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CAMPAIGN CONNECTIONS

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ABSTRACT

This paper explores the labor market returns to working for a victorious political campaign. Using unique administrative data from Brazil, we track the earnings and employment of campaign workers before and after close elections spanning nearly 20 years. We identify sizable returns to working for a winning campaign, especially in areas with a large informal sector and for workers connected to newly elected challengers. The returns are concentrated in the public sector, where connected hires are relatively more qualified. Our results suggest that campaign connections facilitate the hiring of capable but inexperienced workers in the public sector through relational contracting.

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1   Introduction

In most democracies worldwide, electoral campaigns require significant labor and capital.[1] For many voters, contributing time or money to campaigns is the primary means of engaging in politics beyond the ballot box. While some of those contributors may be motivated by public interest, others may be driven by private benefit. Indeed, capital donors, both large and small, reap significant monetary returns to supporting successful candidates.[2] Despite the critical importance of labor in running effective campaigns (see Green and Gerber, 2019), little is known about the incentives driving these workers at the heart of the electoral process.

This paper provides the first empirical study of the returns to campaign labor. We connect several administrative data sources from Brazil that allow for tracking workers before, during, and after their time employed by mayoral campaigns. We find a large labor market return to working for campaigns that barely won close elections relative to those working for campaigns that barely lost. These returns materialize quickly through a combination of more hours worked and higher hourly wages, which together imply significantly higher annual earnings than for the counterfactual connection to a losing candidate four years after the election. The returns are larger for those with deeper connections to the campaign and those working for newly elected challengers rather than re-elected incumbents. Most of these post-election labor market returns come from public sector employment, which provides especially strong earnings opportunities for young, qualified workers in localities with a large informal sector. We explore several mechanisms underlying the returns to campaign connections and find evidence consistent with relational contracting between politicians and workers.

Brazil is a uniquely rich context for studying campaign connections. In most countries, it is not possible to link campaign labor and capital contributions to worker- or firm-level administrative records. Because of its transparent electoral and labor reporting laws, Brazil has made such links possible. We are the first to establish these links for campaign workers. Relative to campaign donors in Brazil (see Colonnelli et al., 2020), campaign workers tend to be younger, have less formal labor market experience, and rarely contribute any capital to campaigns (only 7% of campaign workers are also campaign donors). These patterns suggest possibly distinct motives and, in turn, consequences of building political connections through labor relative to capital.

The administrative data allow us to distinguish those providing contractual services to the campaign on a short-term basis from those working as dedicated salaried employees on the campaign. Payments to both workers range from one-quarter to one-third of total campaign expendi-

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[1]See, for example, Pons (2018) on the impacts of canvas workers on electoral outcomes, and Avis et al. (2022) and Broberg et al. (2022) on the impacts of campaign finance on candidates' performance.

[2]See, for example, Colonnelli et al. (2020) on Brazil and Dawood (2015) for a review of work on the United States with more recent work discussed in Bouton et al. (2021). Bombardini and Trebbi (2020) provide a broader review of the literature on lobbying, of which campaign contributions constitute an important part.

tures in our study period. Contractual staff provide one-off services, including canvassing support, media production and distribution, and management consulting, among others. Dedicated staff work most often as full-time office assistants, secretaries, receptionists, or administrative agents and are eligible for the full benefits of formal sector employment. In addition to the extensive margin of full-time versus part-time campaign labor, we also observe considerable variation in the amount paid to contractual staff.[3] Together, these provide useful measures of connection intensity and can be used to estimate heterogeneous effects of weak versus strong ties to the campaign.

We compare changes in labor market outcomes before versus after the campaign to those working for winning versus losing campaigns. Our empirical strategy combines difference-in-differences (DID) with a regression discontinuity (RD) design for 1,834 close mayoral elections at the municipality level. Although we find smooth observables around the narrow victory margin cutoff in the RD, the inclusion of worker fixed effects in the DID component helps address any residual concerns about time-invariant confounders affecting pre- or post-election dynamics. Our main sample comprises 379,008 contractual campaign staff reported to the Supreme Electoral Court in 2004, 2008, and 2012 whom we link to matched employer–employee records spanning 20 years. We supplement this sample with 3,333 dedicated staff reported in the matched employer–employee records who can be linked to mayoral candidates in the election years 1996–2012.

Brazilian workers see large and persistent returns to prior work with winning campaigns. Their formal sector employment prospects increase substantially in the year after the campaign and remain high for several years after the election. As a summary measure, four years after the election, those working for a winning campaign report 10% higher annual earnings or an additional USD 160 relative to counterfactual earnings among those who worked for narrowly-losing candidates. Much of these differential gains come from the extensive margin of entering and remaining in the formal sector where workers earn a wage premium (see Ulyssea, 2018).

Most of these formal sector returns come from greater access to public sector jobs. We find that workers on a winning campaign are 55% more likely to be public employees three years after the election compared to those who work for the runner up. This estimate is nearly two-thirds larger than the comparable returns to campaign donors identified by Colonnelli et al. (2020). It is also in line with aggregate estimates of electoral turnover in partisan representation within the municipal bureaucracy (Barbosa and Ferreira, 2023; Brollo et al., 2017).[4] We show that the returns are concentrated among workers connected to newly elected challengers; re-elected incumbents tend to retain campaign-connected personnel hired in the previous election when they were challengers. Moreover, the differential access to public sector employment increases in proximity to the campaign: those providing more valuable labor services to the campaign are

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[3]We observe contractual workers reporting zero or close-to-zero payment and find little difference in the prevalence of these presumedly volunteer staff across (narrowly) winning and losing campaigns. However, the administrative data do not generally cover wholly unpaid campaign staff.

[4]On the latter, see also Brassiolo et al. (2020) for related evidence in Ecuador, showing that local politicians who lose their own election nevertheless gain access to municipal jobs when their party's mayoral candidate wins.

significantly more likely to enter the public sector than those providing less valuable services. This holds when comparing dedicated to contractual workers in the same campaign and when comparing contractual staff with higher- versus lower-value contracts.

Our results suggest a distinct patronage mechanism for campaign labor compared to campaign donors. Campaign workers with requisite educational credentials are more likely to be hired into public sector jobs for which they are qualified. This pattern of selection is more pronounced at higher levels of the municipal bureaucracy, suggesting potential upside for the quality of local governance. This stands in contrast to the evidence of negative selection in Colonnelli et al. (2020), who show that campaign donors are more likely to gain access to public sector jobs for which they are not qualified. There are several explanations for this distinct result. First, campaign donors are, on average, eight years older than campaign labor who have comparatively less formal labor market experience in either the private or public sector. Second, victorious campaign staff are more likely to enter the public sector in areas where formal private sector opportunities are limited (proxied by the baseline informal share of private employment). This suggests that in underdeveloped economies, campaign connections may provide a unique opportunity to enter stable formal sector employment for those with requisite qualifications but limited experience.

Further results elucidate the potential mechanisms driving positive selection among former campaign workers. One possibility is that, unlike donors, campaign workers had the opportunity, during the campaign, to reveal some of their labor productivity and match quality to the future politician (or his/her appointees responsible for hiring municipal bureaucrats after the election). While this may have facilitated selection of former staff with higher ability, we find that even after conditioning on campaign pay, connected workers with higher ex ante ability remain more likely to obtain public jobs. This is inconsistent with a screening mechanism insomuch as campaign pay is correlated with observable performance on the campaign.

We find stronger evidence in favor of a second possibility that we refer to as relational contracting whereby candidates use the promise of future, high-paying public jobs to build an effective campaign amidst resource constraints. We show that those working for campaigns with less capital—measured by amount of donations received—are paid less while working on the campaign but are paid more upon hiring in the public sector after the election, compared to those working for capital-rich campaigns for whom the opposite patterns hold. We estimate these post-election wage premiums relative to the wages of all public sector workers in the same occupation, municipality, and year. Through this relational contracting channel, capital-poor candidates can build high-quality campaigns, workers gain better post-election jobs, and newly elected mayors gain more qualified staff for the duration of their time in office. We find less evidence consistent with post-election wage premiums reflecting efficiency wage or gift exchange mechanisms to motivate effort among newly hired municipal staff.

This appears to be the first study to examine the labor market trajectories of campaign

workers. Despite a large literature on the electoral returns to canvassing (see, e.g., Bergan et al., 2005; Green and Gerber, 2019; Hillygus and Shields, 2009; Issenberg, 2012; Pons, 2018), little is known about the private returns to those working for a political campaign. We provide causal evidence that workers see large and persistent labor market returns to working on a winning campaign. Like others in the political economy literature, we interpret those returns as reflecting the value of connections to an influential politician or to those affiliated with that politician and in newfound positions of authority after the election.

Our findings on campaign labor complement related results for those contributing capital to campaigns as elite donors (Colonnelli et al., 2020) or regular party affiliates (Barbosa and Ferreira, 2023; Brollo et al., 2017), who rarely participate as campaign workers.[5] As detailed above, we find important differences in the value of a labor relationship relative to a donor relationship with a winning politician. While small campaign donations are growing in importance globally (Bouton et al., 2021), not all citizens can afford to contribute capital, especially in developing countries. Our results suggest an alternative way to build valuable connections with local politicians by supplying one's labor. Together, our studies offer well-rounded evidence on the private returns to producing a politician.

One takeaway from our results is that not all forms of patronage have adverse implications for personnel quality in the public sector. Akhtari et al. (2022) find adverse effects on public education of bureaucratic turnover induced by mayoral transitions in Brazil. While patronage plays a key role in this process, our findings suggest that campaign labor connections do not result in the same sort of under-qualified public workforce as other types of political connections. Absent lucrative private sector opportunities, public jobs may be the best option for talented young workers, and newly established connections to local political leaders help open doors otherwise closed to those without strong networks. Our findings of positive selection, together with the existing evidence on the tradeoff between talent and prosocial behavior (Ashraf et al., 2020), suggest that talented workers who are motivated to work for political campaigns early in their careers may have generally stronger civic capital that might, in turn, translate into more prosocial behavior as public servants.

Finally, our study offers insight on a novel pathway into the formal sector. A large literature in development explores the challenges of building and sustaining a robust formal sector at early stages of development (see Ulyssea, 2018, 2020). Much of this work emphasizes the importance of regulatory barriers to formal sector deepening. At the same time, other recent studies emphasize obstacles faced by (young) workers when matching with formal sector firms (see, e.g., Bandiera et

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[5]Due to a 2022 change in data privacy laws in Brazil, it is no longer possible to obtain information on regular party affiliates unless one had access to the data prior to 2022. However, through correspondence with the authors of a recent paper using this erstwhile publicly available data (Colonnelli et al., 2023), we were able to learn that only 15% of contractual campaign staff in our study were also regular party affiliates prior to the election during which their campaign connection was established. This indicates that, much like campaign donors, regular party affiliates are also a distinct population from campaign workers.

al., 2021; Carranza et al., 2021). Some of this work highlights the value of building a marketable reputation as well as networking with potential referring employees at desirable firms. Our findings show that campaign connections offer this type of opportunity for qualified workers otherwise precluded from higher-paying jobs in the formal sector.

The paper proceeds as follows. Section 2 provides background on political campaigns and elections in Brazil. Section 3 describes our administrative data and characterizes the different types of contributors to campaigns. Section 4 develops our empirical strategy. Section 5 presents our core results on the returns to campaign connections as well as evidence on the underlying mechanisms. Section 6 concludes with a discussion of policy implications and future work.

2   Background: Political Labor

This section provides brief background on the features of Brazilian politics that are central to understanding campaign connections. We describe the electoral process, the types of campaign labor, and the scope for using campaign connections after the election.

2.1   Elections

Brazil is a federal republic with a presidential system organized in three levels: federal, state and municipal. Each level has an elected government, which includes an executive and a legislative branch. The head of the executive branch, which consists of the president at the federal level, the governor at the state level, and the mayor at the municipal level, is elected by a plurality rule.[6] These winner-take-all elections, held every four years, are amenable to regression-discontinuity methods for causal identification, unlike the legislative elections, which are based on more complicated proportional representation mechanisms. Given the large number of municipalities ($> 5,500$), mayoral races constitute the lion share of executive elections each cycle.

2.2   Campaign Labor

Around each quadrennial election, thousands of workers supply labor to political campaigns across Brazil. Whereas capital contributions to campaigns are capped at 10% of a worker's gross annual income, individual labor contributions are unconstrained (subject to standard labor regulation). Some workers supply volunteer labor to campaigns while many work for pay.

Campaigns hire two types of paid labor.[7] The first are dedicated campaign staff working full

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[6]In municipalities with less than 200,000 registered voters, the mayoral seat is assigned to the candidate with the most votes. In mayoral elections in municipalities with more than 200,000 voters, as well as in presidential and gubernatorial elections, the seat is assigned by majority in a two-round runoff system.

[7]We hired a research assistant to peruse online job advertisements in recent elections, and they found that campaign staff are employed, inter alia, as strategists, canvassing coordinators, web designers, video editors, and communications directors.

time on contracts that begin several months prior to the election and end upon formal cessation of the campaign. In our administrative data described below, the most common occupational categories are office assistants and related workers (22%), secretaries and business assistants (16%), administrative agents (9%), receptionists (9%), and administrative service workers (5%). The second, more pervasive type of campaign staff are short-term contractual providers. These arms-length contractors provide a variety of goods and services to the campaign, including canvassing as well as production and dissemination of outreach material. However, unlike dedicated staff, they are not eligible for formal sector employment benefits provided by the campaign.

Personnel expenses comprise a large share of campaign expenditures. According to electoral reports from the 2012 election, nearly 40% of mayoral campaigns had any paid personnel. Across all campaigns, personnel pay comprised 15% of total campaign expenditures. Among those hiring paid labor, nearly one-quarter of campaign expenditures went towards personnel pay. Other major expenses, summing across all campaigns, include print media (16% of total expenditures), vehicles and related transport expenses (16%), other third-party services (12%), broadcast media (11%), and other advertising (5%).

2.3   Post-Campaign Labor Market Activities

There are several ways in which campaign workers—both dedicated and contractor—can reap labor market benefits from their connections to winning candidates. First, they can draw on their campaign-based network of co-workers to seek out employment elsewhere after the election. Second, they can use their fresh work experience to market new skills acquired on the campaign. Third, they can leverage connections to a newly powerful politician that may prove useful. While staff in winning and losing campaigns both gain on-the-job experience, networks, and skills, the connection to a winning candidate may provide a larger and longer-lived stream of (option) value to the extent that patronage networks are important in the labor market. It is also possible that winning campaigns provide more effective on-the-job learning and more portable skills. However, as discussed in Section 5, such differential human capital acquisition should be limited to non-existent when comparing winning and losing campaigns within narrow victory margins.

Direct hiring by the public sector may be an especially important channel for realizing the benefits of campaign connections. While hiring in this sector is heavily regulated, executive-branch politicians can directly influence worker selection. Applicants to public sector jobs undertake a formal civil service examination, which is job-specific and consists of oral and written tests. There exist, however, three special categories that are exempt from these exams: commissioned posts, positions of trust, and temporary jobs. These exemptions create many opportunities for politician discretion in public sector hiring (see, for further background, Barbosa and Ferreira, 2023; Brollo et al., 2017; Colonnelli et al., 2020). Our data, described next, do not allow us to identify the precise public sector official or entity responsible for hiring campaign workers after

the election, which might help distinguish alternative pathways from the campaign to the public sector. However, the data do provide a unique opportunity to credibly identify the returns to working for victorious campaigns, and we can provide strong suggestive evidence on the underlying mechanisms.

3 Data

This section describes our main sources of data and provides summary descriptives on the different types of campaign labor. First, we collect voting returns from the *Superior Electoral Tribunal* (TSE), which also reports detailed campaign expenditures to contracted personnel for the 2004–12 elections. Second, we observe labor market histories of both dedicated and contractual workers from the *Relação Anual de Informações Sociais* database (RAIS).

3.1 Electoral

We obtain publicly available electoral data from the TSE, which oversees national and local elections in Brazil. TSE provides information on election results, as well as candidates' characteristics, including names, affiliation to parties and coalitions, and basic demographics. We focus our analysis on mayoral elections.

For the 2004, 2008 and 2012 mayoral elections, TSE provides detailed information on campaign expenditures. This dataset includes the unique individual tax identifier number (CPF) and the amount paid to every worker who was contracted during the campaign. In order to link this information to labor market outcomes, we drop a small number of contractual workers with missing CPF. We further exclude the 4.4% of campaign staff contracted by multiple candidates during the study period to avoid including workers as treated in one election and control in a subsequent one.[8] This results in a sample of 379,008 unique contractual campaign workers who (i) are either contracted by a mayoral candidate (57%) or by any party in the coalition of the candidate (43%), and (ii) can be linked to RAIS as described next.

3.2 Tracking Campaign Labor

RAIS is a linked employer–employee dataset that covers the universe of workers formally employed in Brazil (Ministério do Trabalho e Emprego, 2015).[9] For each establishment–worker pair, RAIS provides information on worker characteristics such as age, education, hours worked, occupation, and earnings (see Appendix B). We measure earnings using the annualized December wage, the most reliable wage measure in RAIS, deflated to August 1994 and expressed in US dollars. All

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[8]Campaign labor are less likely than campaign donors to contribute across multiple elections (nearly 17% of donors contribute to multiple elections in Colonnelli et al., 2020).

[9]The RAIS data to which we have access runs from 1986 to 2014.

results for earnings are similar when adjusting the annualization to account for actual months worked during the year. The occupation information allows us to construct an indicator for whether a worker is qualified for a given public sector job (based on educational level, as in Colonnelli et al., 2020).

Each firm in RAIS also provides detailed information on industry (*Classificação Nacional de Atividades Econômicas* or CNAE) and legal status (*Natureza Jurídica*). We use this to identify workers who are employed in the political organization industry (*Atividades de organizacoes politicas*), or who are employed at a firm with legal status of political party (*Partido Político*) or political campaign (*Candidato a Cargo Político Eletivo*).[10] This yields around 25,000 workers representing the universe of dedicated staff of political entities between 1994 and 2014 and for which industry or legal status are consistently reported in RAIS.

We link these dedicated staff to political entities through a matching procedure detailed in Appendix B. In brief, we identify political campaigns, organizations, and parties using the tax register of firms, collected by the Federal Revenue Service Agency (*Receita Federal do Brasil*). We then link dedicated political staff in RAIS, which only reports the firm identifier and not the politician name, to their corresponding political entity in the tax register. After screening on mayoral elections, we end up with 3,333 dedicated campaign workers across 656 electoral campaigns and 872 political organizations. The latter are tied to explicit candidates and hence characterized as campaign labor for the purposes of our analysis below. These sample restrictions allow us to link workers to electoral outcomes, and we show in Appendix Table B.1 that this restricted sample does not substantially differ from the general population of dedicated workers.

We link contractual workers from the TSE to RAIS using the unique tax identifier (CPF) reported in both sources. This results in a match of 379,008 contractual campaign staff with employment spells in RAIS from 1994–2014. These matches cover 57% of all contractual personnel reported to TSE during the 2004, 2008, and 2012 elections. This is higher than the overall share of formal jobs in the Brazilian population in this period ($\approx 50\%$) and comparable to the 66% of donors matched to RAIS in Colonnelli et al. (2020). In Section 4, we show that the number of unmatched workers is statistically indistinguishable between winning and losing campaigns in close elections and, therefore, unlikely to confound our interpretation of the returns to working for a victorious campaign. We treat those unmatched to RAIS as not being employed in the formal sector over the study period, and, in a robustness check, we impute zero (formal sector) employment and earnings for those unmatched workers.

Although we cannot formally distinguish volunteer work, several features of the data allow us to clarify the potential importance of this unobservable margin of campaign connections. While only 7 contractual workers in our sample report zero payment, many workers receive very small payments that are substantially lower than the minimum wage. For instance, the maximum

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[10]Prior to 2002, only the CNAE could be used to identify formal political entities. Afterwards, we could also draw on *Natureza Jurídica*. See Appendix B for details.

payment in the first decile of the distribution of payments in an election year is approximately 1.2% (22%) of the annual (monthly) minimum wage, suggesting that small payments may be a form of reimbursements for campaign expenses incurred by volunteers rather than proper labor income. In Section 4, we show that the number of campaign workers who receive small payments (i.e., first decile of the distribution) does not significantly vary between winning and losing campaigns in close elections, suggesting that the incidence of volunteer work is unlikely to confound our estimates or interpretation. Moreover, in Section 5.3, we show that the returns to campaign connections hold across the entire distribution of payments, suggesting that our effects are not solely driven by workers who receive a proper labor income from the campaign.

Using RAIS, we track the labor market dynamics for contractual and dedicated campaign staff over time and across employers. Since RAIS covers the universe of formal workers, we can classify workers not observed in RAIS in a given year as not being employed by the formal private sector or by the public sector (Colonnelli et al., 2020 do the same for donors). We therefore create a balanced individual×year panel treating working-age-eligible spells of non-observation in RAIS as zero employment and zero earnings.

3.3   Characterizing Campaign Labor

We use RAIS data to describe the major contributors to political campaigns. Table 1 highlights important differences between dedicated and contractual campaign labor based on labor market histories observed prior to their first spell with a political campaign.[11] Dedicated staff tend to be more female (62% compared to 48% of contractual staff), younger (32 versus 34), and more educated (11% post-graduate degree compared to 6% for contractual staff). Relative to dedicated staff, contractual staff tend to have more work experience in the formal sector (40% versus 29% of years prior to the election) and in the public sector (7% vs 5% of prior years).[12]

By comparison, campaign donors tend to be much more established members of the formal workforce. They are roughly 7 years older than campaign laborers at the year of the election and skew male (66%). More distinctively, donors tend to be more experienced and attached to the formal sector, both private and public. Prior to the given election year, the average campaign donor spent 52% of their working years employed in the formal private sector and 18% in the public sector. Donors report significantly higher earnings prior to the election, although this may be partly due to their longer work experience given their age.

Moreover, those contributing capital to campaigns are largely distinct from those contributing labor: 5% of dedicated staff and 7% of contractors ever contributed to political campaigns in our

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[11]There are very few workers who supply both types of campaign labor: 0.2% of dedicated staff also provide contractual labor while 0.1% of contractual labor also provide dedicated labor during the study period.

[12]Note that for dedicated staff, the campaign work is their main, largely full-time job during the election year whereas contractors tend to have additional employment at other firms. Indeed these other jobs, whenever formal, are what we observe in RAIS for most contractors in the election year.

data. In terms of demographics and prior labor market experience, these joint contributors look like the average of campaign labor and campaign donors. We retain these individuals in our main analysis below but find very similar results when restricting solely to campaign labor who are not also donors in the given election.

Together, these descriptive findings suggest different pathways for building connections to political campaigns. Older workers with greater experience in the formal private and public sectors tend to favor capital contributions. For younger, less experienced workers, and especially women, such connections are more readily forged through labor contributions. The sizable ex ante difference in prior labor market experience will be important in understanding the heterogenous returns to labor and capital contributions that we uncover in the analysis below.

