First, I produce difference-in-difference estimates for the effect of the 1890 Act on the type of occupations chosen by sons of eligible veterans. Then, I look at the income effect of pension eligibility as a summary measure.

Figure 2.4: Difference-in-Difference Estimates for the Effect on Occupational Choices



Notes: This figure displays coefficients and 95% confidence intervals for difference-in-differences estimates of the effect of the 1890 Act on the occupational choices of sons of Union veterans. Each coefficient is of a separate regression with an indicator for a different occupation group as the outcome. Section 2.3 gives more details on the coding of occupation groups. See the notes to Table 2.1, Panel B, for details on the chosen specification of Equation 2.1

Figure 2.4 reports coefficients and confidence intervals from five separate difference-in-difference estimates. Each estimate is based on the preferred specification of Equation 2.1 with the full set of fixed effects (as in Table 2.1, Column 4), but with dummies for the current occupation of sons as the dependent variable. Occupations are classified into five groups: high-skilled, semi-skilled, low-skilled, farmer, and none. See Section 2.3 for detail on the classification. Figure 2.4 reveals that pension eligibility for veterans increases the likelihood that their sons work in any formal occupation, makes it more likely that they leave behind farming, and that they enter other low- or semi-skilled occupations, while slightly depressing entry into high-skilled occupations. These results are in line with the theory that old age pensions allowed sons of eligible veterans to transition out of farming and informal work inside their parental home and to pursue other opportunities in the growing urban centers.

Table 2.2: Difference-in-Differences Estimates for the Effect on Occupational Income

	(1) Baseline	(2) +Decade FE	(3) +Son FE	(4) +County-decade FE
Union vet x post	46.942***	47.678***	46.088***	49.566***
	(8.745)	(8.877)	(10.770)	(10.980)
Post 1890	1644.694***			
	(5.454)			
Union veteran	-38.879***	-39.642***		
	(4.134)	(4.305)		
Observations	698156	698156	698156	698156
R^2	0.357	0.389	0.610	0.611

Notes: This table reports difference-in-differences estimates for the effect of the 1890 Act on the occupational income of sons of Union veterans. This repeats the empirical exercise of Table 2.1, but with occupational income as the outcome variable. Occupational income is measured in 1950 dollars and based on occupational income scores. See Section 2.3 for more information on the construction of these scores and Section 2.4.4 for a discussion of the results. See the notes of Table 2.1 for details on the variables in each row and the specification choices for Columns 1-4, and see Section 2.4.1 for a discussion of the underlying research design. Standard errors in parentheses are clustered at the level of the 1870 household.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The question remains whether the shift in the occupations chosen by sons of pension-eligible veterans on average improved their economic outcomes. To shed light on this, I estimate the effect of the 1890 Act on the occupational income of affected sons. Table 2.2 displays difference-in-difference estimates following Equation 2.1, but with occupational income (in 1950 dollars) as the dependent variable. See Section 2.3 for more information on how occupational income is measured. Table 2.2 reveals a remarkably similar pattern across all four specifications. The preferred specification, in Column 4, shows that the 1890 Act increased the yearly occupational income for sons with eligible fathers by 49.6 dollars (or by 3% over the baseline time trend). Taken together with the results discussed in previous sections, this suggests that pension eligibility allowed sons of eligible fathers to pursue better paid opportunities further away from the parental home and in urban areas.

2.5 Conclusion

When the welfare state takes over services that used to be informally provided by family members or the local community it might not only help the intended beneficiaries but also release previous informal providers to pursue new opportunities elsewhere. In this paper, I studied the 1890 transformation of the Civil War survivor pension under the Dependent and Disability Pension Act into a de-facto system of old-age support. I test whether this expansion of the US welfare state enabled the sons of Union veterans to move out of their parental home, to participate in urbanization, and to pursue higher paid occupations. I find that sons of men eligible for old age support are 1.6% less likely to cohabit with their parents and 8.4% percentage points more likely to settle in an urban area. These effects on the residential choices of affected sons are accompanied with a shift in their occupational outcomes. The pension reform contributed to a transition out of farming and into low- and semi-skilled labor in the formal labor market. On average this occupational shift improved the income of sons with pension-eligible fathers. Based on this evidence, I conclude that welfare state expansion, such as the introduction of old-age pensions can affect family structure and enable family members to participate in and benefit from economic modernization.

Chapter 3

Market Structure and Competition for Indigenous Labor

3.1 Introduction

In recent years, there has been a resurgence of interest in understanding imperfect competition in labor markets and its consequences for welfare. Empirical puzzles, including the rise of superstar firms, declining labor share of income, limited geographic and job-to-job mobility, and increasing inequality, have defied attempts at reconciliation with theories of perfectly competitive labor markets. These findings have motivated further research into models of monopsonistic labor markets, which posit that employers curb labor demand to reduce costs and maximize profits by paying wages below workers' productivity levels.

Traditionally, the term "monopsony" has referred to a single buyer dominating a market (Robinson, 1933).[1] Recent definitions in the labor literature allow for a broader interpretation of monopsony, including the employer's wage-setting power attributed to search frictions and job differentiation (e.g. Boal and Ransom (1997), Manning (2003) or Ashenfelter et al. (2010)). However, one of the most striking findings in the recent literature reveals that, for a significant number of workers, the number of potential employers in their local labor market is relatively small, and that employment and wages are highly concentrated in a few firms (Berger et al., 2022). These findings motivate further research into the role of labor demand concentration, and broadly market structure, as a key contributor to monopsony power and wage suppression.

[1] As reported in Thornton (2004), Robinson argued that "is necessary to find a name for the individual buyer which will correspond to the name monopolist for the individual seller. In the following pages an individual buyer is referred to as a monopsonist".

Historically, market structure and the concentration of employers have often granted companies significant monopsony power. For example, colonial trading companies in the early modern era (starting in the 16th century) were well-positioned to exercise market power over the labor force in their areas of operation. The extent of that power depended on the ability of rival firms to enter their markets. Many firms, such as the Dutch and English East India Companies or the Royal African Company, were given charters of monopoly and exclusive rights to trade with a region. Some firms even used force to exclude rivals (Phillips and Sharman, 2020). As the main purchasers of Indigenous production, trading companies could procure their wares at below-market prices, even without direct labor coercion.

Did the entry of rival companies enable Indigenous workers to play off the Europeans against one another and improve their labor market outcomes? In this paper, we leverage newly collected archival data to estimate how the entry of a competitor affects the wages offered by a colonial company to their Indigenous labor force.

This study provides evidence from one of history's oldest and most powerful corporations, the Hudson's Bay Company (HBC). The HBC played a major role in Canadian history,[2] helping to pioneer the transatlantic fur trade. After the French expulsion from Canada in 1763, the HBC's monopsony was challenged by Montreal-based free traders. From 1779, many of these so-called "pedlars" combined forces as the North West Company (NWC). The NWC, which had access to British capital and goods markets, took over the preexisting French trading network and aggressively competed for Indigenous labor.

To understand how the changing market structure affected wages, we combine several historical sources. We geolocate all of the trading posts operated by the HBC and its competitors over the period 1763-1810, during which the HBC lost its dominant position and the NWC was founded. For the same period, we hand-collect and transcribe the HBC Account Books, which contain detailed information on the value of furs hunted by Indigenous people and the trading goods provided in exchange by the company at each trading post. We follow the HBC's accounting system of reference prices for goods and pelts to derive a measure of piece-rate wages paid to Indigenous workers. The piece-rate wages are calculated as the ratio between the value of the goods received by Indigenous hunters over the value of the pelts they delivered to the company.

We find that the period of heightened competition between the two companies led to a proliferation of trading posts, with the HBC and NWC progressively operating in overlapping areas. The distance between HBC and competitor posts decreased over time as the NWC encroached on the HBC's monopsony area around the Hudson Bay and as the HBC expanded

[2]In this paper, "Canada" is used as a shorthand to describe the region encompassed by the modern country. The areas controlled by the British Crown—and then termed Canada—were confined to a relatively narrow band along the St. Lawrence River and the Eastern Great Lakes. Most of the setting discussed in this paper overlapped with Indigenous sovereign territory.

inland. At the same time, the average wages paid to Indigenous trappers increased from 69.9% of the reference price of fur in 1763 to 97.3% in 1810.

By leveraging spatial variation in the change in distance to the closest competitor's post, we show that wage markdowns responded to the entry of a competitor into the local labor market. Indigenous trappers started to receive more for their output compared to the period of HBC's monopsony. Holding the HBC trading post fixed and isolating variation in distance from the entry of competing posts, we show that every 100 kilometers decrease in distance to the closest competitors' post came with a 1.5 percentage point increase in the wages paid to Indigenous hunters for their furs.

