Fathers in Offices, Sons in Jobs: Intergenerational Returns to Local Political Office

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Abstract

This paper studies the intergenerational returns to holding local political office. Using newly digitized rosters of township, village, and city officials in Ohio between 1912 and 1929, linked to full-count U.S. censuses, I examine how fathers' officeholding affects their children's labor market outcomes. Applying a matched difference-in-differences design to children who had reached working age before their fathers' office entry, I find that fathers' officeholding increases sons' occupational income and their likelihood of entering white-collar occupations, with effects persisting for decades. Using wage income data in the 1940 Census, sons of officials earn approximately 13 percent more than their matched counterparts. The effect is limited for daughters' labor force participation and marital outcomes. I present evidence that these intergenerational gains operate through both discretionary hiring in the public sector and improved access to private-sector white-collar jobs. Finally, I provide suggestive evidence that localities with greater exposure to intergenerational political rents subsequently exhibit higher inequality and lower intergenerational mobility among non-politician families.

JEL Classifications: D73, J45, J62, N42

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1 Introduction

Can holding political office create advantages that persist across generations? The prominence of political dynasties suggests that the benefits of office may extend beyond officeholders themselves to their families (Dal Bó et al., 2009; Rossi, 2017). Yet such dynasties are most visible among high-profile politicians, making it unclear whether intergenerational political advantage is confined to prominent families or also arises in the much broader world of lower-level local officeholding. This question matters because the vast majority of political offices are local, and local officials often influence resources within their communities. If intergenerational advantages arise at the local level, then political office may shape not only who governs, but also how local opportunities are allocated, with broader consequences for state capacity and economic development (Aneja and Xu, 2024; Besley and Persson, 2009).

Whether such intergenerational advantages arise in local offices is not obvious. On the one hand, local offices typically have low entry barriers, modest compensation, and limited authority, suggesting little scope for generating intergenerational advantage. On the other hand, these positions are embedded in dense local networks that may facilitate coordination, discretionary allocation, and the exchange of favors, the channels through which private returns to office have been shown to operate (Colonnelli et al., 2020; Fisman, 2001). This paper explores: what are the intergenerational returns to holding local political office, and how do these returns shape broader economic outcomes?

I explore these questions by studying local officeholding in early twentieth-century Ohio in the United States. Ohio provides a particularly suitable setting: the state contains thousands of township, village, and city governments operating under different institutional formats, and offers rich archival records of local officeholders during this period. I construct a new dataset of township, village, and city officials in Ohio between 1912 and 1929 using newly digitized records from the *Ohio Rosters of Township and Municipal Officers*, covering approximately 180,000 year-office observations across more than 2,000 local governments. I link these officials in rosters to the full-count U.S. censuses and use the Census Tree Project crosswalks developed by Buckles et al. (2025) to recover family links and children's outcomes from 1900 to 1940. In addition, many municipal governments during this period began to adopt civil service systems designed to curb discretionary hiring in the public sector (Kuipers and Sahn, 2023; Anzia and Trounstine, 2025). Variation in the timing of these reforms provides institutional leverage for

examining the mechanisms through which local officeholding may affect officials' children.

As a background, I first document who became local officials in this period. Compared with the general population, local officials were predominantly male and less likely to be foreign-born, and they had higher occupational income prior to entering office. However, they were not drawn from the economic elite. Both officials' own pre-office occupations and their fathers' occupational ranks place them largely in the middle of the local income distribution. Especially in villages and townships, officials' economic backgrounds closely resemble those of the general working-age male population, suggesting that holding local office was broadly accessible to middle-class individuals rather than an exclusive privilege of the wealthy. This institutional context makes local officeholding an interesting setting for studying how even modest political power can generate intergenerational advantages.

To identify the impact of holding office on children's labor market outcomes, I use a matched difference-in-differences design comparing children of officials to observationally similar children without an official father in the censuses before and after fathers' office entry. Matching is conducted on detailed pre-office characteristics of both children and fathers, including age, gender, birthplace, county of residency, literacy, and occupational income score. I also show balance on other non-targeted variables, including mothers' characteristics and children's family characteristics. The analysis focuses on children who had reached working age by the first pre-office census. To address concerns of positive selection into office that officials might come from families with higher ambition or better networks, which could also improve their children's labor market outcomes, I implement two alternative designs. The first exploits narrow differences in the timing of office entry by comparing changes in outcomes for children whose fathers had just entered office to those whose fathers would enter office shortly thereafter. The second design compares children of winning and losing candidates.

I find that fathers' officeholding increases sons' occupational income score by 2 percent and raises their likelihood of entering white-collar occupations, with effects persisting for decades. Using earning information from the 1940 Census, I estimate that the sons of officials earn approximately 13 percent more than their matched counterparts. This effect is economically meaningful, with a magnitude comparable to the return to an additional year of education (around 9%) documented in the literature (Card, 1999; Psacharopoulos and Patrinos, 2018). The positive effect remains when I compare sons of elected politicians and not-yet-elected politicians, and when I compare sons of winning and losing candidates. For daughters, I do not find effects on their labor force participation or on marital outcomes,

including whether they marry husbands with better occupations or from wealthier families. Among daughters who participate in the labor force, there is some suggestive evidence of gains in occupational income after fathers' office, although the estimates are imprecise.[1] Taken together, the absence of effects on marital outcomes suggests access to jobs as the main channel through which local political office generated intergenerational returns.

One key mechanism through which fathers' officeholding benefits sons is increased access to public-sector employment. I provide evidence consistent with the explanation of discretionary hiring. First, the increase in public-sector employment for sons of politicians is concentrated in local governments, jobs directly within the discretion of local officials, rather than in state or federal government. Second, exploiting institutional variation in hiring discretion generated by civil service adoption, I find that the effect is strongest in township and village governments, where civil service adoption was largely absent, and substantially smaller in cities, where civil service rules were more prevalent. Among cities, the intergenerational political advantage is substantially attenuated in cities that have adopted civil service reforms. Taken together, these patterns suggest that discretionary hiring plays an important role in explaining the observed increase in public-sector employment among officials' sons. Moreover, the increase in public-sector employment is primarily driven by sons with lower pre-office occupational income scores, indicating negative selection into the public sector. These patterns suggest that discretionary hiring in the public sector plays an important role in transmitting political advantage across generations.

Fathers' officeholding also improves sons' access to private-sector opportunities. Sons of officials are more likely to enter white-collar private-sector occupations, particularly for officials in city governments, where there are more private-sector job opportunities. The effect is concentrated among sons of officials holding executive positions and in industries such as wholesale and retail trade, finance, insurance, and real estate, which are sectors subject to local regulation through licensing, zoning, and financial oversight. This pattern is consistent with private firms hiring officials' children to cultivate political connections or exchange favors.

Beyond the effect on labor market outcomes of officials' sons, I provide suggestive evidence that this intergenerational political advantage has broader economic consequences. In particular, townships with larger exposure to such intergenerational

[1]The pattern is consistent with the relatively low female labor force participation rate in the period, which was 22.44 % in Ohio in 1920. The low female labor force participation rate suggest that fathers' officeholding might have limited impact through job-based channel for daughters.

rents, measured by the share of public workers who are politicians' sons during the sample period, exhibit higher inequality and lower intergenerational mobility for non-politician families in the 1940 census. These patterns suggest that political rents may distort opportunity allocation beyond the politicians' own family.

This paper contributes to three strands of literature. First, it contributes to the literature on the private returns to political office. A growing body of work shows that holding office can increase politicians' wealth and income (Fisman et al., 2014; Eggers and Hainmueller, 2009). Beyond officeholders themselves, political connections may also improve the labor market outcomes of their family members, as documented across several countries using shared family names to proxy for political ties (Gagliarducci and Manacorda, 2020; Fafchamps and Labonne, 2017). Yet less is known about whether such advantages persist beyond an officeholder's tenure and shape the outcomes of politicians' children. The closest work on intergenerational political returns is Folke et al. (2017), who study municipalities in Sweden, a setting characterized by low corruption, and find that children of newly elected mayors experience earnings gains not attributable to nepotistic job allocation. Riaño (2021) studies bureaucratic nepotism in Colombia and finds that bureaucratic promotions benefit distant relatives but not close kin, who face HR audits. Together, these papers suggest that institutional context, particularly the strength of hiring constraints, plays an important role in whether and how political advantage passes across generations. This paper contributes to this literature by studying early twentieth-century local governments in the United States, where patronage networks remained prevalent and staggered civil service reforms generate variation in discretionary hiring power. This setting allows me to identify the channels through which intergenerational political advantages operate and to trace their effects over decades using linked full-count Census data.

This paper also speaks to the literature on patronage and nepotism. Existing literature shows that politicians may allocate resources on the basis of political loyalty and personal connections, with public employment as one important margin (Cruz et al., 2017; Leucht, 2023; Colonnelli et al., 2020). In particular, Colonnelli et al. (2020) show that political connection shapes who gets hired in the public sector and the bureaucratic quality. Such practices can weaken state capacity and affect economic development (Xu, 2018; Aneja and Xu, 2024). This paper extends this literature in two directions: it shows that discretionary hiring in the public sector is an important channel of intergenerational political advantages, and it links township-level exposure to public-sector intergenerational advantages to higher inequality and lower mobility among non-politician families.

Finally, this paper adds to the literature on intergenerational mobility and the transmission of advantages. A large body of work documents substantial intergenerational persistence in human capital, income, and occupation (Barrios Fernández et al., 2024; Dal Bó et al., 2009; Kramarz and Skans, 2014; Long and Ferrie, 2013; Mocetti, 2016). More closely related is work examining how parental access to political power shapes children's opportunities (Dal Bó et al., 2009). This paper adds to this strand by studying the impact of local political officeholding on children's labor market outcomes and by documenting the broader distributional implications of this transmission.

2 Data and Descriptive Statistics

2.1 Data

2.1.1 Ohio Roster of Township and Municipal Officers

My main source of data is *Ohio Rosters of Township and Municipal Officers* (Ohio Secretary of State, 1929) (hereafter, the Rosters). The Rosters are published biannually by the Ohio Secretary of State and provide a full listing of local officials organized by county, locality (township, village, and city), and office. The offices include both elected and appointed officials, such as township trustees, clerks, and treasurers, as well as village mayors, council members, and city mayors, council members, judges, etc, which together oversaw local administration, finance, and public services. Most of these positions were through local elections taking place every two years. Table B1 summarizes these offices by different forms of government. I digitized 9 consecutive rosters covering the years 1912 through 1929.[2] Figure C1, C2, and C3 show the example pages of the Rosters.

In total, my dataset includes 179,952 year-office observations spanning offices in 95 cities, 862 villages, and 1,518 townships.

[2]Volumes were published before 1912 and after 1929, but not in a continuous series. I restrict attention to 1912 - 1929 in order to maintain a consecutive series of rosters.

2.1.2 Linking the Rosters to the Census

To obtain demographic characteristics and intergenerational information for local officials, I link the individuals listed in the Rosters to the full-count U.S. censuses (Ruggles et al., 2024).

I link each official listed in the Rosters to the contemporaneous full-count censuses based on their name and location using an automated procedure that proceeds in two stages. I first attempt to match each official to the closest pre-office census. For example, officials in the 1914 Roster are initially matched to individuals in the 1910 census. A match is defined when there is a unique individual in the census who satisfies three baseline criteria: (1) the same first and last name; (2) residence in the same county as the roster record; and (3) being at least 25 years old at the time of entry into office. If multiple candidates meet these conditions, I incorporate additional criteria: middle name consistency and locality, i.e., township or municipality, of residence in the census. A match is defined if there is a unique individual record in the census that satisfies the criteria (1) $-$ (3) and (4) that there is no conflict in the middle name; or satisfies the criteria (1) $-$ (4) and (5) that the individual resides in the same town or municipality as the office locality recorded in the Roster.

If no match is found in the closest pre-office census, I then turn to the closest post-office census. For instance, an official in the 1914 Roster who cannot be linked in the 1910 census will then be linked to potential records in the 1920 census. The same sequence of matching criteria and tie-breaking rules is applied until either a unique match is found or no eligible candidate remains.

Using this procedure, I successfully link approximately 70% of the office-year-office records in the Rosters to individuals in the full-count censuses.[3] Between 1912 and 1928, I matched 57,936 unique officials in the Rosters. Table B2 reports the linkage rates in detail, disaggregated by roster year and form of government.

2.1.3 Collecting Information on Winning and Losing Candidates

To complement the study, I construct a separate dataset of local election candidates. Starting from the names of officials listed in the Rosters, I search historical newspapers

[3]This linkage rate is higher than what is commonly achieved in historical census linkage, reflecting the detailed name and location information available for local officials. The rate is comparable to that reported by Abramitzky et al. (2024), who link university faculty rosters to the full-count censuses.

around election dates for coverage of local elections. Figure A1 provides an example of such election coverage. Whenever election coverage is available, I digitize candidates' names, offices, locality, and votes. This procedure allows me to get information on losing candidates, who are not recorded in the Rosters.

The resulting dataset covers around 290 races, mostly for city governments. Although this sample is smaller than the Rosters-based sample, it provides a useful complementary source of variation for comparing winning and losing candidates.

2.2 Descriptive: Characteristics of Local Officials

Having linked local officials in the Rosters to the full-count censuses, I obtain their demographic characteristics and pre-office economic backgrounds. Table 1 reports summary statistics for local officials (Panel A) and compares them to the general population of Ohio from the 1920 Census (Panel B). Relative to the population, local officials are overwhelmingly male and less likely to be foreign-born. They also have higher occupational income scores prior to entering office, indicating positive selection into local political positions. The average official enters office approximately at the age of 47, suggesting that local officeholding typically occurs later in the working life rather than at labor market entry.

Figure 1 provides a more detailed view of officials' economic backgrounds by plotting the distribution of pre-office occupational income scores and family background separately for city, village, and township governments. Panels A, C, and E compare officials' pre-office occupational income distributions to those of the working-age male population. Across all three settings, officials tend to come from occupations with higher income scores than the average population, though they are not concentrated in the extreme upper tail of the distribution. This pattern is most pronounced in cities, where officials' occupational distribution is clearly shifted to the right relative to the general population. In contrast, township officials are concentrated in the lower and middle portions of the occupational distribution, with substantial overlap with the average male population and only a limited presence in the upper tail. Village officials fall between these two cases, exhibiting modest positive selection relative to the population.

Panels B, D, and F examine officials' family backgrounds, measured by the percentile rank of their fathers' occupational income scores within their fathers' cohort when these officials were young. City officials tend to come from more advantaged families, although rarely from the very top of the distribution. Village and township officials, by contrast, are

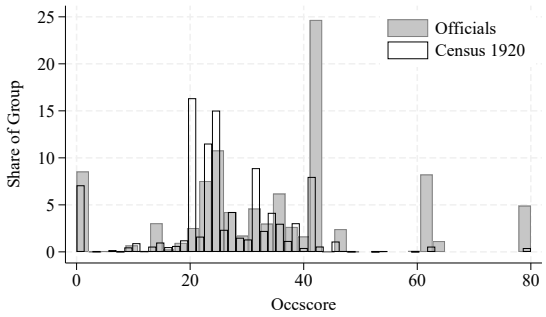
Table 1. Baseline Characteristics

	All	*City*	*Village*	*Township*
	Mean	Mean	Mean	Mean
Panel A: Local officials				
Male	0.936	0.926	0.925	0.950
White	0.985	0.984	0.982	0.988
Age first in office	46.702	46.937	45.792	47.597
Foreign born	0.034	0.055	0.036	0.025
Immigrant parents	0.213	0.290	0.212	0.193
Literate	0.984	0.986	0.981	0.987
Pre-office occscore	21.569	31.757	23.664	16.507
Pre-office public sector	0.022	0.069	0.021	0.010
Obs	57,936	6,634	26,288	25,014
Panel B: Population (Census 1920)				
Male	0.513	0.511	0.493	0.524
White	0.967	0.957	0.978	0.987
Age	29.224	28.418	31.686	28.927
Foreign born	0.119	0.194	0.050	0.051
Immigrant parents	0.332	0.488	0.187	0.177
Literate	0.971	0.963	0.980	0.976
Occscore	13.803	15.820	13.555	9.384
Public sector	0.007	0.010	0.007	0.003
Obs	5,763,198	1,764,029	505,897	1,452,462

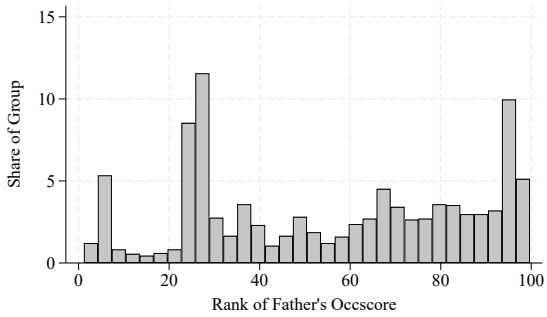
Note: For pre-office occ score, politicians whose in-sample start-year is 1912 are dropped from the sample as they might already be politicians/officials in 1910 which is pre-office census. In the Columns of the Census 1920, Year of education and occ score are calculated in population between age 25 and 65.

substantially more likely to come from families with modest occupational backgrounds. Taken together, these patterns indicate that while local officials were positively selected relative to the general population, especially in cities, township and village offices were broadly accessible to individuals from middle-class backgrounds rather than being dominated by economic elites.

