CENTRO STUDI LUCA D'AGLIANO

WWW.DAGLIANO.UNIMI.IT

CENTRO STUDI LUCA D'AGLIANO
DEVELOPMENT STUDIES WORKING PAPERS

N. 514

August 2026

The Private Returns to Public Service: Evidence from Municipal Councils

Drini Imami *
Mariapia Mendola **
Luca J. Uberti ***

* Agricultural University of Tirana

** University of Milano-Bicocca, CEPR and IZA

*** Centro Studi L.d'Agliano

ISSN 2282-5452

The Private Returns to Public Service: Evidence from Municipal Councils*

Drini Imami † Mariapia Mendola‡ Luca J. Uberti§

August 1, 2026

Abstract

This paper studies the private labor-market returns to holding local elected office. We focus on municipal councillors in Albania, where local elected officials receive only symbolic compensation and have no formal authority over public hiring or procurement. Exploiting a local randomization design around party-list election thresholds in the 2019 municipal elections and linking electoral records to universe-level taxpayer data 19 months later, we find that winning a council seat increases employment by about 9 percentage points and substantially raises earnings. The pattern of results points to political connections, rather than human-capital accumulation or legislative bargaining, as the primary mechanism. Labor market premiums are concentrated in low-ranking public-sector positions, particularly in central-government institutions, and are significantly larger in municipalities with higher corruption and less competitive labor markets. Our findings suggest that even politically weak offices generate substantial private returns by creating access to politically mediated employment networks.

JEL: J45, D72, D73

Keywords: Public-sector employment; political connections; social capital; local randomization

*We are grateful to Florinda Hasa and Anila Rustani for excellent research assistance, and to the University of Milano-Bicocca for financial support for data collection. The usual disclaimer applies.

†Agricultural University of Tirana. Email: dimami@ubt.edu.al

‡University of Milano-Bicocca, CEPR and IZA. Email: mariapia.mendola@unimib.it

§Centro Studi L.d'Agliano. Email: luca.jacopo.uberti@gmail.com

1 Introduction

One of the oldest questions in political economy, which goes as far back as Plato's *Republic*, is why individuals seek public office (Downs, 1957; Besley, 2004; Caselli and Morelli, 2004). While national politicians often enjoy substantial salaries, influence over public resources, and opportunities for career advancement, many local elected positions offer none of these advantages. In several institutional settings, municipal councillors receive only symbolic remuneration, hold little executive authority, and have no formal control over public contracts or personnel. Yet competition for these seemingly powerless offices remains intense. Why do individuals value these positions so highly? Is political office attractive because it generates private returns and, if so, through which mechanisms?

Answering these questions has implications well beyond the careers of elected politicians. Political connections shape firms' access to credit, procurement contracts, regulation, and public resources, making them a central object of study in political economy. Yet remarkably little is known about whether political office itself creates economically valuable connections for those who hold it. More fundamentally, identifying the mechanism through which these returns arise matters for how we think about the interaction between politics and labor markets. If holding public office primarily builds human capital - through experience, access to information, or managerial skills - then subsequent labor-market gains reflect increases in labor productivity. If, instead, office primarily creates access to influential networks, then even relatively unimportant elective positions may become stepping stones into politically mediated gainful employment. Such a mechanism has implications not only for political selection, but also for patronage, meritocratic recruitment, bureaucratic quality, and ultimately the allocation of talent in the economy.

This paper provides causal evidence on the labor-market returns to holding local elected office using a setting that sharply isolates the economic value of political office from other potential sources of political rents. We study municipal councillors in Albania, a country where local representatives only receive a symbolic stipend, have no formal authority to appoint public employees and remain free to obtain full-time jobs. Because the office itself offers neither substantial income nor executive power, it provides an unusually clean setting for identifying the economic value of elective positions even when office confers limited formal authority and few private benefits.

Our empirical strategy exploits a local randomization design that compares candidates just above and just below party-list election thresholds in the 2019 municipal elections (Cattaneo et al., 2015, 2016). Under Albania's closed-list proportional representation system, candidates are elected according to their position on electoral lists drawn by political parties. Yet, uncertainty over parties' electoral performance limits party leaders' ability to place candidates precisely above or below the threshold for election. We therefore compare candidates who narrowly won a council seat with candidates who narrowly missed election, precisely linking candidates' information to the universe of taxpayer records nineteen months after the election. Validation exercises show that marginally elected and unelected candidates are similar across a broad range of observable characteristics. Thus, our design credibly approximates a local randomized experiment and allows us to estimate the causal effect of holding elected office on subsequent labor-market outcomes.

We find that winning a municipal council seat increases formal employment by approximately 9 percentage points and raises monthly earnings by almost 90 percent one and a half years later. These effects are economically large and are not influenced by the modest remuneration paid by the office itself. Moreover, the pattern of results provides strong evidence on the underlying mechanism. The labor market premium is concentrated entirely in wage employment rather than self-employment, arises exclusively in the public sector, and is associated with relatively low-ranking positions rather than managerial occupations. Remarkably, the gains occur primarily in central-government institutions, where mayors have no formal authority over hiring decisions. These findings are difficult to reconcile with explanations based on human-capital accumulation or a direct exchange of legislative support by councilors for mayor-appointed municipal jobs.

Instead, the evidence consistently points toward political social capital as the main channel. Holding office exposes councillors to repeated interactions with mayors, civil servants, party officials, and other influential local actors, enabling them to accumulate valuable political connections that subsequently improve access to employment opportunities. Consistent with this interpretation, the labor-market premium is substantially larger in municipalities characterized by higher levels of corruption, where hiring decisions are more likely to rely on informal networks, and in thinner labor markets, where employers possess greater discretion over hiring decisions and personal connections become more valuable. The labor market premium of elected councillors therefore appears to reflect the broader value of political networks outside municipal government.

This paper contributes to several strands of political economy literature. First, it relates to studies that specifically examine political selection and political careers, addressing why individuals enter politics and how the incentives associated with public office shape the quality of political leaders (Besley, 2004, 2005; Caselli and Morelli, 2004; Gagliarducci and Nannicini, 2013; Fisman et al., 2015; Daniele, 2019). A central insight from this literature is that the private rewards (or costs) associated with political office affect the choice to undertake a political career and, ultimately, the quality of elected representatives. While previous work has primarily emphasized monetary compensation, electoral incentives and career pathways, our findings point to the accumulation of valuable personal connections as a complementary self-interested motivation for entering politics.

Second, the paper contributes to the literature on the private returns to political office. Previous studies have documented substantial economic benefits from political careers, including higher earnings while in office, improved labor-market opportunities after leaving politics (as in the 'revolving door' phenomenon), and the persistence of political dynasties (Dal Bó et al., 2009; Merlo et al., 2010; Cingano and Pinotti, 2013; Brollo et al., 2013; Fisman et al., 2014). Most of this evidence, however, concerns politicians who hold government posts with substantial executive authority or political influence, and whose labor market gains materialize only after leaving office. We show that sizeable private returns also accrue from positions that confer neither meaningful policymaking power nor attractive remuneration. Moreover, we argue that these gains can arise during, rather than only after, an officeholder's tenure.

Third, the paper contributes to the literature on the value of political connections. A large body of research shows that political connections create private value by improving firms' access to credit, procurement contracts and favourable regulations (Fisman, 2001; Khwaja and Mian, 2005; Faccio, 2006; Jayachandran, 2006). More recent work has emphasized the role of political networks in shaping bureaucratic careers and public-sector hiring (e.g. Colonnelli et al., 2020; Brassiolo et al., 2020). Our findings identify a complementary mechanism operating at the individual level. Rather than treating political connections as a pre-existing asset, we show that political office itself generates political connections that can subsequently be transformed into labor-market opportunities. In this sense, political office functions as an investment in political social capital.

Finally, the paper relates to the broad literature on political patronage and public employment. Existing work has shown how politicians use public jobs to reward campaign supporters (Bazzi and Labanca, 2026; Colonnelli et al., 2020), political allies (Barbosa and Ferreira, 2023) and family members (Folke et al., 2017; Gagliarducci and Manacorda, 2020). Here, we highlight a complementary mechanism. Focusing on elected officials who possess virtually no power to distribute patronage, our findings show that office holders themselves can benefit from the political networks accumulated while in office. The fact that these returns are largest in municipalities characterized by higher corruption and less competitive labor markets further suggests that political connections substitute for market-based allocation where institutions are weak.

The paper proceeds as follows. Section 2 describes Albania's municipal electoral system and institutional context. Section 3 presents the data and empirical strategy. Section 4 estimates the labor-market returns to holding municipal office. Section 5 investigates the mechanisms underlying these returns, focusing on political social capital, corruption, bargaining, and labor-market competition. Section 6 concludes.

2 Institutional setting

2.1 Local elections in Albania

In Albania, local elections are held every four years. Voters elect 61 mayors (*kryetarë bashkie*) and 1,619 municipal councillors (*këshilltarë bashkiakë*) across the country's 61 municipalities (*bashki*). Mayors hold executive power and are elected under first-past-the-post rules. Councillors form the municipality's legislative body and are elected through a closed-list proportional representation system.

To compete in council elections, political parties submit ranked candidate lists to the Central Election Commission (*Komisioni Qendror i Zgjedhjeve,* or KQZ) 48 days before polling day. Candidate lists are expected to alternate male and female candidates to satisfy gender-balance provisions. While there are no restrictions on candidate numbers, lists tend to contain roughly as many candidates as there are council seats, which range from 15 to 61 depending on municipal population (mean = 27).[1] Seats are allocated proportionally across party lists using the D'Hondt method (OSCE, 2019). Within each list, candidates are elected in rank order until the party's seats are filled.[2] Although elected candidates may decline their seat, this occurs only very rarely. If a candidate does so, or is subsequently declared ineligible, the mandate passes to the next candidate on the party list. In the 2019 elections, no elected candidate declined to take office, and only two were disqualified *ex post*

[1]Electoral Code of the Republic of Albania (Law No. 10019, dated 29 December 2008).