Our findings contribute to a growing empirical literature on the implications of market structure on wages. In this literature, market structure is often proxied by an index of employers' concentration akin to the Herfindahl-Hirschman index (HHI), used broadly by both academics and antitrust authorities to assess market power in product markets. Following this approach, Autor et al. (2020) find an increase in national sales concentration and a fall in the labor share, while Rossi-Hansberg et al. (2021) highlight a decrease in regional employment concentration, even as national concentration continues to increase. Other papers have documented the relationship between wages and concentration, as measured in various data sources such as administrative data (Benmelech et al. (2022), Yeh et al. (2022), Rinz (2022)) or vacancies from online sources (Azar et al. (2020), Azar et al. (2022)). The closest approach to ours is exploiting changes in market structures due to mergers (Prager and Schmitt (2021), Arnold (2021)). These studies typically demonstrate a negative relationship: increased employer concentration is associated with lower wages.

Related to this literature, our contribution is two-fold. First, our setting allows us to define the market structure and the set of relevant competitors in a clean and straightforward manner. This eliminates the need for relying on a concentration index calculated using predefined industry categorizations like the HHI, which often don't align with workers' outside options (Nimczik, 2020). While Indigenous people had the option not to participate in the fur trade with Europeans, fur trapping constituted the only way to obtain manufactured goods from Europe, which simplifies the assessment of alternative employers competing in the market. Second, the NWC's entry into labor markets that were previously locally dominated by the HBC is a unique case study of the transition from monopsony to duopsony. Our findings of increasing wages with declining distance to the closest competitor complement earlier studies that established a similar relationship leveraging different sources of variation in market structure, such as local employer concentration or mergers.

We also contribute to the literature on the effects of colonial organizations and institutions in economic development (Acemoglu et al., 2001; Dell, 2010; Nunn, 2008; Diaz-Cayeros and Jha, 2016; Dell and Olken, 2020). In particular, our work relates to studies on the role of private or semi-private actors for Indigenous welfare, akin to the studies conducted by

Méndez and Van Patten (2022) and Lowes and Montero (2021). In contrast to these two papers that focus on the long-term consequences of the concessions, we use contemporary data to quantify the rate of extraction that a colonial company exerted on an Indigenous population in settings of local monopsony and duopsony. Our results are compatible with the interpretation that better outside options, as measured through the distance between competing firms, decreased the rate of extraction and improved Indigenous wages.[3]

Furthermore, we contribute to a long-standing literature on the economic history of the Canadian fur trade. Carlos (1981, 1982) and Carlos and Hoffman (1986) studied the rivalry between the HBC and the NWC after 1810. We show the existence of intense spatial competition prior to this point, dating back to at least the 1790s. Other authors have debated the HBC's impact on Indigenous welfare. Innis (1930) argued that the relatively benign role of the fur trade was in part responsible for Canada's more quiescent relationship with its Indigenous peoples. More recent studies, including Carlos and Lewis (1993) and Carlos and Lewis (2010), have argued that the fur trade resulted in higher Indigenous living standards prior to 1760. Ray (1974) emphasizes increasing Indigenous dependence on trade with Europeans as their resources—beaver and buffalo—declined. We build on work by Carlos and Lewis (1993), who study the four HBC posts operating before 1763 and find greater fur prices at posts closer to Montreal at times of competition with France. Expanding the analysis to a panel of prices at all known posts operated by the HBC from 1694 to 1810 allows us to hold post-level characteristics fixed and isolate variation from the entry of competitors. Our empirical exercise quantifies the extent to which Indigenous traders were able to exploit inter-firm rivalry to improve their labor market outcomes.[4]

Our results support the views of economic historians who assigned Canada's Indigenous peoples greater agency in their historical interactions with European colonialists. The sociologist Karl Polanyi (2018) argued that because Europeans organized the fur trade, Indigenous Canadians were passive players in the colonial game. Early studies of the Canadian fur trade, like those of Rich (1958) and Innis (1930), tended to focus on the operations of the trading companies and minimize the role of Indigenous customs and decisions. In particular, they posited that Indigenous people had different economic motivations than the market-oriented Europeans. Even Ray (1974), who emphasized Indigenous agency in the fur trade, suggested

[3]Our study is also related to that of Diaz-Cayeros et al. (2022), who show that settlements producing hard-to-expropriate cochineal dyestuffs in New Spain were more likely to survive the Conquest of Mexico, as the Spanish could not easily replicate their skills. This parallels our argument that the irreplaceable trapping and navigation skills of Indigenous workers allowed them to exploit inter-firm competition to improve their labor market outcomes.

[4]Our analysis does not aim to evaluate the overall impact that the HBC had on the Indigenous populations with which it interacted, and our work is neither an endorsement nor a comprehensive evaluation of European colonialism.

that they were "satisficers"—desirous only to maintain a fixed standard of living. In their seminal book *Commerce by a Frozen Sea* (and in several papers), Ann Carlos and Frank D. Lewis have presented a different view: Canada's Indigenous peoples were sophisticated economic actors who responded to price incentives and shaped the fur trade via their customs and preferences (Carlos and Lewis, 1999, 1993, 2010). Our empirical results support the conclusions of Carlos and Lewis, showing that Indigenous traders knew how to leverage the competition between colonial companies to obtain superior prices for their goods.

The remaining portion of the paper is structured as follows. The historical context of the Canadian fur trade is discussed in Section 3.2. Section 3.3 includes details of the data used in our analysis. We provide graphical evidence and present our empirical results in Section 3.4. Section 3.5 concludes.

3.2 Historical Context

3.2.1 European Entry into the Canadian Fur Trade

The transatlantic fur trade, one of Canada's oldest and most historically significant industries, dates back to the arrival of European explorers in the 16th century. Initially carried out on a small scale, the impetus for the expansion of this commerce came from the rapidly expanding high-fashion felt hat industry, for which beaver wool was the ideal raw material. The near-extinction of the European beaver during the seventeenth century and the growth of the hat-making trade propelled efforts to derive a North American source of supply (Carlos and Lewis, 2010).

The first European participants in the Canadian fur trade were the French "coureurs des bois", who successfully worked the regions along St. Lawrence and Ottawa Rivers. This blocked efforts by the English, based in New England, to enter the market. In 1670, King Charles II granted a charter of monopoly to the Hudson's Bay Company (HBC), establishing its monopoly over all trade through the Hudson Strait and its exclusive possession of the vast Hudson's Bay drainage basin (Easterbrook and Aitken, 1988).[5]

While the HBC's nominal domains were nearly boundless, its actual powers were limited. For the first 150 years of the company's operations, it possessed only a small set of trading posts on the edge of the Bay, manned by a skeleton staff of traders and clerks.

[5] In this sense, the HBC might be properly characterized as a "concessionary company," having been assigned the rights to a nominal trade monopoly in a specific region and product. Carlos (1992) has compared the HBC with the Royal African Company, which also enjoyed a monopoly over the West African slave trade in name but not in practice. The HBC and RAC were both formed during the early 1670s, based in London, and run using similar organizational structures.

Though the Europeans possessed firearms, more populous Indigenous tribes—the Cree and Assiniboin—actually controlled most of the region (Carlos and Lewis, 2010; Innis, 1930; Ray, 1974).

The HBC entered a well-established European fur market, with supplies coming from New France, the Continental colonies, northern Europe, and Russia. However, the HBC had access to top-quality pelts, mainly beaver but including marten, fox, and muskrat, that were highly prized by European furriers (Carlos and Lewis, 2010, p. 16-7). As the HBC was required by a clause in its charter to sell its furs only on the English market (Carlos, 1981), historians have retrieved an estimate of the total value of its commerce and its market share. Already from 1738 through 1748, the company's imports to England totaled more than one million beaver pelts, valued at over £270,000, or more than $63 million in today's currency (Pettigrew and Smith, 2017). Work by Wien and Pritchar (1987) indicates that the HBC was not a monopolist in the *London* market. In 1772, the HBC accounted for less than 20% of the total value of beaver pelts exported from North America to England.

3.2.2 Indigenous Relations with European Fur Traders

The Canadian fur trade was *dependent* on Indigenous peoples.[6] Europeans relied on Indigenous hunters for their superior skill at trapping; their knowledge of animal behavior (e.g. migration patterns and food sources); and their winter survival techniques (i.e., construction of snowshoes and the use of dog teams; cf. Honeyman (2003)). In turn, Europeans provided trappers with a wide range of foreign goods. Initially, manufactured consumer and producer goods, including textiles, firearms, and metal tools, were predominant. With fur prices increasing from the 1730s, higher Indigenous incomes were diverted into luxury goods like tobacco, alcohol, cloth, and beads.[7] The exchange happened at trading posts, mostly located along waterways. The furs would be stored at the posts until they could be shipped to Europe for processing and final sale.

Indigenous traders frequently held significant leverage in dealings with Europeans. In particular, their skills (knowledge of local geography and acuity at trapping) were costly to acquire or imitate. The HBC's directors, therefore, ordered post factors to maintain

[6] In this setting, neither party possessed the power to coerce the other (Carlos and Lewis, 2002).

