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Votes for Work? Job Patronage and Electoral Mobilization in Albania*

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Abstract

We examine the impact of an election campaign on the labor market outcomes of incumbent party supporters. Using unique data on voters' political preferences during a critical pre-election period in Albania, our difference-in-differences estimates show that supporting the ruling party prior to elections significantly improves individuals' employment and earnings. This labor market premium is particularly pronounced among individuals with low costs of campaign participation, whereas patronage jobs are concentrated in lower-tier public sector positions. Administrative data further show that job distribution to party supporters strongly correlates with increased vote shares for the incumbent. Our findings suggest that parties strategically allocate public employment to mobilize grassroots supporters and secure votes—a practice that fosters corruption and weakens democratic institutions.

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JEL Codes: D72, D73, H83, J45, M59.
Keywords: Job patronage; political corruption; vote-buying; Albania; post-communist transition

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1 Introduction

Political corruption and clientelism remain major obstacles to democratic development and consolidation, particularly in emerging democracies. Among these practices, job patronage – the allocation of public-sector positions in exchange for electoral support – is a common strategy through which ruling parties maintain and consolidate political power (Kitschelt and Wilkinson, 2007; Grindle, 2012). This practice distorts the recruitment process, as it prioritizes political loyalty over merit and qualifications. As a result, it can reduce the productivity of civil servants, compromise the quality of public service delivery, and ultimately undermine trust in public institutions (Finan et al., 2017; Xu, 2018; Estrada, 2019; Akhtari et al., 2022).

While case-based accounts of job patronage abound, systematic evidence remains scarce. Furthermore, previous research has focused primarily on the strategic hiring of *elite* supporters – the likes of unelected candidates (Brassiolo et al., 2020), campaign finance donors (Colonnelli et al., 2020), card-carrying party members (Barbosa and Ferreira, 2023) and regularly employed campaign workers (Bazzi and Labanca, 2023). Yet, during election campaigns, politicians may also enlist *ordinary voters*, offering them employment opportunities in exchange for campaign support and help with voter mobilization.

In this paper, we use rich and unique data on the political orientation and employment outcomes of ordinary voters to study job patronage. Specifically, we investigate whether the employment and earnings of individuals who support the ruling party increase just before elections, and whether these outcomes are driven by patronage-based hiring.

Our empirical setting is Albania, a middle-income post-socialist country where democratic consolidation remains fragile. Albania provides a particularly compelling context for this study due to a unique legacy of its communist past: an extensive network of spies and informants that the incumbent Socialist Party of Albania (PS) inherited from its predecessor party, the communist-era Party of Labour (PPSh). Originally designed to identify and repress political dissent (Mëhilli, 2017), this network was later repurposed by the post-

communist PS as a sophisticated elctioneering machine. Since the fall of communism, the PS has gradually come to rely on this apparatus to monitor voter preferences, engage in vote buying, and mobilize turnout on election day.

In both advanced and emerging economies, political parties are known to rely on data-driven methods to understand and influence voter behavior (Dommett et al., 2024). However, the data collected by parties are often unsystematic and almost never publicly available. In contrast, the information collected by the PS in Albania is of high quality and, unusually, has recently been released into the public domain. This provides a rare opportunity to investigate empirically the mechanisms of job patronage using individual-level data.

During election campaigns, politicians are actively engaged in seeking political support and are therefore more likely to use the spoils of power to mobilize and reward their supporters (Kitschelt and Wilkinson, 2007). For this reason, we focus on the three months leading up to the 2021 parliamentary election, in which the ruling PS secured a third consecutive term in office. We employ a voters' survey conducted by the PS's electioneering machine in the run-up to the 2021 (and 2013) elections. This dataset contains individual-level estimates of political orientation for nearly the entire voting-age population of the Prefecture of Tirana, Albania's capital city region. To validate these data, we compare them with official vote shares and show that reported political preferences strongly predict actual voting behavior at the polling station level.

We combine the political preference data with a matched employer-employee dataset on the universe of employed individuals in Tirana. This information is publicly available for the months of January 2021 and April 2021 (the election month), and includes a complete record of formal-sector salaries paid in Albania. Crucially, the data permit linking individuals across the political preference and salary datasets using their tax ID numbers, allowing us to perform a high-quality matching.

Our analysis employs a difference-in-differences (DiD) estimation strategy comparing changes in labor market outcomes for PS supporters during the last three months before elec-

tions with changes in corresponding outcomes for non-PS supporters. While the DiD design controls for individual time-invariant characteristics, PS and non-PS supporters may still exhibit fundamentally different latent trends in employment and earnings. To strengthen causal identification, we restrict our comparisons to PS and non-PS supporters residing in the same close neighborhood. We argue that, in the context of Tirana (and the Balkan region, more generally), the neighborhood plays a fundamental socio-economic function. As a result, the labor market outcomes of close neighbors are likely to evolve in parallel. In addition, we use an event-study analysis to show that there are no pre-trends in aggregate government expenditure for personnel prior to the election campaign; and that the effects of the campaign on payroll expenditure manifests itself precisely in the last three months before elections and tends to fade out afterward. This pattern is consistent with the assumption that PS and non-PS supporters experienced parallel (and flat) trends in labor market outcomes up until January 2021. We further confirm the validity of the identification strategy by showing that results hold when controlling for rich individual characteristics, using matched samples, restricting to pre-unemployed individuals, and running sensitivity analyses within finer geographic units to rule out unobserved confounding.

Our DiD estimates show a significant effect, although small in magnitude, of the election campaign on the labor market outcomes of incumbent supporters. For the average PS supporter, the election campaign increases the probability of being employed (in either the private or public sector) by 0.34 percentage points − a one percent increase relative to baseline employment levels. In addition, the campaign leads to a 0.6 percent increase in earnings compared to baseline levels. In a battery of robustness checks, we further show that the effects are not driven by preference shifts (loyalty) or misclassification in political orientation, and are consistent with aggregate pre-election timing patterns in public payroll spending.

Next, we investigate whether the pre-election improvement in the labor market outcomes of PS supporters is the result of political patronage. To do so, we examine how the

estimated effects vary by voter characteristics. We find that the employment and earnings gains of PS supporters are driven primarily by individuals who can mobilize and aggregate votes at the lowest cost. These include: young voters, whose peers tend to have less entrenched, and hence more malleable, political preferences; voters from staunchly pro-PS families, who may be considered more trustworthy by politicians; and voters who were born in Tirana and never migrated abroad, and are therefore likely to be well-networked with both politicians and potential target voters.

We also explore heterogeneous effects according to job and employer characteristics. We find that the patronage jobs provided to PS supporters are predominantly concentrated in the public administration and state-owned enterprises. Within the public sector, we further show that supporting the PS primarily increases access to low-ranking positions, where the needs or benefits of hiring ideologically aligned co-partisans are less obvious. Overall, these patterns suggest that political patronage is likely to be an important mechanism behind the observed labor market advantage of PS supporters during the 2021 election campaign. In particular, the evidence is consistent with PS supporters receiving low-level public-sector jobs in exchange for mobilizing additional votes.

So far, we have focused on the job side of a "votes for jobs" exchange. Next, we turn to the vote side, exploring whether the provision of jobs leads to the mobilization of votes. This is important since it remains an open question whether rewarding voters with jobs pays off in terms of electoral success. Specifically, we investigate whether the labor market gains of PS supporters are correlated with increases in the PS's vote share at the ballot box. By aggregating labor market data at the polling station level, we find that each additional job filled by a PS supporter in a given polling station (or each million lek spent on hiring PS supporters) is associated with a 0.14 (0.5) percentage-point increase in the PS's vote share. This evidence supports the notion that recipients of patronage jobs actively engage in voter mobilization, and implies that job patronage may be a rational electoral strategy for the incumbent party.

Our results contribute to a growing empirical literature showing that political and campaign connections increase access to public-sector jobs (Colonnelli et al., 2020; Brassiolo et al., 2020; Barbosa and Ferreira, 2023; Bazzi and Labanca, 2023). Our analysis differs from these studies in at least three ways. First, while previous research was focused on the benefits received by individuals with a formal *affiliation* to the ruling party, we examine the advantages associated solely with having a *preference* for the incumbent. In their pursuit of electoral success, politicians are also known to rely on the help of ordinary supporters with no formal link to the party (Stokes et al., 2013; Khan and Jomo, 2000). Yet, there is little systematic evidence on these grassroots strategies of political mobilization and their economic implications.

Second, individuals who support the incumbent may also be rewarded with jobs in the private, as well as the public, sector. Politically connected firms may have an incentive to hire party supporters in exchange for, or in anticipation of, favorable treatment by politicians (Gagliarducci and Manacorda, 2020). Our findings offer limited evidence in support of this mechanism. Yet, to our knowledge, this is the first empirical study to explore this channel in the context of job patronage.

Third, while existing studies typically document partisan advantages in recruitment following elections – conditional on the incumbent's victory – we present evidence that co-partisans begin to receive patronage jobs during the election campaign itself (and potentially retain them into the subsequent legislative term). One possible explanation for this pattern lies in the commitment problem politicians face with so-called "grassroots" supporters (Robinson and Verdier, 2013). While rewards to elite supporters—who enjoy formal or strong informal ties to the party—can credibly be deferred, benefits for ad-hoc grassroots supporters must be delivered immediately to ensure effective mobilization.

Our study is also related to a number of papers studying the private returns to political careers (Fisman et al., 2014) and to having family connections to politicians (Fafchamps and Labonne, 2017; Folke et al., 2017; Gagliarducci and Manacorda, 2020). We contribute to

this strand of literature by analyzing the private returns of voters with the "right" political preference, rather than those of career politicians and their relatives. Relatedly, the type of job patronage we document involves a *quid pro quo* exchange, whereas embezzlement (Fisman et al., 2014) and nepotism (Gagliarducci and Manacorda, 2020) by politicians and their families typically do not.

Although we do not provide direct evidence that the beneficiaries of patronage jobs mobilize votes *by offering cash payments*, our research also relates to previous work on the causes (Anderson et al., 2015; Hicken et al., 2017; Bobonis et al., 2022) and consequences of pure vote-buying (Vicente, 2013; Leight et al., 2020), as well as research that identifies the populations typically targeted by vote-buying politicians (Finan and Schechter, 2012).[1] The phenomenon that we document is novel, however, because it involves politicians rewarding supporters for their help in soliciting or buying *additional* votes, rather than for supplying their *own* vote. In this sense, our findings are consistent with previous case evidence showing that vote buying does not take place in a free market with buyers (politicians) and sellers (voters), but rather involves complex chains of intermediaries, brokers, and facilitators (Baland and Robinson, 2008; Finan and Schechter, 2012; Casella et al., 2014).

The paper is structured as follows. Section 2 provides an overview of the Albanian context. In Section 3, we describe and validate our data in detail. The main results are presented in Section 3, while Section 4 explores job patronage as the most plausible mechanism underlying our findings. Section 5 examines the link between job patronage and voting outcomes. Finally, Section 6 concludes.

[1]These studies highlight factors such as economic vulnerability, the presence of a powerful land-owning class, and voter education campaigns as influencing the incidence of vote buying. Documented consequences include higher voter turnout, reduced incumbency advantage, and lower political accountability

2 Background

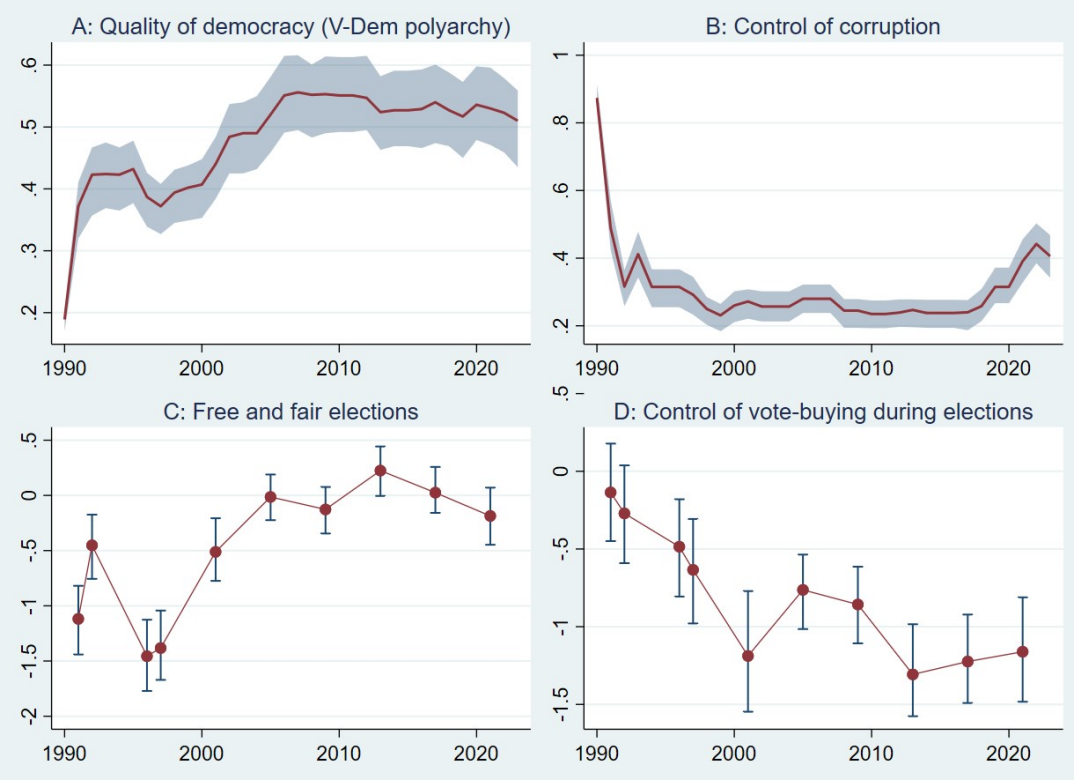
2.1 Democracy and elections in Albania

During 1945-1990, Albania was ruled by one of the most autocratic communist regimes of the former Eastern Bloc (Mëhilli, 2017). Following the transition to democracy, multi-party elections were held for the first time in March 1991. Polls have since taken place every four years, with MPs elected through a closed-list, proportional representation system. The political landscape is characterised by intense competition between two highly polarised dominant parties, which have alternated in power for the last 33 years: the Socialist Party of Albania (PS) and the Democratic Party (PD). The PS is the centre-left successor to the Albanian Party of Labor, the former communist party. The centre-right PD was founded in late 1990 by anti-communist activists, but was soon taken over by reformed elements of the former communist elite (Abrahams, 2016). Both parties are mainly organised around leading personalities and clan affiliation, rather than ideology (Kajsiu, 2016).

