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Essays on Political Economy and Development

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Essays on Political Economy and Development.

Chapter 1. *A Few Signatures Matter: Barriers to Entry in Italian Local Politics.*

This paper examines the causal effect of signature requirements – a widespread ballot access regulation – and finds that their impact goes beyond this goal. I use data on Italian local elections and apply a regression discontinuity design to estimate the effects of these requirements on electoral competition, candidates' selection, and voter participation. I find that signature requirements reduce the number of candidates running for office, decrease electoral competition, lead to a more experienced pool of candidates, and reduce voter turnout. The positive effects of this policy are observed in municipalities with fragmented political systems, where signature requirements lead to fewer wasted votes and fewer spoiler candidates. The downside is observed in municipalities with concentrated councils: signature requirements increase the frequency of uncontested races and reduce voter participation. Findings reveal how this barrier to entry impacts key dimensions of democracy and indicate that designing efficient electoral institutions requires a clear understanding of local political contexts.

Chapter 2. *Using Centralized Assignment to Evaluate Entrepreneurship and Life-Skills Training Programs in Argentina.* (joint with Diego Ubfal)

We implemented a centralized assignment mechanism (random serial dictatorship) to fill seats for a set of training courses offered by the government of Buenos Aires in schools across the city. We then use propensity score stratification to evaluate the short-term impact of two of the courses (an entrepreneurship course and a life-skills course). This strategy leads to important sample size gains relative to full preference stratification, allowing us to fully exploit the random variation in treatment assignment. Using survey-based information collected three months after the courses, we find positive effects on course-related knowledge for both programs. Entrepreneurship training helps participants start a business from an initial idea, leading to increased business ownership and self-employment. Life-skills training leads to higher job-search rates. There are no significant effects on soft skills.

Chapter 3. *Paying Politicians: A Semi-Structural Approach.*

This paper examines how the wage paid to politicians affects citizens' decision to participate in politics and their performance in office. Results show that higher wages lead to a pool of more able but less intrinsically motivated politicians. Overall, higher wages lead to better average performance. The effect is mainly driven by the increase in candidates' skills, but it is also favored by an enhanced role of elections, which discipline the office-motivated incumbents.

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A Few Signatures Matter: Barriers to Entry in Italian Local Politics

Santiago Pérez Vincent*

Abstract

Competition for public office is an essential feature of democracy but having many candidates competing for the same position might lead to voter confusion and be counterproductive. In current democracies, ballot access regulations limit citizens' right to become candidates, seeking to balance this trade-off by discouraging frivolous contenders. This paper examines the causal effect of signature requirements – a widespread ballot access regulation – and finds that their impact goes beyond this goal. I use data on Italian local elections and apply a regression discontinuity (RD) design to estimate the effects of these requirements on electoral competition, candidates' selection, and voter participation. I find that signature requirements reduce the number of candidates running for office, decrease electoral competition, lead to a more experienced pool of candidates, and reduce voter turnout. The positive effects of this policy are observed in municipalities with fragmented political systems, where signature requirements lead to fewer wasted votes and fewer spoiler candidates. The downside is observed in municipalities with concentrated councils: signature requirements increase the frequency of uncontested races and reduce voter participation. Findings reveal how this barrier to entry impacts key dimensions of democracy and indicate that designing efficient electoral institutions requires a clear understanding of local political contexts.

JEL codes: D72, H70, C14.
Keywords: signature requirements, running costs, electoral competition, voter turnout, regression discontinuity design.

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> "In an ideal political democracy competition is free in the sense that no appreciable costs or artificial barriers prevent an individual from running for office, and from putting a platform before the electorate."
>
> — Gary S. Becker, *Competition and Democracy*

> "To simplify matters we have restricted the kind of competition for leadership which is to define democracy, to free competition for a free vote. (...) Between this ideal case which does not exist and the cases in which all competition with the established leader is prevented by force, there is a continuous range of variation within which the democratic method of government shades off into the autocratic one by imperceptible steps."
>
> — Joseph A. Schumpeter, *Capitalism, Socialism and Democracy*

1 Introduction

Competition for public office is an essential feature of democracy. The presence of alternative viable candidates allows voters to express their preferences and keeps incumbents accountable. Indeed, political competition has been associated with positive economic and policy outcomes, such as high economic growth (Besley, Persson and Sturm, 2010) and active legislative representation (Konisky and Ueda, 2010). However, having many candidates competing for the same position may be counterproductive: it increases the complexity of voters' choices, potentially leading to voter confusion and the misrepresentation of the majority (Shue and Luttmer, 2009; Lau et al, 2014). In current democracies, ballot access regulations limit citizens' right to become candidates, seeking to balance this trade-off by discouraging frivolous candidates who do not have popular support and simply add noise to the electoral process.[1]

These artificial barriers to entry separate existing democracies from the democratic ideal of "free competition for a free vote" (Schumpeter, 2003) and might harm voters by substantially limiting the supply of candidates. Their potential value, however, is supported by recent studies that document some features of multi-candidate elections which point to benefits of having few candidates, especially under the plurality rule. First, such barriers may reduce the risk of voter confusion (Shue and Luttmer, 2009) and the prevalence of ballot order and adjacency effects by which candidates (particularly marginal ones) receive votes due to a favorable position on the ballot (Ho and Imai, 2008; King and Leigh, 2009). Second, a small number of candidates may reduce vote splitting, which occurs when people with similar preferences fail to coordinate, vote for different candidates, and dilute their chances of winning (Hall and Snyder, 2015; Pons and Tricaud, 2018). Pons and Tricaud (2018) show, in the context of French parliamentary and local

[1] Abrams (1996) examines the US Supreme Court's reviews of state ballot access laws, presenting arguments for and against them. In *Storer v. Brown* (1974), for example, the US Supreme Court recognized the "substantial state interest" in providing the electorate with an understandable ballot and therefore supported "reasonable requirements for ballot position". In Italy, signature requirements are intended to prove the candidates' representativeness of the electorate (*Istruzioni per la presentazione e l'ammissione delle candidature*, Italian Ministry of Internal Affairs).

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elections, that the presence of a third candidate reduces the vote share for the top candidate closest ideologically to her, frequently affecting the outcome of the election. In their words, the participation of a third candidate "often results in an outcome that harms a majority of her supporters (...) and a majority of voters".

A frequent institutional response to avoid an excess of candidates is requiring prospective candidates to collect a certain number of signatures to run for public office. This rule is now commonly accepted and has become the most widespread method of regulating candidacy submissions in current democracies.[2] Indeed, the "Code of Good Practice in Electoral Matters" (European Commission for Democracy through Law, 2003) provides recommendations on how these signature requirements should be implemented, indicating that they are theoretically compatible with the principle of universal suffrage. The document supports the commonly-held view that "only the most marginal parties seem to have any difficulty gathering the requisite number of signatures".[3]

However, despite its ubiquity and acceptance, there is scarce well-identified evidence on how these requirements impact electoral competition and whether and how they affect other related political outcomes. The ideal experiment to answer these questions requires elections to be compared across constituencies that differ only in the presence or lack of signature requirements. In this paper, I exploit a "natural experiment" that closely resembles this ideal study. I use a regression discontinuity (RD) design to estimate the causal effects of signature requirements on Italian municipal elections, exploiting the fact that candidates in cities with less than 1000 inhabitants are exempt from the signature requirements. I use information on more than 5000 mayoral elections in municipalities with 250 to 1750 inhabitants. I consider the period of 1993 to 2000, when the exemption in signature requirements did not coincide with any other policy change; this allows me to credibly identify the causal effects of this requirement.

This setting makes it possible to test how signature requirements – and their associated costs – affect the entry decisions of potential candidates and how changes in the candidate pool driven by this barrier to entry shape voter behavior. To this end, I examine the impact of signature requirements on a rich set of outcomes, including electoral results, voter participation, and candidates' personal characteristics, and exploit the heterogeneity in political fragmentation across Italian municipalities to assess whether these effects are moderated by local conditions.

[2]Afghanistan, Albania, Algeria, Andorra, Austria, Azerbaijan, Belarus, Belgium, Bosnia and Herzegovina, Bulgaria, Burundi, Canada, Croatia, Denmark, Ethiopia, Germany, Grenada, Guyana, Hungary, Iceland, Italy, Kazakhstan, Libya, Liechtenstein, Lithuania, Luxembourg, Mauritius, Mongolia, Montenegro, Netherlands, Norway, Palau, Paraguay, Poland, Russian Federation, Rwanda, Senegal, Slovenia, Suriname, Switzerland, Tonga, Turkey, Turkmenistan, Tuvalu, and the United Kingdom are among the countries identified by the Inter-Parliamentary Union (IPU) as requiring candidates to provide signatures or nominations from electors to participate in parliamentary elections. In some countries, these requirements apply only to independent candidates or to just one of the chambers. Information obtained from www.ipu.org (accessed October 3rd, 2017).

[3]European Commission for Democracy through Law (2003). *Code of Good Practice in Electoral Matters.* Council of Europe Publishing. p.16.

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I find that signature requirements reduce the number of candidates but that this reduction is not driven solely by marginal candidates and does not seem to substantially simplify voters' choices. The frequency of elections with more than two candidates, the number of wasted votes (defined as votes to candidates other than the top two), and the presence of potential spoiler candidates (that is, third candidates obtaining more votes than the difference between the winner and the runner-up) fall only slightly with signature requirements, and changes are not statistically different from zero (at standard confidence levels). Instead, signature requirements lead to an increase in the number of unopposed races and a reduction in political competition (as measured by the winner's share and the winner's margin).

Baseline RD estimates show that the average number of candidates falls by 0.23 with signature requirements, an 11% drop relative to the mean observed just below the threshold. The decrease in the number of candidates is also observed when considering the number of "effective" candidates (Laakso and Taagepera, 1979) and the number of "non-marginal" candidates – two measures that are mostly unaffected by the addition or exclusion of a candidate who receives a small share of votes.[4] Furthermore, the frequency of unopposed races doubles from 10 to 20%, and the average winner's margin increases 14 percentage points (from 29% to 43%). This reduction in political competition and the decrease in the number of non-marginal candidates exceed the aim of avoiding frivolous candidate applications and point to the first risk of signature requirements: acting as a barrier to entry for serious potential candidates and thereby reducing electoral contestation.

I then assess whether the observed reduction in electoral competition is driven by particular types of candidates. I use information on mayoral candidates to determine whether signature requirements alter the profiles of contenders and elected mayors. I find that signature requirements lead to a more experienced and older candidate pool, although these results are less precise than those observed in electoral competition. Importantly, I construct a measure of candidates' attractiveness (based on personal characteristics associated with winning elections) and find no significant change in this variable at the cutoff, a result that reinforces the idea that signature requirements do not discourage only frivolous or unattractive candidates. I also find no significant impact of these traits on incumbents' re-running and re-election likelihoods.

In addition to investigating these effects on candidate entry, I assess whether voters react to reductions in electoral competition (Palfrey and Rosenthal, 1983; Myatt, 2015; Feddersen and Sandroni, 2006; Coate and Conlin, 2004) or to the absence of their candidates of choice (Pons and Tricaud, 2017). I find evidence consistent with these models of voting behavior: signature requirements lead to a drop in voter turnout (by 4.6 percentage points), and an increase in the number of blank and null votes. These findings point to the second risk presented by signature requirements and barriers to political entry: reduced voter engagement. Electoral participation is considered an essential feature of a healthy democracy, and, therefore, policies with the potential to affect it should be

[4] The number of "non-marginal" candidates is defined as the number of candidates who obtain more than 25% of voter support or at least 85% of the winner's number of votes.

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carefully evaluated.