Before proceeding to our main empirical analysis, we make two important remarks about external validity. First, the campaign labor force is more educated, more female, and younger than the typical Brazilian working in the formal sector (compare columns 1–2 with column 4 in Table 1). Second, the elections in which candidates employ paid campaign labor differ along many dimensions seen in Appendix Table A.1. These elections take place in larger, more urban, more developed municipalities with a larger number of candidates. Given these differences, we caution against over-generalizing. Nevertheless, our focus is on the returns to campaign connections, and for those elections with such connections being built, our identification strategy, developed below, delivers an internally valid, causal parameter. That parameter does not apply to the average Brazilian worker, but it is informative for those who select into political labor markets.

4 Empirical Strategy

Our core empirical strategy combines difference-in-difference (DID) with regression discontinuity (RD) to rule out confounding variation in the types of workers and experiences at winning versus losing campaigns. We begin by developing the DID component:

$$y_{ipmt\ell} = \alpha_i + \lambda_t + \lambda_{m\ell} + \sum_{\ell} \beta_\ell^{DID} win_{pm} \times \lambda_\ell + v_{impt\ell}, \tag{1}$$

where $y_{impt\ell}$ indicates the labor market outcome of individual i who is connected to politician p running for mayor in municipality m in year t, measured ℓ years before or after the election held in $\ell = 0$. The term win_{pm} takes value 1 if candidate p wins the given election in municipality m and zero otherwise. We include individual (α_i), calendar-year (λ_t) and municipality times period (i.e., year-relative-to-election) fixed effects (FE) ($\lambda_{m\ell}$). Our main analysis sample comprises all individuals who worked for one campaign from 1996 to 2012 and could be linked to RAIS in

at least one year.[13] We impute zeros in y for formal sector employment and earnings in those years t when the given worker is not observed in RAIS. In robustness checks, we show that results are nearly identical when including those campaign workers never matched to RAIS and similarly imputing zeros (see Appendix Figure A.15).[14] In the baseline, to account for correlated unobservables among workers in the same labor market and over time, we cluster standard errors at the municipality level. We also show robustness to two-way clustering on municipality and worker to account for correlated unobservables among workers who move across municipalities.

The coefficient of interest, β_ℓ^{DID}, captures the average difference in outcome y between workers connected to winning versus losing candidates in period ℓ. We normalize $\beta_{-1}^{DID} = 0$. If political connections are valuable to campaign labor, then we expect $\beta_\ell^{DID} > 0$ for periods $\ell > 0$ after the election. In contrast, we expect null effects in the post-election period if those connections are not valuable, or, put differently, if the returns to campaign labor are due solely to generalized skill acquisition and peer networks that are similar across winners and losers.

Causal identification rests on a parallel trends assumption: workers employed at winning versus losing campaigns would have followed similar labor market trajectories in the absence of the election and their different political connections. In the results below, we present supportive evidence by appealing to a lack of differential pre-trends. We further solidify this assumption by narrowing in on campaign workers employed during close elections where one expects the trends, pre- and post-election, to have been even more similar.

Our core, RD-augmented DID specification takes the following form:

$$y_{ipmt\ell} = \alpha_i + \lambda_t + \lambda_{m\ell} + \sum_\ell \beta_\ell^{DDRD}(win_{pm} \times \lambda_\ell) + \sum_\ell \theta_\ell(margin_{pm} \times \lambda_\ell) + u_{impt\ell}, \quad (2)$$

where the additional term, $margin_{pm}$, captures the margin of victory of candidate p relative to the runner-up in the municipality. In this combined DID+RD (DDRD) specification (2), we restrict the analysis to workers linked to winning and runner-up candidates in elections within a given victory margin that we vary for robustness. The coefficient of interest, β_ℓ^{DDRD}, measures the average difference in y between workers connected to barely winning versus barely losing candidates in year ℓ relative to the reference year just prior to the election. For consistency with the specification used in the literature on donors, in our baseline specification we do not interact the win indicator with $margin$, but rather we allow $margin$ to vary flexibly over time by interacting with period FE. We further show robustness to alternative functional forms, including

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[13]Note that for the 1996 and 2000 elections is restricted to dedicated staff as we cannot identify contractual staff until the 2004 election. Further note that because our RAIS data panel only extends through 2014, the post-period for the 2012 election is restricted to two years. Results are robust to omitting this election year from the analysis (see Section 5.1).

[14]This is consistent with the worker FE, α_i, effectively excluding such always-informal workers from the identifying variation underlying the DID component of our empirical strategy. As discussed below, we also report RD estimates without a DID component, and in those specifications, the omission of the always-informal workers could induce bias, to the extent that they are overrepresented among barely-winning or barely-losing campaigns.

the interaction of *win* and *margin* or the triple interaction of municipality and election year with period FE. In these specifications, we obtain the same qualitative results and very similar magnitudes, suggesting that the effects are not sensitive to the different identifying assumptions.

In addition to parallel trends, a causal interpretation of β^{DDRD} requires continuous potential outcomes across the victory margin cutoff. Appendix Figures A.1–A.7 provide supportive evidence of smoothness around the cutoff for (A.1/A.2) campaign worker characteristics related to demographics, prior labor market experience, and pay while on the campaign, (A.3) candidate characteristics including education, gender, and ideology, (A.4) campaign-level measures of total workers employed, total contractual workers unmatched to RAIS, total donations, and total expenditures, (A.5) the density of workers, (A.6) the number of workers who receive zero or close-to-zero payments from the campaign used as a proxy of the importance of volunteer work, and (A.7) municipality characteristics including proxies for development and the size of the informal sector.[15] Together, these figures help rule out the possibility that more capable candidates, who are more likely to win, hire more and better staff and run more effective campaigns, which in turn shapes the counterfactual post-election labor market outcomes for those connected to winning and losing campaigns. In other words, this combination of DID and RD addresses endogenous sorting of higher-ability workers who simultaneously increase the likelihood of a victorious campaign and change the mix of peers and on-the-job experiences from which campaign labor might gain skills of value in subsequent labor market activities.[16]

While our main strategy combines DID and RD, we also consider more simply estimating β_ℓ in a pooled RD specification without individual FE. This is the approach in Colonnelli et al. (2020) for campaign donors, and, reassuringly, delivers similar results. One advantage of the combined DID and RD approach is that it rules out any time-invariant unobservable confounders that would lead individual i to work for more effective candidates and realize more favorable post-election labor market outcomes. Of course, within a close-election design, such confounders should be quite limited. And indeed, the RD and DID+RD results look very similar subject to minor nuances discussed below.

Connection Intensity. In our core analysis we consider contractual and dedicated workers as one group. However, we are also interested in comparing the effects of campaign connections for those with deeper involvement in the campaign. One way to do this is to allow β_ℓ to vary across

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[15]See Appendix Table A.7, and the associated discussion thereof, for additional evidence of smoothness in the number of workers around the cutoff.

[16]Because our panel spans several elections, the standard two-way fixed effects (TWFE) estimator may be biased due to time-varying effect heterogeneity. For example, the returns to working for a campaign during the global financial crisis of 2008 (the elections were held in October) may have differed from prior and subsequent elections during which prevailing labor market conditions were stronger. This would introduce bias in the underlying DID comparisons across workers just employed at the campaign in 2008 and those already employed in prior elections (or yet to be employed in future elections). We address this potential bias using the De Chaisemartin and d'Haultfoeuille (2020) DID_M estimator and find very similar results.

contractual workers with higher- versus lower-value contracts, as a proxy for connection intensity. Another is to compare contractual and dedicated staff working for the same campaign. We therefore estimate a triple-difference (DDD) extension of equation (1) that allows β_ℓ^{DID} to vary across dedicated and contractual labor within campaigns. The relatively few campaigns with dedicated staff makes it impractical to combine this triple-difference approach with a narrow-victory RD design. There are simply too few municipalities with competitive elections where winning and losing campaigns have both dedicated and contractual labor. Nevertheless, the DDD effect provides a suggestive test for whether campaign connection intensity matters.

5 Campaign Connections and Labor Market Outcomes

This section reports our main findings. First, we show that connections to victorious campaigns yield sizable and persistent labor market returns. Second, we find that the returns to campaign connections are driven by connections to challenger candidates and by post-election employment in the public sector, especially in areas where the formal private sector is limited. We show that connections to a winning candidate facilitate positive selection into the public sector by allowing more qualified workers to gain access to government jobs. Third, we identify an intensive margin of connections, finding relatively larger returns to more proximate campaign connections. Finally, we provide suggestive evidence that relational contracting rather than screening on productivity help explain the patterns of selection into the public sector.

5.1 Returns to Working for a Victorious Campaign

Before reporting formal econometric estimates, we provide descriptive visual evidence of the returns to victorious campaign connections. Figure 1 reports (a) annual earnings, (b) weekly hours, (c) hourly wage, (d) formal employment, and (e) public employment for workers connected to the winning and losing candidate in mayoral elections determined by less than a 5% victory margin. In the four years leading up to the election, those who worked for the victorious candidate have slightly higher earnings and wages, but the two groups of workers are nevertheless on very similar if not identical labor market trends across all five outcomes. After the election, we see a stark break in trajectories as those who worked for the victor see sharper labor market gains, with the most pronounced difference being an immediate jump in employment within the public sector. At the same time, we also see that those who worked for the runner-up experience an upward break in their labor market trends relative to the pre-election period. This hints at some of the generalized benefits of working for a political campaign and stands in contrast to the marked downturn in public employment prospects for donors to runner-up campaigns (see Figure 1, panel D in Colonnelli et al., 2020). The remainder of this section and the following one corroborate a causal interpretation of this striking visual evidence.

Figure 2 presents causal estimates for (a) annual earnings, (b) weekly hours, (c) hourly wage, and (d) and formal employment, based on equations (1) and (2). We report β_ℓ for four years around each election, though all available worker–years are included in the estimation.[17] For each outcome, we report DID, RD, and the combined DID+RD estimates. Following Colonnelli et al. (2020), we use a consistent bandwidth of a 5% victory margin in the baseline RD and DID+RD; we consider other bandwidths, including the optimal Calonico et al. (2015) one, for robustness.

Figure 2 suggests sizable pecuniary returns to campaign connections, comparing those working for narrowly victorious candidates to those working for the runner-up. Consider the year-four coefficient in the DID+RD estimates for annual earnings (panel a, column iii). Relative to the year prior to the election, those working for the winner saw their earnings increase by nearly USD 160 more than those working for the runner-up. This represents 13% of worker's mean earnings of USD 1,223 in the year prior to the election and 10% of the counterfactual mean earnings of USD 1,660 four years after the election (i.e., for those on losing campaigns).

Campaign connections increase annual earnings through greater hours worked per week (panel b) and higher hourly wages (panel c). Over the four-year-post-election horizon, weekly hours worked increase by 1.3 hours, or 8% of the mean 16 weekly hours worked in the year before the election and 6% of counterfactual post-election hours for those on losing campaigns.[18] Meanwhile, wages increase by roughly USD 0.08 per hour, or 13% of mean hourly wages in the year before the election and 9% of the counterfactual post-election mean hourly wage of USD 0.81.

Importantly, some of these labor market gains to connected workers come from their increased attachment to the formal sector. Four years after the campaign, those who worked for winning candidates are 3.3 p.p. more likely to be employed in the formal sector (panel d). This represents a sizable increase in formality given that nearly 40% of campaign workers were not employed in the formal sector in the year prior to the election; it represents 6% of the counterfactual formality rates four years after the election. These effect sizes suggest that a non-trivial share of the earnings gains in panel (a) may come from those using the campaign connections as a pathway out of informality and into the formal sector where they can fetch a significant wage premium.[19]

The sequence of specifications (i)–(iii) in Figure 2 suggest advantages of combining the DID and RD approaches. While the DID estimates in (i) generally point to a lack of pre-trends, they are based on municipalities with different electoral environments and corresponding political labor markets. One concern is that the types of workers who join campaigns in ex ante obviously uncompetitive elections are inherently different from those in extremely competitive ones. The RD helps address these concerns. However, the standard RD estimates in (ii) suggest a slight *levels*

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[17]Appendix Figure A.8 reports an analogous version of the main results in Figure 2 restricting the regression to include only those four years around the election. The results look nearly identical.

[18]Recall that the outcomes in Figure 2 include zeros imputed for those not observed in RAIS in the given period, which helps explain why the mean weekly hours fall well below full employment.

[19]Using the Brazil National Household Survey (PNAD) data from 2002–2014, we estimate a 25% wage premium in the formal sector based on a standard Mincerian specification with an additional term for formal employment.

imbalance in the pre-election period where those working for winning campaigns have weaker formal sector labor market outcomes.[20] Combining DID and RD in (iii) effectively leverages the best of both designs. By including worker FE, the RD imbalance in the pre-period becomes less problematic, and narrowing in on close elections reduces the influence of selection into campaigns on the basis of expected labor market returns to candidate victory.

In Table 2, we report summary estimates of the post-election DID+RD coefficients and robustness checks on the baseline specification. Column 1 reproduces the baseline. Column 2 further controls for factors specific to location and election year by replacing municipality times period FE ($\lambda_{m\ell}$) with municipality times election year e times period FE ($\lambda_{me\ell}$). Column 3 controls for separate linear functions of margin of victory on the two sides of the cutoff. Column 4 controls for separate quadratic functions of the margin of victory on the two sides of the cutoff, and column 5 controls for separate cubic functions. Across these alternative specifications, we find similar qualitative and quantitative results. Table 3 reports similar robustness for the RD specification without individual FE, which corresponds to the event-study results in panel (ii) of Figure 2.

Further results in the Appendix support a causal interpretation of the DID+RD estimates. First, Appendix Figures A.9-A.10 show similar results when using the De Chaisemartin and d'Haultfoeuille (2020) DID_M estimator to address potential heterogeneous effects for those in early versus later elections over the sample period.[21] Second, Appendix Figures A.11–A.13 show robustness to alternative victory margin bandwidths in the RD component. Third, Appendix Figure A.14 shows robustness to two-way clustering on municipality and worker. Fourth, Appendix Figure A.15 and Appendix Table A.2 show robustness to including contractual workers who could not be matched to RAIS in any year of the sample; we impute zero employment and earnings to these unmatched workers, finding qualitatively similar results.[22] Appendix Figure A.16 shows robustness to excluding the small share of campaign workers who are also campaign donors in the given election. Finally, Appendix Figure A.17 and Table A.3 show similar results when excluding analysis of workers in the 2012 election round who are subject to censoring in the post-election period due to our RAIS data series ending in 2014.

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[20]Note that this imbalance is opposite from the imbalance in the raw data plots in Figure 1 where workers for victorious campaigns had slightly higher pre-election earnings and hourly wages (albeit similar trends) compared to workers for losing campaigns. This discrepancy with the RD highlights the importance of looking within municipality–election–periods to facilitate compelling counterfactuals.

[21]In Appendix Figure A.9, the estimated effects on earnings and hourly wages at time -3 are insignificant but noisy. In order to reduce the variability linked to outliers in earnings, in Appendix Figure A.10 we use alternative functions of earnings and hourly wages as the dependent variable (i.e., the log of 0.001 plus and the inverse hyperbolic sine functions). In these specifications, the effects at time -3, while being more precisely estimated, remain insignificant. The pattern in other time periods is similar in the two figures.

[22]The number of observations in the baseline specification of column 1 in Table 2 is approximately 55% of the number of observations in the same specification of Appendix Table A.2. In line with the difference in the number of observations, the magnitude of the coefficients in the specification that includes unmatched workers as zeroes ranges between 60% and 67% of the baseline effects, depending on the outcome variable of interest.

5.2 Public Sector Employment: Entry, Selection, and Turnover

The results thus far reveal sizable labor market returns to working for a victorious political campaign. We show here that public sector jobs were a major driver of such returns. These jobs were especially valuable in areas with a weaker formal private sector. What is most surprising about these patronage-based opportunities is that they allowed more qualified workers to gain access to stable, high-paying public employment. This sort of positive selection stands in contrast to other findings in the political connections literature, as we discuss below. Finally, we show that the access to public sector jobs is driven by campaign staff for victorious challengers while re-elected incumbents are more likely to retain pre-existing staff.

Entry. Figure 3 shows that workers for victorious mayoral campaigns are significantly more likely to be employed in the public sector after the election. These DID, RD, and DID+RD estimates reveal a similar pattern as the descriptive raw data trends in panel (e) of Figure 1. The DID+RD estimates in (iii) imply that, four years after the election, connections to a winning candidate increase the likelihood of public employment by 5 p.p. This represents 62.5% of mean public employment rates in the year before the election and nearly 55% of the counterfactual post-election rate of public employment among those who worked for the runner up. These results are robust to the full battery of abovementioned checks in panel (e) of the foregoing Table 2, Appendix Figures A.9, A.11-A.16 and Appendix Table A.2.

These estimates suggest that public sector patronage is important for campaign labor just as it is for campaign donors. Colonnelli et al. (2020) find that, three years after the election, donors connected to a winning politician are roughly 6 p.p. more likely to be public employees. This represents nearly one-third of the counterfactual mean of 20% public employment rates for those who donated to the runner up. The effect sizes for campaign labor are slightly larger given their lower baseline rates of public sector employment. And much like campaign donors, connected campaign laborers gain access to a wide array of positions across the public sector hierarchy, including managerial and professional positions, technical and supervisory jobs, as well as other white and blue collar service roles (see Appendix Table A.4).

The large size of the informal sector in Brazil may help explain why campaign connections built through labor are so valuable in generating public sector employment. In many areas of Brazil, there are limited opportunities to work in the formal sector outside of government jobs. In areas with a large informal sector, it is plausible that public jobs provide the highest-return opportunity among the set of options now open to those with political connections.

We explore this potential source of heterogeneity in Figure 4 by adding to equation (2) another difference capturing high- versus low-informality in the municipality, as proxied by the share of total employment in the informal sector in the 1991 Census. The results can be interpreted as the differential effects of campaign connections on public employment in municipalities with above-

versus below-median informality (panel a) or top- versus bottom-quartile informality (panel b).

Among labor connected to winning campaigns, those living in municipalities with a larger informal sector are significantly more likely to gain entry to public employment through those connections. Four years after the election, those in municipalities above the median informality are nearly 3 p.p. more likely to work in the public sector than those in municipalities below the median (panel a). The corresponding differential is even larger, at around 7 p.p., for those in the top 25% compared to those in the bottom 25% of informality (panel b).

Selection. These results raise important questions about the nature of patronage jobs afforded to campaign contributors. Colonnelli et al. (2020) find that supporters of victorious mayoral campaigns are negatively selected into the public sector: those with lower pre-election earnings in the private sector and weaker educational qualifications are more likely to be hired in the public sector after the election, including in jobs for which they are under-qualified. We find a distinct pattern consistent with positive selection: victorious campaign workers with greater private sector experience and strong educational qualifications are more likely to be hired in public sector jobs for which they are well-qualified, especially in managerial and supervisory occupation for which this pattern of positive selection is particularly strong.

Table 4 shows that campaign labor with higher earnings before the election were differentially more likely to enter public employment after the election. These estimates effectively allow β^{DDRD} from Figure 3 (iii) to vary across workers with different private sector labor market experience prior to the election. We distinguish those with no formal private sector experience prior to the election from those in the bottom, middle and top third of the distribution of formal private sector earnings in the 4 years (column 1) or 2 years (column 2) preceding the election. Thus, we treat prior earnings as a measure of a worker's skills (see Dal Bó et al., 2013). The estimates are similar across specifications. Column 1 suggests that those in the top-third (middle-third) are 2 p.p. (1.6 p.p.) more likely to enter the public sector compared to those in the bottom-third or without any formal private sector experience. Those latter two groups of workers also saw a significant 5.6 p.p. increase in public employment. This pattern is consistent with higher-ability workers—as revealed by prior formal sector experience and earnings—being more likely to use their novel connections to gain entry into the public sector.

Table 5 goes a step further in assessing the nature of selection into the public sector by campaign workers. Here, we determine whether workers are qualified for a given public sector job following the approach in Colonnelli et al. (2020) of determining whether a worker has the requisite education for the position in which they are observed in RAIS. We split up this analysis into positions requiring at least middle school (column 1), high school (column 2), and university degrees (column 3). The dependent variable in each equals one if the individual has the requisite education level for the public job in which they are employed and zero otherwise.

Campaign connections to a winning mayoral candidate enable more qualified workers to gain

access to public jobs requiring high school and higher levels of education. In column 1, we see little entry or differential selection into public sector jobs requiring middle school education or higher. Among workers connected to a victorious mayoral candidate, those with at least high school education (column 2) or at least a university degree (column 3) are significantly more likely to gain access to public sector jobs requiring such credentials.

Together, the evidence on positive selection into public sector employment among campaign workers suggests that not all political connections give rise to adverse forms of patronage. This may have important implications for the quality of local governance, especially if we consider that the patterns of positive selection appear to be strongest in managerial and supervisory occupations, which operate at higher levels of the municipal bureaucracy (see Table 6).[23] Our findings suggest that campaign workers who capitalize on their newly established connections tend to be of higher ability and with qualifications appropriate to the public jobs in which they are hired. This stands in contrast to campaign donors who leverage their connections to newly elected mayors to gain access to public sector jobs, and to candidates running for local councils on the same party ticket as mayoral candidates who use those connections to access jobs for which they are actually under-qualified.

There are at least two explanations for this difference across the distinct groups of campaign contributors. First, campaign workers tend to be younger than campaign donors (see Table 1), and hence, on average, may have yet to realize their labor market potential. With a new political connection, they are able to gain entry into higher-paying public sector jobs that would have otherwise been difficult to access despite being qualified. Second, campaigns may also facilitate relational contracts by which capital-constrained candidates can use the promise of future high-pay jobs in the public sector as a way to retain qualified campaign staff. We explore this hypothesis in Section 5.4.

Incumbents, Challengers, and Turnover. Having documented the differential access to public sector jobs for workers connected to victorious campaigns, we now report systematic heterogeneity across those connected to challengers versus incumbents. Such access materializes first and foremost for those connected to newly elected mayors whereas re-elected incumbents are more likely to retain staff hired in the previous election when they were the victorious challenger.

Figure 5 explores the effects of connections to victorious incumbents versus victorious challengers. We re-estimate the specification in Figure 3(iii) restricting the sample, separately, to workers connected to challengers (panel a) and workers connected to incumbents (panel b). The findings are stark and intuitive. Campaign workers connected to newly elected challengers are significantly more likely to enter the public sector post-election compared to workers connected to challengers who lost. By contrast, staff working for incumbent mayoral campaigns (i.e., the

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[23]In Appendix Table A.5, we find qualitatively similar evidence of positive selection in high-ranked public sector jobs when we measure workers' skills based on pre-election private sector earnings.

second time the candidate is running) see more limited changes in their likelihood of working in the public sector after the election.