[7] While some historians emphasize the negative consequences of the increasing share of alcohol in luxury purchases (Usner, 1987; Braund, 1996), Carlos and Lewis (2002) show that even under conservative assumptions, Indigenous adults likely drank no more than contemporary Europeans. In 1740, Indigenous traders received 450 gallons of alcohol, good for 0.5 gallons per capita. By contrast, English consumption per capita at the time was 1.4 gallons annually (Carlos and Lewis, 2002, pp. 300-1).

friendly relations with Indigenous traders.[8] Traders learned local languages and sought to provide their discerning Indigenous suppliers with high-quality goods. When post factors observed that trappers preferred Brazilian roll tobacco to Virginian, the company was forced to acquire greater stocks of the superior variety (despite the longer supply chain) and banned employees from consuming it (Carlos and Lewis, 2010, pp. 88-9). In 1728, James Isham, governor of York Factory, wrote to London that "never was any man so upbraided with our Powder, Kettles and Hatchets, than we have been this summer by all the Natives, Especially by those that border near the French" (Carlos and Lewis, 2010, p. 100). In 1739, Isham reported (at the behest of the London director) on the "Indians dislike of particular goods" and their reasons for not purchasing them. The firm's kettles were said to be "small for the weight, [and] of a very bad shape"; other complained that the knives had "very bad blades and worse handles"; and that "Twine is... very weak and uneven, being as thick as packthread in some places and as thin as thread in other places" (Carlos and Lewis, 2002, pp. 308-9). Thus the company assiduously adapted itself to the consumer demands of its discerning Indigenous counterparties, even when doing so likely increased operating costs (Carlos and Lewis, 2010).

In these respects, the HBC's conduct differed markedly from the settler colonialism of the nineteenth century—predicated on superior military force, large and increasing European populations, and government by a colonial administration.

3.2.3 Phases of Competition

Despite the HBC's charter, the firm always faced some competition. The first forty years of the company's existence were embroiled in geopolitical competition with the French. After the Treaty of Utrecht in 1713 declared the Hudson Bay basin British territory, competition with French traders continued at the fringes of the HBC's monopsony area. The period characterized by HBC monopsony close to the Bay and competition in its periphery lasted until the end of the French and Indian War in 1763 (Easterbrook and Aitken, 1988).

The French expulsion from North America resulted in an influx of free-trading "pedlars" from Montreal. The pedlars were more aggressive in penetrating the HBC's monopsony area and, though disorganized, cut into the company's trade (Rich, 1958, pp. 246-252). In response, the HBC abandoned its policy of operating exclusively on the Bay, establishing its first inland post at Cumberland House in 1774. The combination in 1779 (and more formally in 1783-4) of several independent traders—financed by Anglo-Scottish merchants—as the North West Company (NWC) presented the HBC with a competitor of comparable strength.

[8] A 1727 missive, for example, asked that post factors "carefully observe our former instructions to treat the Natives very civilly" (Carlos and Lewis, 2010, p. 75).

The NWC coupled the skills of the Montreal-based pedlars with access to credit, overseas fur markets, and the cheap British manufactures and Brazilian tobacco that made the HBC competitive (Ray, 1974).

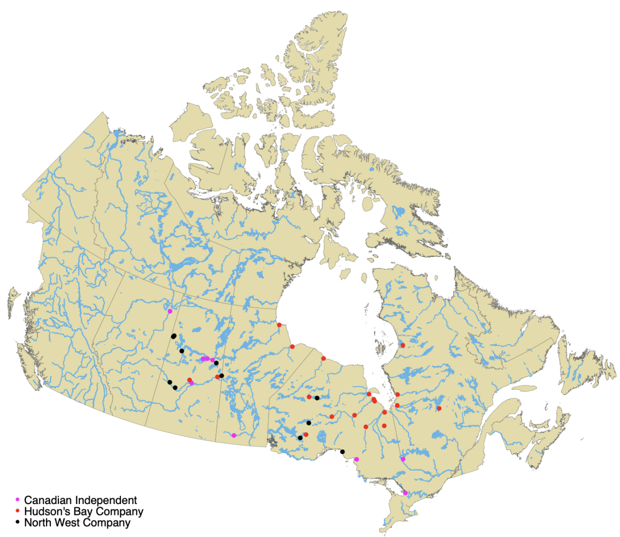
The formation of the NWC kick-started a race to expand the firms' trading and supply networks—a race that involved deliberate encroachment on opposition hinterlands. After the HBC built a post at Portage l'Isle on the Winnipeg River in 1793, for example, the NWC erected a post of their own just two weeks later on the opposite bank. The HBC post closed in 1797 (Freeman and Dungey, 1981, pp. 263-6).[9] This phase of intensifying competition is illustrated by Figure 3.1, which shows the evolution of the fur trade from 1780 to 1805 in four maps. During the 1780s, the HBC slowly responded to the westward drive of the NWC and independents, moving toward Athabasca and the Great Lakes. A more dramatic shift occurred in the period 1790-1805, when the HBC and NWC (and briefly the XY Company) raced west across the prairies. Posts cluster at tight intervals along many major rivers, evidence of deliberate efforts to cut off opposition hinterlands and of the importance of waterways for transportation.[10]

Our analysis concludes in 1810. By this year, the Napoleonic blockade—which inhibited fur exports from England to Europe—had slashed demand for the HBC's goods and induced a severe financial crisis. In response, the HBC moved aggressively into the NWC's Athabasca heartlands, initiating a phase of unprecedentedly aggressive competition between the two firms (Carlos, 1981, pp. 792-4). This included outbreaks of violence between the operatives, from harassment and intimidation to outright massacres and seizures of competing posts (Carlos, 1982, pp. 177-8). We are leaving an investigation of this era of "predatory competition" (Carlos, 1982), which ended in a merger of the two firms, for subsequent research.

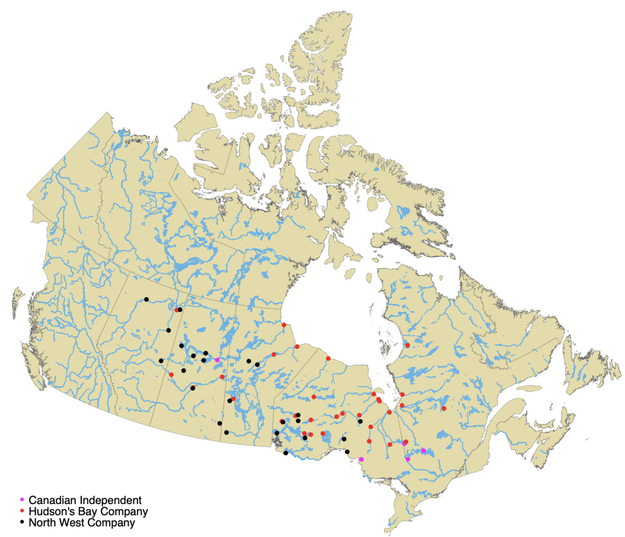
[9] There were two posts on Cat Lake and two posts on Crow's Nest Lake. Each company also had its own Cumberland House, Swan River, and South Branch House.

[10] As there were no roads or railways available, all transportation had to rely on rivers and lakes. The most appropriate means of transportation was the canoe, which had already been perfected by the local inhabitants (Morse, 1969). The most distant posts could be reached from Montreal with fewer than half a dozen portages. The fur trade thus integrated Canada's regions into a single economic system for the first time.

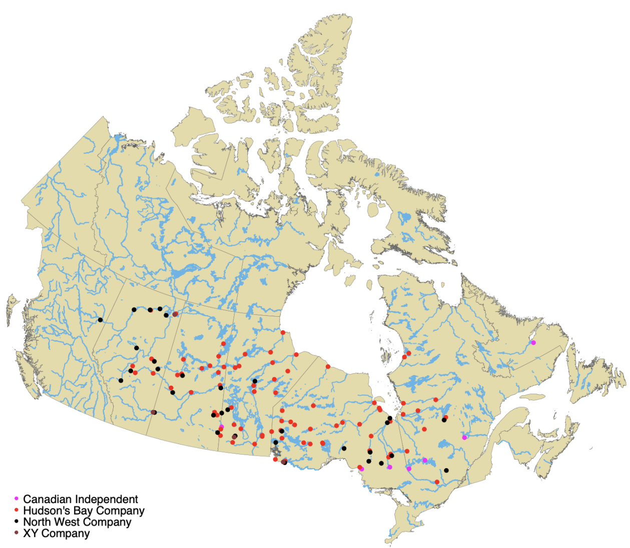
Figure 3.1: The Evolution of the HBC and NWC Rivalry



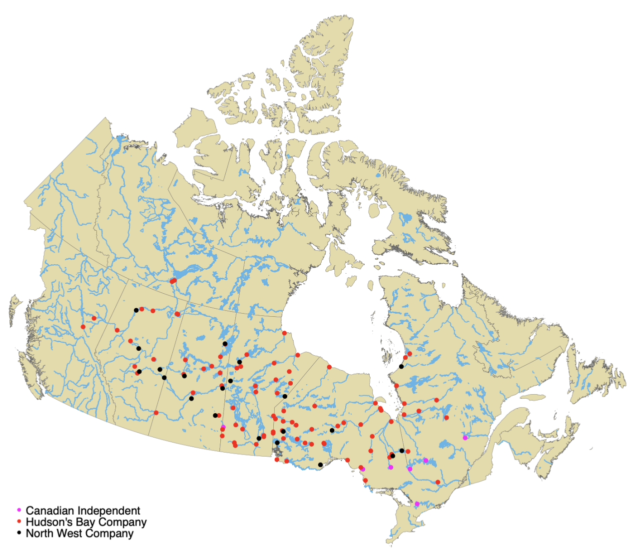
(a) 1780



(b) 1790



(c) 1800



(d) 1805

Notes: These maps depict the evolution of competition between the major fur-trading Companies from 1780 to 1805. Each panel shows the locations of posts extant in the specified year; 1780 in subfigure (a), 1790 in subfigure (b), 1800 in subfigure (c), and 1805 in subfigure (d). See Section 3.3.2 for information on the construction of these maps.