I build on the observed results on electoral competition to propose a framework that guides the normative assessment of the effects of signature requirements and provides insights on features of local contexts that could moderate them. The framework presents a simple trade-off: increasing running costs can reduce vote splitting at the expense of an increased risk of uncontested elections. In the model, there is one sure candidate (representing Group A) and two potential contenders (each representing one of two groups with similar preferences, B and C).[5] If only one contender runs, voters in Groups B and C support her and the expected outcome of the election reflects the relative size of Group A vs. Groups B and C. There are no wasted votes, and the majoritarian side always wins. If the two contenders both run, the electorate's votes are split, and the likelihood of Candidate A's victory increases. If no contender runs, Candidate A runs unopposed and wins. In short, both too many and too few candidates lead to a potential misrepresentation of the majority.

Running costs influence contenders' entry decisions; an increase in these costs reduces the likelihood of three-candidate races occurring ("positive" margin) and increases the chances of uncontested elections taking place ("negative" margin). The loss in terms of majority misrepresentation due to either excessive or insufficient numbers of candidates depends on the political context: (a) the greater the size of Group A, the smaller the expected loss due to uncontested elections; and (b) the greater the degree of coordination among the voters of the two contenders, the smaller the expected loss resulting from three-candidate races. This framework also indicates that the impact of a change in the running costs on each of these two margins depends on the contenders' winning chances and, therefore, on the relative sizes of the groups and the ability of voters to coordinate. For example, the reduction in the number of three-candidate races due to an increase in running costs increases as the coordination among contenders' voters falls.

I try to capture these features of the local political context using a measure of political concentration and assess whether the impact of signature requirements varies with it. I use the distribution of council seats in the year 1992 (before the exemption in signature requirements was introduced in Italy) to build a measure of fragmentation of the political system for each municipality in the sample, and I estimate the RD effect in cities of low and high fragmentation separately. The presence of more groups and no absolute majorities in councils can be associated both with the absence of clear favorites and with more dispersed preferences and lower coordination across groups. Indeed, in cities with fragmented political systems, the introduction of signature requirements has a significant impact on the "positive" margin – the number of races with more than two candidates, the number of wasted votes, and the presence of potential spoiler candidates significantly drop at the cutoff without a significant change in the number of uncontested races. In cities with concentrated political systems, the number of unopposed races more than doubles at the cutoff, and there are no significant changes on the "positive" margin. The described framework provides a clear normative assessment of these results: signature requirements

[5]The setting resembles that of a "divided majority" analyzed in various articles in the literature that examine multicandidate elections (see, for example, Bouton and Ogden, 2017).

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reduce the potential misrepresentation of the majority in cities with fragmented political systems but increase misrepresentation in politically concentrated ones. On a positive side, the framework also implies that in politically concentrated settings with clear favorites, the cost of having uncontested elections is smaller.

The differential impact of signature requirements on political competition across municipalities also helps demonstrate the drivers of voter participation. In cities with concentrated councils where signature requirements lead to a jump in the frequency of unopposed races, voter turnout drops and the number of blank and null votes increases. Contrastingly, voter participation does not significantly change in cities with dispersed political power. This finding links the two main risks of barriers to entry in politics, associating an increase in uncontested elections with a decrease in voter participation.

This paper has two main contributions. First, it highlights the importance, and the potential costs, of institutional barriers to entry; signature requirements change the observed extent of both electoral contestation and participation, two dimensions considered central to democratic functioning. Second, it stresses the relevance of the interaction between institutional and political factors in shaping politicians' and voters' behavior, pointing to the need to understand local political contexts to design efficient institutions.

This paper is related to three strands of literature. First, it provides evidence on the empirical validity of one of the most fundamental results of citizen-candidate models (Osborne and Slivinski, 1996; Besley and Coate, 1997): that running costs reduce the equilibrium number of (non-marginal) candidates. For most parameter configurations, these models admit two-candidate elections. However, if running costs are too low, many individuals might decide to run for office, and spoiler candidates become a possibility. If, instead, running costs are too high, one-candidate equilibria become possible. The results discussed in this paper align with these predictions, supporting the citizen-candidate theoretical framework.

Second, this paper relates to the set of articles examining barriers to entry in politics. The existing literature has focused mainly on legislative elections in the United States where most restrictions apply only to independent candidates, and it has also relied on selection-on-observables assumptions (Ansolabehere and Gerber, 1996; Stratmann, 2005) or on difference in differences (Drometer and Rincke, 2009). The evidence in these papers broadly indicates that these requirements reduce the number of candidates, especially those who are independent or from minor parties. This paper contributes to this literature by providing evidence using a novel identification strategy for a setting different than those previously analyzed – namely executive elections, in which requirements apply equally to all candidates. It also emphasizes the importance of political contexts in moderating the effects of these requirements. Other related papers have examined the role of campaign spending as a barrier to entry in politics (Milligan and Rekkas, 2008). In ongoing research, Avis, Ferraz, Finan and Varjao (2017) exploit a discontinuity in the rule establishing campaign spending limits for Brazilian mayoral candidates. They find that higher spending limits act as a barrier to entry, leading to fewer, more experienced and wealthier candidates. Their results regarding political competition and selection are

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similar to the findings in this paper; this helps in drawing a clear connection between two different forms of barriers to entry. In the setting examined here, in contrast to the Brazilian case, turnout is not mandatory; this allows me to examine how changes in the candidate pool impact voter participation. Avis, Ferraz, Finan and Varjao (2017) do not observe changes in this dimension.

Third, in terms of methodology, this paper ties into a developing strand of literature that uses RD designs based on population thresholds to assess the impacts of various policies on political and economic outcomes. Regarding Italian municipalities, recent articles have examined the effects of politicians' remuneration (Gagliarducci and Nannicini, 2013), electoral rules (Bordignon et al, 2016), and fiscal rules (Grembi et al, 2016).[6]

2 Signature Requirements in Italian Municipalities

Municipalities are the smallest administrative units in Italy and are in charge of the provision of public services (including several social services, and waste management). Each municipal government is composed by a mayor, an executive committee and a local council. These local institutions are regulated by national laws, which have been modified in different occasions during the last decades. In 1993, the National Parliament overhauled the municipalities' institutional framework, and established the direct election of the mayor in replacement of the existent parliamentary system.[7] The changes strengthened the role of the mayor, who became the "crucial player of municipal politics in Italy" (Bordignon, Nannicini and Tabellini, 2016), responsible for the administration and representation of the municipality, and with the right to appoint the members of the executive committee. The local council, also elected by the voters and previously the main local institution, remained only as a supervisory body, controlling governmental activities and voting on the local budget.[8]

In municipalities with less than 15,000 inhabitants, each mayoral candidate must be accompanied by a single list of candidates for the local council. Elections consist of a single round and voters cannot split their decision: they vote jointly for mayor and council members. The candidate with most votes wins the mayor position, and her companion list gets 2/3 of the seats in the council.[9] To participate in the election, each candidate

[6]Eggers et al (2016) provide a brief review of this literature and warn about the manipulation of population figures in Italy and other European countries, which could invalidate the RD assumptions and causal interpretations of the results. In the case examined in this paper, population data is predetermined relative to the policy change, ruling out the possibility of strategic sorting around the threshold. Nonetheless, I provide various validity checks that reveal no evidence of manipulation.

[7]Until 1993, citizens voted in an open list system for council members. The elected council would then choose the mayor. Law 81/1993 introduced the direct election of the mayor and established the institutional setting for Italian municipalities until the year 2000 (when it was replaced by *Legislative Decree 267/2000*). The law specified, among other things, the electoral rules, the requirements for potential candidates, and the responsibilities of elected officials and government bodies.

[8]The council can terminate the mayoral term by approving a vote of no confidence. That decision, which is really infrequent in Italian municipalities, implies also the dissolution of the council itself.

[9]Only if the two most voted candidates receive the exact same amount of votes, there is a second

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must file an administrative programme and a petition undersigned by a number of eligible voters (who cannot be among the list of candidates for the local council). Each citizen can only subscribe to one of the lists, and signatures must be certified either by a public notary or by the local authorities. The set of instructions for the presentation of political candidacies published by the Italian Ministry of Internal Affairs indicates that the collection of signatures is intended to ensure the representativeness of those who participate in the electoral race.[10] The amount of signatures needed depends on the population of the municipality, as computed by the last available national census, and jumps at nine different thresholds.

In municipalities with less than 1000 inhabitants, candidates do not need to present signatures. From that population threshold onwards, all candidates must collect some amount of subscriptions to participate in local elections: in particular, in municipalities with up to 2,000 inhabitants, candidates must collect and certify 30 signatures.[11]

The jumps in the number of signatures required facilitate the use of a regression discontinuity design to assess the causal effect of stricter ballot access restrictions on local political outcomes. However, in most cases, changes in signature requirements coincide with changes in other features of local institutions, compromising the plausibility of the identification assumptions.[12] In the 5,000 and 100,000 thresholds, mayors and council members remuneration increases. In the 10,000, 500,000 and one million thresholds, the size of the council increases. I focus the empirical analysis on the 1,000 inhabitants threshold for two main reasons. First, signature requirements are introduced at this threshold, thus permitting to compare two qualitatively different scenarios: *with* and *without* signature requirements (as opposed, for example, to the 2,000-inhabitants threshold where the change happens only in the intensive margin). The introduction of signature requirements implies that candidates go through a pre-electoral screening and have to deal with a greater amount of bureaucratic procedures (absent in municipalities below the threshold). Second, a practical consideration: sample size is large around the threshold and allows for a sensible statistical analysis.

For the empirical analysis, I consider just the period 1993-2000, since a law passed in October 2000 (*Legislative Decree 267/2000*) set a 10-percent increase in the mayors' wage at the 1,000-inhabitants threshold, introducing a potential confounding factor and compromising the soundness of the assumption needed to identify a causal effect. The law also reduced the number of signature required to be a candidate in municipalities with 1000

round.

[10] *Istruzioni per la presentazione e l'ammissione delle candidature.* Italian Ministry of Internal Affairs. 2015. p.13

[11] The rule implies that, in municipalities with exactly 1,000 inhabitants, candidates must collect signatures from 3 percent of the local population. A 3-percent signature requirement is high relative to the uses in other Western democracies. In US, for example, those states with signature requirements generally ask for less than 1 percent of eligible voters (Ansolabehere and Gerber, 1996). The *Code of Good Practice in Electoral Matters* (European Commission for Democracy through Law, 2003) argues explicitly for signature requirements being lower than 1% of the constituency concerned.

[12] Gagliarducci and Nannicini (2013) and Eggers et al (2016) provide a description of the policy changes in Italian municipal institutions occurring at the different population thresholds. Their nonetheless detailed description overlooks the changes in signature requirements.

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to 2000 inhabitants from 30 to 25. The period and the threshold chosen are particularly fit for the analysis for one additional reason: population figures used to determine the level of signature requirements come from the 1991 population census, and therefore were already determined when the jump in signature requirements at the 1,000-inhabitants threshold was introduced. Before 1993, signature requirements for council lists in municipalities with less than 5000 inhabitants were determined according to the following scale: 10 for municipalities with up to 2000 inhabitants, and 30 for the others.[13] No policies were set to change at the 1000-inhabitants threshold. This is crucial to overcome potential concerns on strategic manipulation of population figures that could invalidate the conclusions of the empirical analysis (I discuss this point further in Section 4.1).

3 Data and Empirical Strategy

To assess the impact of signatures requirements on local political outcomes, I collected information on Italian municipalities with population between 250 and 1750 inhabitants for the period 1993-2000. The sample consists of a total of 2693 municipalities (5408 electoral races), and includes information on electoral results, candidates' personal characteristics, municipal budgets, and socio-demographic indicators.

3.1 Data Sources

Municipal Elections. I obtained the information on municipal elections from the Historical Elections Archives published by the Italian Ministry of Internal Affairs. The information includes the names of all mayoral candidates, the number of eligible voters, the number of votes to each candidate, the number of blank and null votes, and the total seats in the local council obtained by each list in municipal elections since 1993.[14]

I use these data to compute different measures of electoral competition and voter participation. The average number of candidates in the sample is slightly above two: 16.3 percent of the electoral races are uncontested, 64.9 percent have two candidates, and 18.8 percent have three or more. Turnout (computed as total votes over registered voters) is, on average, 80 percent.