Note that because each election includes at most one incumbent, it is not possible to identify the differential returns to winning and losing campaign connections within a given election cycle, as we do in our baseline analysis in Figure 3. Rather, we compare across municipalities within the same election cycle but restricting to municipalities within the same broad local labor market, known as microregions in Brazil (akin to commuting zones in the United States). The coefficients in Figure 5 thus identify the differential public sector employment for those connected to challenger candidates who just barely won their election compared to those connected to challenger candidates who just barely lost their election in nearby municipalities. Because some elections feature two challengers (due to incumbents not running), it is possible to identify the returns to a victorious challenger connection using the same baseline specification looking across winners and losers within the same municipality electoral cycle. Appendix Figure A.18 shows that these are similar to the cross-municipality, within-microregion comparisons in Figure 5 (panel a).

Together, these results are consistent with greater scope, mandate, and incentives for new politicians to adjust government personnel. First, newly elected mayors fire and hire a large number of workers in the municipal government, many of whom may had been hired by the outgoing incumbent (see Akhtari et al., 2022, on bureaucratic turnover). Second, in complementary results in Appendix Figure A.19, we find that those working for a mayoral campaign in which the candidate wins again in the following election are significantly more likely to remain employed in the public sector after that second election, compared to those workers connected to a candidate who loses, as an incumbent, in their second election. There are many reasons why incumbent mayors may prefer to retain their existing municipal government personnel hired through discretionary means. The foregoing evidence on positive selection suggests that some of these workers' staying power may come from their strong qualifications and correspondingly effective work on the job (see Akhtari et al., 2022, for direct evidence on the adverse costs of bureaucratic turnover). Regardless of the specific mechanism, the pattern in Appendix Figure A.19 is consistent with patronage being central to workers' abilities to capitalize on their campaign connections.

5.3 Connection Intensity Matters

Our findings above suggest that experience working for a winning political campaign significantly increases access to public sector jobs after the election. This section shows that these new opportunities are more accessible to those with deeper connections to the campaign.

First, we estimate heterogeneous returns across contractual workers that vary in the amount they were paid by the candidate. Such variation reflects, in part, the intensity of the relationship to the campaign, both in terms of hours and quality of work.[24] Following a similar heterogeneity

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[24]In the TSE data, some campaigns report very detailed descriptions of the type of payment to contractual

analysis for donors in Colonnelli et al. (2020), we split up the distribution of payments into quintiles and allow β_ℓ^{DDRD} in equation (2) to vary across these quintiles where those in the bottom 20% earn less than USD 83 compared to those in the top 20% who earn more than USD 430. Figure 6 provides the mean estimates of β across post-election years $\ell = 1, \ldots, 4$.

We see that contractual laborers with greater financial remuneration from the campaign experience a larger post-election boost in their likelihood of public employment. The gradient is roughly linear across the campaign-pay distribution. Among staff working for victorious candidates, those in the top quintile of campaign pay see an 11 p.p. increase in their likelihood of public sector jobs compared to roughly 4 p.p. for those in the bottom 20%. In Appendix Figure A.20, we find similar effects when augmenting the specification in Figure 6 to include worker characteristics, such as education or age, that may correlate with the financial remuneration from the campaign.

A second way to assess heterogeneous returns across connection intensity is to compare dedicated and contractual staff within the same campaign. The DDD estimates in Figure 7 suggest that among those working for winning campaigns, dedicated staff gain greater entry into the public sector after the election. One interpretation is that contractual staff have weaker connections to the political candidate. Many contractual staff engage in short-term canvassing and outreach whereas dedicated staff tend to work much more closely with the campaign "front office". Dedicated staff are more likely to work closely with the candidate and campaign leadership, which might imply greater value to the campaign connection after the election.

5.4 Campaign Connections: Screening versus Relational Contracting

This final section offers a unifying interpretation of the key mechanisms shaping the labor market dynamics of campaign workers that we identified above. There are two related but potentially distinct underlying mechanisms. First, campaigns may act as screening devices to overcome imperfect information on workers' ability (Stiglitz, 1975): working on the campaign may allow politicians to assess a worker's ability and to subsequently select those workers who are best suited to take a public sector position. The second is relational contracting (e.g., Levin, 2003; MacLeod and Malcomson, 1998): working on the campaign may give rise to an implicit contract by which high-ability workers provide their time to the campaign, possibly foregoing higher pay outside the campaign during the election period, in exchange for a secure and well-paid public sector job after the election. We find stronger suggestive evidence for relational contracting.

In order to probe the productivity screening mechanism, Appendix Table A.6 re-estimates Table 4 adding flexible controls for earnings received while on the campaign. To the extent that the campaign allows politicians to uncover workers' ability, we would expect differences in earnings

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personnel, including, for example, mentions of the number of days worked for individual canvassers. Those working for more days are naturally paid more and also plausibly develop stronger ties to the campaign leadership.

on the campaign to capture relevant differences in productivity across workers. Furthermore, if the positive selection of workers in the public sector is driven by productivity differentials that are uncovered during the campaign, we would expect that such selection patterns weaken after conditioning on earnings in the campaign period. Appendix Table A.6 shows that this is not the case: positive selection (proxied by pre-election private sector earnings) remains strong even when comparing workers with similar campaign-period earnings.

Campaigns may also facilitate relational contracts with benefits to workers, candidates, and newly elected mayors. In particular, capital-constrained candidates may use the promise of future high-pay jobs in the public sector as a way to grow the size and quality of their campaign workforce. The upside for newly elected mayors is that they would have a workforce from which they can draw capable workers to staff their new bureaucracy and, ideally, deliver higher-quality governance that might increase their odds of re-election. This mechanism helps explain the differential selection of higher-ability workers as proxied by prior earnings in the private sector (Table 4). It also helps explain the stronger effects for challengers than for incumbents insomuch as challengers have longer horizons over which to capitalize on the relational contract.

Several additional results shed further light on a relational contracting mechanism. First, connected hires into the public sector earn nearly USD 300 more annually than unconnected hires (Appendix Figure A.21). Second, we find evidence consistent with connected hires earning a wage premium relative to unconnected hires, where we benchmark that premium against other public sector employees working in the same municipality, occupation, and year. Figure 8 shows that this wage ratio—of own to comparator public employees—is roughly 15 p.p. higher for connected hires than for those who worked for campaigns that narrowly lost.[25] This is a sizable differential given that unconnected hires earn, on average, 65% as much as the comparator public employees in the post-election period. Moreover, this premium is relatively larger for connected hires who worked for capital-poor candidates, measured by total monetary donations to the campaign. Compared to connected hires from the richest-50% of campaigns, those from the bottom-50% earn 8.3 p.p. higher relative wages (panel a, Figure 9).[26] Finally, we find, descriptively, that richer campaigns pay their staff relatively more while working on the campaign (panel b, Figure 9).

The post-election wage premium for campaign workers is consistent with multiple theories of the labor market. First, the relative premium may point to an efficiency wage mechanism for retaining productive municipal employees newly hired by challengers facing re-election prospects in four years (see Katz, 1986, for a review of the efficiency wage literature). Alternatively, it may reflect a reciprocity mechanism by which the premium is given as a gift to incentivize greater effort among the new civil servants (e.g., DellaVigna et al., 2022; Gneezy and List, 2006), which in turn

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[25]While workers who enter the public sector after the election may be a selected sample, the premium remains statistically and economically significant when we expand the analysis to all workers who find employment in the formal sector after the election (see Appendix Figure A.22.)

[26]Recall from Appendix Figure A.4 that total donations and campaign expenditures are continuous across the victory margin cutoff.

helps the new mayor to build a stronger reputation of commitment to work. However, the fact that certain (capital-constrained) campaigns rely more heavily than others on wage premiums suggests that these theories are unlikely to fully explain our findings.

Capital-constrained candidates paying lower wages during the campaign and offering higher wage premiums after the election may instead be consistent with an implicit relational contract (e.g., MacLeod and Malcomson, 1998). Under this view, resource-constrained politicians motivate and retain workers who earn lower wages during the campaign through the promise and delivery of future wage premiums and secure public sector jobs. Together, the results and discussion in this section offer a novel lens on the nature of patronage in an electoral system that employs hundreds of thousands of workers every cycle.

6 Conclusion

Why do individuals work on political campaigns? This paper provides an empirical answer to this question. Using uniquely rich administrative data from Brazil, we identify significant private income gains up to four years after working for a victorious mayoral candidate. These returns are fueled by new opportunities in the public sector after the election, especially among those with stronger ties to the candidate and in localities where formal private sector opportunities are more limited. While one often associates this type of patronage with anti-meritocratic forces, we find that campaign connections help young, inexperienced workers gain access to public sector jobs for which they are qualified. In democratic systems, electoral campaigns provide an opportunity for future political leaders to attract and retain high-quality workers in the public sector.

Our findings suggest that political connections need not give rise to unproductive rent-seeking. Putting our results together with Colonnelli et al. (2020) on donor connections, it is natural to ask whether, on net, capital and labor contributions to campaigns foster local government workforces of varying capacity and integrity. This is an important question for future research.
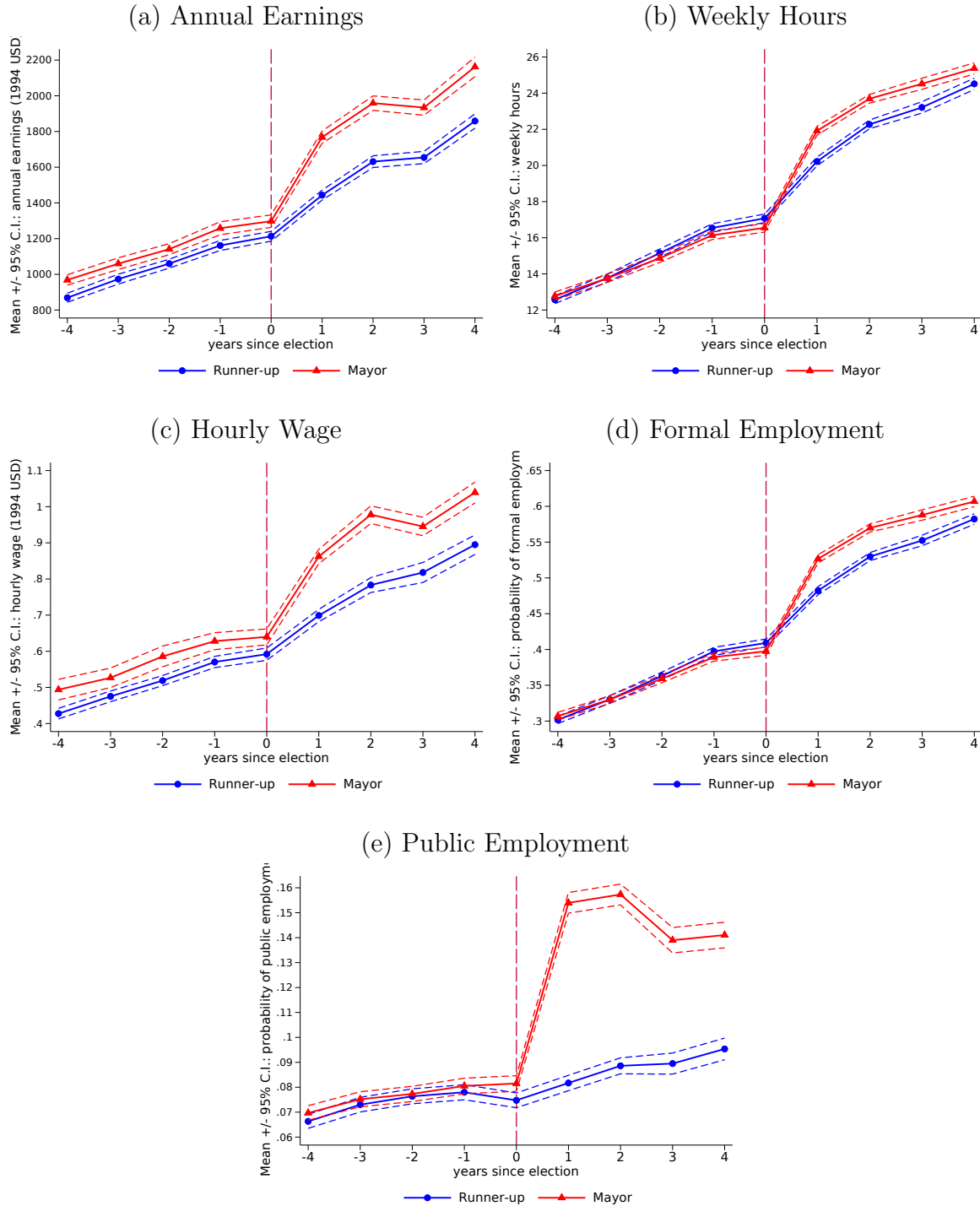
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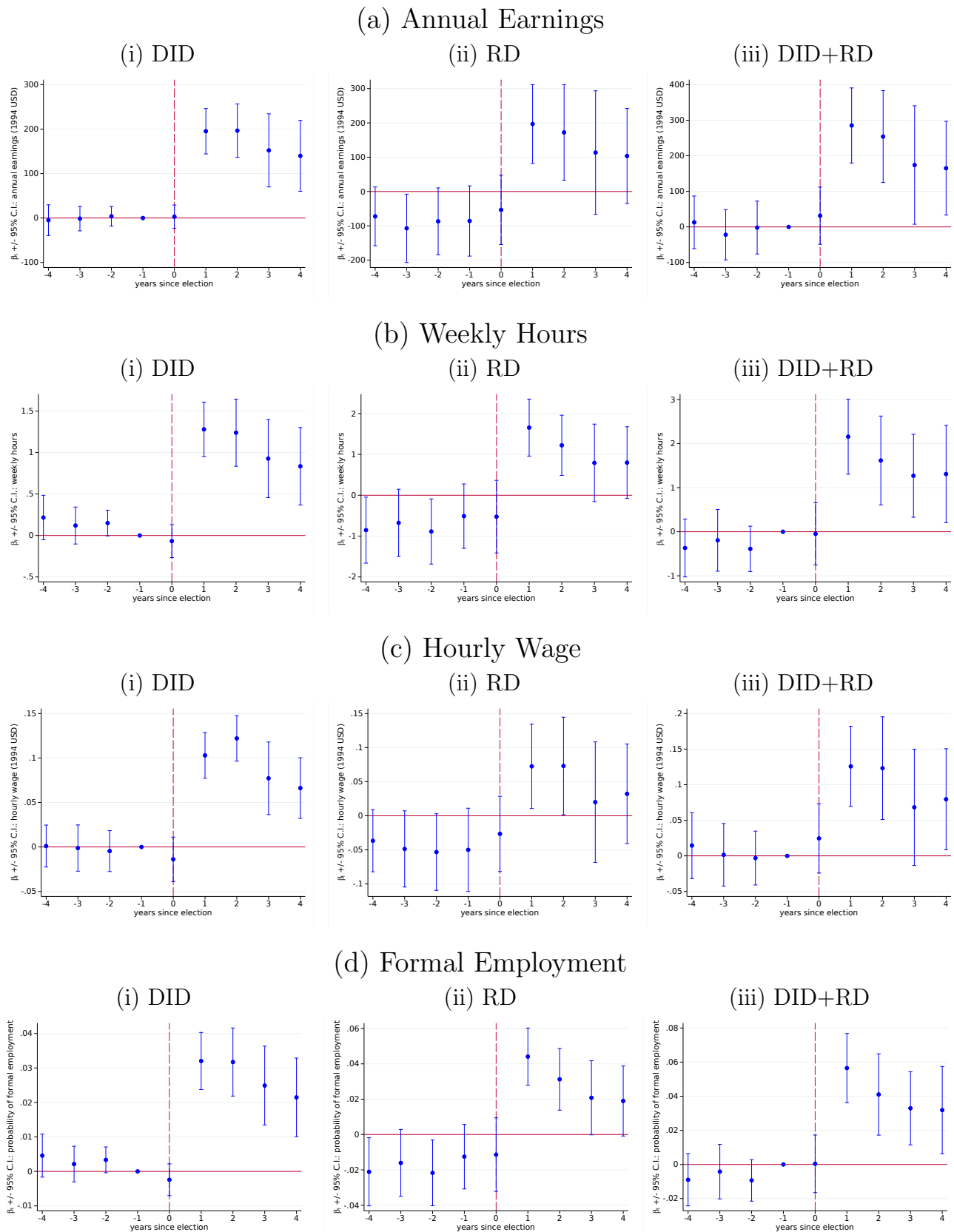_ , "Informality: Causes and consequences for development," *Annual Review of Economics*, 2020, *12*, 525–546.

Figures

Figure 1: Labor Market Trends for Winning and Losing Campaign Workers

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment

Notes: The figure shows average values of the outcome variable of interest in each period (year relative to election) separately for workers connected to a winning candidate ("Mayor") and workers connected to a runner-up candidate ("Runner-up"). We consider the following outcome variables: annual earnings in panel (a), contractual weekly hours in panel (b), hourly wages, obtained as annual earnings divided by contractual weekly hours times 52, in panel (c), a binary indicator for being formally employed (i.e., reported in RAIS) in panel (d), and a binary indicator for being employed in the public sector in panel (e). We restrict this analysis to the sample of dedicated and contractual workers in elections that are decided within a 5% margin.

**Figure 2:** Average Labor Market Returns to Winning Campaign Connections

(a) Annual Earnings

(i) DID

(ii) RD

(iii) DID+RD

(b) Weekly Hours

(i) DID

(ii) RD

(iii) DID+RD

(c) Hourly Wage

(i) DID

(ii) RD

(iii) DID+RD

(d) Formal Employment

(i) DID

(ii) RD

(iii) DID+RD

Notes: See the following page.

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Figure 2 (continued): ... Returns to Winning Campaign Connections

Notes: These graphs report event study results for DID, RD, and DID+RD specifications and the same four outcomes in panels (a)-(d) of Figure 1. Column (i) shows the set of coefficients β_ℓ^{DID} estimated from equation (1) on the dedicated and contractual workers in our sample. This specification controls for individual and year fixed effects as well as as interactions between period (year relative to election) and municipality fixed effects. Column (ii) presents estimates from an RD specification based on the sample of dedicated and contractual workers in elections that are decided within a 5% margin. This specification controls for the interactions between period and municipality fixed effects, and the interaction between margin of victory and period fixed effects. Column (iii) shows the set of coefficients β_ℓ^{DDRD} estimated from equation (2) on the sample of dedicated and contractual workers in elections that are decided within a 5% margin. This specification augments the RDD specification in column (ii) to include individual and year fixed effects. In all specifications, we impute zero earnings, hours or employment for workers who are not observed in RAIS in a given year; we restrict to workers who are linked to elected or runner-up mayoral candidates; we estimate effects in all available periods (i.e., from -18 to period 18); and we plot estimates for the 8-years window around the election. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure 3: A Pathway to Public Sector Employment
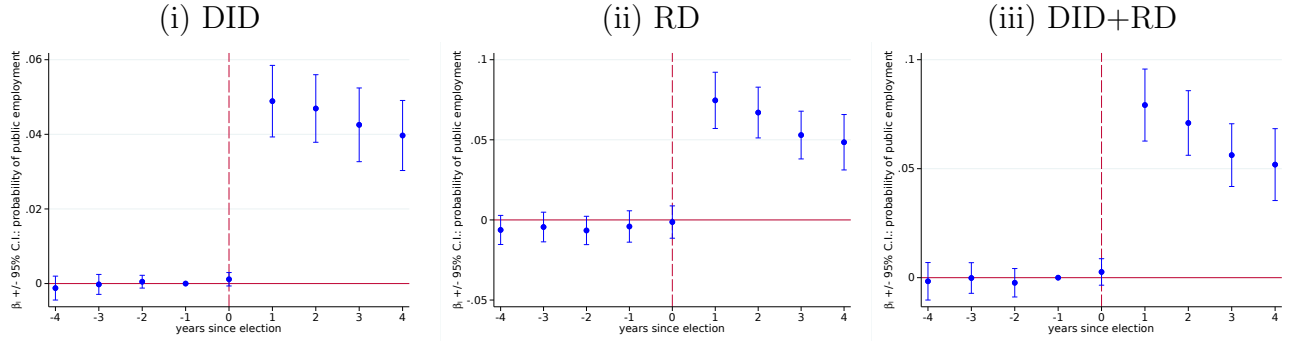
| (i) DID	(ii) RD	(iii) DID+RD

Notes: The outcome variable in this figure is an indicator variable that takes value 1 if a worker is employed in the public sector in a period (year relative to the election) and zero otherwise. See the notes to Figure 2 for additional details on the specifications across (i)–(iii). We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure 4: A Pathway to Public Sector Employment amid Informality
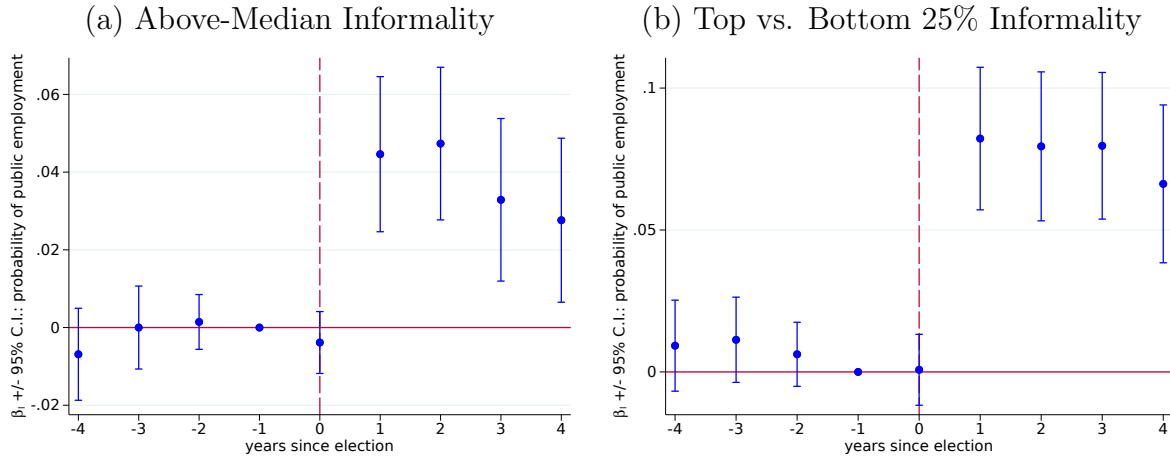
(a) Above-Median Informality   (b) Top vs. Bottom 25% Informality
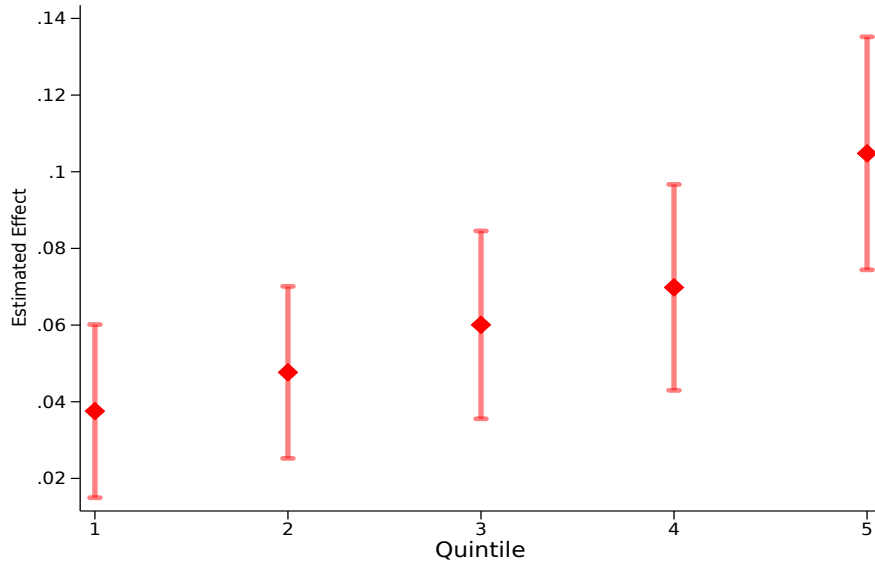

