September 15, 2026

Dear Workshop Organizers,

My name is Francisco Tuñón, and I am a fourth year PhD student in Applied Economics and Management at Cornell University. I am writing to apply to the NBER Workshop on Development Economics and Networks for Early-Career Researchers.

I am interested in Political Economy and Development, with a focus on the determinants of local political entry and political participation in settings with low levels of trust in politicians and high potential returns to effective local governance. I am particularly interested in how networks shape political participation through recruitment, information sharing, and social norms. In my current working paper, I study how workplace networks affect political entry and find that exposure to a coworker who enters politics significantly increases an individual's likelihood of subsequently running for office.

Please find attached my application materials, including my curriculum vitae, brief research statement, and working paper.

If shortlisted, Professor Brian Dillon (bmd28@cornell.edu) will be available to provide a recommendation.

Thank you for considering my application.

Sincerely,

Francisco Tuñón

PhD Student in Applied Economics and Management

Cornell University

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Francisco Tuñón

Personal Information

Email: ft244@cornell.edu, Phone: +1 (313) 349-8763
Nationality: Argentinian, Italian.

Education

PhD in Applied Economics and Management, Cornell University 2023 -

MA in Economics, Universidad de San Andrés 2019 - 2021
GPA: 8.6/10.
Thesis: "Corruption in Peru: Evidence from Nepotism in Congress." [PDF]

BA in Economics, Universidad de Buenos Aires 2013 - 2018
GPA: 8.42/10. *Magna Cum Laude*.

Research

Work in Progress

. "Peer Effects in Political Entry: Evidence from Brazilian Workplaces".
. "Migration Effects on Sending Local Governments: Evidence from Mexico-US Migration", with Shubhalakshmi Nag.
. "Experiencing The State: Interactions with Frontline Workers and Political Behavior in South Africa", with Edoardo Bollatti.

PhD Coursework

Spring 2026

. Economics and Politics, ECON 7385 (A).

Fall 2025

. Text and Networks, GOVT 6619 (A).
. Econometrics of Network Analysis, ECON 7260 (A).

Spring 2025

. Microeconomics of International Development II, AEM 7621 (A+).

Fall 2024

. Empirical Strategies for Policy Analysis, ECON 6590 (A).
. Macroeconomics I, ECON 6130 (A).
. Microeconomics of International Development, AEM 7620 (A).
. Topics in Public Finance and Behavioral Economics, PUBPOL 6971 (A).

Spring 2024

. Econometrics II, ECON 6200 (A+).
. Game Theory, AEM 7020 (A).
. General Equilibrium Analysis, AEM 7021 (A).

Fall 2023

. Applied Microeconomics, AEM 7010 (A).
. Econometrics I, ECON 6190 (B+).
. Intermediate Mathematical Economics I, ECON 6170 (A-).
. Microeconomic Theory I, ECON 6090 (A).

PhD Committee

Professor Brian Dillon, Cornell University (Chair).
Professor Nicolas Bottan, Cornell University.
Professor Sarah F. Thompson, Cornell University.
Professor Milena Djourelova, University of Southern California (External).

Teaching Experience

TA for Applied Econometrics II (G), Cornell — Spring 2026
Instructor: Brian Dillon.

TA for International Trade Policy (UG), Cornell — Spring 2025
Instructor: Nancy Chau.

TA for Financial Markets and Sustainability (UG&G), Cornell — Fall 2024 & 2025
Instructors: John Tobin de la Puente and Sudip Thakor.

TA for Development Economics (UG), UdeSA — 2020 - 2022
Instructor: Mariano Tommasi.

TA for Introductory Economics (UG), UdeSA — 2020
Instructor: Juan Carlos de Pablo.

Other Work Experience

Research Consultant, Development Research Group, The World Bank — 2021 - 2023
RA to Alejandro Molnar and Forhad Shilpi for projects on urban economics, transportation and gender discrimination in Bangladesh, India and Rwanda.

Research Assistant to Prof. Mariano Tommasi (UdeSA) — 2020
RA for a project on the evolution of sociodemographic trends in Latin America.

Research Assistant to Anna Callis (PhD Candidate, UC Berkeley) — 2019 - 2021
RA for a project on the landed elite and democratization in Argentina during the early 20th Century.

Analyst at the Argentine Ministry of Production and Labor — 2018 - 2019

Languages

English (fluent), Spanish (native), Portuguese (intermediate).

Programming

R, Stata, Python, QGIS, MATLAB, LaTeX.

NBER Workshop on Development Economics and Networks for Early-Career Researchers

Research Statement

September 15, 2026

I am a fourth-year PhD student in Applied Economics at Cornell University. I work in Political Economy and Development, with a focus on the determinants of political entry and political participation in local politics in developing democracies.

I am particularly interested in how characteristics common across developing countries, such as low state capacity and low trust in politicians, interact with political participation at the local level. This matters because development outcomes depend heavily on the leaders who make decisions on behalf of a community. I believe networks are central to understanding this: political behavior is inherently social, and social ties shape participation decisions through recruitment, information sharing, and social norms.

In the working paper I am submitting for the workshop, I test this idea in Brazil, showing that the former coworkers of closely elected local city council candidates are more than 50% more likely to run for office in the next election than the former coworkers of narrowly defeated candidates. Preliminary evidence is consistent with a role for political recruitment, particularly among male candidates and their female coworkers.

In other work in progress, I study the political economy effects of migration shocks on sending communities, asking how migration affects local governance in Mexican municipalities. I am also investigating how health interventions in disadvantaged urban areas of South Africa affect beliefs about the state and reduce political protest. Neither project currently incorporates a network framework explicitly, but both involve processes that are inherently social. Migration operates through networks connecting sending communities and migrants abroad, while beliefs about the state and responses to government interventions can diffuse through social ties. I would like to develop these projects further by incorporating network-based approaches.

My research interests therefore align closely with the workshop's focus. I would particularly value feedback on my current research and the opportunity to learn from faculty and other early-career researchers working on related questions. I have taken PhD coursework in networks and found these methods to be particularly useful for studying questions of political participation and social interactions. I see this workshop as an opportunity to deepen my methodological training in networks while connecting these tools to my broader research agenda in Development and Political Economy.

Francisco Tuñón

PhD Student in Applied Economics and Management – Cornell University

Peer Effects in Political Entry: Evidence from Brazilian Workplaces

Francisco Tuñón
Cornell University

September 2026

Preliminary draft. Please do not circulate or cite.