3.3 Data

3.3.1 Account Books

For our analysis, we leverage two particular features of the HBC organizational structure. First, agency problems arising from the distance between the HBC's headquarters in London and the post factors in Canada required the Company to devise an accountability system for the fur trade.[11] For this reason, the HBC's directors mandated the posts' factors to keep comprehensive account books detailing the post's business operations which would then be sent to London at the end of each trading year.

The HBC Account Books constitute an invaluable source of information for several reasons. First, the books' standardized format allows us to compare the state of the fur trade across posts (Ray, 1975). Moreover, HBC accounting procedures required the traders to inventory the goods traded, furs received, and goods remaining for future use (Carlos and Nicholas, 1990), distinguishing between goods shipped to posts for trade purposes and those which were intended for use by the factors.

Second, the lack of a common currency between HBC factors and Indigenous traders forced the HBC to devise its own unit of account.[12] For this reason, the HBC valued both trade goods and furs in *made beaver* (MB), each unit equivalent in value to one prime beaver skin. Trade goods were valued according to the *Official Standard*, while furs received from Indigenous traders were evaluated according to the *Comparative Standard*. Together, the two documents constituted an 'exchange rate' between the HBC's goods and Indigenous-collected pelts.

We build on work by Ray (1974) and (Carlos and Lewis, 1993) to use the standards of trade to assess Indigenous wages at each post, and thereby shed light on the labor market power of the HBC. Post factors had the incentive to minimize the value of goods traded per pelt received. When they could, factors demanded more for their goods (i.e. charged higher 'prices') than the Official Standard warranted, usually by giving short measures (less gunpowder, fewer beads, etc.) during the trading.[13] For this reason, the value of furs received almost always exceeded the value of goods given to the Indigenous traders according to the

[11]See Carlos and Nicholas (1990) for a more detailed explanation of the HBC agency problems and operation strategies aimed at reducing opportunistic behavior.

[12]While Ray (1974, p. 61) argued that "the Indians lacked any concept of money," Carlos (2023, p. 333) writes that North American Indigenous tribes did use currencies, including the Chumash, who employed cupped beads.

[13]Indigenous people understood that this was occurring, but accepted it, within limits, as a necessary part of the barter economy. By the same token, Indigenous traders strove to extract greater quantities of goods from the HBC in exchange for each pelt.

Official Standard valuation, and the difference between the two sums was labeled "overplus" and recorded in the account books at the close of each trading season (Ray, 1974, pp. 63-5).[14] We use this data to quantify wage markdown for each post and year. We measure markdowns as the ratio of the value of goods exchanged with Indigenous traders over the value of furs the HBC received.[15]

We collected and digitized all available Account Books stored at the firm's official archives at the University of Manitoba. For the period of interest (1760-1810), we extracted and manually transcribed the relevant account lines: total furs received, goods expended in trade, and overplus[16] We assembled an annual post-level panel for the available years. We are confident that our sample includes close to all of the fur collected by the HBC. Over 1799-1800, two years for which we have detailed data on the HBC's fur exports, the firm sent 51,341 skins of parchment beaver to Britain, of which we capture 46,337, or roughly 90 percent.[17]

3.3.2 Geospatial Data

To capture the changing economic geography of the fur trade, we also collected, digitized and geo-referenced two separate maps of Canadian fur trading posts.

The first map is from the Manitoba archives, covers the HBC alone, and contains 502 posts ranging from Hawaii to Labrador. The second is extracted from the 1973 *National Atlas of Canada* (Division, 1974). It is restricted to Canadian posts and thus contains fewer belonging to the HBC, but it has several additional benefits: more precise coordinates, dates of establishment and (approximate) closure, and, most importantly, includes the locations of French, Canadian and British independent, XY Company, and NWC posts.

We manually linked the HBC posts in the two collections to create a full dataset of the locations and operation times of every station in the Canadian fur trade. Using this extended

[14] We are not the first to suggest that the overplus may be used for this purpose. Ray (1974, p. 65) observed that the overplus varied with "competitive conditions", while Carlos and Lewis (2010, p. 55) note that the measure indicates the "relative distribution of gains" between Indigenous traders and post factors.

[15] We follow (Carlos and Lewis, 1993) and include both goods explicitly traded and those given as gifts in in the value of goods exchanges.

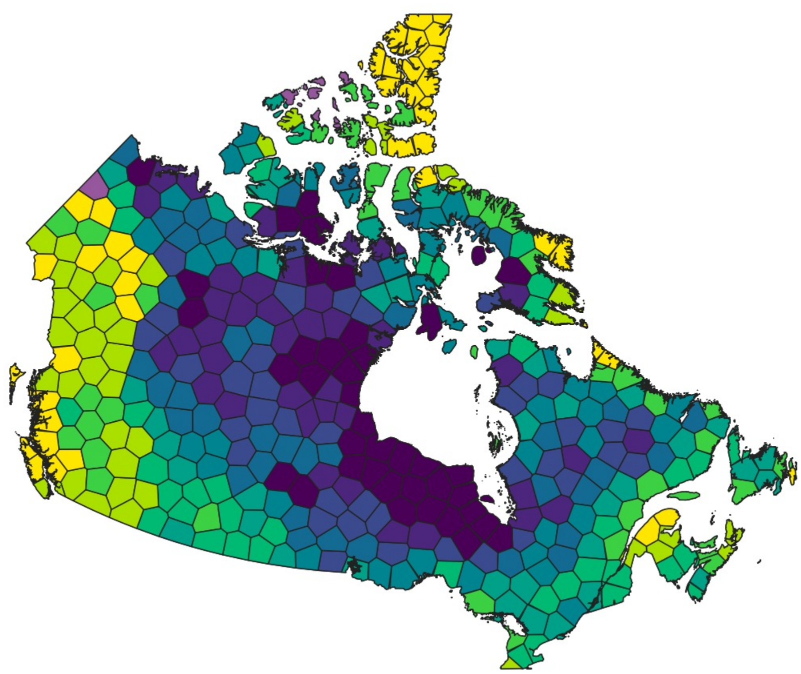
[16] Notice that the value of the fur received should be equal to the value of the goods exchanged plus the overplus. Hence, where one variable was unavailable, the difference between the other two can be used to fill the gap.

[17] Previous work by Carlos and Lewis (1993, 1999, 2002) has focused on the account books of the four main posts (York Factory, Fort Albany, Moose Factory, and Fort Churchill) that have been transcribed over long time ranges. We collected data on over forty-five additional posts—unavailable during the period studied by Carlos and Lewis (1999)—of varying sizes. While many of these posts drop in and out, they are most densely clustered in the period of HBC-NWC rivalry.

set, we created a post-year panel stretching from 1670 to the late twentieth century, although we focus on the period 1763-1810 in our analysis. For each post-year combination, we then calculated the distance in kilometers to the nearest competing establishment.[18]

Moreover, we calculated a proxy for the suitability of each area to being inhabited by beaver, the main animal that was hunted in the fur trade (Morse, 1969). For this purpose, we replicated the US Geological Survey GAP model for beavers across Canada. The final iteration uses 10-meter resolution Landsat-based landcover data for North America in combination with a digital elevation model of Canada (which gives a finely-detailed description of the country's topology) and water-flow vectors to pinpoint the capacity of the country's alluvial habitats.

Figure 3.2: Map of Beaver Suitability



Notes: This figure shows a map of the average suitability for beaver habitation in each of 400 polygons of roughly equal size. Darker values represent more suitable areas. See Section 3.3.2 for a description of the algorithm used to derive the suitability measure, which is based on the USGS GAP model.

[18]As a validation exercise to analyze possible cannibalization between 'friendly' posts (akin to intra-brand competition), we also computed the same measure, but to the nearest other HBC post.

Suitable areas for beavers are considered to be those: a) within 250 meters or 60 meters inside of water bodies; b) located on flat terrain; and c) restricted to elevations below 3400 meters and to afforested or wetland regions. We then divide Canada into 400 polygons of roughly equal size and calculate the share of the area of the polygon covered by land suitable for beavers. Figure 3.2 presents a map of Canada with the polygons colored by average beaver suitability. The map reveals that the initial region of HBC expansion on the southwestern shore of Hudson's Bay has the highest suitability for beaver.[19]

3.4 Empirical Results

3.4.1 Conceptual Framework for Spatial Competition

To set up the analysis discussed below, it will be useful to keep in mind the following stylized model of spatial competition, inspired by Hotelling (1929).

In our setting, there is a labor market with two firms (the HBC and NWC) and only one occupation for Indigenous workers, namely fur trapping. This is in addition to their non-market outside option. While Indigenous Canadians were not obligated to participate in the fur trade for their subsistence, it was only by hunting and gathering pelts that they could obtain manufactured and luxury products of Europe's new and expanding industry.