Notes: The outcome variable in this figure is an indicator variable that takes value 1 if a worker is employed in the public sector in period (year relative to the election) and zero otherwise. The figure shows coefficients estimated on the triple interaction of an indicator for being connected to a winning candidate, period (year relative to election) fixed effects, and an indicator variable that takes value 1 for workers in municipality with informality rates above the median in panel (a) (75th percentile in panel (b)) and 0 for workers in municipality with informality rates below the median in panel (a) (25th percentile in panel (b)). This specification controls for individual fixed effects, year fixed effects, the interactions between period and municipality fixed effects, the interaction between margin of victory and period fixed effects, and the interaction between year fixed effects and an indicator for being in a municipality above the median (75th percentile) or below the median (25th percentile) informality rate, the interaction between an indicator for being connected to a winning candidate and year fixed effects. Informality rates are based on Kovak (2013) from 1991 CENSUS data. We base this analysis on the sample of dedicated and contractual workers in elections that are decided within a 5% margin. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure 5: Returns to Challenger versus Incumbent Candidate Connections

(a) Challenger Connections

(b) Incumbent Connections
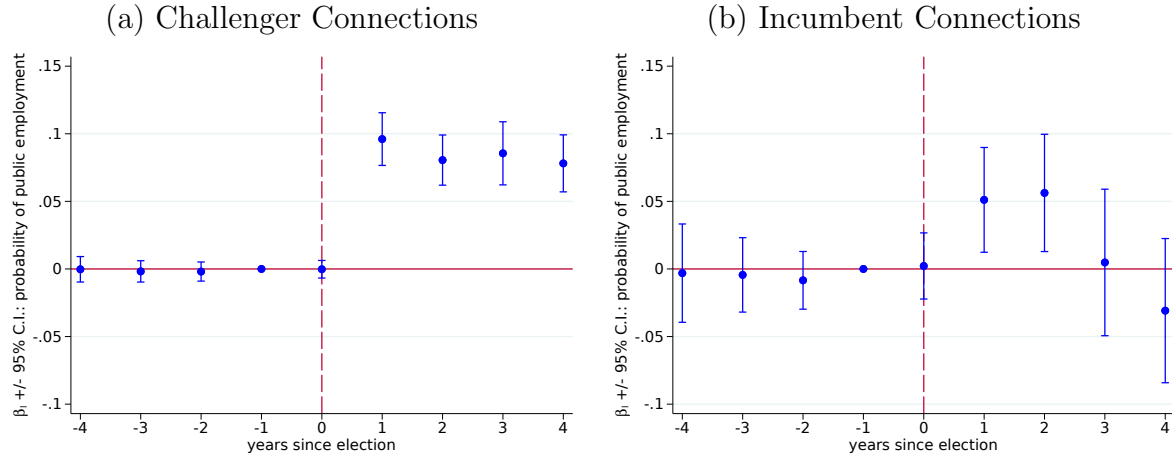
Notes: These figures report event study estimates based on the specification in Figure 3(iii) with two changes. First, we estimate separate regressions for workers connected to mayoral challengers (panel a) and for workers connected to incumbent mayors (panel b). Second, we include microregion × period FE instead of municipality × period FE since there is at most one incumbent running in a given election making it infeasible to identify differential returns to victorious incumbent connections within a given electoral cycle. The average microregion, roughly analogous to a commuting zone in the United States, comprises 10 municipalities. The specification is otherwise identical to Figure 3; see the notes therein for details. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure 6: Heterogeneous Effects of Campaign Connections on Public Employment, by Quintile of Amount Paid on the Campaign


Notes: The outcome variable is an indicator for being employed in the public sector. The figure shows coefficients attached to the interaction between a binary indicator variable for being connected to a winning candidate and a set of indicators for being in the 2nd, 3rd, 4th or 5th quintile of the distribution of payments within an election year. This specification includes controls for: individual fixed effects, year fixed effects, the interaction between post-election indicator and an indicator for being connected to the winning candidate, the interactions between period (year relative to the election) and municipality fixed effects, and the interactions between margin of victory and period fixed effects. We base the analysis on the sample of dedicated and contractual workers in election that are decided within a 5% margin. We restrict the analysis to the 8 years window around the election. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure 7: Heterogeneous Effects of Campaign Connections on Public Employment, Dedicated vs. Contractual Workers
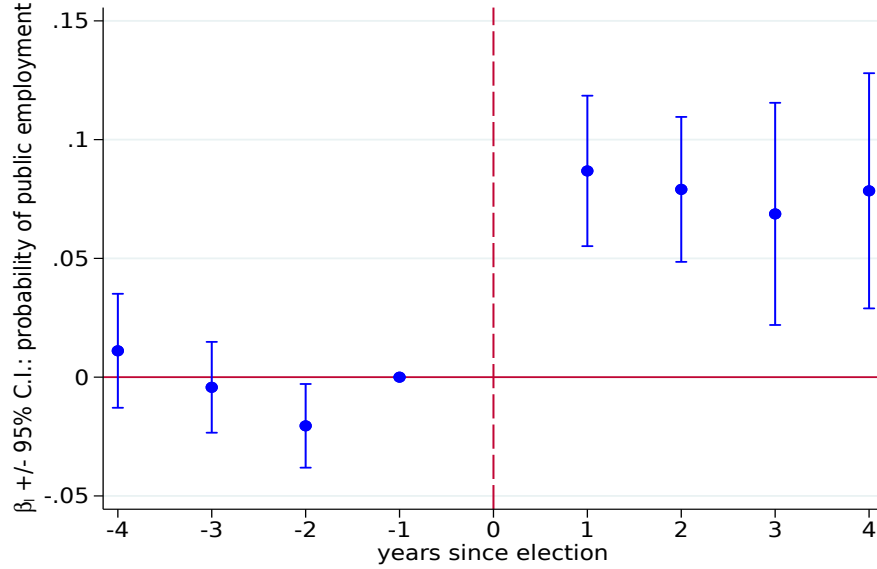
Notes: The outcome variable is an indicator for being employed in the public sector in a period. The figure shows coefficients estimated from the triple interaction of an indicator for being connected to a winning candidate, period (year relative to election) fixed effects and an indicator variable that takes value 1 for dedicated campaign workers and 0 for contractual campaign workers. This specification also includes controls for: individual fixed effects, year fixed effects, campaign (i.e., candidate-municipality-election year) fixed effects, the interactions between period and municipality fixed effects, the interactions between margin of victory and period fixed effects, the interactions between year fixed effects and a dummy for being a dedicated worker, the interactions between a dummy for being connected to a winning candidate and year fixed effects. We base the analysis on the sample of dedicated and contractual workers in election years 2004–12 for which data on both types of campaign workers are available. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure 8: Campaign Workers' Pay Relative to Similar Jobs in the Public Sector
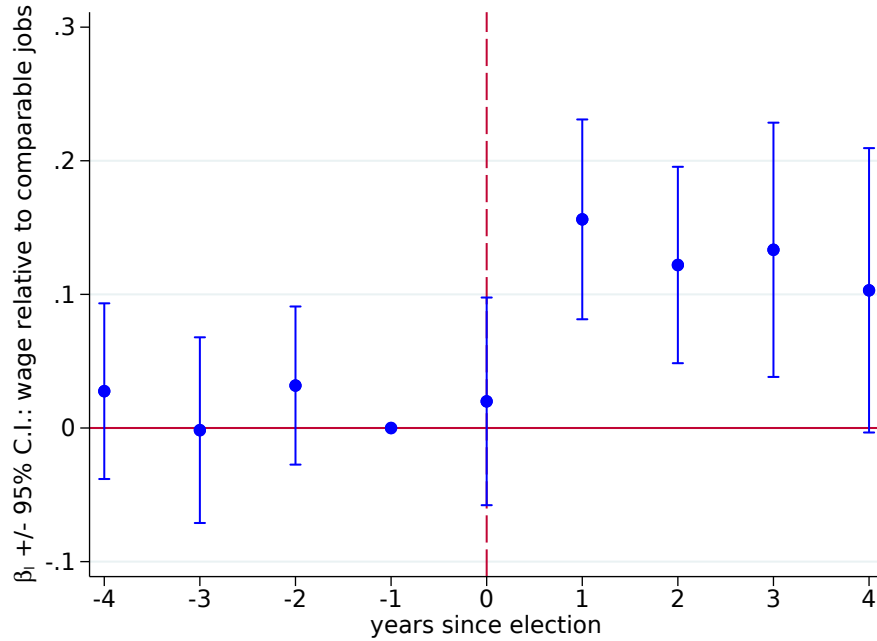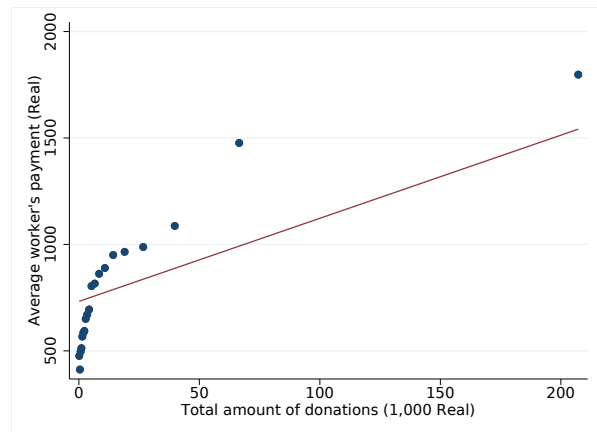

Notes: This figure re-estimates the specification in Figure 2 using as outcome variable the ratio between hourly wage and the average hourly wage among workers in the same municipality, occupation (3 digit CBO), year and sector (public vs others). We focus only on workers who are employed for at least one year in the public sector between time 1 and 4. The specification is otherwise identical to Figure 2.

Figure 9: Campaign Capital and Heterogeneous Returns to Connections

(a) Campaign Workers' Public Pay
Differential by Campaign Capital
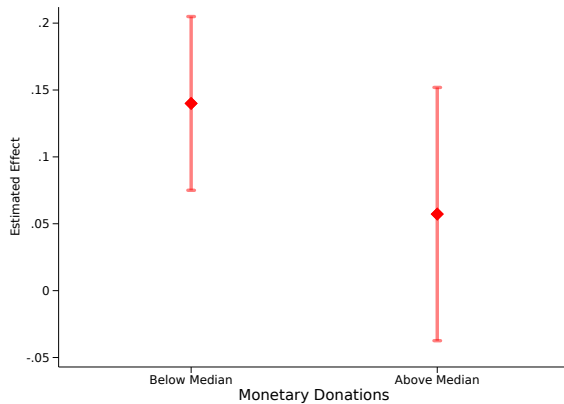
(b) Campaign Labor Payments
vs. Campaign Capital

Notes: Panel (a) re-estimates the specification in Appendix Figure 8 allowing the coefficients to vary with total donations to the campaign being above versus below the median. We report 95% confidence intervals based on standard errors that are clustered at the municipality level. Panel (b) plots the relationship between average pay received by contractual workers during the campaign and total amount of donations received by the campaign. Observations are grouped in 20 equal-sized bins based on the x-variable. For each bin we plot the average value of the observations within the bin. The linear fit line (in red) is estimated starting from the underlying data.

Tables

Table 1: Describing Workers with Different Types of Campaign Connections

| | Campaign Labor | | Campaign | General |
| | Contractual | Dedicated | Donors | Population |
	(1)	(2)	(3)	(4)
Age at the election year	34.3	32.4	39.6	34.4
Male	0.516	0.377	0.655	0.608
Primary Education	0.434	0.255	0.266	0.421
Secondary Education	0.479	0.561	0.404	0.424
College Education	0.028	0.069	0.054	0.037
Post-Graduate Education	0.059	0.113	0.276	0.117
Prior Years in Formal Sector	0.396	0.287	0.519	–
Prior Years in Public Sector	0.071	0.050	0.179	–
Prior Earnings (1994 USD)	3398	3734	7532	–
Ever Campaign Donor	0.074	0.055	1.00	0.005
Number of Individuals	379,008	3,333	605,148	111,243,274

Notes: Column 1 includes the sample of contractual workers who were reported to TSE in the election years 2004–2012 and who can be linked to RAIS in any year from 1994–2014. Column 2 includes the sample of dedicated workers of political organizations in RAIS from 1994–2014 who can be linked to a mayoral candidate in the election years 1996–2012. Column 3 includes the sample of donors in the election years 2004–2012 who can be linked to RAIS in the years 1994–2014. In columns 1 to 3, we consider only workers and donors who are linked either to an elected or to a runner-up mayoral candidate, and we exclude workers who are linked to multiple candidates. Column 4 shows descriptive statistics for the population of formal workers in Brazil based on RAIS. "Prior Years in Formal Sector" and "Prior Years Public Sector" refer to the fraction of years spent in the formal and public sector, respectively, prior to the election year. "Prior Earnings (1994 USD)" refer to average annual earnings prior to the election year. Earnings are deflated to August 1994 and expressed in US Dollars. We consider only workers and donors formally employed between 1994–2014, aged 15 to 65.

Table 2: Summary Estimates and Alternative DID+RD Specifications

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Earnings				
Post-election × Mayor	233.948***	264.645***	233.413***	241.412***	239.412***
	(47.269)	(46.624)	(47.333)	(54.700)	(55.970)
Mean D.V. post-election runner-up	1,520.7	1,520.7	1,520.7	1,520.7	1,520.7
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (b): Weekly Hours				
Post-election × Mayor	1.885***	2.324***	1.911***	1.832***	1.794***
	(0.344)	(0.332)	(0.348)	(0.366)	(0.364)
Mean D.V. post-election runner-up	20.98	20.98	20.98	20.98	20.98
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (c): Hourly Wage				
Post-election × Mayor	0.101***	0.115***	0.100***	0.113***	0.109***
	(0.024)	(0.024)	(0.024)	(0.028)	(0.030)
Mean D.V. post-election runner-up	0.737	0.737	0.737	0.737	0.737
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (d): Formal Employment				
Post-election × Mayor	0.048***	0.058***	0.049***	0.047***	0.046***
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Mean D.V. post-election runner-up	0.500	0.500	0.500	0.500	0.500
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (e): Public Employment				
Post-election × Mayor	0.069***	0.069***	0.069***	0.071***	0.069***
	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
Mean D.V. post-election runner-up	0.085	0.085	0.085	0.085	0.085
Observations	559,221	559,221	559,221	559,221	559,221
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Margin×period FE	yes	yes	yes	yes	yes
Municipality×period FE	yes	no	yes	yes	yes
Municipality×election year×period FE	no	yes	no	no	no
Margin×period FE×Mayor	no	no	yes	no	no
Quadratic Margin×period FE×Mayor	no	no	no	yes	no
Cubic Margin×period FE×Mayor	no	no	no	no	yes

Notes: This table presents DID+RD estimates obtained under alternative sets of controls. We consider the following outcome variables: annual earnings in panel (a), contractual weekly hours in panel (b), hourly wages, obtained as annual earnings divided by contractual weekly hours times 52, in panel (c), a dummy for being formally employed (i.e., reported in RAIS) in panel (d) and a dummy for being employed in the public sector in panel (e). The table shows the set of coefficients estimated from the interaction between a dummy for being connected to a winning candidate and a post-election dummy that takes value 1 in periods 1 to 4. Column 1 is our baseline specification of equation (1) that includes the following controls: individual fixed effects, year fixed effects, municipality times period (years relative to the election) fixed effects and the interaction between margin of victory and period fixed effects. Column 2 replaces municipality times period fixed effects with municipality times election year times period fixed effects. Column 3 replaces the interaction between margin of victory and period fixed effects of Column 1 with the interaction between margin of victory, period fixed effects and a dummy for being connected to a winning candidate. Column 4 replaces the interaction between margin of victory and period fixed effects of Column 1 with the interaction between a quadratic function of margin of victory, period fixed effects and a dummy for being connected to a winning candidate. Column 5 replaces the interaction between margin of victory and period fixed effects of Column 1 with the interaction between a cubic function of margin of victory, period fixed effects and a dummy for being connected to a winning candidate. In all specifications, we impute zero earnings, hours or employment for workers who are not observed in RAIS in a given year; we restrict to workers who are linked to elected or runner-up mayoral candidates; we estimate effects in the 8-years window around the election. Standard errors are clustered at the municipality level.
* p<0.10, ** p<0.05, *** p<0.01.

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Table 3: Summary Estimates and Alternative RD Specifications

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Earnings				
Post-election × Mayor	160.138***	166.867***	157.647***	141.865**	139.439**
	(55.916)	(58.545)	(56.225)	(68.725)	(71.033)
Mean D.V. post-election runner-up	1,520.7	1,520.7	1,520.7	1,520.7	1,520.7
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (b): Weekly Hours				
Post-election × Mayor	1.234***	1.445***	1.235***	1.100***	1.109***
	(0.303)	(0.297)	(0.303)	(0.335)	(0.332)
Mean D.V. post-election runner-up	20.98	20.98	20.98	20.98	20.98
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (c): Hourly Wage				
Post-election × Mayor	0.058**	0.059**	0.056*	0.062*	0.058
	(0.029)	(0.030)	(0.029)	(0.035)	(0.037)
Mean D.V. post-election runner-up	0.737	0.737	0.737	0.737	0.737
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (d): Formal Employment				
Post-election × Mayor	0.032***	0.037***	0.032***	0.030***	0.030***
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
Mean D.V. post-election runner-up	0.500	0.500	0.500	0.500	0.500
Observations	559,221	559,221	559,221	559,221	559,221
	Panel (e): Public Employment				
Post-election × Mayor	0.064***	0.064***	0.064***	0.069***	0.067***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Mean D.V. post-election runner-ups	0.085	0.085	0.085	0.085	0.085
Observations	559,221	559,221	559,221	559,221	559,221
Margin×period FE	yes	yes	yes	yes	yes
Municipality×period FE	yes	no	yes	yes	yes
Municipality×election year×period FE	no	yes	no	no	no
Margin×period FE×Mayor	no	no	yes	no	no
Quadratic Margin×period FE×Mayor	no	no	no	yes	no
Cubic Margin×period FE×Mayor	no	no	no	no	yes

Notes: This table presents RD estimates obtained under alternative sets of controls, following the same sequence of specification checks as in Table 2 for the DID+RD specification (albeit omitting individual and year fixed effects throughout). The estimate in column 1 is the summary result corresponding to panel (ii) specifications in Figure 2. Standard errors are clustered at the municipality level.
* p<0.10, ** p<0.05, *** p<0.01.

Table 4: Positive Selection into the Public Sector among Connected Workers

	(1) Private earnings 4 years before the election	(2) Private earnings 2 years before the election
Mayor × Tercile 3	0.020***	0.014***
	(0.005)	(0.004)
Mayor × Tercile 2	0.016***	0.009**
	(0.004)	(0.004)
Mayor × Tercile 1	0.001	-0.005
	(0.004)	(0.004)
Mayor	0.056***	0.059***
	(0.011)	(0.011)
Mean D.V. post-election, runner-up	0.085	0.085
Observations	485,328	485,328

Notes: The outcome in this table is an indicator for being employed in the public sector in a year. The estimates correspond to the interaction of an indicator variable that takes value 1 for workers connected to a winning candidate in the post-election years and a set of indicator variables for being in the bottom, middle or top third of the within-municipality-election-period distribution of private sector earnings. In column 1, we assign workers to terciles of the distribution of private sector earnings based on average earnings in the 4 years preceding the election, and in column 2 we consider average private earnings in the 2 years preceding the election. The omitted category—identified by the "mayor" coefficient—comprises all workers who were not employed in the private sector in the 4 (2) years preceding the election. This specification otherwise follows the baseline from column 1 of Table 2 and includes the following controls: the interaction between post-election dummy and a dummy for being connected to the winning candidate (which is labelled as "Mayor" in the table), individual fixed effects, year fixed effects, the interactions between period (year relative to the election) and municipality fixed effects, and the interactions between margin of victory and period fixed effects. We base the analysis on the sample of dedicated and contractual workers in elections that are decided within a 5% margin. We restrict the analysis to the 8 years window around the election. Standard errors are clustered at the municipality level.
* p<0.10, ** p<0.05, *** p<0.01.

Table 5: Qualifications and Selection into the Public Sector among Connected Workers

Public employment requiring:	(1) Middle school degree	(2) High school degree	(3) University degree
Mayor × Qualified	0.002	0.019***	0.039***
	(0.004)	(0.002)	(0.006)
Mayor	0.010	0.010	0.001
	(0.008)	(0.007)	(0.003)
Mean D.V. post-election, runner-up	0.021	0.030	0.016
Observations	474,581	474,581	474,581

Notes: The outcome in each column equals one if the individual has the requisite education level for the public job in which they are employed and zero otherwise, where requisite education is defined as in Colonnelli et al. (2020). We split the analysis into positions requiring at least middle school (column 1), high school (column 2), and university degrees (column 3). This specification otherwise follows the baseline from column 1 of Table 2 and includes the following controls: the interaction between post-election dummy and a dummy for being connected to the winning candidate (which is labelled as "Mayor" in the table), individual fixed effects, year fixed effects, the interactions between period (year relative to the election) and municipality fixed effects, and the interactions between margin of victory and period fixed effects. We base the analysis on the sample of dedicated and contractual workers in elections that are decided within a 5% margin. We restrict the analysis to the 8 years window around the election. Standard errors are clustered at the municipality level.
Significance levels: ∗ : 10%   ∗∗ : 5%   ∗ ∗ ∗ : 1%.