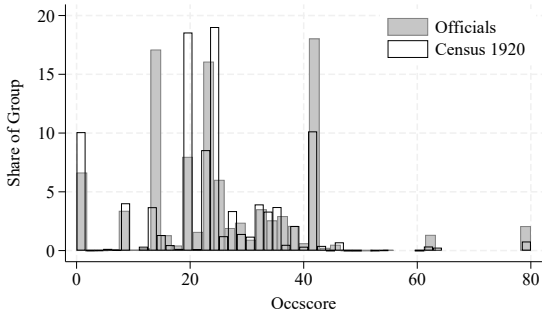
In the Appendix, Figure C4 presents a breakdown of officials' characteristics by year and form of local government, showing that the patterns of pre-office occupational income score are relatively stable over time.



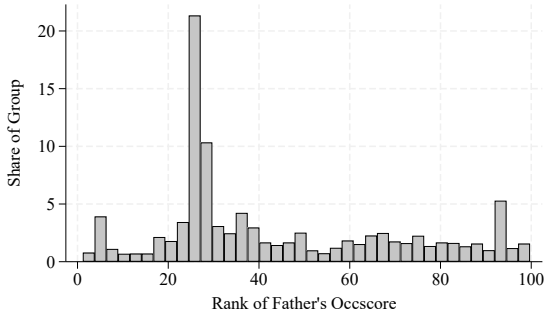
A. Occscore: City



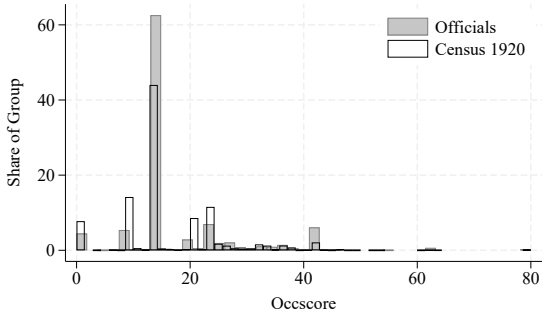
B. Family Background: City



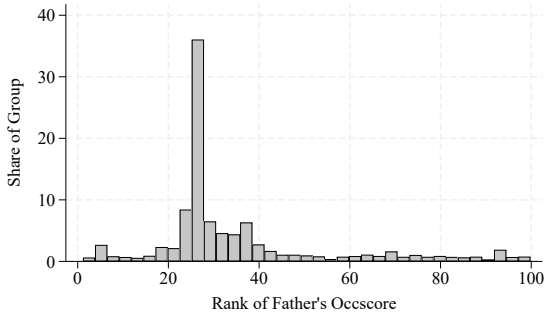
C. Occscore: Village



D. Family Background: Village



E. Occscore: Township



F. Family Background: Township

Figure 1. Distribution of Officials' Pre-office Occupation and Family Background

Note: This figure depicts the distribution of officials' occupational income score (Panels A, C, E) and their family background, measured by the percentile rank of fathers' occupational income scores (Panels B, D, F), across city, village, and township government. The sample consists of only men as the labor force participation for women was low during this period. In Panels A, C, and E, the dark bars represent local officials, while the light bars represent the 1920 Census population ages 25-65. Panel B, D, and F show the distribution of officials' family background measured by their father's occupational income rank among their father's cohort.

2.3 Linking Officials to Their Family

To get the intergenerational information for local officials, I link officials to their children using census-to-census crosswalks from the Census Tree Project (CTP) (Buckles et al., 2025).

I first trace each official across earlier census waves to periods when their children are likely to reside in the same household. This allows me to identify children directly from the household and recover child identifiers. After identifying children, I use the CTP crosswalks to follow them forward across census waves and construct their labor market outcomes in the full-count censuses from 1900 through 1940. This procedure enables me to observe children's occupational trajectories over much of their working lives and to link fathers' officeholding to children's long-run labor market outcomes.

2.4 Outcome Variables

The main outcomes of interest are labor market outcomes, specifically earnings and occupation. Because the full-count censuses did not collect individual earnings information prior to 1940, I proxy for earnings using the occupational income score (OCCSCORE). OCCSCORE is constructed by assigning to each occupation the median income observed in the 1950 Census. The OCCSCORE measure has two limitations: it does not capture within-occupation variation in individual earnings, and it does not reflect changes in the relative earnings of occupations over time. The latter limitation is difficult to address given the absence of individual earnings data for the pre-1940 period. To partially address the former limitation, I use a complementary measure, the Lasso-Industry-Demographic-Occupation (LIDO) scores developed by Saavedra and Twinam (2020), which adjusts occupational earnings by incorporating industry and demographic composition. It is important to note that both the OCCSCORE and LIDO scores provide only occupation-based proxies for labor market outcomes rather than direct measures of earnings.

3 Research Design

As local officials differ systematically from the general population, a simple comparison between children of local officials and children of non-local officials would yield biased estimates of the intergenerational effects of officeholding. To address this concern, I employ a matched difference-in-differences design that compares changes in outcomes for children whose fathers enter local office to those of observationally similar children whose fathers do not.

3.1 Matching

My sample consists of individuals who had already entered the labor force in the census year prior to their fathers' entry into office. I restrict the sample to the children of officials who began officeholding between 1914 and 1928.[4]

I implement nearest-neighbor matching to pair each child of officials (treated) with a comparable individual whose father never held local office (control). Matching is conducted at the closest pre-office census ($t = -1$). For each treated individual, I select control individuals of the same gender and race, residing in the same county, born in the same state/county, having the same immigration family background, same literacy status, and having the smallest Mahalanobis distance based on age, pre-office occupational income score, father's age, and father's pre-office occupational income score.

Table 2 reports balance tests for both targeted and non-targeted covariates in the matched sample. Children of officials and their matched controls are highly similar along observed demographic and family background characteristics. While there is a statistically significant difference in maternal literacy between the two groups, the magnitude of this difference is very small relative to the sample mean.

3.2 Econometric Specification

To estimate the intergenerational impact of fathers' officeholding, I estimate the following difference-in-differences specification using the matched sample:

$$y_{ijbct} = \alpha_i + \beta_1(\text{Office}_{j(i)} \times \text{Post}_{it}) + \beta_2 \text{Post}_{it} + \theta_{bt} + \mu_{ct} + \varepsilon_{ipbct} \tag{1}$$

where i denotes individual, b age bins defined in five-year intervals, c county, and t census year. y_{ijbct} is the outcome of interest for individual i in year t. $\text{Office}_{j(i)}$ is an indicator equal to one if individual i's father j ever holds local office. Post_{it} equals zero if i's father j hasn't started the term in census year t and one otherwise. For matched controls, Post_{it} is assigned based on the timing of their matched treated counterpart. The specification includes individual fixed effects α_i, age-bin-by-year fixed effects θ_{bt},[5] and county-by-year fixed effectμ_{ct}. The regression is weighted by the matching weights to account for cases in

[4]Because the roster data are truncated between 1912 and 1928, I exclude children of officials appearing in the 19121913 Rosters, as their fathers may have entered office before 1912.

[5]The birth cohort fixed effects will be absorbed by the individual fixed effect. Age-bin-by-year fixed effects flexibly control for age profiles, varying within-individual for different census year t.

Table 2. Balance Table for Matched Sample: $t = -1$

	Children of Officials		Matched Individuals		Difference Treated - Control	
	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Obs	Mean	Obs	Diff	SE
Targeted variables						
Male	0.519	21,257	0.520	31,780	-0.000	0.005
White	0.998	21,257	0.998	31,780	-0.000	0.001
Foreignborn	0.002	21,257	0.002	31,780	0.000	0.001
Immigrant Parent	0.070	21,257	0.069	31,780	0.001	0.004
Literate	0.996	21,257	0.996	31,780	0.000	0.001
Age	24.941	21,257	24.901	31,780	0.039	0.072
Occscore	12.178	21,257	12.153	31,780	0.025	0.133
Father Literate	0.992	21,257	0.992	31,780	0.000	0.001
Father Occscore	22.186	21,257	21.978	31,780	0.208	0.162
Father Age	54.270	21,257	54.348	31,780	-0.078	0.101
Non-targeted variables						
Married	0.460	21,257	0.458	31,780	0.002	0.005
Num of Kids	0.656	21,257	0.674	31,780	-0.018	0.013
Mother Literate	0.992	19,893	0.985	29,176	0.007***	0.002
Mother Occscore	0.454	19,893	0.393	29,176	0.060	0.045
Mother Age	50.467	19,893	50.329	29,176	0.138	0.107

Note: This table presents the balance checks for targeted and untargeted variables for children of officials and their matched counterparts.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

which a treated individual is matched to multiple controls. Standard errors are clustered at the father level.

To estimate the dynamic effect, I estimate the following event-study specification:

$$y_{ijbct} = \alpha_i + \sum_{k \neq -1} \beta_1^k (\text{Office}_{j(i)} \times \mathbf{1}\{t = k\}) + \sum_k \beta_2^k \mathbf{1}\{t = k\} + \theta_b + \mu_{ct} + \varepsilon_{ipbct} \qquad (2)$$

where $\mathbf{1}\{t = k\}$ are the event time dummies relative to fathers' office entry, with $k = -1$ corresponding to the first pre-office census and $k = 1$ to the first post-office census. This specification allows me to assess both pre-trends and the dynamic response of children's labor market outcomes following fathers' entry into office.

The key identification assumption is that in the absence of the fathers' entry into local office, the labor market evolution of officials' children should be the same as their matched counterparts. While this assumption is not directly testable,I assess its plausibility by examining pre-treatment trends in the event-study analysis. The parallel pre-trends in the pre-office years (Figure 3) support the validity of the research design. Section 4.2 further discusses potential threats to identification and presents additional evidence addressing these concerns.

4 Main Results

This section examines how fathers' entry into local political office affects their children's labor market outcomes over the life cycle. I begin by documenting raw trends to motivate the analysis, then present causal estimates from the matched difference-in-differences design, and finally interpret the results.

4.1 Intergenerational Effect on Labor Market Outcome

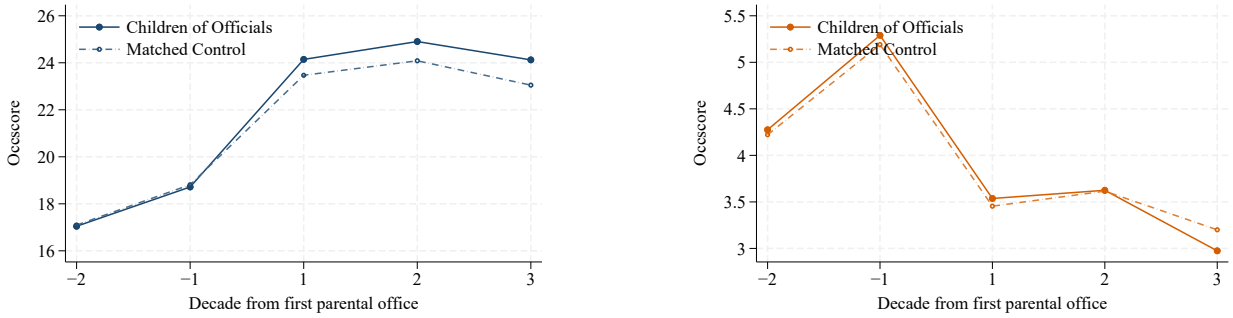
Before turning to the causal analysis of the intergenerational effect on labor market outcomes in Equation 1, I first document raw trends in occupational income scores for officials' children and their matched counterparts. Because female labor force participation was relatively low during the early twentieth century,[6] I present results separately by gender.

Figure 2 plots average occupational income scores for sons and daughters of officials and their matched controls. In the pre-office period, the trajectories for treated and control individuals closely mirror each other for both sons and daughters. Note that although the matching is conducted at $t = -1$, occupational income scores remain balanced at $t = -2$, providing credibility for the quality of the matching. The close alignment of trends prior to fathers' office entry provides credibility for the identification assumption. Following fathers' entry into office, the trajectories diverge for sons. Sons of officials experience a sustained increase in occupational income relative to their matched counterparts. This advantage persists through at least the third post-office census, suggesting that even low-level political officeholding can generate long-lasting effects on children's career trajectories.

[6]The labor force participation rate for working-age (18-64) women is 22.44% and for working-age (18-64) men is 94.62% in Ohio, calculated using the 1920 Census.

In contrast, daughters show no comparable divergence in occupational income following fathers' office entry.

Figure 2. Trend of OCCSCORE in the Matched Sample



A. Sons **B.** Daughters

Note: This figure presents the raw comparison of means of officials' sons' (daughters') occupational income with their matched counterparts.

4.1.1 Effects on Sons' Outcomes

Table 3 examines the impact of fathers' officeholding on sons' labor market outcomes using the matched difference-in-differences specification. Columns (1) to (3) present the effect on sons' occupational income score using different specifications. Column (1) reports the result without controlling for individual fixed effects. The small and insignificant coefficient of Office suggests that before the father enters the office, there is no difference in occupational income score between sons of officials and their matched counterparts. After the office, the occupational income score for sons of officials increases by 2.7 percent compared to their matched counterparts. Column (2) includes individual fixed effects, comparing the change in occupational income score in the same individual before and after the office time. When partialling out the unobservable differences across individuals, fathers' officeholding increases sons' income score by 2.3 percent. Column (3) further controls the county by year fixed effects to allow for flexible labor market dynamics in different counties. The coefficient remains stable that fathers' officeholding increases sons' occupational income score by 2.3 percent.

Because the occupational income score captures only between-occupation differences and not within-occupation earnings variation, I interpret the estimated effects as a lower bound on income gains. The true effect might be larger if fathers' officeholding brings within-occupation benefits, which OCCSCORE cannot capture. To provide suggestive evidence on the magnitude of the income effects, I exploit wage income information

available in the 1940 Census. Column (4) shows that sons of officials earn approximately 13 percent more than their matched counterparts.[7] This magnitude is economically meaningful and comparable to returns from an additional year of schooling (around 9%) documented in the literature (Card, 1999; Psacharopoulos and Patrinos, 2018), suggesting that political officeholding confers substantial labor market advantages.

Columns (5) to (8) explore how these occupational gains arise by examining the types of occupations into which officials' sons sort. I classify occupations into four categories: white-collar, skilled blue-collar, low-skill, and agricultural. Fathers' officeholding significantly increases the probability that sons enter white-collar occupations, which have the highest occupational income score among the four occupation categories. At the same time, sons of officials become less likely to work in skilled blue-collar occupations, with little change in low-skill employment and a modest decline in agricultural work, although the effect is not statistically significant. Taken together, these patterns indicate that the intergenerational returns to local officeholding operate primarily through upward occupational mobility into white-collar positions.

I further explore the dynamic effect of office on sons' occupational income by estimating Equation 2. Figure 3, shows no evidence of differential pre-trends between sons of officials and their matched counterparts prior to fathers' office entry, supporting the identification assumption. Following fathers' office entry, sons' occupational income increases relative to their matched counterparts. This positive effect persists for at least two decades. Figure 4 further shows that the persistent increase in sons' occupational income score following their fathers' office entry is driven primarily by persistent increases in white-collar employment. The dynamic results indicate that fathers' local officeholding shapes sons' long-run occupational trajectories rather than producing short-lived income gains.

4.1.2 Effects on Daughters' Outcomes

I next examine the impact of fathers' officeholding on daughters' outcomes. Given low female labor force participation during this period, rather than focusing solely on occupational outcomes, I consider both labor force participation and marriage-related outcomes, including the characteristics of spouses.

[7] Table E1 further shows that even after conditioning on occupation-fixed effects, sons of officials earn about 4.6 percent more in the same occupation.

Table 3. Intergeneration impact on sons' labor market outcomes

	Log Income Measure$_{it}$				Employment$_{it}$			
	(1) Occ score	(2) Occ score	(3) Occ score	(4) 1940 income	(5) White collar	(6) Skilled blue collar	(7) Low skill	(8) Agricultural
Office$_{j(i)}$ × Post$_{it}$	0.027***	0.023***	0.023***		0.022***	-0.009*	0.001	-0.009
	(0.007)	(0.006)	(0.006)		(0.005)	(0.005)	(0.007)	(0.006)
Office$_{j(i)}$	0.005			0.129***				
	(0.007)			(0.023)				
Mean of Occscore					33.932	30.592	20.544	14.099
Age bin FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE		Y	Y		Y	Y	Y	Y
Pair FE				Y				
County FE	Y	Y		Y				
Year FE	Y	Y						
County × Year FE			Y		Y	Y	Y	Y
Control Mean	22.133	22.133	22.133	1287.695	0.209	0.142	0.385	0.257
R^2	0.160	0.715	0.719	0.615	0.689	0.579	0.550	0.646
Observations	81981	81981	81981	11176	84173	84173	84173	84173

Note: This table reports regression estimates of the effect of political office on sons' labor market outcomes. The unit of observation is at the individual-year level. The dependent variables for Columns (1) to (3) are (log) occupational income score; for Column (4) is log wage income in the 1940 Census; and for (5) to (8) are binary variables denoting being employed in white-collar jobs, skilled blue-collar jobs, low-skill jobs, and agricultural-related jobs. The sample of Columns (1) - (3) and (5) - (8) includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. The sample of Column (4) is collected in the 1940 Census. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post$_{it}$ is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

To study the impact of fathers' officeholding on daughters' labor force participation and occupational outcomes, I estimate Equation 1 using the matched sample. Table 4 reports the results. Column (1) shows no evidence that fathers' officeholding affects daughters' labor force participation. Column (2) examines daughters' occupational income score among those observed in the labor force and shows a positive effect: for daughters who work, fathers' officeholding increases their occupational income score by 3.2 percent. Figure 5 indicates that this effect does not emerge immediately after fathers' office entry. Column (3) turns to their wage income in the 1940 Census and shows a positive but imprecisely estimated effect. In Table E3, I further examine if fathers' officeholding affects daughters' occupation category conditional on labor force participation. There is no effect on daughters working in white-collar, blue-collar, and agricultural jobs.