Abstract

How do social networks shape who enters politics? This paper studies the effect of workplace networks on political entry using administrative data covering the universe of formal workplaces in Brazil and a quasi-experimental design based on close municipal elections. I compare individuals who previously worked with a candidate who narrowly won a city council election to those who worked with a narrowly defeated candidate. Individuals exposed to a narrowly elected coworker are more than 50% more likely to run for office in the subsequent election. The effect is larger among coworkers with more intense and lasting workplace interactions, consistent with workplace relationships playing an important role in political entry. I also find suggestive evidence that political recruitment is an important mechanism, with larger effects among female coworkers of male politicians. These findings show that workplace networks can shape political participation by facilitating access to information, political opportunities, and recruitment, and highlight social networks as an important determinant of political selection.

1 Introduction

Local politicians are responsible for key decisions related to education, health, infrastructure, and other public services. The quality of these decisions affects the provision of public goods and local development outcomes (Chattopadhyay and Duflo, 2004; Fujiwara, 2015). In local elections, voters can only choose among those who decide to run. Understanding the determinants of political entry, therefore, is central to understanding how democracy works, how different groups are represented, and how that affects public service provision. A large literature on political entry has found that individual incentives and institutional rules are important factors in the decision to run for office.[1] Much less attention has been paid to how social networks influence the decision to enter politics.

One particularly important source of social ties for adults is the workplace. Coworkers can affect important economic outcomes such as productivity and wages within a firm (Cornelissen *et. al.*, 2017; Mas and Moretti, 2009). These relationships can also matter after coworkers are no longer working together: former coworkers draw on information accumulated during their working relationship to provide referrals, influencing one another's future employment (Dustmann *et. al.*, 2016; Glitz, 2017). I argue that workplace ties can similarly affect the decision to enter politics by changing the perceived costs and benefits of political entry and by providing access to information and opportunities.

This paper leverages administrative data covering the universe of formal workplaces in Brazil and a quasi-experimental design based on close elections to study the causal effect of exposure to a coworker who is elected to the city council for the first time. The main question is whether exposure to a coworker who runs for and narrowly wins a city council seat increases an individual's likelihood of becoming a candidate in the future.

I consider several mechanisms that could generate a link between candidates and their coworkers' political entry decisions. From the demand side, newly elected politicians have already observed the professional and social skills of their former coworkers and may therefore have information about who could be effective in political roles. From the coworker's side, exposure to someone who enters politics may provide a role model, shape perceptions of political participation, reduce informational barriers to entry, and provide access to recruitment opportunities and campaign resources that would otherwise be difficult to obtain. However, these effects need not always encourage political entry, as personal connections may also provide access to political favors without the costs of holding office, which could reduce an individual's own incentive to run. I use heterogeneity analysis to explore these potential channels of coworker influence on the decision to enter politics.

[1] See Dal Bó and Finan (2018) and Gulzar (2021) for an overview of this topic.

My analysis leads to one central finding: individuals who previously worked with a coworker who was narrowly elected to the city council are more than 50% more likely to run for office in the subsequent election than individuals who previously worked with a narrowly defeated coworker.

I also investigate the mechanisms behind this effect, examining whether it is driven by changes in social norms, role models, knowledge transfer, or political recruitment. Using heterogeneity analysis, I find suggestive evidence that the effect is stronger among coworkers with more intense and durable workplace interactions. Preliminary evidence is consistent with a role for political recruitment, with the largest effects concentrated among female coworkers of male politicians, consistent with Brazil's gender candidacy quotas and the under-supply of female candidates. Finally, I study whether exposure affects lower-cost forms of political engagement, such as party affiliation and campaign contributions and find no effects.

This paper contributes to two strands of the literature. First, within the large literature on political entry, it relates to research on its social determinants, which shows how family ties affect both entry into politics and political performance. Examples include political dynasties in the United States (Dal Bó *et. al.*, 2009) and the role of family centrality in the Philippines (Cruz *et. al.*, 2017). Much less is known about other forms of personal connections, such as friendships and workplace relationships, despite their potential importance as determinants of political entry. A notable exception is Aggeborn and Andersson (2025), who show that exposure to politically active coworkers in Sweden is associated with an increase in the likelihood of running for office of about 12%. Their setting is a high-trust environment, where politicians tend to be positively selected on competence and are relatively representative of society (Dal Bó *et. al.*, 2017). This paper studies a different institutional and political environment and provides causal evidence on the effect of workplace exposure.

Second, this study relates to research showing the effects of workplace networks beyond the workplace. On one hand, coworkers shape economically consequential outcomes over the course of a career, from productivity and wages to future employment opportunities (Cornelissen *et. al.*, 2017; Dustmann *et. al.*, 2016; Glitz, 2017; Mas and Moretti, 2009). On the other hand, workplace relationships and political behavior are linked in several ways. Workplaces are increasingly organized along political lines: in the United States, Democratic and Republican workers are about 10% more likely to have copartisan coworkers than would be expected based on local partisan composition (Chinoy and Koenen, 2024), and in Brazil, a worker affiliated with a political party is 50% more likely to be employed by a copartisan business owner than one affiliated to a different party (Colonnelli *et. al.*, 2025). In Brazil, political connections have also been shown to increase the likelihood of obtaining a public sector job (Colonnelli *et. al.*, 2020), and job loss has been shown to increase political entry by about 12% (Barros and Schmeißer, 2024). Together, these studies show that workplace and political outcomes are closely intertwined, but provide limited evidence on how workplace

networks affect the decision to enter politics.

This paper provides causal evidence on workplace network effects in political entry in a developing democracy with low levels of political participation and trust in politicians (Pring, 2022). These features may be consequential for political selection. Caselli and Morelli (2004) show that countries can become trapped in an equilibrium with low-quality politicians, in which negative perceptions of politics discourage higher-quality individuals from entering. Workplace networks could potentially mitigate such dynamics by reducing informational barriers to entry, facilitating recruitment, and encouraging individuals who would otherwise not consider a political career to run for office. The effects may be particularly relevant for women, who remain underrepresented in political office.

2 Setting

Brazil is a large federal democracy composed of 5,570 municipalities across 26 states. At every level of government, there is both an executive and a legislature, each elected on four-year cycles. Municipal elections, however, are staggered two years apart from state and federal elections. Political parties are organized nationally but can form electoral coalitions at the municipal level. In this paper, I focus on municipal elections for city council (*vereador*) seats, held in 2004, 2008, 2012, and 2016.

Formal barriers to political entry are low. Coalitions and parties may or may not provide campaign funds, but in practice they typically allow interested individuals to run for legislative positions, especially at the municipality level. This is partly due to the electoral rules. The maximum number of candidates that a party can nominate for a legislative election is high, equal to 1.5 times the number of available seats, and twice that for coalitions. Under this open-list proportional representation (OLPR) system, party seats are allocated based on the total number of votes received by all candidates from the party, so nominating more candidates can marginally increase the party's vote total. Once the number of seats won by the party is determined, candidates are ranked by their individual votes, and those with the highest vote counts are elected. Gender quotas apply: each party or coalition must reserve at least 30% of its candidate slots for each gender, while there are no gender restrictions on the elected city councilors themselves. It is important to note that in 2004 and 2008, parties and coalitions were only required to reserve these slots, and leaving them empty legally satisfied the requirement. A 2009 reform made actually filling the slots mandatory, a change that first applied to the 2012 municipal election.