The two firms compete à la Hotelling for labor by choosing two variables: first, they choose the locations of their trading posts and then compete on the value of goods offered to Indigenous trappers for the pelts they hunted. The Indigenous utility is increasing in wages (or the wage markdown, defined as the share of the marginal product of labor that they receive) but decreasing in transportation costs: given the costs of traveling to posts and transporting furs, higher markdowns will expand the labor supply at a company's post. A company can also choose to move the location of its post closer to Indigenous settlements, thereby reducing Indigenous transport costs and enlarging the firm's harvest. But if the other firm chooses a more favorable price-location pair, it will capture a greater share of the season's furs. This simplified model incorporates the two key features of standard monopsony models: (1) upward-sloping firm-specific labor supply and (2) wage posting.

In each period, the two firms decide whether to open a new post, thus entering a new labor market and expanding their network. The entry choice is a strategic decision and a dynamic problem, that depends on the number and characteristics of the competitors and potential markets. At this stage, we provide only descriptive evidence on the patterns of

[19] Figure 3.2 describes the intrinsic suitability for beaver habitation. Suitability can differ from the actually occurring density of beaver populations, for example due to greater hunting in some locations. Table 3.2 presents evidence that greater hunting at older locations is not driving our results.

entry and we abstract from analyzing the location choice in a structural manner. We focus on spatial differentiation and proxy for the intensity of spatial competition by using the distance from each HBC post to the nearest competitor post.[20] Our main interest is in understanding if markdowns respond to changes in the competitor's distance to the post.

3.4.2 Descriptive Evidence on Post Locations

We start our descriptive analysis by providing some graphical evidence of the HBC's patterns of spatial differentiation.

Figure 3.3: Average Distance between HBC Posts and Closest Competitors, 1760-1810

Notes: This figure shows the average distance to the nearest non-HBC post from each HBC post in the sample. We derive post locations from the University of Manitoba archives and from Division (1974).

In Figure 3.3, we plot a yearly time-series of the average distance between each HBC post and its nearest competitor post.[21] Due to the combination of the NWC expanding its

[20] We also use the distance between each HBC post and its two closest rival posts in tables not reported here. Using two closest competitors is in the spirit of Salop (1979), in which each establishment competes directly only with its two nearest neighbors.

[21] We use all HBC posts in this calculation, even though some of them might have been used as points of supply depots for the inland traders.

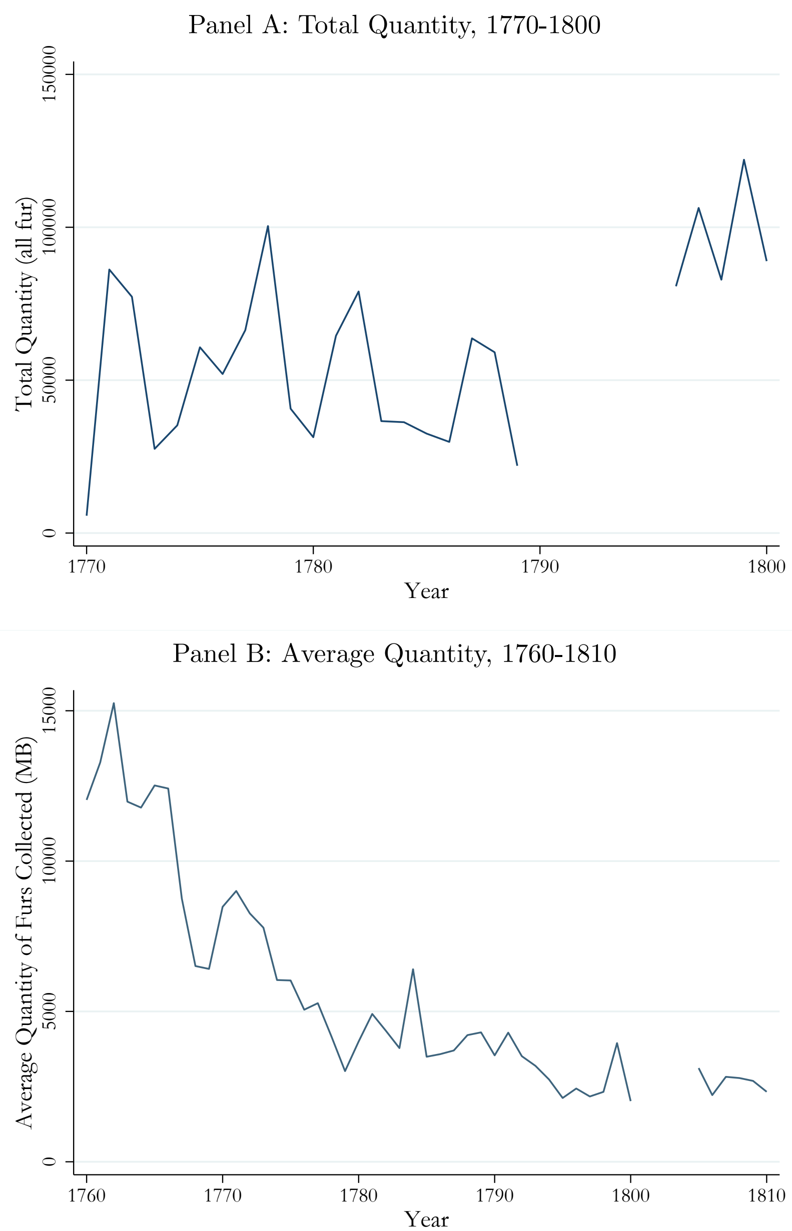
trading network and the HBC opening increasingly more posts over time, the average distance between HBC posts and its competitors' posts declined from 700 kilometers in the 1760s to less than 200 by 1810. The reduced distance between trading posts and the proliferation of posts in all areas of Canada suggests that Indigenous trappers might have benefited from increased competition between the two companies through decreasing transportation costs.

In Figure 3.4, we provide graphical evidence of the quantity of fur collected by the HBC in the period of interest. Panel A 3.4 presents a time-series of the total amount of fur, measured in made beaver, collected across all HBC sampled posts collected by each HBC post across our sample period (1760-1810). There is substantial variation over time, with a noticeable drop around 1780, when the NWC started operating. This suggests that the reorganization of trade in Montreal and the financial means of the NWC allowed this company to compete more effectively than previous independent traders.

Panel B instead shows a time-series of the average quantity of fur, measured in made beaver, collected by each HBC post across our sample period (1760-1810). There is a significant decline from the peak achieved at the close of the Seven Years' War, falling from over 12000 MB in the late 1760s to under 5000 during the late 1770s and to fewer than 2500 in 1810. Several reasons are responsible for this change. First and foremost, the HBC experienced (as shown in Figure 3.3) increasing competition from independent traders, and then the NWC, which cut into their fur harvests. Second, the HBC established an array of minor posts to compete with its rivals. These establishments were smaller than the main factories on the Bay and consequently returned fewer furs per year. They also diluted the returns at the major posts, as inland Indigenous traders preferred to travel to nearer venues.

Moreover, it is important to understand where the trading companies decided to open new posts. As the expansion of the fur trade was mostly driven by the high demand for fur (mostly beaver) in Europe, we expect the decision to set up a new trading post to correlate with the degree of suitability for beaver habitat. Panel A of Figure 3.5 demonstrates that the HBC first built posts in locations with a high share of suitable area for trapping beavers and then later expanded into progressively less suitable areas. Similarly, Panel B describes the pattern of post selection for the NWC: while the first active posts were opened in highly suitable areas (close to the St. Lawrence rivers, taking over the previous French network), the later period witnesses active posts located in less suitable areas.

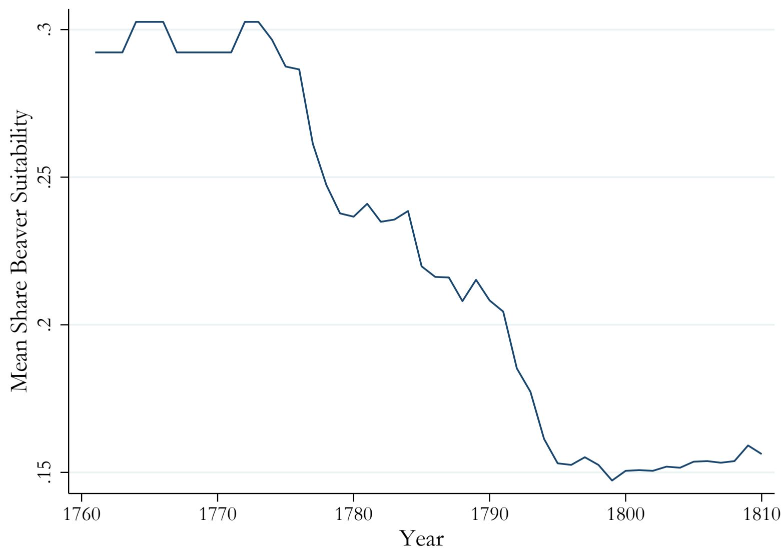
Figure 3.4: Fur Collected by the HBC, 1760-1810



Notes: Panel A of this figure portrays the evolution of the total quantity of fur, aggregated in made beaver (MB), across all sampled HBC posts. Panel B depicts the evolution of the average quantity of fur, aggregated in made beaver (MB), across all sampled HBC posts.