Figure 1 displays two indicators of institutional quality − democracy and corruption control − from the Varieties of Democracy (V-Dem) dataset (Coppedge et al., 2024). The transition from communism brought dramatic improvements in democratic standards (Panel A). Freedom House now rates Albania as "partly free" (freedom score = 68/100), with a "transitional or hybrid" political regime (democracy score = 46/100). Yet, the transition also made political corruption more widespread (Panel B). As noted by Bieber (2018: 78), "parties secure loyalty and support mostly through patronage, the provision of jobs, and other benefits". Thus, "the alternation of power is less a reflection of the competition of alternative political visions" than the outcome of contests for power between opportunistic coalitions (p. 78).

Figure 1 also displays two election-specific measures of electoral integrity from V-Dem − free and fair elections and vote-buying. While the overall quality of electoral contests has improved relative to the 1990s and early 2000s (Panel C), vote-buying has become

FIGURE 1 - Institutional quality (A, B) and electoral integrity (C, D) in post-communist Albania



Source: Varieties of Democracy (V-Dem) dataset, version 14 (2014). The variables used are the following. Panel A: democracy (*v2x_polyarchy*). Panel B: political corruption, reversed (*v2x_corr*). Panel C: Election free and fair (*v2elfrfair*). Panel D: Election votebuying, reversed (*v2elvotbuy*).

an increasingly popular strategy for securing political support (Panel D).[2] While Albania's Electoral Code prohibits the use of the public administration for electoral purposes and bans salary increases before elections (art. 91), it does not explicitly impose a pre-electoral hiring freeze. Indeed, anecdotal evidence indicates that in pre-election periods it is common for the party in power to enlist party supporters into the election campaign effort, rewarding them with (typically, temporary) jobs in the public administration.[3] Further, anecdotal evidence and previous research also suggest that it is also not uncommon for firm owners with connections to politicians to buy their employees' vote with a one-off salary bonus (Imami et al., 2024).

Our empirical analysis focuses on the 2021 parliamentary election, held on 25 April. The incumbent party was the PS, led by Prime Minister Edi Rama, who won 48.7 percent of the vote and secured a third consecutive term in office.[4] While Rama's governments had "brought a fresh move toward reforms, [...] many of the structural vestiges of party patronage persist[ed]", with the PS basing its power primarily on "clientelist and informal control" rather than programmatic legitimacy (Bieber, 2018: 32). In the 2021 election, "international [...] monitors reported numerous electoral irregularities, including allegations of widespread vote buying" (Freedom House, 2021). In the run-up to the vote, "videos recorded the alleged exchange of cash and/or other goods between party militants and presumed voters" (NDI, 2021). After the vote, local courts initiated 30 criminal investigations of vote buying (OSCE, 2021: 3).

[2]The V-Dem data are consistent with vote-buying efforts by political parties being "non-systematic, but rather common", with the most likely score given by the country experts being 3 on a 5-point scale ranging from 0 to 4 (Coppedge et al., 2024: 69).

[3]When interviewed by journalists, an employee of the Vlorë municipality stated on conditions on anonymity that workers hired on temporary contracts before elections are expected to "be involved in the election campaign". Indeed, he continued, "it is assumed that each worker [hired before elections] would bring around twenty votes" – typically, by offering favours, cash, or a share of their salary to neighbours, friends, or relatives (see `https://www.reporter.al/2023/10/02/punesimet-elektorale-fryne-administraten-e-bashkive-perpara-zgjedhjeve-vendore/`).

[4]On 11 May 2025, Edi Rama secured a fourth consecutive term in office. This last election was also mired by accusations of vote-buying and patronage-based electoral mobilization.

2.2 The Albanian economy and labor market

Albania is a middle-income country with a per-capita GDP of US$6,377 (2021), similar to Colombia or Peru (World Bank, 2024). Due to high emigration, the national population declined from 2.8 million in 2011 to 2.4 million in 2023 (INSTAT, 2024; Mendola and Carletto, 2012). The population of the Prefecture of Tirana, however, remained stable as international outflows from the capital-city region were offset by sustained rural-to-urban migration. While it used to be common for many internal migrants initially to settle in informal squatter settlements, most of these have since been formalized and equipped with improved infrastructure (Vullnetari, 2012: 26). With around 760,000 inhabitants, the Prefecture of Tirana accounts for almost a third of Albania's population.

During the transition from central planning, Albania's economy has become increasingly specialized in labor-intensive industries—such as garment manufacturing, low-value added IT services and mining. Between 2015 and 2021, GDP growth and employment were moderately correlated (0.63), while the unemployment rate steadily declined to 12.5 percent (World Bank, 2024). Over the same period, labor force participation rose to 58.7 percent. Nevertheless, youth unemployment remains high at 29.4 percent, and high-skilled job opportunities are limited—particularly for the increasingly well-educated younger population (Pere and Bartlett, 2019). Informal networks continue to be the dominant job search channel. As of early 2021, the average gross monthly wage stood at US$544, the lowest in the Western Balkans (INSTAT, 2024a). Public sector jobs—accounting for 15 percent of total employment and paying, on average, 32 percent more than private sector positions—are highly sought after.

3 Data and descriptive statistics

We construct an individual-level panel dataset of 424,837 registered voters of working age in the prefecture (*qark*) of Tirana, which comprises Albania's capital city and four peri-

urban municipalities (*bashki*) – Kamza, Kavaja, Rrogozhina and Vora. For each voter, we observe their political preferences prior to 2021, assigned polling station, and demographic characteristics. We combine this information with administrative data from the General Tax Office. By exactly matching individuals across the two datasets, we obtain information on voters' labor market outcomes, including their employment status and wage earnings, at two time periods: a non-election period (January 2021) and the election month (April 2021). We complement our panel dataset with additional data sources. Further details are provided in Appendix A.

3.1 Political preferences

The "political preference" survey includes estimates of the political orientation of 570,172 individuals, representing between 90% an 98% of the resident population of voting age (18 years and older) in the Prefecture of Tirana (INSTAT, 2024).[5] This information was collected by PS party activists, known in Albania as *patronazhistë,* in the years leading up to the 2021 parliamentary election.[6]

As mentioned earlier, the *patronazhist* network is a legacy of communism. All communist parties in Soviet bloc countries could count on extensive networks of spies and informants (Mëhilli, 2017). In Albania, the PS inherited this network from its communist-era predecessor and retooled it as an electioneering machine. Today, the *patronazhistë* perform three main functions – track voters' preferences, act as middlemen in vote-buying transactions, and rally voters to the polls on election day. In 2021, the *patronazhistë* were hailed

[5]Note that the number of individuals registered on the voters' rolls (878,539) is *higher* than the resident population aged 15 and above (631,814 in 2023), or 20 and above (583,315). Since Albania's electoral rules did not provide for external voting until 2025, migrants commonly retained their voter registration in their home municipality after moving abroad (Vullnetari, 2012). Since returning to Albania to vote is not a widespread practice (Vullnetari, 2012), the sample likely includes nearly all individuals able to cast their ballot.

[6]Appendix A provides additional details on data sources, while Appendix B discusses the ethical implications of using this kind of data in empirical research.

by the PS as a key driving force in its election win.[7]

To assess voters' political orientation, the *patronazhistë* combined direct elicitation of preferences with indirect inference from observed behavior, background characteristics, and information provided by neighbors.[8] The data was compiled by 8,908 *patronazhistë*, each of whom was responsible, on average, for estimating the preferences of just 63 voters (s.d. = 40.4).[9] There is reason to believe that the monitoring and information-gathering process took several months and therefore began well before the official start of the election campaign. This process also likely concluded before the final pre-election quarter (February-April 2021), by which time the *patronazhistë* had likely shifted their focus from information gathering to active campaigning.

39.9% of surveyed voters are estimated to support the PS, while 18.2% (13.2%) support the PD (other parties). The remaining 28.8% of entries are coded as blank. We take blanks as indicating that there was insufficient information to produce a meaningful estimate of political orientation. Alternatively, blanks may reflect volatile preferences or apolitical attitudes. There is no way to ascertain the exact coding rules.[10]

The *patronazhistë* also rated the precision of their estimates by coding a "certainty" variable, which indicates whether an individual has a "low" or a "high" probability of having his/her political orientation misclassified. Only 32.9% of (non-blank) estimates are rated as "certain".[11] In the main analysis, we use an indicator (PS_i) which takes the value 1 if

[7] See https://shqiptarja.com/lajm/sekretari-organizativ-i-psse-falenderon-patronazhistet-dhe-jep-te-dhenat-per-anetaresine-kemi-122-mije-e-917-anetare

[8] This is evident from some of the narrative comments that are included in the dataset.

[9] The *patronazhistë* that also appear in the dataset as voters are predominantly (71%) male, with an average age of 41.6 years and overwhelmingly (99.7%) no history of migration abroad. A majority of them work in public-sector institutions (43.6 percent) such as local municipalities, the department of motor vehicles, and the national tax agency; or state-owned corporations (18.3%) such as the electricity distribution company, local water providers, or the post office. 31.9% work in the private sector, and only 6.2% are employed by foreign-owned companies.

[10] In addition, 10,261 registered voters enter the political preference dataset with a missing value on political orientation. We take the missingness as meaning that the *patronazhist* did not investigate the voter in the first place, as opposed to having investigated the voter without being able to produce an estimate.

[11] The certainty rating is missing for 4.2% of the (non-blank) estimates.

voter i (registered in polling station p) is estimated to be a PS supporter (with or without certainty), and 0 if he or she supports a party other than the PS or his/her preference is coded as blank. We use this variable to identify individuals that are likely to be regarded as loyal to the party, and may thus be offered jobs in exchange for taking part in the election campaign. We later also distinguish between those who are estimated to be PS supporters with or without certainty.

For a sub-set (59.9%) of surveyed voters, the dataset also provides information on their political orientation prior to the 2013 parliamentary election. At this time, the PS was in opposition and had fewer resources to survey as large a sample of voters as in 2021.[12] Being a PS supporter in 2013 correlates with being a PS supporter in 2021 at 0.22, suggesting only moderate persistence in political preferences over time.

Lastly, the dataset reports several demographic characteristics of surveyed voters, such as birth date, gender, household size, place of birth[13], and whether they had previously migrated abroad.

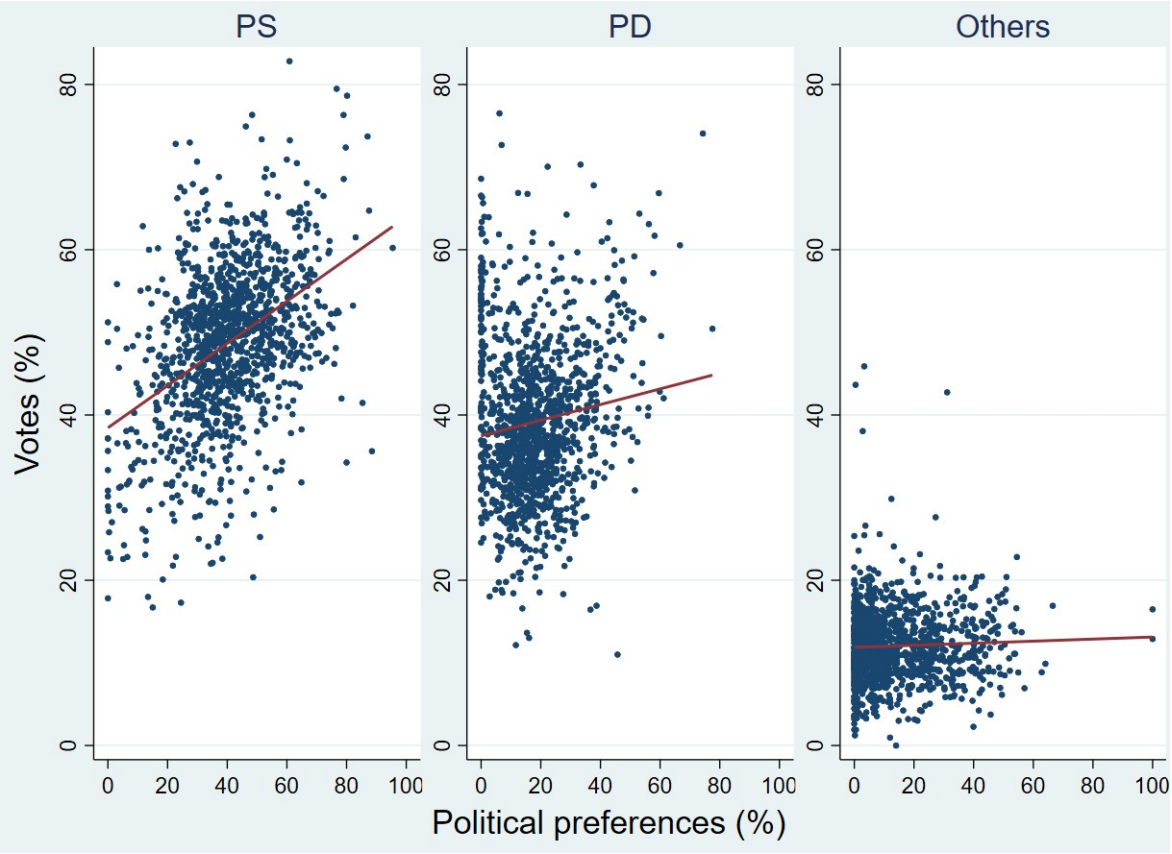
3.1.1 External Validity

To assess the reliability of the political preference estimates, we examine whether they predict voting behaviour. For this exercise, we use data on the 2017 and 2021 election results at the polling station level. This information (henceforth, the "voting dataset") is provided by Albania's Central Election Commission (*Komisioni Qëndror i Zgjedheve*). There are 1,243 polling stations in the prefecture of Tirana, each with an average of 385 ballots cast (s.d. = 121). We compute the vote share of the PS, PD and other parties, defined as the number of votes gained by each party divided by the total number of valid

[12]The results based on 2013 political orientation should be interpreted cautiously, since the sample is smaller than the overall survey sample and may not be representative of the underlying population.