Table 6: Qualification and Selection in Public Sector Jobs by Occupation

Public employment requiring:	(1) Middle school degree	(2) High school degree	(3) University degree
Panel (a): Professional or Managerial Occupations			
Mayor × Qualified	0.000	0.002***	0.033***
	(0.001)	(0.001)	(0.005)
Mayor	0.000	0.003*	0.000
	(0.001)	(0.002)	(0.002)
Mean D.V. post-election, runner-up	0.001	0.002	0.014
Observations	474,581	474,581	474,581
Panel (b): Technical or Supervisory Occupations			
Mayor × Qualified	0.003***	0.015***	0.004***
	(0.001)	(0.002)	(0.001)
Mayor	-0.001	0.004	0.001
	(0.003)	(0.006)	(0.001)
Mean D.V. post-election, runner-up	0.005	0.024	0.001
Observations	474,581	474,581	474,581
Panel (c): Other White Collar Occupations			
Mayor × Qualified	-0.000	0.000	0.002**
	(0.002)	(0.001)	(0.001)
Mayor	0.004**	0.003*	0.001
	(0.002)	(0.002)	(0.001)
Mean D.V. post-election, runner-up	0.004	0.003	0.001
Observations	474,581	474,581	474,581
Panel (d): Blue Collar Occupations			
Mayor × Qualified	-0.001	0.000	0.000
	(0.004)	(0.000)	(.)
Mayor	0.006	0.001	0.000
	(0.006)	(0.000)	(.)
Mean D.V. post-election, runner-up	0.011	0.000	0.000
Observations	474,581	474,581	474,581

Notes: This table re-estimate Table 5 distinguishing between workers in four occupation groups (see also Section B.3 for details on the occupation classification). We consider we consider workers in professional or managerial occupations in panel a; technical or supervisory occupations in panel b; other white collar occupations in panel c; blue collar occupations in panel d. The specification is otherwise identical to Table 5; see the notes therein for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Online Appendix

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A Further Empirical Results

Appendix Figures

Figure A.1: RD balance tests on Campaign Workers' Characteristics (I)
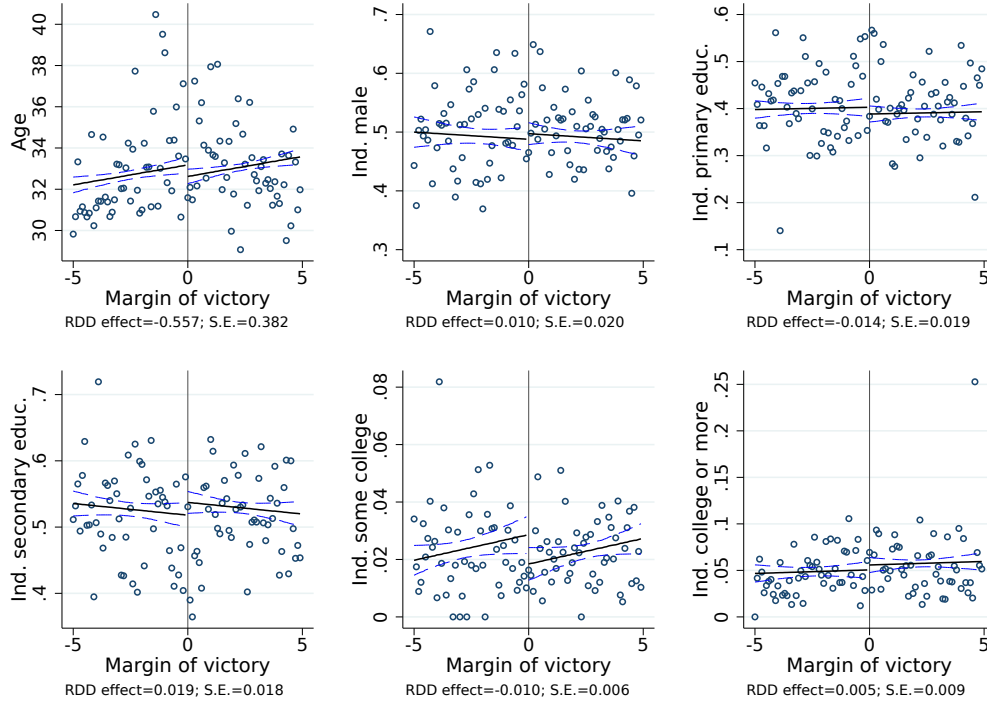
Notes: Moving from the top left to the bottom right figure we consider the following outcome variables measured in the election year: age, gender (1=male, 0=female), primary education dummy, secondary education dummy, college education dummy, post-graduate education dummy. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for a linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.
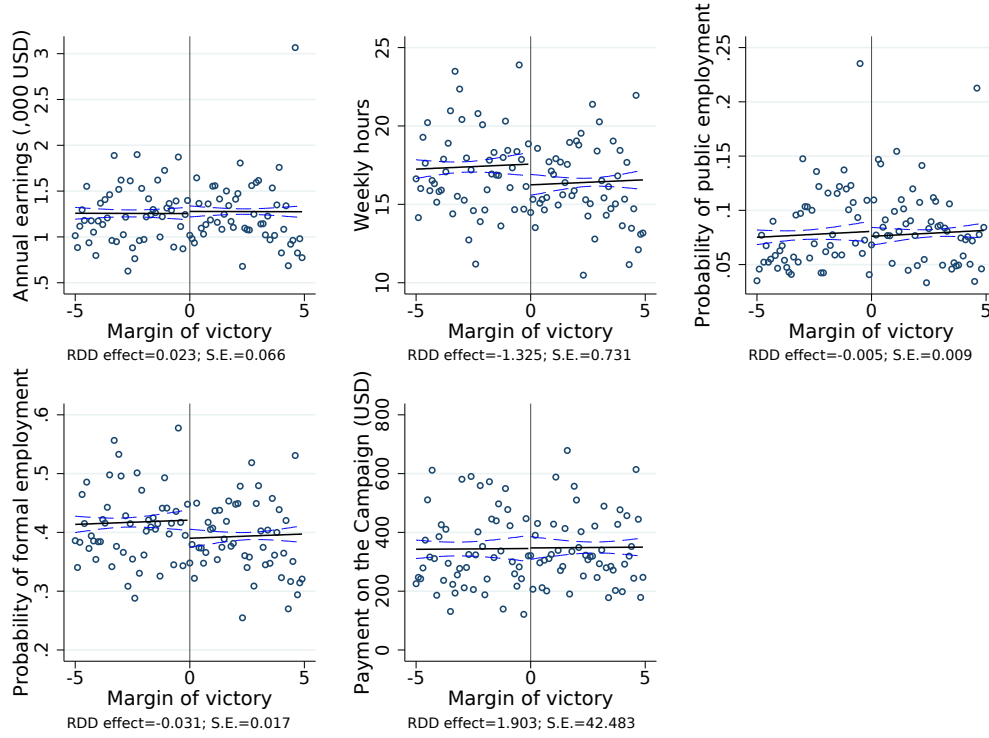
Figure A.2: RD balance tests on Campaign Workers' Characteristics (II)

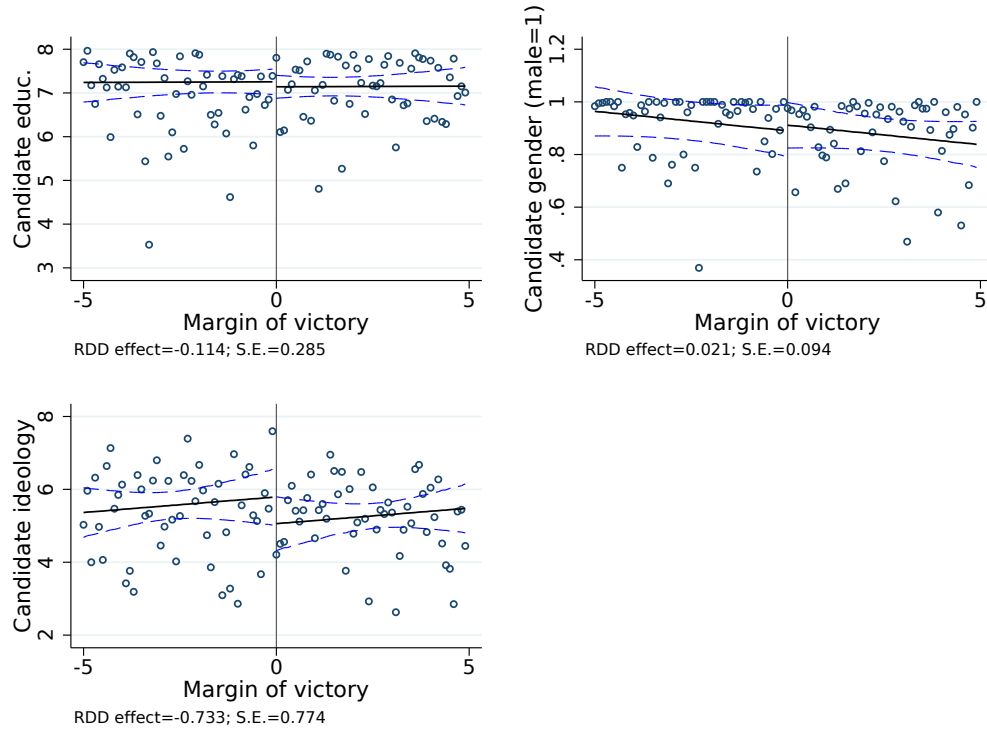*Notes*: Moving from the top left to the bottom right figure we consider the following outcome variables measured in the election year: annual earnings, contractual weekly hours, a dummy variable for being employed in the public sector, a dummy variable for being employed formally, payment amount received (for contractual workers only). The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.
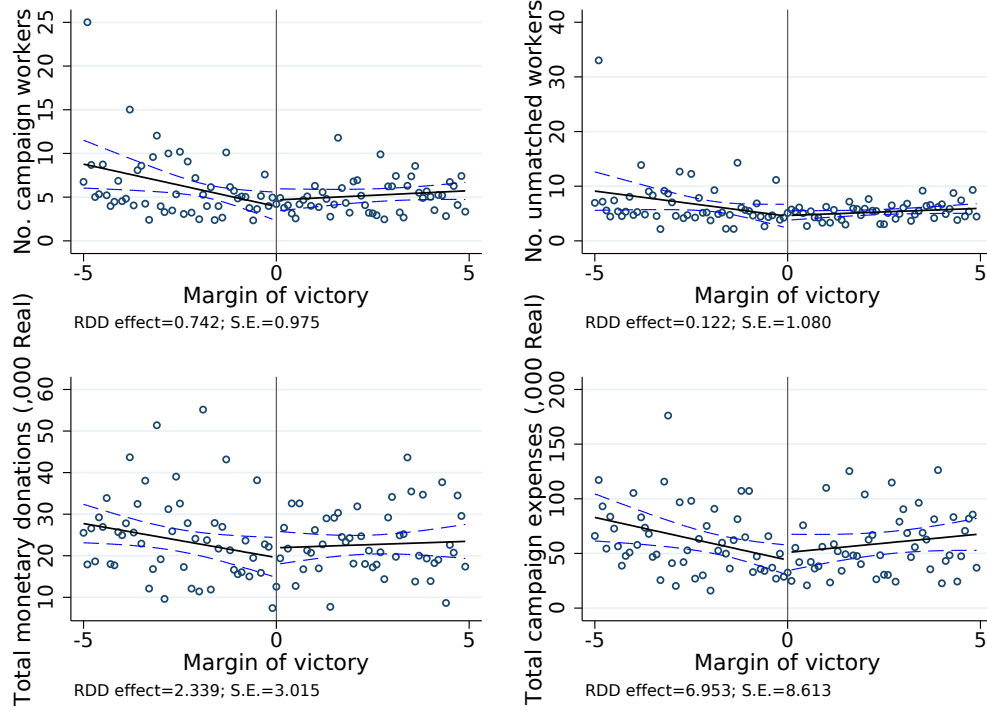
Figure A.3: RD balance tests on Candidate Characteristics

Notes: Moving from the top left to the bottom right figure we consider the following outcome variables measured in the election year: candidate education, candidate gender (male=1), candidate ideology. Candidate education in TSE data is classified as follows: (1) Illiterate; (2) Literate; (3) Incomplete Elementary School; (4) Complete Elementary School; (5) Incomplete High School; (6) Complete High School; (7) Incomplete Higher Education; (8) Complete Higher Education. The ideology index ranges from 1 to 10 with 1 being the extreme left and 10 the extreme right (see Power and Zucco, 2012, for details). The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

**Figure A.4:** RD balance tests on Campaign Characteristics

Notes: Moving from the top left to the bottom right figure we consider the following outcome variables which are measured at the campaign level: number of workers employed by the campaign, number of contractual workers who are employed by the campaign and that cannot be matched to RAIS, total amount of monetary donations and campaign payments measured in thousands of Real. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.5: RD balance tests on Density of Workers

RDD effect=-26.743; S.E.=24.428
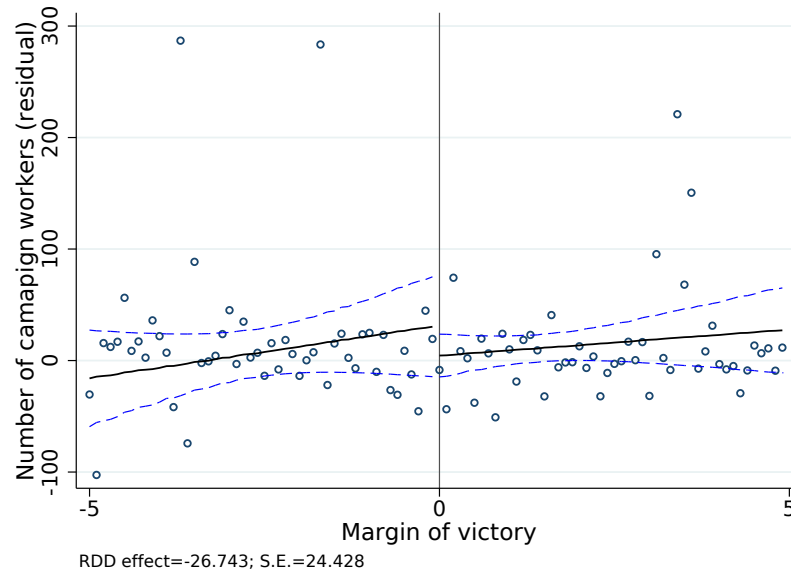
Notes: In this Figure the outcome variable is the residual number of workers in a campaign in the year of the election or prior years. This is obtained as the residual from a regression of the number of workers in a campaign and year on individual, year and municipality time period fixed effects. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level. See Appendix Table A.7, and associated discussion thereof, for more details on this test.

Figure A.6: RD balance tests on Number of Small Payments Workers

RDD effect=0.391; S.E.=1.332
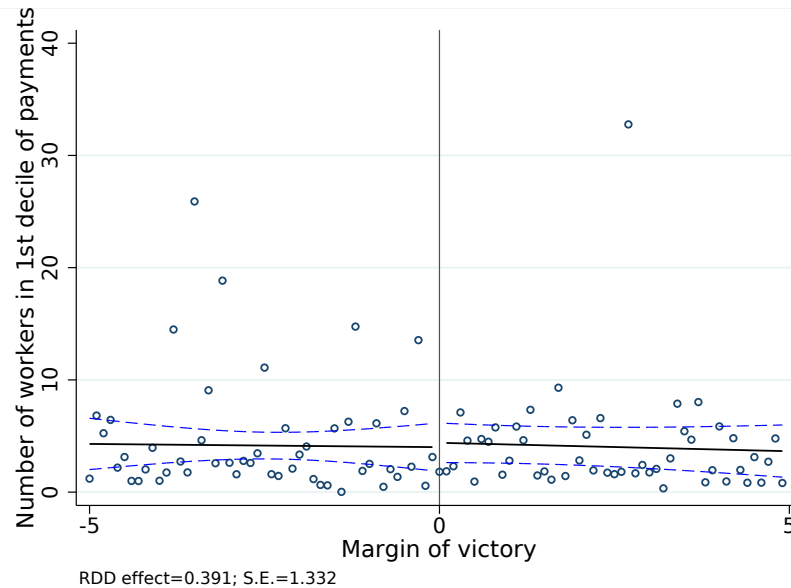
Notes: In this Figure we consider as outcome variable the number of contractual workers employed by the campaign who receive a payment in the first decile of the payment distribution across all campaigns of an election year. In this analysis, we consider the universe of contractual workers that are reported by TSE, including workers who cannot be matched to RAIS. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

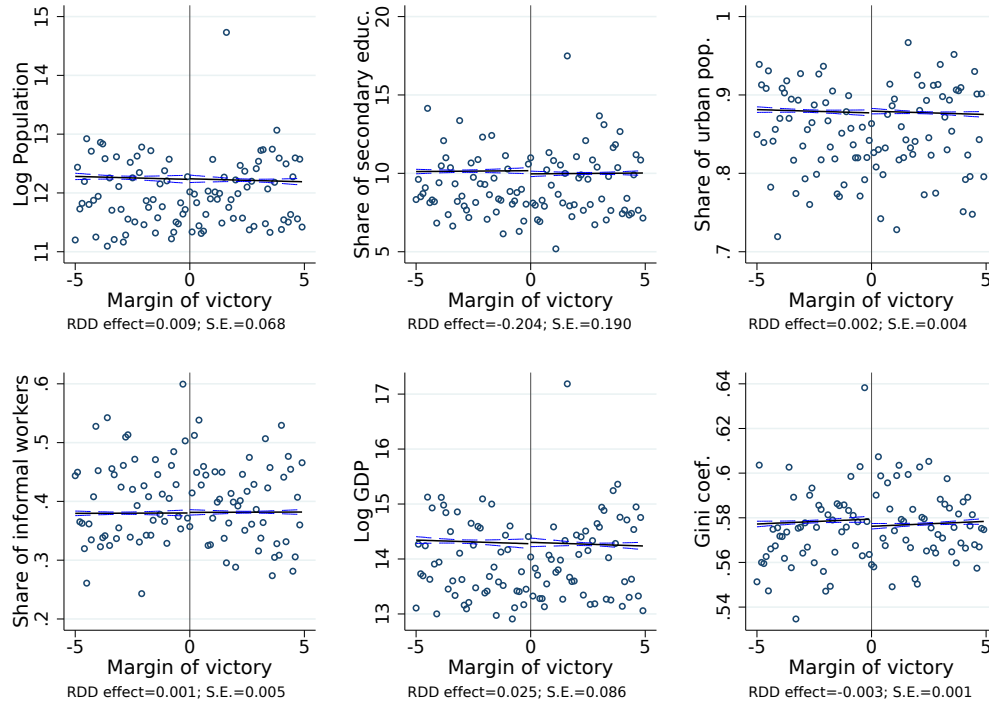**Figure A.7:** RD balance tests on Election Municipality Characteristics

Notes: Moving from the top left to the bottom right figure we consider the following outcome variables which are measured at the municipality level: log population, share of population with secondary education, share of urban population, share of formal workers, log GDP per capita, Gini coefficient. We base these measures on 2000 CENSUS data with the exception of informality rates which are based on Kovak (2013) 1991 CENSUS data. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.8: Restricted Sample to Periods around Election

(a) Annual Earnings

(i) DID

(ii) RD

(iii) DID+RD

(b) Weekly Hours

(i) DID

(ii) RD

DID+RD

(c) Hourly Wage

(i) DID

(ii) RD

(ii) DID+RD

(d) Formal Employment

(i) DID

(ii) RD

(iii) DID+RD

Notes: This figure re-estimates the specifications in Figure 2 including only 4 years prior and 4 years after the election year in the estimating equation. The specification is otherwise identical to that in Figure 2.

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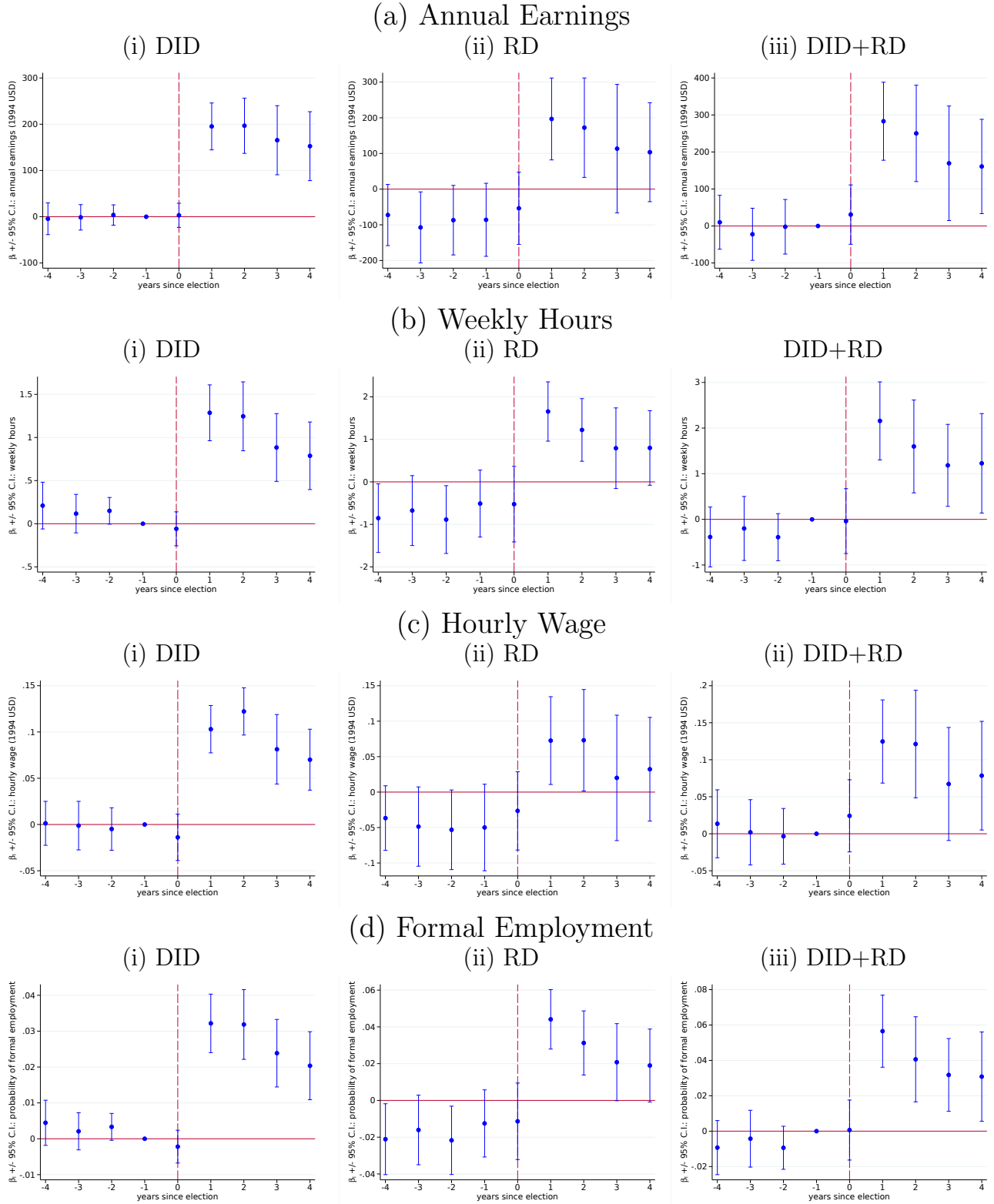
Figure A.9: DID+RD: Alternative TWFE Estimator
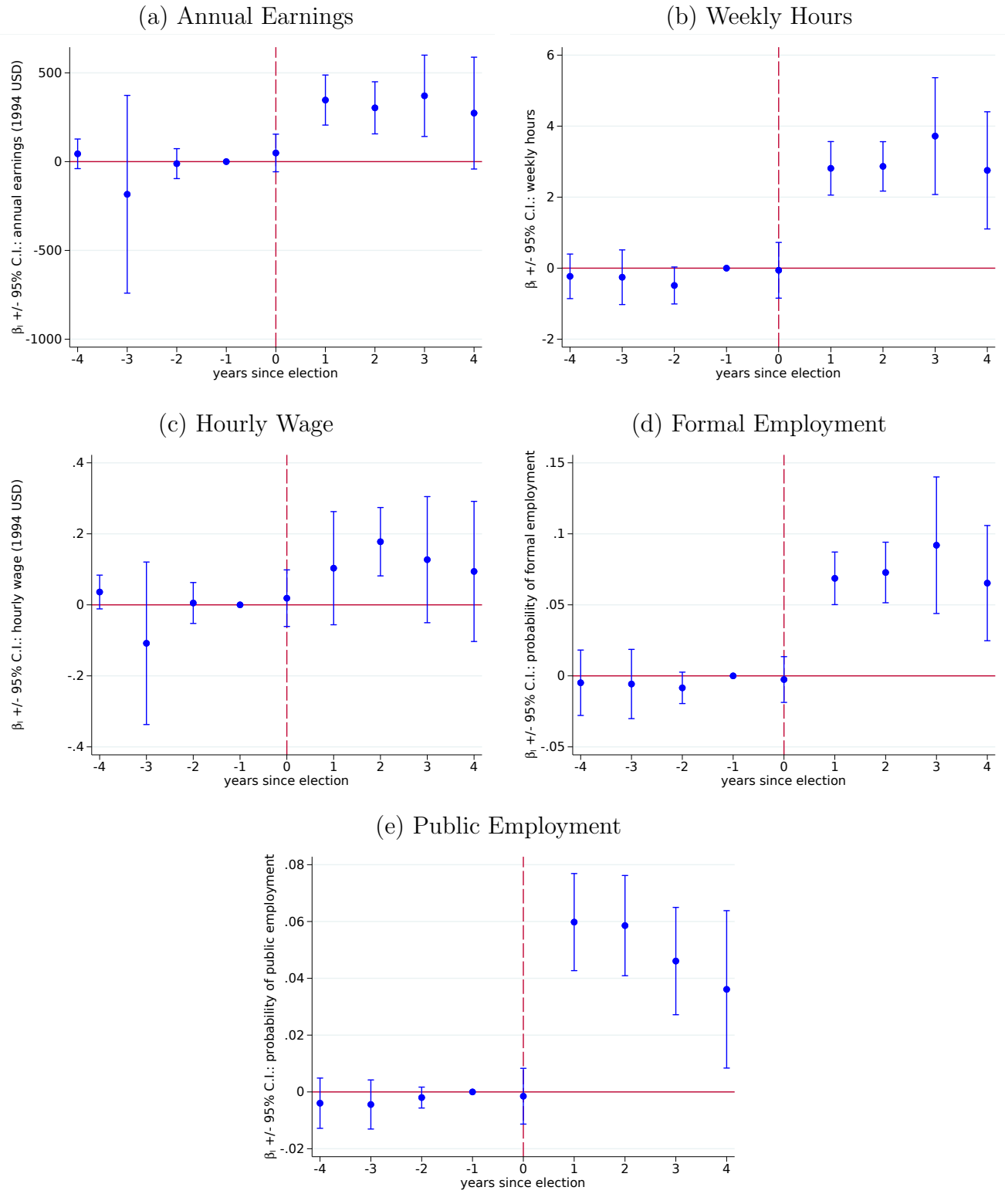
(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment

Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using the estimator based on De Chaisemartin and d'Haultfoeuille (2020).