This setting allows me to implement a regression discontinuity design that exploits the discontinuity between the last elected candidate and the first non-elected candidate within the same coalition[2] and municipality. By comparing these close winners and

[2]From here, I use the term coalition only. A party can be thought of as a coalition of a single party.

losers, it is possible to study the causal effect of an individual entering office on the subsequent political entry decisions of their past coworkers, while holding candidate quality approximately constant.

In principle, this strategy can be applied to legislative elections at any level of government. In this project, I focus on municipal city council elections for two main reasons. First, running for city council represents the lowest level of political entry in Brazil, typically the first office an individual seeks before moving to higher positions, making it a natural stage at which to study initial participation in politics. Second, municipal elections provide a larger number of observations, allowing for more precise estimation of the effects of workplace exposure.

Workplaces are a particularly relevant setting to study social interactions in this context. Most Brazilian workplaces are small, which facilitates social interactions among coworkers (Colonnelli *et. al.*, 2025). The average workplace has 16 employees, while the median has only 3 employees. In addition, political discussions are common in Brazil. Survey evidence from the Brazilian Electoral Panel Survey shows that, before the 2010 presidential elections, about 25 percent of respondents reported having attempted to convince others to vote for their preferred candidate either frequently or sometimes (Huberts and Machado, 2015). Taken together, the small size of workplaces and the prevalence of political discussion suggest that coworkers are likely to be exposed to each other's political views and actions, making workplace networks a meaningful channel for peer effects in political entry.

3 Data

I study close elections for local city council (*vereador*) seats in Brazilian municipalities across four electoral cycles: 2004, 2008, 2012, and 2016. For each cycle, I examine whether the former coworkers of first-time candidates are more likely to enter politics in the subsequent election if their candidate wins than if she loses, so the outcome is measured in elections from 2008 to 2020. To this end, I merge electoral data on all candidates to local city councils from the 2000 to 2020 elections, obtained from the Superior Electoral Court (*Tribunal Superior Eleitoral*, TSE), with administrative employer-employee data on the universe of formal employment in Brazil from 2002 to 2016, from the Annual Report of Social Information (*Relação Anual de Informações Sociais*, RAIS).

3.1 Electoral Data

I obtain electoral data for municipal elections from 2000 to 2020 from the TSE. I build a panel following each candidate over time to track whether individuals run in multiple elections. In this setting, the number of candidates per electoral cycle ranges from 325 to 490 thousand. Typically, around 13% of candidates in a given election

were previously elected, while 61% are newcomers, with the remaining 26% having run before without being elected. In any given election, around 14% of all running candidates are elected as local city councilors. Table 1 breaks down this data by year.

Table 1: Raw Candidate Sample by Election Year

Year	Total Candidates	Previously Elected	Newcomers	Elected
2004	345,453	12.4%	65.2%	14.96%
2008	325,412	15.6%	58.1%	15.91%
2012	416,180	12.8%	62.5%	13.79%
2016	437,376	13.5%	60.2%	13.22%
2020	489,312	12.5%	61.5%	11.85%
Total	2,014,287	13.3%	61.5%	13.74%

For every candidate, I observe each electoral run, along with the number of votes she received, the votes obtained by her coalition, and the total number of votes cast in her municipality. I also observe her level of education, age, gender, marital status, and declared profession, as well as her declared assets and campaign funds raised. Finally, I observe the party under which she runs, the coalition to which that party belongs, and the municipality and state in which the election takes place.

For each coalition-municipality-election cell, I construct a running variable measuring electoral closeness as follows:

1. Within each coalition-municipality-election cell, I keep only the last winner (the elected candidate with the fewest votes) and the first loser (the non-elected candidate with the most votes).[3][4]
2. I compute the vote difference between these two candidates.
3. I normalize this margin by the first loser's total votes, so that closeness is measured relative to the loser's own vote count. This makes the running variable comparable across municipalities, coalitions and candidates of different size and political strength.
4. The running variable is positive for the last winner, measuring her vote surplus over the first loser as a percentage of the first loser's votes, and negative for the first loser, measuring her vote shortfall relative to the last winner, also as a percentage of the first loser's votes. Both are therefore measured on the same scale and symmetric around the threshold.

[3]Given that these are legislative elections with multiple winners and losers per coalition, a natural extension is to keep all candidates near the threshold to increase power.

[4]Coalitions with no elected or no non-elected candidate cannot form a valid last winner/first loser pair and are dropped from the sample, accounting for less than 15% of observations.

I focus on newcomers, defined as candidates who have never run in the past, to capture the effect of people entering politics for the first time. I drop all candidates that had run or that had been elected before.[5] Because this restriction is applied on top of the last winner/first loser selection, a coalition may retain its winner but not its loser, or vice versa, if one of the two had run for office before; there is therefore no reason to expect the number of last winners and first losers in the final sample to coincide.

The final candidate sample contains 58,368 candidates: 16,703 in 2004, 13,614 in 2008, 14,630 in 2012, and 13,421 in 2016, distributed across approximately 46,442 coalition-municipality-election cells. Table 2 breaks down this data by year and by treatment status.

Table 2: First-Time Candidate Sample by Electoral Outcome and Year

Year	First Losers	Last Winners	Total
2004	8,736	7,967	16,703
2008	7,086	6,528	13,614
2012	7,820	6,810	14,630
2016	7,176	6,245	13,421
Total	30,818	27,550	58,368

3.2 Employment Data

I link candidates to their coworkers in the three years prior to the election using RAIS, the universe of formal employment in Brazil.[6] The dataset covers approximately 64.4% of all employed workers in Brazil, given high rates of informality (ILO, 2025). This accounts for 39 to 68 million workers per year in the period under study. I have data on each worker's wage, start date, contract type, reason for termination, and type of firm (private or public). Crucially, I observe the establishment, which is the physical place of work rather than the firm, capturing actual physical contact between coworkers.

For every candidate, I retain every establishment at which she or he is reported working in the three years leading up to the election. I drop establishments where more than one candidate works, to avoid coworkers who could have been influenced by more than one candidate.[7] I then link each candidate to the full set of coworkers held across these establishments in the years leading to the election. I drop candidates whose network

[5]Electoral records are available from 2000 onwards. The share of previously elected candidates is stable across cycles: 12.4%, 15.6%, 12.8%, and 13.5% respectively, suggesting that observing the 2000 election is sufficient to identify previously elected candidates in the 2004 cycle. The same pattern holds for non-elected candidates with prior experience running.