[13]We use this information to construct an indicator for individuals born within the Prefecture of Tirana, as well as an indicator for individuals born in rural areas. We define rural areas as any locality that is not the administrative centre of one of Albania's 61 municipalities (*bashki*).

FIGURE 2 - Estimated political preferences and voting outcomes



Source: The number of observations (polling stations) is 1,231. The horizontal-axis variable is the share of estimated political preferences for each party in the total number of individuals surveyed by the *patronazhistë* in each polling station. The vertical-axis variable is the share of votes gained by each party in the total number of valid votes cast in each polling station. The coefficients (standard errors) of the OLS fits are 0.255 (0.018) for the PS, 0.095 (0.026) for the PD and 0.012 (0.008) for other parties.

ballots cast in each polling station. Next, we compute the preference share as the number of estimated preferences for each party (whether certain or not) divided by the total number of individuals surveyed by the *patronazhistë* in each polling station.

As shown in Figure 2, the proportion of estimated PS supporters across polling stations is positively correlated with the PS vote share. This finding suggests that the *patronazhistë* are able to distinguish reliably between PS and non-PS voters. The proportion of voters who support the PD or other parties, by contrast, are less strongly correlated or uncorrelated with the corresponding vote shares. This finding indicates that the *patronazhistë* are less accurate in estimating the political orientation of non-PS voters. For this reason, in the analysis we

mainly focus on the distinction between PS and non-PS supporters. In Appendix Table A.1, we show that the correlations shown in Figure 2 are robust to conditioning on polling station characteristics and municipality fixed effects.

3.2 Labor market outcomes

To measure labor market outcomes, we use a matched employer-employee dataset that reports the labor income paid under all formal employment contracts in Albania. The dataset is collected and maintained by the General Tax Office (*Drejtoria e Përgjithshme e Tatimeve*), and is only publicly available for the months of April and January 2021. In the analysis, we only focus on individuals employed in the prefecture of Tirana.

We focus on the gross monthly wage paid to individuals with active contracts in April (hirings), January (separations) or both months (incumbents). The information is reported in current Albanian leks and includes bonuses and premia. Since the administrative data covers the full population, and thanks to perfect matching, we classify surveyed voters who do not appear in the wage records for January, April, or both as earning zero wages in the corresponding month(s). Conditional on working, the mean wage was 53,155 leks in January and 53,102 in April (US$ 513). Both means are within 6 percent of the corresponding official figure quoted in section 2.2.[14] We winsorize this variable at the 1 percent level in each tail to remove severe outliers.

We also construct an indicator $I(employed)_{it}$ of employment status that takes the value 1 if an individual i is employed in the formal sector (as indicated by a non-zero wage) at month $t \in \{January,\ April\}$, and 0 if they are unemployed, economically inactive or active in the informal sector (i.e. reports a zero wage). Although the salary dataset does not include detailed information on the number of hours worked, it does provide an indicator

[14] We also note newspaper reports of business owners and managers that confirmed to the media following the leak that the wage figures reported in the salary dataset matched their payroll (https://lapsi.al/2021/12/22/superskandali-dalin-sheshit-emer-per-emer-rrogat-e-mbi-600-mije-shqiptareve/).

$I(full\ time)_{it}$ for working more than 87 hours.[15]

The salary dataset includes information on employers' names, but does not specify their ownership status. To address this limitation, we manually coded indicator variables distinguishing public- vs. private-sector employers for a subsample representing 80.4 percent of all formal workers in Albania. Within the public sector, we further distinguish between the state bureaucracy (such as line ministries and municipal offices), and state-owned enterprises and public service providers (such as public schools, hospitals, and the national oil company). Within the private sector, we further distinguish between domestically owned and foreign-owned companies.[16] For all public-sector workers in this sub-sample, we also applied the classification proposed by Brassiolo et al. (2020: 4) to distinguish between high-ranking (e.g. director and high-skilled analysist) and low-ranking positions (e.g. laborer, assistant and low-skilled analyst).[17]

Lastly, we complement this data with information on individuals' car ownership from Albania's Department of Motor Vehicles (*Drejtoria e Përgjithshme e Shërbimeve të Transportit Rrugor*).[18] Since vehicles are often shared among family members, we use a household identifier from the political preference dataset to calculate the total number of vehicles owned by each individual's household. Based on this, we construct indicators for individuals living in households with zero, one, and more than one vehicle. We interpret these variables as proxies of household income.

[15]About 90 percent of all jobs (in both January and April) fall in this "full-time" category (>87 hours/month), with the mean wage (55,740 leks) earned by full-timers almost twice the mean wage earned by part-timers (28,444) in both months.

[16]We used our knowledge of the Albanian economy and, when in doubt, a number of internet sources (such as the website of the National Business Registration Centre, or *QKB*) to check the ownership status of each entity. In the resulting sub-sample of employment contracts (which only excludes contracts involving micro-enterprises with less than 3 employees), 33 percent of contracts are signed with public-sector employers, and 67 percent with private-sector firms.

[17]Again, we performed this task manually, using reported information on job titles, the average wage of each occupation, and our knowledge of the local context.

[18]This information was leaked in unknown circumstances in December 2021.

3.3 Descriptive statistics

Table 1 reports summary statistics by political orientation. While balance in levels is not strictly required for the validity of our double-difference estimation strategy (see next session), it is nevertheless reassuring that PS and non-PS supporters display relatively similar demographic characteristics (Panel A). While there are slight differences, the two groups are balanced in terms of all observed characteristics except for having been a migrant abroad, as shown by standardized differences below 0.25, a commonly used rule of thumb (Imbens and Rubins, 2015). These mean differences remain small, or become smaller, when comparing PS and non-PS supporters living in the same neighborhood (polling station).

Panel B reports summary statistics for our two outcome variables. Here, the differences between the two groups are more pronounced. Already at baseline (January), PS supporters are 15.2 percentage points more likely to be in formal employment than non-PS supporters; conditional on being employed, they also earn 6.3 percent $(3,322/52,797)$ more.

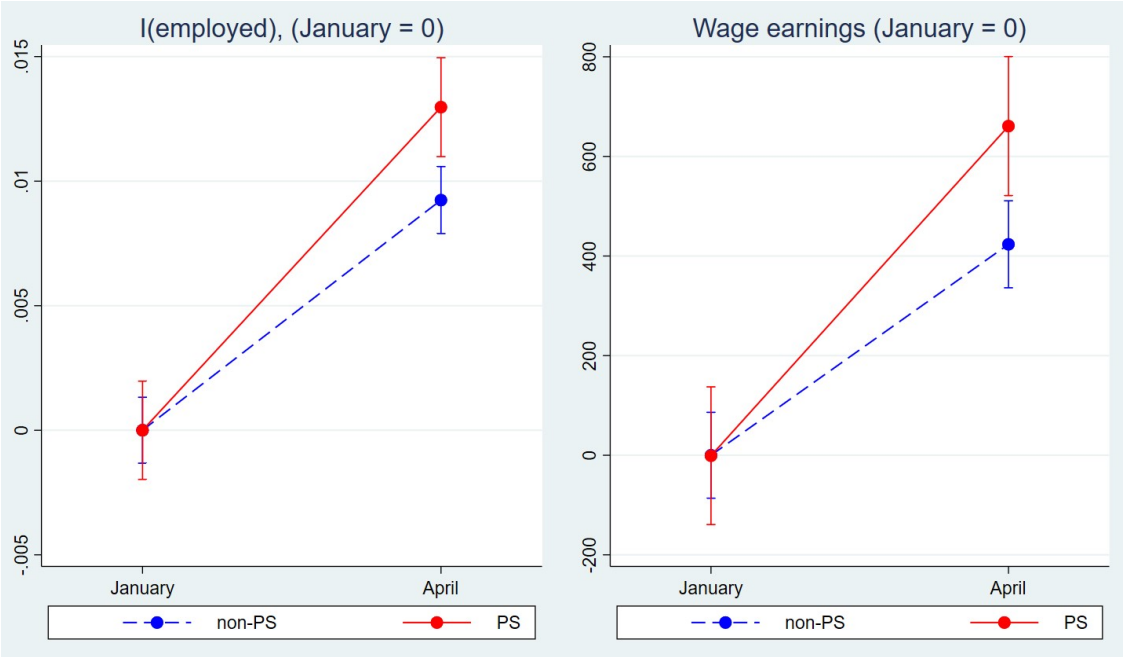
Importantly, the information reported in Panel B indicates that the labor market premium earned by PS supporters becomes *larger* between January and April. During this period, the *gap* in employment probability between PS and non-PS supporters expands by 2.5 percent $(= 100 \times (0.156 - 0.152) \ / \ 0.152)$, unconditional wage earnings by 2.6 percent, and earnings conditional on employment by 3.3 percent. Figure 3 confirms this pattern by showing the mean value of employment (left panel) and earnings (right panel) for PS and non-PS supporters in January and April, normalized at the baseline value for each group. While the probability of employment and wage earnings of non-PS supporters increases by 0.9 $(= 100 \times (0.226 - 0.217))$ percentage points and 424 lek, respectively, between January and April, the corresponding increases for PS supporters are 1.3 percentage points and 661 lek. The double differences − 0.37 percentage points for employment and 237 lek for wage earnings − are statistically different from zero at the 10 percent level.

TABLE 1 - Descriptive statistics (panel dataset)

	Observations	PS (38.2%)		non-PS (61.8%)		mean Δ	mean Δ
	(individuals)	mean	(s.d.)	mean	(s.d.)	Unconditional	In poll.stat.
Panel A: Individual (time-invariant) characteristics							
Age	424,835	40.61	(12.91)	39.56	(12.60)	0.08	0.08
Male	424,835	0.571	(0.495)	0.556	(0.497)	0.03	0.03
Household owns 1 car	424,835	0.309	(0.462)	0.249	(0.432)	0.13	0.14
Household owns >1 car	424,835	0.163	(0.370)	0.126	(0.331)	0.11	0.11
N. of household members	424,835	3.46	(1.38)	3.67	(1.46)	−0.06	-0.02
Born in Tirana Prefecture	424,827	0.489	(0.499)	0.400	(0.489)	0.18	0.09
Born in rural locality	424,827	0.460	(0.498)	0.513	(0.500)	−0.11	-0.03
Never a migrant	424,835	0.883	(0.321)	0.790	(0.407)	0.25	0.26
Panel B: Labour market outcomes							
I(employed), January	424,835	0.370	(0.483)	0.217	(0.412)	0.152	0.151
I(employed), April	424,835	0.383	(0.486)	0.226	(0.418)	0.156	0.155
Wage earnings, January	424,835	20,749	(33,851)	11,465	(26,861)	9,283	9,036
Wage earnings, April	424,835	21,410	(34,181)	11,889	(27,197)	9,522	9,274
Wage earnings ($\neq 0$), January	117,004	56,120	(33,383)	52,797	(33,768)	3,323	2,640
Wage earnings ($\neq 0$), April	121,534	55,947	(33,476)	52,514	(33,673)	3,433	2,793

Standard deviations in parenthesis. The table summarises the individual-level, two-period panel dataset used in the main analysis. PS stands for Socialist Party (the incumbent party). April is the election month. Panel A reports standardized mean differences (Imbens and Rubins, 2015). The standardized difference is the mean difference divided by the squared root of the average variance across the two groups, and ranges between 0 and 1. Panel B reports simple mean differences. In the last column, we report differences (standardized in Panel A) conditional on polling station fixed effects.

FIGURE 3 - Employment and earnings of PS and non-PS supporters, January and April 2021



Source: The plots display the mean value of employment (left) and earnings (right) for PS and non-PS supporters in January and April, minus the corresponding baseline (January) value for each group (joint with 90% confidence intervals for the normalized means).

4 Empirical strategy

To estimate the causal effect of the 2021 election campaign on the labor market outcomes of incumbent party (PS) supporters, we employ a two-period difference-in-differences (DiD) approach. Specifically, we compare PS supporters and non-PS supporters before the start and at the end of the election campaign, i.e., in January and April 2021. Hence, we estimate the following OLS models:

$$I(employed)_{ipt} = \beta(PS_i \times t2_t) + \alpha_i + \tau_{pt} + \varepsilon_{ipt} \tag{1}$$

$$wage_{ipt} = \beta\,(PS_i \times t2_t) + \alpha_i + \tau_{pt} + \gamma I(employed)_{it} + \theta I(full\ time)_{it} + \eta_{ipt} \tag{2}$$

The left-hand side variables represent labor outcomes for voter i registered in polling station p in month t. PS_i is an indicator for the treated group (PS supporters), while $t2_t$ denotes the treatment period (April), during which the election campaign is under way. Individual fixed effects, α_i, absorb all level differences between PS and non-PS supporters, while τ_{pt} is a full set of polling station times $t2_t$ fixed effects, which control for differential trends in labor market outcomes across neighborhoods. Each polling station serves approximately 730 registered voters (s.d. = 190). Thus, our specification effectively compares the outcome evolution of PS and non-PS supporters living on the same street or at least in closely located dwellings. ε_{it} and η_{it} are error terms capturing all residual determinants of labor outcomes. We base inference on standard errors clustered conservatively at the polling station (neighborhood) level.[19]

The DiD parameter of interest (β) measures changes in the employment and earnings of PS and non-PS supporters living in the same neighborhood between January and April 2021. Under standard assumptions discussed in the next section, this double-difference identifies the average effects of the election campaign (the treatment) on PS supporters (the treated group). In equation (1), β captures the extensive margin of treatment. Conditional on employment and a proxy for the number of hours worked, the β parameter in equation (2) identifies the intensive margin of treatment.

4.1 Internal Validity

The OLS estimator of β is consistent for the ATT under the assumptions of no anticipation and conditional parallel trends. No anticipation requires that the employment and earnings of PS supporters in January were not affected by electoral mobilization. In our setting, this assumption is plausible. By law, the official campaign period begins one month prior

[19] We also experiment with clustering at the level at which treatment (the mobilization of PS supporters) is independently assigned (Roth et al., 2023), which in our setting is likely to be the individual level. Yet, doing so does not affect our inferences (results available upon request). Similarly, clustering at the household level does not alter any of our conclusions.

to election day (NDI, 2013). In practice, the canvassing of voters rarely starts earlier than three months before the poll (Kajsiu, 2016).