Candidates' Characteristics. The Register of Local Administrators published by the Italian Ministry of Internal Affairs provides age, gender, party list, place of birth, and self-reported measures of educational attainment and occupation for all members of municipal governments (mayors, members of the executive committee, and councilmen) since 1985. I match this information using candidates' names in the electoral data to retrieve personal characteristics of 10,690 candidates (96.5 percent of the total) and to construct a measure of experience in municipal government for each candidate. Candidates' average age is 46.6 years; more than 90% of them are male; and they have, on average, 13 years

[13] *Decree 570/1960*, and its subsequent modifications.

[14] The information was downloaded from the website: http://elezionistorico.interno.it/ (accessed on April 2nd, 2016). The data set does not include information on municipalities in Sicilia, Valle d'Aosta, Friuli-Venezia Giulia and Trentino-Alto Adige. The electoral information for these regions is not systematically reported in the consulted source.

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of schooling (high school completed) and 5.3 years of experience in government (counting from 1985).

I complement candidates' personal information with data on the distribution of surnames by municipality computed from the universe of personal tax returns in 2005, originally used by Gagliarducci and Manacorda (2016). I use these data to compute the observed frequency of candidates' surnames as a measure of the depth of their social ties. The median candidate in the sample shares her surname with other 5 adults in the municipality, much more than the median tax-reporting adult in these municipalities (who has a unique surname).

Pre-1993 Councils Composition. I also use the information of the Register of Local Administrators to compute the composition of the local councils in the period 1985-1992.[15] I use the name of the party list of each council member to count the number of different groups in the council and to build a Herfindahl-Hirschman index of seat concentration (which gives the probability that two council members chosen at random belong to the same party). This index is a widely-used measure of concentration in legislatures. It is calculated as the sum of the square of the fractions obtained by each of the lists (i.e. $\Sigma_{i=1}^{N} s_i^2$, with s_i being the fraction of seats obtained by list i). The index takes its highest value (equal to 1) when one list has every seat in the legislature.

Socio-Demographic Indicators. I also use information from the Italian 1991 National Census published by the Italian National Institute of Statistics (Istat).[16] Importantly, from this census, I obtain the official number of inhabitants in each municipality. This figure is used to establish the number of signatures required to stand as candidate in local elections. I also obtain information on population density, age structure, and labor market conditions in the different municipalities.

3.2 Empirical Strategy

To estimate the impact of signature requirements on any political outcomes of interest it is necessary to solve the endogeneity problem that arises if these requirements are correlated with other (potentially unobservable) variables that also determine it (as it is likely to happen, for example, if signature requirements are a constant fraction of constituencies' population). I use a sharp regression discontinuity design (RDD) to deal with this potential endogeneity issue, exploiting that signature requirements are introduced at the 1000-inhabitants threshold. This institutional setting generates arguably exogenous variation in signature requirements, allowing me to estimate their causal effect on local political outcomes.

Following Hahn, Todd and Van der Klaaw (2001), I use the Rubin causal framework to state the identification assumption that allows me to estimate the (local) effect of signature requirements. Let $Y_i(r)$ be the potential outcome Y in municipality i given an

[15] Electoral information for the municipalities in the sample is available only from 1993, after the change in the electoral system described in Section 2.

[16] Census results are publicly available at Istat's website: www.istat.it

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institutional setting (r), which can be either "no signature requirements" (n) or "signature requirements" (s). The potential outcome is the value a variable would take under either institutional arrangement and might depend on population (P). I make the following assumption:

RDD Assumption. $E[Y_i(s)|P = p]$ and $E[Y_i(n)|P = p]$ are continuous in P at P_0.

The assumption states that the potential outcomes of the variables of interest do not show a discontinuity at the relevant threshold. Under this continuity assumption, a jump in these variables at that threshold can be interpreted as an effect of the introduction of signature requirements. Hence, the local average treatment effect at the threshold $\tau_{SRD} \equiv E[Y_i(s) - Y_i(n)|P = P_0]$ can be identified by:

$$\tau_{SRD} = \mu_+ - \mu_- \quad \text{with} \quad \mu_+ \equiv \lim_{p \to P_0^+} E[Y_i(s)|P = p] \quad \text{and} \quad \mu_- \equiv \lim_{p \to P_0^-} E[Y_i(n)|P = p]$$

For estimation and inference, I follow Calonico, Cattaneo, Farrell and Titiunik (2018) and use a covariate-adjusted local-linear estimator of τ_{SRD}. The covariate-adjusted estimator can lead to important efficiency gains relative to the standard unadjusted estimator. The consistency of this estimator requires, in addition to Assumption 1, that there is no RD treatment effect on the covariates (Calonico et al, 2018). The estimator is formally given by $\hat{\tau}_{SRD}(h) = b_0^+ - b_0^-$, with b_0^+ and b_0^- resulting from the following local linear least-squares estimation:

$$\underset{b_0^+, b_1^+, b_0^-, b_1^-, \gamma}{\arg\min} \sum_{i=1}^{n} (Y_i - \mathbf{1}_{(P<P_0)}(b_0^- - P_i b_1^-) - \mathbf{1}_{(P>P_0)}(b_0^+ - P_i b_1^+) - \gamma Z)^2 K_h(P_i - P_0)$$

where Z is a set of covariates, h is a positive bandwidth, $K_h(\cdot)$ is a kernel function, and $\mathbf{1}_{(\cdot)}$ denotes the indicator function. The kernel function (that assigns greater weights to observations close to P_0) and the bandwidth localize the fit of the regression near to the threshold. I estimate the regression using covariate-adjusted mean squared error optimal bandwidth (h), a triangular kernel, and compute robust (to choice of bandwidth) confidence intervals, which are shown to provide better empirical coverage than the alternatives available in the literature (Calonico et al, 2014b; Calonico et al, 2018). In the tables and in the appendix, I provide the robustness of the results to the use of a local quadratic regression and alternative bandwidths.

4 Empirical Results: The Effect of Signature Requirements on Local Politics

In this section, I present the RD estimates of the effect of signature requirements on different electoral outcomes. I first discuss a set of validity checks that support the plausibility of the RDD assumptions and the causal interpretation of the estimates.

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4.1 Validity of RDD Assumptions

The two main threats to the RDD identification assumption are the presence of strategic sorting of units around the cutoff and the existence of multiple treatments occurring at the cutoff. Eggers et al (2016) discuss the potential problems of RD designs using population thresholds in the Italian context, and indicate there is suggestive evidence of manipulation around some of these thresholds. However, their evidence refers mainly to thresholds where municipal authorities' salaries change, something that does not occur in the setting analyzed in this paper. In the period 1993-2000, the only policy change at the 1000 population threshold was the introduction of signature requirements.[17] Furthermore, as stated in Section 2, population figures used in this period to determine the level of signature requirements were those of the 1991 National Census. In 1991, there were no changes in municipalities' institutional framework at the 1000-inhabitants threshold.[18] The jump in signature requirements at the 1000-inhabitants threshold was introduced later, in 1993. The fact that population figures were already set when the bill was proposed in 1992 eliminates the possibility of strategic sorting around the threshold.[19] The draft of the bill sent to the legislature didn't mention explicit population thresholds for signature requirements, which were introduced in later readings.

Nonetheless, to further address these concerns, I check for the existence of a jump in the density of the running variable (population) at the 1000-inhabitants threshold, a sign of potential manipulation. Figure A.1 in the appendix displays the frequency of municipalities using two different bin widths (20 and 40 inhabitants), and shows no bunching around the threshold. I also formally test for the presence of a jump using the manipulation test proposed by Cattaneo, Jansson and Ma (2017). Table A.1 (appendix) reports the results of the test, using both a linear and a quadratic local polynomial density estimator: the null hypothesis of no jump cannot be rejected (p-values of 0.70 and 0.80, respectively).

To credibly interpret the RD estimates as causal effects it is also crucial that no other determinant of the outcomes of interest varies discontinuously at the 1000-inhabitants threshold (that is, that there are no multiple treatments). Importantly, as indicated above, no other policy changed at that threshold in the period of analysis. I also check for discontinuities in a set of pre-determined socio-demographic variables obtained from the 1991 National Census. Results, reported in Table A.2 in the appendix, show no signs of systematic discontinuities at the 1000-inhabitants threshold, providing further support to the validity of the empirical design. In Tables A.3 and A.4, I report the (placebo) RD effects on the set of region, year and month fixed-effects used as covariates in the main regressions. There are no significant jumps in these variables at the cutoff.[20]

[17] Eggers et al (2016) provide a detailed list of different policies changing at specific population thresholds, but they overlook changes in signature requirements. The only jump they report at the 1000-inhabitants threshold is the increase in wages introduced in October 2000 (*Decreto Legislativo 267/2000*). I confirmed this by doing an independent institutional background check.

[18] *Decree 570/1960* and its subsequent modifications.

[19] *Bill C.72, April 23rd 1992, XI Italian Legislature*

[20] The continuity at the cutoff of these variables is needed for the covariate-adjusted estimator described in Section 3.2 to be consistent (Calonico et al, 2018).

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Finally, Table A.5 shows the results of placebo regressions on the number of lists and the seat concentration in local councils for the period 1985-1992, when there were no changes in signature requirements. I find no evidence of a pre-existing jump in these outcomes at the 1000-inhabitants threshold.

4.2 Signature Requirements and Electoral Competition

I now examine the impact of signature requirements on the number of candidates and electoral competition. Table 1 reports the RD estimates for a set of outcomes of interest. Each row in the table corresponds to one dependent variable. Column (1) displays the estimates for the baseline specification: local linear regression using the mean-squared-error (MSE) optimal bandwidth proposed by Calonico, Cattaneo, Farrell and Titiunik (2018). Column (2) shows the estimate using a local quadratic regression and MSE optimal bandwidth. Column (3) reports estimates for local linear regressions and a fixed bandwidth of 150 inhabitants. As opposed to the first two, this last column provides estimates using the same effective sample and number of observations across all different outcomes. All regressions include municipality controls, and region, year and month fixed effects (reported in Tables A.2-A.4).

RD results show that signature requirements significantly reduce the number of candidates. The baseline estimate shows a fall in the number of candidates of 0.23, eleven percent of the mean in municipalities just below the threshold. To assess if the fall in the number of candidates is driven solely by marginal candidates, I construct two alternative measures: First, I compute the number of "non-marginal" candidates by counting candidates who obtain the votes of more than 25% of the eligible voters (that is around 230 votes in municipalities close to the threshold or more than 7 times the amount of signatures needed to run) or get at least 85% of winner's number of votes. The idea behind this variable is to leave aside frivolous candidates, and measure how many people with substantial popular support participate in the election. Second, I calcute the "effective" number of candidates (Laakso and Taagepera, 1979). This measure is given by the inverse of the sum of the squared vote shares of all candidates (that is, the inverse of the Herfindahl-Hirschman vote concentration index). If one candidate gets all of the votes, the effective number candidates is equal to one. If all candidates split votes in equal parts, the effective number of candidates is equal to the number of people running. These two measures should be unaffected (or almost) by the addition or exclusion of a candidate who receives a small share of votes. RD estimates indicate that signature requirements significantly reduce both of these measures, showing that also people who potentially receive substantial support are affected by the introduction of these requirements.

Under plurality rule, the potential benefits of reducing the number of candidates are relevant in elections with more than two candidates. It is in these cases when limited strategic behavior from voters might lead to a misrepresentation of the majority. In a recent paper, Pons and Tricaud (2017) show, in the context of French parliamentary and local elections, that the presence of a third candidate reduces the vote share for the top candidate closest ideologically to her, frequently affecting the outcome of the election.

