When Democracy Refuses to Die: Evaluating a Training Program for New Politicians*

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Abstract

We evaluate the effects of a program in Brazil that selects and trains new politicians, addressing three main challenges: selection bias from program screening, self-selection into candidacy, and the need to quantify the contributions of both selection and training in a holistic evaluation. Our findings show that the program raised political entry by doubling candidacy rates and increasing electoral success by 69%. However, much of the overall effect was driven by screening, which accounted for 30% of the increase in candidacy and 43% of the increase in election rates, while also making the candidate pool more diverse, competent, and committed to democratic values. Renewing the political class involves trade-offs, as some traits favored by the program did not align with voter preferences, and also reduced the descriptive representation of low-income individuals.

Keywords: Political Selection, Quality of Politicians, Political Entry, Training Programs, Democratic Backsliding

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1 Introduction

The global secular trend toward democratization has slowed down in recent years and, according to some observers, even reversed. The Institute for Democratic and Electoral Assistance (IDEA 2022) reports that half of the world's democracies are in retreat —the so-called "democratic backsliding"— and that more countries are moving toward autocracy than democracy. Democratic backsliding is often linked to the emergence of political parties and leaders who eschew the basic tenets of liberal democracy. Prominent examples are radical-right parties in Germany and Sweden, elected leaders like Erdogan in Turkey, Trump in the United States, and Bolsonaro in Brazil. Once in power, these leaders try to alter some of the basic rules of the democratic game. According to political scientists, "democracies die" from self-inflicted wounds (Levitsky and Ziblatt, 2018).

These democracy-weakening parties and leaders succeed in a context of distrust among citizens toward the political class. Established political parties do not adequately respond to past economic or policy shocks, get mired in corruption scandals, and do not adjust to new representation demands.[1] If a democracy is to be robust, it would respond to new representation demands or to a crisis of trust in established leaders by promoting the entry of new politicians. And the new politicians would be committed to democratic values even as they provide an alternative to the old order. Unfortunately, democracies can fail to respond in such a manner.

Yet pro-democracy responses have not been entirely missing, even if they have taken place outside established parties. In several countries, civil society has responded creatively, attempting to open new channels for political entry. One such response comes from organizations that help individuals run for office. In some cases, these organizations wish to shape political selection as a means to shift policy. Examples in the United States are *Run For Something*, a support organization for progressive candidates, and *Emily's List*, an organization promoting the election of female, pro-choice, Democratic candidates. An example in France is the *Académie of Futurs Leaders*, which supports progressive leaders on social justice and environmental issues. Other organizations foster new leadership with less of a focus on

[1] By way of example, following economic and policy shocks in Sweden, a gap in descriptive representation and a trust deficit drove the surge in popularity of a radical-right party, the Sweden Democrats (Dal Bó et al., 2022). In Brazil, Bolsonaro gained traction as voters grew disillusioned with democracy (Hunter and Power, 2019; Nicolau, 2020). Bolsonaro obtained support less from voters sharing his extreme views than from a rejection of the traditional Workers' Party (PT, for Partido Trabalhista) which presided over worsening economic conditions and major corruption scandals (Setzler, 2021). See Guriev and Papaioannou (2022) for a recent review on the drivers of populism.

specific policies. Examples are the Emerging Leaders Foundation in Kenya, the Indian School of Democracy, or the Brazilian *RenovaBR* program that we study in this paper. If effective, all of these attempts to renew political selection should help avoid junctures where democracies die. But are these initiatives successful? Answering this question is difficult because it is nearly impossible to establish the counterfactual to the existence of these programs. At the same time, answers are badly needed to decide whether societies should further support these programs through private philantropy and even state funds.

In this paper we focus on the Brazilian *RenovaBR* program ("Renova" means "Renew" and "BR" stands for Brazil). A school for politicians, *RenovaBR* was formed in 2017 to foster political entry. The program screened aspirants and then provided training on, among other things, how to run for office. *RenovaBR* welcomed applicants regardless of partisan affiliation but favored certain traits like competence, diversity, and democratic commitment. We investigate whether the program affected the type of person who runs for local office and gets elected. We faced three challenges doing so.

First, admission into the program was not random, so it is difficult to identify the treatment effect of the program. Second, some of the outcomes of interest are contingent on trainees deciding to run for office. Third, the value of educational credentials goes beyond training and includes the value of screening. Therefore, filtering out selection effects to isolate treatment effects is not enough. Ideally, we can both identify treatment effects *and* assess the relative contribution of each type of selection to the overall difference in outcomes between individuals who were trained by the program and those who were not.

To attain these goals, we start by writing a model of selection by *RenovaBR* and of subsequent candidacy decisions. Part of the value of the model is to elucidate the assumptions needed to reliably identify treatment effects as well as their contribution relative to selection. A key assumption in the theory–and in our initial empirical approach–is that selection into treatment by *RenovaBR* was based on observables. This assumption is plausible given the unprecedentedly rich data collected by the program to guide their admissions, data that they shared with us. To assess the robustness of this approach, we extend the selection framework to account for unobservables and implement a control function method. For identification, we employ a judge leniency design, using the severity of the admissions personnel as an instrumental variable. We further leverage our model and data to assess the relative contribution of selection and treatment effects along the lines of traditional Oaxaca-Blinder decompositions (henceforth, "OBD" – see Oaxaca (1973), Blinder (1973), and also Kitagawa (1955)). The decomposition allows us to assess the contribution of program screening and also to identify a treatment effect on electability that is purged of candidate self-selection effects.

Effects of *RenovaBR* on the Decisions to Run and Electoral Performance Our theory predicts that if *RenovaBR* increases electability or lowers the costs of running for office, candidacy rates should be higher among the treated. In contrast, the theoretical treatment effect on electoral performance is *a priori* ambiguous: *RenovaBR* might induce candidacy more strongly among weak contenders through lower (unobserved) costs of running that matter for candidate self-selection. In line with these theoretical priors, we find clear evidence that *RenovaBR* doubled candidacy rates from a baseline of 16% to 33%. The program also improved electoral performance. On average, and conditional on running, *RenovaBR* trainees obtained vote shares almost a third larger, and their election rates improved from 8% to 12.3%.

We explore potential factors affecting the electoral performance of *RenovaBR* candidates, including their choice of party and fundraising. *RenovaBR* candidates choose the same parties as other candidates, and raise more campaign funds from private donors, an increase that is partly offset by lower funds from their own political parties.

The Overall Effects of the Program and Political Representation Our theory guides the empirical construction of predictors of conditional expectations. Some align with the components of a standard OBD, allowing us to assess the relative contribution of *RenovaBR* screening and treatment to the observed increased in candidacy rates. For electoral performance, the OBD selection effects combine *RenovaBR*'s screening effects with candidate self-selection effects. To disentangle these effects, we construct additional counterfactual conditional expectations that separate the contributions of *RenovaBR* selection, candidate self-selection, and direct treatment effects on candidate electability. We show that the conditional effects of *RenovaBR* on electoral performance included slightly negative candidate self-selection effects, and that the direct effect of the program was to increase electability by roughly 5.5 percentage points. The increased electability (and, possibly, lower costs of running, something we cannot identify) imply that the program lowered barriers to political entry.

Our decomposition further shows that *RenovaBR* screening accounts for 32% of the overall difference in candidacy rates and 43% of the difference in electoral performance between treated and control individuals. The significance of *RenovaBR* screening is also evident in how the program influenced the profile of candidates running for office. It shifted the candidate pool toward greater gender balance, higher competence, and stronger pro-democracy values – outcomes aligned with the objectives of the program. Importantly, we find that this outcome was not due to *RenovaBR* disproportionately encouraging a specific type of candidate to run. Instead, it resulted from the program selectively admitting candidates with these desired traits and subsequently providing uniform encouragement for candidacy among all trainees.

A similar analysis of the profiles of politicians who ultimately get elected yields less conclusive results for two main reasons. First, the smaller sample size limits statistical power. Second, not all traits prioritized by *RenovaBR* align with voter preferences. This highlights a demand-side tradeoff in efforts to renew the political class: not all desirable traits increase electability.

Some tradeoffs arise on the supply side as well. Promoting more competent and demographically diverse aspirants also meant less descriptive representation of lower-income individuals. Previous work posited the possibility of a competence-representation tradeoff but found no evidence for it in the context of a developed democracy (Dal Bó et al., 2022). The Brazilian context features a tradeoff on the supply side between competence and representation and even between different forms of descriptive representation.

Related Literature Our study contributes to the extensive labor economics literature examining job training programs' effects on employment outcomes. For instance, Card et al. (2018) review over 200 recent studies on active labor market programs, revealing that while these interventions generally show minimal impact in the short term, they exhibit positive effects two to three years after the programs conclude.

Despite this substantial body of research, none of the 200 studies specifically address training programs aimed at aspiring politicians, even though such programs exist in many countries. To our knowledge, this study is the first to offer an evaluation of a training program for aspiring politicians. Ravanilla (2021) assessed leadership training for 15-17-year-olds in the Philippines interested in running for a youth council, and found that training filtered out less motivated candidates and encouraged more qualified individuals to act as representatives. Our study focuses instead on adult citizens participating in municipal politics. In addition, we develop approaches that will hopefully be useful for future evaluations, such as the selection framework and the decomposition between screening and training.

Ours is also the first evaluation of an organic, indigenous initiative to shift political supply at a national scale.[2] But there are earlier noteworthy studies of interventions shaping political

[2]In this respect, our paper also relates to a broader body of work studying determinants of political entry and selection, such as wages (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013; Kotakorpi and

supply. Casey et al. (2021) worked with political parties in selected areas of Sierra Leone to randomize the provision of information on aspirant politicians to the party leaders who control the nomination process. They demonstrate that providing information on aspirants causes party leaders to nominate candidates whose profiles are more aligned with citizen preferences. Gulzar and Khan (2024) conducted an RCT in which they varied the way the local office was portrayed to citizens in the Pakistani province of Khyber Pakhtunkhwa. They show that presenting local office as a prosocial endeavor in a public venue motivates individuals to seek candidacy and get elected, which eventually results in a policy that is more aligned with citizen preferences.

Our work complements past contributions concerned with the role of valence in elections, where valence is considered unobservable to the analyst. Kawai and Sunada (2021) estimate candidate valence in House elections in the United States and find that incumbents have significantly higher valence than challengers. Iaryczower et al. (2020) estimate valence for local candidates in Brazil and study how voters trade off valence and ideological positioning.[3] They conclude that there are significant welfare losses due to the low valence of candidates, which underscores the value of programs like *RenovaBR*, which seek to improve the competence of candidates. Our extended selection framework retains an unobservable component. However, the rich data at our disposal render so many aspects of candidate quality observable that the unobservable component does not appear to play a significant role in voter choice.

Plan for the paper The next section offers background information. Section 3 describes the data. Section 4 lays out our theory and derives empirical predictions. Section 5 derives our econometric specifications. After laying out the selection on observables case, we expand our theoretical selection framework to allow for selection on unobservables. Section 6 contains our results on treatment effects and political selection. Section 7 links the theory to Oaxaca-Blinder decompositions and presents the corresponding empirical results. Section 8 discusses supply- and demand-side tradeoffs facing the renewal of the political class in Brazil. We conclude in Section 9.

Poutvaara, 2011), political competition and/or family ties (Cruz et al., 2017; Dal Bó et al., 2009; De Paola and Scoppa, 2011; Galasso and Nannicini, 2011), party leaders (Casey et al., 2021), or campaign finance rules (Avis et al., 2022). See Dal Bó and Finan (2018) for a detailed review of this literature.

[3] See also Kendall et al. (2015) for a field experiment comparing voter responses to information on ideology vs performance–an indication of valence–of an incumbent mayor.

2 Background

The Workers' Party (PT) governed Brazil between 2003 and 2016, with Luiz Inácio Lula Da Silva serving two terms, followed by Dilma Rousseff. Rousseff's first term was marked by massive protests in 2013 demanding better public services and lower corruption (**?**). Despite declining popularity, Rousseff was re-elected in 2014, only to face more considerable challenges: increasing unemployment, budget cuts, and the large-scale Lava Jato corruption scandal (Taylor, 2020). The uncovered corruption involved major firms and politicians from 28 parties, triggering another year-long wave of protests. With dwindling support and amid a recession, Rousseff was impeached in 2016 and replaced by her vice president, Michel Temer.

The political and economic crises damaged trust in political institutions. By 2016/17, 70% of respondents in the LAPOP survey expressed distrust in political parties. In this context, there was a significant turnover in Congress and a sizeable ideological shift to the right, with populist Jair Bolsonaro winning the 2018 election.[4] The political crisis also engendered a response from civil society with several non-partisan initiatives emerging–such as *RenovaBR*– to improve political representation (Gatto and Thomé, 2024).

2.1 The *RenovaBR* School of Politicians

A nonpartisan entity funded by a consortium of entrepreneurs, *RenovaBR* was established in 2017 as a direct response to Brazil's institutional crisis. *RenovaBR* is dedicated to cultivating ethical leaders committed to defending democracy, implementing effective policies, and enhancing representation. According to founder Eduardo Mufarej, the organization's primary objective is to train individuals who wish to enter politics but do not know how to develop a competitive candidacy (Mufarej, 2021).

In order to make new candidates competitive, *RenovaBR* conducted numerous interviews, focus groups, and worked with past candidates to identify obstacles to a bid for office. The result was a customized curriculum that includes political communication and marketing strategies, effective use of social media, networking, and campaign financing. Additionally, participants engage in discussions addressing Brazil's primary challenges and the principles of evidence-based policymaking (Mufarej (2021), pp. 103-108).

In its inaugural training course in 2017, *RenovaBR* selected 133 participants, of whom 117 ran for office in the 2018 election. Seventeen candidates were successfully elected: one senator,

[4] See Nicolau (2020) and Borges (2021)

nine federal deputies, and seven state legislators. As some of these newly elected leaders gained visibility during their terms, *RenovaBR* became Brazil's largest NGO dedicated to training political leaders.

In 2019, *RenovaBR* launched a new program aimed at the upcoming 2020 municipal elections, *RenovaBR* Cidades (*RenovaBR* Cities, henceforth, just *RenovaBR* for short), which is the focus of our study. It sought to prepare candidates for municipal council and mayoral positions. The program attracted significant interest, with over 15,000 individuals nationwide submitting applications. Following a careful selection process, 1,502 aspirants for mayoral and municipal council positions from 455 municipalities were chosen. The training took place between August and November 2019, offering over 100 hours of instruction, mentorship, and networking opportunities. Those running for office needed to register with a political party by April of 2020, and formalize a candidacy by September, before elections took place in November of 2020. Appendix Figure A1 illustrates the program timeline.

2.2 The Selection Process

The selection process for the *RenovaBR* program consisted of two phases. Phase 1 involved an online questionnaire socioeconomic and educational background, previous occupation, political activities, and institutional or party affiliations.

This was followed by three tests, respectively on current events, democratic commitment, and logical reasoning. Scores on these tests determined progression to Phase 2. Specific thresholds were set for women, indigenous people, and Black candidates to ensure diversity in the candidate pool. *RenovaBR* disqualified applicants who, in the democracy test, expressed views contrary to democratic principles, such as endorsing military interventions. These criteria reflect the organization's effort to balance its goals of promoting capable leaders from diverse political backgrounds while maintaining a commitment to democratic values.

In Phase 2, candidates were required to complete three tasks. First, they used the STAR (Situation, Task, Action, Result) method to respond to a questionnaire detailing personal and professional experiences demonstrating their leadership skills, their ability to learn from past challenges, and their problem-solving abilities. They also participated in policy simulations, and answered questions that, being technical and in multiple-choice format, were gradable. Lastly, candidates submitted a short video introducing themselves and explaining their background, life story, as well as their goals and motivation for entering politics.

All materials submitted by aspirants were reviewed by one or two randomly assigned judges, who assessed six competence metrics: Resilience, Motivation, Leadership ability, Communication skills, Learning ability, and Narrative appeal of the aspirant's life-story. Each judge assigned scores ranging from 1 to 4 for each dimension and provided a summary score, ultimately recommending the candidate for acceptance, rejection, or placement on a waitlist (wait-listing ultimately played a similar role to recommending acceptance, so we treat both recommendations equivalently). Senior executives at *RenovaBR* then made final decisions on admission based on these recommendations.

2.3 Training Activities

The 2019 *RenovaBR* program aimed to prepare candidates for city council and mayoral positions. Over four months, students participated in 96 hours of online lectures and 24 hours of practical exercises, covering three key areas: political communication and campaigning, leadership, and municipal public policy. *RenovaBR* enlisted over 40 experts, including economists, political scientists, and urban planners, to deliver lectures on effective municipal governance.

These lectures provided technical insights into policy issues and practical knowledge of leadership and campaign strategies. Topics included urban planning, public education, municipal finances, ethical leadership, and citizen mobilization. Sessions also addressed campaign planning, digital presence, and social media strategy. Graduates gained access to a network of scholars, politicians, and potential donors and participated in events designed to facilitate connections with campaign supporters.[5]

While most training was delivered online, students from 445 municipalities also attended regional in-person meetings. In September 2019, 1,352 students attended these meetings, and in October, over 1,000 students participated in the *Renovar o Brasil* project, which focused on grassroots initiatives to improve local public spaces. Completing the program required attending lectures, completing exercises and quizzes, and submitting a final project outlining a political platform and campaign plan. Ultimately, 1,170 students graduated (an 83% completion rate).

Evidently, *RenovaBR* represents a bundled intervention that includes training, a credential with potential signaling value to parties and voters, and access to networks and information. The program is not just a training initiative aimed at building human capital. Still, for brevity, we will often refer to "treatment" and "training" interchangeably.

[5] See https://piaui.folha.uol.com.br/materia/escola-sem-partido/.

2.4 Municipal Elections

In Brazil, municipal governments are responsible for services such as primary health care, education, and urban infrastructure. These municipalities are governed by a mayor (prefeito), elected via a first-past-the-post system (or a runoff system in municipalities with populations over 200,000), and by municipal council members (vereadores), elected through an open-list proportional representation system. Councils vary in size, from 9 to 55 members, depending on the municipal population. Their duties include passing laws, monitoring the executive branch, proposing budgets, and addressing local concerns through public hearings and committees. Councillors can influence local spending and policy by submitting legislative bills and petitions (Ferraz and Finan, 2009).

Municipal elections occur every four years, and candidates must be affiliated with a political party to run.[6] Our analysis focuses on the municipal council races, where seats are allocated to parties proportionally to their vote share.[7]

Municipal council positions are often the entry point into political careers. In the 2020 elections, over 500,000 candidates ran for council, with 82% never having held office and 56% running for the first time. This context makes municipal races a fitting environment to examine programs like *RenovaBR*, which are aimed at political newcomers.

3 Data

3.1 Data sources

We worked closely with *RenovaBR* and obtained detailed individual-level information about the applicants to the 2019 *RenovaBR* training program. Our initial dataset consists of 15,309 individuals who signed up and completed the full online questionnaire. We focus on races for the municipal council because most *RenovaBR* candidates ran for these positions.[8] For applicants in Phase 1, we observe demographic and socio-economic characteristics, political attitudes and beliefs, and the logic and current affairs test scores. We also observe party

[6] *RenovaBR* did not require students to have a party affiliation to participate in the program. However, everyone needed to register with a political party by April 2020 to run for election.

[7] See Mainwaring (1991) and Ames (1995) for details on Brazil's open list proportional representation system and its effects on candidates and party strategies.

[8] Since mayoral races (first-past-the-post) are not comparable to municipal council races (PR system), and there are few of participants that run on those, we remove mayor and vice-mayor candidates from the sample.

registration, plans to run for office in 2020, and (self-reported) past leadership experience. For aspirants in Phase 2, we also observe the scores assigned by judges on each of the six competence dimensions evaluated in that phase (e.g. communication, resilience, motivation) and the final decision on admission by *RenovaBR* leadership. We also have information on which candidates were evaluated by which judges.

Our second data source is the list of candidates registered for the 2020 municipal elections obtained from Brazil's Electoral Authority (*Tribunal Superior Eleitoral*, henceforth TSE). We have the names and electoral codes of all individuals who registered their candidacy to run for office in 2020. The TSE dataset also includes characteristics such as date of birth, education, gender, race, declared wealth, the municipality where the candidacy was registered, the political party, and campaign finance revenues and spending for each candidate. Finally, for each registered candidate, the TSE has information on votes received and whether the candidate got elected. We classify candidates' partisan ideology following the classification of Zucco and Power (2024).

3.2 Selection

Self-selection The sample we will use to evaluate the effects of the *RenovaBR* program reflects deliberate selection criteria. Thus, describing our data entails venturing into a form of program evaluation, starting with studying how the selection process in *RenovaBR* shaped the set of admitted students. That process begins with an instance of self-selection when a set of applicants volunteer to participate. Table 1 shows the characteristics of individuals who signed up for training with *RenovaBR* and completed the initial questionnaire (column 1) compared to all the candidates in the 2020 election (column 2) and the subset of candidates who run for office in municipalities with at least one applicant to the *RenovaBR* program. Individuals who signed up for the *RenovaBR* course tend to be younger, more educated, more likely to be white and single, less likely to be female, and less likely to have run for office compared to the universe of candidates who sought political office in the 2020 election (column 2) or compared to those that run in municipalities with a *Renova* candidate (column 3). The share of females and the experience running for office is lower for Renova candidates, even compared to the set of all first-time candidates displayed in column (4). On the other hand, the education level of *Renova* aspirants is exceptionally high compared to the set of incumbents running for reelection displayed in column (5).

[TABLE 1 SELF-SELECTION ABOUT HERE]

As stated before, the program's primary goal is to cultivate new politicians who are capable, firmly committed to democratic values, and representative of diverse perspectives. This raises the critical question of whether these priorities were reflected in the selection criteria and whether any tensions arose between them.

Demographics, social background, and ability Table 2 captures the "funnel" created by rejections after Phases 1 and 2. Applicants are split into three categories according to how far they got in the selection process. Accepted applicants are slightly younger and disproportionally female compared to all those who signed up for the program. Despite the goal to address racial disparities, the accepted applicants had a larger share of whites, highly educated, and higher earners than those rejected. These patterns could reflect a failure to implement the stated goals or the result of tension between the respective dimensions of diversity and competence.

[TABLE 2 ON SELECTION FUNNEL ABOUT HERE]

Did political engagement and competence matter for screening? As expected, the knowledge and logic tests in Phase 1 mattered for advancing into Phase 2. Individuals already registered in a political party, with leadership experience, and who planned to run for office in 2020 were more likely to progress in the screening process. The competence traits evaluated in Phase 2 and the overall evaluation by judges also mattered: those accepted fared significantly better along those dimensions. The ability to do well in the various tests and tasks evaluated in Phases 1 and 2 correlates with higher education and higher income, revealing a tension in the pursuit of applicants who can simultaneously appear capable and broaden the representation of diversity along education and economic dimensions.[9]

Political attitudes As shown in Appendix Figure A2, the survey metrics employed by *RenovaBR* to capture political attitudes appear coherent. Applicants who self-identify as aligned with left-wing parties are more supportive of democratic values, progressive values, redistribution, and government regulation. Those identified with right-wing parties are on the opposite end of each dimension, with centrists right down the middle.

How did the two sequential rounds of screening affect who was admitted in terms of ideology? Appendix Figure A3 shows that applicants displayed more commitment to democratic and

[9] Dal Bó et al. (2022) argue that democracy can produce capable and broadly representative leadership. That finding is established in Sweden, a country with high-quality universal education and significant redistribution. The tension between high ability and broad representation will likely appear in other contexts, as the selection data presented here suggests.

progressive values as screening moved from one phase to the next.

Comparison to the rest of political candidates We argue that *RenovaBR* likely made a difference to the set of candidates who ran.

As made clear in Table 1, the pool of applicants to *RenovaBR* had fewer women and more white individuals than among the naturally occurring set of new candidates in the 2020 elections. The *RenovaBR* selection process attempted to undo those biases. When we compare the individuals selected into the program who run for office (column 5 of Table 2) with the runner-ups who were not trained but also run for office (column 4), we find that *RenovaBR*-trained individuals are more likely to be female but also more likely to be white, to have a higher salary, more experienced in previous elections, and more likely to have had a leadership position. The failure to diversify the set of candidates along racial and economic background likely reflects the consequences of screening on various competence and political engagement traits, as reflected in columns 2 and 3 of Table 2.

Regarding political outcomes, the last two columns of Table 2 suggest that *RenovaBR*-trained candidates got significantly more votes, ranked better within the party and were more likely to be elected than those who made it to the last screening round but were not selected.

Conditional analysis of selection While informative, the previous patterns consider each dimension separately. In Figure 1, we plot the coefficients of a multivariate regression using acceptance into the program as the dependent variable and candidate characteristics as regressors, conditional on applicants reaching Phase 2 of the admissions process. Three things stand out. First, some diversity goals of the *RenovaBR* program, such as promoting female candidates, show very strongly, even when we control for all other factors. Other goals, such as commitment to democracy, do not significantly determine selection once we control for race, education and all of the competence measures. Second, competence traits such as motivation, leadership, communication skills, and narrative were important for selection, even after controlling for logic test scores and schooling. Third, signals of genuine political interest, such as previous political affiliation and planning to run, also mattered to make the final cut. Appendix Table A1 presents the regression output in more detail.