Parallel trends assumption. While the conditional parallel trends assumption permits non-neighbors to exhibit divergent labor outcomes[20], it requires that, in the absence of treatment, the employment and earnings trajectories of *close neighbors* with differing political orientations evolve in parallel. Imposing this requirement is plausible. The literature on neighbourhood choice finds that households tend to self-sort into neighbourhoods with matching socio-economic characteristics (Ioannides and Zabel, 2008; Aliprantis et al., 2024). Since the 1990s, Tirana has seen substantial inflows of internal migrants and rapid urban change and expansion (Vullnetari, 2012; Pojani, 2013). In this context, residential sorting is likely to have played a particularly important role in shaping neighborhood composition. Moreover, in the Balkan region, the neighborhood (*mahalla*) has historically functioned as a traditional unit of social organization (Elbasani and Roy, 2015), characterized by segmentation along ethnic, religious, and occupational lines, as well as a strong culture of local solidarity and mutual aid (*komshillëk*). Lastly, individuals from the same location have similar costs of labor supply (commuting times, search frictions). In sum, because of self-sorting, traditional segmentation, peer effects and labor market access, we can plausibly expect close neighbours to: 1. share similar characteristics; and 2. experience similar labor market trajectories in the absence of exogenous shocks.

Indirect evidence. Because we do not have pre-treatment data prior to January 2021, we are unable to formally test the parallel trends assumption. However, to further support our identification strategy, we present time-series evidence using aggregate data on public expenditure for personnel. We show that the government's payroll increases significantly before elections, but that this effect is entirely concentrated in the final pre-election quarter.

[20]These trends may also arise from unobserved characteristics of PS supporters, who tend to be concentrated in specific neighborhoods. Diverging trends may also arise if opposition strongholds receive less fiscal stimulus in pre-election periods (Zhllima et al., 2020).

We argue that this pattern is consistent with the labor market outcomes of PS and non-PS supporters following parallel trends during non-election periods.

We use data on aggregate government expenditure for the compensation of employees ($pexp_t$) collected by the Albanian Ministry of Economy and Finance. This variable includes all salaries and benefits paid by central and local government agencies and is available with monthly frequency between January 1999 and December 2021. To estimate the effect of elections, we construct a set of 10 indicator variables (Q_k, $-5 < k < 5$) for each of the 5 quarterly periods before and after parliamentary elections. In total, six elections took place during 1999-2021. We estimate the following equation:

$$\ln\left(pexp\right)_t = \sum_{k=-5}^{k=+5} \alpha_k Q_{kt} + \eta \ln\left(taxrev\right)_t + \sigma_t + u_t \tag{3}$$

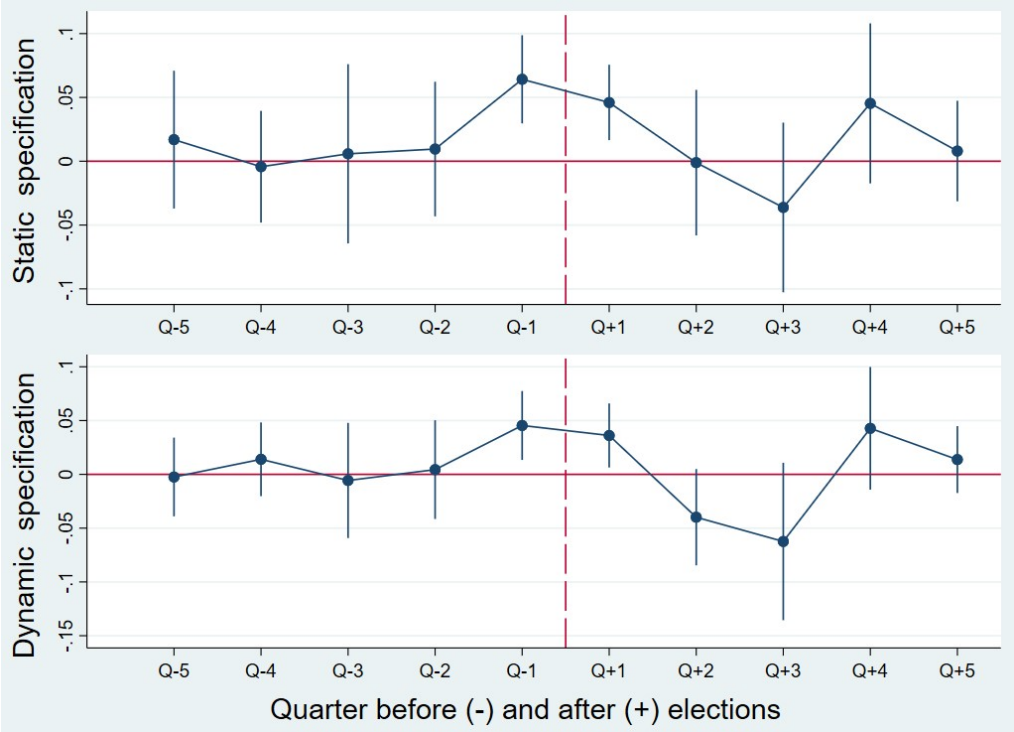
where t indexes months and σ_t is a full set of calendar-month dummies. We also control for aggregate tax revenues ($taxrev_t$), which may influence the government's willingness to spend on personnel and are also potentially responsive to macroeconomic fluctuations induced by elections (Lami et al., 2021).[21] Since u_t, the model error, is subject to autocorrelation, we also consider a dynamic specification that conditions the estimates of α_k on several lags of the dependent variable.[22]

The OLS estimates of α_k in the static (equation 3) and dynamic specifications are plotted in Figure 4 (top and bottom panel, respectively), and also reported in full in Appendix Table C.1 (columns 1 and 2). In the last three months (quarter Q_{-1}) preceding parliamentary elections, the government's aggregate payroll is, on average, 5-6 percent higher than

[21] $taxrev_t$ measures the total revenues collected by tax and customs offices around the country. The data was obtained from the Ministry of Finance and Economy. We deflate the personnel expenditure and tax revenues series, which are reported in current Albanian leks, using a CPI index from Albania's National Institute of Statistics (INSTAT). The base year is 1999.

[22] We use seven lags of the dependent variable, as this is the simplest, "dynamically complete" specification that fully removes serial correlation from the regression residuals (see Appendix Table C.1 for details). In this specification, we also include six distributed lags of $\ln\left(taxrev\right)_t$.

FIGURE 4 - Election cycles in public expenditure for personnel (1999-2021)



Source: The number of observations (months) is 276 in the top panel, and 269 in the bottom panel. The vertical axis indicates the magnitude of the estimated coefficients on the 10 quarterly dummies, along with a 90% confidence interval. Both specifications control for tax revenues (in logs) and include a full set of calendar-month dummies.

in non-election periods.[23] The coefficients on the dummies representing non-election periods (from Q_{-5} to Q_{-2}), which encompass January 2021, are close to zero and statistically insignificant − both individually and jointly − indicating that the pre-election effect is entirely concentrated in the final pre-election quarter. Following an election, the government's payroll remains elevated for an additional three months before reverting to the long-term conditional mean.[24] Appendix Table C.1 shows that these patterns are robust to including additional co-variates and trend terms.[25]

Overall, these findings are consistent with PS and non-PS supporters exhibiting parallel and flat trends in public-sector employment during non-election periods (between Q_{-5} and Q_{-2}), with no significant increase in employment among PS supporters except in the final pre-election quarter (Q_{-1}).[26]

5 Results

The estimates of equation (1) and (2) are displayed in Table 2, columns (1) and (3). For the average PS supporter, the increase in the probability of getting a job over the last pre-election quarter is 0.34 percentage points larger than for the average non-PS supporter. Under no anticipation and parallel trends, this double difference identifies the ATT: the

[23] This finding is consistent with anecdotal evidence. Investigative journalists calculated that before the 2023 local elections, the administrative staff of municipal governments increased by around 9 percent (see `https://www.reporter.al/2023/10/02/punesimet-elektorale-fryne-administraten-e-bashkive-perpara-zgjedhjeve-vendore/`). Newspapers also reported that in 2021 the number of people employed in the public adminsitration expanded by 4.5 percent (see `https://www.syri.net/politike/795983/punesime-elektorale-si-u-fry-administrata-publike-ne-prag-te-zgjedhjeve-te-11-majit/`).

[24] This finding is consistent with anecdotal evidence pointing to the temporary nature of patronage jobs in Albania (see footnote 3).

[25] In particular, these findings remain unaltered if we control for: a dummy for the periods during which the PS was in power, to account for partisan preferences; and a dummy for periods with an active IMF loan programme, to net out the influence of austerity measures (column 3). The results are also robust to adding cubic trend terms ($\theta t + \vartheta t^2 + \varphi t^3$), as shown in column 4.

[26] These findings also align with a large body of literature documenting that incumbents increase public spending and expand the money supply in the run-up to elections (Alesina et al., 1997). Political budgetary and monetary cycles have been found to be particularly pronounced in emerging democracies (Block, 2002) and before elections where international observers report instances of vote buying (Aidt et al., 2020).

TABLE 2 - Political alignment and labour market outcomes: main results

Dependent variable:	I(employed)		wage earnings	
	(1)	(2)	(3)	(4)
PS $\times t2$	0.0034***	0.0021**	116.0***	88.8***
	(0.0006)	(0.0008)	(21.3)	(27.6)
PD $\times t2$ (omitted category)		–		–
Other party $\times t2$		−0.0010		−25.8
		(0.0010)		(31.7)
"Blank" preference $\times t2$		−0.0025***		−50.7
		(0.0009)		(31.3)
Individual fixed effects	YES	YES	YES	YES
Polling-station $\times t2$ fixed effects	YES	YES	YES	YES
I(employed) and I(full-time)	No	No	YES	YES
Individuals	424,835	424,835	424,835	424,835
Observations	849,670	849,670	849,670	849,670

OLS regressions with robust standard errors clustered at the polling station level. PS refers to the incumbent Socialist Party. In models (2) and (4), the reference category is the main opposition party, the Democratic Party (PD). All parties other than PS and PD are classified as 'other party'. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

election campaign increases the probability of employment of the average PS supporter in Tirana by 0.34 percentage points. This effect corresponds approximately to a 1 percent increase relative to a baseline (January) employment probability of 37 percent (see Table 1).

Conditional on having or obtaining a job, the treatment also has a significant positive effect on the average PS supporter's monthly earnings, which increase by 116 leks (1.2 US$) more than those of the average non-PS supporter − a 0.6 percent increase relative to baseline earnings. The effect on earnings is slightly smaller than the effect on employment, suggesting that the election campaign mainly benefits PS supporters at the extensive margin. Furthermore, the effect at the intensive margin may either reflect bonuses and premia received by PS-supporting incumbents, or a wage premium granted to newly hired PS

supporters, or both.[27] Overall, a pre-election increase in the employment and earnings of incumbent supporters is consistent with, and explains, the aggregate increase in government payroll spending observed over the same period (Figure 3).

In columns (2) and (4), we compare the evolution of employment and earnings of PS supporters with that of supporters of the main opposition party (PD), rather than with non-PS supporters in general. The results are broadly consistent. While voters with unclear ("blank") preferences may fare worse than both PD and PS supporters, the labor outcomes of smaller opposition party supporters are statistically indistinguishable from those of PD supporters. In Appendix Table C.3 (columns 1, 4), we also show that these effects are observed in both the city of Tirana and the four peri-urban municipalities that make up the prefecture. They also hold uniformly for male and female supporters (columns 2 and 5).

The estimated labor market returns of "grassroots" supporters, albeit precisely estimated, are substantially smaller in magnitude than the effects on "elite" supporters reported in the literature (Brassiolo et al., 2020; Colonnelli et al., 2020). This difference is plausible. The treated group in our study is composed of *all* voters with a political preference for the incumbent, and is therefore *much* larger numerically than the treated group considered in previous studies (non-elected candidates and campaign donors). For this reason, the effects that we estimate likely conceal substantial heterogeneities, which we explore in section 6.

5.1 Identification and robustness checks

While we find no evidence that contradicts the assumptions required for identification, we cannot rule out that supporting the PS correlates with other individual-level unobservables inducing differential trends in labor outcomes within neighborhoods (Kahn-Lang and Lang,

[27] In additional regressions, we also estimate a positive coefficient on $PS \times t2$ in regressions that restrict the sample to individuals who were newly hired in April (630.7, s.e. = 430.1, N = 17,988) and to incumbents that were in employment in both periods (117, s.e. = 56.9, N = 224,984), suggesting that both mechanisms may be operative. Full results are available upon request.

2020).[28] To attenuate this concern, we conduct a battery of robustness checks.

Covariates and matching. We first consider versions of equations (1) and (2) that condition the estimates of β on an additional set of $X_i \times t2_t$ interactions, where X_i includes all the voter characteristics observed in our data.[29] Including such a rich set of co-variates allows for a wide range of voter characteristics and their correlates to generate differential trends around elections.[30] The estimates are reported in Table 3 (columns 1 and 2). The β coefficients in the employment (wage) equation are 38 (20) percent smaller in magnitude, and slightly less precisely estimated, than the corresponding estimates shown in Table 2 (columns 1 and 3). Yet, they remain positive and highly statistically significant.

As an alternative approach, we also present estimates based on matched samples (Heckman et al., 1997).[31] Specifically, we exactly match PS supporters to their non-PS peers with identical characteristics, using our full set of covariates X_i and the polling station information as matching variables.[32] We then compute DiD estimates between treated units and the matched comparison group (Abadie and Imbens, 2011). This matching estimator is consistent for the ATT provided that PS and non-PS supporters *with the same characteristics (X_i) and location* experience parallel trends in the absence of elections. The results are shown in columns (3) and (4) of Table 3. Since for some combinations of covariate values a match cannot be found, the sample available for estimation is substantially smaller. Yet, the estimates of β are remarkably similar to those reported in Table 2.

Because PS and non-PS supporters are not balanced on pre-treatment outcomes (Table

[28] We note that this concern would remain even if we could generate direct evidence showing no pre-treatment differences in trends.