[2]In Albanian local elections, political parties often form electoral coalitions to support mayoral candidates. For council elections, however, they typically run on separate lists under their own party labels. As a result, each list contains candidates from a single party, and seats are allocated according to the votes obtained by individual parties.

by KQZ (KRIIK, 2020).[3]

During the election campaign, regional party delegates are tasked with recruiting candidates with strong vote-mobilization potential.[4] Bargaining between delegates and prospective candidates generally results in electorally stronger candidates being assigned higher positions on the party list.[5] However, uncertainty about the party's eventual seat allocation is substantial. Large political parties often use or conduct opinion polls, which in Albania are typically subject to a margin of error of 2.5-3 percentage points (IDM, 2020; News24, 2025; RCC, 2025), corresponding to 1-2 seats in an average-size council, and more in larger councils.[6] This uncertainty prevents parties from precisely sorting candidates around the winning threshold based on their ability to attract votes. As a result, candidates immediately above and below the threshold may be considered marginal, since relatively small changes in the party's vote share determine whether they secure a seat or remain unelected.

The 2019 local elections, held on 30 June, unfolded amid a deep political crisis. The governing Socialist Party of Albania (PS) competed largely against smaller parties after the main opposition, led by the Democratic Party of Albania (PD), boycotted the vote. The PD accused Prime Minister Edi Rama's government of corruption and links with organized crime (Todd, 2019). The PD boycott dramatically reshaped the election's competitive landscape, creating opportunities for smaller parties to capture council seats previously held by the PD. At the same time, the new environment increased uncertainty over seat allocation, further limiting parties' ability to sort candidates strategically. In total, 43 parties contested the

[3] These were Jahir Cahani (AD, Has) and Ramadan Baho (PSD, Ura Vajgurore).

[4] Yet, the final decision on candidate selection always rests with the central party leadership, which is also responsible for submitting the lists to KQZ.

[5] Interview with political activist who helped a major party organize local elections in Shkoder, May 2026.

[6] In an average-size municipal council of 27 seats, one seat corresponds mechanically to about $1/27 = 3.7$ percentage points of the vote. A polling margin of error of 2.5–3 percentage points therefore implies uncertainty of roughly $27 \times 0.025 = 0.68$ to $27 \times 0.03 = 0.81$ seats in either direction, or about 1.3–1.6 seats across the full lower-to-upper confidence interval. In a 61-seat council (Tirana), the same polling error corresponds to about $61 \times 2 \times 0.025 = 3.05$ to $61 \times 2 \times 0.03 = 3.66$ seats across the full lower-to-upper interval. The exact seat change depends on the distribution of votes across parties and D'Hondt rounding, but this simple calculation shows that polling uncertainty is easily large enough to affect 1 to 4 marginal seats.

elections, with opposition parties running 7,790 candidates across municipalities and the PS running 1,776. Official turnout was 23%, far below the 48% recorded in the 2015 local elections.

The poll resulted in a landslide victory for the PS, which won 59 out of 61 mayoral contests.[7] Council elections, however, resulted in a far less polarized outcome, with the PS securing 67 percent of council seats (s.d. = 23 p.p.) in the average municipality. While international observers questioned the political legitimacy of the outcome, they generally described election procedures as peaceful and orderly (OSCE, 2019).

2.2 Municipal councils and local labor markets

Beyond overseeing the executive branch, municipal councils are responsible for approving budgets, local taxes, loans, the use and transfer of municipal property, and the creation of municipal enterprises.[8] Councillors meet at least monthly, typically discussing and voting on many decisions per session, which can exceed 20 in large municipalities. In performing these duties, councillors interact regularly with mayors, civil servants, party officials, business leaders, civil society actors, and constituents, creating opportunities to build and cultivate ties with influential local actors.

In practice, however, municipal councils are generally much weaker politically than mayors, who exercise strong executive authority over municipal administration and local resource allocation (Council of Europe, 2015). Much of this power stems from mayors' direct or indirect influence over municipal hiring. Formally, they appoint deputy mayors (*nënkryetarë bashkie*), heads of administrative units and senior advisors (Albanian Association of Municipalities, 2010). Informally, they are also known to exert substantial influence over appointments governed by civil-service procedures, resulting in a highly politicised local public

[7] The mayoral race in Finiq was won by MEGA, a Greek minority party, while in Shkodra it was won by an independent candidate formerly associated with the PD (Voltana Ademi).

[8] Law No. 139/2015 on Local Self-Government

administration (Bieber, 2020; Meyer-Sahling et al., 2025). In contrast, municipal councillors have no power to make appointments or hire staff.

Municipal council positions are not governed by employment contracts and are very modestly paid, with a fixed monthly stipend that usually amounts to just over 10% of the mayor's salary. In 2019, mayoral salaries ranged from 45,000 to 175,000 Albanian lek per month depending on municipality size – approximately €370–1,400 at the 2019 exchange rate.[9] As part-time elected officials, councillors are allowed to hold full-time jobs in both the private and public sector.

Albania is a middle-income country with a GDP per capita of US$6,069 in 2019, similar to Ecuador or Paraguay (World Bank, 2026). In that year, the average gross monthly wage was €426 (INSTAT, 2024), indicating that councillors' monthly stipends (€37-140) amounted to as little as 8–33 percent of average earnings. By 2021, the average gross monthly wage had increased to €467. The unemployment rate stood at 11.5 percent and labor force participation at less than 60 percent, implying that around 53 percent of the working-age population was in employment (World Bank, 2026).[10] In 2021, public-sector jobs accounted for 15 percent of total employment and carried, on average, a 32 percent wage premium relative to private-sector positions (INSTAT, 2024). Informal networks continue to play an important role in job search and worker-firm matching (Pere and Bartlett, 2019).

[9]https://www.balkanweb.com/en/qeveria-rrit-pagat-e-kryebashkiakeve-te-rinj-ja-sa-do-jete-paga-e-veliajt/#gsc.tab=0

[10]Following the progressive lifting of Covid-related mobility restrictions in mid-2020, Albania experienced a rapid increase in emigration, contributing to population decline and constraining domestic labor supply.

3 Empirical methods

3.1 Data

Information on municipal council candidates and party lists was obtained from KQZ.[11] Based on separate information published by KQZ, we also know which candidates were elected and subsequently took office as municipal councillors.[12] For each party list, the records provide the name and rank order of all candidates. In total, the 2019 elections were contested by 9,566 candidates across 535 municipal party lists.[13] On average, each list was composed of 17.9 candidates ($rank \in \{1,\ 2, .., 18\}$), of whom 4.3 were elected (median = 1). For each candidate, KQZ publishes a CV and a Self-declaration Form (*Formular Vetëdeklarimi*). These documents, which are often handwritten, contain each candidate's unique tax identifier (TIN) and rich demographic information, including gender, date and place of birth, residential address, educational qualifications and self-reported employment status as of May 2019.[14]

Focusing on the 356 party lists that won at least one seat, we manually coded these variables for up to five candidates immediately above and five candidates immediately below the election cutoff. We refer to this set of candidates as the [-5,+5] local randomization window (Cattaneo et al., 2015). If the cutoff was fewer than five positions from either the top or bottom of the party list, the resulting window is necessarily asymmetric. This procedure yields a

[11] https://cec.org.al/kandidate-per-keshilltar-bashkie/

[12] https://cec.org.al/wp-content/uploads/2019/07/Kandidatet-fitues-per-keshill-bashkiak.pdf

[13] Not all 43 political parties ran candidates in all 61 municipalities. On average, fewer than nine parties competed per municipality (= 535 / 61).

[14] From these data, we construct indicators for being female, being born after the fall of the communist regime, being born in a rural locality (i.e., outside the municipal seat), residing in the municipality where the candidate is politically active, having studied abroad, and being in active employment in 2019 (as self-reported on the CV). We also measure age in years and define an indicator for candidates born between January and August, who are likely to have started school one year earlier than candidates born later in the same calendar year. Lastly, we construct a measure of years of schooling that takes into account both the candidate's age and the evolution of the Albanian education system across cohorts. In a minority of cases, either the CV was not uploaded or the information provided is incomplete, leading to missing observations.

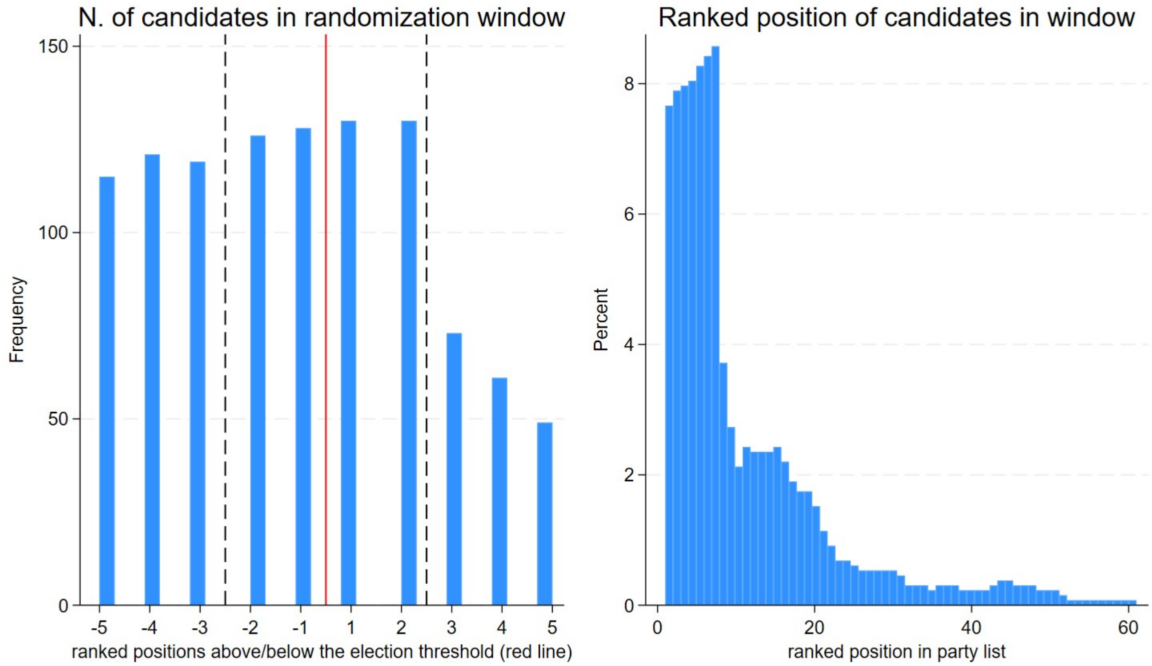
baseline sample of 2,469 candidates, comprising 753 marginally elected and 1,716 marginally unelected candidates across 30 parties. For the three largest parties (PS, PD, and PSD), we additionally coded the remaining candidates on each list, i.e. $(-\infty, -5) \cup (+5, +\infty)$. Appendix I shows that, although candidates inside and outside the [-5,+5] window present some notable differences, those inside the narrower [-2,+2] randomization window are broadly comparable to those outside in terms of both mean characteristics and the full distribution of observed covariates.