[6]Data for 2010 (two years prior to the 2012 election) are currently excluded due to a data-cleaning issue I am resolving.

[7]I only drop non-last winners and non-first losers after merging with the employee data, so that I can avoid keeping establishments where multiple politicians work, even if only one is a last winner or first loser newcomer.

exceeds the 95th percentile in size (defined as the total number of coworkers across establishments); these are candidates with more than 347 people in their network, a pattern that may reflect data issues, candidates who switched jobs frequently during the period, or large firms in which genuine interaction is unlikely. I also drop candidates with a margin bigger than 100pp, meaning that the first loser had to double her votes to reach the last winner.

Given that not all candidates work in formal employment, together with the additional restrictions described above, the final analysis sample contains 452,382 coworker observations corresponding to 7,805 unique candidates, with a mean of 38.72 and a median of 12 coworkers per candidate. The number of first-time candidates in the sample increases steadily across election years, from 1,704 in 2004 to 2,326 in 2016, while average network size remains relatively stable across the period, ranging from 36 to 41 coworkers per candidate. Among the 7,805 unique candidates, 532 belong to a coalition in which both the first loser and the last winner survived the data-cleaning process. Of the full candidate sample, 2,773 (35.53%) fall within the 10pp margin and 1,473 (18.87%) fall within the 5pp margin, my two preferred bandwidths reported throughout the rest of the paper.

4 Empirical Strategy

I estimate the effect of a coworker's electoral victory on the probability of running in the subsequent election cycle using a regression discontinuity design. Formally, for coworker i linked to candidate c in state s and election year t, I estimate

$$Y_{ic} = \alpha + \tau\, D_c + \beta_1 R_c + \beta_2\, D_c R_c + \mathbf{X}_c'\gamma + \delta_{st} + \varepsilon_{ic}, \qquad |R_c| \leq h, \tag{1}$$

where Y_{ic} is an indicator for whether coworker i runs for office in the election following candidate c's race, R_c is the running variable described above, and $D_c = 1[R_c > 0]$ indicates that candidate c was the last winner rather than the first loser. $\mathbf{X}_c$ is a vector of covariates, entering additively with a common coefficient γ on both sides of the threshold (Calonico *et. al.*, 2019). δ_{st} are state $\times$ election year fixed effects. The coefficient of interest, τ, captures the discontinuity in coworkers' running probability at the electoral threshold. Equation 1 is estimated by local linear regression with a triangular kernel, with the polynomial in R_c fit separately on each side of the threshold within a bandwidth h (Calonico *et. al.*, 2014).

The specification includes state $\times$ election year fixed effects and a linear polynomial fit on each side of the threshold. I additionally control for the log number of votes cast in an election to account for municipality size, and for the coalition's share of total municipal votes, which captures the political strength of the coalition. I control for whether the coworker's establishment is classified as public sector, given the substan-

tially higher baseline running rate among public sector workers (1.04% vs 0.31% for private sector workers within 5 percentage points of the threshold).

I report results at fixed bandwidths of 5 and 10 percentage points. Both bandwidths represent plausibly quasi-random margins. The 5pp bandwidth is my preferred specification for the main results, because it provides a relatively narrow window around the electoral threshold and is highly intuitive (the first loser needed 5% more votes to get elected, and the last winner won by getting 5% more votes than the first loser). The 10pp bandwidth is more credible for the heterogeneity analysis, given it retains more observations for the specific cells, while still having intuitive appeal. I report both for every result to show robustness. The data-driven optimal bandwidth (Calonico *et. al.*, 2014) selects approximately 14.5 percentage points on the unrestricted sample (with a 28.3pp bias correction), which I consider too wide for this setting: a 14.5pp candidate vote share margin may reflect systematic differences in candidate quality rather than quasi-random variation around the threshold. Additionally, the optimal bandwidth is sensitive to sample construction: my procedure drops candidates with a margin above 100pp before estimation, and trimming the tails of the margin distribution in this way can mechanically affect the bandwidth the algorithm selects.[8]

The unit of observation is the coworker. Standard errors are clustered at the candidate level, which is the level at which treatment is assigned. Observations where the last winner and first loser received exactly the same number of votes are dropped, as Brazilian electoral law resolves these ties by coin toss.

5 Validity of the Design

5.1 Covariate Balance

Table 3 reports RD estimates using predetermined characteristics of the original candidates as outcomes at both preferred bandwidths. To avoid conditioning on variables whose balance is being tested, I include only state $\times$ election year fixed effects here, without the additional covariates used in the main specification. All estimates are statistically indistinguishable from zero at conventional significance levels across both bandwidths, supporting the validity of the identifying assumption. The sample is smaller than in the main analysis because the unit of analysis in this exercise is the original candidate rather than the coworker.

5.2 Manipulation Test

A standard concern in regression discontinuity designs based on close elections is that candidates or parties might be able to manipulate the running variable near the threshold. I test for this using the local polynomial density estimator of Cattaneo *et. al.*

[8] Results at the optimal bandwidth will be reported in an Appendix.

Table 3: Covariate Balance at the Threshold

Variable	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Candidate age	−13.493	14.332	0.346	769/704	769/704
Female	0.059	0.057	0.306	769/704	769/704
Years of education	−0.531	0.762	0.486	640/608	640/608
Number of coworkers	−3.577	12.867	0.781	769/704	769/704
Network size	1.397	13.247	0.916	769/704	769/704
Number of council seats	0.535	0.788	0.497	769/704	769/704
Log total municipality votes	−0.174	0.200	0.385	769/704	769/704
Coalition vote share	1.810	2.571	0.481	769/704	769/704
Public sector	−0.032	0.042	0.456	769/704	769/704
Bandwidth = 10					
Candidate age	−4.772	6.957	0.493	1,423/1,350	1,423/1,350
Female	−0.026	0.039	0.501	1,423/1,350	1,423/1,350
Years of education	−0.400	0.489	0.413	1,178/1,149	1,178/1,149
Number of coworkers	4.075	8.424	0.629	1,423/1,350	1,423/1,350
Network size	7.083	8.719	0.417	1,423/1,350	1,423/1,350
Number of council seats	0.804	0.526	0.126	1,423/1,350	1,423/1,350
Log total municipality votes	0.085	0.137	0.537	1,423/1,350	1,423/1,350
Coalition vote share	−1.398	1.744	0.423	1,423/1,350	1,423/1,350
Public sector	−0.003	0.029	0.909	1,423/1,350	1,423/1,350

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Robust standard errors are reported.
All balance checks include state × election year FE only.