[29] We include age, age squared, gender, car ownership, household size, being a Tirana native, a dummy for having been born in a rural locality, and a dummy for being a return migrant. Car ownership is measured by two dummies for individuals belonging to households owning 1 and more than 1 car.

[30] Roth et al. (2023: 2230) criticise the choice of augmenting a DiD specification with time-by-covariate interactions.

[31] Caetano and Callaway (2024) argue that DiD specifications that condition the estimates on $X_i \times t2_t$ only recover the ATT under additional homogeneity assumptions.

[32] This procedure almost completely eliminates any remaining imbalance on X_i between PS and non-PS supporters.

1, Panel B), we also report estimates that enforce pre-treatment balance in employment and labour income by restricting the sample to individuals that were not formally employed in January (columns 5-6). In Albania, long-term unemployment (> 12 months) is estimated to account for over 75% of total unemployment (EBRD, 2024). For this reason, the PS and non-PS supporters who were out of formal employment in January are likely to have experienced parallel (and flat) pre-trends in employment and labour income ($= 0$) in the previous few months. The results of this test confirm our previous findings.

TABLE 3 - Correcting for potential violations of parallel trends

	Conditioning on covariates		Matched samples		Not employed in Jan.	
Dependent variable:	I(employed)	wage earnings	I(employed)	wage earnings	I(employed)	wage earnings
	(1)	(2)	(3)	(4)	(5)	(6)
PS $\times t2$	0.0021***	92.4***	0.0033***	109.6***	0.0157***	27.2**
	(0.0006)	(21.5)	(0.0008)	(25.9)	(0.0008)	(25.9)
Individual fixed effects	YES	YES	YES	YES	YES	YES
Polling-station $\times t2$ fixed effects	YES	YES	No	No	YES	YES
Additional interaction terms ($X_i \times t2_t$)	YES	YES	No	No	No	No
$t2$ dummy	No	No	YES	YES	No	No
I(employed) and I(full-time)	No	YES	No	YES	No	YES
Individuals	424,825	424,825	245,572	245,572	307,830	307,830
Observations	849,650	849,650	491,144	491,144	615,660	615,660

OLS regressions with robust standard errors clustered at the polling station level. PS refers to the incumbent Socialist Party. X_i includes controls for age (age and age squared), gender, car ownership (dummies for households owning one car, and more than one car), household size, being a Tirana native, being from a rural area, and a dummy for former migrant status. In columns (3) and (4), the regressions compare the evolution of labour outcomes for PS and non-PS supporters using a matched sample constructed using X_i. For the two continuous variables (age and household size), exact matching is performed using the quintiles of age and a dummy for being from a household of above-median size. Column (5) and (6) restrict the sample to individuals who were not formally employed in January 2021. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

It is still possible that matched PS and non-PS supporters may differ in ways that are not accounted for by our vector of covariates (X_i). Within the same neighborhood, for instance, PS supporters may be concentrated in dwellings of systematically different quality, reflecting omitted influences. To evaluate the extent to which our results are driven by unobservable confounders, we also perform a test in the spirit of Altonji et al. (2005). In particular, we re-run the analysis within progressively finer spatial units, controlling for: a general prefecture-wide trend ($t2_t$), municipality-specific trends, and finally our polling station-specific trends (τ_{pt}). The estimates of β are remarkably stable (see Appendix Table C.2). To explain away the entire estimated effect, the confounding influence of trends induced by PS supporters' unobserved characteristics would need to be – implausibly – 7 to 8 times larger than the influence of neighborhood-specific (polling station) trends. We conclude that our findings are unlikely to be driven by unobserved correlates of political orientation.

Reverse causation. An additional concern is that our estimates of β may reflect endogenous shifts in political orientation. Individuals hired or promoted on grounds of ability may switch their preferences to the PS out of gratitude and reciprocity, implying a violation of parallel trends. We consider this mechanism as implausible because political preferences are measured well before the last pre-election quarter (see section 3.1) and are therefore pre-determined relative to labour outcomes.

Yet, to further address this concern, we estimate versions of equations (1)-(2) that distinguish long-term PS loyalists from recent switchers. The former are defined as voters that supported the PS both in the run-up to the 2013 and the 2021 elections. The latter are defined as voters that switched allegiance from any other party to the PS during 2013-2021. Compared to switchers, loyalists can be assumed to hold more deep-rooted and stable political preferences, and thus to be less likely to revise them in response to obtaining a job. If the findings in Table 2 primarily reflected preference shifts induced by hiring or bonuses, we would expect no effect among loyalists. However, as shown in Table 4 (columns

1–2), the estimated effects for loyalists are both significantly positive and statistically indistinguishable from those observed for switchers. To confirm these findings, columns 3-4 present regressions that examine changes in labor outcomes for non-PS supporters.[33] Here, we distinguish voters that were long-standing opponents of the PS ("non-PS loyalists") from voters that switched *away* from the PS during 2013-2021. Consistent with previous findings, the treatment effect on long-standing opponents is significantly *negative* and statistically indistinguishable from the effect on recent "defectors". Overall, this evidence indicates that our results are unlikely to reflect the reverse effect of hiring on preference formation.

[33]We code a dummy that takes the value 1 if a voter supports any party other than the PS, and 0 if they support the PS or their preference is coded as blank.

TABLE 4 - Reverse causality and measurement error

Dependent variable:	Loyalists vs. switchers (PS)		Loyalists vs. switchers (non-PS)		Measurement error	
	I(employed)	wage earnings	I(employed)	wage earnings	I(employed)	wage earnings
	(1)	(2)	(3)	(4)	(5)	(6)
PS (loyalists) $\times$ $t2$	0.0019**	75.1**				
	(0.0009)	(32.3)				
PS (switchers) $\times$ $t2$	0.0019*	29.9				
	(0.0011)	(44.3)				
non-PS (loyalists) $\times$ $t2$			−0.0020**	−54.4*		
			(0.0009)	(32.9)		
non-PS (switchers) $\times$ $t2$			−0.0019**	−68.9**		
			(0.0009)	(33.7)		
PS (certain) $\times$ $t2$					0.0042***	156.2***
					(0.0008)	(26.5)
PS (uncertain) $\times$ $t2$					0.0021**	59.7**
					(0.0009)	(28.5)
Equality [p-value]	[0.954]	[0.335]	[0.901]	[0.647]	[0.053]	[0.006]
Individual fixed effects	YES	YES	YES	YES	YES	YES
Polling-station $\times$ $t2$ fixed effects	YES	YES	YES	YES	YES	YES
I(employed) and I(full-time)	No	YES	No	YES	No	YES
Individuals	226,674	226,674	226,674	226,674	424,835	424,837
Observations	453,348	453,348	453,348	453,348	849,670	849,674

OLS regressions with robust standard errors clustered at the polling station level. Loyalists had the same preference (PS or non-PS) in 2013 as in 2021; switchers had different political preferences in 2013 and in 2021. Columns (5) and (6) distinguish voters whose preference for the PS is estimated with and without certainty. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Measurement error. Another possible concern is the extent to which measurement error in our PS_{ip} indicator (see section 3.2) is responsible for the effects estimated in Table 2. Misclassification is a type of non-classical measurement error that introduces attenuation bias. When a misclassified indicator enters a multiplicative interaction term, however, the direction of bias (attenuation vs. amplification) is unclear (Muff and Keller, 2015). To rule out a spurious interaction effect, we use the "certainty" estimates from the political preference dataset to distinguish between PS supporters that have a "high" or a "low" likelihood of being misclassified as PS supporters. In columns 5 and 6 of Table 4, we allow the treatment effects to be different across these two groups of PS supporters. In both the employment and earnings equation, the estimated DiD coefficients are about *twice* as large for a treatment group composed of PS supporters with a "low" likelihood of being misclassified. This finding is consistent with misclassification bias causing attenuation. Therefore, our estimates in Table 2 are best interpreted as providing a lower bound to the true treatment effect.

Additional robustness analysis. Lastly, in Appendix D, we report a number of further robustness checks that show that our findings are not an artifact of the choices that we make when constructing our dataset and defining the outcome variables.

6 Mechanisms

Our results show that the 2021 election campaign improved the labor market outcomes of incumbent party supporters. In this section, we delve deeper into the potential mechanisms behind our estimates and assess alternative explanations. Overall, the evidence indicates that mechanisms such as information advantages or ideological alignment cannot explain the results, and instead point to the exchange of jobs for campaign support as the most plausible explanation. We also provide evidence that job patronage originates primarily in the public sector, in line with expectations, and is unlikely to persist beyond a single

electoral cycle.

6.1 Job patronage or informational advantage?

It is possible that, following two consecutive PS terms in office, recruiters in both the public and private sectors may have gained improved access to "soft" information about PS-leaning job candidates, perhaps through access to informal networks. This mechanism implies that the true ability of PS-supporting candidates may be more "legible" to employers, thus making PS supporters more employable than otherwise similar non-PS supporters.

However, if "legibility" was the main mechanism at play, we would expect the estimated effects to be uniform across PS supporters. Instead, we uncover substantial effects heterogeneities based on voter characteristics. Specifically, the gains are concentrated among PS supporters with a low cost of mobilizing and aggregating votes. These include young voters, individuals from staunchly PS-supporting families, those who never migrated abroad, and Tirana natives.

Specification. To examine heterogeneous effects, we introduce triple interaction terms into equations (1) and (2), allowing the estimated treatment effects (β) to vary by voter characteristics. For instance, when the outcome is employment, we consider equations of the following form:

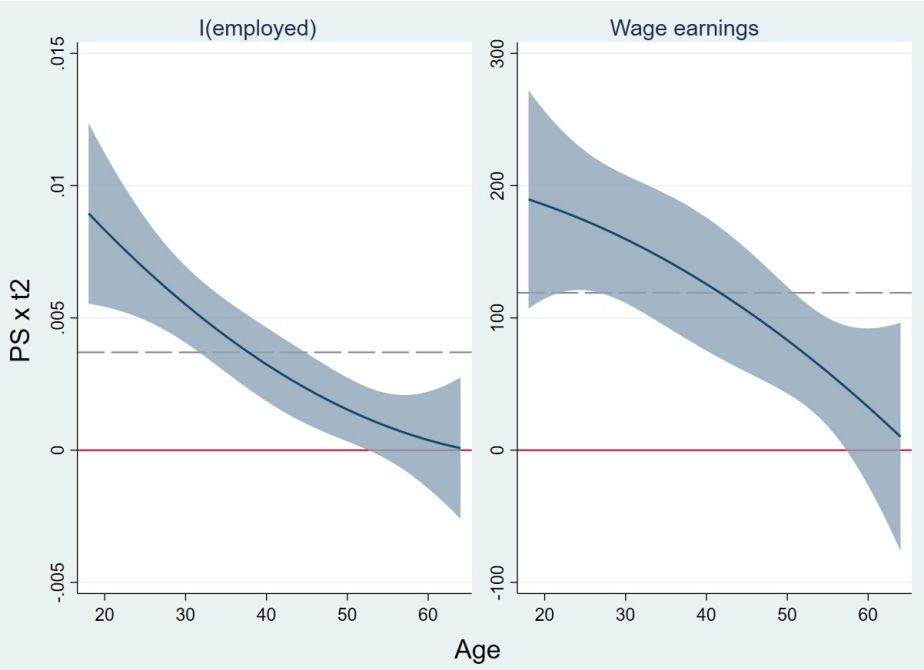
$$I(employed)_{ipt} = \beta_o(PS_i \times t2_t) + \beta_1 (PS_i \times t2_t) \times z_i + \beta_2 (z_i \times t2_t) + \alpha_i + \tau_{pt} + \varepsilon_{ipt} \quad (2)$$

where z_i is an effect-modifying voter characteristic and all the other variables are defined as before.[34]

Voter's age. We begin by investigating how the magnitude of the treatment effect varies with voter age. To do so, we let $z_i = \alpha_1 age_i + \alpha_2 age_i^2$, where α_1 and α_1 are parameters. The

[34] All the results reported in this section are robust to including a full sector of $X_i \times t2_t$ interactions in eq. 4 (full results available upon request).

FIGURE 5 - Heterogeneous effects: voter's age



Notes: The diagram displays the estimated ATT as a function of a voter's age, with the shaded area representing a 90 percent confidence band based on delta-method standard errors. The dependent variable is employment(left panel) and wage earnings (right panel). The dashed (grey) horizontal line represents the average effect across age groups (0.0037 in the left panel, and 119.0 in the right panel). The estimates are based on regression specifications similar to equation (4). The number of observations is 849,650 in both regressions.

estimates of the ATT, as a function of voter age in years ($= \beta_0 + [\beta_1\alpha_1]age_i + [\beta_1\alpha_2]age_i^2$) are shown in Figure 5.[35]

Young PS-aligned voters up to the age of 31 earn a pre-electoral labour market premium, in terms of both employment and earnings, that is significantly higher than the average effect across age groups (horizontal dashed lines).[36] These findings are consistent with a job patronage interpretation for three reasons. First, the political preferences of young adults are typically less entrenched than those of older voters (Quintelier, 2017). Therefore, young PS supporters can mobilize votes within their peer networks at a lower cost than their older counterparts. Second, because of high youth unemployment, the utility derived

[35]The ATTs are equal to $\frac{\partial I(employed)_{it}}{\partial(PS_i \times t2_t)}$ and $\frac{\partial wage_{it}}{\partial(PS_i \times t2_t)}$, respectively.

[36]For wage earnings, a one-sided test of the null that the estimated ATT at $age = 31$ (156.6) is smaller than or equal to 119 (the average effect across age groups) returns a p-value of 0.101.

by younger voters from having a job is higher than the utility derived by older voters. Accordingly, young PS supporters can be expected to exert greater effort in order to secure a job compared to their older counterparts. Third, campaigning work involves fieldwork, repeated meetings, and even participation in night-life events, all of which are better suited to young adults. For these reasons, politicians can earn a higher pay-off, in terms of number of votes gained, by offering patronage jobs to young supporters.

In addition, we note that compared to older individuals, young adults in their 20s are generally less likely to be party members, political candidates, or campaign finance donors. Thus, the evidence shown in Figure 4 should also alleviate concerns that our estimates are driven by a sub-set of elite party supporters, as suggested in previous studies within this literature (Brassiolo et al., 2020; Colonnelli et al., 2020).