Next, we use the TIN numbers to exactly link candidates in the electoral data to their taxpayer records, which provide information on labor market outcomes. This data is collected by the General Tax Directorate (*Drejtoria e Përgjithshme e Tatimeve*) and is publicly available only for January 2021, approximately 19 months after the 2019 local elections. The records consist of a matched employer–employee dataset listing all formal employment relationships in Albania and reporting the wages paid under each employment contract.[15] Because employers are required to submit this information to the tax authorities on a monthly basis, the records cover the universe of formal employment. Candidates who do not appear in the January 2021 records are therefore classified as being out of formal employment in that month and assigned zero wages. Self-employed individuals are classified as in employment. Crucially, municipal councillor remuneration is not recorded in this dataset, ruling out the possibility that our estimates may be influenced by mechanical effects.

The dataset identifies employers by name but does not report their ownership status. To address this limitation, we manually coded indicators distinguishing public- and private-sector employers. Within the public sector, we further classified positions as belonging to either the central or municipal government. Finally, following the classification scheme proposed by Brassiolo et al. (2020), we distinguish between high-ranking positions (e.g., directors and high-skilled analysts) and low-ranking positions (e.g., laborers, assistants, and

[15]For individuals with multiple employment relationships, we calculated total wages earned across all jobs, as well as the number of jobs held.

FIGURE 1 - Candidates within the local randomization window



Notes: The left panel shows the frequency of candidates in the five ranked positions above and below the election cutoff (solid red vertical line). The dotted black lines represent the [-2, +2] window used in the main analysis (Table 2). The right panel presents the distribution of list ranks in the [-5, +5] window. Both panels use the estimation sample, excluding unmatched candidates and party lists in which the lead candidate is the only elected candidate.

low-skilled analysts).

Combining the electoral and taxpayer datasets yields a *matched sample* of 2,033 candidates from 315 municipal party lists. In total, 436 candidates from the KQZ data could not be matched to the taxpayer records due to missing or incorrect TINs.[16] In Appendix II, however, we show that TIN missingness is uncorrelated with being elected, mitigating concerns that sample selection may be a source of bias in our estimates.[17]

[16]Matching fails when KQZ records lack the Self-Declaration Form and, consequently, the candidate's TIN (299 cases); when the form is present but the TIN is missing (58); when the TIN is incorrectly recorded or contains missing digits (28); or when a passport number is reported instead of a TIN (37). We further exclude 14 candidates with duplicate TINs. When the records lack a Self-Declaration Form, candidates' characteristics are also unobserved. In addition, some covariates contain sporadic missing values arising from missing or unclear information on the Self-Declaration Form or a missing CV.

[17]Furthermore, Little (1988)'s test fails to reject the null that the missing values in the data matrix used in the main analysis (Table 2, column 7) are missing completely at random (MCAR) (p-value = 0.282), providing no evidence that the loss of observations systematically alters the composition of the estimation sample.

In January 2021, 54 percent of matched candidates were employed, or 46 percent of unelected candidates and 71 percent of elected candidates. Employed candidates earned, on average, 56,861 lek per month (€464 at the 2021 exchange rate), with mean wage earnings of €429 among the unelected and €518 among the elected. These figures are broadly in line with those of the general population (see section 2.2), suggesting that municipal council candidates are not part of a privileged sub-population.

Lastly, our main analysis excludes party lists in which the lead candidate is the only elected candidate. In Albanian local elections, lead candidates are often prominent public figures with the capacity to anchor the party locally (Jano, 2016). Consequently, they are likely to differ systematically from lower-ranked candidates. In the analysis, we confirm this anecdotal evidence empirically, but also show that our results are robust to including these lists in the sample.

Restricting the sample to lists with at least two elected candidates yields a final *estimation sample* of 1,052 matched candidates (442 marginally elected and 610 marginally unelected) across 136 party lists and 22 political parties. Figure 1 (left panel) displays the distribution of matched candidates in the estimation sample by distance from the election cutoff (solid vertical line), both above (+) and below (-) the threshold. The right panel in Figure 1 shows the distribution of overall party-list ranks within this [-5, +5] window.

3.2 Identification strategy and internal validity

Our identification strategy relies on the assumption that political parties cannot precisely manipulate candidates' positions relative to the election cutoff. This assumption implies that, within a narrow neighbourhood of the threshold, treatment (being elected) may be considered to be quasi-randomly assigned.

This framework addresses the concern that candidates placed higher on a party list are not only more likely to be elected, but may also possess unobserved attributes (leadership skills, local influence) that affect their labor market prospects. By focusing on marginally elected/unelected candidates, our design compares individuals who are likely to be similar in both observable and unobservable characteristics, thus approximating a local randomized experiment (Cattaneo et al., 2015, 2016).[18]

The estimating equation is:

$$Y_{imp} = \rho Y^0_{imp} + \beta elect_{imp} + \gamma X_{imp} + \theta_m \times \varphi_p + \varepsilon_{imp} \tag{1}$$

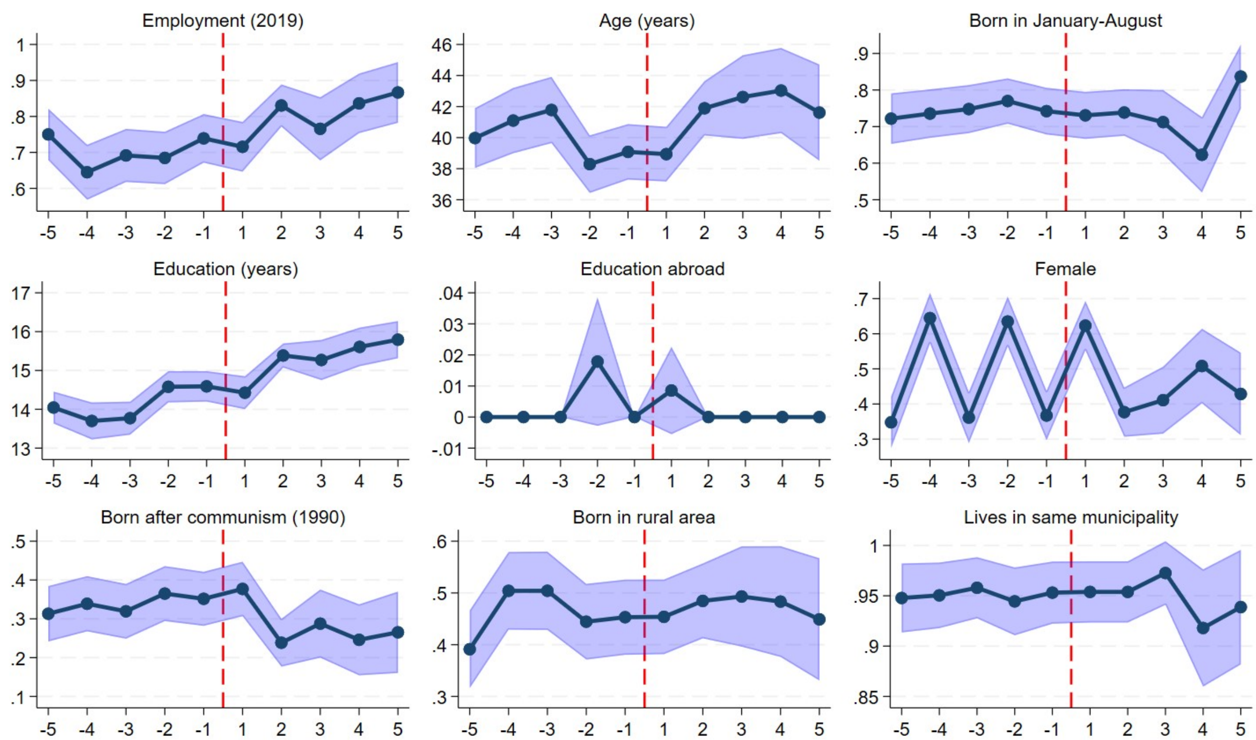
where i denotes marginal candidates running for councillor in municipality m for party p. Y_{imp} denotes the probability of being in employment (or other labor market outcomes) in January 2021, while Y^0_{imp} captures the candidate's employment status as of May 2019, that is, when candidates submit their CVs to KQZ. $elect_{imp}$ is an indicator variable that takes the value 1 if the candidate is (marginally) elected and subsequently takes office as municipal councillor, and 0 if the candidate remains unelected (or fails to assume office). X_{imp} is a vector of individual-level characteristics (see section 3.1); $\theta_m \times \varphi_p$ is a full set of party-list (municipality $\times$ party) fixed effects; and ε_{imp} is a random disturbance.

Under the assumption that parties cannot precisely manipulate candidates' positions relative to the cutoff, β can be interpreted as the causal effect of holding a council seat on candidates' labor market outcomes 1.5 years later. As argued in Section 2, this assumption is both plausible and non-restrictive in the Albanian context. A key testable implication of this assumption is that candidates' observable characteristics should be balanced on either side of the cutoff.

Figure 2 assesses this implication by plotting mean covariate values for candidates at

[18] In a standard RD design, treatment is assigned when a running variable crosses a known threshold. The conventional continuity-based approach identifies the treatment effect at the cutoff by assuming that potential outcomes—and, by implication, predetermined confounders—evolve smoothly through the threshold (Lee and Lemieux, 2010). Estimation relies on local polynomial regressions in the running variable using observations close to the cutoff. Cattaneo et al. (2015) distinguish the continuity-based approach from the local-randomization framework, which treats units within a narrow window around the cutoff as if treatment were randomly assigned. Thus, local randomization requires (more demandingly) that predetermined confounders be balanced within that window, and not merely smooth in the running variable. This framework is particularly useful when the running variable is discrete, since few support points near the cutoff can undermine continuity-based inference, as shown by Kolesár and Rothe (2018)

FIGURE 2 -Distribution of covariates around cutoff



Notes: The diagrams plot the mean value of individual characteristics for the five candidates immediately above (1 to 5) and below (-1 to -5) the election cutoff. Candidates above the cutoff were elected. Bands are presented for the 90 percent confidence interval of the means.