(2020), implemented at the candidate level, which is the unit at which any potential manipulation could occur in this context. The test fails to reject the null hypothesis of no manipulation ($T = -0.459$, $p = 0.646$). Binomial tests comparing the number of observations on each side of the threshold across a range of window widths are similarly uninformative of manipulation, with p-values well above conventional significance thresholds throughout. Together, these results support the validity of the identifying assumption.

6 Main Results

Table 4 reports RD estimates of the effect of a coworker's electoral victory on the probability of entering electoral competition in the subsequent cycle. The running variable is the vote difference between the first loser and the last winner in a given coalition, normalized by the number of votes received by the first loser. The sample consists of coworkers of first-time candidates who had never previously run for office, employed in establishments with only one candidate, in the three years prior to the election. Standard errors are clustered at the original candidate level. All specifications include state × election year fixed effects and control for the log number of total votes

in the municipality, coalition vote share, and whether the establishment is classified as public sector.

The baseline running rate among coworkers of losing candidates within 5 percentage points of the threshold is 0.37%, reflecting the low baseline probability of political entry in this population (0.36% within 10pp). As a descriptive benchmark, a raw OLS regression of coworker running on candidate electoral success yields a coefficient of 0.00049 ($p = 0.029$), suggesting that coworkers of winners are 0.049 percentage points or 13.6% more likely to run. This unconditional difference combines selection and causal effects. The RDD isolates the causal component by comparing coworkers of candidates just above and below the winning threshold, where candidates should be similar in political skills and other unobserved characteristics.

Table 4: Effect of Coworker Winning on Probability of Running (Next Election)

Specification	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
BW = 3	0.00240	0.00182	0.187	27,539/30,575	456/431
BW = 4	0.00255*	0.00154	0.097	35,784/38,319	611/569
BW = 5	0.00260*	0.00136	0.055	46,434/43,900	769/704
BW = 6	0.00297**	0.00123	0.016	53,860/51,380	913/849
BW = 7	0.00259**	0.00114	0.023	63,030/60,740	1,065/992
BW = 8	0.00247**	0.00108	0.022	70,795/67,195	1,190/1,112
BW = 9	0.00211**	0.00102	0.039	79,149/74,907	1,327/1,249
BW = 10	0.00195**	0.00098	0.045	84,498/79,504	1,423/1,350

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Control mean (losers, BW=5): 0.37%; Control mean (losers, BW=10): 0.36%.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

The point estimate is positive and statistically significant at the 10% level at the 5pp bandwidth (0.0026, $p = 0.055$) and at the 5% level at the 10pp bandwidth (0.00195, $p = 0.045$). At 5pp the estimate implies a 70% increase relative to the control mean of 0.37% (54% at 10pp). The effect is similar in magnitude and statistically significant at conventional levels for every bandwidth between 4 and 10pp. Table 4 reports these results, and Figure 1 plots estimates from 3 to 20pp, showing both robust estimates and a slow but steady decline in the coefficient as the bandwidth widens. This is expected because wider bandwidths include candidates farther from the threshold, who are less comparable in political skills and other unobservables, consistent with the identifying variation being concentrated among candidates very close to the threshold. As a robustness check, the specification with only state × year fixed effects yields a similar estimate at the preferred bandwidths (0.00212, $p = 0.120$ at BW = 5pp; 0.00178, $p = 0.069$ at BW = 10pp), suggesting the result is not an artifact of the choice of controls. Figure 2 shows the raw difference in running rates relative to the cutoff.

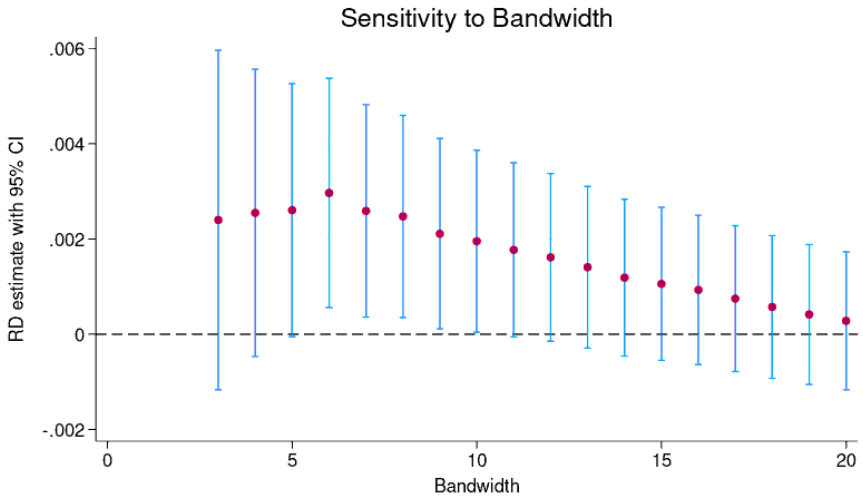


Figure 1: RD Estimates Across Bandwidths (3–20pp)

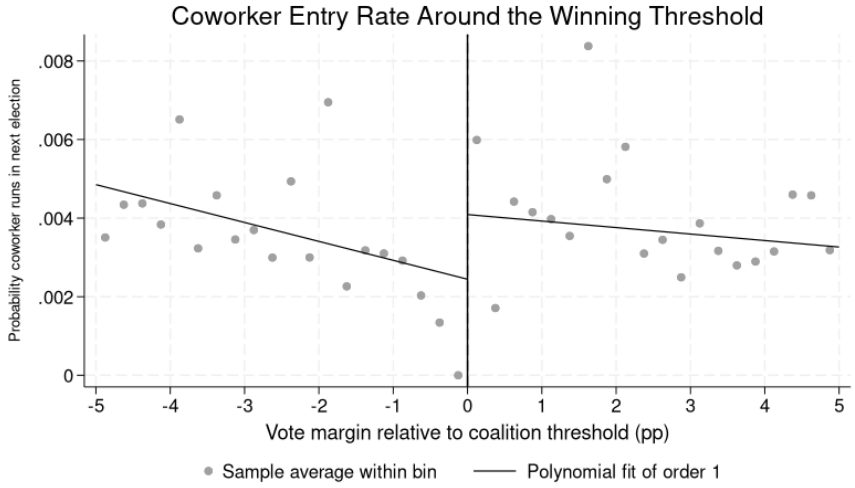


Figure 2: Running Rate by Vote Margin, No Controls

A clarification on the outcome measure used throughout this section: running rates are measured independently of the party, coalition, and municipality in which the coworker eventually runs. That is, a coworker is coded as running regardless of whether she runs for the same party or coalition as the original candidate, or in the same municipality. Future work will incorporate this information to test the information, recruitment, and opportunity channels more directly. Under a recruitment channel in particular, coworkers who run should run for the same party or coalition as the candidate who inspired them. The information and role-model channels do not generate this same prediction as sharply, since they could operate even when the coworker eventually runs for a different party.