[FIGURE 1 ON COEFFICIENTS OF ADMISSION ABOUT HERE]

4 The model

Consider a mass 1 of individuals from which Renova selects a much smaller mass into the program to be treated. To save on notation, we will assume the mass of the selected is so small that the population distribution remains a good approximation for the mass who are not selected –the controls, whom we will think of as retaining mass 1 and reflect no selection. Since it will be important to conceptually separate selection from treatment, let $S \in \{0,1\}$ indicate Selection into the program and $T \in \{0,1\}$ indicate Treatment.

Individuals are characterized by a (column) vector of observable traits $\mathbf{X} = \left[X^1, ..., X^n\right]' \in \chi$, and unobservable benefits from office and costs of running for office $B(\mathbf{X}, T)$ and $C(\mathbf{X}, T)$. Primes on vectors denote transposition. The vector $\mathbf{X}$, and only this vector, contains traits so defined that increasing levels (weakly) help electability. The weak version of the effect allows to include possibly neutral characteristics. Each trait $j = 1, ..., n$ has a highest element $\mathbf{X}^{jh}$ and a lowest element $\mathbf{X}^{jl}$. For simplicity we assume $\mathbf{X}^{jl} = \mathbf{X}^{l}$ and $\mathbf{X}^{jh} = \mathbf{X}^{h}$ $\forall j$. These highest and lowest values are possibly infinite. Because only the ratio C/B will matter for candidacy decisions, we normalize $B = 1$.

Each individual is characterized by her type $(\mathbf{X}, C) \in \chi \times \mathbf{R}$, where the typical realization $(\mathbf{x}, c)$ is drawn from the distribution $G^{S,T}_{\mathbf{X}C}(\mathbf{x}, c)$ with associated density $g^{S,T}_{\mathbf{X}C}(\mathbf{x}, c)$. We allow for negative costs to capture a taste for campaigning. Because the selected have a mass smaller than 1, the family $G^{S=1,T}_{\mathbf{X}C}(\mathbf{x}, c)$ denote functions that are suitably scaled so that they accumulate to 1. Let $G^{S,T}_{\mathbf{X}}(\mathbf{x})$ denote the marginal distribution for $\mathbf{X}$, and $G^{S,T}_{\mathbf{X}|C}(\mathbf{x}, c)$ denote the conditional distribution of $\mathbf{X}$ given $C = c$. The dependence of C on $(\mathbf{X},T)$ is captured by the distribution G, since we do not place any restriction on the correlation between $\mathbf{X}$ and C. We may refer to any given individual either by their type or an index $i = 1, ..., \infty$, so that individual i has traits $\mathbf{X}_i$.

There are three joint distributions to keep track of: the primitive cdf $G^{S=0,T=0}_{\mathbf{X}C}(\mathbf{x}, c)$ characterizing controls, the cdf $G^{S=1,T=1}_{\mathbf{X}C}(\mathbf{x}, c)$ characterizing the selected and treated, and the "intermediate" cdf $G^{S=1,T=0}_{\mathbf{X}C}(\mathbf{x}, c)$ of those who are selected but remain untreated. We will consider the (counterfactual) distribution $G^{S=1,T=0}_{\mathbf{X}C}(\mathbf{x}, c)$ to separate selection from treatment effects.

Individual applicants know their traits, but only learn the cost of running after treatment takes place. The timing is as follows: (i) Individuals apply; (ii) Renova decides on admission; (iii) Renova training (treatment) takes place; (iv) Individuals learn their cost of running, then choose to run for office or not (i.e., pick an action $R \in \{0,1\}$); (v) Elections are held.

4.1 Elections

Conditional on running, the probability of being elected for type $\mathbf{X}$ is $P(\mathbf{X},T) \in (0,1)$, a continuously differentiable, weakly increasing, and quasiconcave function of $\mathbf{X}$ with $\lim_{\mathbf{X}\to\mathbf{X}^l} P(\mathbf{X},T) = 0, \lim_{\mathbf{X}\to\mathbf{X}^h} P(\mathbf{X},T) = 1$ for any T. A candidate with type $(\mathbf{X},C)$ runs whenever $B.P(\mathbf{X},T) - C \geq 0$ or, having normalized $B = 1$, whenever

$$P(\mathbf{X},T) \geq C. \tag{1}$$

Since C has support in $(-\infty,\infty)$ and $P(\mathbf{X},T) \in (0,1)$ and is continuous, it follows immediately that the space $(\mathbf{X},C)$ is eventually partitioned in two regions: a set of types who run (relatively high $\mathbf{X}$, and relatively low C), and a set who do not.

The share of types who run for office out of all those with selection and treatment status (S,T) is given by,

$$E(R|S,T) \equiv \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T)} g_{\mathbf{X}C}^{S,T}(\mathbf{x},c)\, dc d\mathbf{x}.$$

We do not consider the possibility that selection status affects the function $P(\mathbf{X},T)$ separately from treatment. In other words, we cannot distinguish between a signaling vs a training effect from the program, which could be identified in the data if there were individuals who are known to have been selected but receive no treatment.

4.2 Selection

Selection into treatment is not random: *RenovaBR* prefers higher observables for at least some subset of those traits that weakly increase electability. *RenovaBR* may also prefer low cost individuals, which we encode through a preference for some observable that proxies for a lower conditional distribution of C. The function $G_{\mathbf{X}C}(\mathbf{x},c)$ captures the residual uncertainty about cost realizations that are only discovered late, so neither *RenovaBR* nor applicants know it at the time of selection. We assume that the correlation between $\mathbf{X}$ and C is such (either small enough or negative enough) that a preference for higher observables remains.

RenovaBR observes $\mathbf{X}$ directly and cares about a random taste shock ζ_i drawn from the continuous distribution $F(\zeta)$ with associated density $f(\zeta)$, satisfying $E(\zeta) = 0, E(\zeta)^2 = \sigma_\zeta$.

Importantly, the unobservable ζ does not matter for political outcomes. *RenovaBR* preferences are then captured by the index $\Gamma'\mathbf{X}_i+\zeta_i$, where the parameter vector Γ reflects the value *RenovaBR* places on the various observables. We assume *RenovaBR* uses a threshold rule and selects every individual such that $\Gamma'\mathbf{X}_i+\zeta_i \geq t$, such that t yields the desired mass of admits. To save on notation we normalize $t=0$. We impose,

Assumption 1. *RenovaBR selects candidates based on observables* $\mathbf{X}$ *only, and this selection is positive, meaning* $\Gamma \geq 0$ *with strict inequality for some element of* Γ.

Given the threshold rule, for any type $\mathbf{x}$ with marginal density $g_{\mathbf{X}}^{S,T}(\mathbf{x})$ and any type $(\mathbf{x},c)$ with conditional density $g_{\mathbf{X}|C}^{0,0}(\mathbf{x},c)$, only a share $1-F(-\Gamma'\mathbf{x})$ of that type are admitted. This implies, as we show in the Appendix,

Remark 1. *For any* $\Gamma>0$, *Renova selects applicants with higher observables, meaning that the marginal distribution for observables among the selected first-order stochastically dominates the marginal distribution for the controls. That is, the marginal and conditional distributions of* $\mathbf{X}$ *that describe those selected into treatment are shifted to the right.*

4.3 Treatment

We assume the training program has two effects, namely to lower the costs of running and increase electability. To formalize these effects, define $G_{C|\mathbf{X}}^{S,T}(c|\mathbf{x}) \equiv \int_{-\infty}^{c} g^{S,T}(y|\mathbf{x})\,dy$ as the conditional probability of a C realization below c when observables take the value $\mathbf{x}$. Since costs are a negative, we take stochastic dominance to mean lower values. Our assumption then is,

Assumption 2. ***Treatment lowers the cost of running*** *For every possible value of* $\mathbf{X}$*, the conditional distribution of* C *for the treated first-order stochastically dominates the distribution for the controls, while leaving the marginal distribution of* ***X*** *unaffected. More formally, treatment implies* $G_{C|\mathbf{X}}^{S,1}(c|\mathbf{x}) > G_{C|\mathbf{X}}^{S,0}(c|\mathbf{x})\ \forall (c|\mathbf{x}) \in R\times\chi$*, and* $G_{\mathbf{X}}^{S,1}(\mathbf{x}) = G_{\mathbf{X}}^{S,0}(\mathbf{x})$ $\forall \mathbf{x} \in \chi$.

The second treatment effect we consider is,

Assumption 3. ***Treatment increases electability*** *Formally,* $P(\mathbf{X},T=1) > P(\mathbf{X},T=0)$.

4.4 Empirical outcomes of interest

We study the effect of *RenovaBR* training on outcomes $y \in \{R, P\}$, i.e., running for office and electoral performance, respectively. The main magnitude of interest is the difference in expected outcomes between the treated and controls $E(y|S=1, T=1) - E(y|S=0, T=0)$. However, it is theoretically possible to consider several counterfactual expected outcomes. For example, $E(y|S=1, T=0)$ predicts outcomes for individuals who would be selected, but who are not treated, allowing for the separation of selection effects from treatment effects. Written in full, expected outcomes are $E(y|S, C(., T), P(., T'), R_C(C(., T''), P(., T''')))$, where the outcome is conditioned on a population of interest with selection status S, facing costs of running according to treatment status T, facing electoral prospects according to treatment status T' (possibly different from T), and who make running choices R_C as if facing costs of running based on treatment status T'' and electoral prospects based on treatment status T''' (where both T'' and T''' are possibly different from T or T'). In this paper we will not need to consider cases where $T''' \neq T''$. And for the purposes of this section, we only need to keep track of whether an expectation is conditional on running choices by treated individuals, control individuals, or is unconditional–each case denoted with $R_C \in \{R_0, R_1, R_U\}$.[10] Just like allowing $S \neq T$ helps separate selection from treatment effects, allowing for $T' \neq T$ helps separate treatment effects on electability versus on costs, and allowing for $T'' \neq T$ helps isolate effects that arise due to candidate self-selection decisions.

To save on notation, we obviate elements of $\{S, C(., T), P(., T'), R_C(C(., T''), P(., T''))\}$ in the conditioning set whenever they are set factually, i.e., consistent with treatment status and obviating them may not lead to confusion.

4.5 Outcome decomposition and theoretical predictions

The difference in expected outcomes $E(R)$ and $E(P)$ between the treated and the controls are, respectively,

$$\Delta E(R) = E(R|S=1, T=1) - E(R|S=0, T=0)$$
$$= \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x}, c)\, dc d\mathbf{x} - \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x}, c)\, dc d\mathbf{x}, \qquad (2)$$

[10] When the outcome of interest is electoral performance by the controls, expectations are conditional on the control individuals who run, so $R_C = R_0$. In the analogous case of the treated, $R_C = R_1$. But when the outcome of interest is running for office, expectations are taken over the entire population of treated or controls, i.e., the expectation is unconditional on running choices and $R_C = R_U$.

and

$$\Delta E(P) = E(P|S=1,T=1) - E(P|S=0,T=0)$$
$$= \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},1)\, g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\, dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\, dcd\mathbf{x}}$$
$$- \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0)\, g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\, dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\, dcd\mathbf{x}}. \tag{3}$$

Suitable additions and subtractions allow to decompose the total variation as detailed in Figure 2 below. The formalities of the decomposition are in Appendix B. At a first level, we can split $\Delta E(R)$ and $\Delta E(P)$ into, respectively, treatment and selection effects: $\Delta E(R) = TE(R) + SE(R)$ and $\Delta E(P) = TE(P) + SE(P)$. Under our assumptions, the selection effects arise because the selected have higher observables. Treatment effects arise because the treated have higher electability and lower costs of running. We further decompose the Treatment Effect on Running $TE(R)$ into two terms –one reflecting changes in decisions to run due to enhanced electability (labeled $EEE(R)$ in the table) and another reflecting changes in running decisions due to treatment lowering the costs of running (labeled $CRE(R)$– so that $TE(R) = EEE(R) + CRE(R)$. We can also decompose the treatment effect on performance $TE(P)$ into two terms. One is direct treatment effects on electability that affect performance (labeled $DEE(P)$) and the other is treatment effects that occur due to changes in running decisions (labeled $TE\text{-}R(P)$), yielding $TE(P) = DEE(P) + TE\text{-}R(P)$. The latter $TE\text{-}R(P)$ occurs because of the two ways in which treatment operates over candidate self-selection. By enhancing electability, treatment alters who runs, which has effects on average performance (this component of $TE\text{-}R(P)$ is labeled $EEE\text{-}R(P)$). And decisions to run are also affected as treatment lowers the costs of running, which again affects average performance (this component of $TE\text{-}R(P)$ is labeled $CRE\text{-}R(P)$).

[FIGURE 2 DECOMPOSITION HERE]

The shaded cells in $\Delta E(P)$ correspond to sample composition effects – note that these combine *RenovaBR*-selection effects $SE(P)$ and candidate self-selection effects arising from treatment $TE\text{-}R(P)$. The cells with labels in bold correspond to effects that are unambiguously positive, as shown in the following,

Proposition 1. *(i) Under Assumption 1 (positive selection on observables), the sign of selection effects on decisions to run* $SE(R)$ *, and therefore that of* $\Delta E(R)$*, is ambiguous. In* *contrast, selection effects on electoral performance are unambiguously positive:* $SE(P) > 0$.

(ii) All else equal, under Assumption 2 (treatment lowers the cost of running) the costs-of-running effect on decisions to run is positive, implying the treatment effect on decisions to run is positive as well ($CRE(R) > 0$ *and* $TE(R) > 0$*).*

(iii) All else equal, under Assumption 3 (treatment increases electability: $P(\mathbf{X}, T = 1) > P(\mathbf{X}, T = 0)$*), the enhanced-electability effect on running is positve, implying the treatment effect on running is positive as well (*$EEE(R) > 0$ *and* $TE(R) > 0$*).*

(iv) All else equal, under Assumption 3 (treatment increases electability: $P(\mathbf{X}, T = 1) > P(\mathbf{X}, T = 0)$*),the direct effect of enhanced electability on performance is positive (*$DEE(P) > 0$*), but the composition effect from decisions to run on performance* $TE\text{-}R(P)$ *has ambiguous sign depending on the distribution* $G_{\mathbf{X}C}(\mathbf{x}, c)$*. As a result, on average, among those running for office the treated may have higher or lower electoral performance, so the sign of* $\Delta E(P)$ *is ambiguous.*

All proofs are in Appendix C. The substantive message is that treatment effects on candidacy are positive: lower costs and better electoral prospects induce more individuals to run (parts (ii)-(iii)). Selection effects on candidacy have an ambiguous sign, however (part (i)). If, say, positively selected individuals have higher costs, they may run less often despite the fact that, being positively selected, they stand a better chance of winning – such effects cannot be ruled out without placing restrictions on the distribution $G_{\mathbf{X}C}(\mathbf{x}, c)$. For electoral performance, the pattern reverses: selection effects are positive (part (i)) and treatment effects are ambiguous despite the fact that the direct effect of treatment is to enhance electability (part (iv)). The ambiguity arises because the treated may make self-selection decisions that lower their average electoral performance. For example, treatment may lower costs and induce candidacy by disproportionately more individuals with relatively low observables, who then go on to have lackluster electoral performance.

Regression analysis controlling for selection effects should expect to find clearcut treatment effects on decisions to run, without clearcut predicted effects on electoral performance. However, if there is an empirical way to split the treatment effect on performance into its direct and self-selection subcomponents ($DEE(P)$ vs $TE\text{-}R(P)$), the direct electability effect $DEE(P)$ should empirically show as unambiguously positive. In Section 7 we show how a decomposition exercise can achieve that separation.

5 Empirical Design

In this section we explain our approach to identify the treatment effect of the program by controlling for selection effects. In Section 7 we study the selection effects themselves, and assess their contribution relative to treatment.

5.1 Selection on observables

Consistent with the theory in Section 4, our first approach is to assume that selection was based on observable characteristics, and control for those characteristics in our empirical specification. If unobservables such as ζ_i in Section 4 play a role in selection, they are still of no concern because they do not matter for outcomes. The assumption of selection on observables is plausible in our context. *RenovaBR* personnel based admission decisions on a rich set of applicant characteristics that were measured and recorded. As we described in the data section, we have access to all of those records.

Under these assumptions, the data generating process for outcome y_i is represented by,

$$y_i = \alpha_0 + \alpha_1' \mathbf{X}_i + \alpha_2 T_i + \alpha_3' \mathbf{X}_i T_i + \varepsilon_i,$$

where $\mathbf{X}_i$ and T_i respectively capture observables and treatment status for individual i and ε_i is a zero-mean random shock. Then, our default regression equation will be,

$$E\left[y_i | \mathbf{X}_i, T_i\right] = \alpha_0 + \alpha_1' \mathbf{X}_i + \alpha_2 T_i + \alpha_3' \mathbf{X}_i T_i. \tag{4}$$

After deploying our default approach, we will consider whether outcome-relevant unobservables played a role in selection, and correct inference if necessary. To achieve this, we augment the selection framework first laid out in Section 4 to incorporate standard sample selection correction methods, suitably adapted based on our detailed institutional knowledge of the admission process. We describe this extension next.

5.2 Selection on unobservables

Assume that *RenovaBR* cares about some unobservable we will call "valence" that affects political outcomes. We postulate a data generating process for outcome y_i given by,

$$y_i = \alpha_0 + \alpha_1' \mathbf{X}_i + \alpha_2 T_i + \alpha_3' \mathbf{X}_i T_i + \alpha_4 v_i + \alpha_5 T_i v_i + \varepsilon_i,$$

where valence v_i is a randomly drawn unobservable that is by definition uncorrelated with observables. Treatment may interact with observable and unobservable characteristics. To save on notation, in what follows we abstract from the interaction terms and only include them if needed.

In Section 4, under the selection-on-observables assumption, the fine details of the admission process could be left unspecified. Here we closely track the description of the admissions process in Section 2. Thus, we consider up to two judges $j = 1,2$ who assess each applicant's characteristics before issuing a recommendation $R_i^j \in \{0,1\}$ to respectively deny or admit applicant i (not to be confused with the running decision R_i by individual i). The judges observe characteristics $\mathbf{X}_i$ perfectly, but observe valence with some noise: judge j observes a signal $\theta_i^j = v_i + \xi_i^j$ on individual i, with ξ_i^j a zero-mean noise term. Renova leadership take into account observable characteristics and judge recommendations to form an expectation of valence before making an admission decision. In Appendix D we formalize the augmented selection process and show that the expectation of the outcome conditional on $(\mathbf{X}_i, T_i, \mathbf{R}_i)$ (where $\mathbf{R}_i \equiv [R_i^1, R_i^2]$) is,

$$E[y_i|\mathbf{X}_i, T_i, \mathbf{R}_i] = \alpha_0 + \alpha_1'\mathbf{X}_i + \alpha_2 T_i + \alpha_3 E[v_i|\mathbf{X}_i, \mathbf{R}_i], \tag{5}$$

where the last term $E[v_i|\mathbf{X}_i, \mathbf{R}_i]$ captures the variable that would be omitted in a naive OLS regression if there is selection on unobservables.

In the same appendix we show that under our formalization of the selection process (which includes the assumption of normality of all random terms) the term $E[v_i|\mathbf{X}_i, \mathbf{R}_i]$ can be incorporated in regression analysis through a control function. In the case where a single judge evaluates an applicant, this term takes the familiar form of the inverse Mill's ratio,

$$E[v_i|\mathbf{X}_i, \mathbf{R}_i] = E[v_i|\mathbf{X}_i, \mathbf{R}_i]_{1J} = \hat{\sigma}\frac{\phi\left(\frac{-\mathbf{\Gamma}^{1\prime}\mathbf{X}_i}{\sigma_{u^1}}\right)}{R_i - \Phi\left(\frac{-\mathbf{\Gamma}^{1\prime}\mathbf{X}_i}{\sigma_{u^1}}\right)} \equiv \hat{\sigma}\lambda\left(\mathbf{X}_i, R_i^1\right), \tag{6}$$

where $1J$ indicates that a single judge is in charge of recommending admission, ϕ and Φ are respectively the standard normal density and cumulative distribution functions, $u_i^j \equiv E\left[v_i|\theta_i^j\right] + \zeta_i^j$ is the general unobservable driving judge recommendations, and $\hat{\sigma}$ is a function (detailed in the appendix) of parameters of the various distributions of unobservables.

In the case where two judges evaluate an applicant, the term $E[v_i|\mathbf{X}_i, \mathbf{R}_i]$ is a sum of generalized Mill's ratios for judges 1 and 2 involving the first moments of a truncated bivariate normal distribution,

$$\begin{aligned} E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right] &= E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]_{2J} = \tilde{\sigma}^a E\left(u_i^1|\mathbf{X}_i,\mathbf{R}_i\right) + \tilde{\sigma}^b E\left(u_i^2|\mathbf{X}_i,\mathbf{R}_i\right) \\ &\equiv \tilde{\sigma}^a \lambda^1\left(\mathbf{X}_i,\mathbf{R}_i\right) + \tilde{\sigma}^b \lambda^2\left(\mathbf{X}_i,\mathbf{R}_i\right), \end{aligned} \tag{7}$$

where $2J$ indicates two judges are active, and $\left\{\tilde{\sigma}^a,\tilde{\sigma}^b\right\}$ are functions of distribution parameters detailed in the appendix.

Using $I(.)$ as an indicator function, our empirical specification becomes,

$$E\left[y_i|\mathbf{X}_i,T_i,\mathbf{R}\right] = \alpha_0 + \alpha_1'\mathbf{X}_i + \alpha_2 T_i + \alpha_3 \left\{ \begin{array}{c} I(1J).\hat{\sigma}\lambda\left(\mathbf{X}_i,R_i^1\right) \\ +\left(1-I(1J)\right)\left[\tilde{\sigma}^a\lambda^1\left(\mathbf{X}_i,\mathbf{R}_i\right)+\tilde{\sigma}^b\lambda^2\left(\mathbf{X}_i,\mathbf{R}_i\right)\right] \end{array} \right\}. \tag{8}$$

A typical concern with the control function approach is that the correction term is a function of selection into treatment and observables, so that term would be collinear with the regressors $\mathbf{X}_i, T_i$, unless the selection model introduces nonlinearity or an excluded instrument. In our case, the treatment variable T_i is not identical to the recommendation of judges, but we still rely on instruments for judge recommendations in order to introduce further variation in the control function terms.

5.2.1 Instruments

In our control function approach, we adopt a judge leniency design, a method widely used in the economics of crime literature (Frandsen et al., 2023). This design takes advantage of variation in the leniency of judges who review aspirants. To implement this design, we include indicators for the 16 judges active in *RenovaBR* when estimating the first-stage probit on whether a judge recommended the aspirant for admission. Figure 3 shows that judges vary markedly in their propensity to recommend aspirants for admission.

[FIGURE 3 APPROVAL RATES BY JUDGE HERE]

The validity of these instruments relies on several assumptions. First, aspirants must have been assigned to judges in a quasi-random manner. As discussed in Section 2, *RenovaBR* assigned files to judges indiscriminately. Appendix Table A2 supports this claim by reporting the results of a multinomial logit regression, where the judge assigned to each aspirant is the outcome variable, and the aspirant's observable characteristics are the regressors.[11] While a few characteristics, such as the aspirant's gender and their score on the logic test, are statistically significant, aspirant observables have almost no predictive power (pseudo-R2 of 0.0284), which is consistent with the assumption of exogeneity.

Additionally, Figure 4 plots the average predicted probability from our multinomial logit model against the share of aspirants each judge recommended to *RenovaBR* for approval. Again consistent with exogeneity, no discernible association exists.

[FIGURE 4 JUDGE SEVERITY AND PREDICTED ASSIGNMENT HERE]

The second assumption is that judges influenced aspirant outcomes solely through their recommendations on admission to the program. In our context, this exclusion restriction likely holds because aspirants never interacted directly with judges. Judges evaluated aspirants based only on recorded information (personal information, test results, and videos), and the only feedback aspirants received was whether they were admitted into the program.

A further requirement is monotonicity, meaning that judges can be (weakly) ranked in terms of leniency; if one judge admits a set of aspirants, a stricter judge must admit only a subset of those aspirants. We evaluate monotonicity in two ways. First, we explicitly test for violations of monotonicity using the procedure recently proposed by Frandsen et al. (2023). Second, we allow the effect of our instruments to vary based on a subset of aspirant characteristics. This relaxes our monotonicity assumption for this set of observable characteristics.

Weak instruments are a potential concern, particularly when we allow the effects of these instruments to vary according to aspirant characteristics. To address this issue, we examine the robustness of our results by reducing the effective number of judges. Specifically, we group judges into five categories based on their leniency rates. This approach helps mitigate the potential weakness of individual instruments by consolidating judges with similar leniency profiles.

[11] To be conservative, in these regressions we disaggregate the ideological attitudes into all of their subcomponents.

6 Results: the effects of treatment

6.1 Effects on candidacy

In this section, we examine whether participation in the *RenovaBR* program made individuals more likely to run for local office in the 2020 elections. The question of candidacy is important because it represents the act of political entry. Even if these candidates are not ultimately elected, they contribute to the political discourse during the campaign and influence the choice set facing voters.