Other characteristics. In Table 5, we examine heterogenous effects based on other voter characteristics. In columns (1)-(4), we let z_{ip} represent the proportion of PS supporters in voter i's household, excluding i from the count.[37] The estimated effects are found to be concentrated among PS supporters that belong to all-PS households. Specifically, the employment (wage) premium earned by deep-rooted PS supporters is around 37 (20) percent larger than for the average PS supporter.

This finding, too, is consistent with a job patronage mechanism. In elections with secret ballots, it is difficult to write a contract for a vote exchange (Robinson and Verdier, 2013). Being embedded in a staunchly PS-leaning family allows PS supporters to credibly commit to mobilizing votes for the party in the absence of a contract.[38] By contrast, "lone-wolf" PS supporters, whose preferences are at odds with their families', may be considered less trustworthy, and may thus be unable to credibly commit to mobilizing votes upon receiving a job. In line with this interpretation, "lone-wolf" PS supporters are found to enjoy no

[37] In these specifications, we also control for $(PS_{ip} \times t2_t) \times single_i$ and $single_{ip} \times t2_t$, where $single_{ip}$ is an indicator for individuals belonging to single-person households.

[38] For instance, politicians may be able to punish PS-aligned family members in case of opportunistic behavior.

significant labour market premium at all.

In columns (2)-(5), we distinguish between PS supporters with and without migration experience. The estimated effects are entirely concentrated amongst PS supporters that were never migrants abroad. As is well-known, going abroad erodes a migrant's social capital at home (Vullnetari, 2012: 45). Thus, compared to return migrants, PS supporters who never migrated are both more likely to be in patron-client relationships with politicians, and also to have access to a large network of potential voters to mobilize. Both types of connections reduce the costs of aggregating and exchanging votes for a job, making these highly networked PS supporters particularly effective campaign mobilizers.

TABLE 5 - Heterogeneous effect by voter characteristics

Dependent variable:	I(employed)			Wage earnings		
Heterogeneity by:	Family alignment	Migration	Birthplace	Family alignment	Migration	Birthplace
	(1)	(2)	(3)	(4)	(5)	(6)
PS $\times t2$	0.0002	0.0006	0.0018**	51.1	−43.4**	129.5***
	(0.0013)	(0.0008)	(0.0007)	(43.3)	(20.4)	(24.3)
PS $\times t2 \times$ Fraction PS in family	0.0042***			84.6*		
	(0.0015)			(48.9)		
PS $\times t2 \times$ Never a migrant abroad		0.0022**			168.0***	
		(0.0011)			(30.0)	
PS $\times t2 \times$ Born in Tirana Prefecture			0.0030**			−23.3
			(0.0013)			(38.5)
Individual fixed effects	YES	YES	YES	YES	YES	YES
Polling-station $\times t2$ fixed effects	YES	YES	YES	YES	YES	YES
I(employed) and I(full-time)	No	No	No	YES	YES	YES
Observations	848,498	849,670	849,650	848,498	849,670	849,650

OLS regressions with robust standard errors clustered at the polling station level. PS refers to the incumbent Socialist Party. Models (1) and (4) also control for $I(single) \times t2_t$ and $PS \times t2_t \times I(single)$, where $I(single)$ is a dummy for single-person households. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In columns (3)-(6), we distinguish between Tirana natives and internal migrants who were born in other cities. Similar to return migrants, internal migrants have less social capital than Tirana natives, and thus fewer connections with politicians and potential campaign targets. Accordingly, the estimated effects are found to be somewhat larger for (though, in this case, not *entirely* concentrated amongst) Tirana-born PS supporters.

Lastly, we include all four triple interactions − with age, family background, migration experience and the Tirana native dummy − simultaneously in the same specification.[39] Our main conclusions remain unaltered. The estimated treatment effects for a typical 24-year-old Tirana-born PS supporter, who never migrated abroad and belongs to a strongly PS-leaning household are 0.0080 (s.e. = 0.0014) in the employment equation and 188.8 (s.e. = 39.9) in the wage equation. Both effects are about *twice* as large as the corresponding average effects estimated across PS supporters (Table 2). By contrast, the treatment effects on a typical 40-year-old PS supporter who was born outside of Tirana, belongs to a family with mixed political affiliations, and previously lived abroad as a migrant are statistically indistinguishable from zero.[40]

Overall, the above findings indicate that the labour market premium received by PS supporters before the 2021 election is *entirely* driven by groups of supporters who can plausibly aggregate and transfer votes to the PS at a low cost. By contrast, PS supporters with a higher cost of aggregating and transfering votes are found to enjoy no premium at all. Although these observed heterogeneities do not offer direct evidence of politically motivated hiring, they are consistent with incumbent supporters enjoying a labour market advantage stemming from a jobs-for-votes exchange, in line with a job patronage interpretation.

[39] Results not shown in full.

[40] The estimates are -0.0009 (0.0011) in the employment equation and -20.9 (s.e.= 29.6) in the wage equation.

6.2 Vote sellers or vote aggregators?

Our findings are also inconsistent with a simple story of politicians buying *individual* votes with jobs. If patronage jobs were used to buy a prospective employee's *own* preferences, the main targets of patronage would be undecided voters. Yet, the results in Table 2 indicate that the labor outcomes of voters with "blank" preferences, if anything, improve *less* than those of PS supporters, whose allegiance is known in advance by the PS and does not need to be bought. Rather, the findings are consistent with PS supporters' obtaining a job by mobilising and aggregating *additional* votes.

To do so, PS supporters may either rely on ties of reciprocity or use monetary inducements − for instance, a fixed share of their salary. They may also target their immediate family, or canvass more distant relatives and friends. If the immediate family was the main target, we should expect to find that the beneficiaries of patronage jobs are mainly "lone-wolf" PS supporters, whose family members have yet to be won over. Yet, the evidence shows that the estimated effects are concentrated among PS supporters whose families are *already* aligned with the PS (see Table 5), indicating that a supporter's immediate family is more likely to function as a guarantor of trustworthiness than a campaign target.

Furthemore, if votes were mainly obtained by canvassing a supporter's immediate family, it would be rational for the incumbent to channel patronage jobs to PS supporters with large households. Yet, as we show in Appendix Table C.3 (columns 3, 6), PS supporters with large and small households enjoy nearly identical labour market premiums.

6.3 Public or private sector?

Are the jobs used to reward campaigning supporters provided by the public or the private sector? Since politicians may retain some discretion in the selection of public servants, we should expect patronage jobs to come primarily from the public sector. Still, firms with personal ties to the incumbent party may also choose to hire political supporters in exchange for, or in anticipation of, benefits such as favors, policies, or transfer payments

TABLE 6 - Heterogeneous effects: Public vs. private sector

Sector X:	State bureaucracy (1)	State firms & services (2)	Private sector, domestic (3)	Private sector, foreign (4)
PANEL A - Dependent variable: I(employed in sector X)				
PS $\times t2$	0.0009^{***}	0.0002^{**}	0.0004	-0.0003
	(0.0002)	(0.0001)	(0.0004)	(0.0002)
PANEL B - Dependent variable: wage earnings in sector X				
PS $\times t2$	38.3^{***}	15.6^{***}	29.6^{**}	15.6
	(8.7)	(5.9)	(12.8)	(11.7)
I(employed in sector X)	$33,761^{***}$	$35,201^{***}$	$20,123^{***}$	$25,661^{***}$
	(1,155)	(1,914)	(244)	(960)
I(full-time in sector X)	$18,739^{***}$	$20,313^{***}$	$14,624^{***}$	$19,454^{***}$
	(905)	(1,526)	(227)	(829)
Individual fixed effects	YES	YES	YES	YES
Polling-station $\times t2$ fixed effects	YES	YES	YES	YES
Observations	825,586	825,586	825,586	825,586

OLS regressions with robust standard errors clustered at the polling station level. PS refers to the incumbent Socialist Party. The dependent variable is a dummy that takes the value 1 if individual i at time t is employed in the specified sector (X), and 0 if he/she is non-employed or is employed in a different sector to the one specified. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

from politicians (Gagliarducci and Manacorda, 2020).

To examine which sectors supply patronage jobs, we replace $I(employed)_{it}$ in equation (1) with indicators $I(employed\ in\ X)_{it}$ for being employed in sector X at time t. To investigate the impact on wage earnings, we use $wage_{it} \times I(employed\ in\ X)_{it}$ as dependent variables, and condition the estimates on $I(employed\ in\ X)_{it}$ and $I(fulltime)_{it} \times I(employed\ in\ X)_{it}$. We consider four sectors (X): the public administration; state-owned companies and service-providers; the domestically owned private sector; and foreign-owned private firms.

The results are reported in Table 6. The evidence indicates that patronage jobs are

mainly concentrated in the public administration (column 1) and, to a lesser extent, in state-owned enterprises and service-providers (column 2). This finding is consistent with politicians having at least some discretion over recruitment in state-controlled organizations, particularly the civil service. A pre-election increase in the probability of PS supporters' obtaining a job or salary raise in the public administration (column 1) also explains the aggregate increase in the government's total payroll observed in the last quarter before elections (Figure 3). In the domestic private sector (column 3), where politicians can still influence the hiring process but only indirectly, the effect on employment is positive and economically relevant, but quite noisy.[41] In the wage equation, however, the β coefficient is statistically significant, consistent with anecdotal reports of firm owners offering their employees a one-off salary bonus in exchange for their vote (see section 2.1). Lastly, in foreign-owned private firms, where managers are typically less connected to local politicians, the estimated effects are small and statistically insignificant. Similar to those in section 6.1, these findings are consistent with a job patronage interpretation of our main results, but not with an information mechanism.

Since job patronage is primarily concentrated in the public administration, an alternative interpretation is that the estimated pre-election effects on PS supporters reflect recruiters favouring ideologically aligned candidates. Ideological alignment enables team cohesion, promotes workers' motivation, and can facilitate the implementation of the government's policy agenda. We regard this possibility as implausible, however. First, Albanian political parties, including the PS, have a relatively weak ideological profile (see section 2.1). Second, considerations of ideological alignment should be *less* (not more) binding during election campaigns than in non-election periods. Third, and most importantly, ideological alignment may be a relevant consideration in the recruitment of directors and high-level

[41] These findings may suggest that an indirect patronage mechanism may be operative in at least some private sectors (potentially, those with stronger connections to the state). Unfortunately, the salary dataset does not contain an industry classification of employers.

TABLE 7 - Heterogeneous effects: High-/low-ranking occupations

Occupation type X:	High-ranking	Low-ranking
	(1)	(2)
PANEL A - Dependent variable: I(employed in occupation type X)		
PS $\times t2$	0.0002^{**}	0.0012^{***}
	(0.0001)	(0.0004)
PANEL B - Dependent variable: wage earnings in occupation type X		
PS $\times t2$	36.8^{***}	53.8^{***}
	(8.4)	(17.5)
I(employed in occupation type X)	$46,418^{***}$	$22,492^{***}$
	(1,796)	(283)
I(full-time in occupation type X)	$22,049^{***}$	$16,459^{***}$
	(1,552)	(264)
Individual fixed effects	YES	YES
Polling-station $\times t2$ fixed effects	YES	YES
Observations	825,586	825,586

OLS regressions with robust standard errors clustered at the polling station level. PS refers to the incumbent Socialist Party. The dependent variable is a dummy that takes the value 1 if individual i at time t is employed in the specified occupation type (X), and 0 if he/she is non-employed or employed in a different occupation type. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

bureaucrats, but is unlikely to play any role in the selection of frontline providers and low-ranking staff.

To test this, Table 7 presents models that replace $I(employed)_{it}$ in equation (1) with indicators for having a high-ranking (column 1) or a low-ranking (column 2) occupation in the public sector (see section 3.3). The results indicate that, if anything, the election campaign has a *larger* effect on PS supporters' access to *low-ranking* jobs (e.g. labourer, assistant), for which ideological considerations are less relevant (if relevant at all). In fact, being less competitive in the labor market, candidates for low-skill jobs may be more willing to be enlisted in the election campaign. This evidence, therefore, is again indicative of a job patronage interpretation of our main findings.

6.4 Temporary or permanent effects?

Lastly, we ask whether the labor market advantage gained by incumbent supporters during the election campaign persists beyond the pre-election period. As shown in Figure 3, aggregate public expenditure for personnel reverts to the long-term mean three months into the new legislature. This finding is consistent with PS supporters being hired on temporary contracts (see Section 2.1). However, it may also reflect the strategic dismissal of opposition supporters in the public administration to accommodate newly hired PS supporters. In the latter case, the employment effects of job patronage would persist into the new legislature. It is plausible that both mechanisms may be at play.

To further assess the persistence of employment effects, Table 8 (Panel A) presents correlational evidence that in January 2021 (3.6 years after the 2017 parliamentary election), the average PS supporter is 15 percentage points more likely to be employed, and earns 9500 lek more, than the average non-PS supporter.[42] By this time, however, those who had been PS supporters before the 2013 election (7.5 years before) exhibit no significant labour market

[42]These findings are consistent with those reported in Table 1, Panel B.

TABLE 8 - Temporary vs. Permanent Effects

	I(employed) (1)	wage earnings (2)
PANEL A - Dependent variable: Labour outcomes in Jan. 2021 (cross-section)		
PS (2021)	0.153^{*}	$9,536^{*}$
	(0.016)	(895)
	[0.062]	[0.063]
Observations	226,674	226,674
PS (2013)	0.031	1,715
	(0.009)	(669)
	[0.156]	[0.469]
Observations	226,674	226,674
PANEL B - Dependent variable: Labour outcomes (panel)		
PS (2021) $\times\, t2$	0.0019^{**}	60.0^{**}
	(0.0008)	(30.0)
PS (2013) $\times\, t2$	0.0001	8.8
	(0.0007)	(27.7)
Individual fixed effects	YES	YES
Polling-station $\times\, t2$ fixed effects	YES	YES
Individuals	226,674	226,674
Observations	453,348	453,348

Panel A: OLS (cross-sectional) regressions, equivalent to tests of mean differences (as in Table 1), with robust standard errors clustered by political preferences. Bootstrap p-values in brackets. Panel B: OLS (panel) regressions with robust standard errors clustered at the polling station level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

advantage. In Panel B (Table 8), we estimate regression models that augment equations (1) and (2) with a $PS_i^{(2013)} \times t2_t$ interaction term, where $PS_i^{(2013)}$ is an indicator for PS supporters in 2013. The coefficients on this term are small and statistically insignificant, confirming that the causal effects estimated in previous sections are driven by *current* rather than by past PS supporters. Taken together, we interpret these findings as suggestive evidence that the labor market effects of job patronage are unlikely to persist for longer than one electoral cycle (4 years).