6.1 Robustness At the Candidate-Establishment Level

I present three alternative measures of the main outcome to assess the robustness of the results. I conduct these exercises at the candidate-establishment level, rather than at the coworker level used in the main analysis.[9] The first outcome is an indicator for whether at least one coworker from the establishment runs for office in the subsequent election. The second is the number of coworkers who run, which captures the extensive margin of political entry. The third is the share of coworkers who run, which captures the intensive margin. These outcomes provide alternative ways of measuring whether the entry of a politician affects political participation within the workplace.

The results are reported in Table 5. Across all three measures, the point estimates point in the same direction as the main result. The probability that at least one coworker runs is statistically significant at both bandwidths, with an increase of 13.2 percentage points at the 5pp bandwidth and 12.4 percentage points at the 10pp bandwidth, relative to control means of 13.9% and 14.3%, respectively (roughly a doubling of the baseline probability). The number and share of coworkers running are each significant at only one of the two bandwidths, though the point estimates are similar in magnitude across both. Overall, these results suggest that the main finding is not driven by the particular measure of political entry used in the baseline analysis, though the number and share outcomes are less precisely estimated than the extensive-margin measure.

Table 5: Extensive and Intensive Margins of Coworker Entry

Outcome	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Any coworker runs (0/1)	0.13208**	0.05811	0.023	875/788	769/704
Number of coworkers running	0.13478	0.08495	0.113	875/788	769/704
Share of coworkers running	0.01086*	0.00603	0.072	875/788	769/704
Bandwidth = 10					
Any coworker runs (0/1)	0.12437***	0.04038	0.002	1,611/1,514	1,423/1,350
Number of coworkers running	0.17752***	0.05856	0.002	1,611/1,514	1,423/1,350
Share of coworkers running	0.00746	0.00472	0.114	1,611/1,514	1,423/1,350

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Unit of observation is the candidate-establishment cell.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

[9] A candidate may be linked to more than one establishment during the three years prior to the election, in which case she contributes more than one observation to this table. This is why the number of observations exceeds the number of clusters.

7 Mechanisms

In this section, I conduct a series of heterogeneity analyses to explore the mechanisms that could explain the main result. I first examine whether the effect is stronger for more intense and more recent workplace contact. I then split the sample by demographic characteristics of candidates and coworkers to test whether individuals are more likely to run when they resemble the candidate, consistent with a role-model channel, or whether the pattern is better explained by recruitment or other mechanisms. I plan to expand this analysis further, for example by testing whether candidates in higher-ranked or higher-paying positions than their coworkers are more likely to inspire them to run than candidates in lower-ranked or lower-paying positions. I report results at both the 5pp and 10pp bandwidths throughout: the 5pp bandwidth is my preferred specification, but some subsamples are small at this bandwidth, so the 10pp bandwidth is often more informative for a given cell. Reporting both allows me to assess the robustness of each pattern.

7.1 Contact Intensity

I exploit three features of the data to test whether the effect is stronger for more intense, longer, and more recent workplace relationships: establishment size, years of overlap with the candidate, and years since separation from the candidate prior to the election.

I first examine establishment size. Table 6 splits the sample into three bins of similar candidate size. At the 10pp bandwidth, the coefficient declines monotonically as establishment size increases, and the effect is statistically significant for the smallest firms. The 5pp bandwidth shows a similar overall pattern: the effect is concentrated in small and medium-sized firms and is close to zero for the largest firms, though it is not strictly monotonic, which may reflect the smaller number of clusters available in the smallest-firm bin at this bandwidth.

Second, I examine the duration of overlap between each candidate and coworker. Because I observe workers at every establishment they work in during a given year, coworkers vary in whether they overlapped with the candidate for one, two, or all three years leading up to the election. Table 7 reports the results. Both bandwidths show an unexpected dip at two years of overlap. At the 5pp bandwidth, the effect is strongest and most precisely estimated at one year of overlap, though similar in magnitude to the full three years of overlap. At the 10pp bandwidth, with more observations, the effect is instead clearly strongest at three years of overlap, and statistically significant. Although the pattern is not fully consistent across bandwidths, it suggests that longer relationships matter.

Third, I examine heterogeneity by the number of years between the end of the candidate-coworker relationship and the election. At the 5pp bandwidth, the coefficient is small-

Table 6: Heterogeneity by Firm Size

Firm size (n coworkers)	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
$\leq$ 10 coworkers	0.00442	0.00498	0.375	2,222/2,629	393/389
11–50 coworkers	0.00615*	0.00341	0.072	11,898/9,100	307/253
$\geq$ 51 coworkers	0.00046	0.00121	0.701	32,314/32,171	189/161
Bandwidth = 10					
$\leq$ 10 coworkers	0.00718*	0.00417	0.085	4,439/4,806	757/738
11–50 coworkers	0.00224	0.00224	0.317	19,372/16,651	534/480
$\geq$ 51 coworkers	0.00088	0.00100	0.379	60,687/58,047	358/313

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

Table 7: Heterogeneity by Years of Overlap

Years of overlap	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
1 year	0.00528***	0.00197	0.007	26,860/23,292	696/649
2 years	0.00009	0.00181	0.960	13,127/13,599	480/452
3 years	0.00510	0.00383	0.184	6,447/7,009	215/209
Bandwidth = 10					
1 year	0.00280**	0.00127	0.027	49,560/44,470	1,298/1,245
2 years	−0.00029	0.00133	0.826	25,286/23,409	866/824
3 years	0.00543**	0.00262	0.038	9,652/11,625	378/377

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

est for pairs that were still together in the year of the election, and largest for pairs separated one year prior. At the 10pp bandwidth, the coefficients are all of similar magnitude and statistically indistinguishable from zero. One speculative interpretation is that the effect is driven more by recruitment than by a role-model or inspiration channel: a candidate recruiting a former coworker may already know that person's abilities regardless of whether they are still in regular contact, whereas a role-model channel would more plausibly require closer, ongoing contact. This interpretation is speculative and would require further evidence to substantiate.

7.2 Establishment Sector

One relevant dimension of heterogeneity is whether the effect differs between coworkers employed in the public and private sectors. Public sector workers have a substantially higher baseline running rate than private sector workers (approximately 1% vs. 0.31%).