6.2 Selection on observables

Our first approach to studying the effects on candidacy takes advantage of the rich dataset available to us. In Table 3, we present the impact of admission into *RenovaBR* on the decision to run for office. The dependent variable indicates whether a candidate ran for city council in the 2020 elections, while the key independent variable reflects whether a candidate was accepted into *RenovaBR*'s training program. We explore these effects using various specifications and estimation methods, all under the assumption of selection based on observable characteristics. The control group comprises aspirants that reached Phase 2 but were not admitted. We report intention-to-treat estimates since not everyone who was accepted enrolled or completed the program.

We believe the assumption of selection on observables is reasonable because the admission process was designed to rely on recorded measures, all of which we have in our data. Columns 1 through 5 of the table report OLS coefficients, incorporating basic demographic controls such as gender, age, race, income, religion, and education (see the table notes for a full list of controls). In each subsequent column, we introduce additional controls to account for differences in political intentions, competence, and ideology. Column 5 includes municipal fixed effects, enabling a comparison between *RenovaBR* candidates and non-selected candidates who decided to run for the same position in the same municipality. We include this specification for completeness, but a within-municipality estimate may be less reliable: the presence of a *RenovaBR* candidate in a municipality may affect the votes obtained by their direct rivals in the control group. Therefore, our preferred specification is that in column (4) which includes all observable traits as controls.

[TABLE 3 CANDIDACY HERE]

Our findings indicate that participation in *RenovaBR* significantly increased the likelihood of an aspirant running for office. For instance, in column 4, we observe that *RenovaBR* increased the probability of running by 17.8 percentage points, compared to a control group mean of just 16 percent. These results are statistically significant at the 1 percent level and are consistent across various specifications.

In column 6, we relax the linearity assumption by using entropy balancing (Hainmueller, 2012) to estimate the effects of the program. This method uses maximum entropy to reweight the covariate distributions, ensuring that the treatment and control groups are balanced in terms of the first and second moments of the covariates. With this approach, our estimated effect increases slightly to 19.5 percentage points.

Column 7 employs a double lasso method to estimate the treatment effect. This approach is beneficial given the large number of covariates, as it helps select the appropriate control variables without needing to pre-specify them, thereby preventing overfitting. The results using this method are similar to those found with other approaches, further confirming the robustness of our findings.

The effect of covariates on the decision to run for office is of independent interest. As shown in Appendix Figure A4, individuals who are evangelical, have higher leadership and communication skills, and a stronger initial political intent, are more likely to run.

6.2.1 Selection on unobservables and control function approach

First-Stage Estimation. In this section, we consider the possibility that the selection of *RenovaBR* students may be influenced by factors visible to *RenovaBR* but not captured by their recorded measures, making such factors unobservable to the econometrician. To address this concern, we adopt a control function approach that leverages the structure of selection, as detailed in equation (8) in Section 5. The approach further relies on the fact that judges vary in their leniency and that aspirants were allocated to judges at random.

Appendix Table A3 presents the first-stage estimates of our control function approach. The dependent variable is an indicator of whether a judge recommends an aspirant for training. Column 1 includes all 16 judge indicators, while Column 2 reduces this to five indicators based on the severity of judges' recommendation rates. Column 3 interacts all 16 judge indicators with covariates that were not balanced across treatment vs. control, and Column 4 performs the same interaction using the five severity-based indicators.

The results suggest that judges generally favored recommending female, younger, and single aspirants. They also tended to recommend those with strong political intentions, measured by previous membership in a political party and intentions to run in the upcoming election. Not surprisingly, aspirants' competence and ideology correlate with their likelihood of being recommended. For example, the coefficient on Leadership indicates that a one standard deviation increase in the Leadership score is associated with a 17 percentage point increase in the likelihood of receiving a recommendation. Ideology appears to play a limited role in judges' recommendation decisions, though there is some evidence that judges prefer aspirants who show stronger support for democracy.

When using a judge-leniency design, two important considerations are the possibility of weak instruments due to the presence of many judges and concerns about monotonicity. As shown at the bottom of the table, our instruments do not appear to suffer from either issue. In Column 1, the F-statistic for all 16 judge indicators is 34.89. When we use only the five severity-based indicators, the F-statistic increases to 111.46. The predictive power of our instruments remains robust even when they are interacted with covariates that were imbalanced at assignment, as seen in Columns 3 and 4.

Additionally, we conduct the test for monotonicity suggested by Frandsen et al. (2023). For both sets of instruments (judge indicators and leniency scores), we fail to reject the hypothesis that the instruments violate the monotonicity assumption implicit in this design.

Generalized Mills Ratio Table 4 presents the effects of *RenovaBR* on the decision to become a candidate, using a control function approach. In Column 1, we provide OLS estimates based on the preferred specification from Column 4 of Table 3, serving as a baseline for comparison. Column 2 shows estimates using judge indicators as excluded instruments, while Column 3 uses judge severity indicators. In Column 4, we interact the judge indicators with covariates that significantly differ across judge assignments (seen in table A2). Similarly, Column 5 reports estimates where the judge severity indicators are interacted with these same unbalanced covariates.

[TABLE 4 CANDIDACY CONTROL FUNCTION HERE]

Panel A includes all Phase 2 candidates evaluated by a single judge, while Panel B covers those evaluated by two judges. For Panels B and C, the Mills Ratios for each judge are calculated by assuming a correlation $\rho = 0.9$ between the generalized unobservables $u_i^j \equiv E\left[v_i|\theta_i^j\right] + \zeta_i^j$ driving judge recommendations. We demonstrate in Appendix Tables A4 and A5 that our results are not sensitive to this assumption. (In Panel C, which combines both samples, we assign a value of zero to missing data for aspirants evaluated by only one judge and include an indicator to denote this.)[12] The results in Appendix Tables A4 and A5 show that the Mills Ratios matter most when ρ is high. This suggests that, other than observables, what drives judge recommendations is not the idiosyncratic judge tastes ζ^j but a correlated signal. Because the signals are highly collinear, only one of the ratios shows up as significant.

Overall, the control function approach produces estimates that are statistically indistinguishable from our previous ones. For instance, in Panel C, which includes all Phase 2 aspirants, we estimate an effect of 0.173, compared to our earlier estimate of 0.178. This consistency holds whether we use all 16 judge indicators, the 5 judge severity indicators as instruments, or when include the interactions between the judge variables and the imbalanced covariates.

Under our assumption of normality in the selection equation, we find minimal evidence of endogeneity. The Mills ratio is statistically significant only in the two-judge sample (Panel B), but even then, the difference in estimated effects between the OLS and control function approaches remains statistically insignificant.

Given these findings, we will maintain the assumption of selection on observables when analyzing other electoral outcomes. But the interested reader can refer to the Appendix to find a replication of all of our results under the control function approach.

6.3 Effects on electoral outcomes

While fostering new candidacy is a crucial step toward reshaping the political class, it is also important to see if *RenovaBR* candidates can get elected. In Table 5, we examine the impact of *RenovaBR* on several measures of electoral performance, focusing on Phase 2 individuals who chose to run. The electoral outcomes we consider include the vote share (Panel A), within-party ranking by votes (Panel B), vote total as a share of the electoral quotient (Panel C), whether their votes surpassed 0.2 of the electoral quotient (Panel D), getting elected (Panel E), and the total votes their party received in the candidate's municipality

[12] We assume $\rho = 0.9$ because this gives selection on unobservables the strongest chance to affect outcomes. The default approach to compute the Mills Ratios for two judges is to estimate a bivariate probit for each pair of judges, which would jointly yield estimates for judge preferences Γ^j and for ρ. Because the number of aspirants evaluated by each judge pair is relatively small, we estimate judge probits individually, and then compute the Mills Ratios for each pair of judges for *all* possible values of ρ and select the value that yields the most significant Mills Ratios.

minus the candidate's own votes (Panel F).[13]

[TABLE 5 ELECTORAL OUTCOMES HERE]

Our findings show that *RenovaBR* improved the electoral performance of its trainees. For example, using our preferred specification in Column 4, *RenovaBR* increased the vote share by 0.16 percentage points. Although this might appear modest, it reflects a 29 percent increase relative to the control group, whose average vote share was only 0.55 percent. These additional votes were not concentrated in municipalities where votes might matter less. The additional votes of *RenovaBR* candidates also represented a higher share of their municipality's electoral quotient and made them more likely to clear the minimum threshold that *RenovaBR* considers for a candidate being competitive. Similarly, *RenovaBR* candidates improved their within-party rankings by 2.5 positions and were more likely to get elected. We estimate that *RenovaBR* increased the probability of election by 4.3 percentage points, a roughly 40 percent increase over the control group.

Panel F also suggests some evidence of spillover effects for political parties. Parties that ran a *RenovaBR* candidate saw their vote share increase by 1.6 percentage points compared to parties with untrained candidates. However, this spillover effect is less consistent across different econometric specifications. The fact that, if anything, within-party spillovers are positive, implies that parties have no reason to restrict entry to *RenovaBR* candidates. Moreover, during the 2020 election there were nearly 30 parties competing in an open list proportional representation system, which reduces the role of parties as nomination gate-keepers. Parties almost never field lists that reach the maximum number of allowed candidates. Consistent with this, we had no reports of parties playing a restrictive gate-keeping role in allowing candidates to run.

6.3.1 Conditional outcomes and self-selection bias

We estimated the effects of *RenovaBR* on electoral outcomes conditional on Phase 2 aspirants who ran for office. Since *RenovaBR* influenced who entered the race, the sample is endogenously selected based on the treatment. We address this issue in three ways.

Our first approach is to run regressions unconditional on running decisions (see Appendix

[13] The electoral quotient is defined as total valid votes divided by total number of available seats. For example, if there are 100,000 valid votes in a municipality and 20 seats in its City Council, then $EQ = 5,000$. Each party gets seats in proportion to its votes relative to the EQ ($Partyseats = Partyvotes/EQ$). And each candidate must get at least $0.1 * EQ$ votes to be elected. In our example, a candidate with less than 500 votes cannot get elected. *RenovaBR* used the threshold of $0.2 * EQ$ to consider a candidate competitive.

Table A7), where missing values are replaced with zeros. Our preferred specification in Column 4 again shows a positive effect on all electoral outcomes. The effect on vote share, at 0.145 percentage points, is statistically indistinguishable from the 0.16 percentage points in the conditional regressions. The effect on election rates is a point down at 3.3 percentage points.

The second approach is to compute bounds as proposed by Anagol and Fujiwara (2016). The idea is that observations correspond to either Always Takers, who run in both the treatment and control conditions, or to Compliers, who only run under treatment. (In terms of our model, the former are all types ($\mathbf{x}$,c) lying under the curve $P(\mathbf{X}, T = 0)$, and the latter are all types between the curves $P(\mathbf{X}, T = 0)$ and $P(\mathbf{X}, T = 1)$.)

Lacking observations for how Compliers would fare in the control if forced to run, one can produce estimates of the treatment effect under different assumptions about that performance. One assumption is that Compliers would fare as well as Always Takers and Compliers do under treatment, the other is that they would obtain zero votes and not be elected. Under each of those assumptions we estimate bounds of [0.06, 0.36] for the vote share effect and of [0.01, 0.08] for election rates. Both of these intervals exclude zero.

The third approach to deal with candidate self-selection is developed in Section 7. In that section, we provide a decomposition of treatment and selection effects that allows us to estimate electoral treatment effects purged of candidate self-selection decisions.

6.3.2 Selection on unobservables and observable valence

Consistent with the assumption that selection on observables is reasonable, we relegate to the appendix our results on electoral outcomes under the control function approach. But two aspects are worth mentioning here. First, Appendix Table A6 shows coefficients that are statistically indistiguishable from those just shown in Table 5. The message remains that *RenovaBR* increased the electoral performance of its trainees.

The Mills Ratio terms in Table A6 are, for both the 1- and 2-judge samples, statistically insignificant and close to zero. This suggests that the observables capture well all aspects that matter to electability – in other words, we have rendered valence observable.

6.4 Effects on campaign finance

The electoral success of *RenovaBR* candidates raises the question of what aspects of the training contributed to their improved performance. While it's challenging to pinpoint the exact factors, one potential channel could be campaign financing. As discussed in Section 2, fundraising was a key module in the training program. Given the critical role of money plays in elections, and local Brazilian elections in particular (Avis et al., 2022), improved fundraising skills could explain some of the electoral gains we observe.

In Table 6, we analyze the effects of *RenovaBR* on campaign revenues for Phase 2 aspirants who ran for office. Column 1 reports the impact on total revenue, showing that, on average, *RenovaBR* candidates raised an additional BRL $4,617 over other candidates. In log terms, this translates to a 34 percent increase (see Column 2). Columns 3 through 8 explore the sources of these funds, distinguishing between self-financing, private donations, and party donations. Columns 3 through 5 use a binary dependent variable to indicate whether a candidate received a donation from each respective source, while Columns 6 through 8 show the share of funds from each source.

[TABLE 6 CAMPAIGN FINANCE HERE]

Our findings reveal that the additional funds *RenovaBR* candidates raised came exclusively from private donations, which aligns with one of the objectives of the training. *RenovaBR* candidates were 9.8 percentage points more likely to receive private donations, representing above a 13 percent increase over the control baseline.

This increase in private donations appears to have led to some substitution effects, as *RenovaBR* candidates received a lower share of party funds. We found no significant changes in self-financing. Overall, these results suggest that part of the electoral success of *RenovaBR* candidates can be attributed to what on net was higher fundraising.

6.5 Encouragement or discouragement?

An alternative interpretation of the results on candidacy is not that the program had a positive treatment effect, but that rejection had a discouragement effect. To explore the likelihood of a discouragement effect, we compare candidacy outcomes between two groups of ultimately rejected aspirants. One is the group of individuals who reached Phase 2 but were turned down to participate in the program. The other group is those who reached Phase 1 but did not advance to Phase 2.

We see two main arguments for discouragement effects. One argument relies on a behavioral effect, such as demoralization. This effect is likely stronger among aspirants who reached Phase 2, and thought themselves close to admission, and failed in the final stretch. If this force is present, all else equal, aspirants who reached Phase 2 should run for election less often than those who only reached Phase 1. Another argument is informational: those who are rejected early may conclude they stand no chance in politics, and run for office less often than those who reached Phase 2.

To approximate "all else equal" conditions, we exploit the fact that advancement into Phase 2 depended on the score in a logic test. In addition, a threshold in the test score provides a discontinuity in the probability of advancement, as shown in the left panel of Figure 5. Those who score above the threshold are more likely to move on to Phase 2. The right panel of the same figure shows that, close to the test-score discontinuity, aspirants eventually run for office at the same rate, regardless of whether they reached Phase 2 or were held up in Phase 1. This finding is compatible with the absence of discouragement effects. Alternatively, of course, the null finding could reflect the relatively unlikely case that the behavioral and informational discouragement effects exactly cancel out.

[FIGURE 5 DISCOURAGEMENT HERE]

Another argument against a discouragement effect takes advantage of the simple fact that a program is unlikely to further discourage an individual who is already reluctant to run. Yet, there is a positive treatment effect even on individuals who do not initially intend to run, taking candidacy rates from near zero to above 20%.

6.6 Effects of *RenovaBR* on Political Entry and Selection

Research in political economy has investigated how the attributes of elected officials influence representation and governance (see Dal Bó and Finan (2018) for references). Thus, one of the key questions in political economy research is who decides to run for office. Here we examine the pool of candidates across three broad dimensions: demographics and social background, competence, and ideology. For each dimension, we split our analysis into two parts. First, we explore whether candidates from the *RenovaBR* program are more likely to possess certain characteristics compared to those who did not participate in the program but still became candidates. Specifically, we compare aspirants admitted to *RenovaBR* with those who reached Phase 2 but were not admitted. Our comparison relies on regressions where candidate traits are the dependent variables, the independent variable is admission into the program, and the sample is conditional on running. These results are reported in Panel A of the three tables in this section.

Next, we investigate whether the distinctive characteristics of *RenovaBR* candidates are a result of the program's selection process or if the program spurred individuals with certain traits to run. We address this by running regressions of candidacy on the *RenovaBR* treatment, including interactions between treatment and each individual trait. These regressions are presented in Panel B of each table. If no significant interaction exists, it suggests that the program did not deferentially motivate aspirants with that trait to run, meaning any contribution to altering the candidate pool came from *RenovaBR*'s initial screening.

Demographics and social background *RenovaBR* aims to diversify the political class. The data suggest that the program has had mixed success in this regard. Panel A of Table 7 reveals that candidates who completed the *RenovaBR* program were, on average, about two and a half years younger than their counterparts, with a baseline age of 38. Moreover, *RenovaBR* candidates were 13 percentage points more likely to be female, with a baseline of 15 percent. However, there were no significant differences in terms of race, income, or party affiliation, although *RenovaBR* candidates were generally more educated and less likely to be married. While the program has not reversed the underrepresentation of racial minorities, it has made strides in reducing the underrepresentation of women. Panel B shows no interaction between treatment and socioeconomic characteristics, indicating that *RenovaBR*'s younger and more female candidate profile is due to recruiting aspirants with these traits rather than differentially activating them into candidacy.

[TABLE 7 POLITICAL ENTRY - DEMOGRAPHICS HERE]

Competence Panel A of Table 8 compares the competence of *RenovaBR* candidates to those who did not make it into the program but still ran for office. *RenovaBR* candidates appear stronger across all competence characteristics measured. These effects are substantial, with *RenovaBR* trainees scoring approximately 0.7 standard deviations higher in the six competence measures used to decide which Phase 2 aspirants would be admitted (columns (3) to (8)). The effects are smaller but still significant for the two competence measures used to screen aspirants in Phase 1 (Logic and General Knowledge). This suggests that the strength of screening is what shaped the candidate pool.

The importance of screening is confirmed in Panel B, which shows that participation in the program did not differentially spur candidacy by more competent individuals. Taking Panels

A and B together, we conclude that *RenovaBR*'s screening, not heterogenous treatment, is responsible for the stronger competence of its candidates.

[TABLE 8 POLITICAL ENTRY - COMPETENCE HERE]

Ideology In terms of ideology, *RenovaBR* aims to promote politicians committed to democracy without partisan bias. Panel A of Table 9 shows that *RenovaBR* candidates are more likely to support democracy, redistribution, and progressivity. Similar to the findings on demographics and competence, *RenovaBR* did not differentially spur candidacy based on ideological traits (see Panel B), indicating that the pro-democracy bias among *RenovaBR* candidates resulted from screening rather than program intervention.

[TABLE 9 POLITICAL ENTRY - IDEOLOGY HERE]

Finally, we examine whether *RenovaBR* candidates exhibit partisan tendencies due to their progressive and redistributive views. Table 10 investigates whether *RenovaBR* candidates are more likely to belong to left-leaning, right-leaning, or larger parties, or to parties formed after 2000. The data show no evidence of *RenovaBR* candidates being more likely to align with any particular type of party, further supporting the program's goal of avoiding partisan bias.

[TABLE 10 PARTY CHOICES HERE]

Political Selection Beyond changing the composition of the candidate pool, the next question is whether *RenovaBR* also influenced which candidates were elected. As mentioned earlier, *RenovaBR* succeeded in getting a small number of its trainees elected — 87, to be exact. This raises the question of how these elected Renova trainees differ from the Phase 2 aspirants who were not selected for the training but still managed to get elected.

To explore this, we replicate the analysis from Tables 7 through 9, but now focus on comparing the traits of elected *RenovaBR* candidates with those of non-*RenovaBR* aspirants who were also elected. Although the sample size limits the ability to make statistically definitive conclusions, we observe some suggestive patterns. Specifically, *RenovaBR* appears to have helped elect more women (see Table 11), more capable politicians (see Table 12), and individuals with more progressive views (see Table 13). Again, the reason lies in *RenovaBR* having screened for individuals with those characteristics.

If *RenovaBR*'s objectives are to enhance the competence of the political class and improve the representation of underrepresented groups, it seems to be making some progress toward achieving these goals.

[TABLES POL. SELECTION HERE – 11- DEMOGRAPHICS, 12-COMPETENCE, 13-IDEOLOGY]

7 Separating treatment and selection effects

If the assumption of selection on observables is valid, then a regression of y on T including the appropriate controls produces an unbiased estimate of the treatment effect. That analysis remains limited in two ways, though. First, that analysis says nothing about the extent to which treatment contributes to raw differences in outcomes relative to the selection effects induced by *RenovaBR* screening. Second, the treatment effects on electoral outcomes $TE(P)$ contain composition effects stemming from candidate self-selection ($TE\text{-}R(P)$) that make it difficult to evaluate whether treatment improved electability. This section offers an approach to overcome both limitations, based on the construction of predictors and Oaxaca-Blinder decompositions (OBD).

7.1 Predictors

Let $\mathbf{X}_i^{T^M,R_C^M}$ denote observables of those with selection and treatment status $T^M \in \{0,1\}$, and self-selection patterns arising from running choices $R_C^M \in \{R_0, R_1, R_U\}$; let $u_i^{T^M,R_C^M}$ be a zero-expectation random disturbance. Define $\hat{\beta}^{T^M,R_C^M}$ to denote the estimate stemming from the (possibly nonlinear) regression,

$$y_i^{T^M,R_C^M} = f\left(\beta' \mathbf{X}_i^{T^M,R_C^M} + u_i^{T^M,R_C^M}\right).$$

The label M tracks the sample on which we estimate the model. The outcome of interest is $y_i \in \{R, P\}$ for individual i. Let $\left\{T^D, R_C^D\right\}$ characterize a sample of subjects for whom a prediction is made. T^D tracks whether the subjects are treated ($T^D = 1$) or controls ($T^D = 0$), and $R_C^D \in \{R_0, R_1, R_U\}$ tracks their running choices.[14] For a sample with observables $\mathbf{X}^{T^D,R_C^D}$, define the predictor,

[14]The case $R_C = R_0$ is one where only the controls who ran are considered and where the treated are considered by weighting their observables to resemble the profile of those who would run if they were placed in the control. The case $R_C = R_1$ is one where only the treated who ran are considered, and where controls are considered by weighting their observables to resemble the profile of those who would run if they were placed under treatment.

$$E_{\hat{\beta}_y^{T^M,R_C^M}}\left(y|\mathbf{X}^{T^D,R_C^D}\right); y=R,P; T^h\in\{0,1\}, R_C^h\in\{R_0,R_1,R_U\}, h=M,D,$$

that projects expected outcomes for a sample with observables $\mathbf{X}^{T^D,R_C^D}$ using the coefficients corresponding to a sample with observables $\mathbf{X}^{T^M,R_C^M}$. These predictors are the building blocks of the decompositions detailed below.

7.2 Predictor-based decompositions

Given selection on observables, we can perform decompositions in the spirit of Oaxaca and Blinder and identify the selection and treatment effects formalized in Section 4. The decompositions are expressed in terms of the predictors defined above. The key message is that a standard OBD between effects driven by observables versus coefficients correctly separates treatment from *RenovaBR*-selection effects on decisions to run. When it comes to electoral performance, a standard OBD lumps *RenovaBR*-selection effects and candidate self-selection effects that are a result of treatment. Therefore, we use the detailed structure of our predictors to conduct the appropriate decomposition between treatment and *RenovaBR*-selection effects.

The difference in decisions to run between individuals in the treatment and control conditions is $\Delta E(R) = E(R|S=1, T=1, R_C=U) - E(R|S=0, T=0, R_C=U)$, or,

$$\begin{aligned}
& \Delta E(R) \\
= \; & E\left(R|T^M=1, R_C^M=U; T^D=1, R_C^D=U\right) \\
& -E\left(R|T^M=0, R_C^M=U; T^D=0, R_C^D=U\right) \\
= \; & E_{\hat{\beta}_R^{T^M=1,R_C^M=U}}\left(y|\mathbf{X}^{T^D=1,R_C^D=U}\right) - E_{\hat{\beta}_R^{T^M=0,R_C^M=U}}\left(y|\mathbf{X}^{T^D=0,R_C^D=U}\right).
\end{aligned}$$

Likewise, the difference in electoral performance between individuals in the treatment and control conditions is $\Delta E(P) = E(P|S=1, T=1, R_C=R_1) - E(P|S=0, T=0, R_C=R_0)$, or,

$$\begin{aligned}
&\Delta E(P) \\
=\ & E\left(P|T^M=1,R_C^M=R_1;T^D=1,R_C^D=R_1\right) \\
&-E\left(P|T^M=0,R_C^M=R_0;T^D=0,R_C^D=R_0\right) \\
=\ & E_{\hat{\beta}_P^{T^M=1,R_C^M=R_1}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right)-E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0,R_C^D=R_0}\right),
\end{aligned}$$

where the difference is conditional on individuals choosing to run.