7 Electoral consequences of job patronage

If jobs are awarded in exchange for votes, in equilibrium we should expect newly hired PS supporters to honor the *quid pro quo* and make effective efforts to mobilize votes. Is this indeed the case? Does it pay off for the incumbent to reward campaigning supporters with jobs? So far, we have observed only the "job side" of a presumed "jobs-for-votes" exchange. To zoom in on the "vote side", we examine whether the labor market gains of PS supporters are correlated with increases in the vote share of the PS (part III of the analysis, as described in the introduction).

To do so, we aggregate the labour market information from the "salary dataset" at the polling station level and combine it with the "voting dataset" (see section 3.2). We estimate the following equation for the vote share of the PS in the 2021 election:

$$PS\_voteshare_{ij}^{(2021)} = \rho PS\_voteshare_{ij}^{(2017)} + \beta \Delta y_{ij} + \gamma X_{ij} + \varphi_j + \epsilon_{ij} \tag{2}$$

where Δy_{ij} is either the total number of newly hired PS supporters[43], or the increase in total wage payments to PS supporters between January and April 2021 in polling station i

[43]This is the difference between the total number of jobs held by PS supporters in April and January.

of municipality j. This variable provides a measure of patronage transfers to PS supporters across neighborhoods (polling stations). In the average polling station, PS supporters started 1.7 new jobs during January-April 2021 (s.d. = 2.7) and total wage payments to PS supporters increased by 0.14 mln leks (s.d. = 0.36). The parameter of interest, β, measures the relationship between (presumed) patronage transfers and the PS vote share, conditional on the PS vote share in the previous election (2017). X_{ij} is a vector of control variables. In particular, to prevent β from picking up the influence of local economic conditions on voting behavior, we control for the total number of new jobs filled by neighborhood residents, regardless of political orientation (or the change in aggregate wage payments, depending on the specification).[44] Finally, φ_j represents municipality fixed effects.

The estimates of equation (5) are reported in Table 9. In Panel A, we use the change in the number of jobs secured by PS supporters as the main independent variable (Δy_{ij}), while in Panel B we use the change in total wage payments. In the models shown in column (1), we explore the simple relationship between the labour market gains of PS supporters and the party's electoral gains in 2021 relative to 2017. In columns 2-4, we progressively add controls for the total number of new jobs (or the change in aggregate wage payments), the full set of polling station-level controls used in Appendix Table A.1, and municipality fixed effects. In column 5, we use the number of new *public-sector* jobs awarded to PS supporters (Panel A), or the increase in *public-sector* wage payments to PS supporters (Panel B), as alternative measures of patronage transfers.

Across specifications, we find a positive correlation between the labor market gains of PS supporters and the gains in vote share experienced by the PS. Except for column 5, Panel B, the β coefficients are always statistically significant at conventional levels. Each additional job taken up by a PS supporter is associated with a 0.14 percentage-point increase in the PS vote share (column 4, Panel A). Similarly, an increase in pay-roll spending of 1 mln

[44] To reduce the correlation between this variable and Δy_{ij}, we remove the jobs and wages obtained by PS supporters from the total count.

lek on hiring PS supporters (corresponding approximately to 18 average monthly salaries), is associated with an increase in vote share of 0.9 percentage points (column 4, Panel B). These relationships are more pronounced when focusing on *public-sector* jobs (column 5, Panel A), in line with previous findings. Hiring an additional PS supporter in the public administration or in a state-owned company increases the PS vote share by almost half a percentage point, which in the average polling station corresponds to around two valid ballots ($= 0.464 \times 390 \ / \ 100$).

Since PS supporters took up 279 public-sector jobs during January-April, a back-of-the envelope calculation suggests that in the 2021 election a total of 505 PS votes may be attributable to the kind of job patronage documented in this paper. These votes account for 1.5 percent of the PS's gains at the ballot box relative to the previous election ($= 505 \ / \ (235,964 \ - \ 200,460)$), enough to determine the outcome of a close election. In practice, these effects are likely to be higher, as hired supporters may also mobilize votes outside of their assigned polling station, while the estimates in Table 9 assume no spill-over or general equilibrium effects.[45]

Overall, this correlational evidence is consistent with PS supporters reciprocating job offers by mobilzsing additional votes in their local network. Although the findings do not provide direct evidence of an illicit exchange, they document a suggestive relationship between jobs and votes. As such, they provide additional evidence in support of a job patronage interpretation of our DiD estimates.

[45]Furthermore, once hired, the PS supporters who helped mobilize votes may be asked to perform other tasks for the party.

TABLE 9 - Electoral consequences of job patronage

Dependent variable: PS vote share, 2021 (%)	(1)	(2)	(3)	(4)	(5)
PANEL A - Impact of additional jobs to PS supporters					
PS vote share, 2017 (%)	0.613***	0.603***	0.604***	0.697***	0.702***
	(0.025)	(0.025)	(0.026)	(0.028)	(0.027)
Δ (Apr-Jan) jobs to PS supporters (#) [β]	0.150**	0.160**	0.206***	0.136**	
	(0.067)	(0.066)	(0.066)	(0.062)	
Δ (Apr-Jan) public-sector jobs to PS supporters (#) [β]					0.464***
					(0.169)
Δ (Apr-Jan) jobs (#)		−0.198***	−0.089	-0.052	0.021
		(0.060)	(0.060)	(0.053)	(0.039)
Adjusted R^2	0.47	0.48	0.50	0.60	0.60
PANEL B - Impact of additional wage payments to PS supporters					
PS vote share, 2017 (%)	0.618***	0.617***	0.616***	0.704***	0.704***
	(0.024)	(0.024)	(0.026)	(0.028)	(0.028)
Δ (Apr-Jan) wages to PS supporters (mln lek) [β]	0.492*	0.519*	0.864***	0.877***	
	(0.299)	(0.296)	(0.293)	(0.290)	
Δ (Apr-Jan) public-sector wages to PS supporters (mln lek) [β]					0.677
					(1.619)
Δ (Apr-Jan) wages (mln lek)		−0.859**	−0.331	-0.335	0.238
		(0.363)	(0.276)	(0.255)	(0.216)
Adjusted R^2	0.47	0.47	0.50	0.60	0.60
Controls	No	No	YES	YES	YES
Municipality FE	No	No	No	YES	YES
Observations (polling stations)	1,165	1,165	1,165	1,165	1,165

OLS regressions with robust standard errors. The unit of analysis is the polling station. PS refers to the incumbent Socialist Party. "Δ (Apr-Jan) jobs to PS supporters" measures the number of PS-aligned registered voters who obtained a job between January and April 2021. "Δ (Apr-Jan) wage payments to PS supporters" measures the increase in total wage payments (in mln lek) to PS-aligned registered voters between January and April 2021. Controls include: the average age of registered voters, the gender balance, the share of former migrants and the share of Tirana-born registered voters. There are five municipalities within the Tirana county. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

8 Conclusions

Previous studies found that "elite" supporters of the political party in power (e.g. non-elected candidates, campaign finance donors) are more likely than political opponents to obtain a job in the public administration. Our paper complements these findings by showing that politicians may also use patronage jobs to enlist and reward ordinary voters who take part in the election campaign by mobilizing votes, leading to consequential effects on voting behavior and electoral outcomes.

Using a unique dataset of voters' political preferences from Albania, we showed that the employment and earnings of the average supporter of the incumbent party increase more in a pre-election period than those of the average opposition supporter. We presented evidence that supports a causal interpretation of this finding, and that pointed to job patronage as the most plausible mechanism. In particular, we found that: the incumbent party supporters who improved their labor market position the most in the pre-election period are those with the lowest cost of mobilizing and aggregating votes; that alignment with the incumbent mainly improves access to *public-sector* jobs, including low-ranking positions for which considerations of ideological alignment are mostly irrelevant; and that providing jobs to supporters helps the incumbent increase its vote share at the ballot box. These findings are consistent with anecdotal evidence from Albania indicating that, in pre-election periods, the party in power recruits politically aligned voters into the election campaign by offering them jobs in exchange for their help in securing additional votes for the party (see section 2.1).

We note that the form of job patronage documented in this paper is only one of several electioneering tools available to incumbents in pre-election periods. In addition to hiring ordinary voters in exchange for their help in the campaign effort, politicians may also directly buy votes from undecided voters. Yet, previous studies suggested that this second strategy is more likely to be used by challenger parties, who lack discretionary access to state resources such as jobs in the public administration, but must rely instead on cash payments

to reward supporters and finance their election campaign (Vicente, 2013). Further, using jobs in pre-election periods to reward "grassroots" supporters is compatible with using jobs in post-election periods to reward "elite" supporters, as documented in previous research (Brassiolo et al., 2020). We do not have the data or research design to study or compare these alternative forms of patronage and vote buying in the context of Albania. Yet, our results call for more research on the determinants of politicians' decision to use alternative patronage and vote-buying strategies.

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APPENDIX A. Data appendix

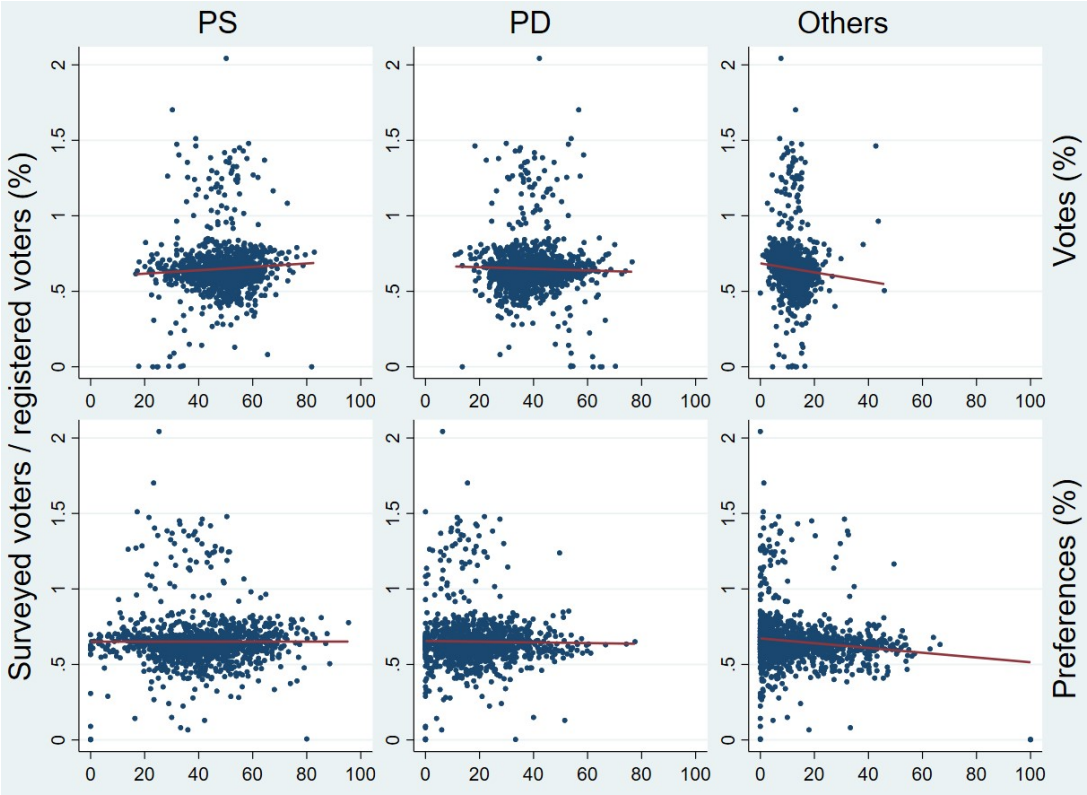
This appendix provides additional details on data construction and data validation that complement the data description provided in section 3.

Data sources. The (so-called) political preference dataset, compiled by the Albanian Socialist Party (PS), is contained in an Excel file that was leaked in unknown circumstances to an Albanian media portal (lapsi.al) on 11 April 2021. Conceivably, the leak was facilitated by an internal whistleblower. The file was then circulated informally via social media (mostly Whatsapp), and is now widely available amongst the general public.

The (so-called) salary dataset, which refers to the months of January and April 2021 and is based on administrative data collected from the General Tax Office, was created by two employees of the National IT Agency (*Agjencia Kombëtare e Shoqërisë së Informacionit*). In 2021, these two employees were based in the IT department of the General Tax Office. The data was first sold (illegally) to a foreign company's manager. It was then leaked in unknown circumstances in December 2021, and began to circulate via social media in January 2022. Similar to the political preference dataset, the salary dataset is now widely available amongst the general public.

The political preference dataset. In Table A.1 (column 1), we report the coefficient estimates of the bivariate regressions corresponding to the best-fit lines shown in Figure 2. Next, we collapsed various characteristics of surveyed voters at the polling station level to estimate the average age of voters in each polling station, the sex ratio, the share of former migrants, and the share of Tirana-born individuals. In column 2 (Table A.1), we condition the relationships shown in column 1 on these demographic characteristics of polling stations, which may predict voting outcomes (average age, sex ratio, share of former migrants and Tirana-born individuals). We also control for the number of registered voters, as a proxy for neighborhood size, and for the turnout rate (mean = 54.3), which we compute as the number of votes cast (whether valid or invalid) divided by the total number of registered voters. In column 3, we add municipality fixed effects to the specification in column 2. In both cases, the relationship between political preferences and voting behaviour remains qualitatively unaltered, confirming our conclusions regarding the reliability of the political

FIGURE A.1 - Sampling of voters across polling stations



Source: The number of observations (polling stations) is 1,231. The vertical-axis variable is the number of voters that enter the political preference dataset as a share of each polling station's registered voters.

preference estimates.