Table 8: Heterogeneity by Distance to Election

Years of separation	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
0 (still together)	0.00024	0.00164	0.885	25,291/24,974	559/508
1 year prior	0.00609**	0.00250	0.015	12,653/10,706	490/455
2 years prior	0.00488	0.00369	0.186	8,490/8,220	337/337
Bandwidth = 10					
0 (still together)	0.00128	0.00125	0.305	43,285/42,082	993/950
1 year prior	0.00150	0.00160	0.349	24,908/22,432	919/859
2 years prior	0.00166	0.00221	0.451	16,305/14,990	616/627

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

I estimate this heterogeneity without controlling for the public sector indicator, since the sample is already split by sector, but the remaining specification is unchanged. Table 9 reports the results. The public sector coefficient is roughly ten times larger than the private sector coefficient at the 5pp bandwidth, and the gap widens further at the 10pp bandwidth. Both effects are statistically significant, including for private sector workers, despite the much smaller estimated coefficient. In relative terms, the estimates imply a 229% and 50% increase, respectively, at the 10pp bandwidth. The public sector estimate should be interpreted with caution, however, as it is based on a very small number of clusters.

Table 9: Effect by Establishment Sector

Sample	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Public Sector	0.02845***	0.00426	0.000	3,842/4,781	53/46
Private Sector	0.00284**	0.00128	0.027	42,592/39,119	719/663
Bandwidth = 10					
Public Sector	0.02131***	0.00394	0.000	7,619/6,300	95/86
Private Sector	0.00149*	0.00090	0.097	76,879/73,204	1,334/1,274

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Control means (BW=5): public sector 1.04%, private sector 0.31%.
Control means (BW=10): public sector 0.93%, private sector 0.30%.
All specifications include state × election year FE, log number of voters, and coalition vote share.

7.3 Demographic Characteristics

I study how the effects differ by demographic characteristics. My hypothesis is that, if the effect is driven by role models, the effect will be stronger among pairs of candidates and coworkers who share demographic characteristics. On the other hand, if the main mechanism behind the results is recruitment, the effect should be stronger for less politically represented coworkers, regardless of candidate characteristics.

The first analysis shows differences by gender, splitting first by candidate gender and then by coworker gender. The candidate-gender split shows that the effect is significant only for male candidates, which is the gender most represented in politics, at both bandwidths (Table 10). The coworker-gender split is less conclusive: female coworkers show a significant effect at the 5pp bandwidth, but neither female nor male coworkers are significant at the 10pp bandwidth, and the point estimates are similar in magnitude between the two groups at that bandwidth (Table 11). Taken together, these patterns are suggestive of a recruitment mechanism, in which male candidates may be more likely to draw on their professional networks to bring underrepresented coworkers into candidacy. It seems plausible that, during this period of tightening regulation around gender candidacy quotas, male politicians in Brazil used their professional networks to recruit candidates in this way. More work is needed to substantiate this channel; I plan to examine differences across election years, as quota enforcement became more stringent over time, and to look separately at parties and municipalities that historically struggled to comply with the quota.

Table 10: Heterogeneity by Candidate Gender

Candidate	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Female	−0.00355	0.00325	0.275	3,763/3,432	88/82
Male	0.00305**	0.00138	0.027	42,671/40,468	681/622
Bandwidth = 10					
Female	0.00071	0.00247	0.773	8,075/7,803	193/161
Male	0.00192*	0.00098	0.051	76,423/71,701	1,230/1,189

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

Table 12 divides the sample into cells by both candidate and coworker gender. This cross-tabulation is more exploratory than the single-dimension splits above: the number of clusters becomes small in the cells involving female candidates, even at the 10pp bandwidth, and the pattern is not stable across bandwidths. At the 5pp bandwidth, the Male candidate – Female coworker cell is the only positive and significant cell, consistent with the recruitment story suggested above; at the 10pp bandwidth, however, it loses significance and the Male candidate – Male coworker cell instead becomes marginally

Table 11: Heterogeneity by Coworker Gender

Coworker	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Female	0.00325**	0.00154	0.035	30,161/28,627	736/669
Male	0.00083	0.00213	0.698	12,895/12,282	627/582
Bandwidth = 10					
Female	0.00169	0.00110	0.125	53,609/51,872	1,355/1,274
Male	0.00226	0.00175	0.196	24,685/22,007	1,173/1,111

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Control means (BW=5): male coworkers 0.39%, female coworkers 0.36%.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

significant.

Table 12: Heterogeneity by Candidate and Coworker Gender

Cand. × Cow.	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Female × Female	−0.00729***	0.00203	0.000	1,746/1,242	79/75
Female × Male	−0.00120	0.00729	0.869	1,768/1,890	77/76
Male × Female	0.00334**	0.00159	0.036	28,415/27,385	657/594
Male × Male	0.00208	0.00199	0.298	11,127/10,392	550/506
Bandwidth = 10					
Female × Female	−0.00189	0.00184	0.303	3,491/3,452	170/143
Female × Male	0.00247	0.00511	0.629	3,942/3,722	169/150
Male × Female	0.00136	0.00111	0.221	50,118/48,420	1,185/1,131
Male × Male	0.00308*	0.00160	0.054	20,743/18,285	1,004/961

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

I also report findings by race. Note that race is missing for a large share of observations in both datasets: approximately 63% of candidates and 50% of coworkers, so these results should be interpreted with caution. Given this, I do not report the fully disaggregated candidate-by-coworker race cells, which would leave too few observations per cell to be informative. Black and brown candidates show significantly stronger effects than white candidates, who make up the majority of candidates in the sample, at both bandwidths. At the 5pp bandwidth, this is also reflected in a larger, though not statistically significant, point estimate among non-white coworkers relative to white coworkers; this gap narrows and reverses at the 10pp bandwidth, and neither coworker-race estimate is significant at either bandwidth. These results provide little evidence that racial similarity drives the effect. More complete race data would allow

a cleaner test of whether racial similarity strengthens the effect, as would be expected under a role-model channel.

Table 13: Heterogeneity by Candidate and Coworker Race

Cand. × Cow.	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
By candidate race (all coworkers) — Bandwidth = 5					
White	0.00093	0.00270	0.730	8,689/7,925	161/136
Brown or Black	0.01019**	0.00447	0.023	6,429/7,399	82/85
By candidate race (all coworkers) — Bandwidth = 10					
White	−0.00074	0.00229	0.746	15,793/16,219	267/268
Brown or Black	0.00630**	0.00279	0.024	11,374/12,253	164/155
By coworker race (all candidates) — Bandwidth = 5					
White	0.00145	0.00152	0.340	22,022/19,953	650/585
Brown or Black	0.00310	0.00198	0.118	13,846/12,968	508/457
By coworker race (all candidates) — Bandwidth = 10					
White	0.00169	0.00110	0.127	40,049/37,980	1,186/1,121
Brown or Black	0.00088	0.00144	0.542	24,203/22,876	937/865

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Race missing for a large share of candidates and coworkers.
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

8 Intermediate Outcomes of Political Participation

In this section, I explore whether past coworkers of elected politicians are more likely to increase their political participation through outcomes that are less costly and less career-changing than running for office. In particular, I examine party affiliation and campaign donations. This section is highly preliminary.