TABLE 1 - Local randomization tests

	(1)	(2)	(3)	(4)	(5)
Window:	[-5, +5]	[-4, +4]	[-3, +3]	[-2, +2]	[-1, +1]
Employment (2019)	0.087***	0.087***	0.062*	0.055	−0.024
	(0.029)	(0.031)	(0.035)	(0.041)	(0.059)
Age (years)	1.108	1.030	1.001	1.412	−0.147
	(0.808)	(0.878)	(0.970)	(1.091)	(1.526)
Born in January-August	−0.015	−0.034	−0.021	−0.017	−0.011
	(0.028)	(0.030)	(0.033)	(0.038)	(0.055)
Education (years)	1.002***	0.892***	0.639***	0.292	−0.165
	(0.168)	(0.185)	(0.204)	(0.236)	(0.352)
Education abroad	−0.001	−0.002	−0.003	−0.004	0.009
	(0.004)	(0.004)	(0.005)	(0.008)	(0.009)
Female	0.009	−0.013	0.030	0.008	0.255***
	(0.031)	(0.034)	(0.038)	(0.044)	(0.060)
Born after communism (1990)	−0.045	−0.047	−0.039	−0.045	0.025
	(0.029)	(0.031)	(0.035)	(0.042)	(0.060)
Born in rural area	0.009	−0.005	0.002	0.012	0.001
	(0.031)	(0.038)	(0.038)	(0.044)	(0.062)
Lives in same municipality	−0.001	0.000	0.006	0.004	0.001
	(0.014)	(0.015)	(0.016)	(0.019)	(0.026)
Test of joint significance [p-value]	[0.000]	[0.001]	[0.087]	[0.721]	[0.012]
Observations (range)	1,052-932	888-787	706-625	514-453	231-258

Notes: The table reports mean differences in the indicated variables between elected and unelected candidates, using progressively narrower windows around the election cutoff. Standard errors assume unequal variances. The number of observations varies across variables due to missing values. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

different distances from the cutoff (dashed vertical line). We consider all observed covariates described in Section 3.1. Although employment status in 2019 and educational attainment tend to increase for candidates placed higher on the party list, there is no evidence of a clear discontinuity in observed characteristics at the threshold. The only partial exception is the indicator for being born after the fall of communism, whose mean declines noticeably beginning with the next-to-last (+2) elected candidate.

Table 1 examines covariate balance more formally within progressively narrower symmetric windows around the cutoff. We report both individual balance tests and a joint significance test. Within the [-5,+5] window, elected candidates are both more educated and more likely to be employed already when running for councillors than non-elected candidates, consistent with the visual evidence (Figure 2). Covariate balance improves as the estimation window narrows around the cutoff. In the [-2,+2] window, which compares the last two elected candidates with the first two unelected candidates on each party list, none of the observed characteristics differ significantly across groups, and the joint test fails to reject the equality of means. Importantly, elected candidates were not significantly more likely to be in employment prior to treatment than non-elected candidates. Balance breaks down in the narrowest [-1,+1] window, ostensibly as a result of the statutory provision mandating gender alternation within party lists (see also Figure 2).

Taken together, the visual and statistical evidence indicates that the local randomization assumption is plausible within the [-2,+2] window. In our main analysis, we therefore restrict the sample to this narrow window, although we show that our results are robust to the use of wider or narrower windows. Since treatment assignment is assumed to be as good as random *within* each party list's relevant window, standard errors are adjusted for party-list stratification (Cochran, 1977).[19] In addition, our preferred specification includes strata (party-list) fixed effects to reflect the stratified assignment design.

[19]Inference remains unchanged when using heteroskedasticity-consistent standard errors.

TABLE 2 - The labor market performance of municipal councilors: [-2, +2] window

Dependent variable: Employment in..	2019	2021	2019	2021	2019	2021	2021
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Elected	0.055	0.108**	0.027	0.080**	0.017	0.087***	0.086***
	(0.041)	(0.042)	(0.040)	(0.039)	(0.030)	(0.031)	(0.030)
Employment in 2019							0.277***
							(0.048)
Individual controls	No	No	YES	YES	YES	YES	YES
Party list FE	No	No	No	No	YES	YES	YES
Mean dep. var., unelected	0.715	0.598	0.715	0.585	0.715	0.585	0.583
Adjusted-R^2	0.00	0.01	0.10	0.24	0.23	0.30	0.35
Party lists	119	136	119	120	119	120	119
Observations (candidates)	454	514	453	456	453	456	453

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality × party) level. The sample comprises the last two elected candidates and the first two unelected candidates − the [-2, +2] randomization window. Individual controls include: *age*, *age*^{2}, being born in January-August, years of schooling, having a foreign degree, gender, being born after the fall of communism (1990), being born in a rural area, and residing in the municipality in which the candidate is running for office.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4 The labor market premium of holding public office: Main results

We begin by examining the labor market premium associated with holding an elected councillor position. Table 2 reports estimates of β from equation (1) for progressively richer specifications. Models 1-2 correspond to unconditional regressions; models 3-4 condition on the full set of individual candidate characteristics; and models 5-6 add municipality × party fixed effects, leading to comparisons between marginally elected and unelected candidates belonging to the same municipal party list and with similar observed characteristics. Across all three specifications, we examine the impact of winning a seat in the council on both pre-treatment (Y^0_{imp}) and post-treatment outcomes (Y_{imp}). Lastly, the model reported in column 7 includes Y^0_{imp} as an additional control. This is our preferred specification used in the rest of the paper.

The results indicate that elected councillors began their terms with no significant labor market advantage, reinforcing our previous findings based on mean difference tests (section 3.2). Models 1, 3, and 5 provide no evidence that election to municipal office affected employment outcomes before the 2019 elections.[20] However, one and a half years into the mandate, holding a municipal council seat was associated with a statistically and economically significant labor market premium (see models 2,4 and 6). According to Model 7, our preferred specification, being elected to the municipal council increased the probability of having entered employment by January 2021 by almost 9 percentage points, corresponding to a 15 percent increase relative to the baseline employment rate among marginally unelected candidates.[21]

Under local randomization, the inclusion of covariates should improve the precision of the OLS estimator of β without affecting its expected value. Consistent with this prediction, the estimated coefficients remain relatively stable across specifications 4, 6 and 7, while their standard errors *decrease* in magnitude as additional controls are introduced and the regression's R-squared increases. This evidence further supports our causal interpretation of the estimates.

Robustness. We assess the robustness of our main findings in several ways. Table 3 shows that the estimated effecs of winning a seat in the council are not sensitive to the choice of randomization window (Panel A), and remains unaltered when using the full matched sample, including party lists in which the lead candidate was the sole winner (Panel B). In the $[-1, +1]$ window (Panel A), the estimate is no longer significant due to the loss of statistical power. Yet, its magnitude remains comparable to those obtained from wider windows. Including all matched candidates (Panel B) yields a larger estimated labor market premium, consistent with lead candidates having higher unobserved ability.

[20] Obviously, the unconditional regression in column 1 is identical to a mean difference test.

[21] The covariates enter this regression as expected: *age* displays a significant hump-shaped relationship with employment, while years of schooling enters positive and significant (0.061, s.e. = 0.008). Being born in a rural area marginally increases the probability of employment, as does living in the municipality of election. Full results available upon request.

TABLE 3 - Alternative windows and samples

Window:	[-5, +5]	[-4, +4]	[-3, +3]	[-2, +2]	[-1, +1]
	(1)	(2)	(3)	(4)	(5)
PANEL A: Estimation sample: Alternative randomization windows					
Elected	0.066***	0.080***	0.067**	0.086***	0.063
	(0.025)	(0.027)	(0.028)	(0.030)	(0.059)
Observations	927	783	624	453	230
PANEL B: Matched sample: including lists where only winner is lead candidate					
Elected	0.093***	0.103***	0.100***	0.118***	0.114***
	(0.021)	(0.022)	(0.022)	(0.022)	(0.041)
Observations	1,751	1,468	1,169	860	500

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality × party) level. The dependent variable is employment in 2021. All models control for a full set of individual characteristics, employment in 2019, and party list FE. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Next, we examine a range of alternative specifications (Appendix III). Excluding all lead candidates (rather than omitting the party lists where lead candidates are the only winners) leaves our main result unchanged, as does controlling separately for lead candidates. Controlling for a quadratic function of candidates' party-list rank yields very similar estimates. We also estimate a local linear regression that uses candidates' normalized rank on the party list as the running variable (see Lee and Lemieux (2010)). Normalized rank ($norm\_rank_{imp}$) is defined as $cutoff_{mp} - rank_{imp} + 1$, where $cutoff_{mp}$ is the rank of the last elected candidate in list mp. Treatment switches on ($elect_{imp} = 1$) when $norm\_rank_{imp} \geq 1$.[22] Including both $norm\_rank_{imp}$ and its interaction with $elect_{imp}$ yields an estimate of β that is very similar to our baseline (column 4). Furthermore, we restrict the sample to candidates that were not employed in 2019, exclude candidates affiliated with the PS (the ruling party at the national level), omit party lists for which the $[-2, +2]$ window is not symmetric, and drop municipalities in the bottom quartile of population.[23] The results remain unaltered throughout.

[22] As noted by Kolesár and Rothe (2018), however, this approach does not support valid inference as $norm\_rank_{imp}$ is not a continuous running variable.

[23] In municipalities with larger councils, uncertainty over marginal seats is greater, making our identifying assumption yet more credible (see section 2).

Likewise, using a logit (instead of a linear) specification of the conditional probability of employment leaves our conclusions unaffected (column 9).

An important concern relates to non-compliance with treatment assignment. In our setting, no elected candidate withdrew before taking office, and only two candidates were disqualified *ex post* by the KQZ (see section 2.1), making non-compliance both extremely rare and plausibly exogenous. Nevertheless, we also estimate the effect of being elected irrespective of whether the candidate ultimately assumed office as a councillor—that is, the intention-to-treat (ITT) effect (Appendix III, column 10). The ITT is nearly identical to our baseline as-treated estimate, suggesting that our findings are mostly unaffected by non-compliance.[24]

Next, in Appendix IV, we conduct a simple falsification exercise using placebo cutoffs to assess whether similar discontinuities in employment outcomes emerge at other points on the party lists where treatment status remains unchanged. The results indicate that a significant effect emerges only at the true threshold, when comparing the last two elected ($norm\_rank_{imp} = 1, 2$) with the first two unelected candidates ($norm\_rank_{imp} = -1, 0$). By contrast, shifting the cutoff spuriously upward or downward by one or two positions on the party list yields null effects.