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In the context analyzed here, most candidates belong to local parties without common denominations and it is not possible to sort them in terms of ideology. To assess if signature requirements are potentially reducing cases of misrepresented majorities, I estimate their effect on three aspects of elections: (a) the frequency of races with more than two candidates; (b) wasted votes (calculated as votes going to candidates other than the two top as a percentage of eligible voters); and (c) the frequency of elections where the number of wasted votes is larger than the difference between the winner and the runner-up. This last variable indicates the potential presence of a spoiler candidate. RD estimates show no significant impact of signature requirements on these dimensions. Coefficients are negative for the three variables across all different specifications, showing that signature requirements might be "simplifying" voters' problem by reducing the need of strategic considerations. However, standard errors are large and it is not possible to reject the null hypothesis of no effect.

Signature requirements have a clearer impact on a different margin: the frequency of unopposed races. The introduction of these requirements leads to a sharp increase in the number of one-candidate elections. RD baseline estimate indicates that the frequency of unopposed races doubles at the cutoff, jumping 10 percentage points (from 10 to 20 percent). This increase underscores one potential risk of stricter barriers to entry: reducing "too much" the number of candidates. While, as discussed above, having too many candidates might be prejudicial, it is harder to argue in favor of unopposed races. Electoral contestation and, in particular, the presence of at least two valid alternatives is usually considered an essential feature of well-functioning democracies.

Figure 1 shows the distribution of the number of candidates and "non-marginal" candidates in elections for municipalities just around the threshold (with 950 to 1050 inhabitants), and helps contextualize the above results. The histograms reveal two key facts about mayoral elections in these municipalities: First, even in the absence of signature requirements, there are few candidates running (almost 80 percent of the races have just two candidates). Second, most candidates get substantial support, with runners-up obtaining, on average, more than 230 votes (or almost 8 times the number of signatures needed to run in cities above the threshold). This electoral context implies that changes in the number of candidates are likely to result in important changes in the extent of political competition. Indeed, Table 1 shows that signature requirements lead to a significant fall in electoral competition, as measured by the winner's vote share (percentage of votes obtained by the winning candidate), and the winner's margin (the difference between the votes obtained by the winner and the runner-up divided by the sum of their votes). In the baseline specification, average winners' share increases 7.9 percentage points; and average winners' margin increases 14 percentage points, from 29 to 43 percent.

Figure A.2 provides the graphical representation of the regression discontinuity design. The jump at the 1000-inhabitants threshold can be clearly seen for each of the variables. To further assess the robustness of the results to different bandwidths, I estimate the RD effects for each variable using bandwidths between 50 and 300 inhabitants (every 10 inhabitants). The different coefficients (and their 95% confidence interval) are displayed in Figure A.3. The graphs show that coefficients maintain their statistical significance

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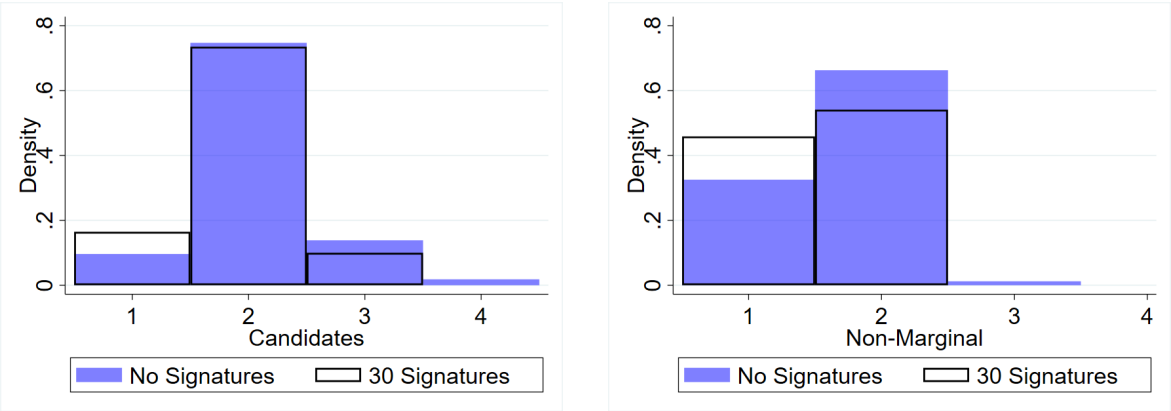
Figure 1: Signature Requirements and Number of Candidates



[a] Left panel: raw number of candidates. Right panel: non-marginal candidates (as defined in Section ??). Frequencies computed using information from elections in municipalities with 950 to 1050 inhabitants. Number of elections below (above) the threshold: 166 (170).

and are stable across specifications. Lastly, I estimate placebo RD effects at 300 arbitrary thresholds (specifically, at every value in the ranges 700-850 and 1150-1300) and I compare them with the true coefficient. This exercise helps to assess the reliability of the design by checking if there is evidence of systematic discontinuities at other cutoffs. The expectation is to find few jumps similar to the baseline results. Figure A.6 (first five graphs) shows the distribution of the placebo RD effects: for each variable, as expected, the distribution of the false coefficients is centered around zero and the value of the true coefficient lies at (or very close to) one of the extremes of the distribution. Table A.7 reports some summary statistics of the placebo effects and facilitates the comparison with the true coefficient.

Overall, results show that signature requirements have a large and significant impact on local electoral races, which goes beyond the goal of ensuring the representativeness of the candidates and avoiding frivolous ones. These findings underscore that signature requirements act also as a barrier to entry for serious potential candidates, who might be deterred from running for office. This is particularly important in settings as the analyzed here where the perks of office might not compensate the additional costs. This has two important implications: First, the normative evaluation of this policy should carefully weigh the potential benefits of avoiding frivolous candidates against the potential costs of discouraging serious contenders. Second, the fact that "only the most marginal parties seem to have any difficulty gathering the requisite number of signatures"[21] cannot be used as a sound criterion for such evaluation.

[21] European Commission for Democracy through Law (2003). *Code of Good Practice in Electoral Matters.* Council of Europe Publishing. p.16.

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4.3 Signature Requirements and Selection

To further assess the impact of signature requirements on local politics, I estimate their effect on a set of candidates' and mayors' personal characteristics. Table 2 reports RD estimates of the effects for candidates' characteristics (using averages across candidates for each election). Table 3 reports results for mayors. Both tables show estimates using alternative specifications, with each row corresponding to one dependent variable.

Signatures requirements do not impact on the gender distribution of candidates and mayors. More than 90% of candidates and mayors are male, both above and below the cutoff. The clearer findings in terms of political selection, which are still less precise than those observed in electoral competition, refer to governmental experience and age. Coefficients on age are consistently positive across specifications (both for mayors and candidates). This result is consistent with older candidates being more able to bear the costs associated to signatures collection, something that could be explained, for example, by them having better connections among neighbors or more spare time to devote to the associated bureaucratic procedures. I test this hypothesis by checking if the "surname frequency" (% of tax-reporting adults sharing the same surname of the candidate or mayor) of mayors and candidates increases at the cutoffs: coefficients are consistently positive but imprecise, giving no clear conclusions on whether signature requirements favor people with deeper social ties or not. Signature requirements do increase the average governmental experience of candidates. Baseline estimates show that candidates' average experience in local government jumps almost 0.85 years at the cutoff, a 16% increase relative to the mean just below the threshold. This jump translates to a similar (but less precisely estimated) increase in mayors' average governmental experience. Mayors' average education, on the other hand, falls with the introduction of signature requirements.

To observe if, beyond changes in some specific characteristics, signature requirements impact on candidates' general appeal or quality, I follow Avis et al (2017) and construct a measure of candidates' "propensity to win". I use electoral races in the period 1993-2000 in municipalities with 2000 to 3000 inhabitants (that are not included in the RD sample) to estimate how different personal characteristics (gender, age, schooling, experience in government, incumbency, and surname frequency) relate to the probability of winning an election. The model shows that schooling, experience in government and incumbency are strong predictors of electoral victory (Table A.6). I then use the estimated coefficients to predict the "propensity to win" of each candidate in the RD sample. RD estimates show a slight increase in the average propensity to win at the cutoff, which is not statistically significant at standard confidence levels. The absence of a significant change is again consistent with signature requirements not just discouraging frivolous or unattractive candidates. As mentioned in the previous section, most candidates in these municipalities (both with and without signature requirements) receive a substantial amount of votes and therefore it is not surprising to see that the average probability of winning does not significantly change at the cutoff.

Finally, I then estimate the effects signature requirements on incumbents' behavior. In these period (1993-2000), incumbents did not face binding term-limits (since terms served as mayors previous to 1993 were not taken into consideration). Signature requirements

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do not significantly impact on incumbents' unconditional probability of re-running (Table 2, last row) or being re-elected (Table 3, last row). In this last case, coefficients are consistently positive across specification (pointing to signatures increasing incumbency advantage) but estimates are imprecise and the null hypothesis of no effect cannot be reject at standard confidence levels.

4.4 Signature Requirements and Voter Participation

In addition to electoral competition and candidates' selection, I estimate the impact of signature requirements on voters' electoral participation. I consider three different variables: turnout, blank and null votes, and candidate votes (that is, votes casted for one of the candidates in the election). In all cases, I construct the variable as a percentage of registered voters. Table 4 reports the RD estimates for alternative specifications, and Figure A.4 provides graphical evidence of the effects. Baseline RD estimates indicate that signature requirements lead to a drop of 4.6 percentage points in turnout, and a 1.4 percentage points increase in null and blank votes. These effects add up to a large and significant fall in the number of candidate votes: 6 percentage points. All results are stable and statistically significant across different bandwidths (as shown in Figure A.5). Figure A.6 (last 3 graphs) shows the distribution of the RD effects estimated at placebo cutoffs for the different variables: again, as expected, placebo effects are centered around zero and the true effects lie at or very close the extremes of the distributions.[22]

The observed drop in turnout and in the number of candidate votes could be explained both by a rational response to the fall in electoral competition or by an expressive reaction to the absence of a candidate of choice. These results are not informative of the relative empirical validity of alternative models of voting behavior, but they still provide some additional information. First, they show that voters respond to the characteristics of the electoral race (as it is fair to assume that signature requirements do not have a direct impact on voter participation). Second, they confirm that the impact of signature requirements on competition and selection are non-trivial. Importantly, these results point to another risk of introducing barriers to entry in politics: reducing voters' engagement. Electoral participation is an essential feature of healthy democracies, and, therefore, policies that have the potential of affecting it should be carefully evaluated.

5 The Costs and Benefits of Signature Requirements

The observed effects of signature requirements on electoral outcomes (especially those in the number and characteristics of candidates) can be understood through the lenses of citizen-candidate models (Osborne and Slivinski, 1996; Besley and Coate, 1997). These models provide a theoretical framework to examine how running costs influence the entry

[22] In the case of turnout and valid votes, there seem to be some "discontinuities" at a few other thresholds (also evident in graphs (a) and (c) in Figure A.4). It is important for the reliability of the results that these jumps are far away from the 1000-inhabitants threshold and that only 4.6 percent (turnout) and none (candidate votes) of the placebo effects are larger in absolute value than the true one (Table A.7).

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decision of potential candidates under plurality voting. For most parameter configurations, these models admit two-candidate elections, giving theoretical support to Duverger's law (Duverger, 1963). However, if running costs are too low, more people might decide to run for office and the presence of spoiler candidates turns into a possibility. If, instead, running costs are too high, one-candidate equilibria (with the sole candidate proposing policies distant from the "median" position) become possible. Therefore, the normative value of a policy that modifies running costs should consider its impact on both these margins and the relative costs of having an excessive or an insufficient number of candidates.