I then examine whether fathers' political office affects whom their daughters marry. To do so, I link daughters to their husbands in the census and get information on husbands'

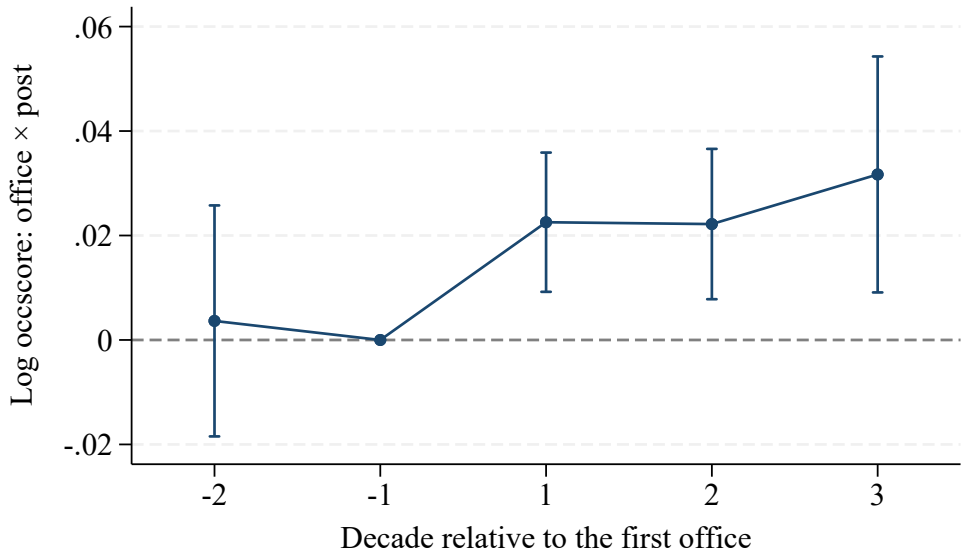


Figure 3. Dynamic Effect of Office on Son's Occupational Income

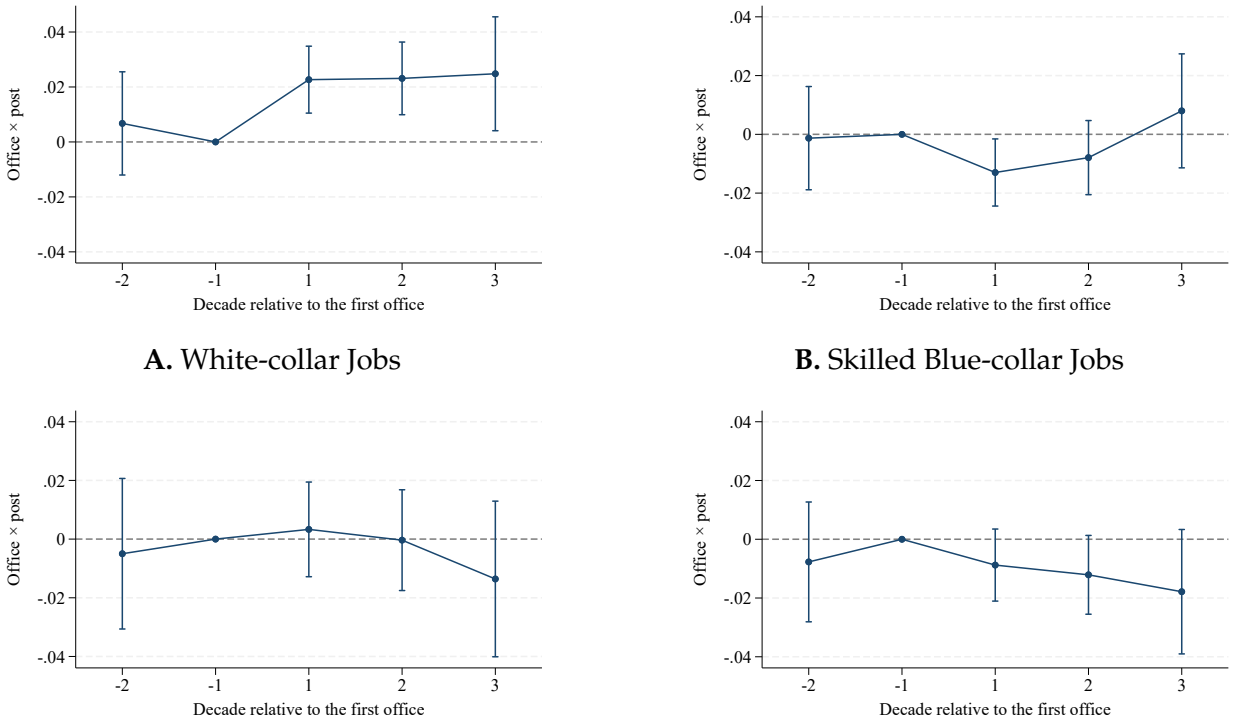
Note: This figure presents the dynamics of sons' occupational income from estimating Equation 2. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

characteristics at the time of marriage. Because spousal characteristics are observed only for married women residing in the same household as their husbands, this analysis is necessarily restricted to matched daughters who are observed as married and residing in the same household as their husbands. Moreover, since each daughter is observed only once, at the census in which she first appears as married, the data structure is cross-sectional rather than panel. For these marital sorting outcomes, I estimate the following equation:

$$y_{ijbcmt} = \alpha_m + \beta_1(\text{Office}_{j(i)} \times \text{Post}_{it}) + \beta_2 \text{Post}_{it} + \theta_b + \mu_{ct} + \varepsilon_{ipbct} \tag{3}$$

where m indexes the matched pairs generated in the matching procedure described in Section 3.1, and α_m denotes the matched pair fixed effects. Standard errors are clustered at the matched-pair level. The outcomes of interest, y_{ijbcmt}, are husbands' characteristics at the time of marriage, including occupation, occupational income score, and family background, proxied by the father-in-law's occupational income score.

Columns (4) to (6) of Table 4 show no evidence that fathers' officeholding affects daughters' marital sorting. Daughters of officials are not more likely to marry white-collar husbands, nor do their husbands have higher occupational income scores or come from



A. White-collar Jobs

B. Skilled Blue-collar Jobs

C. Low-skilled Jobs

D. Agricultural Jobs

Figure 4. Dynamic Effect of Office on Son's Occupation

Note: This figure presents the dynamics of sons' occupation category from estimating Equation 2. Panel A, B, C, and D separately present the dynamic effect of office on the probability of working in white-collar jobs, skilled blue-collar jobs, low-skilled jobs, and agricultural jobs. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

more advantaged family backgrounds. These null results indicate that fathers' political office does not translate into improved marriage-market matches for daughters.

4.2 Validity of the Design

The key identification assumption is that in the absence of the father getting the office, the labor market evolution of officials' children should be the same as their matched counterparts. Although this assumption is not testable, I examine the pre-trends in the event study analysis. The parallel pre-trends in the pre-office years lend credibility to the design's validity.

Below, I discuss some potential concerns for the identification.

Positive selection into politics. A potential concern is that the matched controls may not represent an adequate counterfactual if unobserved characteristics, such as access to

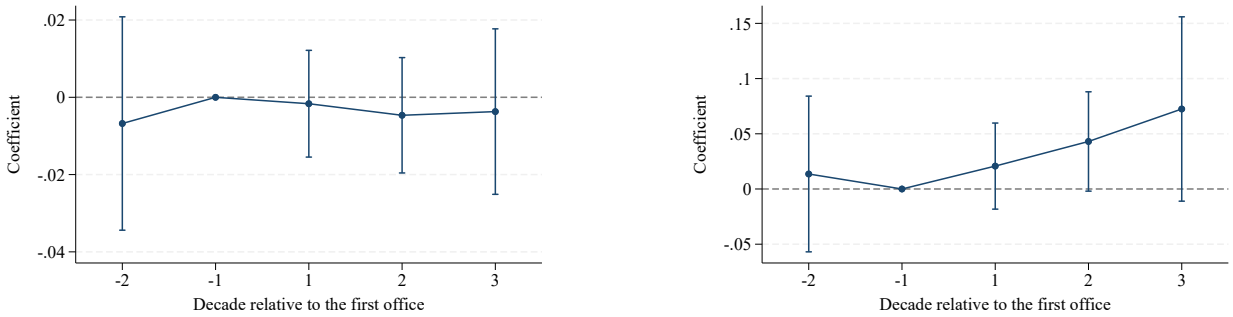
Table 4. Intergeneration impact on daughters' labor market and marital outcomes

	Daughters			Marital: Husband		
	(1) LFP	(2) Log Occscore	(3) Log 1940 Income	(4) White collar	(5) Log Occscore	(6) Log Father Occscore
$\text{Office}_{j(i)} \times \text{Post}_{it}$	-0.002	0.032*		0.017	0.061	-0.078
	(0.006)	(0.019)		(0.064)	(0.055)	(0.056)
$\text{Office}_{j(i)}$			0.076	0.043***	0.006	0.011
			(0.169)	(0.015)	(0.016)	(0.015)
Age bin FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y				
Pair FE			Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y	Y	Y
Control Mean	0.149	22.132	801.755	0.145	20.074	17.515
R^2	0.518	0.781	0.747	0.619	0.670	0.638
Observations	91022	15764	2540	9855	9663	9395

Note: This table reports regression estimates of the effect of political office on daughters' labor market and marital outcomes. The unit of observation in Columns (1) and (2) is at the individual-year level. Column (3) include matched daughters' sample in the 1940 Census. Columns (4) to (6) restrict the sample to the married individuals and are therefore cross-sectional. The dependent variable in Column (1) is a binary variable of labor force participation equal to 1 if the individual is in the labor force and 0 otherwise; in Column (2) is log occupational income score if individual is in the labor force; in Column (3) is log wage income observed in the 1940 Census. Note that Column (1) does not contain observations in the 1900 Census, as it does not record information on labor force participation. Columns (4) to (6) examine characteristics of the husband in the first census where the daughter is observed as married: whether the husband holds a white-collar job, the log occupational income score (occscore) of the husband, and the log occscore of the husband's father when the husband was young. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level for analysis in Columns (1) to (2), and are clustered at the match pair level for analysis in Columns (3) to (6).
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

social networks, both select certain individuals into political office and meanwhile benefit their children's labor market outcomes. In that case, the estimated effect might reflect these unobserved characteristics rather than the causal impact of officeholding.

To address this concern, I implement two complementary designs that use different control groups instead of the baseline matched counterparts. The first design compares children whose fathers had just entered office to children whose fathers would enter office shortly thereafter. Specifically, I exploit narrow differences in the timing of office entry



A. Labor Force Participation

B. Occupational Income

Figure 5. Dynamic Effect of Office on Daughters

Note: This figure presents the dynamics of daughters' labor force participation and occupational income conditional on working from estimating 2. Panel A and B separately present the dynamic effect of office on the labor force participation and log occupational income score. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

among politicians who started office between 1918 and 1920 (or 1922). Because both groups come from families that eventually enter politics, and because the comparison is made over a relatively short window, they are likely to be similar in underlying characteristics such as ambition, social networks, and political engagement. The key difference is that fathers in the 1918 group are already treated by the 1920 Census, whereas fathers in the 1920 (or 1922) group are not yet treated.

Figure 6 presents the event-study plot estimated from this design. The estimates show no evidence of differential pre-trends before fathers' office entry. After office entry, sons of politicians starting office in 1918 experience a relative increase in their occupational income compared to sons of politicians who would enter office in 1920 or 1922, as observed in the 1920 Census. The pattern is consistent with the baseline results, suggesting that the positive effect of fathers' officeholding is unlikely to be driven solely by positive selection into politics.

The second design compares the children of winning and losing candidates. Specifically, I compare the children of the last winner to those of the first losers based on the vote returns.[8] Relative to the baseline matched design, this approach narrows the comparison to families whose fathers were actively competing for offices and are therefore likely to be more similar in unobserved ambition, political engagement, and local standing.

[8]The ideal design would be a regression discontinuity design. However, because election returns are available for only a limited sample of races recovered from historical newspapers, the sample is too small to implement an RDD.

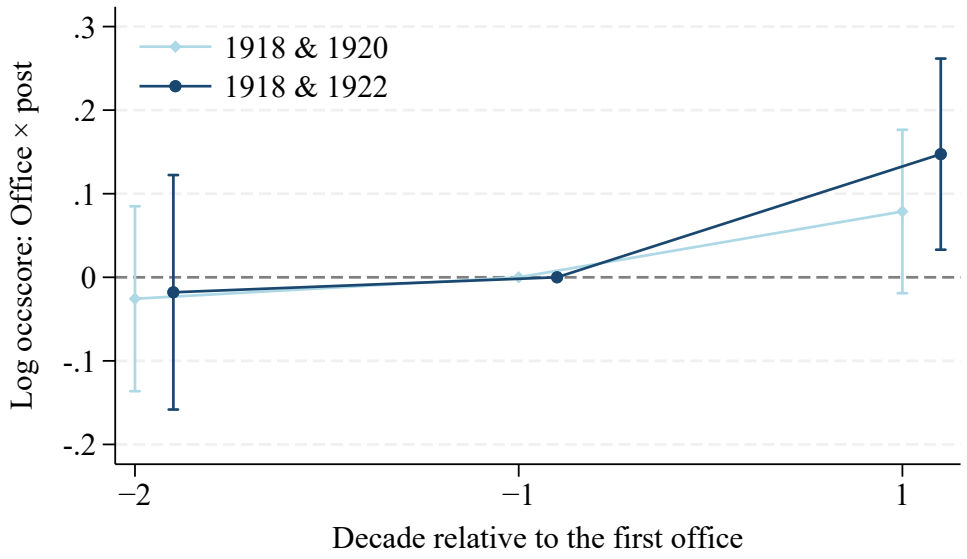


Figure 6. Comparing Children of Politicians and Future Elected Politicians

Note: This figure presents the dynamics of sons' occupational income from estimating Equation 2. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

Figure 7 presents the corresponding event-study plot using a sample consist of children of the last winning and first losing candidates. Although the estimates are less precise because of the small sample, the post-election pattern is again consistent with the baseline finding that sons of winning candidates tend to have higher occupational income after fathers' office entry. Taken together, these two complementary designs provide support for the validity of the main results.

Measurement error in office entry. The information of officials are collected from the rosters between 1912 and 1929. Because of the censored nature of these data, a concern is that the recorded start year may not reflect the true beginning of officeholding, introducing measurement error in treatment timing. In the main analysis, I exclude officials who were first observed in 1912 to address the left-censoring problem. However, the concern still remains that some officials recorded as starting in 1914 or later may in fact have held office prior to 1912 or held the office in non-continuous years. To mitigate this possibility, I re-estimate the analysis restricting the sample to individuals whose fathers are first observed entering office after 1920 and their matched controls, since it is less likely that officials had a gap of more than a decade in their political careers. Table D2 reports these results and gets similar effects of fathers' officeholding.

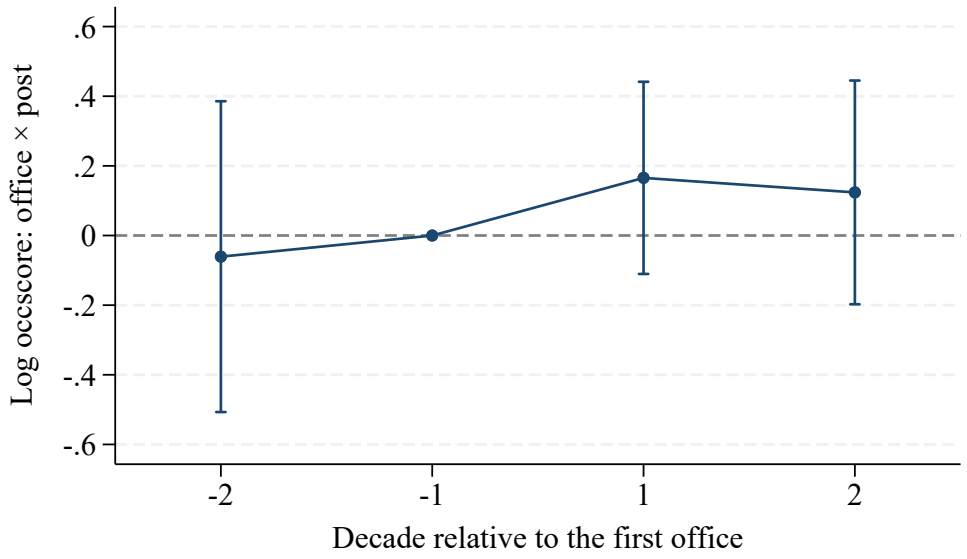


Figure 7. Comparing Children of Winning and Losing Candidates

Note: This figure presents the dynamics of sons' occupational income from estimating Equation 2. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

Reverse Causality. Another concern is that the estimated effect reflects reverse causality that it is children's prestige or success that may have helped their fathers get the office. To test whether this is the explanation, I reestimate the effect restricting the sample to individuals who were young at the time their fathers started the office and their matched counterparts. The rationale is that young children are unlikely to have the leverage to influence their fathers' political careers. Table D3 still finds a positive impact of fathers' officeholding on sons' occupational income and the magnitude is even larger for the young sample.