Adding and subtracting $E_{\hat{\beta}_R^{T^M=0,R_C^M=U}}\left(y|\mathbf{X}^{T^D=1,R_C^D=U}\right)$ to $\Delta E(R)$, and likewise adding and subtracting $E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right)$ to $\Delta E(P)$, we can write,

$$\begin{aligned}
\Delta E(R) &= \underbrace{E_{\hat{\beta}_R^{T^M=0,R_C^M=U}}\left(y|\mathbf{X}^{T^D=1,R_C^D=U}\right)-E_{\hat{\beta}_R^{T^M=0,R_C^M=U}}\left(y|\mathbf{X}^{T^D=0,R_C^D=U}\right)}_{\textit{OBD Selection effect on decisions to run}} \\
&\quad \underbrace{+E_{\hat{\beta}_R^{T^M=1,R_C^M=U}}\left(y|\mathbf{X}^{T^D=1,R_C^D=U}\right)-E_{\hat{\beta}_r^{T^M=0,R_C^M=U}}\left(y|\mathbf{X}^{T^D=1,R_C^D=U}\right)}_{\textit{OBD Treatment effect on decisions to run}},
\end{aligned}$$

and

$$\begin{aligned}
\Delta E(P) &= \underbrace{E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right)-E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0,R_C^D=R_0}\right)}_{\textit{OBD Selection effect on performance}} \\
&\quad \underbrace{+E_{\beta_P^{T^M=1,R_C^M=R_1}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right)-E_{\beta_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right)}_{\textit{OBD Treatment effect on performance}}.
\end{aligned}$$

In the appendix, we demonstrate the following mapping holds between the selection and treatment effects stemming from OBD and those formalized in Section 4:

Remark 2. *(i) The OBD selection effect on decisions to run equals $SE(R)$ and the OBD treatment effect on decisions to run equals $TE(R)$.*

(ii) The OBD treatment effect on performance equals the direct effect on electability DEE(P), and the OBD selection effect on performance equals SE(P)+TE-R(P) (the shaded cells in Table 2).

The message from this remark is that OBD can separate the relative contributions of *RenovaBR* training vs screening to decisions to run, but cannot do so for electoral performance. The reason is that the OBD selection effect on performance reflects both *RenovaBR* screening and candidate self-selection decisions that are a consequence of training, not screening. An important aspect of this remark, however, is that once all selection and self-selection effects can be accounted for in terms of observables, the OBD treatment effect yields $DEE(P)$, which is the treatment effect of interest in our analysis of election outcomes in Section 6. In terms of the bounds analysis, the OBD treatment effect identifies the treatment effect on Always Takers and Compliers free of self-selection effects.

Since OBD selection effects bundle *RenovaBR* selection and candidate self-selection, we next ask whether some alternative decomposition can identify the specific contribution of *RenovaBR* selection to performance. The answer is in the following,

Remark 3. *The counterfactual predictor difference* $E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_0}\right) - E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0,R_C^D=R_0}\right)$ *identifies* $SE(P)$.

Taking as baseline the control individuals who run, this predictor yields the differential performance that can be expected if those same individuals had the observable characteristics of the treated, but still made self-selection decisions as if they were controls. Once we have an estimate of $SE(P)$, we can subtract it from the OBD selection effect on performance to calculate $TE\text{-}R(P)$, the contribution to electoral performance of treatment-induced candidate self-selection decisions. Then we can compute the overall contribution of treatment $TE(P) = TE - R(P) + DEE(P)$. Armed with empirical measures of $TE(P)$ and $SE(P)$ we can evaluate the relative contribution of *RenovaBR* screening vs training to electoral performance.

7.3 Empirical decomposition results

The approach we take in our empirical implementation adapts the traditional OBD approach to a context with a limited sample and a high number of covariates. In order to reduce the noise from insignificant covariates, we restrict attention to the union of covariates that are ever significant at driving running rates or election rates. To reduce clutter, our tables report a single 90% bootstrapped confidence interval.

Table (14) shows the decomposition of $\Delta E(R)$ in terms of variation in observables and variation in coefficients. The left column of the table shows that the difference in observables produces an increase in the propensity to run of 7.5 percentage points. As shown in the previous section, this variation in observables captures the *RenovaBR* selection effects $SE(R)$. The variation in coefficients (on the treated, bottom row) is associated with an increase in the propensity to run of 17.6 percentage points; as shown before, this variation captures the *RenovaBR* treatment effect $TE(R)$. Note this figure matches the estimated treatment effects on candidacy rates in Tables 3 and 4. Bootstrapped confidence intervals at the 90% level show both $SE(R)$ and $TE(R)$ effects are significantly different from zero.

[TABLE 14 DECOMPOSITION CANDIDACY HERE]

Panel A of Table (15) decomposes $\Delta E(P)$ in terms of variation in observables and variation in coefficients. Here our metric of electoral performance is getting elected. The variation in observables yields an increase in electoral performance of 2.6 percentage points, which is borderline significant at the 10% level. The variation in coefficients yields a marginally significant increase in electability of 5.5 percentage points. As shown in the previous section, this variation in coefficients reflects the Direct Enhanced Electability $DEE(P)$ produced by treatment. The treatment effects we had identified in Section 6 were conditional on self-selection decisions made by candidates, which could depend on their treatment status. The $DEE(P)$ effects reported here hold self-selection effects constant, and isolate a pure treatment effect of *RenovaBR* on electability that is positive, as predicted in Proposition 1.

[TABLE 15 DECOMPOSITION PERFORMANCE HERE]

As said above, the OBD selection effect on performance reflects both *RenovaBR* selection and candidate self-selection. In order to quantify the contribution of *RenovaBR* selection alone, we compute that effect, $SE(P)$, through the counterfactual predictor in Remark 3. Panel B of Table (15) reports this effect to be a positive and significant 3.5 percentage points. This positive effect indicates that *RenovaBR* contributed to the higher election rates of its candidates not only through training, but by selecting more electable individuals in the first place.

The reason why the pure *RenovaBR* selection effect is larger than the raw OBD effect is that the latter includes a negative self-selection effect by candidates of almost 1 percentage point (although this small figure falls short of significance – see second row of Panel B). This self-selection effect we obtain by computing $TE\text{-}R(P) = OBDSelection - SE(P)$. Our theory indicated that if self-selection effects were negative, this could mute the empirical results on electoral performance reported in Table 5. Indeed, when we compute the overall treatment effect TE(P) (see third row in Panel B) and bootstrap the confidence interval, we see an effect of 4.6 percentage points that falls short of significant. In contrast, the direct treatement effect on electoral performance $DEE(P)$ that is “clean” of self-selection effects is instead 1 percentage point larger and on the significant side.

We now use our estimates of selection and treatment effects to calculate the relative contribution of the two components of the program, namely screening and training. Table 16 reports that treatment contributed 70 percent of the effect on decisions to run relative to *RenovaBR* selection, and that treatment contributed 57 percent of the effect on electoral success relative to *RenovaBR* selection. The takeaway is that *RenovaBR* training had important effects, but from the perspective of a political party recruiting candidates, or a voter who may not be able to easily observe all politician characteristics, there is value in the screening conducted by *RenovaBR*.

[TABLE 16 TRAINING VS SCREENING HERE]

8 Tradeoffs

Two difficulties may arise when trying to shift the political class toward being more competent, diverse, and pro-democracy. One is a tradeoff on the supply-side between competence and the goals of diversity and democratic commitment. The other difficulty is a tradeoff on the demand side: *RenovaBR* may value traits differently from voters, so it may end up promoting candidates that are not the most electable.

8.1 Supply side

The original applicant pool is best reflected in the Phase 1 aspirants, before the first big screening takes place. Candidates were allowed into Phase 2 only if they scored above a particular threshold (60 out of 100) in a logic test that attempts to measure intellectual competence. Cognizant of a tradeoff, *RenovaBR* used different thresholds for under-represented groups in order to ensure diversity. So here we will focus on a tension between the traits of competence, demographic diversity, and a pro-democratic stance –the traits that *RenovaBR* positively screened for– against another dimension of diversity, namely lower socioeconomic status –a dimension that *Renova* did not deliberately screen for.

Table 2 in Section 3 showed that the transition from Phase 1 to Phase 2 skewed the income distribution toward higher incomes. Here we explore the reasons. Table 17 shows that a focus on metrics of competence, like performance in the logic test, or having continued education after college, favors individuals in the top quarter of the earnings distribution. While 89 percent of those in the top quarter satisfy the cutoff applied in the logic test, only 76 percent in the bottom three quarters do, and the proportion decreases monotonically as one moves down the earnings distribution. If one focuses on the poor, defined as those earning less than the minimum wage, the proportion of individuals meeting the logic score threshold is down to 64 percent. Higher education is even more strongly associated with income: those in the top quarter of the income distribution attempt post-college education more than twice as often. In sum, promoting more competent aspirants comes into tension with promoting the descriptive representation of lower income individuals. The focus on a pro-democratic stance is also associated with a high-income bias, although this (significant) effect is small, about 3 percent of a standard deviation.

[TABLE 17 TRADEOFF-SUPPLY SIDE HERE]

On the contrary, privileging applicants who are non-white tends to help the representation of lower income individuals. The promotion of female candidates is income-neutral.

8.2 Demand side

Did the priorities at *RenovaBR* match what voters want in a political candidate? Figure 6 shows the relationship between the chance that an applicant is admitted into *RenovaBR* training and the chance that the individual is elected into office. The analysis is conditional on individuals who run for office. As shown in the figure, individuals with better electoral prospects had higher chances of admission into *RenovaBR*.

[FIGURE 6 SCREENING AND ELECTABILITY HERE]

Yet *RenovaBR* may have been far from maximizing electability, and some of the traits the program favored may even have been punished at the polls. Table 18 shows that in Phase 1 *RenovaBR* screened strongly for competence and pro-democracy positions. For example, in the first column we see that those promoted to Phase 2 had logic test scores that were 60% of a standard deviation higher. The second column shows that voters elected individuals more often who had higher education and logic scores. The third column reflects the aforementioned fact that admissions were done favoring candidates who were female, competent along several different dimensions, and pro-democracy. Voters appear indifferent to all of those traits with the exception of favoring more educated candidates.

[TABLE 18 TRADEOFF-DEMAND SIDE HERE]

Taken as a whole these patterns justify our assumption linking *RenovaBR* selection to observables that weakly improve electability. But it is also true that voters appear neutral rather than positive on traits like diversity, or a pro-democratic stance, that are central to *RenovaBR*'s goals.

9 Conclusion

We evaluate a program in Brazil that aims to renew the political class and to make it more competent, diverse, and committed to democracy. The combination of theory and unprecedentedly rich data allows us to meet various challenges, like the absence of exogenous assignment into the program, the fact that electoral performance is only observed conditional on running for office, and the need to not just control for selection but estimate the contribution of selection to outcomes.

Our analysis reveals that the program was highly effective in promoting candidacy and made trainees more electable. These treatment effects encompass training — broadly defined to include access to networks — and possibly signaling, a factor we are unable to disentangle. As such, approximately 70% of the effect on candidacy and 57% of the effect on electoral performance was attributable to treatment, with the remainder driven by selection, reflecting the program's rigorous screening process. Notably, treatment did not disproportionately encourage electoral participation or success among individuals with specific traits. Instead, it was the program's screening for competence, diversity, and democratic values that played a key role in shifting the profile of politicians in the intended direction.

In the Brazilian context, the program we study faced two tradeoffs in pursuing its goals. First, a tradeoff on the supply side of politicians, namely that screening for competence reduces descriptive representation of less educated, low-income people – a tradeoff that does not show up in more advanced democracies (Dal Bó et al., 2017). Second, a tradeoff seems present on the demand side as well: unlike the program, the electorate does not place a premium on politicians who are diverse or hold stronger democratic values. While the quantification of these tradeoffs, and more broadly our quantitative findings on treatment and screening effects, are specific to the Brazilian case, we believe that the approach we have developed can be redeployed to study similar programs elsewhere.

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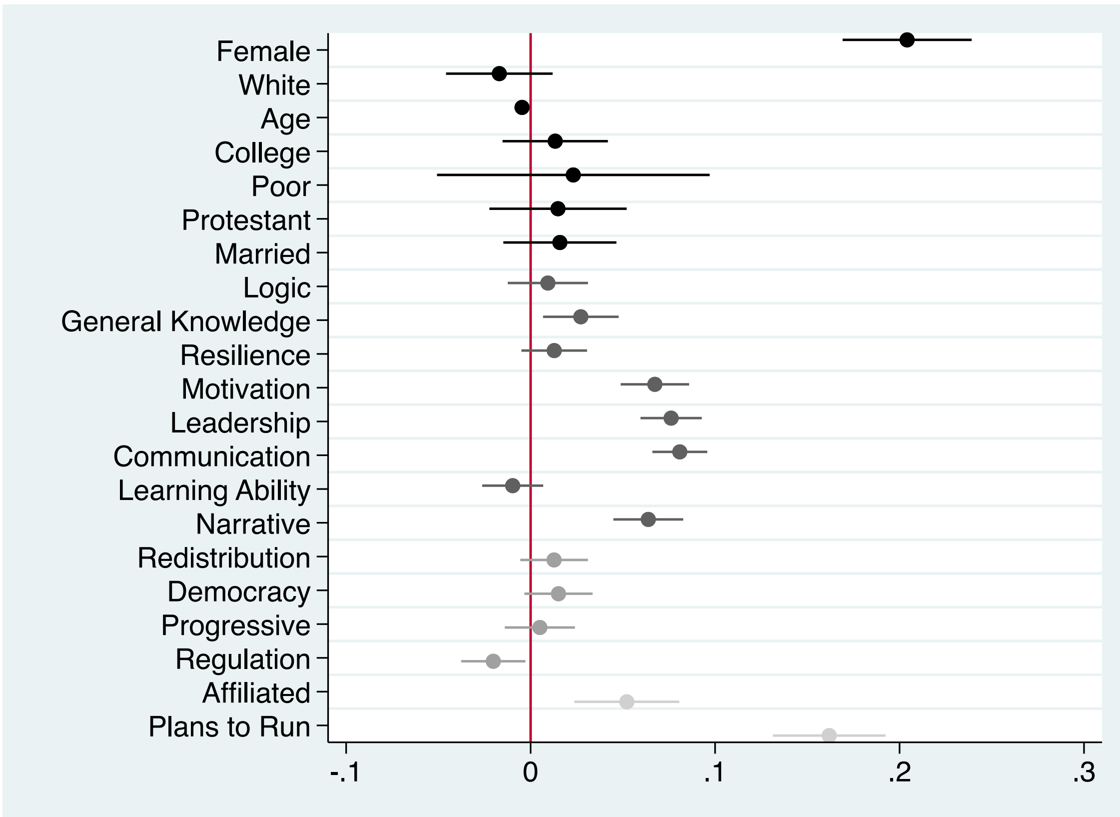
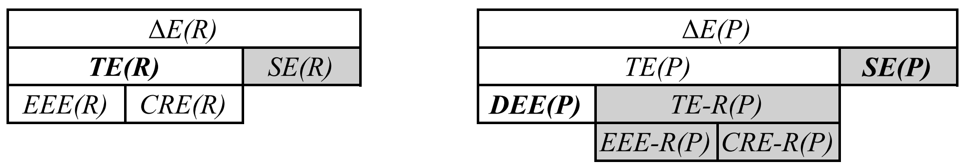
Figure 1: Selection into *RenovaBR* - Among Phase 2 Applicants



Note. This figure plots the partial correlates, along with their 95% confidence intervals, of the candidates who were selected into *RenovaBR*.

Figure 2: Theoretical decomposition of expected outcomes



ΔE(R), ΔE(P) : Difference in expected outcomes between treated and control individuals, where R is the probability of Running for office, and P is electoral Performance

TE(R), SE(R) : Treatment and Selection Effects on decisions to Run

TE(P), SE(P) : Treatment and Selection Effects on electoral Performance

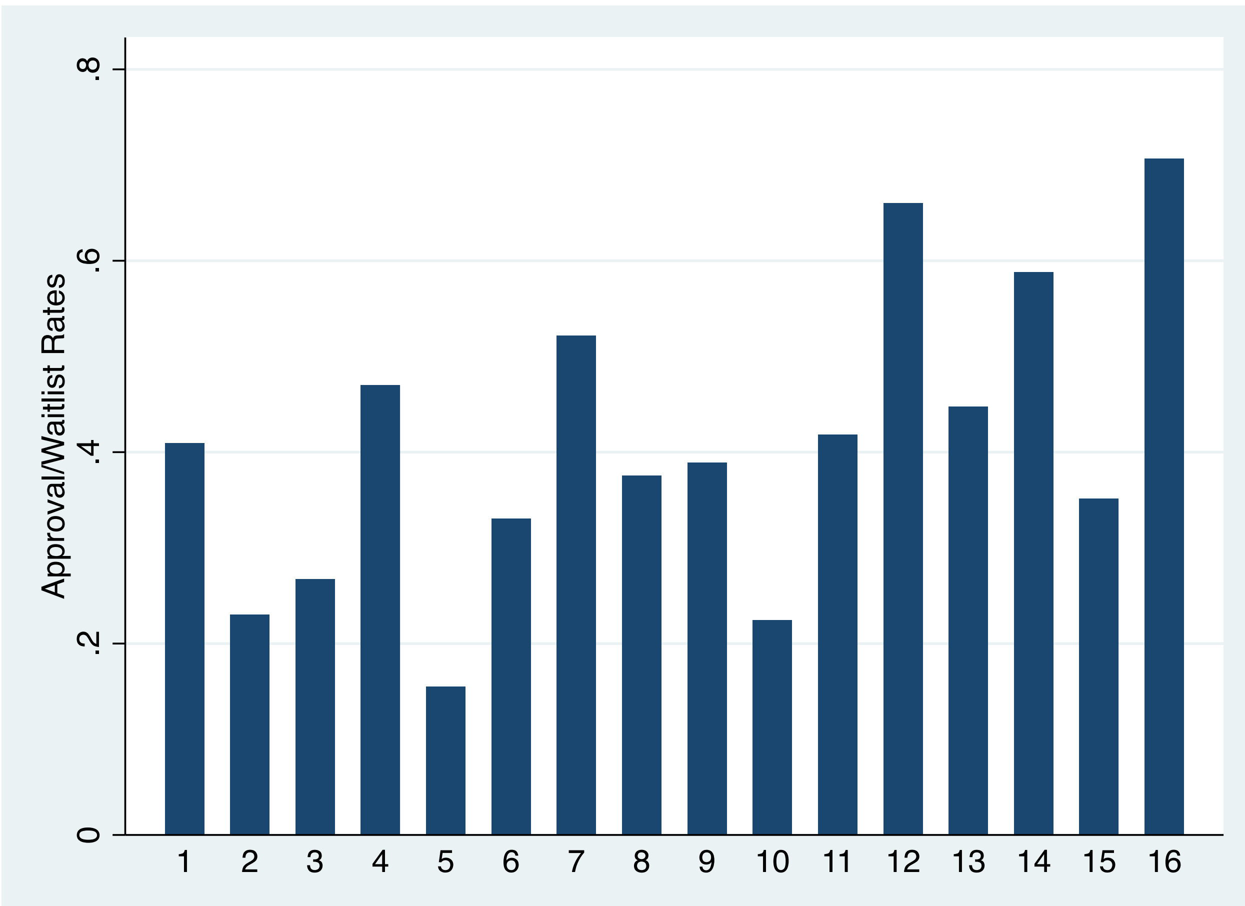
EEE(R), CRE(R) : Enhanced Electability Effects, and Cost-of-Running Effects, on decisions to Run

DEE(P), TE-R(P) : Direct Electability Effects, and Treatment Effects via Running decisions, on Performance

EEE-R(P), CRE-R(P) : Enhanced Electability Effects, and Cost-of-Running Effects, via Running decisions, on Performance

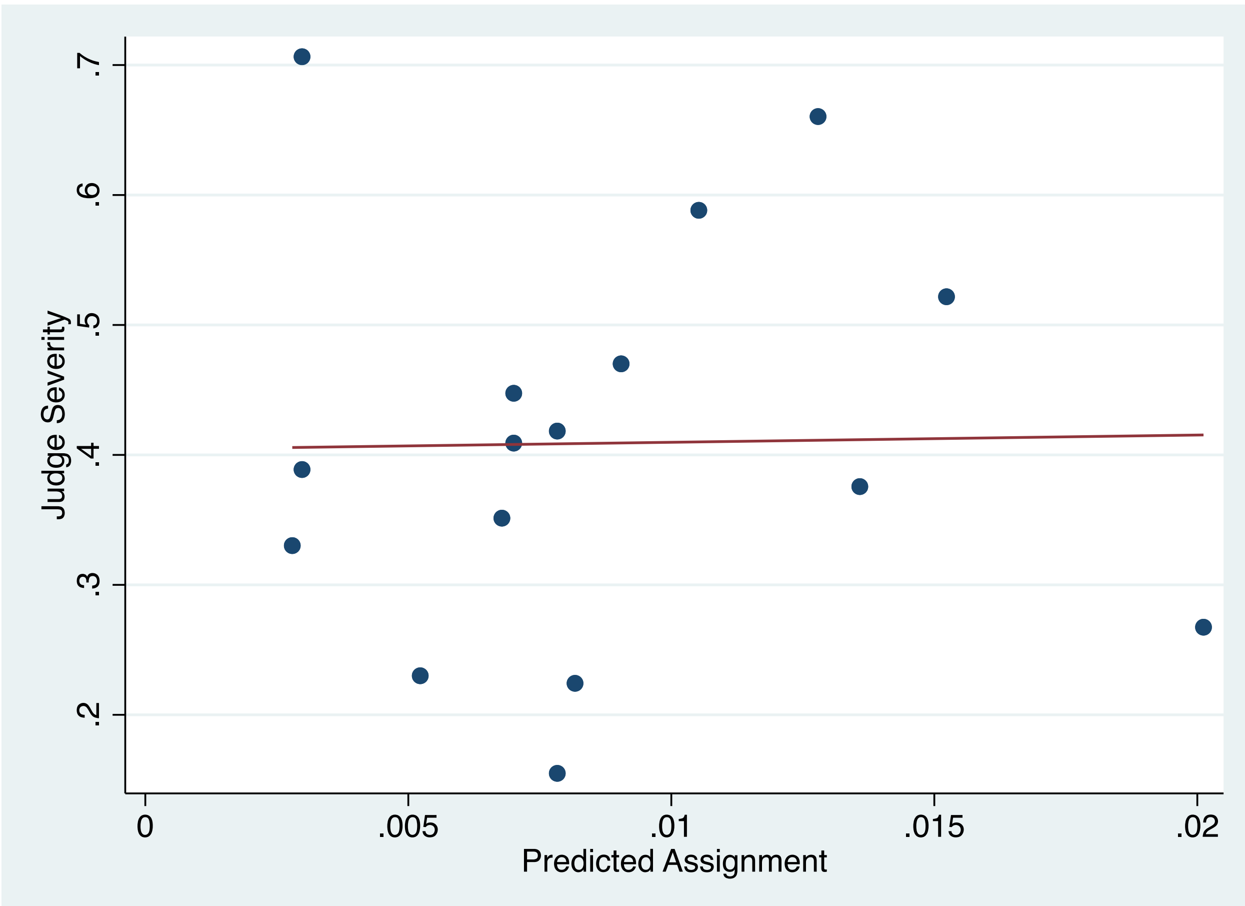
Note: The effects in boldface are predicted by theory to be positive; effects are otherwise ambiguous. The shaded cells represent compositional effects from *RenovaBR* selection ($SE(P)$ and $SE(R)$) or from candidate self-selection ($TE\text{-}R(P)$).

Figure 3: Approval Rates by Judge



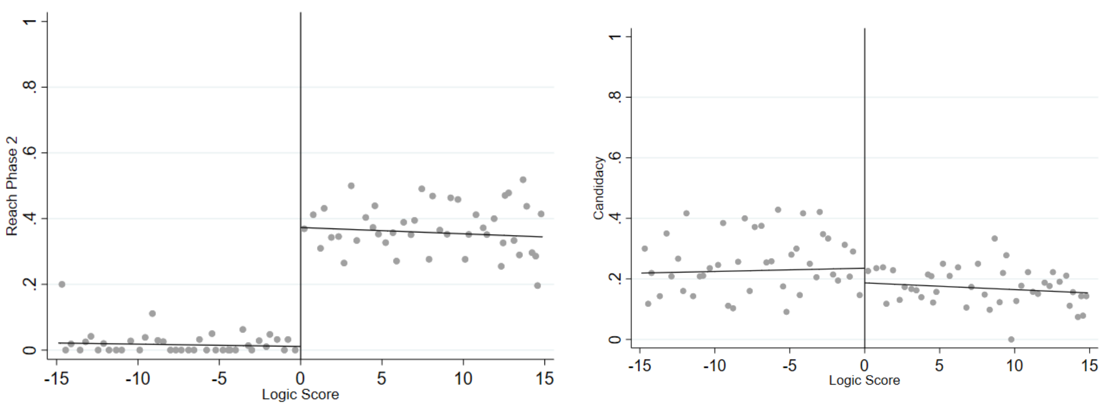
Note. This figure shows the share of applicants each judge recommended for admission into *RenovaBR*.