Figure A.10: DID+RD: Alternative TWFE Estimator and Functional Form
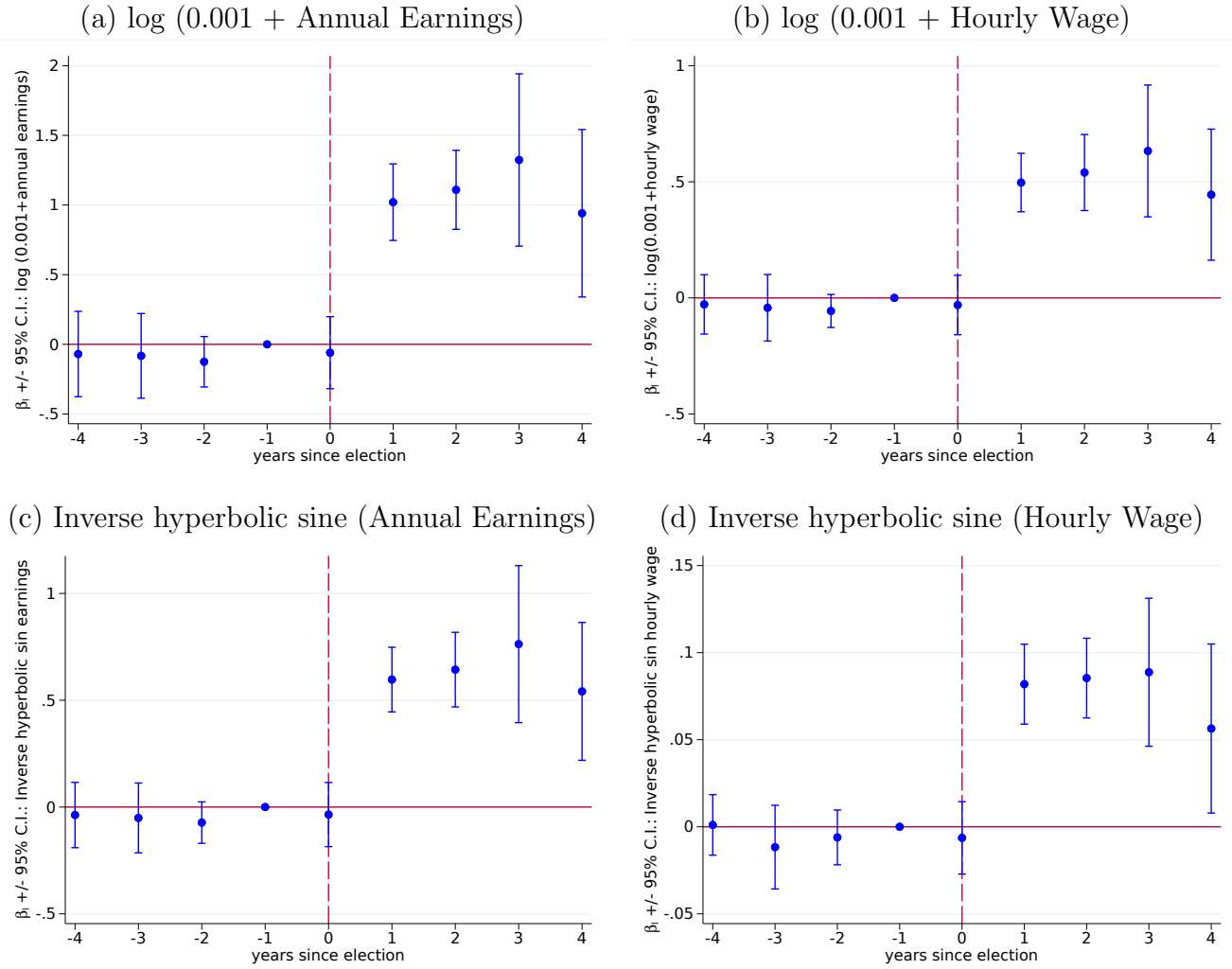
(a) log (0.001 + Annual Earnings)

(b) log (0.001 + Hourly Wage)

(c) Inverse hyperbolic sine (Annual Earnings)

(d) Inverse hyperbolic sine (Hourly Wage)

Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using the estimator based on De Chaisemartin and d'Haultfoeuille (2020). In panel (a) we use the log of 0.001 plus annual earnings as the dependent variable; in panel (b) we use log of 0.001 plus hourly wages as the dependent variable; in panel (c) we use the inverse hyperbolic sin of annual earnings as the dependent variables; in panel (d) we use the inverse hyperbolic sin of hourly wages as the dependent variables. The specification is otherwise identical to that in Figure A.9.

Figure A.11: DID+RD: Close Election within the Optimal Bandwidth

(a) Annual Earnings

Optimal bandwidth=10.197

(b) Weekly Hours

Optimal bandwidth=9.894

(c) Hourly Wage

Optimal bandwidth=9.987

(d) Formal Employment

Optimal bandwidth=9.640000000000001

(e) Public Employment

Optimal bandwidth=10.077
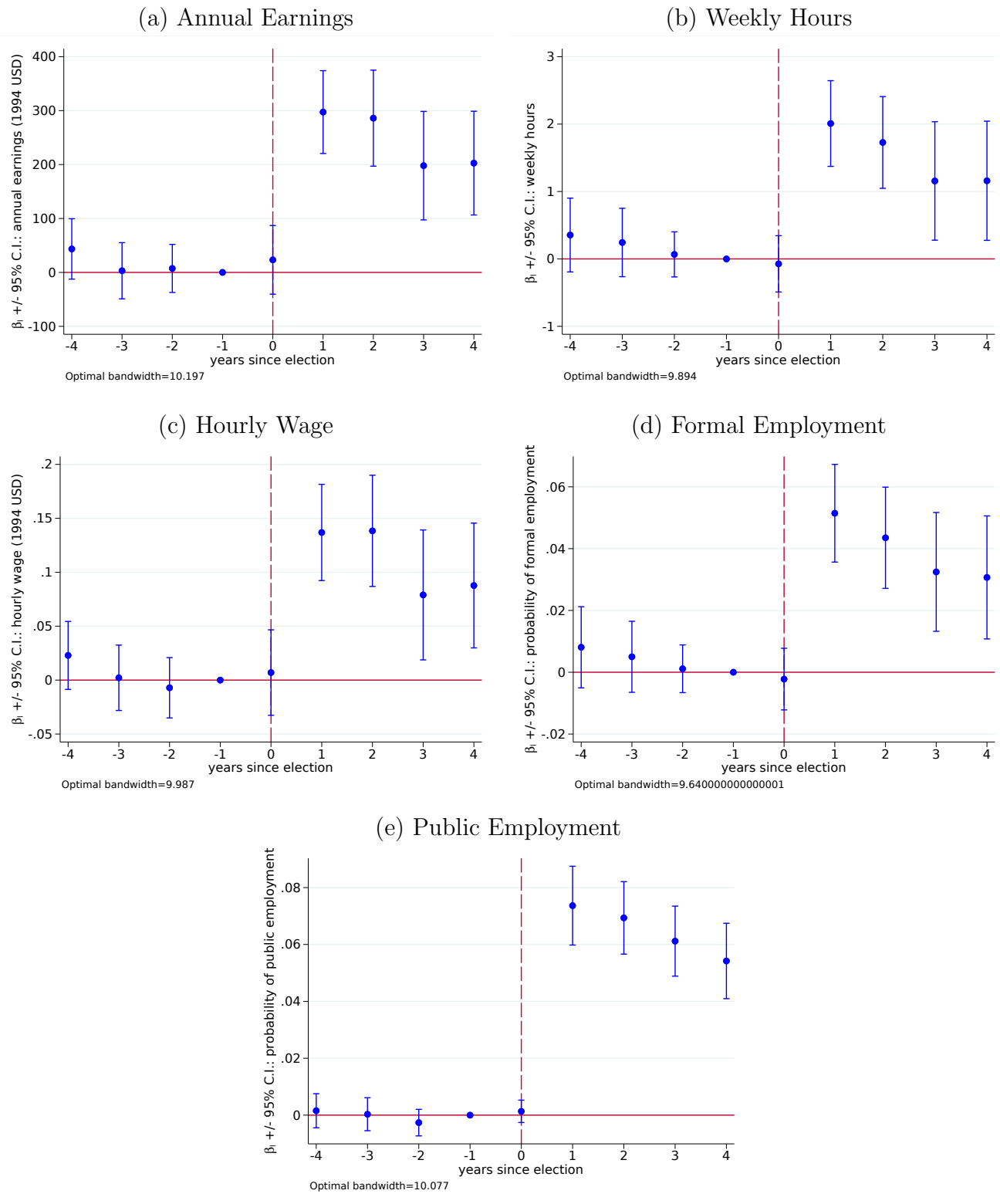
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using the optimal bandwidth based on Calonico et al. (2015). The specification is otherwise identical to that in Figure 2.

Figure A.12: DID+RD: Close Election within a 1% Margin

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment
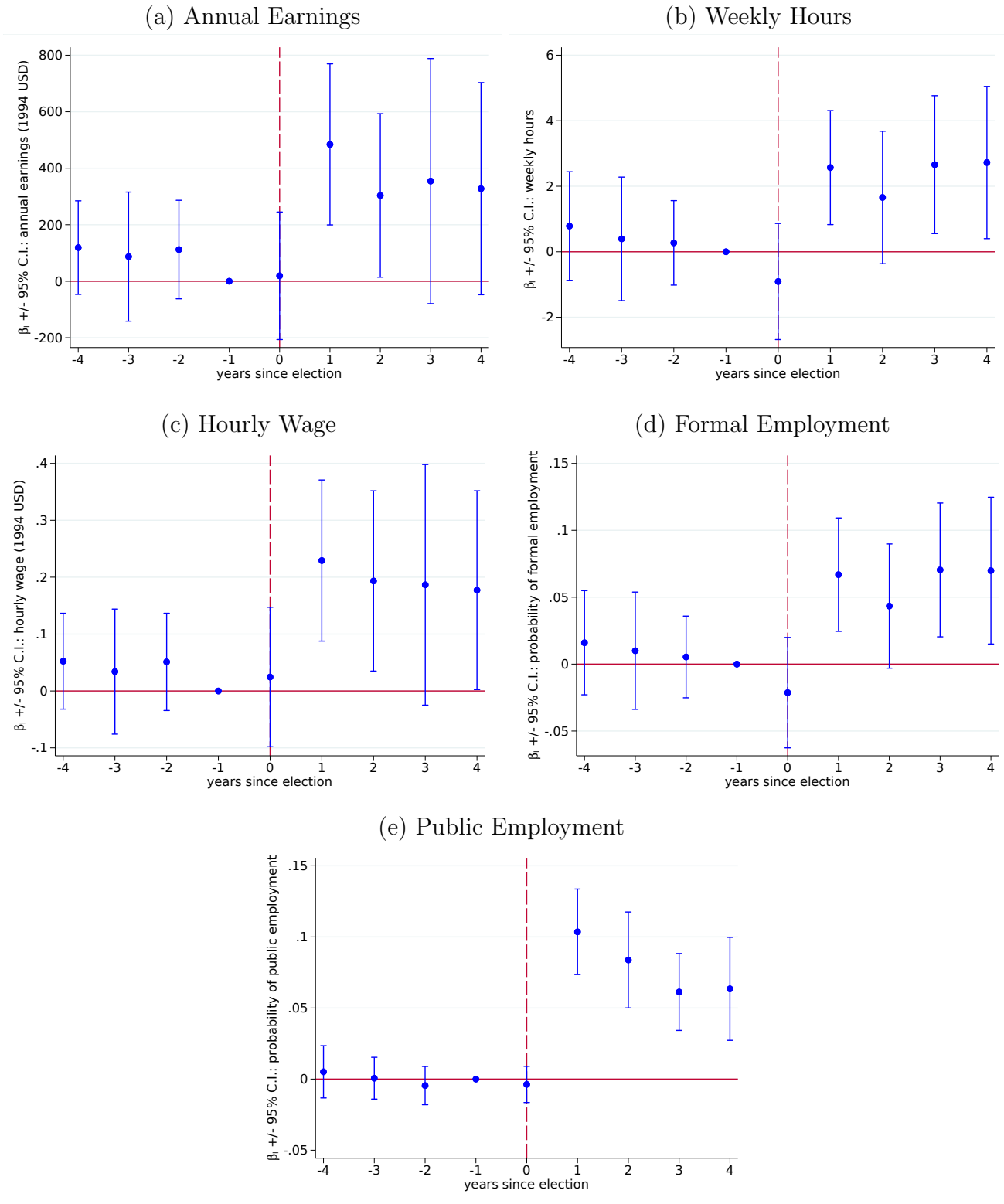
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using a 1% victory margin bandwidth instead of 5%. The specification is otherwise identical to that in Figure 2.

Figure A.13: DID+RD: Close Election within a 10% Margin

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment
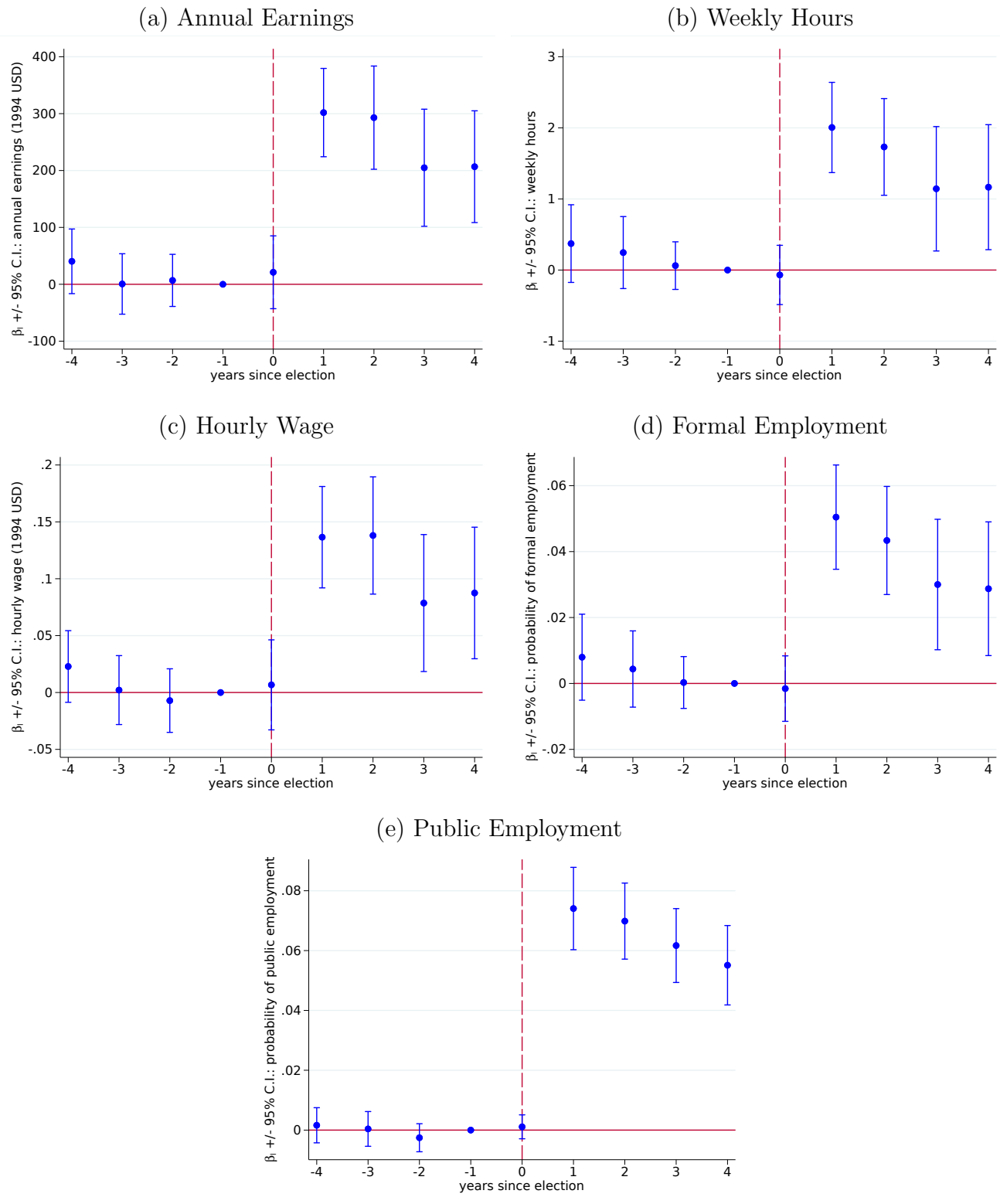
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using a 10% victory margin bandwidth instead of 5%. The specification is otherwise identical to that in Figure 2.

Figure A.14: DID+RD: Two-way Clustering on Municipality and Worker

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment
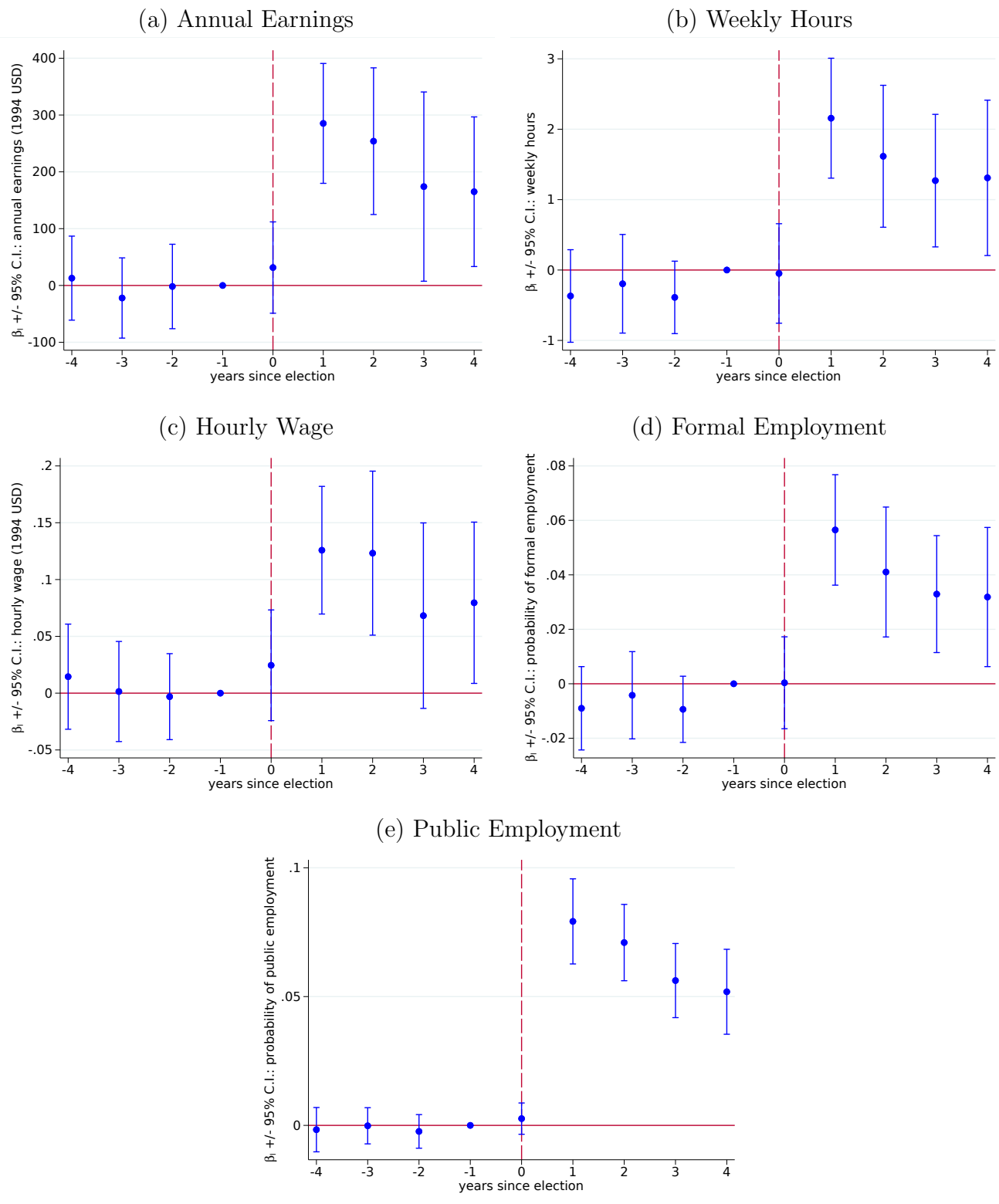
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using two-way clustering by election municipality and worker. The specification is otherwise identical to that in Figure 2.