I propose a theoretical framework to capture this basic trade-off. The main idea is that, in a plurality election, the effective competition is among two candidates, an intuition that, according to Riker (1982) goes back to Droop (1869).[23] The second idea is that adding a second candidate to a one-candidate election cannot reduce voters' welfare and might actually improve it. In elections with two candidates, the only non-weakly dominated strategy is to vote sincerely. Therefore, the extra candidate cannot harm voters: she will only be elected if preferred by the majority. The final idea is that third candidates can harm voters by splitting the votes of the supporters of one of the top two. As mentioned in the introduction, Hall and Snyder (2015) and Pons and Tricaud (2017), among others, provide empirical evidence on voting behavior in multi-candidate elections that support this last point.

The framework focuses on the "quantity" dimension of the pool of candidates, neglecting most of the discussion on its "quality". In classic citizen-candidate models (Osborne and Slivinski, 1996; Besley and Coate, 1997), changes in entry costs might impact on the profile of candidates without necessarily changing the number of candidates. The main intuition obtained from these models in this regard is that, as entry costs increase, equilibria with more extreme candidates become possible. The literature on politicians' wages and candidate selection also provides useful insights on the relationship between entry costs and the politicians' quality, especially if these costs are heterogeneous across candidates (Besley, 2004; Caselli and Morelli, 2001, 2004). In these cases, the joint distribution of entry costs and governmental ability become crucial to understand if increasing barriers to entry could increase or decrease the average quality of candidates. Despite the theoretically importance of these channels, I focus on the quantity dimension since results from the previous section indicate that, in the examined setting, the most relevant (and clearer) impacts of signature requirements occur in this margin. The following framework tries to capture how signature requirements might impact on the number of candidates and obtain normative implications, examining how some characteristics of the political context moderate this relationship.

General Setting. Individuals in a municipality elect their mayor under plurality voting:

[23] Droop (1869), as quoted in Riker (1982): "Each elector has practically only a choice between two candidates or sets of candidates. As success depends upon obtaining a majority of the aggregate voters of all the electors, an election is usually reduced to a contest between the two most popular candidates or sets of candidates. Even if other candidates go to the poll, the electors usually find out that their votes will be thrown away, unless given in favour of one or other of the parties between whom the election really lies."

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the person with the most votes among those who enter the election gets the position. They are divided in 3 groups $\{A, B, C\}$, each one with a preferred policy in the set $\{a, b, c\}$. Preferences are as following:

$$A : a \succ b \sim c \qquad B : b \succsim c \succ a \qquad C : c \succsim b \succ a$$

People in group A strongly prefer a over the other two alternatives (and they are indifferent between b and c). People in B weakly prefer b over c, and strongly prefer any of these two over policy a, while people in C weakly prefer c over b, but also strongly prefer any of these over a.

Each group is potentially represented by one candidate, who is committed to implement the group's preferred policy if elected. The model has two stages: an entry stage and a election stage. For simplicity, I assume that candidate A always participates in the election. Therefore, in the entry stage, only candidates B and C decide whether to enter or not in the electoral race.

Election Stage. Elections might have one, two or three candidates. In one-candidate elections, there is no uncertainty: candidate A runs alone and is elected mayor. In two-candidate elections, candidate A is only supported by her group regardless of who she is running against. She gets an expected share of votes equal to $\alpha \in (1/3, 2/3)$, which represents the fraction of citizens in group A. Her contender (candidate B or C) receives the support of the other two groups. The result of the election is influenced by the realization of a random variable $\mu \sim U[-\xi, \xi]$, which represents a popularity shock that drives the support of swing voters in the different groups.[24] The actual share of votes for candidate A is equal to $\alpha + \mu$, and the probability that she wins a two-candidate election is:

$$p_{A/2} \equiv \quad P(\alpha + \mu > 1/2) \quad = \frac{\alpha - 1/2 + \xi}{2\xi}$$

From the formula, we obtain that the greater is α, the higher are the chances that candidate A wins the election. In three-candidate elections, candidate A still receives a share $\alpha + \mu$ of the votes, but candidates B and C split the rest of the votes: with probability 1/2 candidate B (C) receives a share $p \in (1/2, 1]$ of these votes and candidate C (B) obtains the rest. This simple voting behavior can be contrasted to the results by Bouton and Ogden (2017), who propose a model of ethical voting in multi-candidate elections in a "divided majority" setting.[25] In their model, there are two types of equilibria: a sincere voting one (each person supports her preferred candidate) and Duverger's law equilibria (voters in B and C coordinate over one of the two candidates). This last type of equilibria is less likely to occur whenever the utility differential between b and c is large, the utility differential between a and the other two policies is small, candidate A is a not an extremely serious threat, and groups B and C are of similar sizes. The difficulty to

[24] In the following analysis, I restrict the attention to cases where $\xi > \max\{\frac{1}{2} - \alpha, \alpha - \frac{p}{1+p}\}$. This restriction implies that there is no sure winner. The uncertainty surrounding is large enough so that candidate A's probability of winning is strictly between 0 and 1. Parameter p is defined in the following paragraph.

[25] The situation described here corresponds to a "divided majority" setting whenever $\alpha \in (1/3, 1/2)$.

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coordinate could be further augmented if there is limited strategic behavior from voters. Parameter p in this model captures all these different factors that might enhance or undermine the ability of voters in groups B and C to coordinate. In three-candidate races, the winning probability of candidate A:[26]

$$p_{A/3} \equiv \quad P(\alpha + \mu > p(1 - \alpha - \mu)) \quad = \frac{\alpha - \frac{p}{1+p} + \xi}{2\xi}$$

The expression shows that the greater is p and the smaller is α, the lower is the probability that candidate A wins in a three-candidate race. In the extreme case, with $p = 1$, candidate A's probability of winning is not affected by the entry of a third candidate.

Entry Stage. In the entry stage, candidates B and C decide simultaneously whether to enter the electoral race or not. Candidates are office-motivated. The value of office (V) is drawn from a distribution with cumulative density function F_V and is observed by both candidates before taking the entry decision. They compare the expected value of running (that is, the probability of winning multiplied by V) against the cost of doing so (given by parameter c). The pure-strategy Nash equilibrium of the game can be characterized by two threshold values V_1 and V_2, such that for all $V < V_1$ neither of the potential contenders runs and for all $V > V_2$ both do it. The value of these two thresholds is given by:

$$V_1 = \frac{c}{1 - p_{A,2}} \qquad\qquad V_2 = \frac{2c}{1 - p_{A,3}}$$

The probabilities of observing a one-candidate race (p_1) and and a three-candidates race (p_3) are:

$$p_1 \equiv F_V(\frac{c}{1 - p_{A,2}}) \qquad\qquad p_3 \equiv 1 - F_V(\frac{2c}{1 - p_{A,3}})$$

The expressions show that running costs affect both the frequency of unopposed races and the frequency of elections with more than two candidates, and that this relationship is moderated by the distribution of the value of office (V) and contenders' expected probability of winning the election.

Normative Analysis. To obtain normative implications regarding the effects of signature requirements, I consider how the probability that the majority is misrepresented is affected by a change in running costs, and how this depends on the other characteristics of the political context. In races with one candidate, candidate A wins with probability one. However, in many cases, candidate A would lose if she faced the competition of one of the other two potential candidates. The difference in the probability that candidate A wins gives a measure of the "loss" of having a one-candidate race. This loss (L_1) is equal to:

$$L_1 \equiv |1 - p_{A,2}| = \frac{1/2 - \alpha + \xi}{2\xi}$$

[26] Note that the contender that receives a share $1 - p$ of their votes never wins.

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The expression indicates that the greater is α (that is, the expected support of candidate A), the smaller is the loss of having a one-candidate race. Intuitively, if candidate A is the clear favorite and wins most contested elections, the loss of not having a contender is low.

In the case of three-candidate races, vote splitting and the associated wasted votes also generate a misrepresentation of the majority and candidate A wins more often than in two-candidate races. The loss of having three-candidates races (L_3) is:

$$L_3 = |p_{A,3} - p_{A,2}| = \xi \frac{1-p}{1+p}$$

The expression indicates that the greater is p (the more able are groups B and C to coordinate), the smaller is the loss of having a third candidate. In the extreme case, with $p = 1$, members of the groups B and C do not waste any vote and there is no loss of adding candidates. The overall loss (L), given a set of parameter values, is defined as the expected fraction of races where the majority is misrepresented:

$$\begin{aligned} L &\equiv p_1 \cdot L_1 + p_3 \cdot L_3 \\ L &\equiv F_V(\frac{c}{p_{X,2}})\frac{1/2 - \alpha + \xi}{2\xi} + [1 - F_V(\frac{2c}{p_{X,3}})]\xi\frac{1-p}{1+p} \end{aligned}$$

A change in running costs impacts on the extent of misrepresentation of the majority in two ways. The potential reduction of three-candidate races and wasted votes ("positive" margin) comes at the expense of an increase in the frequency uncontested elections ("negative" margin). The relative importance of these two forces depends on the political context, and, in particular, on the extent of coordination and concentration of the political groups. Assuming a uniform distribution with support $[0, \bar{V}]$ for the value of office V (with $\bar{V}$ large enough so that $p_3 > 0$) the expression of the losses can be expressed as:

$$L = L_3 + \frac{c}{\bar{V}}[3 - 2\frac{p_{X,2}}{p_{X,3}}]$$

This expression shows that, under this distributional assumption, the increase in running costs reduces the overall loss whenever the chances that a contender wins (B or C) are seriously affected by the inclusion of a third candidate, which occurs, for example, if p is sufficiently low:

$$\frac{\partial L}{\partial c} < 0 \iff \frac{p_{X,3}}{p_{X,2}} < \frac{2}{3} \iff \frac{p}{1+p} < \frac{1}{3}(1 + \alpha + \xi)$$

6 Empirical Results: Political Concentration and Signature Requirements

I now assess if the impact of signature requirements actually varies with the local political context. I build a measure of political concentration for each municipality in the sample using information on the council seat distribution in the year 1992 (before the jump in signature requirements was introduced). I compute the Hirschman-Herfindahl seat concentration index (HHI) for each municipality, and consider separately those municipalities

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above and below an index equal to 0.40: this threshold ensures that in cities below it (that is, municipalities with "dispersed" political power) there were at least three groups in the council and there was no group with more than two thirds of the seats. I obtain RD estimates of the effect of signature requirements in each of the two subsamples. I consider the concentration of the political power as a feature related to some of the parameters introduced in the framework described in section 5. The presence of more groups and no absolute majorities in the council can be associated both to unclear favoritisms (low α) and to more dispersed preferences and lower coordination across groups (low p). The impact of signature requirements on the pool of candidates and electoral competition is therefore likely to depend on the extent of political concentration.

Municipalities with Dispersed Political Power.

I report RD results for municipalities with dispersed political power (that is, with a seat concentration lower than 0.40) in Table 5. In these municipalities, signature requirements significantly reduce the frequency of elections with more than 2 candidates. Baseline RD estimates show that the frequency of these races drops 0.16 percentage points at the cutoff, a 75% of the mean just below the threshold. Wasted votes and the frequency of races with potential spoiler candidates are also significantly reduced by the introduction of signature requirements. The RD coefficient on the number uncontested races is positive but smaller in absolute value and imprecise, and it is not possible to reject the null hypothesis of no effect at standard confidence levels. These results point to signature requirements acting on the "positive" margin and helping to simplify voters' problem, potentially reducing vote splitting and the misrepresentation of the majority. The change in candidates' characteristics and voter participation also point in this direction. The average candidates' propensity to win increases at the cutoff, pointing to less attractive candidates being discouraged. Furthermore, there are no significant changes in turnout and in the number of blank and null votes.

Municipalities with Concentrated Political Power.

Results for municipalities with concentrated political power are reported in Table 6. In these municipalities, signature requirements do not significantly affect the number of races with more than two candidates, the share of wasted votes or the fraction of elections with potential spoiler candidates. Instead, the frequency of unopposed races increases sharply at the cutoff. Baseline RD estimate shows that the frequency of one-candidate elections jumps 13 percentage points with the introduction of signature requirements, doubling with respect to the mean in municipalities below the threshold. These changes in the extent of political competition are accompanied by a drop in voters' electoral participation. Turnout falls 5.9 percentage points and the incidence of blank and null votes increases 2 percentage points at the cutoff. In municipalities with concentrated political power, signature requirements impact on the "negative" margin. These results highlight the potential costs of barriers to entry for potential candidates: reducing both electoral contestation and voter participation, two essential dimensions of healthy democracies.