Figure 3.5: Average Suitability for Locations of Active HBC and NWC Posts, 1760-1810

Panel A: Suitability at HBC Posts, 1760-1810



Panel B: Suitability at NWC Posts, 1780-1810

Notes: Panel A of this figure shows the declining suitability for beaver at the post level for HBC posts, demonstrating the occupation of marginal trapping areas with the intensification of competition. Similarly, Panel B shows the average share of suitable area for all active NWC posts. See Section 3.3.2 for details on the construction of our geospatial data.

3.4.3 Descriptive Evidence on Markdowns

The spatial competition between the HBC and its rivals did not only involve the proliferation of posts, but it also affected the level of wage markdown. The data presented in Figure 3.6 indicate a significant rise in the average wage markdown (across all posts) paid by the HBC over time. In the 1760s, trappers used to receive on average not even 70% of the value produced by their labor, hunting, and gathering pelts. However, post-1780, average wages were nearly equal to the total value of the gathered fur.

Figure 3.6: Average Wage Markdown at HBC Posts, 1760-1810

Notes: This figure shows the average wage markdown across HBC posts in the sample. Note that this measure can be greater than one; this indicates that the HBC is paying *above* the standard of trade. See Section 3.3 for the logic underlying the markdown measure.

We further investigate the evolution of the wage markdown over time and the degree of heterogeneity in markdowns across posts. Panel A of Figure 3.7 displays a scatter plot illustrating wage markdowns for each individual post from the establishment of the HBC up until 1810. In the initial period of operations, the HBC opened only a few posts and paid the Indigenous workers less than the full value of the furs they gathered.[22] For instance, in the years between 1730 and 1750, post factors were paying trappers between 60% and 90%

[22]While the HBC had a limited presence on the territory between 1700 and 1763, its posts were exposed to

of the value of the furs received. However, after 1760, the number of HBC posts started to increase, with an exponential growth following the reorganization of trade in Montreal around 1775. Moreover, the wage markdown paid to natives started to increase considerably around 1770, approaching the full value of pelts by the beginning of the 1780s. Note that the increasing trend in wage markdown leads to decreased volatility in this variable, as all posts started to pay workers close to their full value when competition intensified in the 1780s.

In Panel B of Figure 3.7, we narrow our focus to the period of interest, allowing for a more immediate visualization of wage markdown patterns within and across trading posts between 1760 and 1810. In the immediate years following the cessation of the rivalry with the French, the few open posts offered wage markdowns ranging from 50% to 80% of the value of the gathered furs. However, by 1780, the compensation received by Indigenous individuals approached parity with the value of the pelts they collected. The figure allows to visualize both the within-post variation over time, with post increasing the wage markdowns by 30 percentage points over 10 years, and the across-posts variation, even in the later period of enhanced competition. We exploit these two sources of variation in the empirical analysis.

3.4.4 Panel Specification: Spatial Competition and Markdowns

We investigate how the markdowns offered by the HBC to Indigenous traders correlate with the distance to the closer competitor. Given the greater ability of Indigenous trappers to switch their business between firms in more contested regions, the Hotelling model of competition would predict that wages offered as a share of workers' productivity would decrease with the distance to the nearest competitor.

With this in mind, we estimate the following equation:

$$Y_{it} = \beta_0 + \beta_1 MINDIST_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{3.1}$$

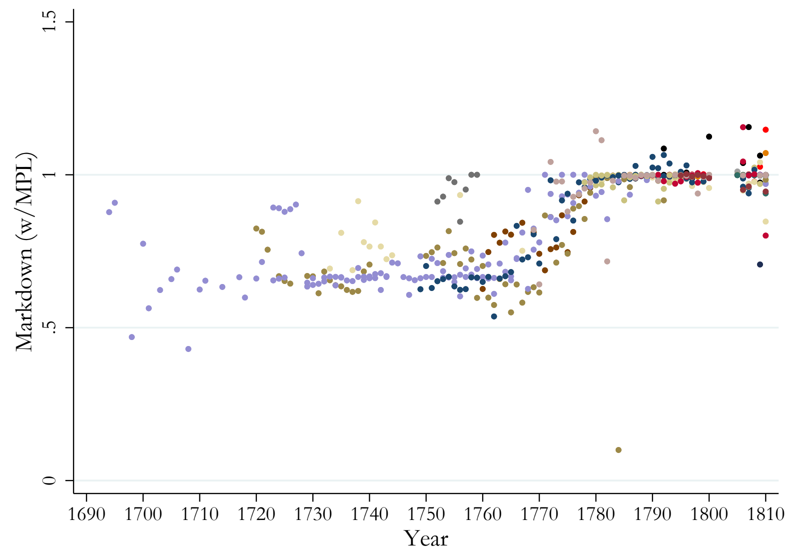
where i denotes the post, t the year, Y the outcome of interest, and $MINDIST$ the distance in kilometers to the nearest opposition post. We also estimate specifications where we focus on the within-location comparison by including province[23] or post (α_i) fixed effect, and we alleviate concerns of time-varying confounds by adding decade fixed (α_t) effects. Across all specifications, standard errors are clustered at the post level.

different degrees of competition by the French. This is documented in Carlos and Lewis (1993), which show that fur prices were different in levels and growth depending on the proximity with the French competitors.

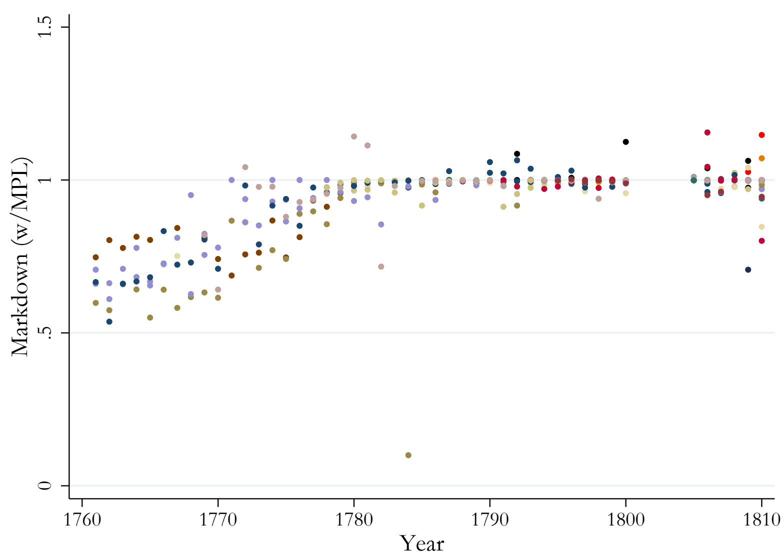
[23]We use provinces as more aggregate geographical units, even though they have been established later than our period of interest.

Figure 3.7: Scatter Plots of Wage Markdown by HBC Post, 1690-1810

Panel A: Post-Level Markdown, 1690-1810



Panel B: Post-Level Markdown, 1760-1810



Notes: Panel A of this figure shows a scatter plot of the post-level wage markdown over time between 1690 and 1810. Each dot represents a post-year observation, with individual posts coded by color. Panel B zooms in on the period of our analysis (1760 to 1810), showing that on top of the increasing trend in markdowns, Indigenous workers are paid close to the value of the fur they provide after 1780.

Table 3.1 presents the results of estimating equation 3.1. Column 1 shows the basic OLS result without controls. The relationship between distance and wage markdown is negative and highly significant. An increase of 100 kilometers in minimum distance lowers the markdown paid out to Indigenous traders by 2.8 percentage points. The coefficient is of the same sign, significance, and magnitude when adding province fixed effects (column 2) or post fixed effects (column 3). In columns 4 and 5, where we add decade fixed effects to the specifications of columns 2 and 3 respectively, the relationship weakens somewhat but remains negative and significant at the 1% level.

We inspect the relationship graphically in Figure 3.8. Panel 3.8a shows the strong linear relationship in a binscatters of the baseline specification in column 1 of Table 3.1. The relationship is similar to our preferred specification in column 5 of Table 3.1, as seen in Panel 3.8b.