8.1 Party Affiliation

As a complementary outcome, I examine whether a coworker's candidate winning increases the probability of the coworker affiliating with a political party for the first time. Party affiliation is a lower-stakes form of political engagement than running for office, and may capture a broader inspirational effect.

I link coworkers to party affiliation records from the TSE using name and municipality of residence.[10] This matching procedure is imperfect for two reasons. First, name matching is inexact, as small differences in spelling or name order across datasets may prevent valid matches, and some individuals share common names. Second, a coworker may work in one municipality but be registered as a party affiliate in another. As

[10] I remove accents before matching on name.

a result, only approximately 8% of coworker observations are matched to affiliation records.

Table 14 reports RD estimates for two outcomes: first affiliation in the three years prior to the election (placebo) and first affiliation in the three years following the election, excluding the election year itself to avoid ambiguity about whether affiliation occurred before or after the result was known.[11]

The pre-election placebo estimate is negative and significant at the 5pp bandwidth (-0.01486, $p = 0.002$), though not at the 10pp bandwidth (-0.00420, $p = 0.249$). Since this outcome is measured before the election takes place, it cannot be causally affected by the election's outcome, and a significant placebo estimate is more a warning sign for this specific exercise than a substantive finding. The post-election estimates are small, unstable in sign across bandwidths (-0.00385 at 5pp, 0.00231 at 10pp), and not statistically significant at either bandwidth, providing no clear evidence of an effect on first-time party affiliation.

Table 14: Effect on First-Time Party Affiliation

Outcome	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
First affiliation (pre-election)	-0.01486^{***}	0.00479	0.002	40,524/38,590	768/701
First affiliation (post-election)	-0.00385	0.00382	0.313	40,524/38,590	768/701
Bandwidth = 10					
First affiliation (pre-election)	-0.00420	0.00364	0.249	73,917/69,520	1,421/1,341
First affiliation (post-election)	0.00231	0.00247	0.350	73,917/69,520	1,421/1,341

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Coworkers matched to affiliation records on name and municipality.
Control means pre-election: 1.29% (BW=5), 1.44% (BW=10).
Control means post-election: 0.79% (BW=5), 0.82% (BW=10).
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

There are several ways I plan to improve this exercise. First, I will check whether coworkers affiliate specifically with the party of the candidate who inspired them. Second, I will examine changes in party affiliation more broadly, rather than only first-time affiliations. Third, I will work to improve the underlying name-matching procedure, which may also help resolve the placebo concern raised above.

8.2 Campaign Donations

To supplement the previous exercise on party affiliation, I examine whether coworkers of winning candidates are more likely to donate to the next political campaign, and

[11]The data do not allow me to identify the exact month of affiliation, so the election year itself is dropped.

whether they donate larger amounts. Campaign donations represent another lower-cost form of political participation. I link coworkers to TSE campaign finance records for the 2008, 2012, and 2016 elections using their fiscal ID.

The baseline donation rate among coworkers of losing candidates is 0.62% at both bandwidths, and the baseline donation amount is R$12.82 (BW=5) and R$10.48 (BW=10), including non-donors. Table 15 reports RD estimates for three outcomes: whether the coworker donates, the donation amount (including non-donors), and the donation amount conditional on donating. None of the three outcomes shows a statistically significant effect at either bandwidth. The probability of donating is close to zero at both bandwidths. The unconditional donation amount is similarly small and switches sign across bandwidths. The conditional donation amount is less stable, probably due to the small amount of actual donors. Overall, I find no evidence that exposure to a winning coworker affects campaign donations, in contrast to the effect found for running for office itself; this may reflect genuinely limited spillover to this outcome, or simply insufficient statistical power given how rare donations are in this population or, as with the party affiliation results above, imperfect matching to the donation records.

Table 15: Effect on Campaign Donations

Outcome	Coef.	Std. Err.	p-value	N_L/N_R	C_L/C_R
Bandwidth = 5					
Donated (0/1)	−0.00008	0.00218	0.971	46,434/43,900	769/704
Donation amount (R$)	1.255	4.979	0.801	46,434/43,900	769/704
Amount | donated (R$)	761.100	608.140	0.211	288/266	142/121
Bandwidth = 10					
Donated (0/1)	0.00121	0.00155	0.434	84,498/79,504	1,423/1,350
Donation amount (R$)	−4.927	4.389	0.262	84,498/79,504	1,423/1,350
Amount | donated (R$)	−711.280	674.760	0.292	526/470	267/231

*** p<0.01, ** p<0.05, * p<0.10.
Robust bias-corrected estimates reported throughout.
Standard errors clustered at the original candidate level.
Control means donation rate: 0.62% (BW=5/10).
Control means donation amount: R$12.82 (BW=5), R$10.48 (BW=10).
All specifications include state × election year FE, log number of voters, coalition vote share, and public sector indicator.

Conclusion

Politics is inherently social, and the decision to enter politics is therefore influenced by social ties. Entering politics is also a career choice, and it may be subject to the same mechanisms documented in the referral literature on labor markets. This paper provides evidence that workplace relationships affect political entry. Using administrative data on Brazilian workplaces and a regression discontinuity design based on close municipal elections, I find that individuals who previously worked with a narrowly elected city council candidate are more than 50% more likely to run for office in the subsequent election. The effect is economically large and statistically significant across a range of bandwidths and alternative measures of political entry.

The heterogeneity results provide some evidence on the channels behind this effect. The effect is larger among coworkers in smaller establishments and among those with longer workplace overlap, consistent with more intensive workplace relationships facilitating political entry. Two further patterns point toward a role for political recruitment: the effect is concentrated among male candidates and female coworkers, the over- and under-represented genders in Brazilian politics respectively, during a period of tightening gender candidacy quotas; and the effect is significantly stronger for black and brown candidates than for white candidates, who make up the majority of the sample, at both bandwidths. The effect is also substantially larger among public-sector coworkers. Together, these patterns are more consistent with candidates drawing on professional networks to recruit coworkers from underrepresented groups than with a pure role-model channel, though more work is needed to substantiate this directly.

More broadly, the findings suggest that political participation can be influenced by social networks formed outside explicitly political settings. Workplaces provide repeated interaction, information about political opportunities, and opportunities for recruitment. These relationships may therefore affect not only whether individuals participate in politics, but also who enters the political system. Understanding these social influences is important for understanding the formation of the political candidate pool and, ultimately, the composition of elected officials.

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