Lastly, we compute the proportion of registered voters in each polling station that appear in the political preference dataset (mean = 64.9, s.d. = 17.8), bearing in mind that the number of individuals on the voters' rolls may be substantially greater than the resident population of voting age due to out-migration (see section 3.1). Appendix Figure A.1 shows that the proportion of surveyed voters is unrelated to either preferences or voting behaviour. This finding suggests that the probability of being sampled does not depend on political orientation, consistent with a random sampling scheme. Overall, these findings suggest that the political preference data is reliable and representative of the underlying population.

The salary dataset. The original dataset consists of two separate files, which provide information on all wage payments reported to the General Tax Office in January and April 2021, respectively. There are 737,518 entries corresponding to active employment contracts. Of these, 100,381 entries are found only in the April file. As such, they represent all the

TABLE A.1 - Estimated political preferences and voting outcomes

Dependent variable: Votes (%)	(1)	(2)	(3)
PANEL A - Socialist Party (PS)			
Political preferences (%)	0.255***	0.221***	0.207***
	(0.018)	(0.017)	(0.020)
Adjusted R^2	0.18	0.25	0.28
PANEL B - Democratic Party (PD)			
Political preferences (%)	0.095***	0.107***	0.277***
	(0.026)	(0.023)	(0.025)
Adjusted R^2	0.02	0.15	0.31
PANEL C - Other parties			
Political preferences (%)	0.012	0.008	0.009
	(0.008)	(0.009)	(0.009)
Adjusted R^2	0.00	0.02	0.06
Controls	No	YES	YES
Municipality FE	No	No	YES
Observations (polling stations)	1,231	1,231	1,161

OLS regressions with robust standard errors. The unit of analysis is the polling station. The votes gained by each political party are measured as a share of the total number of valid ballots cast. The estimated number of individuals supporting a given party (preferences) is measured as a share of the total number of voters surveyed by the *patronazhistë*. The slope coefficients reported in column (1) correspond to the best-fit lines shown in Figure 2. Polling station-level controls include: the average age of registered voters, the gender balance, the share of former migrants and the share of Tirana-born registered voters. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

new hirings that occurred during February-April. 47,950 entries are found in the January but not in the April file, reflecting separations. The remaining 589,187 entries are found in both the January and April files and correspond to incumbencies. For each contract, the files provide information on the name and unique tax identifier of both employer (*NIPT*) and employee (*TIN*). We use the *NIPT* and *TIN* codes to merge the January and April files.

On average, the employees in the salary dataset hold 1.05 jobs, which implies that 7.6 percent of individuals in Albania have more than one job. To obtain an individual-level dataset that may be easily matched with the political preference dataset, we dropped individuals having more than one job. In total, there were 521,098 individuals in a single employment relationship in either January, April or both months. In Appendix D, we assess the sensitivity of our results to this choice.

Merging the datasets. We use the *TIN* number to match voters and workers across the political preference and salary datasets. This strategy enables a high-quality match across datasets. 29.6 percent of the Tirana voters surveyed in the political preference dataset appear in the salary dataset, indicating that they were formally employed in either January (separations), April (hirings) or in both months (incumbencies). The remaining voters who do not appear in the salary dataset can be classified as "non-employed".

12.7 (16.5) percent of the entries in the January (April) file of the salary dataset reflect declarations of self-employment. Since the outcome of interest if wage employment, we drop these entries from the dataset. In Appendix D, we assess the sensitivity of our results to this choice. Note that we only drop these entries after merging the salary and political preference datasets to ensure that unmatched self-employed voters are not mistakenly classified as non-employed in the main dataset.

In addition, since our population of interest is the population of voting *and* working age, we dropped all males younger than 18 and older than 65 years of age, and all females younger than 18 and older than 61 years of age, from the dataset. Appendix D shows that our results do not depend critically on dropping voters who are not of working age from the dataset.

APPENDIX B. Ethical appendix

Here, we briefly discuss the ethical implications of doing empirical research with leaked data such as the political preference and salary datasets. Previous economics research based on leaked or hacked sources includes Mironov (2013) and Fajardo (2020). Yet, unlike other disciplines (Boustead and Herr, 2020), economics has yet to grapple with the ethics of using unethically produced data.

Typically, when collecting private identifiable information from human subjects, researchers must first obtain informed consent. Informed consent requires that the subjects agree to the information provided being used for research purposes, and that they reliably communicate this agreement. When the data used in our research was collected, informed consent was neither sought nor obtained. The political preference data was collected (unethically) by the PS for political purposes, and leaked (arguably, ethically) by an unknown whistle-blower. The salary dataset was collected (ethically) by the Albanian tax authority for administrative purposes, and leaked (unethically) by internal employees. Thus, even though both sources are now in the public domain (at least in Albania), they should still be thought of as containing private information.

Previous authors argued that leaked private data can still be used ethically without the subjects' informed consent if it is "uniquely valuable and offers insights that would otherwise be unavailable" (Michael, 2015: 177), or if it was impossible to "gather all or most of the same data [about salaries and political orientation] in a more legal or more accepted manner" (Poor and Davidson, 2016: 5). In our case, both conditions arguably apply. Furthermore, US ethical guidelines for research involving human subjects (e.g. the Belmont Report) stipulate that informed consent may be waived if the research poses minimal risk and would be impossible without a waiver. We argue below that, indeed, our research only poses minimal risk. Lastly, US guidelines allow for a waiver on informed consent if the data is produced by observing public behaviour, as when a *patronazhist* estimates the political orientation of an individual without asking direct questions or interacting with them.

Our research poses minimal risk because it is based on information that is already in the public domain, as we were never involved in either hacking or leaking any information. In

the data-cleaning process, we deleted from our copies of the leaked datasets all individual identifiers except for the *NIPT* and *TIN* numbers (see Appendix A), which we use to match information across datasets. We also never examine, let alone report, the political preferences and salaries of either specific individuals or small groups of individuals such as the employees of specific firms. Thus, our readers could never infer, based on the results we report, the salaries or political preferences of any one individual, and cause them harm on that basis. Furthermore, while both the political preference and salary datasets circulated widely both in and outside of Albania, we are not actively contributing to their further dissemination.

Thus, the risks posed to the subjects *by our research* are close to nil. The benefits, by contrast, are substantial, since our (novel) research question can only be tackled with the leaked data that we have. Our research results can also contribute to denouncing the unethical practice of political parties' spying on voters, and thus provide a measure of redress to those affected by this practice in Albania.

Lastly, we note that we have no basis to conclude that the political preference data was collected by targeting vulnerable segments of the population. In fact, the aim of the PS was to obtain as wide and representative a sample as possible of the full population of Tirana Prefecture voters. We also have no basis to believe that the salary dataset was released selectively to harm specific individuals, firms, or industries.

APPENDIX C. Additional figures and tables

TABLE C.1 - Election cycles in public expenditure for personnel (1999-2021)

Dependent variable: ln(personnel expenditure)	(1)	(2)	(3)	(4)
Lags of the dependent variable (lincom)		0.733***	0.617***	0.214
		[0.108]	[0.119]	[0.293]
Quarters before (−) and after (+) elections:				
Q(+5)	0.008	0.014	0.012	0.003
	(0.024)	(0.019)	(0.018)	(0.020)
Q(+4)	0.045	0.043	0.042	0.037
	(0.038)	(0.035)	(0.032)	(0.030)
Q(+3)	−0.036	−0.062	−0.058	−0.054
	(0.040)	(0.044)	(0.045)	(0.045)
Q(+2)	−0.001	−0.040	−0.035	−0.020
	(0.035)	(0.027)	(0.027)	(0.026)
Q(+1)	0.046**	0.036**	0.039**	0.046**
	(0.018)	(0.018)	(0.019)	(0.022)
Q(-1)	0.064***	0.045**	0.047**	0.051***
	(0.021)	(0.019)	(0.021)	(0.019)
Q(-2)	0.009	0.004	0.006	0.005
	(0.032)	(0.028)	(0.027)	(0.025)
Q(-3)	0.006	−0.006	−0.005	−0.004
	(0.043)	(0.032)	(0.032)	(0.032)
Q(-4)	−0.004	0.014	0.013	0.009
	(0.027)	(0.021)	(0.021)	(0.024)
Q(-5)	0.017	−0.002	−0.002	0.004
	(0.037)	(0.022)	(0.023)	(0.021)
ln(tax revenues)	0.350***			
	(0.022)			
ln(tax revenues), long-run effect		0.263***	0.272***	0.212*
		[0.069]	[0.048]	[0.130]
Calendar month FE	YES	YES	YES	YES
Additional controls	No	No	YES	No
Cubic time trends	No	No	No	YES
[p-value of joint test]				[0.011]
Breusch-Godfrey χ^2	40.3***	5.7	5.3	5.1
Bartlett's B-statistic	2.761***	0.747	0.740	0.724
Adjusted R^2	0.69	0.74	0.74	0.75
Observations (months)	276	269	269	269

OLS regressions with Newey-West standard errors correcting for heteroskedasticity and error auto-correlation in parentheses. Models (1) and (2) correspond to the results plotted in Figure 3 (top and bottom panel, respectively). Columns (2)-(4) report a linear combination of the seven lags of the dependent variable that enter the equation, with delta-method standard errors in brackets. They also report the long-run effect of tax revenues, which is equal to the sum of the coefficients on the seven lags of tax revenues, divided by 1 minus the sum of the coefficients on the seven lags of the dependent variable (delta-method standard errors in brackets). The additional controls included in column (3) are: a dummy for periods during which the PS was in power; a dummy for periods during which an IMF loan programme was ongoing. The Breusch-Godfrey test is for the null of no error auto-correlation up to 3 lags. Bartlett's B statistic tests the null that the regression residuals are generated by a white-noise process. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE C.2 - Allowing for trends within progressively finer spatial units

Spatial unit (N units)	I(employment)		wage earnings	
	$\hat{\beta}$	$\hat{\beta}/\left\lvert\hat{\beta}^{prefect}-\hat{\beta}\right\rvert$	$\hat{\beta}$	$\hat{\beta}/\left\lvert\hat{\beta}^{prefect}-\hat{\beta}\right\rvert$
Prefecture (1)	0.00037		96.3	
	(0.0006)		(20.5)	
Municipalities (5)	0.0036	39.4	94.6	55.6
	(0.0006)		(20.8)	
Polling stations (1,232)	0.0033	7.1	109.7	8.2
	(0.0006)		(21.6)	

OLS regressions with robust standard errors clustered at the polling station level. Each coefficient corresponds to a different regression model along the lines of equation (1), but with different time trends (at the prefecture, municipality or polling station-level). All estimated coefficients are significant at the 1 percent level. All models are estimated on a restricted sample for which all municipality and polling station information is available. The number of observations is always 801,578. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE C.3 - Homogeneous effects: individual characteristics

Dependent variable:	I(employed)			Wage earnings		
Heterogeneity by:	City vs province	Gender	Household size	City vs province	Gender	Household size
	(1)	(2)	(3)	(4)	(5)	(6)
PS $\times t2$	0.0033***	0.0029***	0.0029**	96.5***	100.2***	139.7**
	(0.0007)	(0.0009)	(0.0009)	(26.2)	(29.9)	(54.9)
PS $\times t2 \times$ Peri-urban	0.0001			71.8*		
	(0.0014)			(43.0)		
PS $\times t2 \times$ Male		0.0007			28.4	
		(0.0012)			(38.5)	
PS $\times t2 \times$ N. of h'hold members			0.0001			−6.7
			(0.0004)			(12.9)
Individual fixed effects	YES	YES	YES	YES	YES	YES
Polling-station $\times t2$ fixed effects	YES	YES	YES	YES	YES	YES
I(employed) and I(full-time)	No	No	No	YES	YES	YES
Observations	849,670	849,670	849,670	849,670	849,670	849,670

OLS regressions with robust standard errors clustered at the polling station level. PS refers to the incumbent Socialist Party. The 'Peri-urban' dummy is equal to 1 if an individual is registered to vote in any of the four peri-urban municipalities that comprise the Tirana prefecture (*qark*), and 0 if they are registered to vote in the urban municipality (city) of Tirana. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX D. Additional robustness checks

Here, we present additional robustness checks on our main findings (section 5). Taken together, the findings presented here indicate that our conclusions are robust to alternative constructions of the main dataset (see Appendix A) and dependent variables.

In our main analysis, we dropped individuals with more than one job. In an alternative sample, we also include these individuals into the main dataset. Here, wage earnings are calculated as the sum total of the wages and salaries earned by an individual from all their active jobs, while individuals are classified as employed if they have *at least* one active job. The estimates of β in equations (1) and (2) are now 0.0033 (s.e. = 0.0006, N = 870,976) in the employment equation, as compared to 0.0034 in Table 2, column 1; and 187.6 (s.e. = 25.6) in the wage equation, as compared to 88.8 in Table 2, column 4. Including individuals with multiple jobs in our baseline sample leads to a substantially larger effect of treatment on wage earnings, conditional on employment. This finding is consistent with secondary jobs being awarded primarily based on political considerations.

In our analysis, we also dropped self-employed individuals to focus on transitions from non-employment (as defined in section 3.3) to employment, and vice versa. Yet, including formally self-employed individuals in the analysis, and treating them as non-employed (zero *wage* earnings), leaves our results qualitatively unaltered. The estimate of β is 0.0030 (s.e. = 0.0006, N = 883,830) in the employment equation, and 107.0 (s.e. = 19.9) in the wage equation.

Next, we check the robustness of our main findings to keeping individuals of retirement age in the sample. The β coefficients are now slightly smaller: 0.0020 (s.e. = 0.0005, N = 1,071,700) in the employment equation, and 85.0 (s.e. = 16.8) in the wage equation. Yet, they remain highly statistically significant.

Lastly, since the parallel trends assumption is sensitive to the chosen functional form of the outcome (Roth and Sant'Anna, 2023), we also estimate an alternative version of equation (2) with $\ln(wage_{ipt} + 1)$ as the dependent variable. The estimate of β (0.0026) remains positive and statistically significant (s.e. = 0.0010), implying that, on average, the wage earnings of PS-supporting workers increase almost 0.3 percent more than those of

opposition-supporting workers during January-April 2021. This estimate is quantitatively consistent with our main findings.