Spillovers from the officials. As discussed in Section 3.1, I match children of officials to children of non-officials. A concern is that some control children may be relatives of officials, in which case spillover effects could bias the effect. To address this, I exclude individuals sharing the same last name with the officials in their county from the control group. Table D4 shows the result.

4.3 Robustness

In Appendix E.3, I show that the result is robust using alternative standard errors, measures of outcomes, specifications, and samples.

Alternative Inference. Table E4 shows that the result is robust when the standard errors are clustered at the fathers' level, matched-pairs level, and county level.

Alternative measures of outcomes and specifications. In Table E5 and Figure E4, I adopt different forms of the dependent variable (level, log, and LIDO score obtained from Saavedra and Twinam (2020) described in Section 2.4) and Poisson regression for the non-negative nature of the dependent variable (Silva and Tenreyro, 2006). All results show a positive effect of fathers' officeholding on sons' occupational income.

Alternative samples. Table E6 and Figure E5 show that the result is robust using a balanced panel, using only the sample of sons whose fathers started the office between 1914 and 1920 and their matched counterparts, and using only the sample of sons whose fathers started the office between 1922 and 1928 and their matched counterparts. Figure E6 shows that the result is robust to the exclusion of any county in Ohio.

5 Mechanism

This section explores how fathers' local officeholding benefits sons' occupational income. To clarify, because the analysis focuses on individuals who were already in the labor force before their fathers entered office, channels related to human capital investment, such as education, are unlikely to explain the results. I therefore focus on two labor-market access channels. First, the effect could arise from nepotistic hiring within the public sector: local officials may possess discretion over certain appointments in the public sector and use it to favor their sons. Second, the observed increase might reflect better job opportunities for officials' sons in the private sector as firms might want to hire them to exchange political favor from local governments. I discuss other potential mechanisms in Section 5.3.

5.1 Discretionary Hiring in the Public Sector

Politicians often have discretion over hiring decisions in the public sector. This was especially true in the United States before the widespread adoption of civil service reforms, when American politics operated largely under a patronage system(Wilson, 1961). Politicians could allocate government positions based on personal loyalty or family ties rather than merit. If local officials could influence hiring in the public sector, their children might gain preferential access to public-sector jobs. This section provides evidence consistent with this mechanism.

I begin by examining whether fathers' officeholding increases sons' probability of getting employed in the public sector. Column (1) in Table 5 and Panel A in Figure 8 show a sizable and statistically significant increase in the probability that sons work in the public sector after fathers enter office. On average, fathers' entry to office increases their sons' probability of getting employed in the public sector by 1.2 percentage points, roughly 70% of the control mean. This effect persists for nearly two decades, indicating that access to public employment is an important channel through which local officeholding generates intergenerational benefits.

To explore whether this pattern reflects nepotistic hiring, I exploit three complementary sources of variation in hiring discretion. The central implication of nepotism is that intergenerational impact on public-sector employment should be strongest in settings where local officials have greater hiring discretion.

First, I examine variation across types of public-sector jobs. If discretionary hiring is the mechanism, sons' public-sector gains should be concentrated in positions that lie within the sphere of influence of local officials. Columns (2) to (5) in Table 5 disaggregate public employment by employer type. The increase in sons' public-sector employment is concentrated in local government and the postal service, sectors in which local officials could plausibly exert direct influence over hiring, while there is no corresponding increase in state or federal employment, which lies largely outside local discretion. This pattern suggests that the observed increase in public-sector employment for officials' sons might be driven by discretionary allocation.

Second, I exploit institutional differences in hiring discretion across government formats. During the Progressive Era, many Ohio cities adopted civil service systems aimed at curbing patronage and formalizing recruitment, following the 1902 Ohio Civil Service Act. In contrast, villages and townships typically operated under weaker or non civil service mandates, granting local officials substantially greater discretion over public-sector hiring. Consistent with this institutional distinction, Columns (1) to (3) in Table 6 show that fathers' officeholding significantly increases sons' probability of public-sector employment in village and township governments, while the effect in city governments is much smaller and noisier. This heterogeneity aligns with the idea that civil service rules limit discretionary hiring and reduce the scope for intergenerational access to public jobs.

I then examine which sons are more likely to enter the public sector following their fathers' officeholding. Columns (4) and (5) in Table 6 split the sample by sons' pre-

office occupational income. The increase in public-sector employment is concentrated among sons with lower pre-office occupational income relative to the public-sector mean. Rather than attracting higher-quality workers into government employment, fathers' officeholding disproportionately facilitates entry for sons with weaker prior labor market positions. This negative selection is difficult to reconcile with role-model or information-based explanations, which would predict stronger responses among higher-performing sons, and instead points to discretionary access to public-sector jobs that disproportionately benefit individuals with weaker prior labor market prospects.

Table 5. Impact on Getting Jobs in the Public Sector

	Aggregate	Category			
	(1) Public sector	(2) Postal service	(3) Local government	(4) State government	(5) Federal government
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.0118^{***}	0.0068^{***}	0.0043^{***}	0.0003	0.0008
	(0.0022)	(0.0014)	(0.0015)	(0.0003)	(0.0007)
Age bin FE	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y	Y
Control Mean	0.0173	0.0068	0.0081	0.0003	0.0020
R^2	0.5126	0.5802	0.4196	0.3082	0.3699
Observations	88251	88251	88251	88251	88251

Note: This table reports regression estimates of the effect of political office on the probability of getting a job in the public sector. The dependent variable for Column (1) is a dummy equal to one if the individual is employed in the public sector and zero otherwise. Columns (2) to (5) disaggregate the public sector into four categories: postal service (Column (2)), local governments (Column (3)), State government (Column (4)), and Federal government (Column (5)). The unit of observation is at the individual-year level. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.

Finally, I examine whether institutional constraints on discretionary hiring limit the intergenerational benefits of fathers' officeholding by exploiting the adoption of municipal civil service reforms. If nepotistic hiring in the public sector is a key channel, then reforms that formalize recruitment and raise the cost of patronage should weaken the intergenerational returns to officeholding.

Historically, local governments in the United States operated under patronage-based systems in which local officials exercised substantial discretion over public-sector hiring. Beginning in the late nineteenth century, and accelerating during the Progressive Era,

Table 6. Heterogeneity on Getting Jobs in the Public Sector

	Dependent variable: Public-sector employment				
	Government Format			Pre-office Occscore	
	(1)	(2)	(3)	(4)	(5)
	City	Village	Township	Low income	High income
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.0040	0.0093***	0.0147***	0.0129***	0.0076
	(0.0093)	(0.0036)	(0.0029)	(0.0022)	(0.0065)
Age bin FE	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y	Y
Control Mean	0.0222	0.0196	0.0156	0.0140	0.0363
R^2	0.5491	0.5291	0.5104	0.4903	0.5735
Observations	5684	31900	50667	70387	14377

Note: This table reports regression estimates of the effect of political office on the probability of getting a job in the public sector across different samples. The dependent variables are dummy variables equal to one if the individual is employed in the public sector and zero otherwise. Columns (1) to (3) separate the sample by the format of governments: city governments, village governments, and township governments. Columns (4) and (5) separate the sample by sons' occupational income in $t = -1$. Specifically, the sample in Column (4) includes sons with occupational income lower than the mean occupational income earned in the public sector and their matched counterparts while the sample in Column (5) includes son earned higher than the average public sector income and their matched counterparts. The unit of observation is at the individual-year level. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.

many cities adopted municipal civil service reforms aimed at curbing patronage and promoting merit-based employment. Municipal civil service reforms typically established commissions to oversee recruitment, examinations, promotions, and dismissals of public employees. While the institutional details varied across cities, commissions could be appointed by mayors or city councils, and in some cases elected, the common objective of these reforms was to limit discretionary hiring and reduce political influence over public employment.

I exploit the staggered adoption of municipal civil service reforms across Ohio cities using data from Kuipers and Sahn (2023). This variation allows me to compare the intergenerational effects of fathers' officeholding in cities before and after civil service adoption, relative to cities that had not yet reformed. Figure A2 plots the distribution of reform timing across Ohio cities. To explore how the civil service adoption affects the intergenerational political benefits, I estimate a triple-differences specification that interacts

fathers' officeholding, post-office timing, and civil service reform status at the city level. The specification is as follows:

$$y_{ijbcmt} = \alpha_i + \beta_1 \text{Reform}_{mt} \times \text{Office}_{j(i)} \times \text{Post}_{it} + \beta_2(\text{Office}_{j(i)} \times \text{Post}_{it}) + \beta_3 \text{Reform}_{mt} \times \text{Post}_{it} + \beta_4 \text{Reform}_{mt} + \beta_5 \text{Post}_{it} + \theta_{bt} + \mu_{ct} + \varepsilon_{ipbct} \tag{4}$$

where m denotes the municipality where i's father gets the office and Reform_{mt} is a binary variable equal to one if city m has already adopted the civil service reform in year t and zero otherwise. Because civil service reforms were more likely to be adopted earlier in larger cities (Kuipers and Sahn, 2023), where labor market conditions may differ systematically, I additionally control for interactions between city population size and $\text{Office}_{j(i)} \times \text{Post}_{it}$, addressing the concern of $\text{Reform}_{mt} \times \text{Office}_{j(i)} \times \text{Post}_{it}$ capturing such effect of city size.

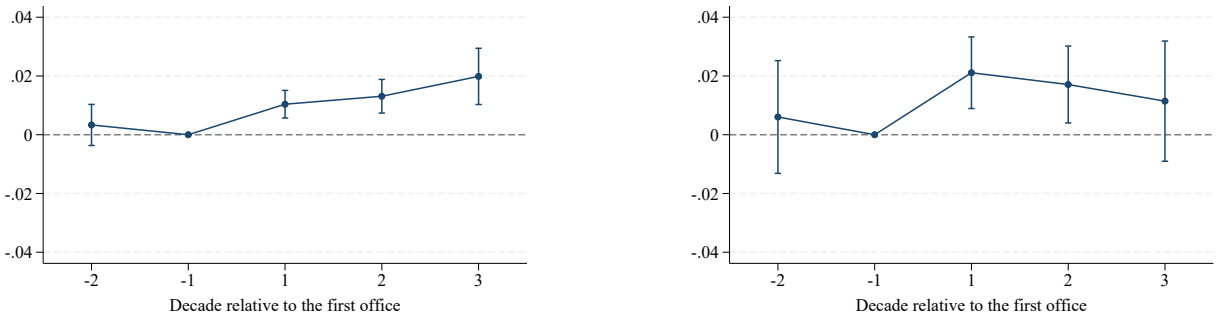
Table 7. Impact by Civil Service Reform Status

	Logged Income Score$_{it}$		Goverment Job$_{it}$		Private White-collar$_{it}$		Private Blue-collar$_{it}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Reform}_{j(i)} \times \text{Office}_{j(i)} \times \text{Post}_{it}$	-0.114*	-0.114*	-0.030	-0.031	0.137*	0.138**	-0.012	-0.013
	(0.066)	(0.066)	(0.029)	(0.029)	(0.070)	(0.070)	(0.065)	(0.065)
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.136**	0.139**	0.030	0.023	-0.081	-0.062	0.025	0.018
	(0.064)	(0.064)	(0.027)	(0.027)	(0.065)	(0.066)	(0.060)	(0.060)
$\text{Population}_{j(i)} \times \text{Office}_{j(i)} \times \text{Post}_{it}$		-0.001		0.001		-0.004**		0.001
		(0.001)		(0.001)		(0.001)		(0.001)
Age bin FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Control Mean	29.793	29.793	0.026	0.026	0.403	0.403	0.313	0.313
R^2	0.739	0.739	0.556	0.557	0.690	0.691	0.613	0.613
Observations	5925	5925	6245	6245	6245	6245	6245	6245

Note: This table reports regression results of triple differences estimated using Equation 4. The dependent variables are log occupational income score (Columns (1)-(2)), whether employed in the public sector (Columns (3)-(4)), whether employed in the private-sector white-collar jobs (Columns (5)-(6)), and whether employed in the private-sector blue-collar jobs (Columns (7)-(8)). The sample includes sons of officials in the city governments and their matched counterparts. The unit of observation is at the individual-year level. Reform is a binary variable equal to 1 if individual's father starts the office when the city has already adopted the civil service reform and equal to 0 otherwise. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.

Table 7 reports the results. Columns (1) and (2) show that the occupational income gains associated with fathers' officeholding are substantially attenuated in cities that adopted civil service reforms, consistent with institutional constraints limiting discretionary access to public employment for officials' sons. Columns (3) and (4) directly examine the public employment channel and show a negative but imprecisely estimated effect of reform on sons' public-sector employment. In contrast, Columns (5) and (6) show

that civil service reform is associated with a significant increase in sons' private-sector white-collar employment, suggesting that when public-sector nepotism becomes more costly, intergenerational advantages shift toward the private-sector channel. The null effect of the reform on the private-sector blue-collar employment (Columns (7) and (8)) provides a placebo test, reinforcing that the observed pattern is not driven by labor-market differences across reform and non-reform cities.



A. Government Jobs **B.** Private-sector White-collar Jobs

Figure 8. Dynamic Impact on Jobs

Note: The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

Taken together, these three pieces of evidence point to the discretionary hiring in the public sector as a key mechanism underlying the intergenerational effects of local officeholding. Officials' sons' public-sector employment is concentrated in sectors subject to local officials' influence, is strongest in institutional settings with weaker civil service constraints, and is driven by individuals with lower pre-office occupational income scores. Moreover, when the civil service reform limits discretionary hiring, the intergenerational political rent is substantially attenuated. These patterns are difficult to reconcile with alternative explanations, such as role-model effects or information transmission, and instead highlight the central role of discretionary hiring in shaping sons' access to public-sector employment.

5.2 Better Opportunities in the Private Sector

In addition to discretionary hiring in the public sector, fathers' officeholding may also affect sons' labor market outcomes in the private sector. Local governments exercise regulatory authority over firms through licensing, zoning, procurement, and financial oversight. As a result, firms may have incentives to hire the children of local officials either to facilitate access to government resources or to cultivate favorable relationships

with local authorities. This section examines whether fathers' officeholding improves sons' access to white-collar jobs in the private sector.

Column (1) in Table 8 and Panel B of Figure 8 show that, beyond increased access to public employment, sons of officials are also more likely to enter white-collar positions in the private sector following their fathers' entry into office. Columns (2) to (4) further document substantial heterogeneity across government formats: the increase in private-sector white-collar employment is largest in cities, smaller in villages, and statistically indistinguishable from zero in townships. This pattern is consistent with the distribution of private-sector opportunities, as cities have more industrial firms and more interactions between local governments and businesses.

I next examine heterogeneity by the branch of office held by fathers. Figure 9A shows that sons of officials in executive offices experience the largest increase in private-sector white-collar employment. In contrast, there is no detectable effect for sons of officials in judicial offices, where authority over firms and routine interactions with businesses are limited. Effects for legislative, fiscal, and public works offices are smaller in magnitude than those for executive offices. This heterogeneity by the branch of office closely mirrors differences in administrative authority and engagement with local businesses.

To further understand which types of private-sector jobs drive these effects, Figure 9B disaggregates white-collar employment by industry. The results show that sons of officials are disproportionately more likely to work in wholesale and retail trade, finance, insurance, and real estate. These sectors are directly affected by local licensing, zoning, and financial oversight, where firms may have stronger incentives to maintain close relationships with local officials.

Taken together, these findings suggest that private-sector white-collar employment is a complementary channel through which local officeholding benefits sons' occupations. Unlike the public-sector channel, which operates through discretionary hiring, the private-sector channel appears to reflect firms' incentives to employ politically connected individuals in regulated economic environments.