Lastly, we conduct stratified permutation tests, following the recommendations of Cattaneo et al. (2016). Treatment is repeatedly reassigned within strata (party lists) to obtain p-values from the permutation distribution (Heß, 2017). For our preferred specification (Table 2, column 7), the permutation-based p-value is 0.018, compared with an asymptotic p-value of 0.004 based on stratification-adjusted standard errors. Under the null of no treatment effect, only 18 out of 1,000 random reassignments of treatment status within party lists yield an estimated effect of winning a council seat at least as large in absolute value as the observed effect. Thus, using randomization-based instead of design-based inference

[24]To compute the ITT, we modified the *elect* variable by recoding the two subsequently disqualified candidates as 1 and their two replacements as 0.

TABLE 4 - Alternative outcomes

Dependent variable:	Wage employment	Self-employment	Number of jobs		Tot. wage earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
Elected	0.081***	−0.007	0.076**	−0.010	0.955***	0.262**
	(0.031)	(0.019)	(0.037)	(0.015)	(0.363)	(0.131)
Employment (2021)				1.060***		
				(0.022)		
Number of jobs (2021)						9.112***
						(0.415)
Mean DV, unelected	0.539	0.075	0.570	0.570	6.25	6.25
Observations	453	453	453	453	453	453

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality $\times$ party) level. The dependent variables are dummies for having a wage-paying job (1) and being self-employed (2); the number of wage-paying jobs (3 and 4); and the inverse hyperbolic sine (*arcsinh*) of total earnings from wage employment (5 and 6). All models control for a full set of individual characteristics, employment in 2019, and party list FE. All models use the [-2, +2] randomization window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

procedures does not affect our conclusions.[25]

Alternative outcomes. Employment is defined as including both salaried and self-employment. Columns (1) and (2) of Table 4 show that elected candidates earn a labor-market premium entirely through salaried positions, which account for 92 percent of total employment among candidates. Because individuals may hold multiple jobs simultaneously[26], columns (3) and (4) use the number of jobs held as the outcome variable, instead of the employment indicator, without and with controls for employment status, respectively. The estimated effect of winning a seat remains qualitatively unchanged in column (3)[27], while the null effect in column (4) indicates that elected candidates do not benefit by securing additional jobs. Instead, their advantage stems from a higher likelihood of obtaining employment in the first place.

[25] Similarly, using heteroskedasticity-robust (0.037) instead of stratification-adjusted standard errors (0.030) leaves our conclusions qualitatively unchanged.

[26] In the estimation sample, only 11 candidates hold two (9) or three (2) jobs.

[27] Estimating a Poisson specification that accounts for the count nature of the outcome yields similar results, with a marginal effect of winning a seat of 0.084 (s.e. = 0.039) instead of 0.076 (s.e. = 0.037) in the OLS.

Columns (5) and (6) use the inverse hyperbolic sine of total wage earnings as the dependent variable, without and with a control for the number of jobs held, respectively. The results show that the labor-market premium of elected candidates translates into higher monthly earnings and operates at both the extensive and intensive margins. On average, elected candidates experience a 96 percent increase in monthly earnings, equivalent to approximately 32,000 lek (€260) per capita, or 60 percent of the average gross monthly wage in 2019. Even after conditioning on the number of jobs held, elected candidates earn approximately 26 percent more per position held than their non-elected counterparts (column 6), suggesting access to higher-paying jobs.

Taken together, these findings indicate that winning a municipal council seat not only increases officeholders' likelihood of securing a full-time job, but also enables them to obtain better-paid positions and, ultimately, higher earnings.

5 Mechanisms

We now turn to exploring the mechanism that allows elected councillors to realize a private gain from holding public office. We present evidence that elected councillors quickly accumulate social capital that can be converted into employment opportunities, especially in locations where corruption is more widespread. We also show that jobs are unlikely to be traded in exchange for legislative support, consistent with municipal councils being politically weak bodies in practice. Rather, the effect arises from the value of connections in imperfectly competitive labor markets.

5.1 Human or social capital?

A possible explanation for the findings reported in Table 2 is that elected candidates, but not unelected ones, accumulate valuable human capital while in office; this human capital quickly translates into higher productivity, employment and earnings. The evidence, however, is

TABLE 5 - Job types

Dependent variable: Having a job that is..	Skill level: High-skill (1)	Low-skill (2)	Employer type: Public adminis-tration (3)	Other than pub. admin. (4)	Level of government: Central gov't (5)	Municipl gov't (6)
Elected	−0.026	0.113***	0.067**	0.018	0.078***	−0.011
	(0.032)	(0.031)	(0.030)	(0.032)	(0.026)	(0.020)
Mean dep. var., unelected	0.395	0.189	0.285	0.298	0.211	0.075
Observations	453	453	453	453	453	453

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality × party) level. All models control for a full set of individual characteristics, employment in 2019, and party list FE. All models use the [-2, +2] randomization window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

difficult to reconcile with this interpretation. At the average age of unelected candidates (38.6 years), the effect of an additional 18 months of work experience, as proxied by age, on employment is essentially zero (0.2 p.p., s.e. = 0.5 p.p.) based on the estimates of model 7 in Table 2.[28] By contrast, 18 months of part-time service as a municipal councillor increase the probability of employment by as much as 9 p.p. This large discrepancy suggests that the premium earned by councillors is unlikely to arise from factors related to the accumulation of general human capital.

To further examine this possibility, Table 7 re-estimates model 7 in Table 2 separately by job type. Columns (1) and (2) distinguish between high- and low-skill occupations using the classification proposed by Brassiolo et al. (2020).[29] If the observed labor-market premium was the result of human capital, the effect should be stronger for high-skill occupations, where such capital is likely to be more valuable. The results, however, point in the opposite direction. Winning a seat on the municipal council increases the probability of being employed in a low-skill occupation 18 months later by 11 percentage points, but it has no

[28] The coefficients on *age* and age^2 in model 7 of Table 2 are not reported in full to save space. They are, respectively, 0.021 (s.e. = 0.012) and -0.0003 (s.e. = 0.0001) and are both significant at the 10 percent level.

[29] In columns (1) and (2), and likewise in columns (3) and (4), the two dependent variables are mutually exclusive and jointly exhaustive, such that their sum equals the overall employment indicator used as the outcome in Table 2.

discernible effect on employment in high-skill occupations.

Furthermore, if human capital accumulation were the main mechanism, the premium should be observable in both the public administration and the private sector. Instead, columns (3) and (4) show that it arises entirely from additional employment in the public administration—that is, the sector with which municipal councillors interact most closely while carrying out their duties. This finding provides more direct evidence that the labor-market returns to holding an elective position are likely to stem from the social capital generated while in office, rather than from the accumulation of general human capital.

Next, columns (5) and (6) focus on public-sector employment and distinguish between positions in institutions under the direct authority of municipalities and their mayors (e.g. municipal administrations, local agencies) and those under the central government (e.g. ministries, independent agencies, local or regional offices of central-government bodies). Interestingly, the employment gains associated with holding municipal office are concentrated entirely in central-government institutions (column 5), where mayors have limited direct influence over appointments. This finding suggests that the labor-market returns to serving as a municipal councillor are unlikely to be driven primarily by direct patronage from the mayor. Instead, they are consistent with the broader political connections acquired while in office.

5.2 Corruption and personal connections

If councillors extract private benefits from public office primarily by accumulating social capital, the estimated premium should be larger in municipalities with higher levels of corruption. In more corrupt environments, hiring decisions are less likely to be governed by meritocratic criteria and more likely to rely on patronage and informal networks, increasing the value of personal connections in accessing employment opportunities (Colonnelli et al., 2020; Meyer-Sahling and Mikkelsen, 2016).

Albania exhibits relatively high levels of perceived corruption by both regional and global standards. Transparency International's 2024 *Corruption Perceptions Index* assigns Albania a score of 39 out of 100 and ranks it 91st among 182 countries, below the global average score of 43. Nevertheless, national-level indicators are likely to conceal substantial variation in the prevalence of corruption across sub-national regions. To measure corruption at the municipality level, we use the *Local Governance Mapping* index published by the Institute for Development Research and Alternatives (IDRA), a Tirana-based NGO. The index combines citizen perceptions of corruption, self-reported experiences with corrupt practices, and assessments of local anti-corruption institutions and mechanisms, and is based on the UNDP *Local Governance Barometer* and *Citizen Report Card* methodologies (IDRA, 2017). The scores are normalized to a 0–100 scale where higher values indicate higher levels of corruption. In Appendix IV, we validate this measure by showing that it correlates negatively with both an indicator of municipal transparency (IDP, 2024) – a measure of local institutional quality – and the mean annual wage[30] – a proxy for the level of local economic development.[31]

We use the IDRA index to estimate a version of equation (1) that allows for heterogeneous effects by the level of municipal corruption:

$$Y_{imp} = \rho Y^0_{imp} + \beta_0 elect_{imp} + \beta_1 elect_{imp} \times f(corr_m) + \gamma X_{imp} + \theta_m \times \varphi_p + \varepsilon_{imp} \qquad (2)$$

where $f(.)$ denotes a polynomial function. The estimated employment effects of being marginally elected, $\beta_0 + \beta_1 \times f(corr_m)$, are plotted against $corr_m$ in Figure 3, using both a linear (left-hand panel) and a quadratic (right-hand panel) fit to the data. The results clearly

[30]The data on average gross wages at the municipality level are from INSTAT (2019)

[31]The lowest levels of corruption are observed in the municipalities of Lezhë, Mirditë, and Korçë (see Appendix IV). The first two are predominantly Catholic municipalities in otherwise Muslim-majority Albania, while Korça—home to the first Albanian-language school in Ottoman times—is known to have relatively high levels of educational attainment to this day. Previous studies have shown that political corruption correlates negatively with the quality of institutions, the level of economic development and the educational attainment of the population (Treisman, 2000). Islamic institutions have also been linked to higher exposure to corruption (Becker et al., 2016).