Figure 4: Approval Rates by Predicted Judge Assignment



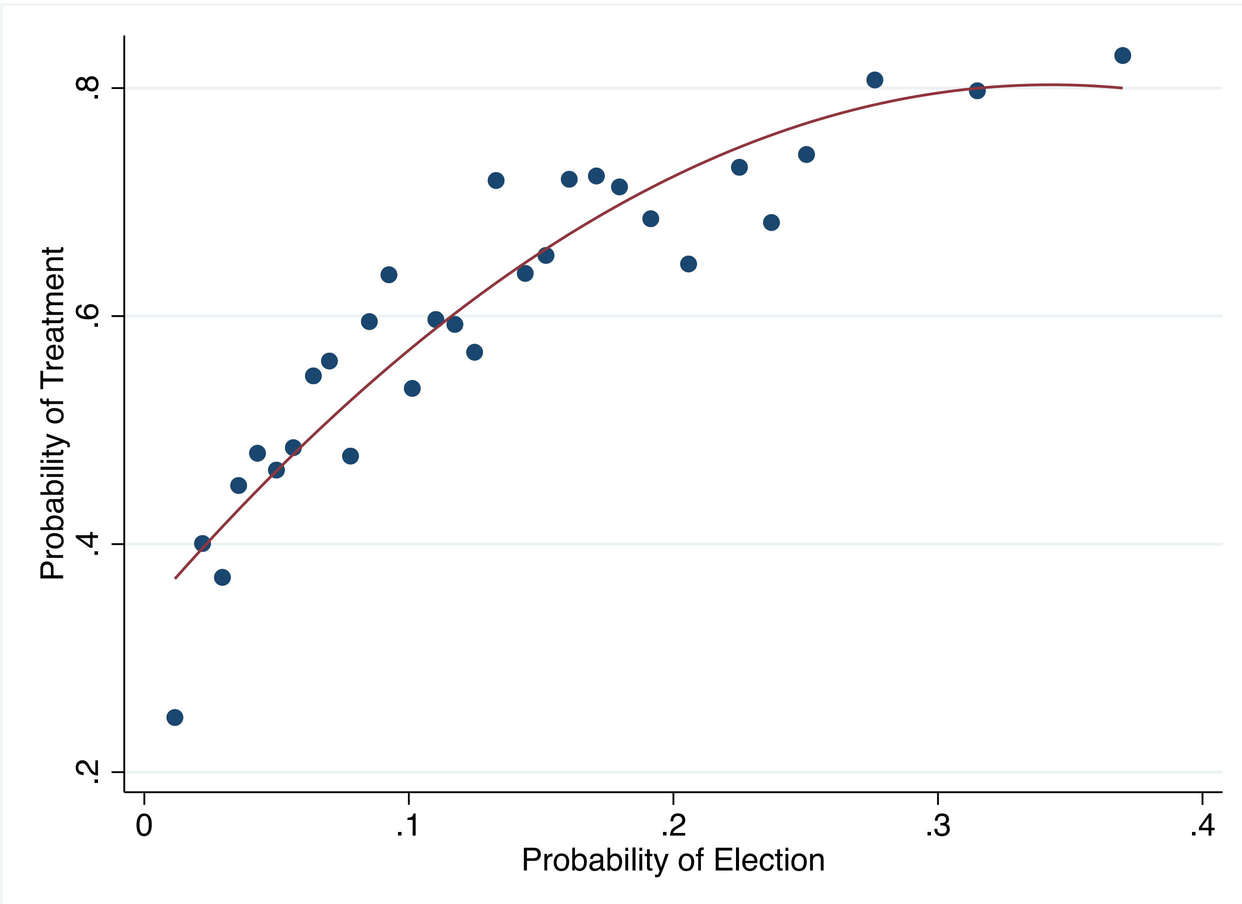
Note. This figure depicts the relationship between predicted judge assignment and the judge's recommendation rates. To predict judge assignment, we use a multinomial logit to predict the probability a judge receives a case to review based on the aspirant's characteristics. For the list of aspirant characteristics see the correlates in Figure 1.

Figure 5: Discouragement effects? Comparison between Phase 1 and Phase 2 applicants



Note. The panel on the left plots acceptance rates into Phase 2 by aspirants' logic scores. The panel on the right plots candidacy rates by aspirants' logic scores.

Figure 6: Renova Screening and Electability



Note. This figure plots the predicted probability of acceptance into *RenovaBR* (y-axis) by the predicted probability of being elected (x-axis) for the sample of aspirants who ran for election. Both predicted probabilities were estimated using a logit model and our full set of controls (see Figure 1 for the list of controls).

Table 1: Aspirant characteristics compared to other candidates

	All Renova aspirants	Candidates in all cities	Candidates in cities with Renova aspirants	Never elected candidates in cities with Renova aspirants	Incumbent candidates in cities with Renova aspirants
	(1)	(2)	(3)	(4)	(5)
Age	36.92	45.30	46.12	45.66	51.68
	(10.08)	(11.52)	(11.34)	(11.31)	(10.21)
% female	0.23	0.35	0.34	0.36	0.11
	(0.42)	(0.48)	(0.47)	(0.48)	(0.31)
% white	0.55	0.47	0.48	0.47	0.55
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
% married	0.33	0.50	0.50	0.49	0.68
	(0.47)	(0.50)	(0.50)	(0.50)	(0.47)
% college or more	0.64	0.22	0.27	0.26	0.38
	(0.48)	(0.42)	(0.45)	(0.44)	(0.48)
% previous candidates	0.14	0.40	0.37	0.32	1.00
	(0.34)	(0.49)	(0.48)	(0.47)	(0.00)
Observations	15,309	517,516	216,316	199,850	16,466

Note: This table compares Renova aspirants with individuals who ran for office as local councillors in the 20202 elections. Column 1 includes all aspirants for the *RenovaBR* training program. Column (2) includes all candidates who run for office, and column (3) displays characteristics for the restricted sample of candidates in municipalities where there is at least one *RenovaBR* aspirant. Columns (4) and (5) restrict the sample to first-time candidates and incumbents running for reelection.

Table 2: Aspirant characteristics along the phases of the admission process

	All Sample			Registered as candidates	
	Rejected phase 1	Rejected phase 2	Accepted Renova	Rejected phase 2	Accepted Renova
	(1)	(2)	(3)	(4)	(5)
Age	36.69	38.25	35.75	38.53	35.89
	(10.15)	(10.21)	(9.15)	(9.67)	(8.88)
% Female	0.23	0.18	0.32	0.15	0.28
	(0.42)	(0.39)	(0.47)	(0.36)	(0.45)
% White	0.52	0.62	0.60	0.57	0.56
	(0.50)	(0.49)	(0.49)	(0.50)	(0.50)
% Black	0.11	0.08	0.10	0.10	0.11
	(0.31)	(0.27)	(0.31)	(0.31)	(0.31)
% Married	0.32	0.35	0.32	0.38	0.32
	(0.47)	(0.48)	(0.47)	(0.49)	(0.47)
% College or more	0.60	0.74	0.74	0.69	0.72
	(0.49)	(0.44)	(0.44)	(0.46)	(0.45)
% Income more 6 min. wages	0.35	0.48	0.53	0.37	0.49
	(0.48)	(0.50)	(0.50)	(0.48)	(0.50)
% Previous party member	0.36	0.41	0.53	0.68	0.64
	(0.48)	(0.49)	(0.50)	(0.47)	(0.48)
% Plans run for office	0.51	0.68	0.84	0.95	0.95
	(0.50)	(0.47)	(0.37)	(0.21)	(0.21)
% Candidate previous election	0.12	0.11	0.26	0.29	0.38
	(0.33)	(0.31)	(0.44)	(0.46)	(0.49)
% Leader	0.30	0.32	0.55	0.50	0.65
	(0.46)	(0.47)	(0.50)	(0.50)	(0.48)
Score current affairs	-0.20	0.36	0.47	0.21	0.43
	(1.07)	(0.71)	(0.69)	(0.76)	(0.75)
Score logic test	-0.26	0.40	0.44	0.25	0.40
	(1.08)	(0.69)	(0.68)	(0.71)	(0.70)
Mean judge score	–	-0.35	0.56	-0.29	0.60
		(0.85)	(0.94)	(0.86)	(0.88)
% Vote share	–	–	–	0.51	0.67
				(0.79)	(0.88)
Rank within party	–	–	–	11.01	8.51
				(11.21)	(10.29)
% Electoral quotient	–	–	–	0.07	0.12
				(0.09)	(0.12)
% Elected	–	–	–	0.08	0.16
				(0.27)	(0.37)
Observations	10647	2242	1306	338	520

Note: This table presents the characteristics of *RenovaBR* aspirants, distinguishing between those who advanced through each phase of the screening process. The final two columns compare treated and control *RenovaBR* aspirants that run for office.

Table 3: The Effects of *RenovaBR* on the Decision to Become a Candidate

	OLS					Entropy Balancing	Double Lasso
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RenovaBR	0.261***	0.199***	0.178***	0.178***	0.183***	0.195***	0.177***
	(0.016)	(0.016)	(0.018)	(0.018)	(0.022)	(0.025)	(0.018)
DV Control Mean	0.16	0.16	0.16	0.16	0.16	0.16	0.16
R^2	0.09	0.16	0.17	0.17	0.28	0.14	
Number of Obs.	3548	3548	3548	3548	2916	3548	3548
Basic Controls	Y	Y	Y	Y	Y	Y	Y
Political Intentions	N	Y	Y	Y	N	Y	Y
Competence	N	N	Y	Y	Y	Y	Y
Ideology	N	N	N	Y	Y	Y	Y
Municipality FE	N	N	N	N	Y	N	N

Note: This table reports the effects of the *RenovaBR* program on the decision to become a candidate. The dependent variable indicates whether the candidate ran for city council in the 2020 elections. *RenovaBR* indicates whether the candidate was accepted into *RenovaBR*'s training program. Columns 1-5 report OLS coefficients. Column 6 reports estimates from a weighted least squares regression, in which the weights balance the first and second moments of the covariate distribution using an entropy method (Hainmueller, 2012). Column 7 reports the results from a Double Lasso model. The sample consists of applicants who succeeded in Phase 2. Basic controls include an indicator of whether the candidate is female, an indicator of whether the candidate is white, age years, an indicator of having at least a college degree, an indicator of having an income at or below the minimum wage, an indicator of being Protestant, and an indicator of being married. Political intentions include an indicator of being a member of a political party and an indicator of intending to run in the 2020 elections. Competence includes the candidate's standardized test scores in logic, general knowledge, resilience, motivation, leadership, communication, learning ability, and narrative. Ideology includes standardized indices measuring their beliefs about redistribution, democracy, progressiveness, and regulation. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 4: The Effects of *RenovaBR* on the Decision to Become a Candidate: Control Function with $\rho = 0.9$

	OLS	Judges All	Judges Grouped	All Interacted	
	(1)	(2)	(3)	(4)	(5)
Panel A: 1-Judge Sample					
RenovaBR	0.186***	0.184***	0.184***	0.183***	0.182***
	(0.022)	(0.025)	(0.025)	(0.025)	(0.025)
Mills - Judge 1		0.003	0.002	0.005	0.004
		(0.017)	(0.017)	(0.017)	(0.017)
Number of observations	2381	2381	2381	2381	2381
R^2	0.18	0.18	0.18	0.21	0.19
Panel B: 2-Judge Sample					
RenovaBR	0.168***	0.145***	0.142***	0.144***	0.137***
	(0.032)	(0.034)	(0.034)	(0.034)	(0.035)
Mills - Judge 1		-0.045	-0.058	-0.042	-0.073*
		(0.036)	(0.039)	(0.037)	(0.039)
Mills - Judge 2		0.076**	0.089**	0.073**	0.102***
		(0.035)	(0.038)	(0.036)	(0.038)
Observations	1167	1167	1167	1167	1167
R^2	0.19	0.19	0.20	0.22	0.21
Panel C: All Judge Sample					
RenovaBR	0.181***	0.174***	0.174***	0.173***	0.173***
	(0.018)	(0.020)	(0.020)	(0.020)	(0.020)
Mills - Judge 1		-0.002	-0.003	0.002	-0.003
		(0.015)	(0.015)	(0.015)	(0.015)
Mills - Judge 2		0.033	0.032	0.027	0.030
		(0.021)	(0.021)	(0.021)	(0.021)
Observations	3548	3548	3548	3548	3548
DV Control Mean	0.16	0.16	0.16	0.16	0.16
R^2	0.18	0.18	0.18	0.20	0.18
Basic Controls		Y	Y	Y	Y
Political Intentions		Y	Y	Y	Y
Competence		Y	Y	Y	Y
Ideology		Y	Y	Y	Y
Municipality FE		N	N	N	N

Note: This table reports the effects of *RenovaBR* on the decision to become a candidate. The dependent variable is an indicator of whether the candidate ran for city council in the 2020 elections. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. Column 1 reports the OLS specification as in Table 3. Column 2 reports estimates using judge indicators as excluded instruments. Column 3 reports estimates using judge severity indicators as excluded instruments. Column 4 reports estimates in which the judge indicators are also interacted with covariates that are significantly different between treatment and control: an indicator of whether the candidate is female, age, an indicator of whether the candidate is white, their score on the logic test, and their beliefs about torture. Column 5 reports estimates in which the judge severity indicators are also interacted with "unbalanced" covariates used in column 4. The sample in Panel A includes all Phase 2 candidates evaluated by a single judge. The sample in Panel B includes all Phase 2 candidates evaluated by two judges. Panel C includes all Phase 2 candidates. The regressions in Panel C also control for whether one or two judges evaluated the candidate. See the table notes from Table 3 for a list of the controls. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 5: The Effects of *RenovaBR* on Electoral Outcomes - Conditional on Running

	OLS					Entropy Balancing	Double Lasso
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Vote Share							
RenovaBR	0.127**	0.127**	0.159**	0.163**	0.170***	0.243***	0.136**
	(0.058)	(0.058)	(0.067)	(0.067)	(0.037)	(0.065)	(0.067)
Panel B: Candidate's Within Party Ranking							
RenovaBR	-2.044***	-2.042***	-2.090***	-2.094***	-2.524**	-1.813*	-2.091***
	(0.751)	(0.756)	(0.809)	(0.805)	(1.275)	(1.025)	(0.776)
Panel C: Fraction of Quotient							
RenovaBR	0.035***	0.035***	0.028***	0.028***	0.031***	0.038***	0.027***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.011)	(0.008)
Panel D: Fraction of Quotient > 0.20							
RenovaBR	0.085***	0.085***	0.070**	0.072**	0.069**	0.125***	0.061**
	(0.026)	(0.026)	(0.030)	(0.030)	(0.032)	(0.035)	(0.030)
Panel E: Elected							
RenovaBR	0.062***	0.062***	0.041	0.043*	0.028	0.076**	0.038
	(0.023)	(0.022)	(0.026)	(0.026)	(0.028)	(0.031)	(0.026)
Panel F: Party Vote Share Excluding the Candidate's							
Treatment	1.095	1.068	0.691	0.684	1.611**	1.035	0.746
	(0.817)	(0.813)	(0.913)	(0.916)	(0.791)	(1.073)	(0.878)
Observations	858	858	858	858	611	858	858
Basic Controls	Y	Y	Y	Y	Y	Y	Y
Political Intentions	N	Y	Y	Y	Y	Y	Y
Competence	N	N	Y	Y	Y	Y	Y
Ideology	N	N	N	Y	Y	Y	Y
Municipality FE	N	N	N	N	Y	N	N

Note: This table reports the effects of *RenovaBR* on electoral outcomes, conditional on running. The dependent variable is labelled in each panel. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. Quotient is the minimum needed for the candidate to be elected in that municipality. See the table notes from Table 3 for more details. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 6: The Effects of Renova on Campaign Finances

	Total Revenue	Log Total Revenue	$1\{Self\}$	$1\{Private\}$	$1\{Party\}$	Share Self	Share Private	Share Party
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RenovaBR	4616.964*	0.337***	0.038	0.098***	-0.117***	-0.017	0.054*	-0.056**
	(2549.283)	(0.122)	(0.041)	(0.036)	(0.041)	(0.024)	(0.029)	(0.027)
DV Control Mean	13560.25	8.57	0.59	0.65	0.58	0.24	0.32	0.28
R^2	0.14	0.22	0.06	0.09	0.10	0.08	0.10	0.12
Number of Obs.	841	841	841	841	841	841	841	841
Basic Controls	Y	Y	Y	Y	Y	Y	Y	Y
Political Intentions	Y	Y	Y	Y	Y	Y	Y	Y
Competence	Y	Y	Y	Y	Y	Y	Y	Y
Ideology	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	N	N	N	N	N	N	N	N

Note: This table reports the effects of *RenovaBR* on campaign finances. The dependent variable is labelled at the top of each column. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. See the table notes from Table 3 for more details. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 7: Political Entry - Demographics

	Age	Female	White	Poor	College	Married	Party Member	Intends to Run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Difference in Means - Conditional on Running								
RenovaBR	-2.555***	0.131***	-0.007	-0.015	0.086**	-0.064*	-0.033	0.003
	(0.647)	(0.027)	(0.034)	(0.014)	(0.033)	(0.033)	(0.033)	(0.015)
Number of observations	880	880	880	880	880	880	880	880
R^2	0.02	0.02	0.00	0.00	0.01	0.00	0.00	0.00
DV Mean - Control	38.46	0.15	0.57	0.05	0.35	0.38	0.67	0.95
Panel B: Decision to Run - Heterogeneous Treatment Effects								
RenovaBR	0.181***	0.181***	0.180***	0.181***	0.181***	0.181***	0.180***	0.173***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Interaction	0.001	-0.047	-0.037	-0.129	-0.013	-0.011	0.018	0.152***
	(0.002)	(0.032)	(0.031)	(0.080)	(0.030)	(0.032)	(0.030)	(0.030)
Observations	3548	3548	3548	3548	3548	3548	3548	3548
R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
DV Mean - Control	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
All Controls	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	N	N	N	N	N	N	N	N

Note: This table reports the effects of *RenovaBR* on political entry. The dependent variable is indicated at the top of each column in panel A. In panel B, the dependent variable is whether the candidate decided to run for office. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. Interaction reports the coefficient of the interaction term between the *RenovaBR* indicator and the characteristics indicated at the top of each column. All controls in panel B are listed in Table 3. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 8: Political Entry - Competence

	Logic	General Knowledge	Learning Ability	Motivation	Resilience	Leadership	Narrative	Communication
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Difference in Means - Conditional on Running								
Treatment	0.170***	0.227***	0.633***	0.769***	0.697***	0.675***	0.832***	0.716***
	(0.049)	(0.052)	(0.067)	(0.061)	(0.061)	(0.061)	(0.060)	(0.064)
Number of observations	880	880	880	880	880	880	880	880
R^2	0.01	0.02	0.10	0.16	0.12	0.12	0.17	0.13
DV Mean - Control	0.23	0.20	-0.32	-0.26	-0.30	-0.13	-0.31	-0.17
All Controls	N	N	N	N	N	N	N	N
Municipality FE	N	N	N	N	N	N	N	N
Panel B: Decision to Run - Heterogeneous Treatment Effects								
Treatment	0.181***	0.181***	0.179***	0.184***	0.181***	0.183***	0.180***	0.179***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)
Interaction	0.005	-0.007	0.010	-0.018	-0.004	-0.019	0.006	0.006
	(0.022)	(0.021)	(0.017)	(0.017)	(0.015)	(0.016)	(0.015)	(0.017)
Observations	3548	3548	3548	3548	3548	3548	3548	3548
R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
DV Mean - Control	0.16	0.16	0.16	0.16	0.16	0.16	0.16	0.16
All Controls	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	N	N	N	N	N	N	N	N

Note: This table reports the effects of *RenovaBR* on political entry. The dependent variable is indicated at the top of each column in panel A. In panel B, the dependent variable is whether the candidate decided to run for office. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. Interaction reports the coefficient of the interaction term between the *RenovaBR* indicator and the characteristics indicated at the top of each column. All controls in panel B are listed in Table 3. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 9: Political Entry - Ideology

	Redistribution	Democracy	Progressive	Regulation
	(1)	(2)	(3)	(4)
Panel A: Difference in Means - Conditional on Running				
Treatment	0.188***	0.233***	0.326***	0.046
	(0.072)	(0.063)	(0.063)	(0.072)
Number of observations	880	880	880	880
R^2	0.01	0.02	0.03	0.00
DV Mean - Control	-0.13	0.04	-0.13	-0.14
All Controls	N	N	N	N
Municipality FE	N	N	N	N
Panel B: Decision to Run - Heterogeneous Treatment Effects				
Treatment	0.181***	0.181***	0.181***	0.181***
	(0.018)	(0.018)	(0.018)	(0.018)
Interaction	0.003	-0.015	-0.029*	-0.001
	(0.015)	(0.018)	(0.017)	(0.014)
Observations	3548	3548	3548	3548
R^2	0.18	0.18	0.18	0.18
DV Mean - Control	0.16	0.16	0.16	0.16
All Controls	Y	Y	Y	Y
Municipality FE	N	N	N	N

Note: This table reports the effects of *RenovaBR* on political entry. The dependent variable is indicated at the top of each column in panel A. In panel B, the dependent variable is whether the candidate decided to run for office. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. Interaction reports the coefficient of the interaction term between the *RenovaBR* indicator and the characteristics indicated at the top of each column. All controls in panel B are listed in Table 3. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 10: The Effects of *RenovaBR* on Party Choices

	Left		Right		Large		New	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RenovaBR	-0.033	-0.065	0.049	0.033	-0.030	-0.052	0.041	0.037
	(0.035)	(0.045)	(0.036)	(0.048)	(0.041)	(0.057)	(0.038)	(0.050)
DV Mean - Control	0.38	0.41	0.47	0.49	0.49	0.40	0.33	0.39
R^2	0.24	0.48	0.21	0.45	0.04	0.27	0.09	0.35
Number of Obs.	884	624	884	624	884	624	884	624
Basic Controls	Y	Y	Y	Y	Y	Y	Y	Y
Political Intentions	Y	Y	Y	Y	Y	Y	Y	Y
Competence	Y	Y	Y	Y	Y	Y	Y	Y
Ideology	N	Y	N	Y	N	Y	N	Y
Municipality FE	N	Y	N	Y	N	Y	N	Y

Note. This table reports the effects of *RenovaBR* on the type of party the candidate joined. The dependent variable in columns 1-2 indicates whether the candidate joined a left-leaning party. In columns 3-4, the dependent variable is an indicator of whether the candidate joined a right-leaning party. In columns 5-6, the dependent variable is an indicator of whether the candidate joined a large party. In columns 7-8, the dependent variable indicates whether the candidate joined a new party formed after 2000. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. See the table notes from Table 3 for more details. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 11: Political Selection - Demographics

	Age	Female	White	Poor	College	Married	Party Member	Intends to Run
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Difference in Means - Conditional on Running								
RenovaBR	1.086	0.150**	-0.103	-0.007	0.092	0.041	0.004	0.019
	(1.298)	(0.068)	(0.099)	(0.034)	(0.099)	(0.086)	(0.095)	(0.032)
Number of observations	120	120	120	120	120	120	120	120
R^2	0.00	0.03	0.01	0.00	0.01	0.00	0.00	0.00
DV Mean - Control	32.12	0.09	0.67	0.03	0.33	0.21	0.70	0.97
Panel B: Elected - Heterogeneous Treatment Effects								
RenovaBR	0.033	0.033	0.032	0.033	0.031	0.032	0.030	0.018
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)	(0.029)
Interaction	-0.000	0.001	-0.012	-0.000	-0.020	0.024	0.016	0.071
	(0.002)	(0.053)	(0.045)	(0.096)	(0.046)	(0.044)	(0.047)	(0.077)
Observations	879	879	879	879	879	879	879	879
R^2	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06
DV Mean - Control	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09
All Controls	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	N	N	N	N	N	N	N	N

Note. This table reports the effects of *RenovaBR* on political selection. In panel A, the dependent variable is indicated at the top of each column. In panel B, the dependent variable is whether the candidate was elected. *RenovaBR* is an indicator of whether the candidate was accepted into *RenovaBR*'s training program. Interaction reports the coefficient on the interaction term that interacts the *RenovaBR* indicator with characteristics indicated at the top of each column. All controls, which only applied to panel B, includes all the controls listed in Table 3. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 12: Political Selection - Competence

	Logic	General Knowledge	Learning Ability	Motivation	Resilience	Leadership	Narrative	Communication
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Difference in Means - Conditional on Running								
Treatment	0.386**	0.402***	0.573***	0.612***	0.752***	0.810***	0.689***	0.646***
	(0.173)	(0.147)	(0.172)	(0.166)	(0.154)	(0.166)	(0.158)	(0.174)
Number of observations	120	120	120	120	120	120	120	120
R^2	0.05	0.06	0.08	0.11	0.12	0.14	0.10	0.13
DV Mean - Control	0.05	0.12	-0.12	0.01	-0.29	-0.05	-0.15	0.02
All Controls	N	N	N	N	N	N	N	N
Municipality FE	N	N	N	N	N	N	N	N
Panel B: Elected - Heterogeneous Treatment Effects								
Treatment	0.039	0.039	0.033	0.032	0.035	0.028	0.033	0.030
	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
Interaction	0.040	0.049*	0.025	0.013	0.024	0.049*	-0.004	0.017
	(0.035)	(0.029)	(0.024)	(0.025)	(0.023)	(0.025)	(0.026)	(0.023)
Observations	879	879	879	879	879	879	879	879
R^2	0.06	0.06	0.06	0.06	0.06	0.06	0.06	0.06
DV Mean - Control	0.09	0.09	0.09	0.09	0.09	0.09	0.09	0.09
All Controls	Y	Y	Y	Y	Y	Y	Y	Y
Municipality FE	N	N	N	N	N	N	N	N

Note. This table reports the effects of *RenovaBR* on political selection. See table notes in Table 11.