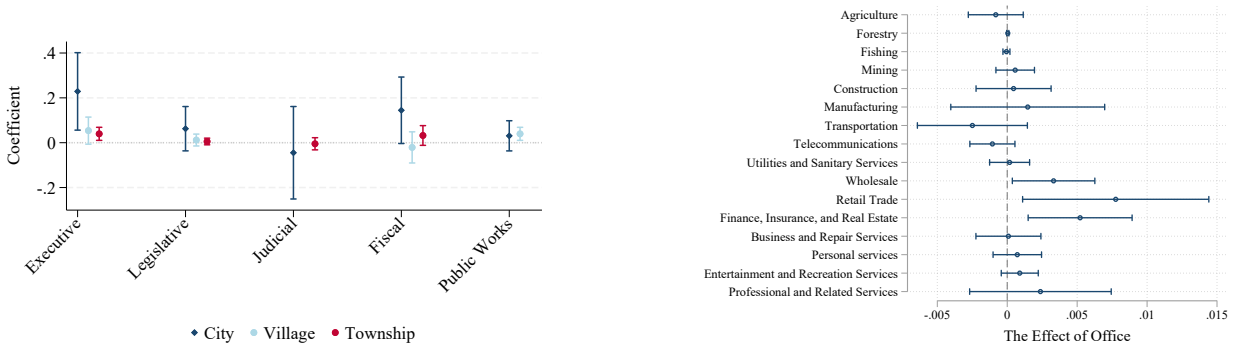
5.3 Other Mechanisms

This section examines alternative mechanisms through which fathers' officeholding increases sons' occupational income.

Table 8. Impact on Getting White-collar Jobs in the Private Sector

	Aggregate	Government Format		
	(1) Private sector	(2) City	(3) Village	(4) Township
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.0172***	0.0717***	0.0232**	0.0070
	(0.0054)	(0.0242)	(0.0093)	(0.0065)
Age bin FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y
Control Mean	0.1887	0.3755	0.2375	0.1429
R^2	0.6402	0.6503	0.6399	0.6329
Observations	88251	5684	31900	50667

Note: This table reports regression estimates of the effect of political office on the probability of getting a white-collar job in the private sector. The dependent variables are dummy variables equal to one if the individual is employed in a white-collar job in the private sector and zero otherwise. Column (1) includes the full sample. Columns (2) to (4) separate the sample by the format of governments: city governments, village governments, and township governments. The unit of observation is at the individual-year level. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.



A. Government Branch **B.** Sector

Figure 9. Heterogeneity of Private-sector White-collar Jobs

Note: This figure plots the coefficient of Office × Post on the probability of getting employed in white-collar jobs in the private sector. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

One potential explanation is occupational inheritance, whereby sons take over their fathers' pre-office jobs after fathers' office entry. This channel is unlikely for two reasons.

First, many local offices during this period were not full-time positions and did not require officials to leave their previous employment (Ohio, 1910). Second, Table F1 directly tests whether sons are more likely to enter their fathers' pre-office occupations and shows no evidence consistent with this mechanism.

Another possibility is social-network expansion. Fathers' officeholding may broaden families' access to social and professional networks, which could indirectly improve sons' labor market prospects. While social networks are difficult to measure, Table F2 partially explores this channel by examining whether fathers' officeholding affects sons' marriage networks. Table F2 shows no evidence that sons of officials are more likely to marry spouses from wealthier families, from families with fathers employed in the public sector, or from families in white-collar occupations. These results suggest limited scope for marriage-based network expansion as a driver of the observed occupational gains.

Finally, the observed advantage could reflect a reputational effect, whereby sons benefit from the prestige or visibility associated with their fathers' political status. If reputation operates primarily through name recognition, its effects should be stronger for sons with less common surnames, whose family identity is more easily identifiable. Table F3 tests this implication and finds no significant heterogeneity by surname commonness, indicating that reputational effects through name recognition are unlikely to explain the results.

6 Downstream Impact of Intergenerational Rents

So far, this paper documents sizable private returns to holding local political office, transmitted across generations through both public- and private-sector channels. A natural question is whether these intergenerational political rents generate broader spillovers for the local economy. This concern connects to a broader literature showing that politically connected allocation of jobs could weaken bureaucratic quality and performance incentives, reduce state capacity, and affect the economic development (Xu, 2018; Aneja and Xu, 2024; George and Ponattu, 2019). In small jurisdictions such as townships, where political offices, public-sector jobs, and high-status positions are limited, this concern is particularly salient. In such settings, better access to opportunities for officials' sons may directly crowd out the opportunities for other families and the performance of the public sector shapes the local development directly. In this section, I examine whether greater exposure to intergenerational political rents is associated with higher inequality and lower intergenerational mobility among non-politician families.

For the analysis of downstream outcomes, I focus on townships. Townships represent small jurisdictions and meanwhile the intergenerational political rents operate mostly through the channel of public sector as discussed in Section 5. To measure township-level exposure to intergenerational political rents, I construct a proxy based on the share of public-sector employees who are sons of local officials, as observed in the 1930 Census. I identify the township in the Censuses by linking the individuals to their residential towns using linkages from Berkes et al. (2023).[9] Figure A3 plots the distribution of this measure across townships. Approximately 79% of townships have no public-sector employees who are sons of local officials, while the remaining townships exhibit varying degrees of intergenerational political rents. I then examine how exposure to intergenerational political rentsmeasured using both (i) a binary indicator for whether a township employs at least one official's son in the public sector and (ii) a continuous measure equal to the share of such employeesrelates to township-level inequality and intergenerational mobility outcomes.

Table 9 examines whether intergenerational political rents are associated with broader distortions in the local distribution of economic opportunity. Columns (1) and (2) show that townships with greater exposure to intergenerational political rents, measured by either the share of public-sector employees who are sons of local officials or an indicator for employing at least one such individual, exhibit significantly higher income inequality in the 1940 Census. Importantly, these relationships hold conditional on baseline township characteristics, including the level of inequality in 1930, suggesting that the results are not driven by pre-existing inequality. A similar pattern emerges when inequality is measured using the top 10% earning share. Columns (3) and (4) show that townships with higher exposure to intergenerational political rents display a more concentrated upper tail of the income distribution. Specifically, Column (4) implies that having at least one official's son in the public sector is associated with around a 2 percentage point increase in the top 10 percent earning share. A simple calculation at the sample mean level suggests that this magnitude corresponds to about $149 more annual earnings per top-decile person, which is around 17 % of the mean annual wage income ($839) in the 1940 township. Taken together, these findings suggest that intergenerational political rents were associated not only with higher average returns for politically connected families, but also with greater local income concentration.

Building on the findings in Section 5 that sons of local officials have better access to well-paying public-sector jobs, a natural question is whether such advantages crowd out

[9]This procedure yields a matched sample of 1,014 Ohio townships.

Table 9. Downstream Effect of Intergenerational Political Rents

	Gini Coefficient		Top-10% Earning Share		Non-official's Son-Father Rank	
	(1)	(2)	(3)	(4)	(5)	(6)
Share of politicians' sons in pub	0.032**		0.037*		-0.136	
	(0.014)		(0.022)		(0.099)	
Sons in pub		0.017***		0.017***		-0.081**
		(0.006)		(0.005)		(0.039)
Male share	-0.611***	-0.598***	-0.395***	-0.382***	2.631**	2.570**
	(0.131)	(0.131)	(0.107)	(0.108)	(1.059)	(1.066)
White share	0.088	0.090	-0.067	-0.064	-0.822	-0.828
	(0.085)	(0.086)	(0.090)	(0.091)	(0.539)	(0.540)
Native share	0.074	0.079	-0.066	-0.062	-0.636	-0.663
	(0.061)	(0.063)	(0.062)	(0.062)	(0.640)	(0.638)
Urban share	-0.002	-0.005	0.017	0.015	-0.016	-0.003
	(0.012)	(0.012)	(0.011)	(0.011)	(0.091)	(0.090)
Employment share	-0.266***	-0.268***	-0.236***	-0.240***	2.735***	2.744***
	(0.080)	(0.080)	(0.077)	(0.076)	(0.557)	(0.556)
Agricultural employment share	0.567***	0.566***	0.463***	0.462***	-1.757***	-1.754***
	(0.023)	(0.023)	(0.025)	(0.025)	(0.153)	(0.153)
Public employment share	1.045	0.760	0.714	0.414	-0.780	0.593
	(0.807)	(0.756)	(0.697)	(0.639)	(3.642)	(3.407)
Log population	0.026***	0.024***	0.019***	0.016***	0.004	0.016
	(0.007)	(0.007)	(0.006)	(0.006)	(0.047)	(0.048)
Gini coef (1930)	0.177***	0.169**	-0.047	-0.055	0.887	0.926
	(0.066)	(0.067)	(0.058)	(0.058)	(0.571)	(0.574)
Dependent Mean	0.605	0.605	0.370	0.370	1.485	1.485
Dependent S.D.	0.122	0.122	0.106	0.106	0.589	0.589
R^2	0.669	0.671	0.598	0.600	0.369	0.371
Observations	1007	1007	1007	1007	1007	1007

Note: This table reports regression estimates of the downstream effects of intergenerational political rents at the township level. The dependent variables are the township-level Gini coefficient (Columns (1)(2)), the township-level top 10 percent income share (Columns (3)(4)), and the average sonfather income rank among non-official families in the township (Columns (5)(6)). The key explanatory variable in Columns (1), (3), and (5) is the share of public-sector employees who are sons of local officials; in Columns (2), (4), and (6), it is a binary indicator equal to one if at least one public employee is the son of local official. The unit of observation is at the township level in the 1940 Census. The standard errors are clustered at the county level.

the economic opportunities for individuals without political connections. To examine this, I measure township-level intergenerational mobility using the average fatherson income rank among non-official families, defined as the ratio of sons' income ranks within their birth cohorts in the 1940 Census to their fathers' occupational income score ranks within their cohorts in the 1920 Census. Columns (5) and (6) show that townships employing at least one public-sector worker who is the son of a local official exhibit significantly lower intergenerational mobility among non-official families, although the estimate is

statistically insignificant when exposure to intergenerational political rents is measured using the continuous share of such employees.

7 Concluding Remarks

This paper studies the intergenerational returns to holding local political office. Using newly digitized records of township, village, and city officials in Ohio between 1912 and 1929, linked to full-count U.S. census data, I trace the long-run labor market outcomes of officials' children. I show that fathers' officeholding increases sons' occupational income and their likelihood of entering white-collar occupations for decades after office entry, while having little effect on daughters' labor force participation or marital outcomes. These effects together suggest that local political power operated through access to jobs rather than through broader wealth transmission or marriage-market channels.

The paper provides evidence on the mechanisms underlying these intergenerational effects. Fathers' officeholding benefits sons through two channels: discretionary hiring in the public sector and improved access to private-sector opportunities. The public-sector channel is strongest in institutional settings with weaker constraints on discretionary hiring, while the private-sector channel is most pronounced in cities and in industries subject to greater local regulation. Importantly, institutional reforms such as civil service adoption attenuate the intergenerational political benefits but at the same time redirect political rents toward the private sector.

Beyond the private returns to officeholding families, the paper provides suggestive evidence that intergenerational political rents have broader economic consequences. Localities with greater exposure to officials' sons in public employment subsequently exhibit higher inequality and lower intergenerational mobility among non-politician families, consistent with the crowding out of economic opportunities in small local labor markets.

Taken together, the findings show that political power at the local level can generate persistent intergenerational advantages and shape the broader distribution of opportunity. While institutional reforms that limit discretion in one domain may reduce particular forms of political rent extraction, they might not eliminate political advantage altogether. More generally, the results highlight that routine local political offices, often viewed as inconsequential, can play an important role in shaping economic inequality and mobility over the long run.

I conclude this paper by noting some limitations. First, I focus on a historical setting in which labor market and gender roles differed from those today, and caution is warranted in extrapolating magnitudes to contemporary contexts. Second, while the paper documents downstream associations with inequality and mobility, the mechanism behind and a full welfare analysis is beyond its scope. Understanding how intergenerational political rents affect public-sector performance, firm behavior, and the allocation of economic resources remains an important direction for future research.

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Online Appendices

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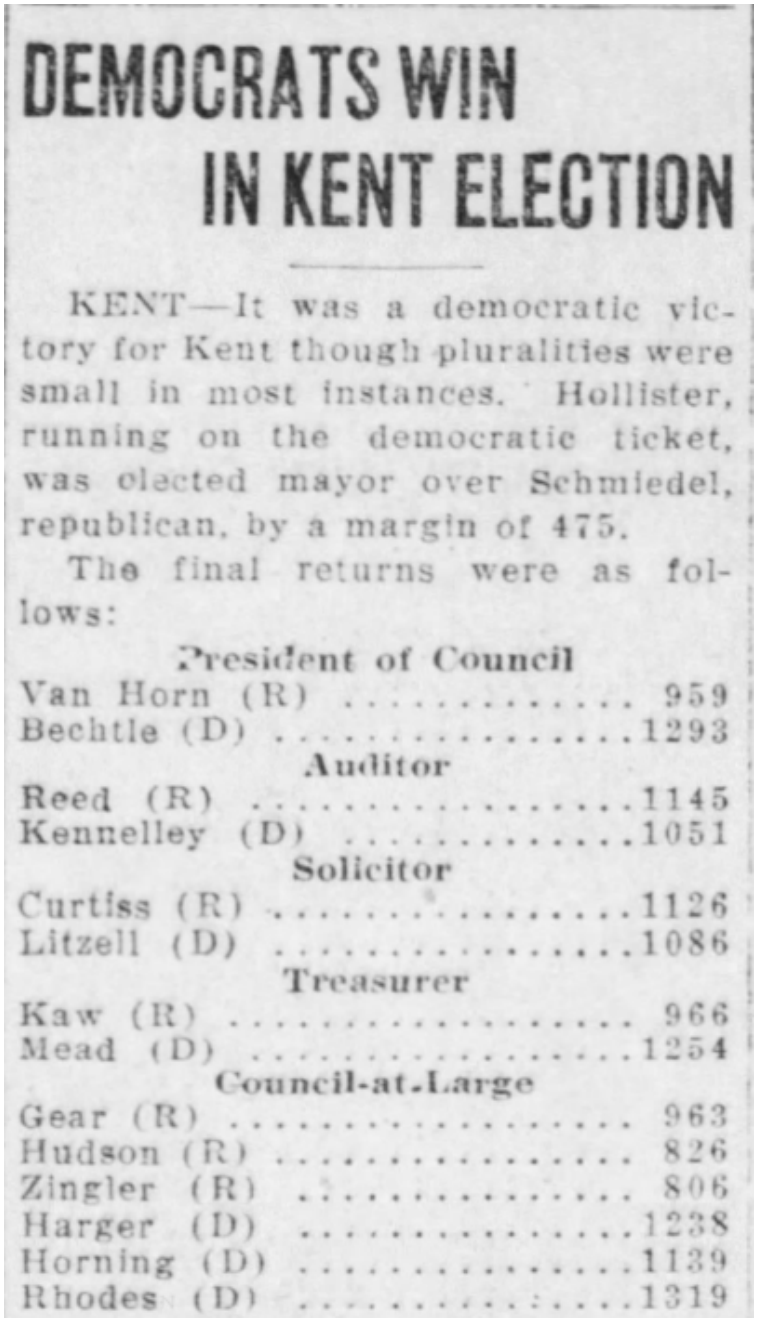
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A Additional Figures



DEMOCRATS WIN IN KENT ELECTION

KENT—It was a democratic victory for Kent though pluralities were small in most instances. Hollister, running on the democratic ticket, was elected mayor over Schmiedel, republican, by a margin of 475.