FIGURE 3 - Heterogeneous effects by level of corruption

Notes: The diagram reports the marginal effects of being elected on the probability of being in employment in 2021, as a function of municipality-level corruption. The average marginal effect is 0.087 (linear model) and 0.086 (quadratic model), in line with previous findings. The model uses the [-2, +2] randomization window (sample size is 453). A higher index score denotes a higher level of corruption.

show that the estimated labor-market premium is small and insignificant in low-corruption environments and becomes larger the higher the level of municipal corruption, consistent with the value of connections increasing in contexts where appointments are more likely to rely on patronage.

Lastly, Appendix VI shows that the estimated labor market advantage is smaller among candidates whose individual characteristics—such as being female, highly educated, or born after the fall of communism—are generally associated with a lower propensity to engage in corruption. Taken together, these findings confirm that the labor market premium of elected candidates is likely to arise from the value of connections in high-corruption environments.

5.3 Bargaining with mayors?

Municipal councillors may convert the social capital accumulated in office into employment opportunities through two channels. First, they may bargain with mayors and exchange legislative support to the mayor's programme for access to mayor-appointed jobs in the municipal administration. Second, they may leverage the broader networks developed while in office to secure positions elsewhere in the public sector, especially where hiring decisions are highly discretionary.

In the Albanian context, the second mechanism is likely to be more important than the first. While mayors exercise considerable influence over municipal hiring decisions, municipal councils are relatively weak bodies. As a result, mayors can often make and implement decisions informally without seeking support from the council, reducing the value of councillors' legislative backing as a bargaining currency in the allocation of municipal jobs. Consistent with this interpretation, Table 5 (columns 5–6) shows that the labor-market premium of councillors arises entirely from access to jobs in central-government institutions.

To further assess this interpretation, Table 6 examines whether the estimated effects vary with the political alignment between councillors and the mayor. If mayors exchange jobs for legislative support, councillors already aligned with the mayor should possess less bargaining power and therefore enjoy a smaller labor-market premium. Column 1 tests this hypothesis by augmenting Equation 1 with an interaction between $elect_{imp}$ and an indicator for candidates belonging to the mayor's party. Although political alignment reduces the estimated premium by nearly half, the interaction term is not statistically significant at conventional levels.

A related implication of the bargaining mechanism is that councillors should command a larger premium in municipalities where opposition parties hold a greater share of council seats, making their legislative support marginally more valuable to the mayor. However, column 2 provides little evidence in support of this interpretation. Using the opposition's

TABLE 6 - Mechanisms: Legislative bargaining and labor market concentration

Heterogeneity by:	Councilors' alignment	Opposition in council	labor market concentration		All
Dep. var.: Employment (2021)	(1)	(2)	(3)	(4)	(5)
Elected	0.112***	0.140**	0.190***	0.115***	0.479***
	(0.042)	(0.059)	(0.057)	(0.033)	(0.117)
Elected × politically aligned	−0.065				−0.089
	(0.061)				(0.063)
Elected × (% seats held by opposition)		-0.001			−0.004**
		(0.001)			(0.002)
Elected × (active firms/100 inhabitants)			−0.022*		−0.029**
			(0.011)		(0.013)
Elected × (active firms/km^2)				−0.005*	−0.006**
				(0.002)	(0.003)
Observations	453	453	453	453	453

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality × party) level. All models control for a full set of individual characteristics, employment in 2019, and party list FE. All models use the [-2, +2] randomization window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

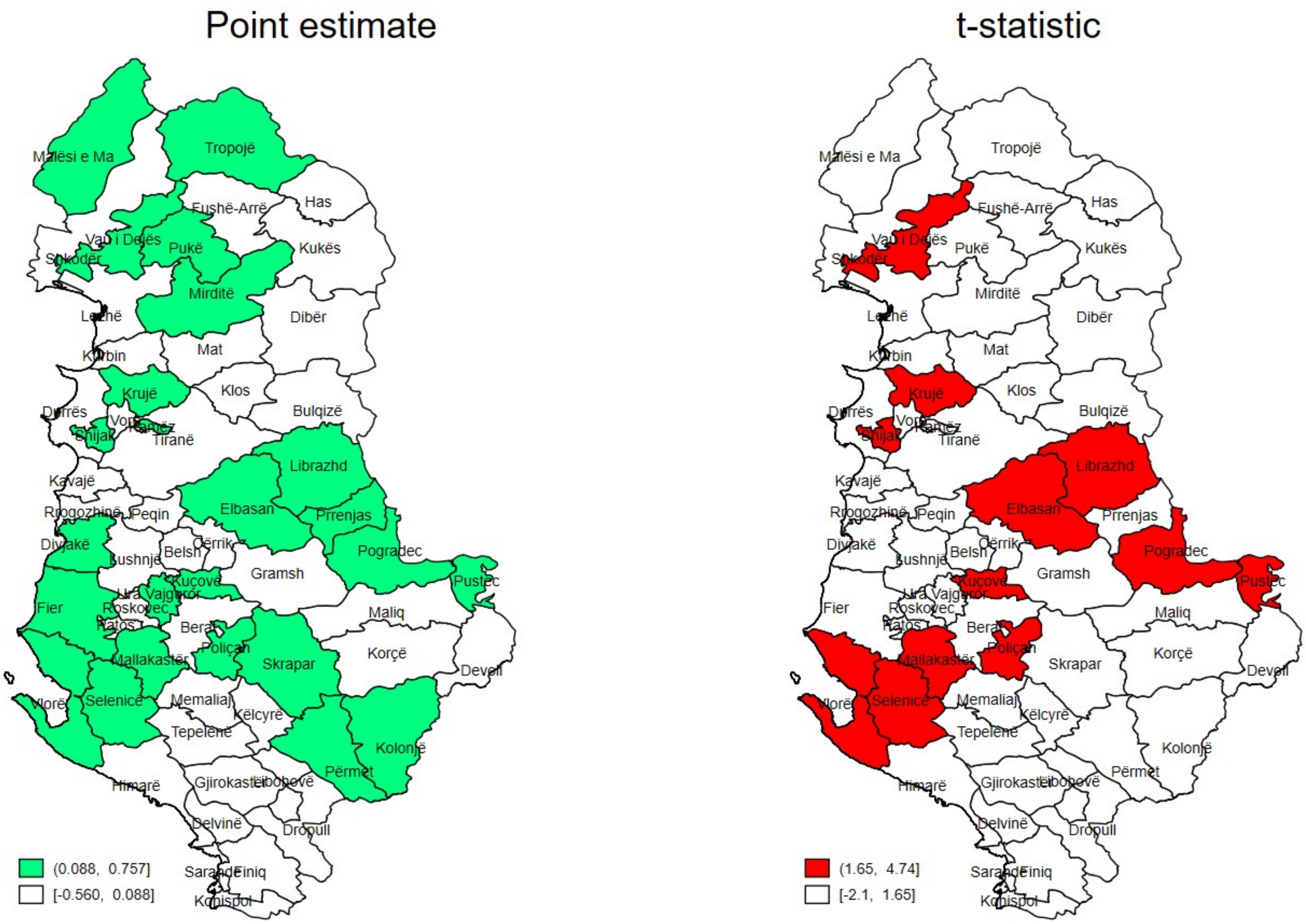
share of seats as a measure of the council's potential resistance to the mayor, we find that the largest premium (14 p.p.) accrues to candidates elected in councils fully aligned with the mayor. If anything, a larger opposition presence is associated with a *smaller* premium for elected candidates, although the interaction term is again statistically insignificant.

Taken together, these findings are difficult to reconcile with a bargaining mechanism based on an exchange of jobs for legislative support between mayors and councillors. Instead, the results suggest that a stronger opposition may even constrain councillors' ability to convert political connections into employment opportunities, potentially by increasing monitoring and imposing constraints on peer behaviour.

5.4 Labor market concentration and the value of social capital

Next, we turn to the second potential mechanism: elected candidates may earn a labor-market premium by building and mobilizing a broad network of personal connections across the public sector. If personal connections are the main mechanism, the observed premium

FIGURE 4 - Heterogeneous effects by municipality



Notes: The maps report the results of a similar regression model to that in column 7, Table 2, but allowing for heterogeneous effects across municipalities. The left panel highlights the municipalities with estimated treatment effects higher than the average ($\hat{\beta} > 0.086$). The right panel highlights the municipalities with estimated effects that are positive and statistically significant at 10 percent ($t > 1.65$).

should be larger in municipalities characterized by more concentrated labor markets. When workers have fewer outside options, employers face less competition for labor and enjoy greater discretion in hiring decisions, creating more scope for recruitment based on subjective preferences rather than meritocratic criteria (Baert et al., 2015; Berson, 2016; Fanfani, 2022; Dodini and Willén, 2025). In the Albanian context, personal connections are known to have a substantial impact on search, matching and hiring decisions (Pere and Bartlett, 2019). Thus, an imperfectly competitive labor market can be expected to amplify the value of social connections, and hence the premium earned by elected candidates.

To proxy for labor-market competition, we use data from INSTAT (2019) to construct municipality-level measures of establishment density. Specifically, we calculate the number of active firms per 100 inhabitants and per square kilometre, with higher establishment density interpreted as indicating a more competitive labor market.[32] Columns (3) and (4) of Table 6 show that establishment density exerts a moderating influence on the premium earned by councillors. A standard-deviation (2.9) increase in the number of active firms per 100 inhabitants (mean = 5.3) reduces the estimated premium by 6.4 percentage points (column 3), equivalent to three-quarters of the average effect (8.6 p.p.) estimated in Table 2.

Next, we further investigate the origins and geography of the labor-market premium of municipal councillors by allowing the treatment effect in equation (1), β, to vary flexibly across municipalities (β_m). The resulting municipality-specific estimates, plotted in Figure 4, reveal substantial heterogeneity. The largest gains from accumulating political connections are concentrated in municipalities that hosted major strategic industries during the communist period (1945-1990). These include Vau i Dejës (hydropower), Krujë (cement), Elbasan (metallurgy), Kuçovë (oil refining), Poliçan (ammunitions), Librazhd (mining), Selenicë (bitumen) and Vlorë (maritime logistics) (Mëhilli, 2017).