Table 13: Political Selection - Ideology

	Redistribution	Democracy	Progressive	Regulation
	(1)	(2)	(3)	(4)
Panel A: Difference in Means - Conditional on Running				
Treatment	0.004	0.200	0.311*	0.138
	(0.211)	(0.194)	(0.171)	(0.185)
Number of observations	120	120	120	120
R^2	0.00	0.01	0.03	0.00
DV Mean - Control	0.08	-0.01	-0.12	-0.15
All Controls	N	N	N	N
Municipality FE	N	N	N	N
Panel B: Elected - Heterogeneous Treatment Effects				
Treatment	0.032	0.031	0.032	0.035
	(0.027)	(0.027)	(0.027)	(0.027)
Interaction	-0.014	-0.017	-0.002	0.019
	(0.022)	(0.026)	(0.023)	(0.020)
Observations	879	879	879	879
R^2	0.06	0.06	0.06	0.06
DV Mean - Control	0.09	0.09	0.09	0.09
All Controls	Y	Y	Y	Y
Municipality FE	N	N	N	N

Note. This table reports the effects of *RenovaBR* on political selection. See table notes in Table 11.

Table 14: Empirical decomposition of expected outcomes: Decisions to Run

		Coefficients (β)			
		Control	**Treatment**	**Δβ**	
Observables (**X**)	**Control**	0.154	0.334	0.180 [0.144, 0.217]	
	Treatment	0.229	0.404	**0.176** [0.144, 0.207]	**TE(R)**
	ΔX	**0.075** [0.050, 0.099] **SE(R)**	**0.070** [0.042, 0.099]		

Note: This table reports the contribution of changes in observables and changes in coefficients to the difference in running rates between treated and control individuals, thus identifying selection effects on decisions to run (SE(R)) and treatment effects on decisions to run TE(R)). 90% confidence intervals in between brackets.

Table 15: Empirical decomposition of expected outcomes: Electoral Performance

Panel A

		Coefficients (**β**)			
		Control	**Treatment**	**Δβ**	
Observables (**X**)	**Control**	0.079	0.120	0.041 [0.003, 0.080]	
	Treatment	0.105	0.159	**0.055** [0.007, 0.102]	**DEE(P)**
	ΔX	**0.026** [0.000, 0.052] **OBD Selection**	0.039 [0.014, 0.064]		

Panel B

Counterfactual predictors	
SE(P)	0.035 [0.002, 0.068]
TE-R(P) = OBD Selection - SE(P)	-0.009 [-0.022, 0.003]
TE(P)=DEE(P)+TE-R(P)	0.046 [-0.007, 0.099]

Note: Panel A reports the contribution of changes in observables and changes in coefficients to the difference in election rates between treated and control individuals. OBD selection comprises both *RenovaBR* selection SE(P) and candidate-self selection TE-R(P); DEE(P) captures the direct treatment effect on electability. Panel B reports the counterfactual predictor for SE(P) that isolates *RenovaBR* selection, and the implied candidate-self selection TE-R(P) and overall treatment effect TE(P). 90% confidence intervals in between brackets.

Table 16: Relative contribution of *RenovaBR* selection and treatment

ΔE(R)		ΔE(P)	
Renova Selection SE(R)	Treatment TE(R)	*Renova* Selection SE(P)	Treatment TE(P)
29.85%	70.15%	43.32%	56.68%

Note: This table reports the relative contributions of *RenovaBR* selection –i.e., screening– and treatment to the difference in running rates and election rates between treated and control individuals.

Table 17: Supply side tradeoffs: competence vs representation

	Top 25% income	Bottom 75% income	Difference P-value
High Logic Score	0.89	0.76	0.0000
Postgraduate	0.53	0.23	0.0000
White	0.69	0.45	0.0000
Female	0.23	0.23	0.4749
Pro-democracy	0.02	-0.01	0.0892

Note. This table shows the means of the variables in the left column for the top vs bottom quartile of the wage distribution in our Phase 1 sample. The p-values correspond to two-tailed tests.

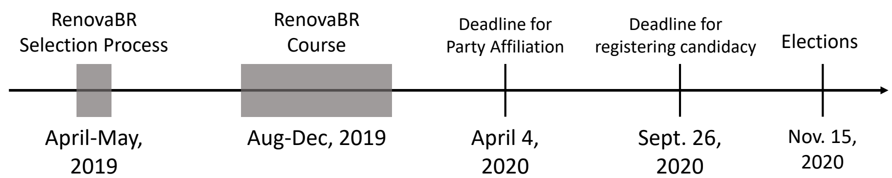
Table 18: Demand side tradeoffs: *RenovaBR* favored characteristics and electability

	Phase 2 vs Phase 1		Treatment vs Phase 2	
	(1)	(2)	(3)	(4)
	ΔX	$\Delta X*\beta$	ΔX	$\Delta X*\beta$
Diversity				
Female	0.009	-0.03%	0.305	0.91%
White	0.169	1.78%	-0.014	-0.14%
Competence				
College	0.283	5.86% *	0.060	1.25% *
Logic score	0.614	12.14% *	0.087	-1.07%
Knowledge score	0.540	-0.24%	0.171	0.61%
Resilience			0.728	-5.97%
Motivation			0.856	13.94%
Leadership			0.939	16.73%
Communication			0.807	4.89%
Learning ability			0.504	9.72%
Narrative			0.960	-4.20%
Democracy	0.227	-2.90%	0.241	-6.69%

Note: Characteristics are standardized using the mean and variance of applicants rejected in Phase 1. Column (1) captures the means for applicants who moved on to Phase 2 vs the (zero) mean of those who did not; Column (3) captures the difference between admitted aspirants and those rejected in Phase 2. Columns (2) and (4) report the product of Columns (1) and (3) respectively times the marginal effects of each characteristic on the probability of election conditional on running for office. * significant at the 10% level.

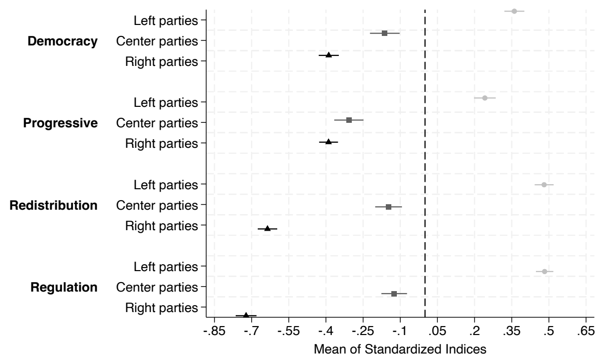
A Appendix: Additional tables and figures

Figure A1: Timeline



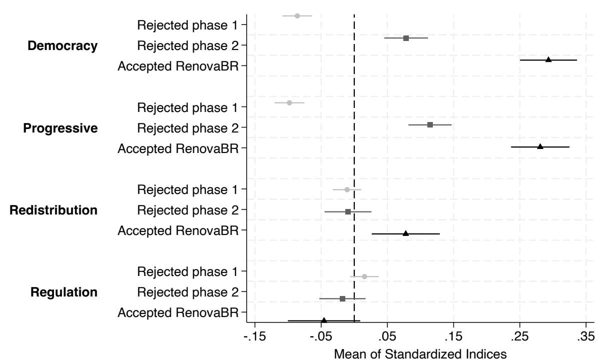
Note. This figure depicts the sequence of events leading up to the 2020 elections.

Figure A2: Political Attitudes by Party Alignment



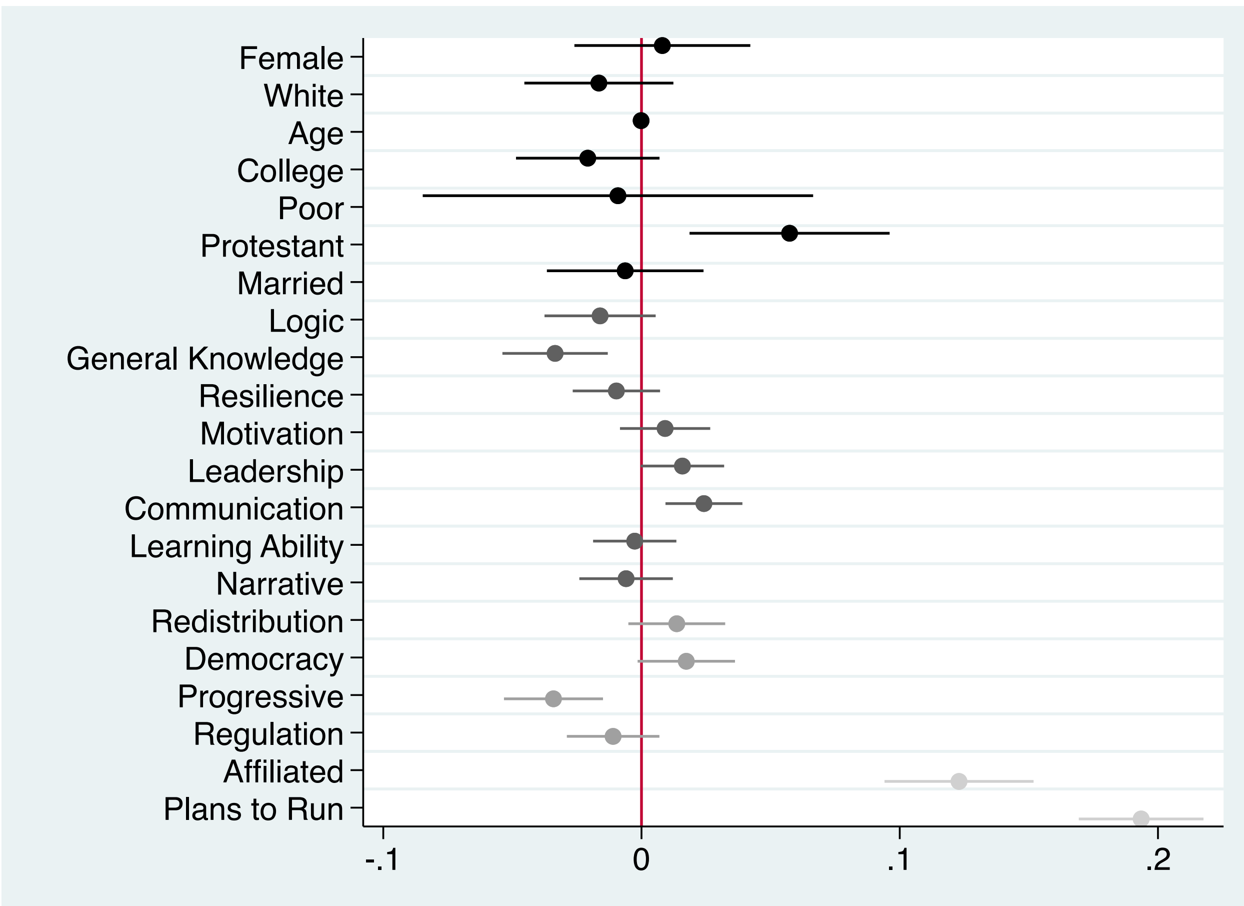
Note. This figure plots the political attitudes of the aspirants on the y-axis, by their self-identified party leanings. The sample consists of all *RenovaBR* aspirants who identified with a party at the time of application (N=6,000).

Figure A3: Political Attitudes by Selection Phase



Note. This figure plots aspirants' support for the political attitudes displayed on the y-axis, along the phases of the evaluation process.

Figure A4: Correlates of the Decision to Become a Candidate



Note. This figure plots the partial correlates, along with their 95% confidence intervals, of the decision to run for office among Phase 2 aspirants.

Table A1: Correlates of Selection for Training

	OLS	Lasso
	(1)	(2)
Female	0.204***	1.080
	(0.018)	
White	-0.017	-0.015
	(0.015)	
Age	-0.005***	-0.024
	(0.001)	
College	0.013	0.030
	(0.015)	
Poor	0.023	
	(0.038)	
Evangelical	0.015	0.020
	(0.019)	
Married	0.016	0.032
	(0.016)	
Party Member	0.052***	0.264
	(0.015)	
Intends to Run	0.162***	0.936
	(0.016)	
Logic	0.009	
	(0.011)	
General Knowledge	0.027***	0.111
	(0.010)	
Resilence	0.013	0.065
	(0.009)	
Motivation	0.067***	0.423
	(0.009)	
Leadership	0.076***	0.431
	(0.008)	
Communication	0.081***	0.550
	(0.008)	
Learning Ability	-0.010	0.012
	(0.008)	
Narrative	0.064***	0.391
	(0.010)	
Redistribution	0.013	0.007
	(0.009)	
Democracy	0.015	0.065
	(0.009)	
Progressive	0.005	0.006
	(0.010)	
Regulation	-0.020**	-0.020
	(0.009)	
Control Mean	0.37	0.37
R^2	0.32	
Number of Obs.	3548	3548

Note. This table reports the correlates of those who were selected for training among Phase 2 aspirants. Column 1 reports the estimates from a linear probability model. Column 2 reports the estimates from a Lasso model.

Table A2: Aspirant Assignment to Judges

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Female	1.107***	-0.472	0.685***	-0.519	0.416**	0.0201	-0.626	-0.163	0	-0.271	-0.472*	0.506	0.701***	-1.787	-0.366	-0.521*
	(3.65)	(-0.72)	(5.02)	(-1.77)	(3.23)	(0.10)	(-0.36)	(-0.94)	(.)	(-0.68)	(-2.04)	(0.43)	(3.68)	(-1.03)	(-0.37)	(-2.38)
White	-0.259	-0.285	0.149	-0.284	-0.192	-0.133	-0.219	0.0480	0	0.0458	0.422*	-0.238	-0.187	-0.0848	-0.353	-0.0586
	(-0.86)	(-0.60)	(1.14)	(-1.35)	(-1.63)	(-0.79)	(-0.19)	(0.33)	(.)	(0.14)	(2.21)	(-0.24)	(-1.03)	(-0.11)	(-0.49)	(-0.36)
Age	0.00849	0.0345	-0.0138	0.0225	0.00122	-0.0114	0.00677	0.00842	0	0.0179	-0.000611	-0.0309	0.00503	-0.00309	0.0123	-0.00411
	(0.52)	(1.43)	(-1.87)	(1.94)	(0.18)	(-1.16)	(0.10)	(1.04)	(.)	(1.05)	(-0.06)	(-0.48)	(0.50)	(-0.07)	(0.29)	(-0.42)
College	-0.185	-0.260	0.0707	-0.160	-0.0349	0.0147	0.658	-0.225	0	-0.252	-0.115	0.127	0.0327	0.376	0.332	0.0725
	(-0.62)	(-0.57)	(0.55)	(-0.77)	(-0.30)	(0.09)	(0.57)	(-1.58)	(.)	(-0.80)	(-0.63)	(0.13)	(0.18)	(0.49)	(0.47)	(0.44)
Poor	0.465	0.778	0.238	0.197	-0.129	0.344	1.153	0.395	0	-0.126	-0.0369	-11.04	0.0790	-0.366	1.267	-0.00861
	(0.65)	(0.72)	(0.77)	(0.35)	(-0.40)	(0.83)	(0.44)	(1.15)	(.)	(-0.13)	(-0.08)	(-0.01)	(0.15)	(-0.12)	(0.84)	(-0.02)
Evangelical	-0.171	-0.653	-0.0946	0.0278	0.192	-0.280	-0.936	0.170	0	-0.168	0.119	0.127	-0.163	-0.411	0.898	0.0419
	(-0.39)	(-0.83)	(-0.55)	(0.10)	(1.27)	(-1.23)	(-0.46)	(0.91)	(.)	(-0.39)	(0.50)	(0.10)	(-0.64)	(-0.34)	(1.08)	(0.20)
Married	0.352	-0.233	0.0715	-0.205	0.0227	0.112	-0.153	-0.0735	0	0.0625	-0.314	0.223	0.215	0.0937	-0.131	-0.0508
	(1.15)	(-0.48)	(0.51)	(-0.90)	(0.18)	(0.62)	(-0.13)	(-0.47)	(.)	(0.19)	(-1.54)	(0.21)	(1.13)	(0.11)	(-0.17)	(-0.28)
Logic	0.652**	1.096**	0.276**	0.643***	0.364***	0.500***	1.846	0.640***	0	0.503*	-0.147	1.322	0.440**	0.847	1.701**	0.326**
	(2.84)	(2.83)	(2.89)	(3.92)	(4.14)	(3.91)	(1.74)	(5.74)	(.)	(2.06)	(-1.09)	(1.54)	(3.18)	(1.31)	(2.63)	(2.63)
General Knowledge	0.269	0.759	0.110	0.516**	0.0315	0.273*	0.969	0.205	0	0.328	0.177	1.224	0.301*	0.682	0.950	0.155
	(1.21)	(1.64)	(1.27)	(2.70)	(0.41)	(2.10)	(0.77)	(1.87)	(.)	(1.28)	(1.32)	(1.06)	(2.16)	(0.84)	(1.20)	(1.26)
Redistribution	0.0130	-0.199	-0.0773	-0.135	-0.0826	-0.0503	-0.0752	-0.0873	0	-0.122	-0.0728	-0.221	-0.117	-0.0769	-0.128	0.0453
	(0.11)	(-1.16)	(-1.58)	(-1.69)	(-1.85)	(-0.79)	(-0.17)	(-1.59)	(.)	(-1.03)	(-1.09)	(-0.61)	(-1.70)	(-0.24)	(-0.47)	(0.70)
Tax Progressivity	-0.0125	-0.103	0.0471	0.0510	-0.0166	-0.0307	-0.260	-0.00734	0	0.00893	-0.0166	-0.0271	0.0550	-0.131	0.177	-0.0461
	(-0.10)	(-0.59)	(0.90)	(0.57)	(-0.35)	(-0.48)	(-0.67)	(-0.13)	(.)	(0.07)	(-0.24)	(-0.07)	(0.72)	(-0.42)	(0.55)	(-0.71)
Race Quotas	0.0243	0.201	-0.0260	0.101	0.0579	0.0343	0.222	0.0260	0	0.0185	-0.0342	-0.154	0.0504	0.0578	0.169	0.00220
	(0.19)	(1.02)	(-0.49)	(1.12)	(1.16)	(0.49)	(0.44)	(0.42)	(.)	(0.14)	(-0.46)	(-0.37)	(0.66)	(0.17)	(0.55)	(0.03)
State intervention	0.0790	-0.0815	-0.00425	-0.0944	0.0269	0.0499	0.201	0.0725	0	0.0368	-0.107	0.0703	0.113	-0.0372	-0.0935	0.0829
	(0.37)	(-0.30)	(-0.05)	(-0.79)	(0.34)	(0.46)	(0.22)	(0.71)	(.)	(0.17)	(-1.08)	(0.10)	(0.85)	(-0.07)	(-0.21)	(0.72)
Schools	0.0369	0.101	0.00539	0.0862	0.0618	-0.0236	-0.108	0.134**	0	0.00289	0.00471	0.336	0.0478	0.0597	0.161	-0.00144
	(0.42)	(0.71)	(0.14)	(1.32)	(1.75)	(-0.47)	(-0.30)	(3.01)	(.)	(0.03)	(0.09)	(1.02)	(0.88)	(0.24)	(0.70)	(-0.03)
Same Sex Married	-0.0290	0.129	-0.0582	0.0261	0.0460	-0.0157	-0.198	-0.00675	0	-0.0389	0.0810	-0.109	-0.0798	0.211	-0.0740	-0.0174
	(-0.19)	(0.51)	(-0.94)	(0.25)	(0.73)	(-0.19)	(-0.38)	(-0.09)	(.)	(-0.25)	(0.85)	(-0.26)	(-0.90)	(0.41)	(-0.23)	(-0.21)
Unions	-0.0556	-0.00419	-0.0207	-0.110	-0.0288	-0.0566	0.0733	-0.0193	0	0.0294	0.00501	-0.209	-0.0606	-0.0558	-0.0630	0.0397
	(-0.47)	(-0.02)	(-0.40)	(-1.36)	(-0.62)	(-0.87)	(0.16)	(-0.34)	(.)	(0.23)	(0.07)	(-0.54)	(-0.85)	(-0.18)	(-0.22)	(0.61)
Pro-business	0.00935	0.151	0.0306	-0.0739	-0.0288	0.0374	-0.0130	-0.0489	0	-0.0886	-0.00734	0.171	-0.0227	-0.0758	-0.129	-0.117
	(0.07)	(0.63)	(0.50)	(-0.79)	(-0.53)	(0.48)	(-0.03)	(-0.76)	(.)	(-0.64)	(-0.09)	(0.36)	(-0.28)	(-0.22)	(-0.43)	(-1.66)
Pro-state	-0.0833	-0.0966	-0.0477	-0.0136	-0.0217	-0.0947	-0.258	-0.0100	0	0.0264	-0.0831	-0.0453	-0.0585	0.156	-0.147	-0.0449
	(-0.77)	(-0.59)	(-1.00)	(-0.17)	(-0.50)	(-1.56)	(-0.61)	(-0.19)	(.)	(0.22)	(-1.29)	(-0.12)	(-0.88)	(0.52)	(-0.55)	(-0.74)
Pro-military	-0.0734	-0.0769	0.0342	0.0602	-0.0529	-0.0158	-0.0683	-0.0164	0	0.0194	-0.0395	-0.0143	0.00605	-0.159	-0.0292	-0.0171
	(-0.73)	(-0.51)	(0.77)	(0.82)	(-1.32)	(-0.29)	(-0.18)	(-0.33)	(.)	(0.18)	(-0.67)	(-0.04)	(0.10)	(-0.63)	(-0.12)	(-0.30)
Pro death penalty	-0.000505	-0.0802	0.0457	0.00415	-0.000140	-0.00414	0.0427	-0.000640	0	0.103	-0.0104	0.0823	0.0134	0.0770	-0.0218	-0.0246
	(-0.01)	(-0.55)	(1.06)	(0.06)	(-0.00)	(-0.08)	(0.11)	(-0.01)	(.)	(0.95)	(-0.19)	(0.25)	(0.22)	(0.28)	(-0.09)	(-0.46)
Civil liberties	-0.110	0.00988	0.0413	-0.00176	-0.00147	0.0310	0.203	0.0244	0	0.0402	-0.0445	0.198	-0.0604	0.0871	0.0503	0.0616
	(-1.14)	(0.06)	(0.95)	(-0.02)	(-0.04)	(0.56)	(0.49)	(0.50)	(.)	(0.38)	(-0.77)	(0.57)	(-1.00)	(0.32)	(0.20)	(1.10)
Prison	-0.0527	0.0567	0.0245	-0.0227	-0.0217	-0.0163	0.0168	0.0253	0	-0.0255	-0.0438	-0.0697	-0.0125	0.0424	0.0347	-0.0379
	(-0.60)	(0.41)	(0.64)	(-0.36)	(-0.62)	(-0.33)	(0.05)	(0.60)	(.)	(-0.27)	(-0.82)	(-0.24)	(-0.23)	(0.18)	(0.16)	(-0.78)
Torture	0.0996	0.138	0.0238	-0.0133	0.197*	0.126	-0.181	-0.0106	0	0.0439	0.361**	0.210	-0.0264	0.351	0.0800	0.111
	(0.50)	(0.46)	(0.31)	(-0.11)	(2.34)	(1.20)	(-0.31)	(-0.13)	(.)	(0.21)	(2.61)	(0.31)	(-0.24)	(0.48)	(0.17)	(1.05)
Tradiional Values	0.0856	-0.149	0.00916	-0.0775	0.0214	-0.0749	0.0560	0.0150	0	-0.0485	0.0949	-0.207	0.0342	-0.0589	-0.0308	-0.0445
	(0.77)	(-0.88)	(0.19)	(-1.00)	(0.49)	(-1.22)	(0.13)	(0.28)	(.)	(-0.42)	(1.45)	(-0.57)	(0.51)	(-0.21)	(-0.11)	(-0.74)
Pro Art/Science	0.0502	0.0649	-0.0240	-0.00929	0.0323	-0.0529	-0.167	0.0259	0	0.0572	-0.0692	-0.217	0.0340	0.0229	-0.0748	0.00578
	(0.49)	(0.41)	(-0.56)	(-0.13)	(0.81)	(-0.96)	(-0.44)	(0.53)	(.)	(0.52)	(-1.18)	(-0.70)	(0.55)	(0.08)	(-0.31)	(0.10)
Monopolies	0.0877	-0.0157	-0.0165	-0.0195	-0.0560	0.0361	0.275	-0.00837	0	-0.0176	-0.0259	0.000406	0.0684	0.0142	-0.0935	-0.00792
	(0.74)	(-0.09)	(-0.34)	(-0.24)	(-1.28)	(0.57)	(0.54)	(-0.15)	(.)	(-0.15)	(-0.39)	(0.00)	(0.95)	(0.04)	(-0.36)	(-0.13)
Protectionist	-0.00277	-0.0727	0.0217	-0.0383	-0.0153	0.0712	-0.150	-0.00928	0	0.0442	0.0507	0.212	0.0150	0.0482	-0.106	0.0208
	(-0.02)	(-0.42)	(0.45)	(-0.49)	(-0.35)	(1.13)	(-0.35)	(-0.17)	(.)	(0.36)	(0.77)	(0.52)	(0.22)	(0.15)	(-0.39)	(0.34)
Inequality	0.0438	-0.0773	0.0223	0.0378	0.0512	0.0152	-0.103	-0.00266	0	-0.0380	0.0605	-0.0104	0.0596	-0.0461	-0.0217	0.0776
	(0.40)	(-0.48)	(0.47)	(0.50)	(1.17)	(0.26)	(-0.25)	(-0.05)	(.)	(-0.33)	(0.94)	(-0.03)	(0.89)	(-0.16)	(-0.08)	(1.29)
Marxism	-0.0357	-0.0203	-0.0192	0.0427	-0.0303	-0.0297	0.228	-0.0365	0	-0.0226	-0.00766	0.107	-0.0383	-0.128	-0.0496	-0.0488
	(-0.34)	(-0.13)	(-0.43)	(0.55)	(-0.73)	(-0.51)	(0.52)	(-0.74)	(.)	(-0.20)	(-0.12)	(0.32)	(-0.61)	(-0.49)	(-0.21)	(-0.85)
Observations	4715															
Pseudo R2	0.0305															
P-value Chi-sq	0.206															

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note. This table reports the results of a multinomial logit, where the judge assigned to each aspirant is the outcome variable and the aspirant's observable characteristics are the regessors.