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7 Concluding Remarks

This paper examines the causal effect of signature requirements on electoral competition, candidates' selection and voter participation. I use data on small Italian municipalities and apply a regression discontinuity (RD) design, exploiting that these requirements are only present in municipalities with more than 1000 inhabitants.

I find that signature requirements significantly reduce the number of candidates, decrease electoral competition, and lead to a more experienced pool of candidates. Signature requirements lead also to a large drop in voters' electoral participation, measured both as turnout and the number of candidates votes. The different results point to signature requirements acting as a barrier to entry for serious potential candidates and not only as a screening tool for frivolous ones. The positive effects of this policy are observed in municipalities with fragmented political systems, where signature requirements lead to fewer wasted votes and fewer potential spoilers. The downside is observed in municipalities with concentrated councils: signature requirements increase the frequency of uncontested races and reduce voter participation.

These findings highlight that the potential impact of signature requirements exceeds the goals of ensuring the representativeness of candidates and avoiding frivolous ones. The normative evaluation of this policy should therefore carefully weigh the benefits of avoiding frivolous candidates against the costs of discouraging non-marginal ones, a trade-off that, as argued, is likely to be affected by local political factors.

From a broader perspective, the paper serves to (a) highlight the importance of institutional details: the introduction of signature requirements have a large and significant impact on local electoral races, changing the observed extent of both contestation and participation (two dimensions considered central to the functioning of democracies); and (b) provide an interesting example and explanation about the need to understand the local political environment to design efficient institutions.

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Table 1: Effects of Signature Requirements on Number of Candidates and Competition

	Mean	Obs.	(1)	(2)	(3)
Candidates	2.08	1304	-0.23***	-0.22**	-0.21***
			(0.0779)	(0.0967)	(0.0826)
Effective Cands.	1.89	1144	-0.18***	-0.18***	-0.18***
			(0.0622)	(0.0629)	(0.0626)
Non-Marginal	1.69	1088	-0.25***	-0.26***	-0.25***
			(0.0752)	(0.0807)	(0.0757)
> 2 Candidates	0.16	1189	-0.067	-0.054	-0.059
			(0.0490)	(0.0582)	(0.0492)
Wasted Votes	0.021	1171	-0.0091	-0.0088	-0.0084
			(0.00718)	(0.00806)	(0.00711)
Potential Spoiler	0.083	972	-0.025	-0.028	-0.038
			(0.0344)	(0.0382)	(0.0327)
Unopposed	0.095	1974	0.10***	0.11**	0.12**
			(0.0380)	(0.0485)	(0.0499)
Winner's Share	0.63	1136	0.079***	0.079***	0.080***
			(0.0232)	(0.0247)	(0.0236)
Winner's Margin	0.29	1304	0.14***	0.15***	0.15***
			(0.0414)	(0.0479)	(0.0446)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table 2: Effects of Signature Requirements on Candidates' Characteristics

	Mean	Obs.	(1)	(2)	(3)
Female	0.10	1308	0.0027	-0.00045	-0.0017
			(0.0342)	(0.0393)	(0.0351)
Age (Yrs.)	45.0	1821	1.69*	2.17*	2.09*
			(0.885)	(1.254)	(1.080)
Education (Yrs.)	13.4	1596	0.0026	-0.041	-0.10
			(0.300)	(0.340)	(0.354)
Surname Freq.	0.018	1286	0.0035	0.0052	0.0040
			(0.00379)	(0.00459)	(0.00393)
Gov. Experience	5.13	1304	0.85**	1.10*	0.90*
			(0.430)	(0.570)	(0.466)
Propensity to Win	0.47	2008	0.014	0.016	0.011
			(0.0158)	(0.0191)	(0.0201)
Sindaco (Re-run)	0.62	1800	0.011	-0.025	-0.0099
			(0.0463)	(0.0596)	(0.0544)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table 3: Effects of Signature Requirements on Mayors' Characteristics

	Mean	Obs.	(1)	(2)	(3)
Female	0.089	1514	0.0046	-0.0044	0.0073
			(0.0422)	(0.0488)	(0.0479)
Age (Yrs.)	45.5	1843	2.40*	2.97*	2.70*
			(1.257)	(1.786)	(1.595)
Education (Yrs.)	13.8	1262	-0.82*	-0.82*	-0.82*
			(0.463)	(0.474)	(0.484)
Surname Freq.	0.018	1359	0.0012	0.0038	0.0019
			(0.00404)	(0.00480)	(0.00419)
Gov. Experience	6.09	1282	1.07*	0.95	1.04
			(0.622)	(0.768)	(0.653)
Propensity to Win	0.56	1961	0.019	0.024	0.025
			(0.0227)	(0.0327)	(0.0290)
Sindaco (Re-elected)	0.52	2033	0.054	0.073	0.076
			(0.0457)	(0.0661)	(0.0596)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table 4: Effects of Signature Requirements on Voters' Participation

	Mean	Obs.	(1)	(2)	(3)
Turnout	0.82	1119	-0.046***	-0.042***	-0.047***
			(0.0132)	(0.0136)	(0.0133)
Blank/Null	0.057	1907	0.014**	0.015	0.015
			(0.00707)	(0.00979)	(0.00936)
Candidates' Votes	0.76	1171	-0.060***	-0.055***	-0.062***
			(0.0166)	(0.0161)	(0.0169)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table 5: Municipalities with Dispersed Political Power: Effects of Signature Requirements on Electoral Competition and Voters' Participation

	Mean	Obs.	(1)	(2)	(3)
Candidates	2.13	493	-0.26***	-0.30**	-0.25**
			(0.0977)	(0.128)	(0.105)
Effective Cands.	1.95	485	-0.26***	-0.28**	-0.26***
			(0.0852)	(0.111)	(0.0920)
Non-Marginal	1.70	502	-0.20**	-0.30**	-0.24**
			(0.0949)	(0.126)	(0.104)
> 2 Candidates	0.20	533	-0.16**	-0.15**	-0.14**
			(0.0614)	(0.0667)	(0.0658)
Wasted Votes	0.030	520	-0.030***	-0.029**	-0.028**
			(0.0104)	(0.0115)	(0.0111)
Potential Spoiler	0.14	516	-0.15***	-0.18***	-0.14**
			(0.0535)	(0.0688)	(0.0579)
Unopposed	0.079	685	0.059	0.12	0.088
			(0.0575)	(0.0871)	(0.0744)
Turnout	0.82	709	-0.019	-0.026	-0.028
			(0.0155)	(0.0190)	(0.0188)
Blank/Null	0.060	557	0.0045	0.0049	0.00047
			(0.0131)	(0.0149)	(0.0142)
Candidates' Votes	0.76	607	-0.026	-0.028	-0.028
			(0.0224)	(0.0249)	(0.0256)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table 6: Municipalities with Concentrated Political Power: Effects of Signature Requirements on Electoral Competition and Voters' Participation

	Mean	Obs.	(1)	(2)	(3)
Candidates	2.04	888	-0.20**	-0.16	-0.19*
			(0.0935)	(0.122)	(0.101)
Effective Cands.	1.84	754	-0.16**	-0.14	-0.15**
			(0.0704)	(0.0874)	(0.0713)
Non-Marginal	1.69	782	-0.26***	-0.27**	-0.27***
			(0.0852)	(0.104)	(0.0897)
> 2 Candidates	0.13	701	-0.0032	0.033	0.0038
			(0.0585)	(0.0707)	(0.0592)
Wasted Votes	0.014	658	0.0022	0.0063	0.0016
			(0.00781)	(0.00907)	(0.00771)
Potential Spoiler	0.032	449	0.032	0.058*	0.028
			(0.0265)	(0.0307)	(0.0275)
Unopposed	0.11	1145	0.13***	0.13**	0.16***
			(0.0484)	(0.0635)	(0.0587)
Turnout	0.81	632	-0.059***	-0.050***	-0.058***
			(0.0167)	(0.0167)	(0.0165)
Blank/Null	0.055	1194	0.020**	0.022**	0.028**
			(0.00874)	(0.0109)	(0.0115)
Candidates' Votes	0.76	640	-0.087***	-0.070***	-0.086***
			(0.0202)	(0.0201)	(0.0202)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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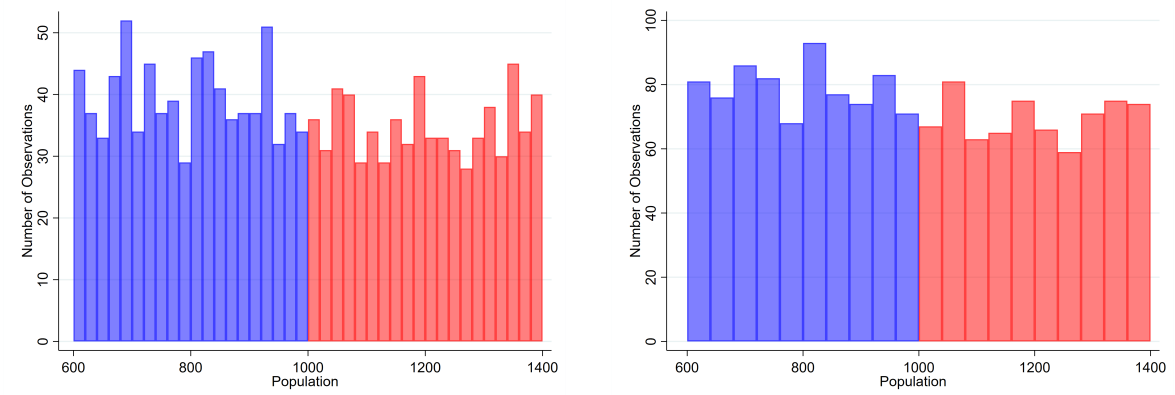
A Appendix

Figure A.1: Validation Check: Density of the Running Variable (Histogram)



[a] Left panel: distribution of municipalities by population (1991 census), using 20-inhabitants bins. Right panel: distribution of municipalities by population (1991 census), using 40-inhabitants bins

Table A.1: Validation Check: Density of the Running Variable (Manipulation Test)

	(1) b/se/p	(2) b/se/p
Density Jump	-.0000511	-.0000388
	(.0001041)	(.0001426)
	[0.623]	[0.786]
N	2693	2693
Effective N	1009	1315
Polynomial Order	1	2

[a] Results of manipulation test using the local polynomial density estimators proposed in Cattaneo, Jansson and Ma (2017). Robust standard errors reported between parentheses. P-Value reported between squared brackets.
[b] Stars denote significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table A.2: Validation Check: Continuity in Predetermined Variables (I)

	Mean	Obs.	(1)	(2)	(3)
Density	87.5	1788	77.3	77.8	82.0
			(0.853)	(0.898)	(0.876)
Men-Women	96.9	1268	-1.36	-1.47	-1.67
			(-1.098)	(-1.100)	(-1.287)
Less 6yr. (%)	5.14	1800	0.17	0.42	0.34
			(0.804)	(1.427)	(1.325)
More 75yr. (%)	9.88	1445	0.27	0.20	0.45
			(0.468)	(0.289)	(0.713)
Family Size	2.57	1676	0.059	0.055	0.054
			(1.130)	(0.986)	(0.867)
BA Degree (%)	14.5	1288	0.15	0.33	0.23
			(0.148)	(0.318)	(0.222)
Labor Force (%)	46.9	1235	-0.78	-0.52	-0.84
			(-0.656)	(-0.380)	(-0.688)
Unemp. (%)	15.1	1479	1.44	0.73	2.23
			(0.688)	(0.339)	(0.965)
High-Skill (%)	15.5	1760	0.24	0.58	0.68
			(0.276)	(0.510)	(0.614)
Middle-Skill (%)	51.3	1256	-1.12	-0.59	-1.23
			(-0.474)	(-0.252)	(-0.517)
Low-Skill (%)	10.7	1088	-0.64	-1.13	-0.78
			(-0.417)	(-0.726)	(-0.530)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table A.3: Validation Check: Continuity in Predetermined Variables (II)