A common feature of these municipalities is the historical presence of large state-owned enterprises (e.g. Elbasan's Metallurgical Combine) or socialist "monotowns" (e.g., Kuçovë).[33] The historical dominance of a single employer may have discouraged entrepreneurship and private-sector development, contributing to thinner local labor markets and a higher value of personal connections that persist to this day. Consistent with this interpretation, the 13 municipalities highlighted in Figure 4 (right panel), where the estimated labor-market premium of councilors is largest, also have significantly lower establishment density than the remaining municipalities, with 3.9 active firms per 100 inhabitants compared with 5.7

[32]Since local employment shares are not available, we could not construct a Hirschman-Herfindahl index. Several previous studies use establishment density as a proxy for labor market concentration, including Rzepka and Tamm (2016) and Azar et al. (2024).

[33]Tellingly, until 1991 Kuçova was named *Qyteti Stalin* (Stalin City).

elsewhere (p-value of t-test = 0.001). This finding further confirms the regression results reported in Table 6.

Finally, column 3 of Table 6 includes all interaction terms simultaneously in the same regression, leading to qualitatively similar conclusions. The estimates of this model imply that, in a municipality with the lowest observed levels of labor market competition and a council fully aligned with the mayor, elected candidates earn a labor-market premium of 41 percentage points (s.e. = 9.8 p.p.) – roughly four times the average premium estimated in Table 2.

6 Conclusion

This paper investigates why individuals compete for elected offices that offer little remuneration and formal authority. Exploiting quasi-random variation around party-list election thresholds in Albania's 2019 municipal elections, and linking candidate records to taxpayer data nineteen months later, we show that holding a municipal council seat generates substantial labor-market returns. Elected candidates are about 9 percentage points more likely to be formally employed than unelected candidates one and half years later, while their monthly earnings almost double.

The estimated employment gains arise through wage employment rather than self-employment, are concentrated in low-ranking public-sector positions, and are associated with central-government rather than municipal jobs. These findings make it difficult to explain the observed gains as a return to human-capital accumulation or as a direct exchange of legislative support by newly elected councilors for mayor-controlled jobs. Instead, the evidence points to political social capital: officeholding enables interactions with party officials, administrators, and influential actors, allowing councillors to build connections that can quickly be converted into employment opportunities. Consistent with this interpretation, the premium is larger where corruption is higher and labor markets are thinner – settings in which hiring is more discretionary and personal ties more valuable.

These findings extend previous research on political selection by identifying network formation as a private incentive for entering politics, complementing the existing emphasis on salaries, careers, and policymaking power. They broaden the literature on the private returns to public office by showing that benefits can arise even from politically weak positions that pay only a symbolic stipend. Finally, they add to work on political connections and patronage by showing that connections need not be pre-existing and that officeholders may accumulate social capital and benefit from it while in office even when they cannot distribute jobs themselves.

Our finding that municipal council positions are used as a stepping stone into the labor market should not be over-interpreted. Individuals may also seek public office for public-spirited or other-regarding reasons, and the existence of private returns does not establish that such motives are absent. Future research should examine whether these private returns persist after councillors leave office and whether comparable mechanisms operate elsewhere.

References

Albanian Association of Municipalities (2010). Local government in albania. Technical report, United Nations Economic Commission for Europe (UNECE).

Azar, J., E. Huet-Vaughn, I. Marinescu, B. Taska, and T. von Wachter (2024). Minimum wage employment effects and labour market concentration. *The Review of Economic Studies 91*(4), 1843–1883.

Baert, S., B. Cockx, N. Gheyle, and C. Vandamme (2015). Is there less discrimination in occupations where recruitment is difficult? *ILR Review 68*(3), 467–500.

Barbosa, K. and F. Ferreira (2023). Occupy government: Democracy and the dynamics of personnel decisions and public finances. *Journal of Public Economics 221*, 104852.

Bazzi, S. and C. Labanca (2026). Campaign connections. *American Economic Journal: Economic Policy*. Forthcoming.

Becker, S. O., K. Boeckh, C. Hainz, and L. Woessmann (2016). The empire is dead, long live the empire! long-run persistence of trust and corruption in the bureaucracy. *The Economic Journal 126*(590), 40–74.

Berson, C. (2016). Local labor markets and taste-based discrimination. *IZA Journal of Labor Economics 5*(1), 5.

Besley, T. (2004). Paying politicians: Theory and evidence. *Journal of the European Economic Association 2*(2-3), 193–215.

Besley, T. (2005). Political selection. *Journal of Economic Perspectives 19*(3), 43–60.

Bieber, F. (2020). *The Rise of Authoritarianism in the Western Balkans*. London: Palgrave Macmillan.

Brassiolo, P., R. Estrada, and G. Fajardo (2020). My (running) mate, the mayor: Political ties and access to public sector jobs in ecuador. *Journal of Public Economics 191*, 104286.

Brollo, F., T. Nannicini, R. Perotti, and G. Tabellini (2013). The political resource curse. *American Economic Review 103*(5), 1759–1796.

Caselli, F. and M. Morelli (2004). Bad politicians. *Journal of Public Economics 88*(3–4), 759–782.

Cattaneo, M. D., B. R. Frandsen, and R. Titiunik (2015). Randomization inference in the regression discontinuity design: An application to party advantages in the U.S. senate. *Journal of Causal Inference 3*(1), 1–24.

Cattaneo, M. D., R. Titiunik, and G. Vazquez-Bare (2016). Inference in regression discontinuity designs under local randomization. *The Stata Journal 16*(2), 331–367.

Cingano, F. and P. Pinotti (2013). Politicians at work: The private returns and social costs of political connections. *Journal of the European Economic Association 11*(2), 433–465.

Cochran, W. G. (1977). *Sampling Techniques* (3 ed.). New York: John Wiley & Sons.

Colonnelli, E., M. Prem, and E. Teso (2020). Patronage and selection in public sector organizations. *American Economic Review 110*(10), 3071–3099.

Council of Europe (2015). Study on the strengthening of the political role of local and regional authorities in albania. Technical report, Congress of Local and Regional Authorities, Council of Europe, Strasbourg.

Dal Bó, E., P. Dal Bó, and J. Snyder (2009). Political dynasties. *Review of Economic Studies 76*(1), 115–142.

Daniele, G. (2019). Strike one to educate one hundred: Organized crime, political selection and politicians' ability. *Journal of Economic Behavior & Organization 159*, 650–662.

Dodini, S. and A. Willén (2025). The power to discriminate. IZA Discussion Paper 17830, IZA Institute of Labor Economics, Bonn.

Downs, A. (1957). *An Economic Theory of Democracy.* New York: Harper & Row.

Faccio, M. (2006). Politically connected firms. *American Economic Review 96*(1), 369–386.

Fanfani, B. (2022). Tastes for discrimination in monopsonistic labour markets. *Labour Economics 75*, 102107.

Fisman, R. (2001). Estimating the value of political connections. *American Economic Review 91*(4), 1095–1102.

Fisman, R., N. A. Harmon, E. Kamenica, and I. Munk (2015). Labor supply of politicians. *Journal of the European Economic Association 13*(5), 871–905.

Fisman, R., F. Schulz, and V. Vig (2014). Private returns to public office. *Journal of Political Economy*.

Folke, O., T. Persson, and J. Rickne (2017). Dynastic political rents? economic benefits to relatives of top politicians. *The Economic Journal 127*(605).

Gagliarducci, S. and M. Manacorda (2020). Politics in the family: Nepotism and the hiring decisions of italian firms. *American Economic Journal: Applied Economics 12*(2), 67–95.

Gagliarducci, S. and T. Nannicini (2013). Do better paid politicians perform better? disentangling incentives from selection. *Journal of the European Economic Association 11*(2), 369–398.

Heß, S. (2017). Randomization inference with stata: A guide and software. *The Stata Journal 17*(3), 630–651.

IDM (2020). *Albania: Political Engagement 2020.* Tirana: Institute for Democracy and Mediation (IDM) and National Democratic Institute (NDI).

IDP (2024). Indeksi i transparencës proaktive i njësive të vetëqeverisjes vendore. Technical report, Office of the Commissioner for the Right to Information and Protection of Personal Data (IDP), Tirana, Albania.

IDRA (2017). Governance perception in a reforming albania: Nationwide local governance mapping in albania. Technical report, Institute for Development Research and Alternatives, Tirana, Albania.

INSTAT (2019). *Vjetari Statistikor Rajonal 2019.* Tiranë, Albania: Instituti i Statistikave (INSTAT).

INSTAT (2024). *Wage Statistics.* Tirana.

Jano, D. (2016). Organizations of political parties in albania. In K. Sobolewska-Myślik, B. Kosowska-Gastoł, and P. Borowiec (Eds.), *Organizational Structures of Political Parties in Central and Eastern European Countries*, pp. 21–39. Kraków: Jagiellonian University Press.

Jayachandran, S. (2006). The jeffords effect. *Journal of Law and Economics 49*(2), 397–425.

Khwaja, A. I. and A. Mian (2005). Do lenders favor politically connected firms? rent provision in an emerging financial market. *Quarterly Journal of Economics 120*(4), 1371–1411.

Kolesár, M. and C. Rothe (2018). Inference in regression discontinuity designs with a discrete running variable. *American Economic Review 108*(8), 2277–2304.

KRIIK (2020, June). Institutional and legal behavior of the institutions and other stakeholders involved in the electoral process of 30 june 2019. Technical report, Coalition for Reforms, Integration and Consolidated Institutions (KRIIK), Tirana.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economics. *Journal of Economic Literature 48*(2), 281–355.

Little, R. J. A. (1988). A test of missing completely at random for multivariate data with missing values. *Journal of the American Statistical Association 83*(404), 1198–1202.

Mëhilli, E. (2017). *From Stalin to Mao: Albania and the Socialist World*. Ithaca, New York: NY: Cornell University Press.

Merlo, A., V. Galasso, M. Landi, and A. Mattozzi (2010). The labor market of italian politicians. *In: The Ruling Class: Management and Politics in Modern Italy*.

Meyer-Sahling, J.-H. and K. S. Mikkelsen (2016). Civil service laws, merit, politicization, and corruption: The perspective of public officials from five east european countries. *Public Administration 94*(4), 1105–1123.