Table A3: Judge Decisions to Recommend an Aspirant

	(1)	(2)	(3)	(4)
Female	0.480***	0.451***	0.771*	0.823***
	(0.064)	(0.063)	(0.446)	(0.221)
White	-0.012	-0.008	-0.084	-0.036
	(0.054)	(0.054)	(0.224)	(0.140)
Age	-0.009***	-0.008***	-0.007	-0.011
	(0.003)	(0.003)	(0.011)	(0.007)
College	0.054	0.060	0.070	0.062
	(0.054)	(0.053)	(0.056)	(0.054)
Poor	0.004	0.002	0.033	0.012
	(0.139)	(0.138)	(0.146)	(0.139)
Evangelical	0.003	0.015	-0.007	0.003
	(0.071)	(0.070)	(0.074)	(0.071)
Married	-0.118**	-0.116**	-0.130**	-0.111*
	(0.059)	(0.058)	(0.062)	(0.059)
Party Member	0.211***	0.208***	0.222***	0.209***
	(0.053)	(0.053)	(0.055)	(0.053)
Intends to Run	0.520***	0.522***	0.550***	0.533***
	(0.062)	(0.061)	(0.065)	(0.062)
Logic	-0.013	0.012	-0.187	-0.193*
	(0.043)	(0.041)	(0.174)	(0.112)
General Knowledge	0.096**	0.109***	0.119	0.162
	(0.040)	(0.039)	(0.183)	(0.128)
Resilience	0.075**	0.073**	0.065*	0.070**
	(0.036)	(0.035)	(0.038)	(0.035)
Motivation	0.452***	0.423***	0.499***	0.443***
	(0.038)	(0.036)	(0.040)	(0.037)
Leadership	0.480***	0.462***	0.511***	0.463***
	(0.035)	(0.033)	(0.037)	(0.034)
Communication	0.451***	0.431***	0.466***	0.446***
	(0.034)	(0.033)	(0.036)	(0.034)
Learning Ability	0.048	0.024	0.026	0.011
	(0.037)	(0.035)	(0.040)	(0.037)
Narrative	0.382***	0.377***	0.409***	0.384***
	(0.037)	(0.037)	(0.039)	(0.037)
Redistribution	0.015	0.021	0.008	0.018
	(0.035)	(0.035)	(0.037)	(0.035)
Democracy	0.056	0.058	0.045	0.042
	(0.036)	(0.036)	(0.042)	(0.039)
Progressive	0.034	0.044	0.035	0.041
	(0.037)	(0.036)	(0.039)	(0.037)
Regulation	-0.027	-0.030	-0.025	-0.027
	(0.034)	(0.033)	(0.036)	(0.034)
DV Mean	0.38	0.38	0.38	0.38
Number of Obs.	4715	4715	4715	4715
F-test	34.36	109.94	31.76	107.15
Test of Monotonicity	0.26	1.00		
Municipality FE	N	N	N	N

Note. This table displays the first-stage to our control function approach. The dependent variable is an indicator of whether the judge recommends the aspirant for training. Column 1 includes all 16 judge dummies. Column 2 includes 5 judge dummies based on the severity of the judges recommendation rates. Column 3 interacts all 16 judges with covariates that were not balanced across assignment. Column 4 does the same using the 5 judge severity indicators. The judge indicators and their interactions are not displayed. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A4: Effects of *RenovaBR* on the Decision to Run: Judge Indicators

	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: 1-Judge Sample										
RenovaBR	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Mills - Judge 1	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Number of observations	2381	2381	2381	2381	2381	2381	2381	2381	2381	2381
R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
P-value	0.85	0.85	0.85	0.85	0.85	0.85	0.85	0.85	0.85	0.85
Panel B: 2-Judge Sample										
RenovaBR	0.143***	0.143***	0.144***	0.144***	0.144***	0.144***	0.144***	0.144***	0.144***	0.145***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
Mills - Judge 1	0.007	0.004	0.001	-0.002	-0.006	-0.009	-0.014	-0.020	-0.029	-0.045
	(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.023)	(0.024)	(0.027)	(0.030)	(0.036)
Mills - Judge 2	0.042**	0.042**	0.042**	0.043**	0.044**	0.046**	0.049**	0.054**	0.062**	0.076**
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.021)	(0.022)	(0.025)	(0.029)	(0.035)
Observations	1167	1167	1167	1167	1167	1167	1167	1167	1167	1167
R^2	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19
P-value	0.08	0.08	0.08	0.08	0.08	0.08	0.07	0.07	0.06	0.05
Panel C: All-Judge Sample										
RenovaBR	0.171***	0.171***	0.171***	0.172***	0.172***	0.173***	0.173***	0.173***	0.174***	0.174***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Mills - Judge 1	0.005	0.004	0.003	0.002	0.001	0.000	-0.001	-0.001	-0.002	-0.002
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)
Mills - Judge 2	0.035*	0.034*	0.034*	0.034*	0.034*	0.034*	0.033*	0.033*	0.033*	0.033
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.020)	(0.021)
Observations	3548	3548	3548	3548	3548	3548	3548	3548	3548	3548
R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
P-value	0.15	0.15	0.16	0.16	0.17	0.17	0.18	0.19	0.20	0.21

Note. This table reports the effects of *RenovaBR* on the decision to run for different values of ρ. The dependent variable is an indicator of whether the judge recommends the aspirant for training. Each regression contains all the control variables included in column 4 of Table 3. The excluded instruments are all 16 judge indicators. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A5: Effects of *RenovaBR* on the Decision to Run: Judge Severity Indicators

	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: 1-Judge Sample										
RenovaBR	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***	0.184***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)	(0.025)
Mills - Judge 1	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Number of observations	2381	2381	2381	2381	2381	2381	2381	2381	2381	2381
R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
P-value	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90
Panel B: 2-Judge Sample										
RenovaBR	0.139***	0.140***	0.140***	0.140***	0.140***	0.140***	0.140***	0.141***	0.141***	0.142***
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)
Mills - Judge 1	0.003	-0.000	-0.003	-0.007	-0.011	-0.015	-0.020	-0.027	-0.038	-0.058
	(0.021)	(0.020)	(0.021)	(0.021)	(0.022)	(0.023)	(0.025)	(0.027)	(0.031)	(0.039)
Mills - Judge 2	0.046**	0.046**	0.047**	0.048**	0.049**	0.052**	0.055**	0.061**	0.070**	0.089**
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.021)	(0.023)	(0.026)	(0.030)	(0.038)
Observations	1167	1167	1167	1167	1167	1167	1167	1167	1167	1167
R^2	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20	0.20
P-value	0.05	0.05	0.05	0.05	0.05	0.04	0.04	0.04	0.04	0.03
Panel C: All-Judge Sample										
RenovaBR	0.170***	0.171***	0.171***	0.172***	0.172***	0.173***	0.173***	0.174***	0.174***	0.174***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Mills - Judge 1	0.003	0.002	0.001	0.000	-0.001	-0.002	-0.002	-0.003	-0.003	-0.003
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)
Mills - Judge 2	0.037**	0.037**	0.036**	0.036**	0.036**	0.035*	0.035*	0.035*	0.034*	0.032
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.019)	(0.019)	(0.020)	(0.021)
Observations	3548	3548	3548	3548	3548	3548	3548	3548	3548	3548
R^2	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
P-value	0.11	0.12	0.12	0.13	0.14	0.15	0.16	0.17	0.20	0.22

Note. This table reports the effects of *RenovaBR* on the decision to run for different values of ρ. The dependent variable is an indicator of whether the judge recommends the aspirant for training. Each regression contains all the control variables include in column 4 of Table 3. The excluded instruments are the 5 judge indicators grouped based on severity. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A6: Effects on Electoral Outcomes - Control function

	Vote Share	Quotient	Quotient > .20	Rank	Elected	Revenue BRL 1,000s	Revenue logs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RenovaBR	0.185**	0.028***	0.081**	-1.612*	0.060**	3859.525	0.256*
	(0.075)	(0.009)	(0.032)	(0.934)	(0.029)	(4307.432)	(0.133)
Mills - Judge 1	-0.035	0.001	-0.013	-1.248	-0.031	-9.419	0.065
	(0.073)	(0.009)	(0.030)	(0.884)	(0.025)	(4752.915)	(0.114)
Mills - Judge 2	-0.001	-0.002	-0.004	0.219	0.004	4360.326	0.247
	(0.076)	(0.010)	(0.036)	(1.044)	(0.030)	(5054.625)	(0.151)
DV Control Mean	0.51	0.07	0.13	11.01	0.08	15395.18	8.59
R^2	0.11	0.13	0.07	0.13	0.07	0.09	0.23
Number of Obs.	858	858	875	858	870	839	839
Basic Controls	Y	Y	Y	Y	Y	Y	Y
Political Intentions	Y	Y	Y	Y	Y	Y	Y
Competence	Y	Y	Y	Y	Y	Y	Y
Ideology	Y	Y	Y	Y	Y	Y	Y
Municipality FE	N	N	N	N	N	N	N

Note. This table reports the effects of *RenovaBR* on all the electoral outcomes, using the control function approach.

Table A7: Electoral performance, unconditional

	OLS					Entropy Balancing	Double Lassso
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Vote Share							
RenovaBR	0.194***	0.153***	0.145***	0.145***	0.108***	0.172***	0.140***
	(0.020)	(0.019)	(0.022)	(0.022)	(0.017)	(0.023)	(0.022)
Panel B: Candidate's Within Party Ranking							
RenovaBR	-2.089***	-2.092***	-2.015**	-2.020**	-2.354*	-1.721*	-1.981**
	(0.751)	(0.755)	(0.814)	(0.809)	(1.283)	(1.026)	(0.781)
Panel C: Fraction of Quotient							
RenovaBR	0.035***	0.029***	0.025***	0.025***	0.021***	0.030***	0.024***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Panel D: Fraction of Quotient > 0.20							
RenovaBR	0.073***	0.059***	0.048***	0.049***	0.035***	0.068***	0.047***
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)
Panel E: Elected							
RenovaBR	0.051***	0.042***	0.032***	0.033***	0.023***	0.047***	0.031***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)
Panel F: Party Vote Share Excluding the Candidate's							
Treatment	2.528***	1.941***	1.666***	1.665***	1.704***	1.893***	1.644***
	(0.244)	(0.254)	(0.279)	(0.279)	(0.248)	(0.382)	(0.278)
Observations	3526	3526	3526	3526	2898	3526	3526
Basic Controls	Y	Y	Y	Y	Y	Y	Y
Political Intentions	N	Y	Y	Y	Y	Y	Y
Competence	N	N	Y	Y	Y	Y	Y
Ideology	N	N	N	Y	Y	Y	Y
Municipality FE	N	N	N	N	Y	N	N

Note. This table displays results on electoral performance unconditional on individuals running for office. Individuals who do not run are imputed an electoral success or vote share of zero. Robust standard errors are reported in parentheses. *p<0.1; **p<0.05; ***p<0.01.

B Appendix: Formalities of Decomposition

Adding and subtracting $E(R|S=1,T=0) = \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,dcd\mathbf{x}$ to (2) and $E(P|S=1,T=0,R_C=R_0)$
$= \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}$ to (3), we can decompose $\Delta E(R)$ and $\Delta E(P)$ in terms of treatment and selection effects:

$$\begin{aligned}\Delta E(R) &= TE(R) + SE(R)\\ &\equiv \underbrace{\begin{array}{c}\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\,dcd\mathbf{x}\\ -\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,dcd\mathbf{x}\end{array}}_{TE(R)}\\ &+ \underbrace{\begin{array}{c}\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,dcd\mathbf{x}\\ -\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\,dcd\mathbf{x}\end{array}}_{SE(R)},\end{aligned} \tag{9}$$

and

$$\begin{aligned}\Delta E(P) &= TE(P) + SE(P)\\ &\equiv \underbrace{\begin{array}{c}\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},1) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}\\ -\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}\end{array}}_{TE(P)}\\ &+ \underbrace{\begin{array}{c}\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}\\ -\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)dcd\mathbf{x}}\end{array}}_{SE(P)}.\end{aligned} \tag{10}$$

The terms $TE(y)$ and $SE(y), y = R, P$ capture respectively treatment and selection effects on y.

Adding and subtracting $\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\,dcd\mathbf{x}$, $TE(R)$ can be further decomposed thus,

$$TE(R) = \underbrace{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},T=1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\,dcd\mathbf{x} - \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\,dcd\mathbf{x}}_{EEE(R)}$$
$$+\underbrace{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\,dcd\mathbf{x} - \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},T=0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,dcd\mathbf{x}}_{CRE(R))},$$

where the first difference $E(R|S=1,T=1,P(\mathbf{x},T=1)) - E(R|S=1,T=1,P(\mathbf{x},T=0))$ captures the treatment Effect of Enhanced Electability on decisions to Run $(EEE(R))$ by the selected and treated, and the second difference $E(R|S=1,T=1,P(\mathbf{x},T=0)) - E(R|S=1,T=0,P(\mathbf{x},T=0))$ captures the Cost Reduction Effect $(CRE(R))$ from treatment on the selected. The $EEE(R)$ arises because, conditional on observables $\mathbf{x}$, a higher chance of winning makes individuals with marginally higher costs want to run. The $CRE(R)$ arises because, conditional on observables $\mathbf{x}$, a decrease in costs makes more individuals find it advantageous to run.

Adding and subtracting $E(P|S=1,T=1,P(\mathbf{x},T=0),R_C=R_1) = \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},0)g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}$, $TE(P)$ can be written as,

$$TE(P) = \underbrace{\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},1)g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}} - \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},0)g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}}_{DEE(P)}$$
$$+\underbrace{\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},0)g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}} - \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0)g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}}_{TE-R(P)},$$

where the first difference in the RHS ($DEE(P)$) captures the Direct Electability Effect (an improvement in electoral chances conditional on type among the selected) on the treated; the second difference captures the Treatment Effect on Performance through endogenous decisions to Run ($TE\text{-}R(P)$). Thus, the treatment effect on electoral outcomes includes effects that, like the selection effects induced by Renova admissions, are about sample composition. The difference is the $TE\text{-}R(P)$ arises from self-selection decisions made by candidates as a result of treatment. Adding and subtracting $E(P|S=1,T=1,P(\mathbf{x},T=0),R_C=R_0) = \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}$, $TE\text{-}R(P)$ can be further decomposed, so $TE(P)$ reads,

$$TE(P) = \underbrace{\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},1) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}} - \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}}_{DEE(P)}$$

$$+ \underbrace{\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}} - \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}}_{EEE-R(P)} +$$

$$+ \underbrace{\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)dcd\mathbf{x}} - \frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}}\int_{-\infty}^{P(\mathbf{x},0)} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)dcd\mathbf{x}}}_{CRE-R(P)},$$

where the second and third lines decompose the $TE\text{-}R(P)$ into two parts. The first, $EEE\text{-}R(P)$, arise because of Electoral Effectiveness Effects on Running that in turn shape electoral Performance: better electoral chances from treatment induce people with higher costs to run, and these may on average have lower or higher observables, impacting performance. The second, $CRE\text{-}R(P)$, are Cost Reduction Effects that alter decisions to Run, in turn impacting electoral Performance. These arise because treatment effects on costs induce more people of a given quality to run. On average, the additional people who run may have lower or higher observables, impacting performance.

Thus, the overall effect on electoral performance can be written as $\Delta E(P) = DEE(P) + TE\text{-}R(P) + SE(P) = DEE(P) + EEE\text{-}R(P) + CRE\text{-}R(P) + SE(P)$.

C Appendix: Proofs

Proof. of Remark 1: The formal statement of the remark is as follows: $G^{1,0}_{\mathbf{X}}(\mathbf{x},c) = \int_{\{\mathbf{X}^l \leq \mathbf{q} \leq \mathbf{x}\}} g^{1,0}_{\mathbf{X}}(\mathbf{q})\, d\mathbf{q} \leq G^{0,0}_{\mathbf{X}}(\mathbf{x}) = \int_{\{\mathbf{X}^l \leq \mathbf{q} \leq \mathbf{x}\}} g^{0,0}_{\mathbf{X}}(\mathbf{q})\, d\mathbf{q}\ \forall \mathbf{x} \in \chi$ with strict inequality for some $\mathbf{x}$. And the analogous relation holds for the conditional distribution $G^{S,T}_{\mathbf{X}|C}(\mathbf{x})$ We demonstrate the statement for the case of the conditional distribution. Note that $G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c)$ can be written as

$$G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c) = \int_{\{\mathbf{X}^l \leq \mathbf{q} \leq \mathbf{x}\}} g^{1,0}_{\mathbf{X}}(\mathbf{q}|c)\, d\mathbf{q} = \frac{1}{k} \int_{\{\mathbf{X}^l \leq \mathbf{q} \leq \mathbf{x}\}} g^{0,0}_{\mathbf{X}|C}(\mathbf{q}|c) \left[1 - F\left(-\Gamma' \mathbf{q}\right)\right] d\mathbf{q}$$

where $k \equiv \int_{\{\mathbf{X}^l \leq \mathbf{q} \leq \mathbf{X}^h\}} g^{0,0}_{\mathbf{X}|C}(\mathbf{q}|c)\left[1 - F(-\Gamma' \mathbf{q})\right] d\mathbf{q}$ is a scaling constant to to 1 and is a distribution. Then notice that for both $\mathbf{X}^l$ and $\mathbf{X}^h$ the two distributions $G^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)$ and $G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c)$ adopt the same values, at 0 and 1. Notice also that $\frac{\nabla G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c)}{\nabla \mathbf{x}} = \frac{1}{k}$ $g^{0,0}_{X|C}(\mathbf{x}|c)\left[1 - F(-\Gamma' \mathbf{x})\right]$ and $\frac{\nabla G^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)}{\nabla \mathbf{x}} = g^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)$. Since $\frac{1-F\left(-\Gamma' \mathbf{x}\right)}{k}$ is continuously increasing in $\mathbf{x}$ with min=0 and max=$\frac{1}{k} > 1$, it follows by IVT that there exists a unique $\tilde{\mathbf{x}} \in (0,1)$ such that $\frac{\nabla G^{1,0}_{\mathbf{X}|C}(\tilde{\mathbf{x}}|c)}{\nabla \mathbf{x}} = \frac{\nabla G^{0,0}_{\mathbf{X}|C}(\tilde{\mathbf{x}}|c)}{\nabla \mathbf{x}}$, and we have $\frac{\nabla G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c)}{\nabla \mathbf{x}} < \frac{\nabla G^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)}{\nabla \mathbf{x}}$ for all $\mathbf{x} < \tilde{\mathbf{x}}$ and $\frac{\nabla G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c)}{\nabla \mathbf{x}} > \frac{\nabla G^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)}{\nabla \mathbf{x}}$ for all $\mathbf{x} > \tilde{\mathbf{x}}$. Since both distributions start at 0 for $\mathbf{x} = \mathbf{X}^l$, this means that $G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c) < G^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)$ for all $\mathbf{x} < \mathbf{x}'$. Since there is a unique $\mathbf{x}' \in \left(\mathbf{X}^l, \mathbf{X}\right)$ at which the slopes of the distributions are the same, the distributions cannot cross. And since both end at 1 for $\mathbf{x} \to \mathbf{X}^h$, it follows that $G^{1,0}_{\mathbf{X}|C}(\mathbf{x}|c) < G^{0,0}_{\mathbf{X}|C}(\mathbf{x}|c)$ for all $\mathbf{x} \in \left(\mathbf{X}^l, \mathbf{X}^h\right)$. □

Proof. of Proposition 1:

(i) $SE(R)$ ambiguous and $SE(P) > 0$. The first result obtains because selection could alter the marginal distribution of C and a simple example suffices: Suppose there is only one observable $X \in \left\{X^l, X^h\right\}$, and the population is equally split between those two levels. The conditional distribution of C is such that all those with X^l have costs $c^l < \tilde{c}^l \equiv P\left(X^l, T=0\right)$ so they choose to run, and all those with X^h have costs $\tilde{c}^h >> \tilde{c}^h \equiv P\left(X^h, T=0\right)$ so they choose not to run. Thus, among the controls, where low and high observables are equally represented, half of all individuals choose to run. Suppose now that Γ is large relative to the variance of σ_ξ so positive selection is arbitrarily strong: almost all selected individuals have high quality X^h: then among the selected the rate at which individuals choose to run is arbitrarily close to zero.

The selection effect would be unambiguously positive if $\mathbf{X}$ and C were independent. To see this, recall $SE(R) = E(R|S=1,T=0) - E(R|S=0,T=0)$ can be written as,

$$\begin{aligned} SE(R) &= \int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,d\mathbf{x}dc - \int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\,d\mathbf{x}dc \\ &= \int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} \left[g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c) - g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\right] d\mathbf{x}dc, \end{aligned}$$

where $\mathbf{X}(c)$ denotes the set of values of $\mathbf{X}$ that lie on a level curve of $P(\mathbf{X},T) = c$. Under independence, selection does not affect the marginal of distribution of costs, so $g_{C}^{S=1,T=0}(c) = g_{C}^{S=0,T=0}(c)$ and we can write,

$$\begin{aligned} SE(R) &= \int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} \left[g_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c) - g_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c)\right] g_{C}^{S=1,T=0}(c)\,d\mathbf{x}dc \\ &= \int_{0}^{\infty} \left[1 - G_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{X}(c)|c) - 1 + G_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{X}(c)|c)\right] g_{C}^{S=1,T=0}(c)\,dc \\ &= \int_{0}^{\infty} \left[G_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{X}(c)|c) - G_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{X}(c)|c)\right] g_{C}^{S=1,T=0}(c)\,dc > 0, \end{aligned}$$

where the inequality follows from the assumption that, for any $\mathbf{x}$, $G_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c) > G_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c)$. The intuition is that as selection privileges higher quality types, and $P(\mathbf{X},T)$ is increasing and quasiconcave in $\mathbf{X}$, then any shift of $\mathbf{X}$ toward higher values places more mass above the convex level curve $\mathbf{X}(c)$. To see that $SE(P) > 0$, note $SE(P)$ is,

$$\begin{aligned} SE(P) &= \frac{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} P(\mathbf{x},0)\, g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,d\mathbf{x}dc}{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c)\,d\mathbf{x}dc} - \frac{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} P(\mathbf{x},0)\, g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\,d\mathbf{x}dc}{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x},c)\,d\mathbf{x}dc} \\ &= \frac{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} P(\mathbf{x},0)\, g_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c)\, g_C(c)\,d\mathbf{x}dc}{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x}|c)\, g_C(c)\,d\mathbf{x}dc} \\ &- \frac{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} P(\mathbf{x},0)\, g_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c)\, g_C(c)\,d\mathbf{x}dc}{\int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} g_{\mathbf{X}C}^{S=0,T=0}(\mathbf{x}|c)\, g_C(c)\,d\mathbf{x}dc} \\ &= \int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} P(\mathbf{x},0)\, \tilde{g}_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c)\,d\mathbf{x} g_C(c)\,dc \\ &- \int_{-\infty}^{\infty}\int_{\{\mathbf{x}:\mathbf{X}(c)\leq\mathbf{x}\}} P(\mathbf{x},0)\, \tilde{g}_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c)\,d\mathbf{x} g_C(c)\,dc, \end{aligned}$$

where $\tilde{g}$ denotes the normalized distribution over $\mathbf{X}$ conditional on c and $\mathbf{X}(c) \leq \mathbf{x}$. Then we can write,

$$SE(P) = \int_{-\infty}^{\infty} \int_{\{\mathbf{x}:\mathbf{X}(c)\leq \mathbf{x}\}} P(\mathbf{x},0) \left[\tilde{g}_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c) - \tilde{g}_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c) \right] d\mathbf{x} g_C(c)\, dc > 0$$

It is easy to show that if $g_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c)$ stochastically dominates $g_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c)$ (as assumed), then the distributions $\tilde{g}_{\mathbf{X}|C}^{S=1,T=0}(\mathbf{x}|c)$ and $\tilde{g}_{\mathbf{X}|C}^{S=0,T=0}(\mathbf{x}|c)$ truncated below preserve stochastic dominance, yielding the sign.