The final returns were as follows:

President of Council

Van Horn (R) 959
Bechtle (D) 1293

Auditor

Reed (R) 1145
Kennelley (D) 1051

Solicitor

Curtiss (R) 1126
Litzell (D) 1086

Treasurer

Kaw (R) 966
Mead (D) 1254

Council-at-Large

Gear (R) 963
Hudson (R) 826
Zingler (R) 806
Harger (D) 1238
Horning (D) 1139
Rhodes (D) 1319

Figure A1. Example: Newspaper Coverage on Election

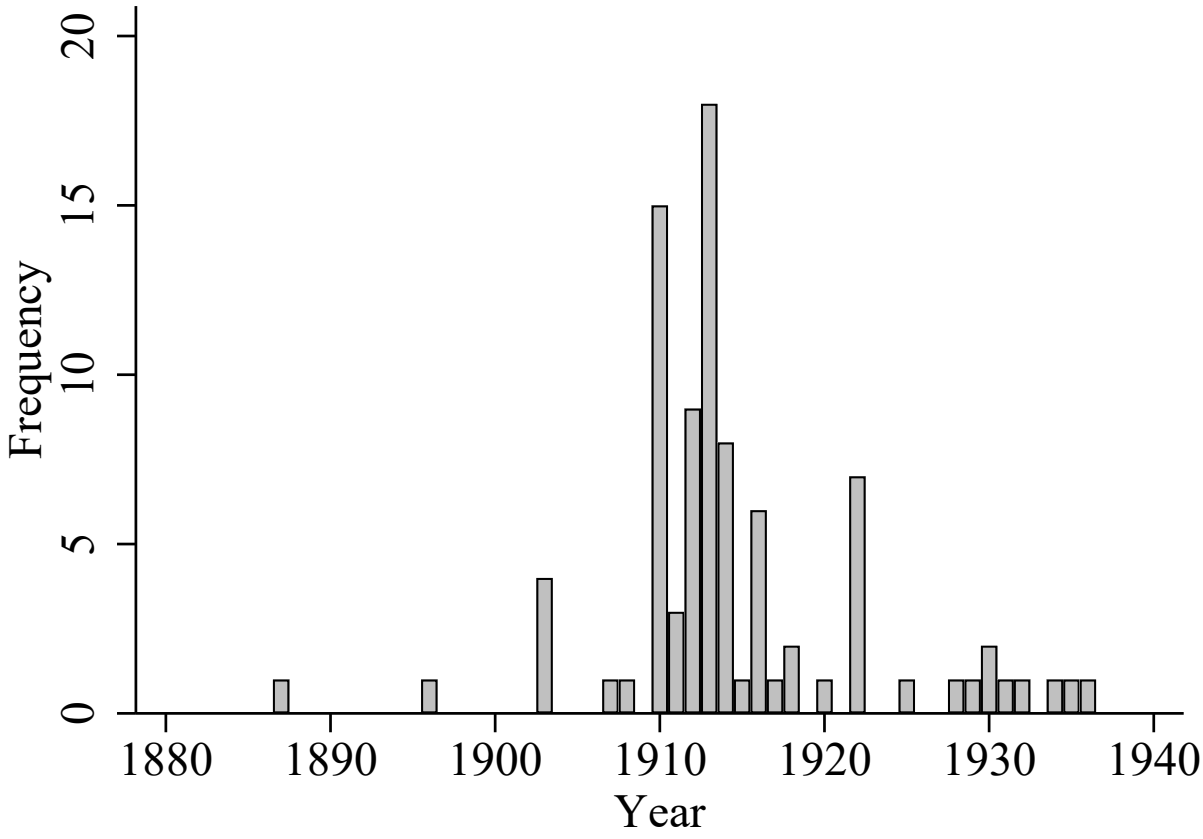


Figure A2. Adoption of Civil Service Reform in Ohio Municipalities

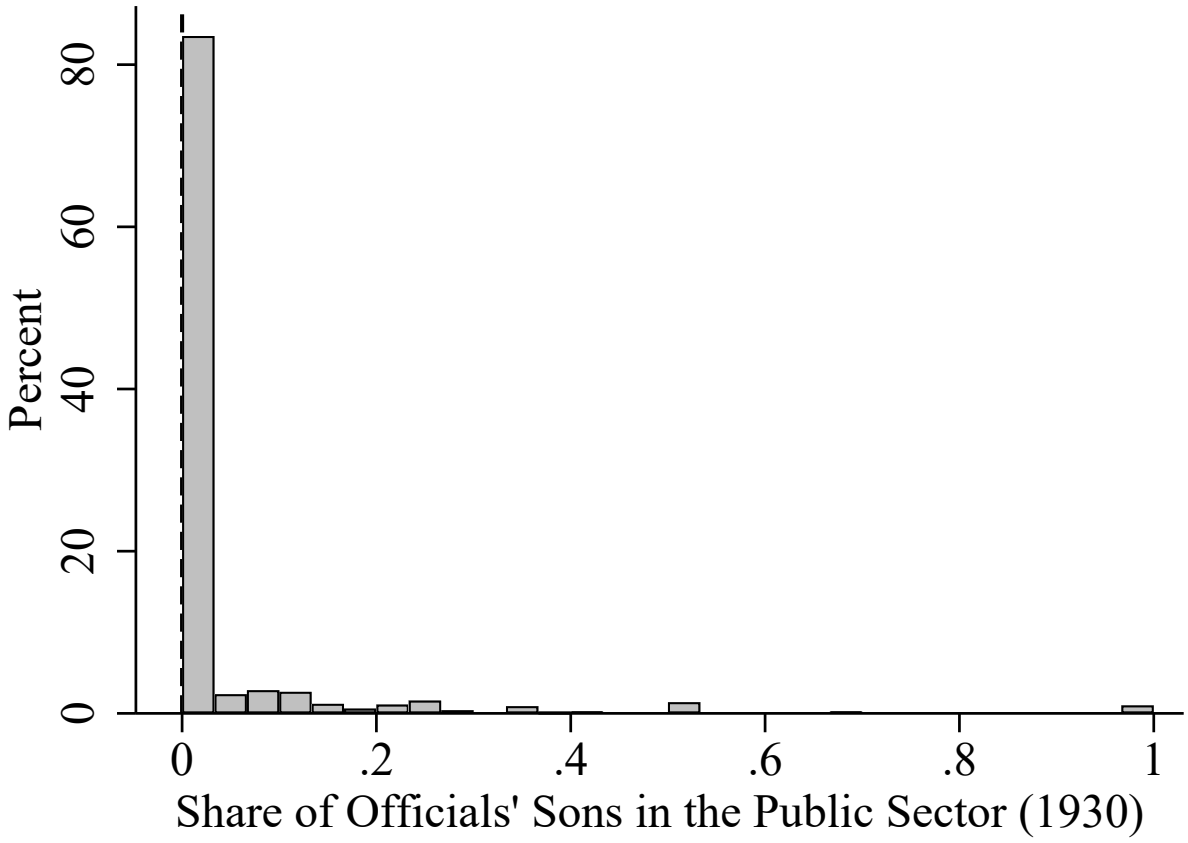


Figure A3. Distribution of Share of Officials' Sons in the Public Sector

B Additional Table

Table B1. List of Offices by Forms of Government

Government Form	Offices
Township	Clerk; Treasurer; Trustee; Justice of the Peace
Village	Assessor; Board of Education; Board of Public Affairs; Clerk; Commissioner; Health Officer; Manager; Marshal; Mayor; Members of Council; Solicitor; Street Commissioner; Treasurer
City	Assessors; Assistants; Auditor and Treasurer; Bailiff; Boards of Health, Education, Library, or Review; Chemist at Water Plant; Chief Justice; Chief of Police/Fire; City Attorney; City Engineer; City Manager; Civil Service Commission; Clerk; Commissioners of Accounts, Public Buildings, Treasury, Water; Directors of Finance, Law, and Public Services/Safety/Health; Judges; Mayor; Members of Council; Members of City Commission; Municipal Court; Planning, Playground, Smoke Abatement, Sinking Fund, and Tax Commissions; Police Justice; Sanitary Officer; Sealer of Weights and Measures; Solicitor; Superintendent; Vice Mayor; Vice President of Council

Note: This table presents the lists of offices reported in the Rosters.

Table B2. Linking Roster to Census: Linking rate

	All		*City*		*Village*		*Township*	
	Link rate	Total	Link rate	Total	Link rate	Total	Link rate	Total
All	0.70	179952	0.72	21007	0.66	82559	0.73	76386
1912-1913	0.70	20866	0.71	2402	0.67	9266	0.73	9198
1914-1915	0.70	19398	0.72	1924	0.67	8396	0.73	9078
1916-1917	0.71	18432	0.74	1825	0.66	7570	0.74	9037
1918-1919	0.71	19735	0.74	2046	0.67	8673	0.75	9016
1920-1921	0.73	19385	0.73	1879	0.71	8800	0.75	8706
1922-1923	0.71	19880	0.72	2420	0.68	8681	0.75	8779
1924-1925	0.69	18754	0.72	2514	0.66	8767	0.71	7473
1926-1927	0.68	20627	0.70	2552	0.65	10600	0.72	7475
1928-1929	0.67	22875	0.70	3445	0.63	11806	0.71	7624

Note: This table presents the link rates of linking roster data to full-count individual censuses.

C Local Officials in Ohio

C.1 Ohio Rosters of Township and Municipal Officers

AKRON—POPULATION, 69,000.

Office.	Name of Officer.	Politics.	Term Expires.
Mayor	Isaac Myers	D	Jan. 1, 1920
President of Council	Jesse Merriman	R	Jan. 1, 1920
Auditor	Thomas Heffernan	D	Jan. 1, 1920
Treasurer	Wm. Hoover	D	Jan. 1, 1920
Solicitor	Scott D. Kenfield	R	Jan. 1, 1920
Police or Municipal Judge	John R. Vaughan	D	Jan. 1, 1919
Director of Public Service	Carl F. Beck	R	Jan. 10, 1918
Director of Public Safety	Charles R. Morgan	R	Jan. 10, 1918
Trustees of Sinking Fund and Board of Tax Commissioners	Joseph Winum	D	
	Armar Carnaham	R	
	P. Tobrie	D	
	W. H. Means	R	
Board of Healt	George Kuhlke	D	
	A. B. Jones	R	
	Charles Hoffman	....	
	Dr. Armin Sicherman	....	
	Dr. Van der Hulse	....	
Board of Library Trustee	Harry L. Snyder	R	
	Jesse P. Dice	R	
	Hugh Allen	R	
	A. Holm	D	
	John Gross	D	
Board of Education	H. T. Waller	R	
	Mrs. A. Ross Read	....	
	J. Asa Palmer	D	
	D. W. Kaufman	R	
	C. W. Berry	R	
	C. M. Woodruff	R	
Chief of Police	John Durkin	D	Civil Service.
Chief of Fire Department	John T. Merty	R	Civil Service.
City Engineer	Earl Zeisloft	R	Civil Service.

Figure C1. Example page for city officials

ADAMS COUNTY.

Village.	Population.	Office.	Names.	Politics.	Term Expires.
Manchester	2,000	Mayor	A. H. Holderness	R	Dec. 31, 1919
		Clerk	S. N. Greenle	R	Dec. 31, 1919
		Treasurer	Guy Ellison	R	Dec. 31, 1919
		Marshal	Richard Harris	R	Dec. 31, 1919
		Members of Council	G. E. Neal	....	Dec. 31, 1919
			Robert Poole	....	Dec. 31, 1919
			Dan Grimsby	....	Dec. 31, 1919
			John Bell	....	Dec. 31, 1919
			Samuel Batharlomew	....	Dec. 31, 1919
		Board of Public Affairs	Ed. Smelling	....	
			Rufus McCormick	....	
			D. J. Slableton	....	
		Board of Education	John Koeple	....	Dec. 31, 1919
			J. C. Henderson	....	Dec. 31, 1919
			P. D. Vance	....	Dec. 31, 1921
			R. E. Shelton	....	Dec. 31, 1921
			Rufus Cox	....	Dec. 31, 1921
Peebles	.....	Mayor	J. N. Askren	R	Jan. 1, 1920
		Clerk	H. B. Hunter	D	Jan. 1, 1920
		Treasurer	G. E. Henty	D	Jan. 1, 1920
		Marshal	Geo. Hoop	D	Jan. 1, 1920
		Members of Council	W. A. Thompson	D	Jan. 1, 1920
			C. C. Condon	R	Jan. 1, 1920
			E. W. Thomas	R	Jan. 1, 1920
			Harry Parker	D	Jan. 1, 1920
			Jacob Custer	D	Jan. 1, 1920
			M. T. Newman	R	Jan. 1, 1920
		Board of Education	C. A. Watts	....	Jan. 1, 1920
			D. W. Reynolds	....	Jan. 1, 1920
			P. A. Campbell	....	Jan. 1, 1920
			D. W. Shoemaker	....	Jan. 1, 1922
			E E. Mathias	....	Jan. 1, 1922
Seaman	600	Mayor	J. T. Ryan	D	Jan. 1, 1920
		Clerk	Homer King	R	Jan. 1, 1920
		Treasurer	F. G. Young	R	Jan. 1, 1920
		Marshal	John Hannah	D	Jan. 1, 1920
		Members of Council	C. E. Haas	D	Jan. 1, 1920
			C. E. Sturn	R	Jan. 1, 1920
			C. E. Heidrick	R	Jan. 1, 1920
			J. A. Caskey	R	Jan. 1, 1920
			C. W. Elmore	R	Jan. 1, 1920
			C. S. Moore	D	Jan. 1, 1920
		Board of Education	J. O. Kendall	R	Jan. 1, 1920
			H. A. Grooms	D	Jan. 1, 1922
			E. C. Zimmerman	R	Jan. 1, 1922

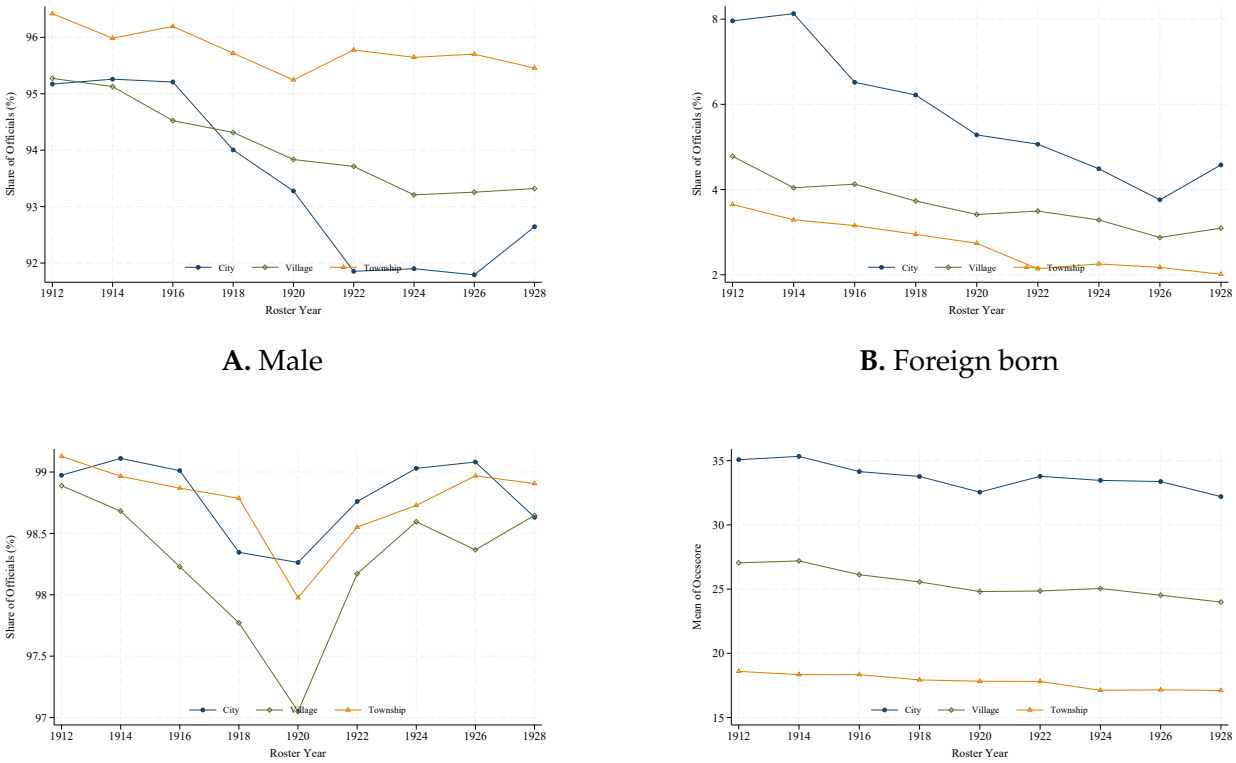
Figure C2. Example page for village officials

ADAMS COUNTY.

Township.	Names of Trustees.	Postoffice Address.
Bratton	Thomas Breslan	Peebles, Ohio, R. F. D. No. 2.
	Gilbert Smily	Peebles, Ohio.
	C. W. Jones	Peebles, Ohio.
Franklin	R. T. Copeland	Peebles, Ohio, R. F. D. No. 6.
	H. F. Hirdman	Peebles, Ohio, R. F. D. No. 6.
	S. E. Stodgel	Peebles, Ohio, R. F. D. No. 6.
Green	M. H. Ralston	Stout, Ohio.
	A. W. Foster	Sandy Springs, Ohio.
	Adam Richter	Buena Vista, Ohio.
Jefferson	N. W. Cross	Wamsley, Ohio, R. F. D. No. 1.
	Clayton Moore	Wamsley, Ohio, R. F. D. No. 1.
	W. D. Vogler	West Union, Ohio, R. F. D. No. 2.
Liberty	T. E. Shell	West Union, Ohio, R. F. D. No. 5.
	J. T. Moore	West Union, Ohio, R. F. D. No. 1.
	W. S. Jackson	West Union, Ohio, R. F. D. No. 1.
Manchester	J. N. Matthews	Manchester, Ohio.
	Grimes N. Morgan	Manchester, Ohio.
	W. H. Gray	Manchester, Ohio.
Meigs	W. P. Newman	Peebles, Ohio.
	T. J. Newman	Rarden, Ohio.
	H. C. Wright	Peebles, Ohio.
Monroe	Homer Baldwin	West Union, Ohio, R. F. D. No. 5.
	James Clark	Manchester, Ohio, R. F. D. No. 2.
	J. E. Young	West Union, Ohio, R. F. D. No. 4.
Oliver	Robbie Moore	West Union, Ohio.
	Earl Lewis	Peebles, Ohio.
	Wm. McClelland	West Union, Ohio.
Scott	J. C. McKenzie	Seaman, Ohio, R. F. D. No. 2.
	J. H. Plummer	Seaman, Ohio, R. F. D. No. 2.
	Jno. I. Cornelius	Peebles, Ohio, R. F. D. No. 2.
Sprigg	Charles Scott	Manchester, Ohio, R. F. D. No. 3.
	C. C. Harover	Manchester, Ohio, R. F. D. No. 1.
	Fred Kimble	Manchester, Ohio, R. F. D. No. 3.
Tiffin	J. C. Crawford	West Union, Ohio.
	J. A. Craigmile	West Union, Ohio.
	Wm. Shumaker	West Union, Ohio.
Wayne	J. M. Kepperling	Cherry Fork, Ohio.
	Edward Siddens	Peebles, Ohio, R. F. D.
	Ernest Shelton	Winchester, Ohio, R. F. D.
Winchester	R. C. Fulton	Winchester, Ohio.
	G. N. Baker	Winchester, Ohio.
	D. H. Pence	Winchester, Ohio.