	Mean	Obs.	(1)	(2)	(3)
Piedmont	0.28	1736	-0.084	-0.068	-0.092
			(-1.311)	(-0.930)	(-1.179)
Lombardy	0.24	1340	-0.024	-0.042	-0.017
			(-0.302)	(-0.536)	(-0.202)
Veneto	0.025	1387	0.024	0.020	0.0078
			(0.767)	(0.555)	(0.228)
Liguria/Emilia-Romagna	0.060	1779	0.023	0.021	0.012
			(0.580)	(0.376)	(0.235)
Tuscany	0.018	1310	0.019	0.014	0.016
			(1.380)	(0.971)	(1.183)
Umbria/Marche	0.032	1619	0.036	0.035	0.014
			(0.893)	(0.812)	(0.305)
Lazio	0.049	1158	0.013	0.017	0.021
			(0.511)	(0.636)	(0.841)
Abruzzo	0.053	1676	0.0059	-0.010	-0.0012
			(0.161)	(-0.218)	(-0.0279)
Molise	0.049	1684	-0.0034	0.0039	0.0067
			(-0.120)	(0.123)	(0.218)
Campania	0.070	1676	-0.0014	0.0048	0.042
			(-0.0305)	(0.0873)	(0.769)
Apulia/Basilicata	0.023	1736	0.016	0.0054	0.0087
			(0.717)	(0.202)	(0.347)
Calabria	0.049	2072	0.0097	0.0034	0.012
			(0.282)	(0.0830)	(0.282)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table A.4: Validation Check: Continuity in Predetermined Variables (III)

	Mean	Obs.	(1)	(2)	(3)
1993	0.063	1569	0.022	0.019	0.023
			(0.838)	(0.600)	(0.763)
1994/1995	0.42	1340	-0.021	-0.019	-0.023
			(-0.665)	(-0.539)	(-0.700)
1996	0.011	1617	0.017	0.015	0.019
			(1.563)	(1.068)	(1.450)
1997	0.086	1430	0.011	0.0040	0.017
			(0.349)	(0.125)	(0.495)
1998/1999	0.38	1583	-0.0061	-0.016	-0.026
			(-0.173)	(-0.371)	(-0.630)
May	0.011	1838	0.0088	-0.0041	-0.00037
			(0.672)	(-0.212)	(-0.0221)
June	0.44	1565	-0.011	-0.0046	-0.028
			(-0.411)	(-0.160)	(-0.879)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table A.5: Validation Check: Continuity in Predetermined Variables (IV)

	Mean	Obs.	(1)	(2)	(3)
Council Concentration (HHI)	0.54	1344	0.0014	0.0086	0.013
			(0.0331)	(0.0413)	(0.0365)
Lists in Council	3.28	1707	0.10	0.094	0.050
			(0.164)	(0.237)	(0.208)
Bandwidth			MSE	MSE	150 Inhab.
Polynomial Order			One	Two	One
Fixed Effects			Yes	Yes	Yes

[a] In columns (1)-(3), each figure reports the estimate of a separate regression. Robust standard errors adjusted for clusters at the municipality level are in parentheses. Estimates are obtained from local regressions with triangular kernel. The MSE bandwidth is the mean squared error optimal bandwidth computed using the procedure by Calonico, Cattaneo, Farrell and Titiunik (2018). The effective number of observations for this bandwidth is reported (Obs.). Mean is the average value of the dependent variable for municipalities with 850 to 1000 inhabitants. Regressions include municipality controls, and region, year and month fixed effects. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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Table A.6: Candidates' Characteristics and Propensity to Win Election

	Coeff.	Mg. Effect
Female	-0.096	-0.031
	(0.0742)	(0.0239)
Age (Yrs.)	0.037**	0.012**
	(0.0170)	(0.00555)
Age (Yrs.) Squared	-0.00047***	-0.00015***
	(0.000177)	(0.0000579)
College	0.33***	0.11***
	(0.0672)	(0.0220)
High School	0.16**	0.053**
	(0.0661)	(0.0214)
Gov. Experience	0.028***	0.0093***
	(0.00706)	(0.00229)
Politician	0.13**	0.045**
	(0.0594)	(0.0203)
Sindaco	1.09***	0.40***
	(0.0663)	(0.0226)
Surname Freq.	0.75	0.25
	(0.951)	(0.311)
Constant	-1.51***	
	(0.401)	
Observations	4077	4077

[a] Column (1) reports the coefficient of a probit model with dependent variable an indicator of winning the election. Column (2) reports the marginal effect at the covariates mean. The model is estimated using information on electoral races during the period 1993-2000 in municipalities with 2000 to 3000 inhabitants. Stars denote statistical significance level: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

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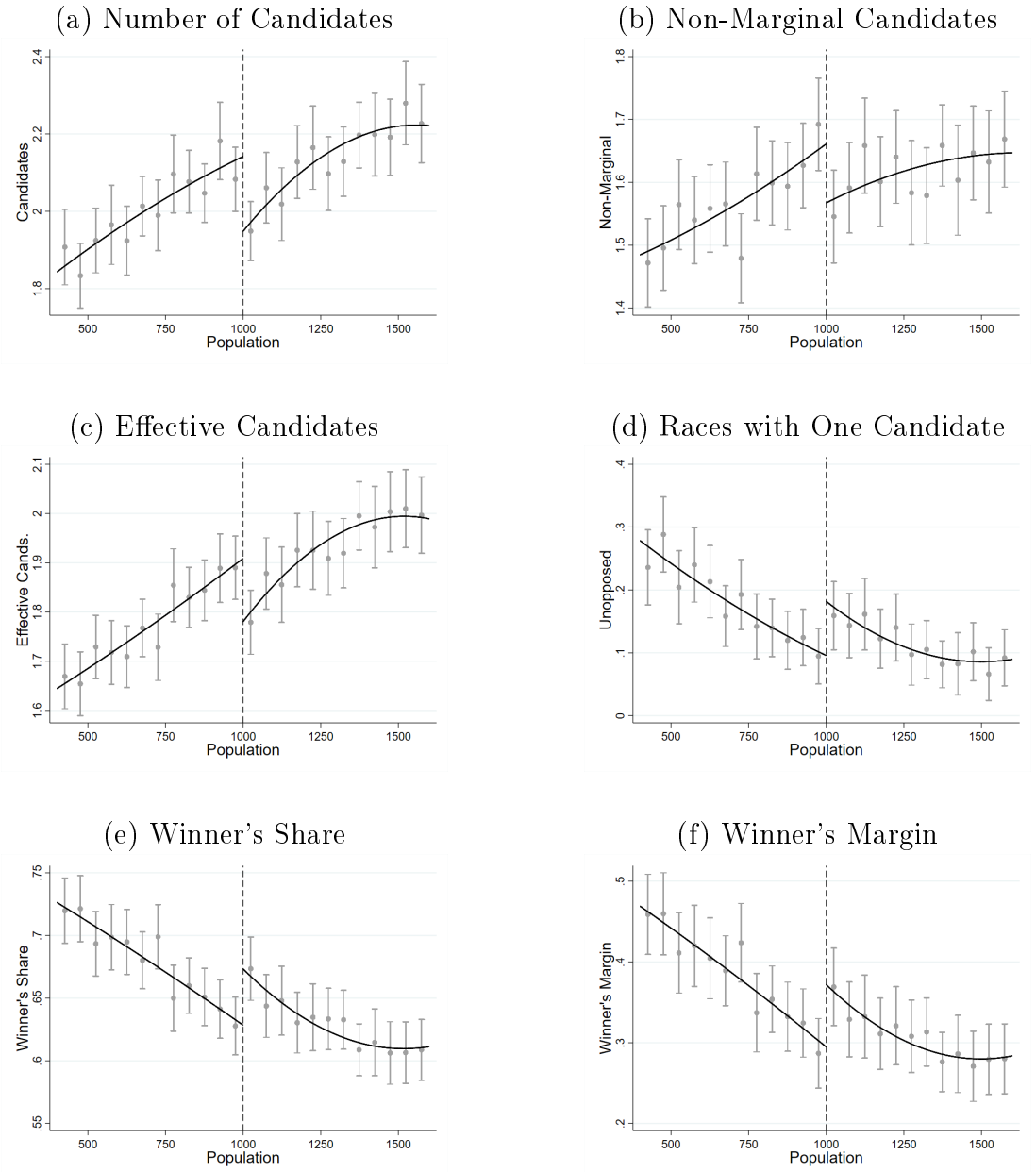
Figure A.2: The Effect of Signature Requirements on Political Competition



The above figures provide a graphical representation of the regression discontinuity design. The dependent variables are indicated in the title of each graph, as defined in section 4.2. The horizontal axis indicates municipalities' population size according to the 1991 Census. Each point denotes the average of the dependent within a 50-inhabitants bin and its 95% confidence interval. The line shows a second-order global polynomial estimated on each side of the discontinuity.

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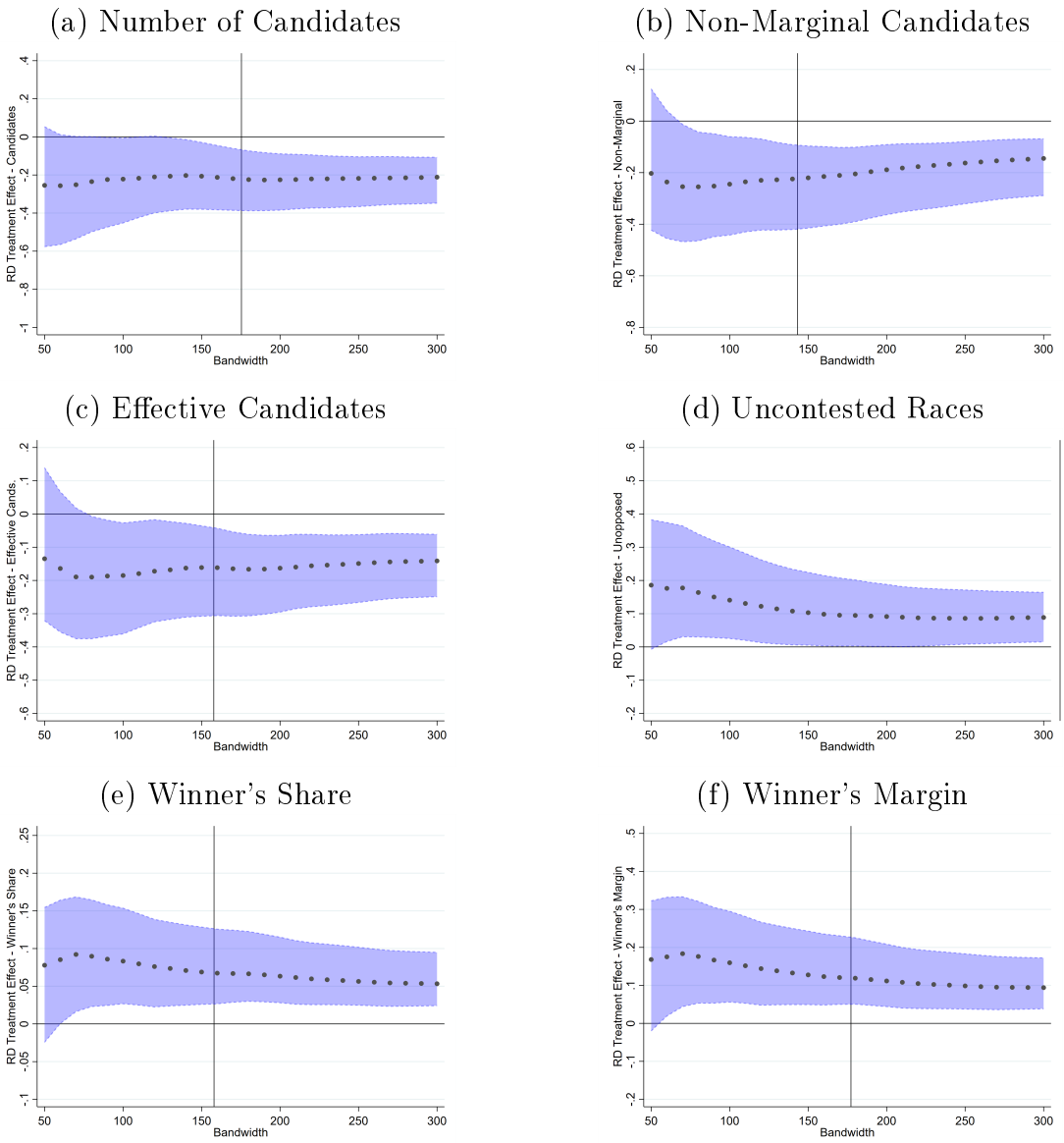
Figure A.3: Robustness: RD Effects for Different Bandwidths



The above figures show the sensitivity of the estimated coefficients to the bandwidth choice. Dots represent the estimated treatment effect of the signature requirements using different bandwidths (reported in x-axis). All estimates are obtained from local linear regressions with triangular kernel. Regressions include municipality controls, and region, year and month fixed effects. The shaded areas represent the 95% confidence interval. The vertical line indicates the value of the MSE optimal bandwidth (Calonico et al, 2018).