Meyer-Sahling, J.-H., C. Schuster, and F. Toth (2025). Civil service politicisation and party patronage in the western balkans: A way of life in public administration? In B. G. Peters, C. Knox, F. Panizza, C. Ramos Larraburu, and K. Staroňová (Eds.), *Handbook of Politicization and Political Patronage*, pp. 274–290. Cheltenham: Edward Elgar Publishing.

News24 (2025). *SP or DP, Who Wins on May 11? News24 and Notosondaggi Publish the First National Poll*. Tirana: News24/BalkanWeb and Notosondaggi.

OSCE (2019). Republic of albania: Local elections, 30 june 2019—odihr election observation mission final report.

Pere, E. and W. Bartlett (2019). On the way to europe: Economic and social developments in albania. In R. Osbild and W. Bartlett (Eds.), *Western Balkan Economies in Transition: Recent Economic and Social Developments*, pp. 73–87. Cham: Springer International Publishing.

RCC (2025). *Balkan Barometer Public Opinion 2024*. Sarajevo: Regional Cooperation Council (RCC).

Rzepka, S. and M. Tamm (2016). Local employer competition and training of workers. *Applied Economics 48*(35), 3307–3321.

Todd, A. (2019). Political turmoil in albania casts voters' choices aside. Technical report, Organized Crime and Corruption Reporting Project (OCCRP).

Treisman, D. (2000). The causes of corruption: A cross-national study. *Journal of Public Economics 76*(3), 399–457.

World Bank (2026). *World Development Indicators.*

APPENDIX I - Comparing candidates outside and inside the randomization window

		[-5,+5] window			[-2,+2] window		
	Obs.	mean Δ	[p-value]	K-S test [p-value]	mean Δ	[p-value]	K-S test [p-value]
Age (years)	2,578	0.623	[0.256]	**[0.073]**	0.860	[0.202]	[0.556]
Born in January-August	2,582	−0.012	[0.503]	[0.999]	−0.017	[0.451]	[0.999]
Education (years)	2,358	0.280**	**[0.013]**	**[0.042]**	0.073	[0.602]	[0.210]
Education abroad	2,360	0.021***	**[0.000]**	[0.873]	0.014**	**[0.016]**	[0.999]
Female	2,583	0.012	[0.555]	[0.999]	0.043*	**[0.097]**	[0.496]
Born after communism	2,578	−0.017	[0.344]	[0.994]	−0.005	[0.817]	[0.999]
Born in rural area	2,583	−0.045**	**[0.014]**	[0.166]	−0.027	[0.260]	[0.955]
Lives in same municipality	2,548	0.016	[0.142]	[0.997]	0.015	[0.295]	[0.999]

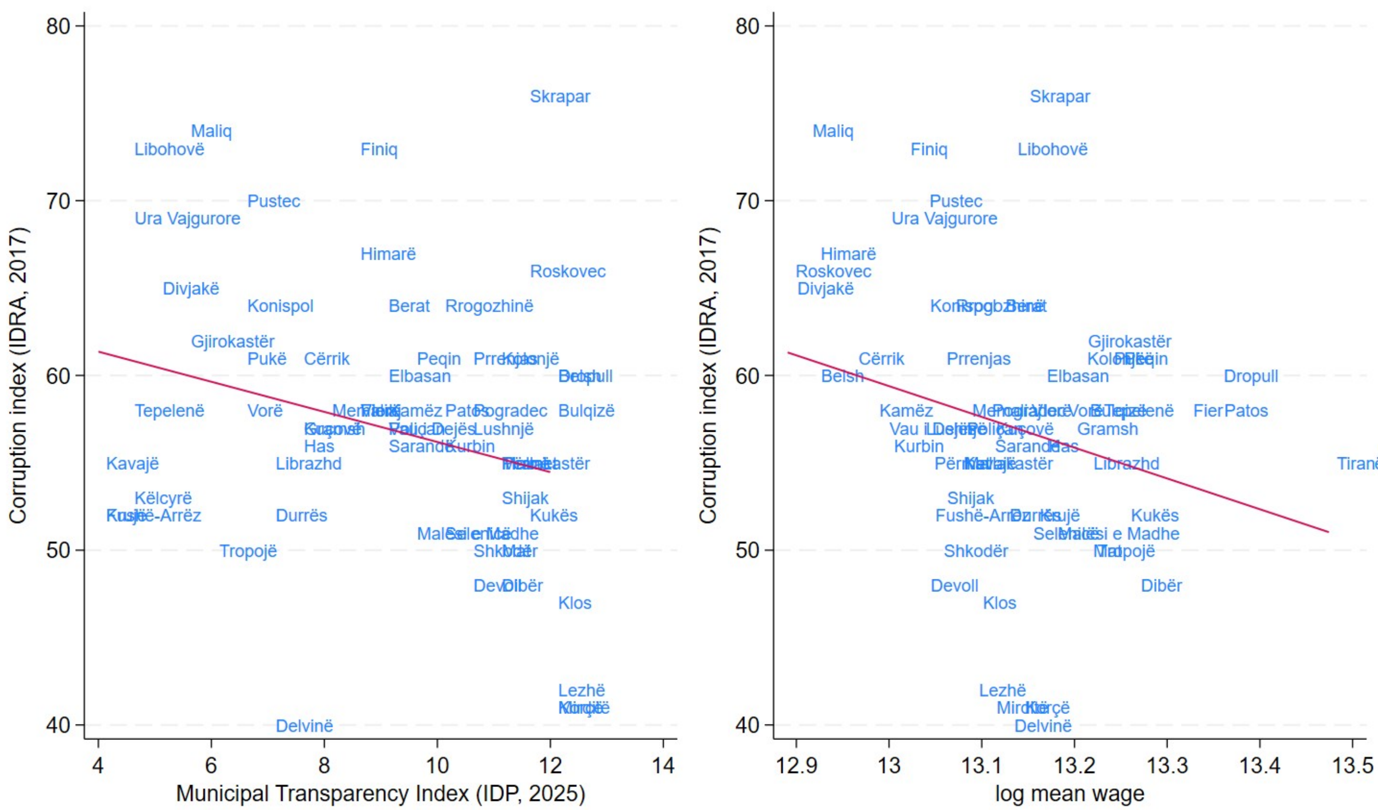
Notes: The table reports t-tests for mean differences between candidates outside and inside the specified randomization window. It also reports Kolmogorov–Smirnov (K–S) tests for equality of distributions across the two groups. The sample includes *all* candidates from the three parties that fielded the largest number of candidates: PS, PSD, and PDS. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX II - Checking for sample selection bias

Window:	[-5, +5]	[-4, +4]	[-3, +3]	[-2, +2]	[-1, +1]
Dep. var.: Missing ID	(1)	(2)	(3)	(4)	(5)
Elected	0.027	0.019	0.021	0.020	0.012
	(0.017)	(0.018)	(0.020)	(0.022)	(0.029)
Observations	2,469	2,071	1,654	1,217	706

Notes: OLS regressions with robust standard errors, for progressively narrower windows around the election cutoff. The dependent variable is an indicator equal to one if the tax ID number is missing. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX V - Correlates of municipal corruption in Albania



Notes: The left panel shows a negative correlation ($coeff. = -0.862$, $s.e. = 0.450$) between a municipal-level indicator of political corruption (IDRA, 2017) and an index of transparency in municipal governance (IDP, 2025). The right panel shows a negative correlation ($coeff. = -17.6$, $s.e. = 6.5$) between the same municipal-level corruption indicator and the mean annual wage (in logs), a proxy for economic development.

APPENDIX III - Robustness analysis: Alternative specifications

Specification:	w/out lead cand.	control for lead cand.	quadratic in rank	loc. lin. reg.	Only non-emp. 2019
	(1)	(2)	(3)	(4)	(5)
Elected	0.064**	0.076**	0.084*	0.102*	0.128***
	(0.031)	(0.032)	(0.047)	(0.054)	(0.042)
Lead candidate		0.084**			
		(0.042)			
Rank			−0.036		
			(0.025)		
$Rank^2$			0.001**		
			(0.000)		
Normalized rank				0.023	
				(0.047)	
Normalized rank × Elected				-0.019	
				(0.047)	
Observations	674	860	860	860	242

Specification:	w/out PS	w/out asymetric windows	w/out small munic.	logit (ML)	ITT
	(6)	(7)	(8)	(9)	(10)
Elected	0.114***	0.085***	0.104***	0.128***	0.090***
	(0.043)	(0.030)	(0.034)	(0.043)	(0.030)
Observations	271	442	322	453	453

Notes: OLS regressions (ML in column 9) with robust standard errors adjusted for stratification at the party list (municipality × party) level. Dependent variable: employment (2021). All models control for a full set of individual characteristics, employment in 2019 (except for model 5), and party list FE. Column 9 (logit specification) reports marginal effects. The candidate's normalized rank is defined as $(cutoff - rank + 1)$. All models use the [-2, +2] randomization window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX IV - Placebo cutoffs

	-2	-1	True cutoff	+1	+2
Normalized rank of:					
Elected	(-1,0)	(0,1)	(1,2)	(2,3)	(3,4)
Non-elected	(-3,-2)	(-2,-1)	(-1,0)	(0,1)	(1,2)
	(1)	(2)	(3)	(4)	(5)
Elected	0.009	0.023	0.086***	0.045	−0.003
	(0.035)	(0.031)	(0.030)	(0.033)	(0.041)
Observations	438	448	453	406	345

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality × party) level. Dependent variable: employment (2021). All models control for a full set of individual characteristics, employment in 2019, and party list FE. The candidate's normalized rank is defined as $(cutoff - rank + 1)$. All models use a [-2, +2] randomization window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX VI - Individual characteristics associated with corruption

Dep. var. Employment (2021)	(1)	(2)	(3)	(4)
Elected	0.105***	0.132***	0.146***	0.189***
	(0.031)	(0.043)	(0.037)	(0.047)
Elected × having a postgrad degree	−0.140*			−0.112
	(0.081)			(0.078)
Elected × female		-0.089		-0.072
		(0.060)		(0.061)
Elected × born after communism (1990)			−0.171**	−0.143**
			(0.069)	(0.068)
Observations	453	453	453	453

Notes: OLS regressions with robust standard errors adjusted for stratification at the party list (municipality × party) level. All models control for a full set of individual characteristics, employment in 2019, and party list FE. All models use the [-2, +2] randomization window. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.