Figure C3. Example page for township officials

C.2 Who Are These Local Officials?



A. Male

B. Foreign born

C. Literacy

D. Pre-office occupational income score

Figure C4. Characteristics of Officials by Year

Note: This figure depicts the baseline characteristics of local officials by year and government format. Panels A, B, C, and D separately plot the trend of share of white officials, share of foreign-born officials, share of literate officials, and officials' average pre-office occupational income score.

D Validity of the Design

Table D1. Validity: Comparing Treated VS Not-yet Treated

	(1)	(2)
	1918 & 1920	1918 & 1922
Office early × Post	0.089*	0.155***
	(0.046)	(0.058)
Age bin FE	Y	Y
Individual FE	Y	Y
County × Year FE	Y	Y
Mean	22.473	22.226
R^2	0.838	0.865
Observations	1261	885

Note: This table reports regression estimates of the effect of political office on sons' labor market outcomes in the sample of officials' sons. The unit of observation is at the individual-year level. The dependent variables are log occupational income score. The sample in Column (1) includes sons of officials starting office in year 1918 and 1920 and Column (2) includes sons of officials starting office in year 1918 and 1922. The sample period is from censuses 1900 to 1920. Office early is a binary variable equal to 1 if the individual's father starts the office before 1920, and 1 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

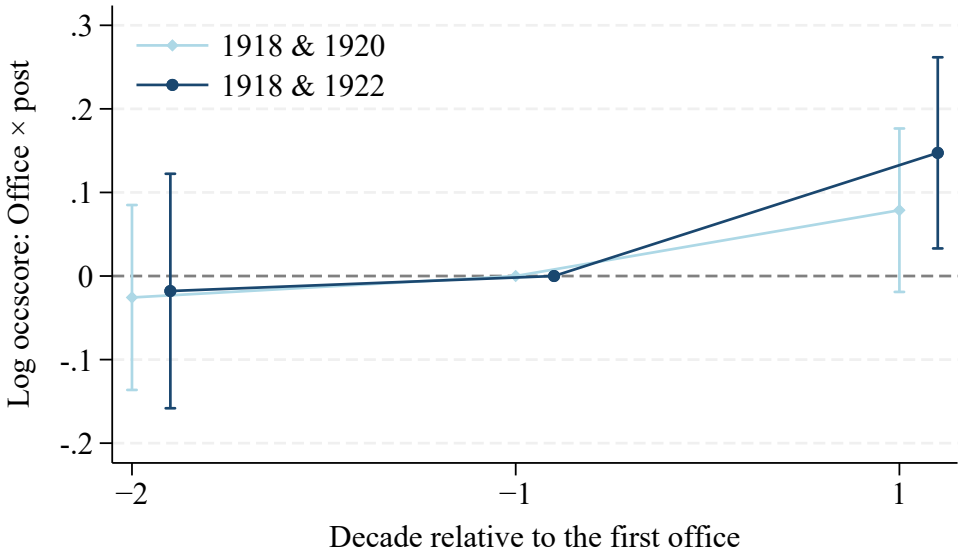


Figure D1. Dynamic Effect of Office on Son's Occupational Income

Note: This figure presents the dynamics of sons' occupational income using sample of sons of officials (starting offices in 1918) and sons of officials in the near future (starting offices in 1920 or 1922). The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

Table D2. Validity: Restricting to Officials First Observed After 1920

	Income Score$_{it}$ (Logged)			Employment$_{it}$			
	(1) Occ score	(2) Occ score	(3) Occ score	(4) White collar	(5) Skilled blue collar	(6) Low skill	(7) Agricultural
Office$_{j(i)}$ × Post$_{it}$	0.031***	0.024***	0.022**	0.022***	-0.015*	-0.001	-0.007
	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)	(0.011)	(0.009)
Office$_{j(i)}$	0.005						
	(0.010)						
Age bin FE	Y	Y	Y	Y	Y	Y	Y
Individual FE		Y	Y	Y	Y	Y	Y
County FE	Y	Y					
Year FE	Y	Y					
County × Year FE			Y	Y	Y	Y	Y
Mean	22.585	22.585	22.585	0.224	0.149	0.383	0.244
R^2	0.164	0.721	0.730	0.725	0.604	0.589	0.665
Observations	32053	32053	32053	32053	32053	32053	32053

Note: This table reports regression estimates of the effect of political office on sons' labor market outcomes in the sample of sons' officials who start their office after 1920 and their matched controls. The unit of observation is at the individual-year level. The dependent variables for Columns (1) to (3) are (log) occupational income score; and for (4) to (7) are binary variables denoting being employed in white-collar jobs, skilled blue-collar jobs, low-skill jobs, and agricultural-related jobs. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office early is a binary variable equal to 1 if the individual's father starts the office before 1920, and 1 otherwise. Post$_{it}$ is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table D3. Validity: Restricting to Young Sons

	Income Score$_{it}$ (Logged)			Employment$_{it}$			
	(1) Occ score	(2) Occ score	(3) Occ score	(4) White collar	(5) Skilled blue collar	(6) Low skill	(7) Agricultural
Office$_{j(i)}$ × Post$_{it}$	0.045***	0.039***	0.038***	0.039***	-0.017**	-0.018	-0.004
	(0.010)	(0.010)	(0.010)	(0.008)	(0.008)	(0.011)	(0.009)
Office$_{j(i)}$	-0.008						
	(0.010)						
Age bin FE	Y	Y	Y	Y	Y	Y	Y
Individual FE		Y	Y	Y	Y	Y	Y
County FE	Y	Y					
Year FE	Y	Y					
County × Year FE			Y	Y	Y	Y	Y
Mean	22.133	22.133	22.133	0.202	0.143	0.399	0.256
R^2	0.180	0.726	0.734	0.725	0.604	0.591	0.657
Observations	44778	44778	44778	44778	44778	44778	44778

Note: This table reports regression estimates of the effect of political office on sons' labor market outcomes in the sample of sons' officials who were young when fathers entered the office. The unit of observation is at the individual-year level. The dependent variables for Columns (1) to (3) are (log) occupational income score; and for (4) to (7) are binary variables denoting being employed in white-collar jobs, skilled blue-collar jobs, low-skill jobs, and agricultural-related jobs. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office early is a binary variable equal to 1 if the individual's father starts the office before 1920, and 1 otherwise. Post$_{it}$ is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table D4. Validity: Excluding Potential Relatives from the Control Group

	Income Score$_{it}$ (Logged)			Employment$_{it}$			
	(1) Occ score	(2) Occ score	(3) Occ score	(4) White collar	(5) Skilled blue collar	(6) Low skill	(7) Agricultural
Office$_{j(i)}$ × Post$_{it}$	0.030***	0.023***	0.022***	0.023***	-0.008	-0.005	-0.009
	(0.008)	(0.007)	(0.007)	(0.006)	(0.006)	(0.009)	(0.007)
Office$_{j(i)}$	0.006						
	(0.008)						
Age bin FE	Y	Y	Y	Y	Y	Y	Y
Individual FE		Y	Y	Y	Y	Y	Y
County FE	Y	Y					
Year FE	Y	Y					
County × Year FE			Y	Y	Y	Y	Y
Mean	22.401	22.401	22.401	0.214	0.152	0.384	0.249
R^2	0.185	0.719	0.725	0.715	0.595	0.570	0.653
Observations	52514	52514	52514	52514	52514	52514	52514

Note: This table reports regression estimates of the effect of political office on sons' labor market outcomes in the sample excluding individuals who share the same last name with the officials in the county from the control group. The unit of observation is at the individual-year level. The dependent variables for Columns (1) to (3) are (log) occupational income score; and for (4) to (7) are binary variables denoting being employed in white-collar jobs, skilled blue-collar jobs, low-skill jobs, and agricultural-related jobs. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office early is a binary variable equal to 1 if the individual's father starts the office before 1920, and 1 otherwise. Post$_{it}$ is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

E Intergenerational Impact of Office

E.1 Intergenerational Impact on Sons

Table E1. Intergeneration impact on sons' wage income

	Log Wage Income	
	(1)	(2)
$\text{Office}_{j(i)}$	0.129***	0.046**
	(0.023)	(0.021)
Birth Cohort FE	Y	Y
County FE	Y	Y
Pair FE	Y	Y
Occupation FE	N	Y
Control Mean	1287.695	1290.578
R^2	0.615	0.731
Observations	11176	11176

Note: This table reports regression estimates of the effect of political office on sons' wage income. The unit of observation is at the father level. The dependent variables are log wage income. The sample includes all matched individuals observed in the 1940 Census. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. Individual controls include the matched variables of both the individual and father's characteristics described in Section 3.1 and whether the individual is in an urban area. The standard errors are clustered at the matched pair level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

E.2 Intergenerational Impact on Daughters

Table E2. Intergeneration Impact on Sons' Marital Outcomes

	Wife's Father		
	(1)	(2)	(3)
	Log occscore	White-collar	Public sector
$\text{Office}_{j(i)} \times \text{Post}_{it}$	-0.058	-0.038	-0.002
	(0.046)	(0.039)	(0.008)
$\text{Office}_{j(i)}$	0.012	0.025*	0.005
	(0.017)	(0.013)	(0.004)
Age bin FE	Y	Y	Y
Pair FE	Y	Y	Y
County × Year FE	Y	Y	Y
Mean	18.797	0.097	0.007
R^2	0.661	0.626	0.600
Observations	8768	9005	8733

Note: This table reports regression estimates of the effect of political office on sons' probability of getting married. The unit of observation is at the individual-year level. The dependent variable is a binary variable of marital status equal to 1 if the individual is married in year t and 0 otherwise. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

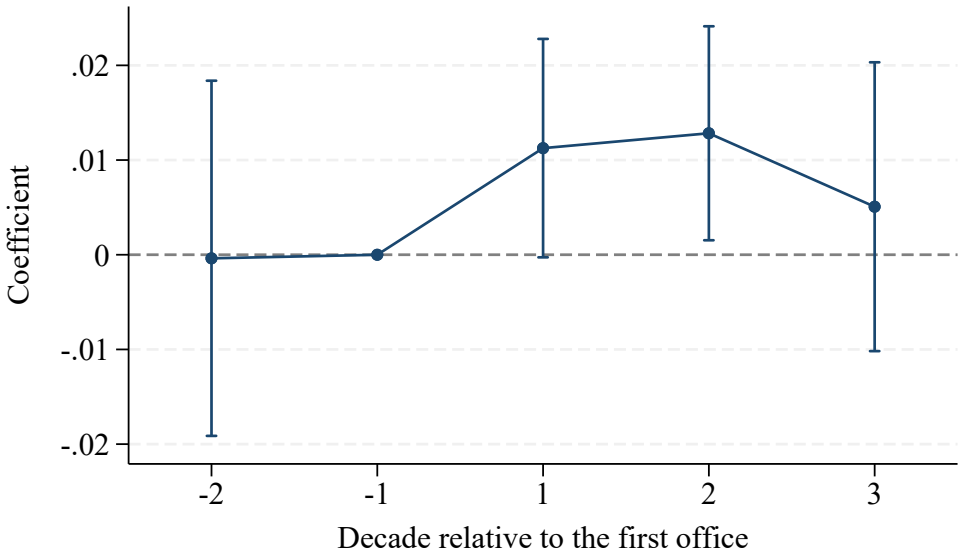


Figure E1. Dynamic Effect of Office on Son's Marital Outcome

Note: This figure presents the dynamics of sons' marital probability from estimating Equation 2. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

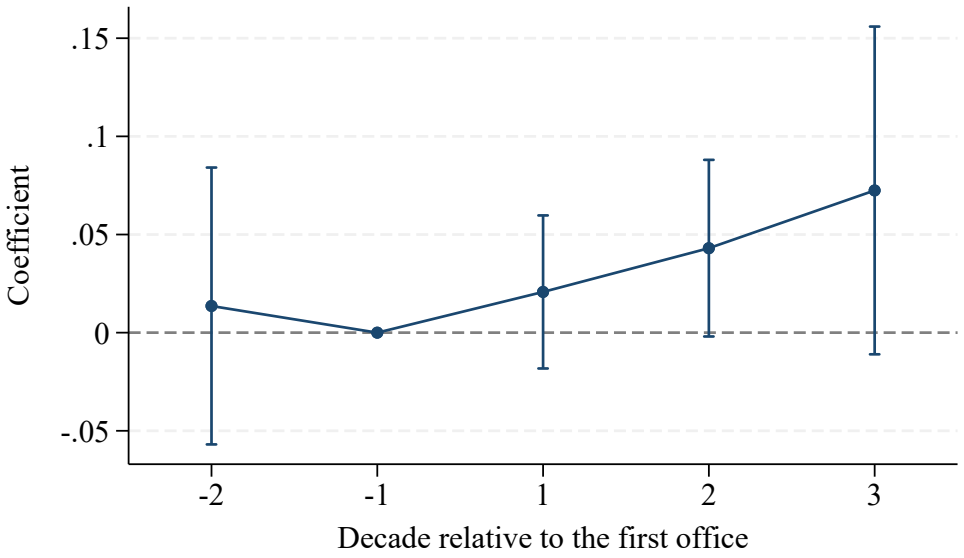


Figure E2. Dynamic Effect of Office on Daughters' Occupational Income

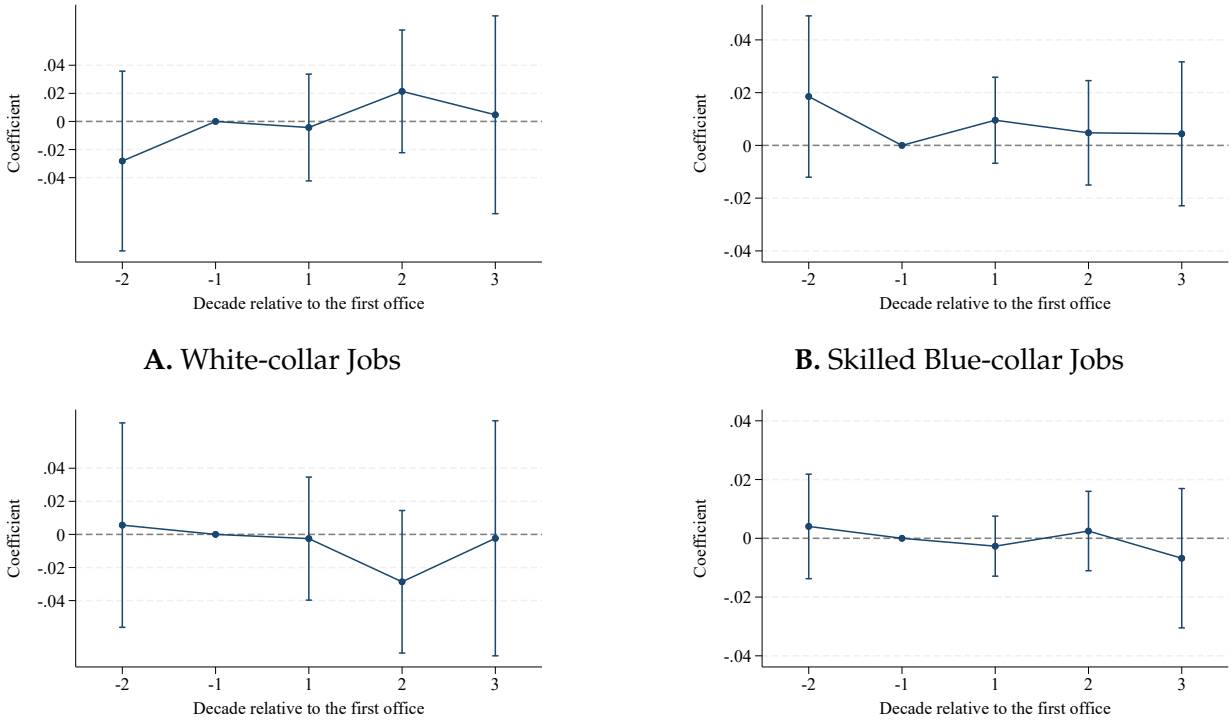
Note: This figure presents the dynamics of daughters' occupational income from estimating Equation 2. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

Table E3. Intergeneration impact on daughters' labor market outcomes

	Income Score$_{it}$ (Logged)			Occscore$_{it}$	Employment$_{it}$			
	(1) Occ score	(2) Occ score	(3) Occ score	(4) Occ score	(5) White collar	(6) Skilled blue collar	(7) Low skill	(8) Agricultural
Office$_{j(i)}$ × Post$_{it}$	0.006	0.031*	0.032*	-0.034	0.011	0.004	-0.014	-0.002
	(0.017)	(0.019)	(0.019)	(0.141)	(0.018)	(0.008)	(0.018)	(0.005)
Office$_{j(i)}$	0.029**							
	(0.013)							
Mean of Occscore					24.769	29.842	12.182	15.297
Age bin FE	Y	Y	Y	Y	Y	Y	Y	Y
Individual FE		Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y						
Year FE	Y	Y						
County × Year FE			Y	Y	Y	Y	Y	Y
Control Mean	20.961	22.128	22.132	3.072	0.646	0.021	0.319	0.015
R^2	0.049	0.752	0.781	0.536	0.818	0.605	0.808	0.658
Observations	15764	15764	15764	94853	15764	15764	15764	15764

Note: This table reports regression estimates of the effect of political office on daughters' labor market outcomes if the daughters are in the labor market. The unit of observation is at the individual-year level. The dependent variables for Columns (1) to (3) are (log) occupational income score; and for (4) to (7) are binary variables denoting being employed in white-collar jobs, skilled blue-collar jobs, low-skill jobs, and agricultural-related jobs. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post$_{it}$ is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.

* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$



A. White-collar Jobs

B. Skilled Blue-collar Jobs

C. Low-skilled Jobs

D. Agricultural Jobs

Figure E3. Dynamic Effect of Office on Daughters' Occupation

Note: This figure presents the dynamics of daughters' occupation category from estimating Equation 2. Panel A, B, C, and D separately present the dynamic effect of office on the probability of working in white-collar jobs, skilled blue-collar jobs, low-skilled jobs, and agricultural jobs. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

E.3 Robustness Check

Table E4. Robustness: Alternative Inference

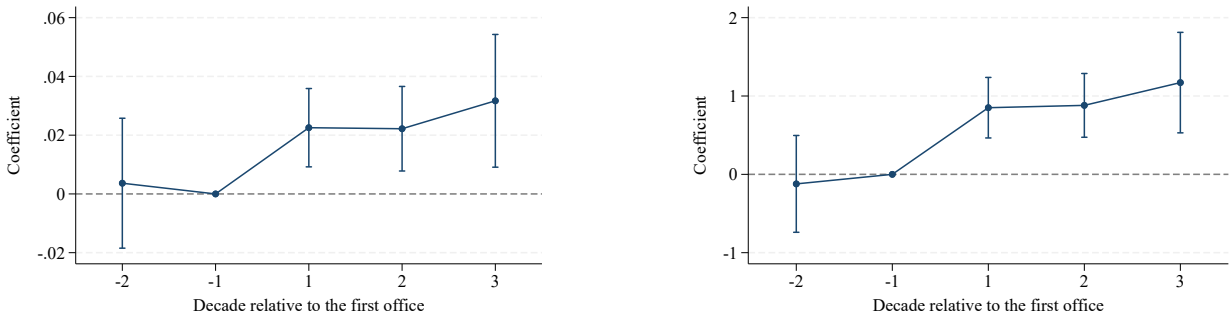
	Clustering at the level of			
	(1) Father	(2) Individual	(3) Matched pair	(4) County
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.0225***	0.0225***	0.0225***	0.0225***
	(0.0061)	(0.0060)	(0.0062)	(0.0055)
Age bin FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y
Mean	22.881	22.881	22.881	22.881
R^2	0.719	0.719	0.719	0.719
Observations	81981	81981	81981	81981

Note: This table reports regression estimates of the effect of political office on sons' occupational income with standard errors clustered at different levels. The unit of observation is at the individual-year level. The dependent variables are logged occupational income score. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors in Column (1) to (4) are separately clustered at the level of fathers, the level of individuals, matched pairs, and counties.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

Table E5. Robustness: Alternative Outcome Measures and Specification

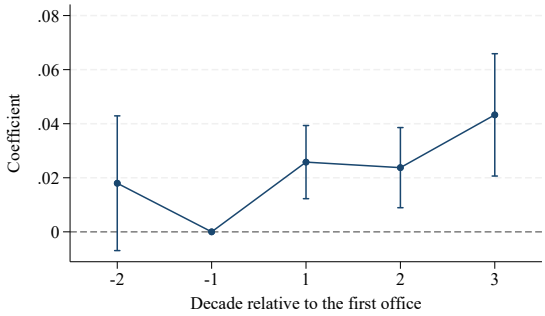
	IPUMS Occscore$_{it}$		LIDO Score$_{it}$		PPML	
	(1)	(2)	(3)	(4)	(5)	(6)
	Log	Level	Log	Level	IPUMS	LIDO
Office$_{j(i)}$ × Post$_{it}$	0.023***	0.922***	0.023***	0.614***	0.040***	0.027***
	(0.006)	(0.172)	(0.006)	(0.163)	(0.008)	(0.008)
Age bin FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y	Y	Y
Mean	22.881	21.084	22.237	21.385	21.159	21.751
R^2	0.719	0.610	0.677	0.596		
Observations	81981	89690	73306	76363	89690	76363

Note: This table reports regression estimates of the effect of political office on sons' occupational income using different outcome measures (Columns (1) - (4)) and estimation specifications (Columns (5) - (6)). The unit of observation is at the individual-year level. The dependent variables for Columns (1) to (2) are logged and level of occupational income score obtained from IPUMS; and for (3) to (4) are logged and level of Lasso-Industry-Demographic-Occupation (LIDO) score obtained from Saavedra and Twinam (2020). Note the sample size for LIDO score is smaller because Saavedra and Twinam (2020) constructed the score variable for individuals aged 25 and above. Columns (5) and (6) implement the pseudo Poisson maximum likelihood (PPML) estimation on outcomes of OCCSCORE and LIDO score. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post$_{it}$ is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$

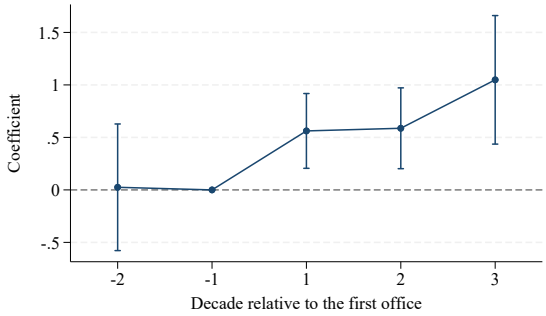


A. Log Occscore

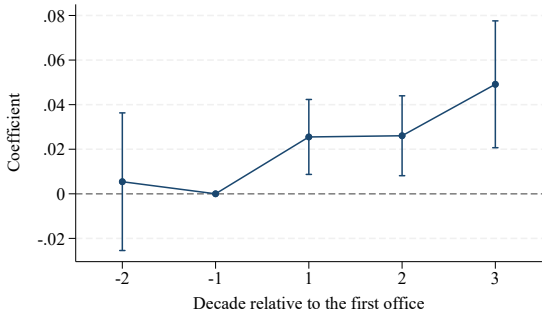
B. Occscore



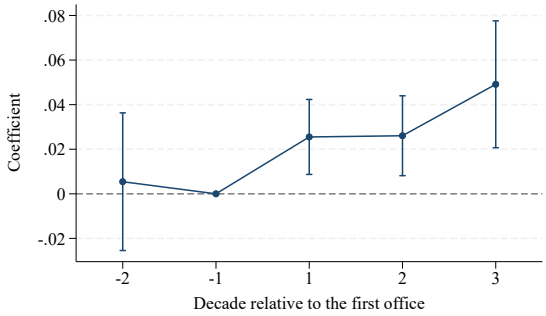
C. Log LIDO Score



D. LIDO Score



E. PPML: Occscore



F. PPML: LIDO Score

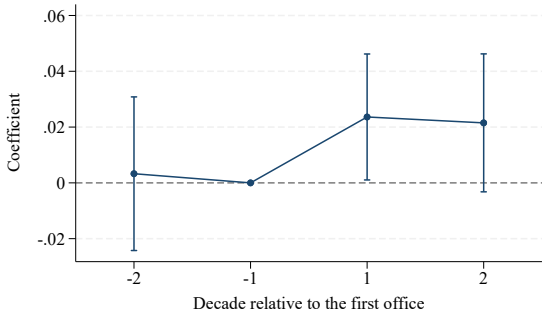
Figure E4. Robustness: Alternative Outcome Measures and Specification

Note: This figure presents the event study plots using alternative outcome measures and specifications. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

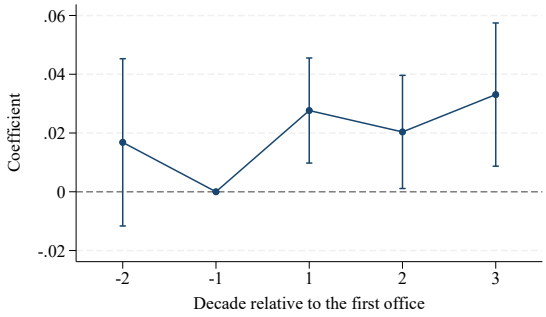
Table E6. Robustness: Alternative Sample

	(1) Balanced Panel	(2) Office 1914-1920	(3) Office 1920-1928
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.021**	0.022***	0.025***
	(0.010)	(0.008)	(0.008)
Age bin FE	Y	Y	Y
Individual FE	Y	Y	Y
County × Year FE	Y	Y	Y
Mean	22.921	22.624	23.073
R^2	0.725	0.717	0.727
Observations	20351	49928	49555

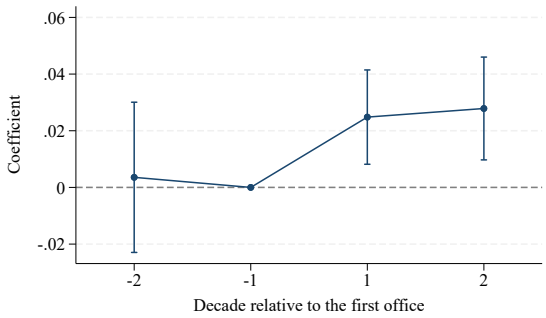
Note: This table reports regression estimates of the effect of political office on sons' occupational income using different samples. The dependent variables are log occupational income score. The unit of observation is at the individual-year level. Column (1) uses a fully balanced sample. Column (2) uses the sample of sons of officials who started their office between 1914 and 1920 and their matched counterparts. Column (3) uses the sample of sons of officials who started their office between 1922 and 1928 and their matched counterparts. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.
* $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$



A. Balanced Panel



B. Office 1914-1920



C. Office 1920-1928

Figure E5. Robustness: Alternative Sample

Note: This figure presents the event study plots using alternative samples. Note that for individuals whose fathers started offices after 1920, there are only 2 post periods given the access to the full count census in 1940. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

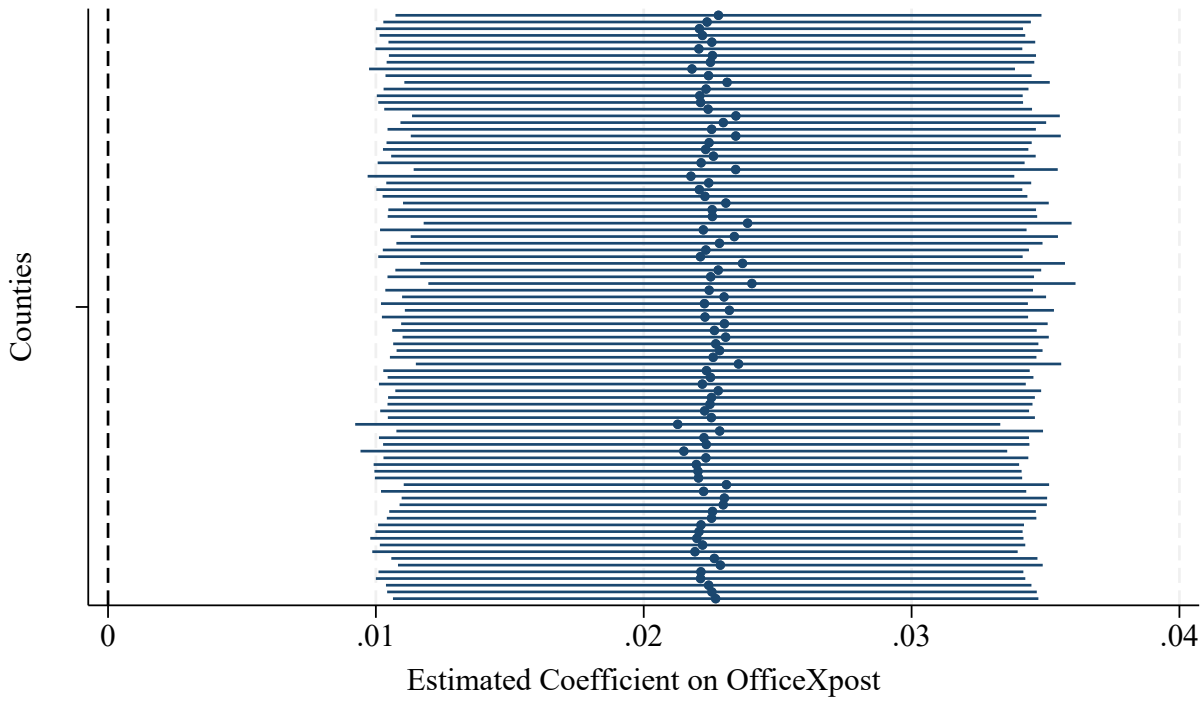


Figure E6. Robustness: Dropping One County Each Time

Note: This figure presents the coefficient on Office × Post from Equation 1, using the entire sample excluding one county at a time. The solid dots are point estimates, and the caps are the 95 percent confidence intervals. Standard errors clustered at the father level are used for constructing the confidence intervals.

F Other Potential Mechanisms

Table F1. Testing Occupational Inheritance

	Broad category		Detailed category	
	(1) Same industry	(2) Same occupation	(3) Same industry	(4) Same occupation
$\text{Office}_{j(i)} \times \text{Post}_{it}$	-0.006	0.003	-0.001	0.005
	(0.006)	(0.006)	(0.006)	(0.006)
Age bin FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y
Mean	0.408	0.298	0.376	0.260
R^2	0.630	0.562	0.652	0.578
Observations	89690	89690	89690	89690

Note: This table reports regression estimates of the effect of political office on the probability of getting into their fathers' pre-office ($t = -1$) jobs. The dependent variables are dummy variables equal to one if the individual is employed in their father's pre-office industry (Columns (1) and (3)) or occupation (Columns (2) and (4)). Columns (1) and (2) use the broader definition of industry and occupation, while Columns (3) and (4) use the finest definition of industry and occupation provided in IND1950 and OCC1950 variable in Censuses. The unit of observation is at the individual-year level. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.

Table F2. Testing Marriage Network

	Wife's Father		
	(1)	(2)	(3)
	Log occscore	White-collar	Public sector
$\text{Office}_{j(i)} \times \text{Post}_{it}$	-0.058	-0.038	-0.002
	(0.046)	(0.039)	(0.008)
$\text{Office}_{j(i)}$	0.012	0.025*	0.005
	(0.017)	(0.013)	(0.004)
Age bin FE	Y	Y	Y
Pair FE	Y	Y	Y
County × Year FE	Y	Y	Y
Mean	18.797	0.097	0.007
R^2	0.661	0.626	0.600
Observations	8768	9005	8733

Note: This table reports regression estimates of the effect of political office on sons' marital outcomes. The sample consists of married individuals and are therefore cross-sectional. Columns (1) to (3) examine characteristics of the wife's family in the first census where the son is observed as married: the log occupational income score (occscore) of the wife's father, whether the wife's father works in white-collar jobs, and whether the wife's father works in the public sector. The sample includes all matched individuals who are at least 18 years old in the pre-office census and the data is collected in the census 1900 to 1940. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the match pair level.

Table F3. Testing Reputation Channel

	Log Occupational Income		Private-sector White-collar	
	(1)	(2)	(3)	(4)
	Common	Rare	Common	Rare
$\text{Office}_{j(i)} \times \text{Post}_{it}$	0.023***	0.020**	0.013*	0.021***
	(0.008)	(0.009)	(0.008)	(0.008)
Age bin FE	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y
County × Year FE	Y	Y	Y	Y
Mean	22.7594	22.9564	0.2104	0.2209
R^2	0.7211	0.7240	0.6400	0.6464
Observations	39055	39850	41990	42774

Note: This table reports regression estimates of the effect of political office on sons' occupational income and probability of getting employed in white-collar jobs in the private sector by the frequency of their last names. The dependent variables for Columns (1) and (2) are sons' log occupational income score, and for Columns (3) and (4) are dummy variables with 1 denoting sons employed in white-collar jobs in the private sector. Samples in Columns (1) and (3) include sons of officials with common surnames defined by with surname frequency larger than the median of the surname frequency; while Columns (2) and (4) include sons of officials with less common surnames defined by with surname frequency lower than the median of the surname frequency. The unit of observation is at the individual-year level. Office is a binary variable equal to 1 if the individual's father is ever a local official, and 0 otherwise. Post_{it} is a dummy equal to 0 if i's father j hasn't started the term in year t, and 1 otherwise. The standard errors are clustered at the father level.