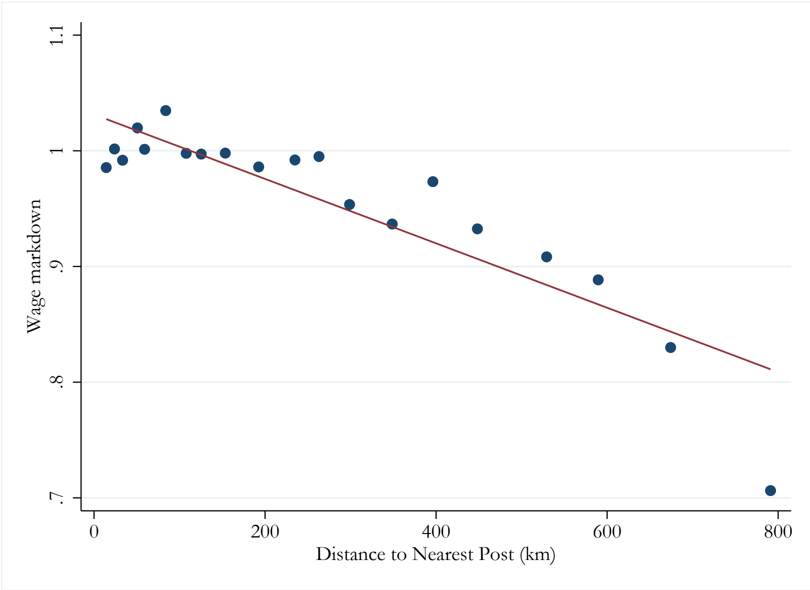
Table 3.1: Regression Estimates of Spatial Competition on Wage Markdown

	(1) w/MPL	(2) w/MPL	(3) w/MPL	(4) w/MPL	(5) w/MPL
MinDistance (100km)	-0.028***	-0.029***	-0.033***	-0.013***	-0.015***
	(0.004)	(0.003)	(0.008)	(0.005)	(0.005)
Province FE	No	Yes	No	Yes	No
Post FE	No	No	Yes	No	Yes
Decade FE	No	No	No	Yes	Yes
Observations	405	405	405	405	405
Posts	38	38	38	38	38

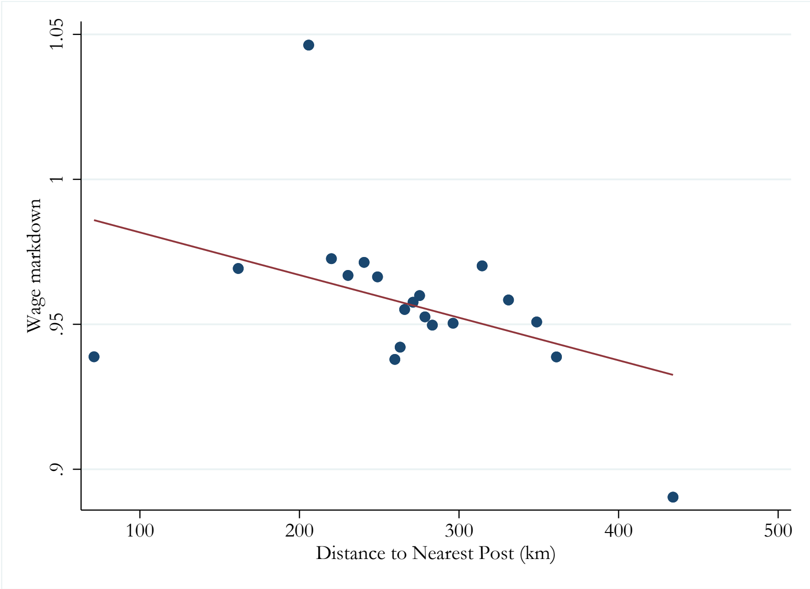
Notes: This table shows the relationship between the distance to the nearest opposition post at the post level and the wage markdown. Column 1 reports the effect of the markdown without controls. Columns 2 and 3 add province and post fixed effects alternately. Columns 4 and 5 add decade fixed effects to Columns 2 and 3 respectively.

Figure 3.8: Binned Scatterplot of Relationship between Distance and Markdown

(a) Baseline Specification



(b) With Post and Decade Fixed Effects



Notes: This figure presents binned scatter plots of the relationship between distance to the nearest post and wage markdown at the post level with (Figure 3.8b) and without (Figure 3.8a) the full battery of controls.

3.4.5 Robustness: Spatial Competition and Markdown

To interpret the relationship between minimum distance and markdowns as a causal effect of competition on wages, we would have to assume that there are no confounding characteristics correlated with both variables. One leading example of such a threat to identification could be a depletion of local beaver populations caused by the heightened competition between the two companies. Some historians have argued that depletion weakened the bargaining positions of Canada's Indigenous peoples with the Europeans.

Table 3.2: Regression Estimates, Controlling for Years of Operation

	(1) Price	(2) Price	(3) Price	(4) Price	(5) Price
MinDistance (100km)	-0.028***	-0.030***	-0.016***	-0.010*	-0.012***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Years since post opening	0.000	0.000	0.003**	-0.000	0.004**
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Province FE	No	Yes	No	Yes	No
Post FE	No	No	Yes	No	Yes
Decade FE	No	No	No	Yes	Yes
Observations	408	408	408	408	408
Posts	38	38	38ù	38	38

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows the relationship between the distance to the nearest opposition post at the post level and the wage markdown, controlling for the age of the post. Column 1 reports the effect of the markdown without controls. Columns 2 and 3 add province and post fixed effects alternately. Columns 4 and 5 add decade fixed effects to Columns 2 and 3 respectively.

Ideally, we would directly control for a time-varying measure of local beaver populations. Unfortunately, such data does not exist. Instead, we can control for how many years a post has already operated in a location as a proxy for the local history of resource exploitation. It is likely that posts that have operated for a longer period, even in suitable areas, might have caused over-harvesting of pelts and animal depletion. In Table 3.2 we repeat the exercise of

Table 3.1 but explicitly control for the number of years a post has already been active. The relationship between spatial competition and wage markdowns remains virtually unchanged. The results also confirm that wages for Indigenous traders decrease over time as a post operates longer in the same location.

Finally, one might worry that the HBC posts were not only responding to the NWC's competitive pressure, but were also suffering from some intra-brand competition. The proliferation of HBC posts might have indeed led the HBC post factors to compete with each other for Indigenous labor and to offer higher markdowns. We test for this hypothesis by running the same regression as in equation 3.1, but our main regressor is now the minimum distance to another HBC post, rather than a competitor's post. Results are reported in Table 3.3. Across all specifications, we find no evidence of intra-brand competition, as wage markdowns are not impacted in a statistically significant way when another HBC post moves closer.

Table 3.3: Regression Estimates of Cannibalization on Wage Markdown

	(1) Price	(2) Price	(3) Price	(4) Price	(5) Price
MinDistance (100km) to HBC	-0.031	-0.031*	-0.035	-0.004	0.005
	(0.021)	(0.018)	(0.041)	(0.004)	(0.016)
Province FE	No	Yes	No	Yes	No
Post FE	No	No	Yes	No	Yes
Decade FE	No	No	No	Yes	Yes
Observations	404	404	404	404	404
Posts	38	38	38	38	38

Notes: This table shows the relationship between the distance to the nearest *HBC* post at the post level and the wage markdown. Column 1 reports the effect on the markdown without controls. Columns 2 and 3 add province and post fixed effects alternately. Columns 4 and 5 add decade fixed effects to Columns 2 and 3 respectively.

3.4.6 Discussion and Next Steps

The results are in line with the predictions of a simple model of spatial competition as outlined in section 3.4.1. Increasing encroachment on HBC posts by its competitor forced the

HBC to pay higher wages and decreased the distance between trading posts and Indigenous settlements. The timing of wage increases and the expansion of the HBC post network coincides with the rise of the NWC. Markdowns rose steeply with the advent of free trade and the NWC's challenge to the HBC monopsony position.

A few remaining challenges prevent us from interpreting the panel estimate of Equation 3.1 as the causal effect of spatial competition on Indigenous wages. The current design is unable to account fully for strategic decisions on post locations made by the NWC. In particular, the decision to open a new trading post in an area could depend on latent characteristics of that area, which in turn are correlated with the wages of Indigenous trappers. Such confounders would bias our estimates of spatial competition on wage markdowns. Our preferred specification already controls for time-invariant characteristics of an area by including post fixed effects. This means that we should not be worried if wages and NWC locations are affected by such factors as the beaver suitability of an area or by the distance of a post to the headquarters of the two companies.

There could also be time-varying characteristics of areas that make them more or less attractive locations for a new trading post. Those would bias our estimates if they are correlated with wage markdowns. An example for such a potential confounder could be local trends in the supply and hunting of beaver or other animals. The exercise in the previous section suggests that our results are unlikely to be biased by this factor in particular. But other phenomena of a similar flavor like local productivity shocks or price shocks to the furs of particular regions could still pose threats.

To overcome these threats to causal identification, in the next interation of this paper, we are planning to isolate exogenous variation in the spread of the NWC post network. The advantage of this setting is that geography and historical evidence provide us with rich information on the drivers of post selection. The NWC expanded from the previous French network based at Lake Superior following pre-established trading routes, that were reported on early European explorers' maps thanks to the collaboration between the latter and Indigenous people. Besides, European traders exploited portages to connect the waterways involved in the fur trade. These portages were locations where canoes and goods were transported overland to avoid obstacles such as rapids, rocks, and treacherous currents, or to reach the next navigable body of water. Indigenous communities had constructed and maintained these portages for many years before the arrival of European settlers.

Hence, we plan to combine information on established trading routes, geographical features (portages and rivers, share of suitable land for beaver), and flexible time-trends to predict the selection of NWC posts. Explicitly modelling the post selection decision will allow us to identify the causal effect of spatial competition on wage markdowns.