Figure A.15: DID+RD: Sample that Includes Unmatched CPF Numbers

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment
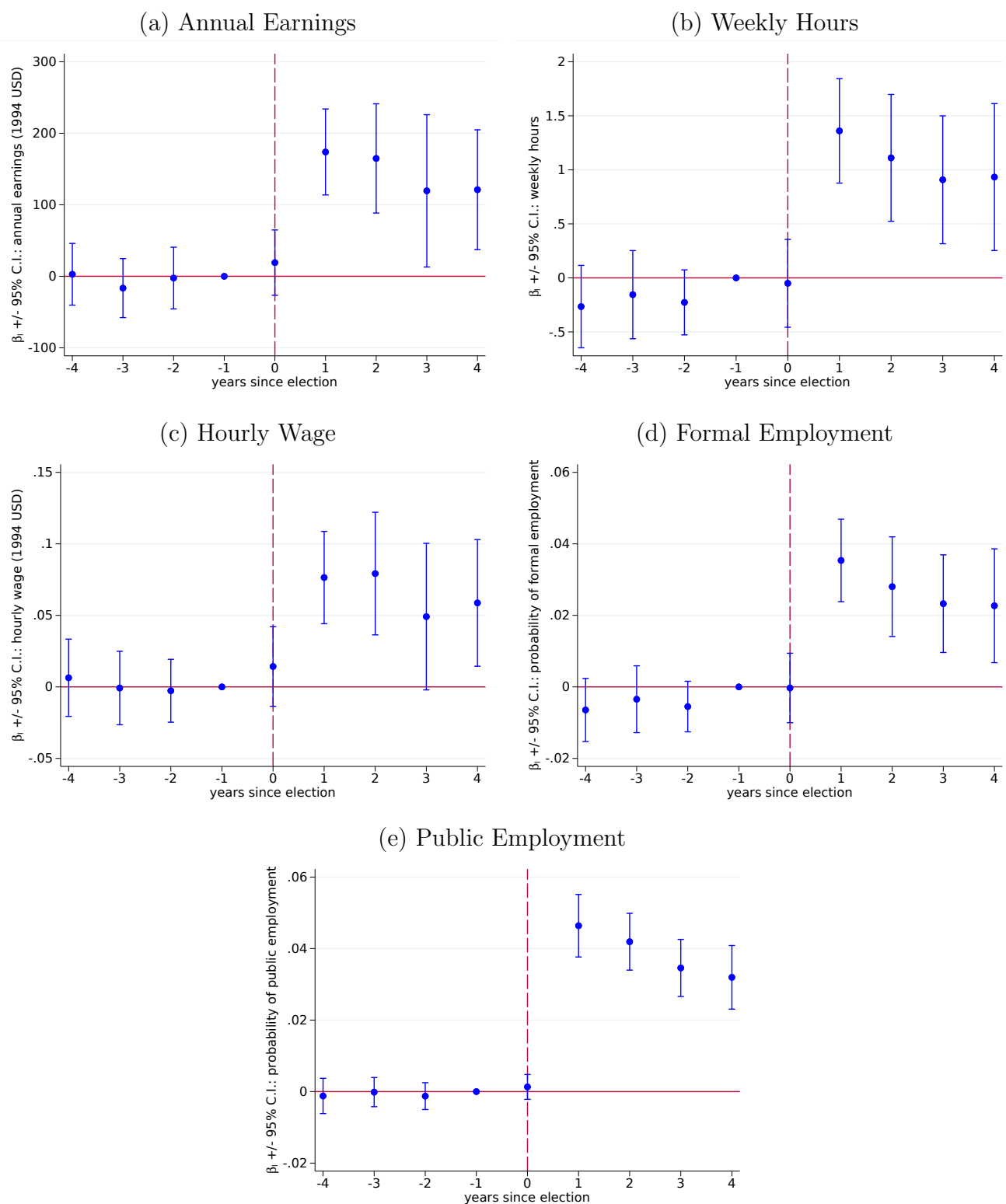
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 including all workers who were never formally employed over the study period (i.e., their CPF numbers were never observed in RAIS). The specification is otherwise identical to that in Figure 2.

Figure A.16: DID+RD: Sample that Excludes Donors

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment
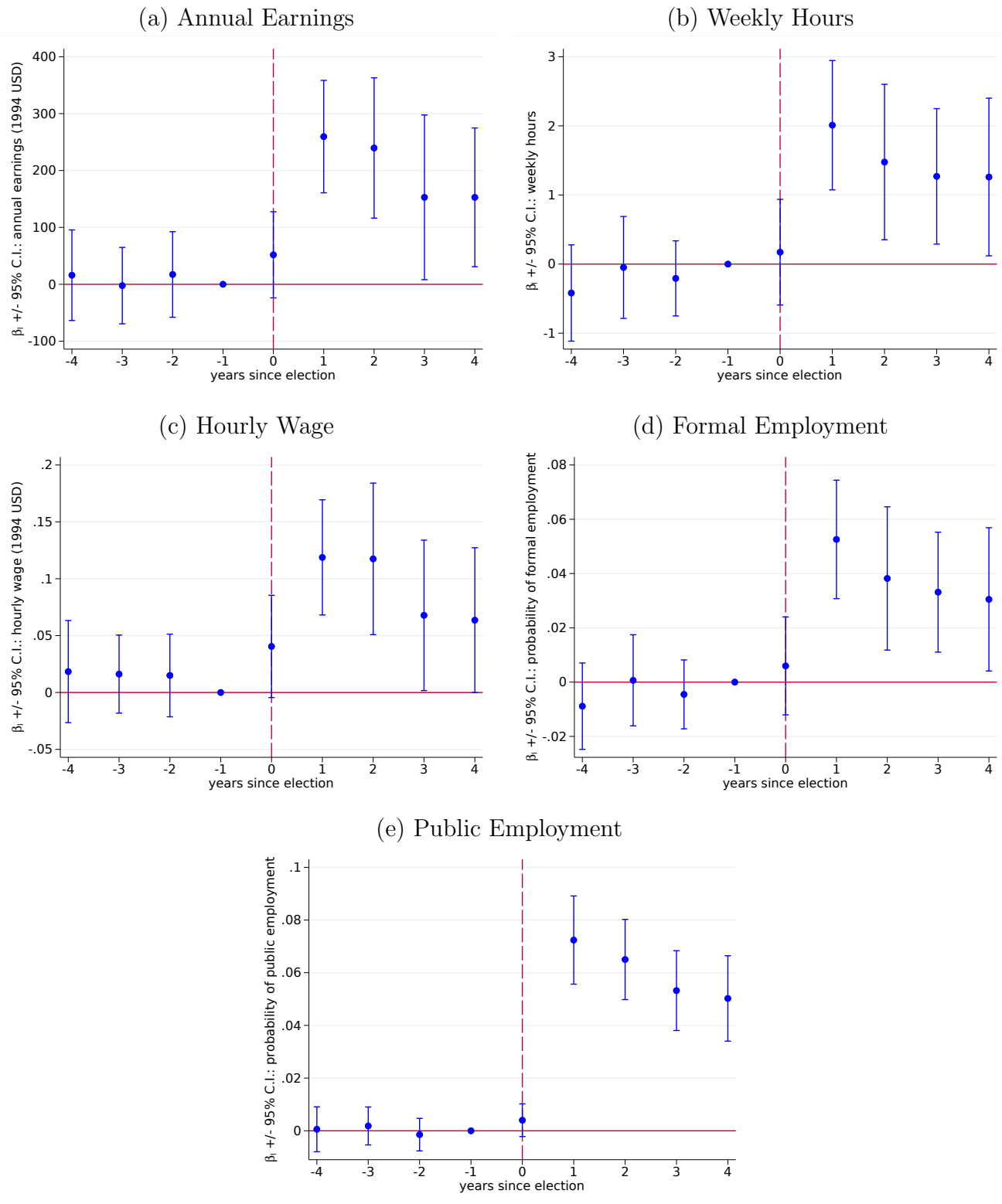
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 excluding all workers who donated to a political campaign in any of the years in our sample period. The specification is otherwise identical to that in Figure 2.

Figure A.17: DID+RD: Excluding 2012 Elections

(a) Annual Earnings

(b) Weekly Hours

(c) Hourly Wage

(d) Formal Employment

(e) Public Employment
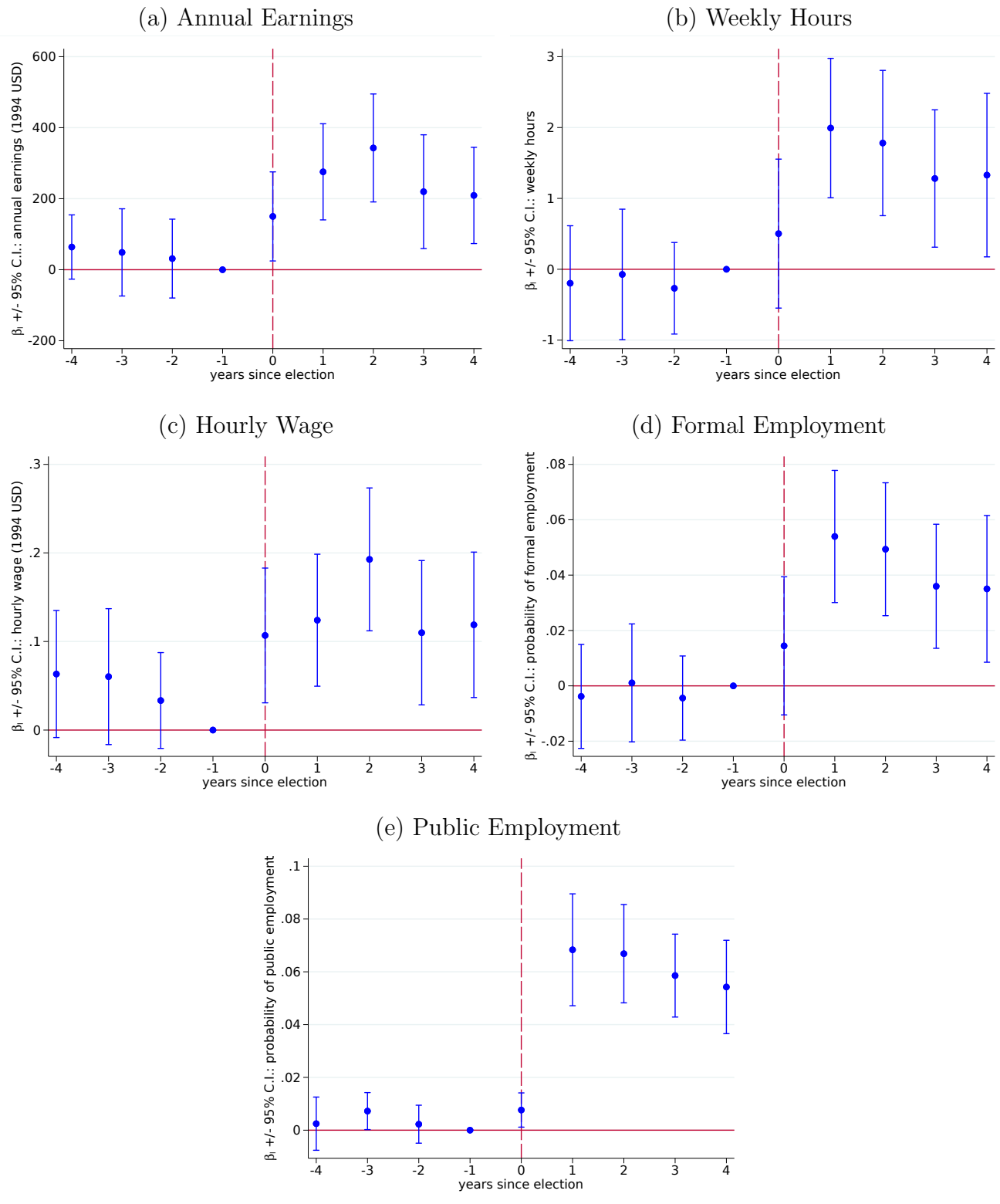
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 excluding workers in 2012 elections.

Figure A.18: Figure 3 Re-Estimated for Challenger Connections Only
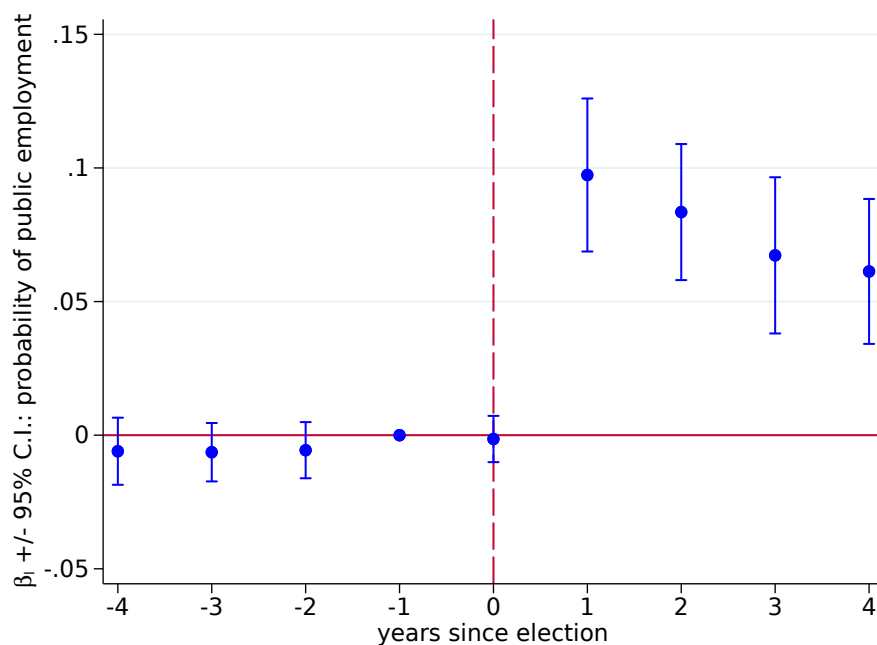
Notes: This figure re-estimate the specification in Figure 3 restricted to workers connected to candidates running in elections in which there are no incumbents running.

Figure A.19: Heterogenous Public Sector Employment Effects across Workers Connected to One-Term versus Two-Term Mayors
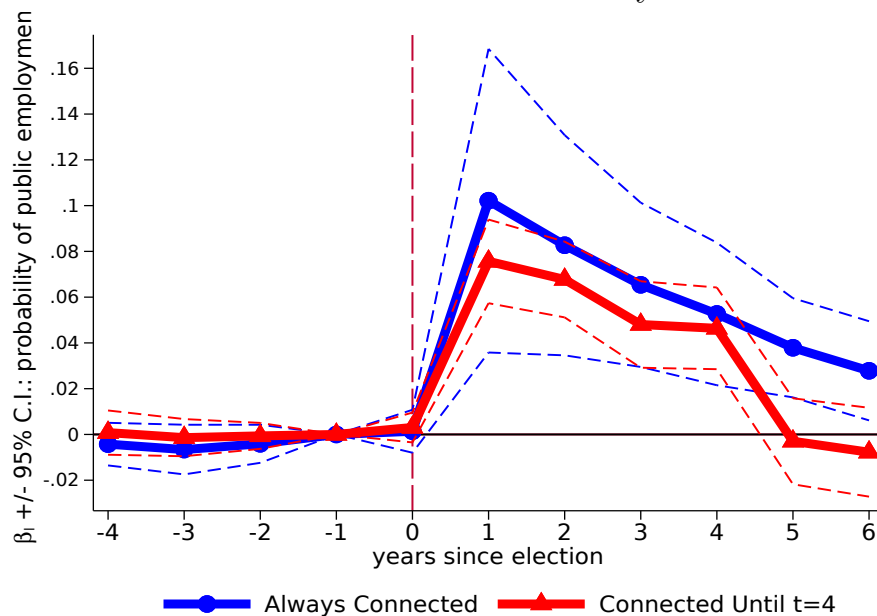
Notes: This figure re-estimates the specification in Figure 3 allowing for differential responses by workers connected to candidates who win a second election (*always connected*) versus those connected to candidates who lose the second election (*connected until* $t = 4$).

Figure A.20: Heterogeneous Effects by Amount Paid on the Campaign, with Additional Controls
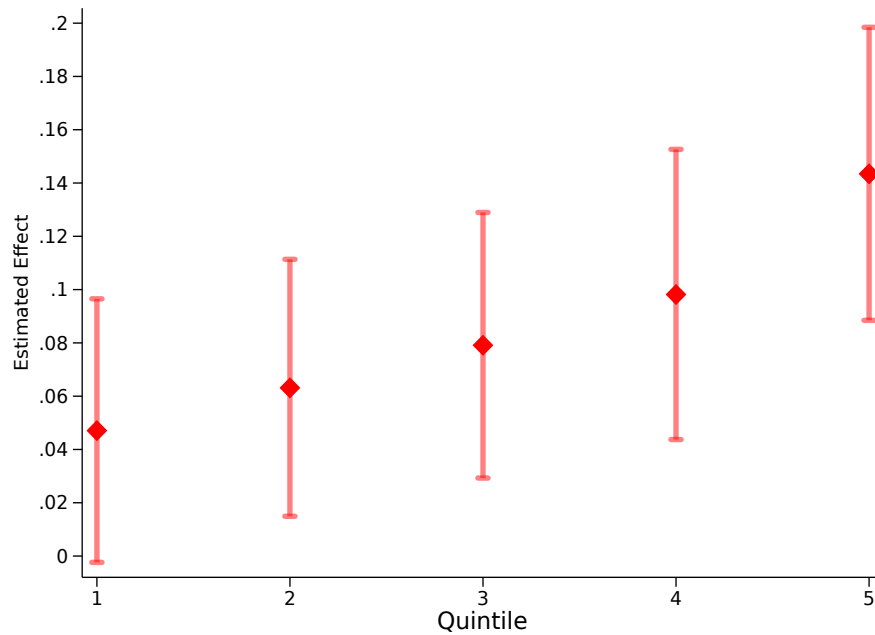
Notes: This figure re-estimates the specification in Figure 6 including a set of interactions between a dummy for being connected to a winning candidate and the following controls: average total earnings in the four years before the election, the share of years the supporter was employed in the public sector in the fours years prior to the election, an indicator for high school completion, an indicator for primary school completion, age and gender. The specification is otherwise identical to Figure 6.

Figure A.21: Earnings in the Public Sector
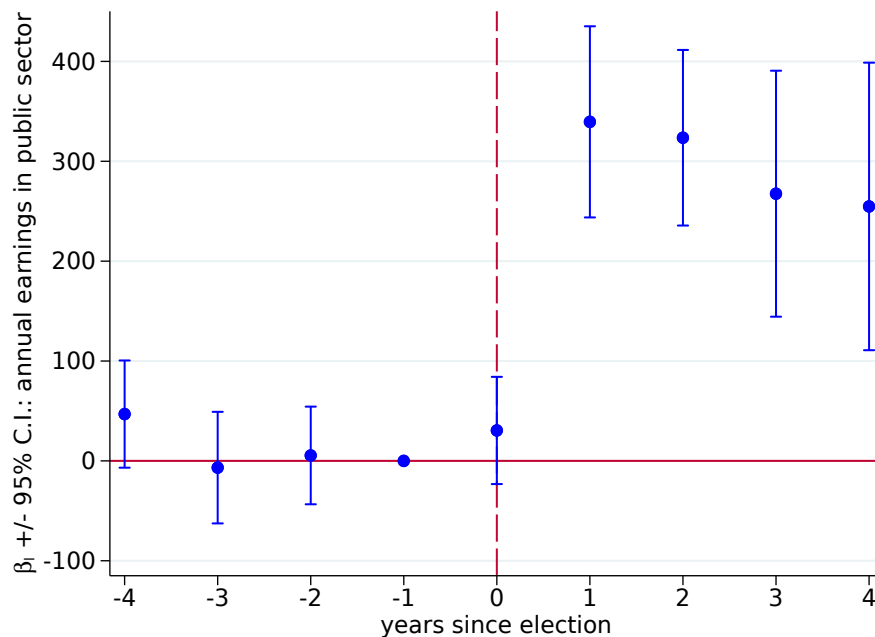
Notes: This figure re-estimate the specification in Figure 2 Panel a (iii) using earnings from the public sector (in 1994 USD) as the dependent variable. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure A.22: Campaign Workers' Pay in all Sectors Relative to Similar Jobs
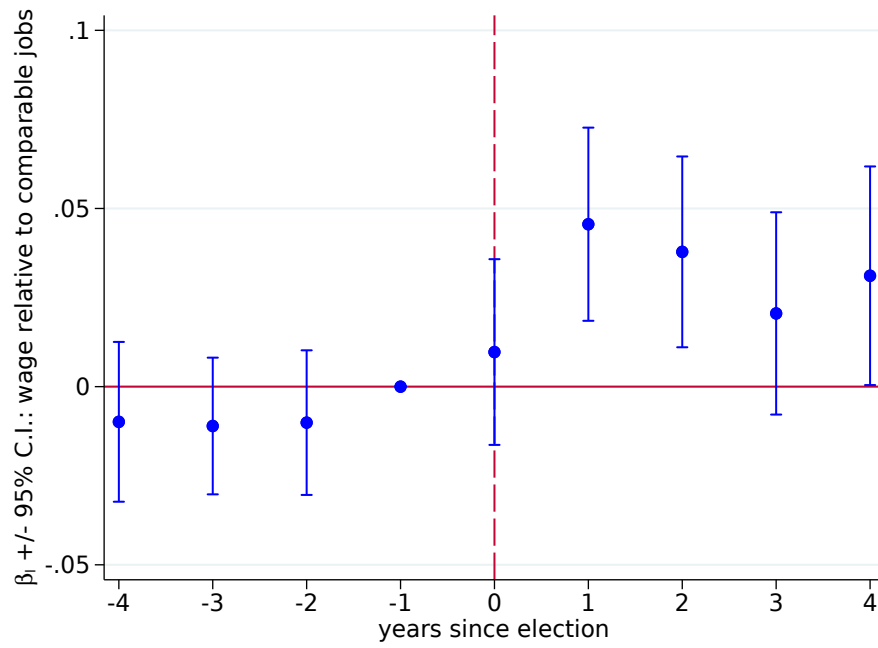
Notes: This figure re-estimates the specification in Figure 8 to include also workers who were not employed in the public sector for at least one year between time 1 and 4. The specification is otherwise identical to Figure 8.