(ii) and (iii) $EEE(R) > 0; CRE(R) > 0$. The two components of $TE(R)$ can be rewritten as,

$$\begin{aligned} TE(R) &= EEE(R) + CRE(R) \\ &= \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(x,1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\, dc d\mathbf{x} - \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(x,0)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c)\, dc d\mathbf{x} \\ &\quad + \int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(x,0)} \left[g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c) - g_{\mathbf{X}C}^{S=1,T=0}(\mathbf{x},c) \right] dc d\mathbf{x} > 0, \end{aligned}$$

where the first difference is positive whenever $P(\mathbf{x}, T=1) > P(\mathbf{x}, T=0)$ because $G_{\mathbf{X}C}^{S,T}(\mathbf{x},c)$ is increasing in each of its arguments, proving (iii), and the second difference is positive whenever treatment lowers costs proving (ii). To see this last point, note that the second difference equals $\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(x,0)} \left[g_{C|\mathbf{X}}^{S=1,T=1}(c|\mathbf{x}) - g_{C|\mathbf{X}}^{S=1,T=0}(c|\mathbf{x}) \right] g_{\mathbf{X}}^{S=1,T}(x)\, dc d\mathbf{x}$ (reflecting the assumption that the treatment effect on costs does not alter the marginal distribution of $\mathbf{X}$) or, equivalently, $\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \left[G_{C|\mathbf{X}}^{S=1,T=1}(P(x,0)|\mathbf{x}) - G_{C|\mathbf{X}}^{S=1,T=0}(P(x,0)|\mathbf{x}) \right] g_{\mathbf{X}}^{S=1,T}(x)\, d\mathbf{x}$, which is positive from assumption (2).

(iv) $DEE(P) > 0$. $DEE(P)$ is $\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},1)} \left[P(\mathbf{x},1) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c) - P(\mathbf{x},0) g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c) \right] dc d\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c) dc d\mathbf{x}}$ or, equivalently,

$\frac{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},1)} [P(\mathbf{x},1) - P(\mathbf{x},0)] g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c) dc d\mathbf{x}}{\int_{\{\mathbf{x}:\mathbf{x}\in\chi\}} \int_{-\infty}^{P(\mathbf{x},1)} g_{\mathbf{X}C}^{S=1,T=1}(\mathbf{x},c) dc d\mathbf{x}} > 0$ whenever $P(\mathbf{x},1) > P(\mathbf{x},0)$ as assumed.

The ambiguity of the sign for $EEE\text{-}R(P)$, and $CRE\text{-}R(P)$ follows from a similar arguments as those made for the ambiguity of $SE(R)$. In the case of $EEE\text{-}R(P)$, suppose there is only one observable $X \in \left\{X^l, X^h\right\}$, and the population is equally split between those two levels. The conditional distribution of C is such that all those with X^l have costs $c^l > \tilde{c}^l \equiv P\left(X^l, T=0\right)$ so they choose not to run as controls, and all those with X^h have costs $\tilde{c}^h < \tilde{c}^h \equiv P\left(X^h, T=0\right)$ so they choose to run. Thus, among the controls, where low and high observables are equally represented, only the high quality individuals run and their electoral performance is $P\left(X^h, T=0\right)$. Suppose that treatment increases electability so that $P\left(X^l, T=1\right) > c^l > P\left(X^l, T=0\right)$, inducing X^l individuals to run, and assume that observables have a large impact on P relative to the effect of treatment so that the following condition holds,

$$P\left(X^h, T=1\right) - P\left(X^h, T=0\right) + P\left(X^l, T=1\right) - P\left(X^l, T=0\right) < P\left(X^h, T=0\right) - P\left(X^l, T=0\right). \tag{11}$$

Then, electoral performance among the treated is $\frac{P\left(X^l, T=1\right)+P\left(X^h, T=1\right)}{2}$ which is lower than the electoral treatment of the controls $P\left(X^h, T=0\right)$ as long as the condition (11) holds. An analogous example establishes the ambiguity of $CRE\text{-}R(P)$. □

Proof. of Remark 2:

(i) Note $E(R|T^M=0, R_C^M=U; T^D=1, R_C^D=U) - E\left(R|^M=0, R_C^M=U; T^D=0, R_C^D=U\right) = E(R|S=1, T=0) - E(R|S=0, T=0) = SE(R)$, and the OBD treatment effect is $E(R|S=1, T=1) - E(R|S=1, T=0) = TE(R)$.

(ii) For the first statement, note that the OBD treatment effect holds constant the set of individuals on whom the prediction is made to the treated who run, but note the predictors use coefficients that are computed on the treated who run and the controls who run, respectively. Therefore, the only difference stems from the coefficients, implying the OBD treatment effect reflects only Direct Electability Effects ($DEE(P)$). For the second statement, adding and subtracting $E_{\hat{\beta}_P^{T^M=0, R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1, R_C^D=R_0}\right)$, we can express the OBD selection effect on performance as,

$$\underbrace{E_{\hat{\beta}_P^{T^M=0, R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1, R_C^D=R_0}\right) - E_{\hat{\beta}_P^{T^M=0, R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0, R_C^D=R_0}\right)}_{SE(P)}$$
$$\underbrace{+E_{\hat{\beta}_P^{T^M=0, R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1, R_C^D=R_1}\right) - E_{\hat{\beta}_P^{T^M=0, R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1, R_C^D=R_0}\right)}_{TE\text{-}R(P)}.$$

Note this OBD selection effect contains both Renova selection effects $SE(P)$ (the difference in predictions made for $\mathbf{X}^{T^D=1, R_C^D}$ and $\mathbf{X}^{T^D=0, R_C^D}$), and electoral treatment effects from endogenous decisions to run $TE\text{-}R(P)$ (the difference in predictions made for $\mathbf{X}^{T^D, R_C^D=R_1}$ and $\mathbf{X}^{T^D, R_C^D=R_0}$) which are part of the overall treatment effect $TE(P)$, but are excluded from the OBD treatment effect. □

Proof. of Remark 3:

Construct the predictor difference

$$\begin{aligned}
& E\left(P|T^D = 1, R_C^D = R_0, T^M = 0, R_C^M = R_0\right) \\
& -E\left(P|T^D = 0, R_C^D = R_0; T^M = 0, R_C^M = R_0\right) \\
= \quad & E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_0}\right) - E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0,R_C^D=R_0}\right) = SE(P)
\end{aligned}$$

and subtract this from the conditional OBD selection effect on performance to get

$$\begin{aligned}
& E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right) - E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0,R_C^D=R_0}\right) \\
& -\left[E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_0}\right) - E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=0,R_C^D=R_0}\right)\right] \\
= \quad & E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_1}\right) - E_{\hat{\beta}_P^{T^M=0,R_C^M=R_0}}\left(P|\mathbf{X}^{T^D=1,R_C^D=R_0}\right) = TE\text{-}R(P).
\end{aligned}$$

□

D Appendix: Selection on unobservables

D.1 Augmented selection framework

The variable $s \in \{0,1\}$ indicates the situation when only a single judge evaluates an applicant, in which case we assume there is just one recommendation R_i^1 made by judge 1. When deciding on admission, *RenovaBR* leadership take into account the vector of recommendations $\mathbf{R}_i = sR_i^1 + (1-s)\left[R_i^1, R_i^2\right]'$ about individual i issued by the judge(s).

The values $V_i^L, V_i^j, j = 1,2$ that the leadership and each judge j place on admitting an individual i are,

$$
\begin{aligned}
V_i^L &= \mathbf{\Gamma}^{L\prime}\mathbf{X}_i + v_i + \zeta_i^L, \\
V_i^j &= \mathbf{\Gamma}^{j\prime}\mathbf{X}_i + v_i + \zeta_i^j, j = 1,2.
\end{aligned}
$$

These expressions reflect three facts. First, leadership and judges value the observable traits of applicants, albeit possibly differently, as expressed in the vectors $\mathbf{\Gamma}^{j\prime}$ and $\mathbf{\Gamma}^{L\prime}$. Second, both judges and leadership care about admitting individuals with higher valence (to save on notation, we assume they care equally about v). And third, judges may care about some element $\zeta_i^j, j = 1,2$, and leadership about an element ζ_i^L, that is uncorrelated with valence and observables. The elements ζ_i^L,$\zeta_i^j, j = 1,2$,remain unobservable to the analyst and do not affect our outcomes of interest. These elements might be relevant for selection, but do not pose an identification challenge because they are by definition unrelated to political outcomes.

We assume judges and leadership observe the vector $\mathbf{X}_i$ for each candidate without noise. Valence is not observable to leadership. Only judges get a signal about valence, and the signal is noisy: judge j observes a signal $\theta_i^j = v_i + \xi_i^j$, with ξ_i^j a zero-mean noise term. Getting no direct signal about valence, the leadership must form an expectation based on the recommendations from judges. We assume the random disturbances we have introduced are all independently normally distributed with zero expectation and respective variances $(\sigma_\varepsilon^2, \sigma_v^2, \sigma_{\xi^1}^2, \sigma_{\zeta^1}^2, \sigma_{\xi^2}^2, \sigma_{\zeta^2}^2)$.

We assume that refusing admission to a candidate yields 0 value to judges and leadership. Then, judge j recommends admission ($R_i^j = 1$) whenever the expected value of admission is non-negative, i.e., iff $EV_i^j = \mathbf{\Gamma}^{j\prime}\mathbf{X}_i + u_i^j \geq 0, j = 1,2$, where $u_i^j \equiv E\left[v_i|\theta_i^j\right] + \zeta_i^j$, also normally distributed. Thus, denoting with 1 the indicator function, judge j's recommendation is

$R_i^j = 1\left[\mathbf{\Gamma}^{j\prime}\mathbf{X}_i + u_i^j \geq 0\right]$. As both judges obtain signals about v_i,then u_i^1 and u_i^2 can be seen as drawn from a bivariate normal distribution with some correlation $\rho = \frac{cov(u^1,u^2)}{\sigma_{u^1u^2}} \geq 0$.

When deciding whether to admit an individual i, the leadership cares about expected value, $EV_i^L = \mathbf{\Gamma}^{L\prime}\mathbf{X}_i + u_i^L$, where $u_i^L \equiv E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right] + \zeta_i^L$, so the admission (i.e., treatment) decision is $T_i = 1\left[\mathbf{\Gamma}^{L\prime}\mathbf{X}_i + u_i^L \geq 0\right]$. Then the expectation of the outcome conditional on $(\mathbf{X}_i, T_i, \mathbf{R}_i)$ is,

$$E\left[y_i|\mathbf{X}_i,T_i,\mathbf{R}_i\right] = \alpha_0 + \alpha_1'\mathbf{X}_i + \alpha_2 T_i + \alpha_3 E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right], \tag{12}$$

where we have used the fact that (i) by definition ε_i is uncorrelated with $\mathbf{X}_i$,v, $\mathbf{R}_i$, ζ_i^L, and T_i, so $E\left[\varepsilon_i|\mathbf{X}_i,T_i,\mathbf{R}\right] = 0$ and (ii) since T_i yields no information about v_i beyond that contained in the recommendation from judges, then $E\left[v_i|\mathbf{X}_i,T_i = 1,\mathbf{R}_i\right] = E\left[v_i|\mathbf{X}_i,T_i = 0,\mathbf{R}_i\right] = E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]$ for any $\mathbf{X}_i,\mathbf{R}_i$.

Assuming selection on observables amounts to assuming that judges get no meaningful information about valence v_i, implying $E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right] = 0$. In this case, our empirical specification in (5) reduces to that in (4), as treatment can be considered exogenous conditional on observables.[15]

If judge recommendations carry information about valence, they enter the empirical specification in (5) through the term $E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]$. This term takes different forms depending on whether an applicant is evaluated by a single judge or two judges. Thus, we write $E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right] = I(1J).E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]_{1J} + (1 - I(1J))\,E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]_{2J}$, where $I(1J)$ is an indicator function for the case where a single judge is active. The appendix contains our development of the terms $E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]_k, k = 1J, 2J$. In the case where a single judge evaluates an applicant, this term takes the familiar form of the inverse Mill's ratio,

$$E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right] = E\left[v_i|\mathbf{X}_i,\mathbf{R}_i\right]_{1J} = \hat{\sigma}\frac{\phi\left(\frac{-\mathbf{\Gamma}^{1\prime}\mathbf{X}_i}{\sigma_{u^1}}\right)}{R_i - \Phi\left(\frac{-\mathbf{\Gamma}^{1\prime}\mathbf{X}_i}{\sigma_{u^1}}\right)} \equiv \hat{\sigma}\lambda\left(\mathbf{X}_i, R_i^1\right), \tag{13}$$

where ϕ and Φ are respectively the standard normal probability and cumulative density functions, and $\hat{\sigma} \equiv \frac{\sigma_v^2}{\sigma_v^2 + \sigma_{\xi^1}^2}\frac{\sigma_v^2}{\sigma_{u^1}}$. This term expresses the expectation about the valence unobservable given that the judge recommended admission (or not), expressed in terms of the expectation of the correlated generalized unobservable u_i^1 that drove the judge's decision.

[15] Since judge recommendations become irrelevant under selection on observables, the treatment decision $T_i = 1\left[\mathbf{\Gamma}^L\mathbf{X}_i + u_L \geq 0\right] = 1\left[\mathbf{\Gamma}^{L\prime}\mathbf{X}_i + E\left[v_i|\mathbf{R}_i\right] + \zeta_i^L \geq 0\right]$ becomes $T_i = 1\left[\mathbf{\Gamma}^{L\prime}\mathbf{X}_i + \zeta_i^L \geq 0\right]$, i.e., a function of observables and an outcome-irrelevant unobservable.

In the case where two judges evaluate an applicant, the term $E[v_i|\mathbf{X}_i,\mathbf{R}_i]$ is a sum of generalized Mill's ratios for judges 1 and 2 involving the first moments of a truncated bivariate normal distribution,

$$\begin{aligned} E[v_i|\mathbf{X}_i,\mathbf{R}_i] &= E[v_i|\mathbf{X}_i,\mathbf{R}_i]_{2J} = \tilde{\sigma}^a E\left(u_i^1|\mathbf{X}_i,\mathbf{R}_i\right) + \tilde{\sigma}^b E\left(u_i^2|\mathbf{X}_i,\mathbf{R}_i\right) \\ &\equiv \tilde{\sigma}^a \lambda^1(\mathbf{X}_i,\mathbf{R}_i) + \tilde{\sigma}^b \lambda^2(\mathbf{X}_i,\mathbf{R}_i), \end{aligned} \tag{14}$$

where $\left[\tilde{\sigma}^a,\tilde{\sigma}^b\right]$ are functions (detailed in the appendix) of distribution parameters $\sigma_v^2,\sigma_{\xi_j}^2,\sigma_{u^j}^2,\rho; j=1,2$. In the case where two judges observe independent dimensions of valence, their signals are fully independent and each term $\lambda^j(\mathbf{X}_i,\mathbf{R}_i), j=1,2$ adopts the standard Mill's ratio form $\lambda^j\left(\mathbf{X}_i,R_i^j\right)$, $j=1,2$, just as if that judge were acting alone. But in the more general case of judges who observe signals about the same valence magnitude, the control function term $\lambda^1\left(\mathbf{X}_i,R_i^1\right)$ for judge 1 when acting alone is different from the term when that judge acts in tandem with judge 2. In this more general case, the generalized Mill's ratio formulas are more involved, because we expect one judge to have seen a different unobservable if the other judge recommended the applicant for admission vs not. In the appendix, we derive formulas for $\lambda^j(\mathbf{X}_i,\mathbf{R}_i), j=1,2$ using the expressions for moments of truncated bivariate normal distributions in Rosenbaum (1961) and Muthén (1990).

Our empirical specification then becomes,

$$E[y_i|\mathbf{X}_i,T_i,\mathbf{R}] = \alpha_0 + \alpha_1'\mathbf{X}_i + \alpha_2 T_i + \alpha_3 \left\{ \begin{array}{c} I(1J).\hat{\sigma}\lambda\left(\mathbf{X}_i,R_i^1\right) \\ +(1-I(1J))\left[\tilde{\sigma}^a\lambda^1(\mathbf{X}_i,\mathbf{R}_i) + \tilde{\sigma}^b\lambda^2(\mathbf{X}_i,\mathbf{R}_i)\right] \end{array} \right\}. \tag{15}$$

In terms of empirical implementation, the expressions $\hat{\sigma},\tilde{\sigma}^a$, and $\tilde{\sigma}^b$ come out as part of the regression coefficient on the control function term. The term $\lambda\left(\mathbf{X}_i,R_i^1\right)$ can be computed once we estimate $\frac{-\mathbf{\Gamma}^{1\prime}}{\sigma_{u^1}}$ through a probit regression of recommendation decisions by single judges on applicant observables. The terms $\lambda^j(\mathbf{X}_i,\mathbf{R}_i), j=1,2$ can be computed once the econometrician recovers the Γ parameters that capture judge preferences and the correlation ρ in the unobservables taken into account by any pair of judges. Given a sufficiently high number of aspirants evaluated by any pair of judges, the respective Γ vectors and ρ can be obtained from a bivariate probit on the recommendations made by each judge pair. Unfortunately, not all judge pairs in *Renova* evaluated a sufficiently high number of aspirants. So we estimate individual judge probits to recover the Γ vectors and then construct the terms $\lambda^j(\mathbf{X}_i,\mathbf{R}_i), j=1,2$ for every possible level of ρ. We then study whether there is any ρ such that the control function terms are significant and/or affect the coefficient on treatment.

D.2 Expected valence

We start with the more involved case for two judges and drop the subscript i to save on notation. Denote $\mathbf{u} \equiv \left[u^1, u^2\right]$, $\Sigma_{v\mathbf{u}} \equiv \left[cov\left(v, u^1\right), cov\left(v, u^2\right)\right], \Sigma_{\mathbf{uu}} \equiv \begin{bmatrix} \sigma^2_{u^1} & \sigma_{u^1u^2} \\ \sigma_{u^1u^2} & \sigma^2_{u^2} \end{bmatrix}$, and $[\Sigma_{\mathbf{uu}}]^{-1} = \frac{1}{\sigma^2_{u^1}\sigma^2_{u^2} - \sigma^2_{u^1u^2}} \begin{bmatrix} \sigma^2_{u^2} & -\sigma_{u^1u^2} \\ -\sigma_{u^1u^2} & \sigma^2_{u^1} \end{bmatrix}$.

The law of iterated expectations implies, $E[v|\mathbf{X}, \mathbf{R}] = E[E[v|\mathbf{X}, \mathbf{R}, \mathbf{u}]|\mathbf{X}, \mathbf{R}]$ and joint normality of $v_i, \mathbf{u}$ implies $E[v|\mathbf{X}, \mathbf{R}, \mathbf{u}] = \mathbf{u}[\Sigma_{\mathbf{uu}}]^{-1}\Sigma'_{v\mathbf{u}}|(\mathbf{X}, \mathbf{R})$, so we can write,

$$E[v|\mathbf{X}, \mathbf{R}] = E\left[\mathbf{u}[\Sigma_{\mathbf{uu}}]^{-1}\Sigma'_{v\mathbf{u}}|\mathbf{X}, \mathbf{R}\right] = E[\mathbf{u}_i|\mathbf{X}_i, \mathbf{R}_i][\Sigma_{\mathbf{uu}}]^{-1}\Sigma'_{v\mathbf{u}}. \tag{16}$$

Therefore,

$$E[v|\mathbf{X}, \mathbf{R}] = E[\mathbf{u}|\mathbf{X}, \mathbf{R}] \frac{1}{\sigma^2_{u^1}\sigma^2_{u^2} - \sigma^2_{u^1u^2}} \begin{bmatrix} \sigma^2_{u^2} & -\sigma_{u^1u^2} \\ -\sigma_{u^1u^2} & \sigma^2_{u^1} \end{bmatrix} \begin{bmatrix} \frac{\sigma^2_v}{\sigma^2_v + \sigma^2_{\xi_1}}\sigma^2_v \\ \frac{\sigma^2_v}{\sigma^2_v + \sigma^2_{\xi_2}}\sigma^2_v \end{bmatrix}$$

where we have used $cov\left(v, u^j\right) = cov\left(v, E\left[v|\theta^j\right] + \zeta^j\right) = cov\left(v, \frac{cov\left(v, \theta^j\right)}{\sigma^2_{\theta_j}}\theta^j + \zeta^j\right)$, which using $\theta^j = v + \xi^j$ becomes, $cov\left(v, \frac{cov\left(v, v + \xi^j\right)}{\sigma^2_{\theta_j}}\left(v + \xi^j\right) + \zeta^j\right) = \frac{\sigma^2_v}{\sigma^2_v + \sigma^2_{\xi^j}}\sigma^2_v \equiv \tilde{\sigma}^j, j = 1, 2$. Therefore,

$$E[v|\mathbf{X}, \mathbf{R}] = \tilde{\sigma}^a E\left[u^1|\mathbf{X}, \mathbf{R}\right] + \tilde{\sigma}^b E\left[u^2|\mathbf{X}, \mathbf{R}\right],$$

where $\tilde{\sigma}^a \equiv \sigma^2_v \frac{\sigma^2_{u^2}\tilde{\sigma}^1 - \sigma_{u^1u^2}\frac{\sigma^2_v}{\sigma^2_v + \sigma^2_{\xi^2}}}{\sigma^2_{u^1}\sigma^2_{u^2} - \sigma^2_{u^1u^2}}$ and $\tilde{\sigma}^b \equiv \sigma^2_v \frac{\sigma^2_{u^1}\tilde{\sigma}^2 - \sigma_{u^1u^2}\frac{\sigma^2_v}{\sigma^2_v + \sigma^2_{\xi^1}}}{\sigma^2_{u^1}\sigma^2_{u^2} - \sigma^2_{u^1u^2}}$.

If u^1, u^1 are perfectly correlated, the matrix $\Sigma_{\mathbf{uu}}$ is not invertible. In that case, for a single judge with unobservable u^1, similar steps yield,

$$\begin{aligned} E\left[v|u^1, \mathbf{X}, R^1\right] &= u\frac{cov\left(u^1, v\right)}{\sigma^2_{u^1}}|\mathbf{X}, R^1 \\ E\left[v|\mathbf{X}, R^1\right] &= E\left[u^1.\frac{cov\left(u^1, v\right)}{\sigma^2_u}|\mathbf{X}, R^1\right] \\ &= E\left[u^1|\mathbf{X}, R^1\right]\frac{\sigma^2_v}{\sigma^2_{u^1}}\frac{\sigma^2_v}{\sigma^2_v + \sigma^2_{\xi^1}}. \end{aligned}$$

The well-known expression for the first moment $E\left[u^1|\mathbf{X},R^1\right]$ of a univariate truncated normal $(0,\sigma^2_{u^1})$ is $\frac{\phi\left(\frac{-\mathbf{\Gamma}^{1\prime}\mathbf{X}_i}{\sigma_{u^1}}\right)}{R^1_i-\Phi\left(\frac{-\mathbf{\Gamma}^{1\prime}\mathbf{X}_i}{\sigma_{u^1}}\right)} \equiv \lambda\left(\mathbf{X}_i,R^1_i\right)$ yielding the expression in (13) and (6). In the case of two judges, to obtain the terms $\lambda^1\left(\mathbf{X}_i,\mathbf{R}_i\right)$ and $\lambda^2\left(\mathbf{X}_i,\mathbf{R}_i\right)$ we rely on the expressions in Rosenbaum (1961) and Muthén (1990) to get $E\left[u^j|R^j,R^{-j}\right]$ given by,

$$\frac{\begin{array}{c}\left[R^j+(-1)\left(1-R^j\right)\right]\phi\left(\frac{-\mathbf{\Gamma}^{j\prime}\mathbf{X}_i}{\sigma_{u^j}}\right)\left\{\Phi\left[\left[R^{-j}+(-1)\left(1-R^{-j}\right)\right]\frac{\frac{\mathbf{\Gamma}^{-j\prime}\mathbf{X}_i}{\sigma_{u^{-j}}}+\rho\left(\frac{-\mathbf{\Gamma}^{j\prime}\mathbf{X}_i}{\sigma_{u^j}}\right)}{\sqrt{1-\rho^2}}\right]\right\}\\ +\left[R^{-j}+(-1)\left(1-R^{-j}\right)\right]\rho\phi\left(\frac{-\mathbf{\Gamma}^{-j\prime}\mathbf{X}_i}{\sigma_{u^{-j}}}\right)\left\{\Phi\left(\left[R^j+(-1)\left(1-R^j\right)\right]\frac{\frac{\mathbf{\Gamma}^{j\prime}\mathbf{X}_i}{\sigma_{u^j}}+\rho\left(\frac{-\mathbf{\Gamma}^{-j\prime}\mathbf{X}_i}{\sigma_{u^{-j}}}\right)}{\sqrt{1-\rho^2}}\right)\right\}\end{array}}{\begin{array}{c}R^jR^{-j}\pi\left(b_j,b_{-j}\right)+R^j\left(1-R^{-j}\right)\pi\left(b_j,a_{-j}\right)\\ +\left(1-R^j\right)R^{-j}\pi\left(a_j,b_{-j}\right)+\left(1-R^j\right)\left(1-R^{-j}\right)\pi\left(a_j,a_{-j}\right)\end{array}}, \tag{17}$$

where $\pi\left(b_j,b_{-j}\right)$ is the normal cdf truncated below at points $\left(b_j,b_{-j}\right)$, $\pi\left(a_j,a_{-j}\right)$ is the same distribution truncated above at points $\left(a_j,a_{-j}\right)$, etc.