3.5 Conclusion

We study the effects of a competitor's entry on the wage markdown imposed by a near-monopsonist. We exploit the unique case study of the fur trade in Canada, where the Hudson's Bay Company's dominance during the late eighteenth and early nineteenth centuries gave it significant influence over the geographical distribution and material welfare of Indigenous peoples, for whom the Company was in many places the sole provider of the European goods.

We compiled new data from the HBC's archives on the post-level quantities, the value of fur gathered by Indigenous trappers, and the value of manufactured and luxury goods they received in exchange for their labor. Preliminary analysis of the data suggests that a decrease of 100 kilometers in the minimum distance to the nearest competitor was associated with a decrease of 1.5 percentage points in the markdowns paid to Indigenous trappers.

These results indicate that competitive pressure improved the outside options available to Indigenous traders, allowing them to threaten to move to rival establishments to obtain higher piece rates. This evidence from the Canadian fur trade demonstrates that differential outcomes for Indigenous populations in colonized regions depend crucially on the extent of the colonizing power's control over production. When Indigenous people can withhold their output or shift allegiances to other groups, colonial regimes may be forced to negotiate with them for a share of the gains from trade.

This project focuses on labor market outcomes and how these depend on the monopsony power of colonial companies. Our measure of wage markdowns does not purport to measure the totality of colonial extraction or Indigenous welfare. We therefore do not make a definitive statement on the effects of colonialism on Indigenous populations generally or the specific case of the HBC's role in either promoting or hindering the prosperity of Canada's First Nations.

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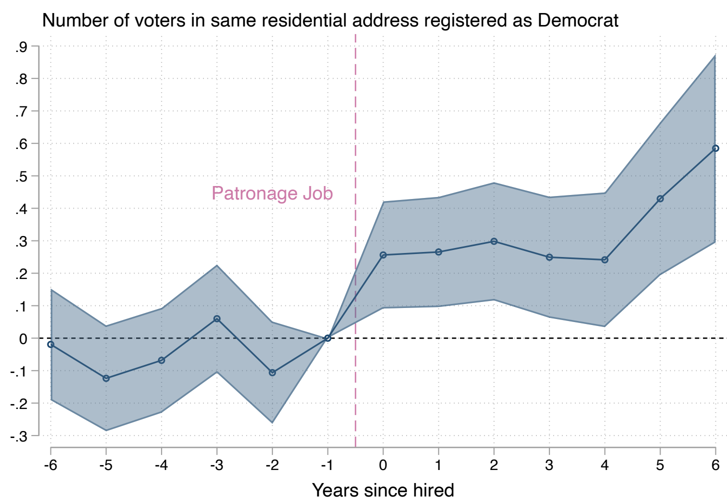
Appendix A

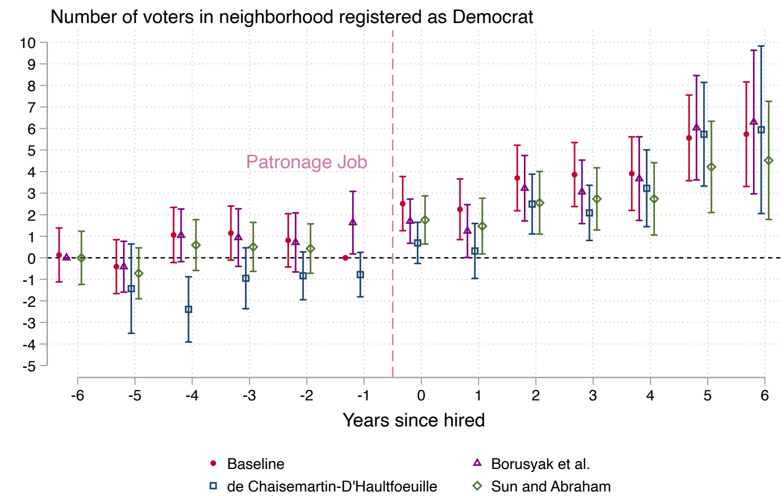
Additional Materials for Chapter 1

Figure A.1: 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 1.4, but with outcomes measured at the exact address instead of in 50 meter neighborhoods.

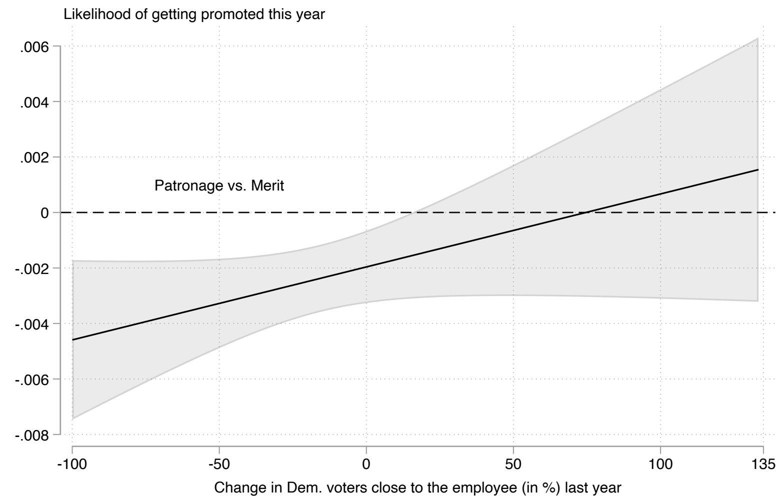
Figure A.2: Comparison of Estimates from Alternative Event Study Models



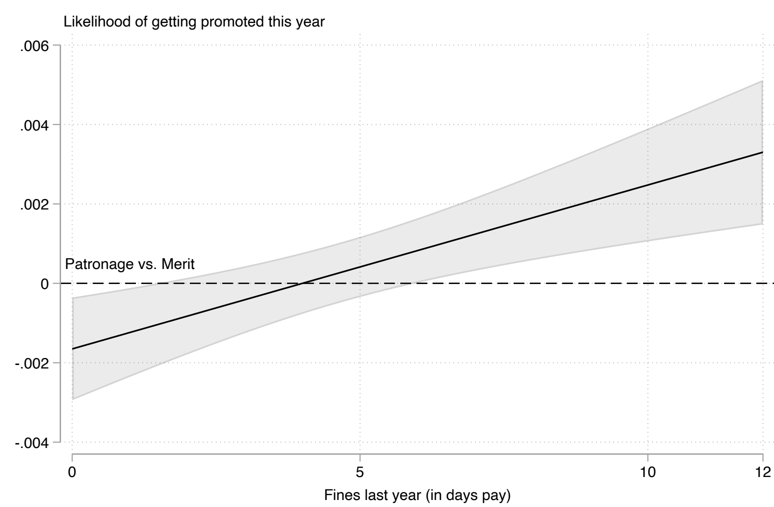
Notes: This figure compares the "Baseline" dynamic treatment effect of patronage on electoral support with 95% confidence intervals (following the specification of Table 1.1, column 3, and as displayed in Figure 1.4) with estimates from alternative models as proposed by Borusyak et al. (2023), de Chaisemartin 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 A.3: Differences in Promotion Chances for Patronage vs. Merit Employees

(a) Promotions and Democratic Registration, Marginal Effects

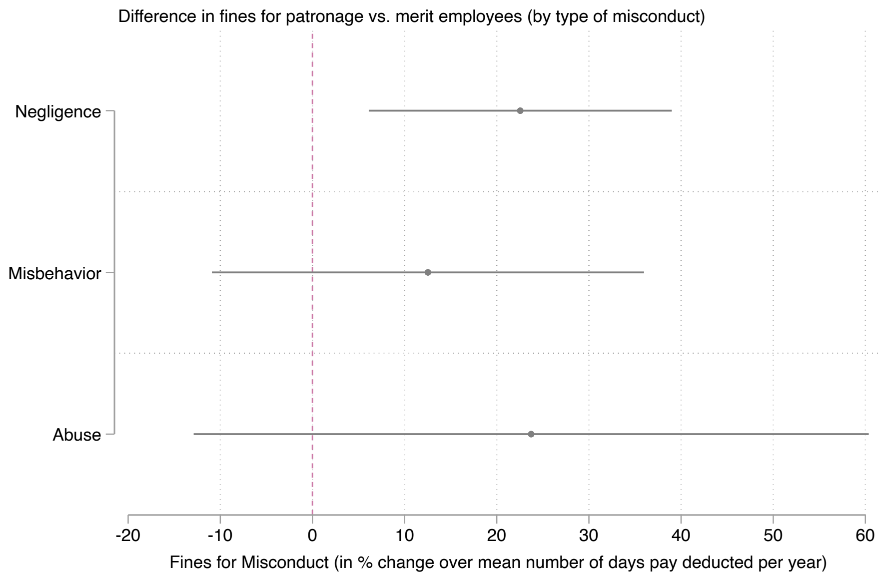


(b) Promotions and Performance, Marginal Effects



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 1.9, but with last year's electoral support and performance (instead of in the same year as the promotion). See the note to Figure 1.8 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 A.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 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 1.2, 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 A.1: 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 1.1) and following the specification of Table 1.1, 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 1.1 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 A.2: 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 1.1) and following the specification of Table 1.1, column 3. Column 1 replicates Table 1.1, 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 1.1 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 A.3: 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 1.6 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 A.4: 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 1.5. 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.