Appendix Tables

Table A.1: Descriptive Statistics on Elections

	(1) Elections with dedicated staff	(2) Elections with contractors	(3) Elections with both labor types	(4) Population of elections
Population (1,000 ppl)	320.146	51.404	435.295	31.257
Share adults with secondary educ.	9.380	4.109	10.553	3.312
Share urban pop.	0.858	0.667	0.905	0.588
GDP per capita (1,000 Real)	3,376.259	407.661	4,862.099	215.357
Gini coefficient	0.560	0.562	0.561	0.554
Turnout	0.858	0.860	0.843	0.873
Candidate Education	7.196	6.642	7.304	6.335
Share Male Candidates	0.904	0.877	0.906	0.900
Mean candidate Ideology (1-extreme left, 10 extreme right)	4.568	5.216	4.504	5.187
Winner ideology (1-extreme left, 10 extreme right)	4.866	5.366	4.938	5.421
Margin of victory	0.171	0.166	0.191	0.158
Share of incumbent candidate	0.124	0.183	0.176	0.144
# Candidates	3.360	2.929	3.544	2.679
Observations	656	4,548	169	22,240

Notes: Statistics on electoral district are based on 2000 Census data. Candidate education in TSE data is classified as follows: (1) Illiterate; (2) Literate; (3) Incomplete Elementary School; (4) Complete Elementary School; (5) Incomplete High School; (6) Complete High School; (7) Incomplete Higher Education; (8) Complete Higher Education. The ideology index ranges from 1 to 10 with 1 being the extreme left and 10 the extreme right (see Power and Zucco, 2012 for details). In this table first and second rounds of elections are considered as different elections.

Table A.2: DID+RD Specification that Includes Unmatched CPF Numbers

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Earnings				
Post-election × Mayor	153.802***	167.812***	153.440***	156.216***	155.425***
	(29.828)	(30.028)	(29.822)	(35.463)	(36.710)
Mean D.V. post-election, runner-up	830.123	830.123	830.123	830.123	830.123
Observations	1,004,814	1,004,814	1,004,814	1,004,814	1,004,814
	Panel (b): Weekly Hours				
Post-election × Mayor	1.271***	1.481***	1.280***	1.213***	1.194***
	(0.209)	(0.203)	(0.210)	(0.225)	(0.226)
Mean D.V. post-election, runner-up	11.450	11.450	11.450	11.450	11.450
Observations	1,004,814	1,004,814	1,004,814	1,004,814	1,004,814
	Panel (c): Hourly Wage				
Post-election × Mayor	0.067***	0.073***	0.066***	0.074***	0.071***
	(0.015)	(0.015)	(0.015)	(0.018)	(0.020)
Mean D.V. post-election, runner-up	0.403	0.403	0.403	0.403	0.403
Observations	1,004,814	1,004,814	1,004,814	1,004,814	1,004,814
	Panel (d): Formal Employment				
Post-election × Mayor	0.032***	0.037***	0.032***	0.031***	0.030***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean D.V. post-election, runner-up	0.273	0.273	0.273	0.273	0.273
Observations	1,004,814	1,004,814	1,004,814	1,004,814	1,004,814
	Panel (e): Public Employment				
Post-election × Mayor	0.041***	0.041***	0.041***	0.042***	0.041***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Mean D.V. post-election, runner-up	0.046	0.046	0.046	0.046	0.046
Observations	1,004,814	1,004,814	1,004,814	1,004,814	1,004,814
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Margin×period FE	yes	yes	yes	yes	yes
Municipality×period FE	yes	no	yes	yes	yes
Municipality×election year×period FE	no	yes	no	no	no
Margin×period FE×Mayor	no	no	yes	no	no
Quadratic Margin×period FE×Mayor	no	no	no	yes	no
Cubic Margin×period FE×Mayor	no	no	no	no	yes

Notes: This table re-estimates Table 2 including all workers who were never formally employed over the study period (i.e. their CPF numbers were never observed in RAIS). See the notes below able 2 for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.3: DID+RD Specification that Excludes 2012 Elections

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Earnings				
Post-election × Mayor	205.135***	223.815***	200.963***	220.508***	222.953***
	(65.950)	(65.496)	(66.688)	(83.323)	(85.928)
Mean D.V. post-election runner-up	1429.7	1429.7	1429.7	1429.7	1429.7
Observations	355,871	355,871	355,871	355,871	355,871
	Panel (b): Weekly Hours				
Post-election × Mayor	1.648***	1.930***	1.669***	1.513***	1.531***
	(0.404)	(0.391)	(0.403)	(0.454)	(0.447)
Mean D.V. post-election runner-up	20.6	20.6	20.6	20.6	20.6
Observations	355,871	355,871	355,871	355,871	355,871
	Panel (c): Hourly Wage				
Post-election × Mayor	0.084**	0.093***	0.080**	0.107**	0.105**
	(0.033)	(0.032)	(0.033)	(0.042)	(0.045)
Mean D.V. post-election runner-up	0.697	0.697	0.697	0.697	0.697
Observations	355,871	355,871	355,871	355,871	355,871
	Panel (d): Formal Employment				
Post-election × Mayor	0.043***	0.050***	0.044***	0.041***	0.041***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
Mean D.V. post-election runner-up	0.491	0.491	0.491	0.491	0.491
Observations	355,871	355,871	355,871	355,871	355,871
	Panel (e): Public Employment				
Post-election × Mayor	0.058***	0.058***	0.057***	0.058***	0.059***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Mean D.V. post-election runner-up	0.081	0.081	0.081	0.081	0.081
Observations	355,871	355,871	355,871	355,871	355,871
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Margin×period FE	yes	yes	yes	yes	yes
Municipality×period FE	yes	no	yes	yes	yes
Municipality×election year×period FE	no	yes	no	no	no
Margin×period FE×Mayor	no	no	yes	no	no
Quadratic Margin×period FE×Mayor	no	no	no	yes	no
Cubic Margin×period FE×Mayor	no	no	no	no	yes

Notes: This table re-estimates Table 2 excluding workers in 2012 elections. See the notes below able 2 for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: Breakdown of the Effects on Public Sector Employment by Occupation

	(1) Professional or Managerial	(2) Technical or Supervisory	(3) Other White Collars	(4) Blue Collars
Post-election × Mayor	0.014***	0.036***	0.007***	0.012***
	(0.003)	(0.004)	(0.002)	(0.003)
Mean D.V. post-election, runner-up	0.023	0.040	0.009	0.020
Observations	559,221	559,221	559,221	559,221

Notes: This table present a breakdown of the effects on public sector employment by occupation. We group workers in the public sector in four categories of occupations as described in Appendix B.3. Thus we consider the following outcome variables: probability of working in the public sector in professional or managerial occupations (column 1), probability of working in the public sector in technical or supervisory occupations (column 2), probability of working in the public sector in other white collar occupations (column 3), probability of working in the public sector in blue collar occupations (column 4). For each outcome variable, the table shows the coefficient estimated from the interaction between a dummy for being connected to a winning candidate and a post-election dummy that takes value 1 in periods (years relative to the election) 1 to 4. This specification otherwise follows the baseline from column 1 of Table 2 and includes the following controls: the interaction between post-election dummy and a dummy for being connected to the winning candidate (which is labelled as "Mayor" in the table), individual fixed effects, year fixed effects, the interactions between period (year relative to the election) and municipality fixed effects, and the interactions between margin of victory and period fixed effects. We impute a zero probability of employment for workers who are not observed in RAIS in a given year; we restrict to workers who are linked to elected or runner-up mayoral candidates in close election (i.e. 5% victory margin bandwidth); we estimate effects in the 8-years window around the election. Standard errors that are clustered at the municipality level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Positive Selection in Public Sector Jobs by Occupation

	(1) Private earnings 4 years before the election	(2) Private earnings 2 years before the election
Panel (a): Professional or Managerial Occupations		
Mayor × Tercile 3	0.002	0.001
	(0.002)	(0.002)
Mayor × Tercile 2	0.000	0.001
	(0.002)	(0.002)
Mayor × Tercile 1	-0.003*	-0.003*
	(0.002)	(0.002)
Mayor	0.012***	0.012***
	(0.004)	(0.004)
Mean D.V. post-election, runner-up	0.020	0.020
Observations	485,328	485,328
Panel (b): Technical or Supervisory Occupations		
Mayor × Tercile 3	0.012***	0.007**
	(0.003)	(0.003)
Mayor × Tercile 2	0.008**	0.002
	(0.003)	(0.003)
Mayor × Tercile 1	-0.003	-0.004
	(0.003)	(0.003)
Mayor	0.027***	0.028***
	(0.009)	(0.008)
Mean D.V. post-election, runner-up	0.037	0.037
Observations	485,328	485,328
Panel (c): Other White Collar Occupations		
Mayor × Tercile 3	0.003**	0.005***
	(0.001)	(0.001)
Mayor × Tercile 2	0.004***	0.003**
	(0.001)	(0.001)
Mayor × Tercile 1	0.003**	0.001
	(0.002)	(0.001)
Mayor	0.008***	0.008***
	(0.002)	(0.002)
Mean D.V. post-election, runner-up	0.008	0.008
Observations	485,328	485,328
Panel (d): Blue Collar Occupations		
Mayor × Tercile 3	0.003	0.000
	(0.002)	(0.002)
Mayor × Tercile 2	0.003*	0.003
	(0.002)	(0.002)
Mayor × Tercile 1	0.002	-0.000
	(0.002)	(0.002)
Mayor	0.009*	0.010**
	(0.005)	(0.005)
Mean D.V. post-election, runner-up	0.018	0.018
Observations	485,328	485,328

Notes: This table re-estimates Table 4 distinguishing between workers in four occupation groups (see also Section B.3 for details on the occupation classification). We consider workers in professional or managerial occupations in panel a; technical or supervisory occupations in panel b; other white collar occupations in panel c; blue collar occupations in panel d. The specification is otherwise identical to Table 4; see the notes therein for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Positive Selection into the Public Sector Controlling for Earnings on the Campaign

	(1) Private earnings 4 years before the election	(2) Private earnings 2 years before the election
Mayor × Tercile 3	0.019***	0.013***
	(0.004)	(0.004)
Mayor × Tercile 2	0.016***	0.009**
	(0.004)	(0.004)
Mayor × Tercile 1	0.003	-0.003
	(0.004)	(0.004)
Mayor	0.048***	0.050***
	(0.012)	(0.012)
Campaign earnings × mayor × period fixed effects	yes	yes
Mean D.V. post-election runner-up	0.085	0.085
Observations	485,321	485,321

Notes: This table re-estimates Table 4 including controls for earnings received on the campaign interacted with a dummy for working in a winning campaign (mayor) and period (years relative to the election) fixed effects. The specification is otherwise identical to Table 4; see the notes therein for details. Significance levels: $*: 10\%$ $**: 5\%$ $***: 1\%$.

Further Probing of the Manipulation Test

In our setting, the implementation of standard RD manipulation tests is challenging for multiple reasons. First, differently from the standard RD specification, our core regression conditions on individual, year and municipality times period fixed effects. Second, we cluster the standard errors at the municipality level. Unfortunately, standard routines to implement manipulation tests cannot easily accommodate fixed effects or clustering (McCrary, 2008; Cattaneo et al., 2020).

In order to test whether the density of workers is smooth around the cutoff we proceed as follows. First, for each campaign worker and year, we determine the number of workers employed in the campaign to which she/he participates. We imputed zeros in years in which a campaign employs no worker. Then, we run a regression of the number of workers employed in each campaign and year on individual, year and municipality times period fixed effects. Thus we use the estimated residuals from this regression as the dependent variable in a (RD) regression having on the right-hand side an indicator variable for being connected to a winning candidate and flexible controls for the margin of victory (i.e., the running variable). We cluster the standard errors at the municipality level. We base this regression on a sample that includes only the election year and the years prior to the election. We estimate this RD regression based on two alternative specifications. First, we restrict the analysis to elections within a 5% margin of victory, and we control for linear splines of the running variable on both sides of the cutoff. The results of this specification are presented in Appendix Figure A.5. Second, we base the estimation on the Calonico et al. (2017) `rdrobust` procedure that selects the bandwidth using a data-driven procedure, and that includes local polynomial functions of the running variable as controls. The results of this specification are presented in Table A.7 below. Across specifications, we do not find a significant discontinuity in the number of campaign workers at the cutoff. This suggests that manipulation is unlikely to be a concern in our setting.

Table A.7: Manipulation Test

Dep. Var.: Residual number of campaign workers	
RD effect	-21.27
	(24.232)
Cluster-robust z-score for H_0:RD effect=0	-0.6239
Cluster-robust p-value for H_0:RD effect=0	0.533
Bandwidth	5.246
Observations	6389790

Note: The table shows the RD effects obtained from a regression of the residual number of campaign workers on an indicator variable for being connected to a winning candidate. We estimate this regression based on the estimator proposed by Calonico et al. (2017) controlling for local linear functions of the running variable and using local quadratic regressions for the bias correction. The bandwidth is determined based on Calonico et al. (2017). Standard errors in parenthesis are clustered at the municipality level.

B Data Construction

B.1 RAIS linked employer-employee data

Brazilian law requires every establishment in the country to submit detailed annual reports with individual information on its employees to the Ministry of Labor (*Ministério de Trabalho*, MTE). The collection of the reports is called *Relação Anual de Informações Sociais*, or RAIS. By design, RAIS covers all formally employed workers in any sector and tracks workers nationwide over time as they transition between formal jobs. MTE estimates today that 97 percent of all formally employed workers in Brazil are covered in RAIS, and that coverage exceeded 90 percent throughout the 1990s.

RAIS contains job-spell-level information on workers' characteristics, such as earnings, contractual weekly hours and education, as well as establishment' characteristics, such as legal status and industry. As concerns earnings, RAIS reports, among other measures, average monthly wages in multiples of the minimum wage. In order to obtain annual earnings we multiply the average monthly wage by the December U.S. dollar equivalent of the minimum wage times 12. We deflate this earning measure to August 1994 using the U.S. consumer price index (from Global Financial Data). We select August 1994 for deflation because in that month U.S. dollars and Brazilian reals were exchanged at parity. Hourly wages are obtained as the ratio of annual earnings divided by the product of contractual weekly hours multiplied by 52.14 (the number of weeks in a year). For workers with multiple employment spells during a calendar year, we keep the worker's last recorded job spell and, if there are multiple spells spanning into the final month of the year, the highest-paid job spell (randomly dropping ties). Workers with no formal employment in a year are not in RAIS. In order to obtain a balanced panel, we assign zero earnings, contractual hours and employment to missing years for workers who appear in RAIS at least once in our sample period. As in Colonnelli et al. (2020), we classify a worker as employed in the public sector if she/he is employed by a plant reporting a legal status (*Natureza Jurídica*) of "Public Administration" (*Administração Pública*).

We select dedicated staff of political organizations based on the legal status and industry of their employers. Specifically, we select workers in establishments associated with a CNAE (1.0 or 2.0) industry classification code of 91928 (*Atividades de organizacoes politicas*), or legal status classification codes 3123, 3255, 3263, 3271, 3280 (*Partido Político*) or 4090 (*Candidato a Cargo Político Eletivo*). Since political parties and campaigns enter the legal status classification only in 2002, the selection of dedicated staff in years prior to 2002 is based on *CNAE* only. However, 62% of the political campaigns and 73% of the political parties in our final sample are classified as political organizations in CNAE between 2002 and 2014, suggesting that CNAE likely captures most of the workers in political organizations prior to 2002. We exclude from the analysis dedicated staff who also work for a campaign on a contractual basis (0.18% of the observations), and we start the analysis in 1994 when the CNAE industry classification is introduced in RAIS allowing us to track workers in political organizations.

In Table 1 we assign workers to education groups based on RAIS categories as follows: RAIS categories 1 to 5 are classified as Primary Education; RAIS categories 6 and 7 as Secondary Education, RAIS category 8 is College Education; and RAIS category 9 is Post-Graduate Education.

B.2   Matching Dedicated Staff to Election Data

In order to match dedicated staff of political organization to election data we use the Tax Register of Brazilian Firms from the Brazilian Federal Revenue Service Agency (*Receita Federal do Brasil*). In any given year, this register provides information on starting, ending date (if any), ownership and type of activity for the universe of businesses created up to that year. Important for this study, the register provides a match of each employer tax-identifier (CNPJ) to the business name (*Nome Empresarial*) which, in the case of a political organization, often contains the name of the political party or candidate associated to a CNPJ.[1] We match business names from the tax register to RAIS based on CNPJ. Thus we use the resulting dataset to match labor market outcomes of dedicated staff to election data in TSE, following a procedure which differs for workers in political campaigns versus political parties. In what follows we describe this procedure.

Procedure to match campaign workers to election data.   The matching of dedicated staff of political campaigns from RAIS to political candidates in TSE data consists of the following steps:

1. We merge the two datasets using candidate name and year, where year in RAIS is the year(s) in which a political worker is employed by the campaign in RAIS, and year in TSE data refers to the year of an election. Candidate name in RAIS is obtained from the business name (*Nome Empresarial*) that is reported in the tax register of Brazilian firms.

2. For the observations that are not matched in the previous step, we replace the year from RAIS with the year as indicated in the business name (*Nome Empresarial*). This is available only for a limited number of observations. Then we merge with TSE data based on candidate name and year.

3. For the observations that are not matched in the previous steps, we use candidate name and political position, where political position is extracted, when available, using the business name contained in the tax register of Brazilian firms.

4. For the remaining observations we merge based on candidate name only.

All candidate names that could be matched through this procedure appear only once in a mayoral election and election year. More than 90% of observations that find a match are merged in step 1.

Procedure to match political party workers to election data.   The matching of dedicated staff of political parties in RAIS to political candidates in TSE data consists of the following steps:

1. We merge the two datasets based on year, party name, municipality and federal level. The variable year in RAIS refers to the year in which a worker is employed in the political party in RAIS, while year in TSE data refers to the year of an election. The variable federal level indicates whether a party branch in RAIS is federal or local (based on information available from the tax register in 2014).

2. For observations that are not merged in step 1 we merge based on year, party name, and municipality.

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[1]We use the 2016 version of this register, which includes firms registered until that year.

3. For observations that are not merged in the previous steps we augment the year in RAIS by i and merge based on year ($+i$ in RAIS), party name, municipality and federal level

4. For observations that are not merged in the previous steps, we augment the year in RAIS by i and merge based on year ($+i$ in RAIS), party name and municipality
   We repeated the prior steps (together) for each $i = 1,2,3,4$ until we find a match.

5. For observations that are not merged in the previous, we repeat steps 1 to 4 using coalition name rather than party name. This is done because parties may not have their own candidate in an election but they support the candidate of a coalition.

Unfortunately, not all business names in the tax register contain useful information for the matching. This results in the loss of some organizations and workers among those selected in RAIS. Appendix Table B.1 compares characteristics of workers in our final sample (columns 1 and 2), to the population of formal workers in political organizations (column 3 and 4) and to the overall population of formal workers (column 5 and 6). Relative to the overall population of workers, workers in our final sample are more likely to be females, younger, educated, and, despite working slightly more hours on average, they earn less than the average worker (see column 7). A comparison of columns 3 and 5 indicates, however, that most of these facts hold if we compare the population of workers in political organization to the overall population of workers. This suggests that our sample, while allowing us to link workers to electoral outcomes, does not substantially distort the composition of the population of formal workers in political organizations.[2]

Table B.1: Descriptive statistics on workers in political organizations

	Workers linked to a candidate (1994-2014)		Population of Workers in political organizations (1994-2014)		Population of workers (1994-2014)		Difference (1 vs 5)	Difference (3 vs 5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mean	Std. Dev	Mean	Std. Dev.	Mean	Std. Dev.	T-stat	T-stat
Annual earnings (USD)	4946.216	6485.930	3546.051	8422.452	7503.431	11960.371	-29.38	-110.00
Contractual hours	41.668	6.823	41.722	5.916	41.417	5.898	2.74	12.05
Age	32.655	10.983	36.276	11.726	34.356	11.050	-11.45	38.15
Male	0.379	0.485	0.440	0.496	0.608	0.488	-35.16	-79.22
Primary Education	0.256	0.436	0.282	0.450	0.421	0.494	-28.18	-72.26
Secondary Education	0.560	0.496	0.493	0.500	0.424	0.494	20.44	32.32
College Education	0.069	0.253	0.071	0.257	0.037	0.188	9.49	31.16
Post-Graduate Education	0.112	0.316	0.151	0.358	0.117	0.322	-1.29	21.93
Observations	5,554		54,819.00		867,054,239.00			

Note: The table shows descriptive statistics for workers in our final sample (columns 1 and 2), the population of formal workers in political organizations (column 3 and 4) and the overall population of formal workers (column 5 and 6). In column 7 and 8 we show t-statistics of the difference between average values in column 1 and 5 and column 3 and 5, respectively. These t-statistics are obtained as the ratio of the difference between mean values and the square root of the sum of the standard deviations divided by the number of observations: T-stat i vs j= $\frac{Mean(X_i) - Mean(X_j)}{\sqrt{\frac{Std.Dev.(X_i)^2}{N_i} + \frac{Std.Dev.(X_j)^2}{N_j}}}$.

Finally, we note that the matching procedure links workers in a political party to elections that take place up to 4 years after the employment in the political organization. This is done with the idea that a political connection may affect a worker's career even after being employed in the organization. The majority of workers in our sample (55%), however, are employed in the organization at the time of the election, and more than 75% of all workers are linked to an election that is held no later than one year after the worker left the organization.

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[2]One noticeable exception to this is workers' age, which is higher in the overall population of political workers than the population average.

B.3 Education and occupation categories in Rais

We group education information from nine RAIS education categories into four categories as shown in Table B.2.

Table B.2: Education Categories

	RAIS category	Education Level
1.	9.	Post-Graduate Education
2.	8.	College Education
3.	6.-7.	Secondary Education
4.	1.-5.	Primary Education

Occupation indicators derive from the 3-digit CBO classification codes in our nationwide RAIS data and are reclassified to conform to ISCO-88.[3] We map RAIS occupations into ISCO-88 occupations and regroup them into four categories as shown in Table B.3.

Table B.3: Occupation Categories

	ISCO-88 occupation category	Occupation Level
1.	Legislators, senior officials, and managers	Professional or Managerial
	Professionals	Professional or Managerial
2.	Technicians and associate professionals	Technical or Supervisory
3.	Clerks	Other White Collar
	Service workers and sales workers	Other White Collar
4.	Skilled agricultural and fishery workers	Blue Collar
	Craft and related workers	Blue Collar
	Plant and machine operators and assemblers	Blue Collar
5.	Elementary occupations	Blue Collar

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[3]See the online documentation at *econ.ucsd.edu/muendler/brazil.*