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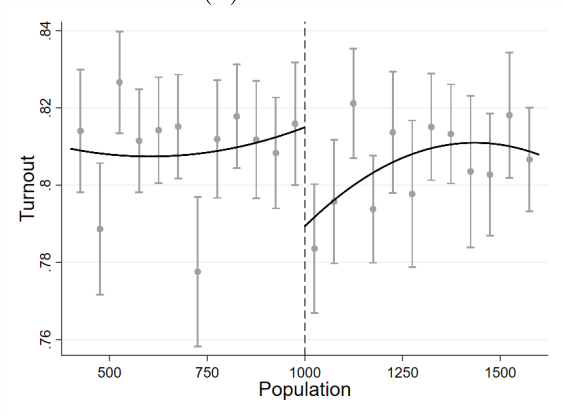
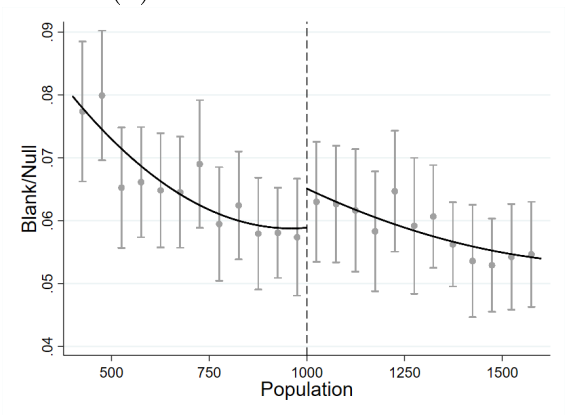
Figure A.4: The Effect of Signature Requirements on Political Participation

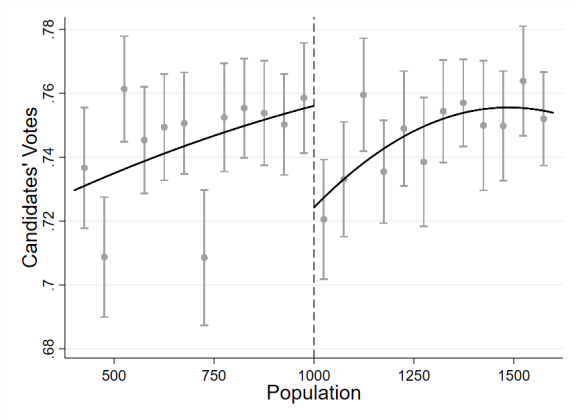
(a) Turnout



(b) Null and Blank Votes



(c) Candidate Votes



The above figures provide a graphical representation of the regression discontinuity design. The dependent variables are (a) turnout, (b) the percentage of blank and null votes, and (c) the percentage of valid votes. The horizontal axis indicates municipalities' population size according to the 1991 Census. Each point denotes the average of the dependent within a 50-inhabitants bin and its 95% confidence interval. The line shows a second-order global polynomial estimated on each side of the discontinuity.

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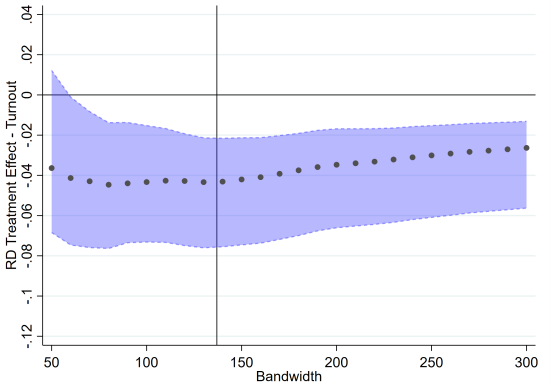
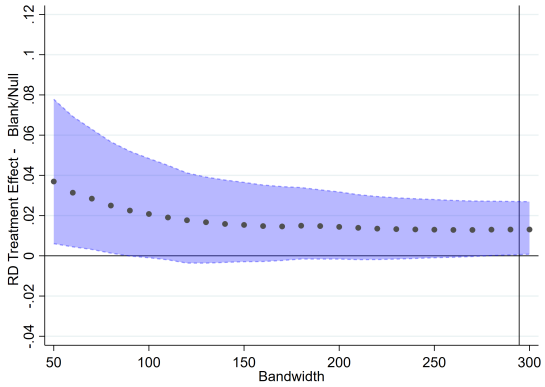
Figure A.5: Robustness: RD Effects for Different Bandwidths

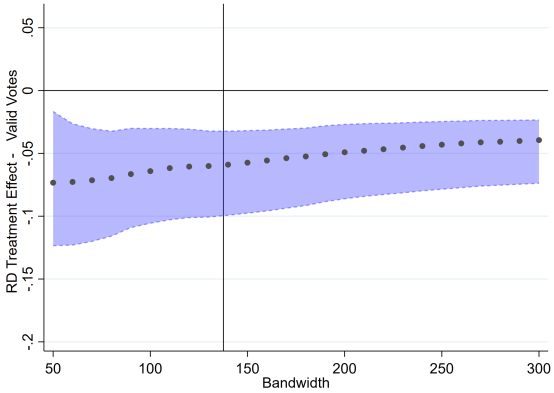
(a) Turnout



(b) Null and Blank Votes



(c) Candidate Votes



The above figures show the sensitivity of the estimated coefficients to the bandwidth choice. Dots represent the estimated treatment effect of the signature requirements using different bandwidths (reported in x-axis). All estimates are obtained from local linear regressions with triangular kernel. Regressions include municipality controls, and region, year and month fixed effects. The shaded areas represent the 95% confidence interval. The vertical line indicates the value of the MSE optimal bandwidth (Calonico et al, 2018).

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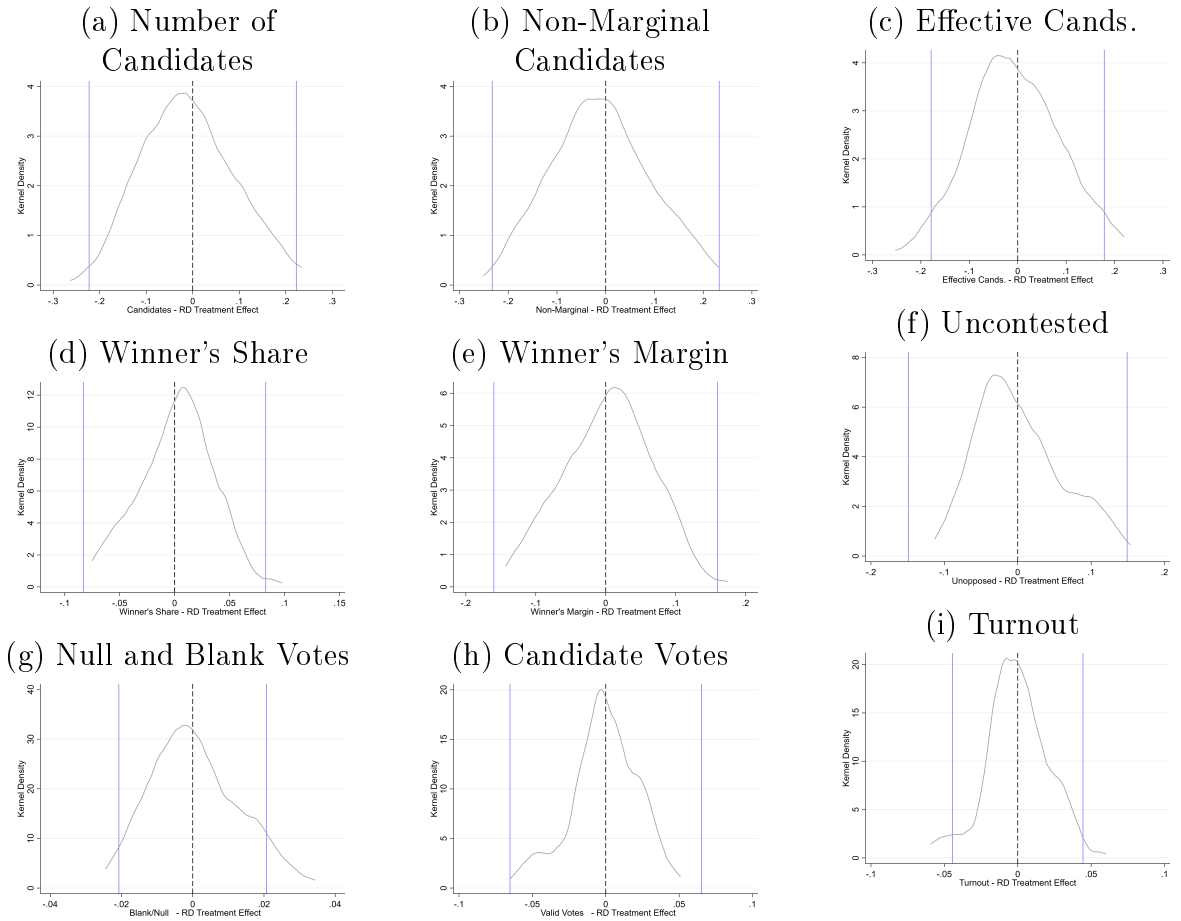
Figure A.6: Robustness: RD Effects at Placebo Thresholds (Main Variables)



The above figures provide the kernel density of the point estimates computed at 300 placebo cutoffs (for each population value in ranges 700-850 and 1150-1300). The vertical blue lines show the value of the true coefficient and its opposite. The vertical dotted line indicates value zero. All estimates (both the true coefficient and the placebo ones) are obtained from local linear regressions with a 150-inhabitants bandwidth.

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Table A.7: Robustness: RD Effects at Placebo Thresholds (Main Variables)

	True Cutoff		Placebo Cutoffs (Coefficients)		
	(1) Coeff.	(2) p-value	(3) Mean	(4) SD	(5) % > |True|
Candidates	-0.22	0.052	-0.0090	0.098	0.0099
Effective Cands.	-0.18	0.041	-0.0042	0.092	0.056
Non-Marginal	-0.23	0.019	-0.0096	0.10	0.0033
Unopposed	0.15	0.036	0.0017	0.058	0.0033
Winner's Share	0.083	0.012	0.0013	0.033	0.0066
Winner's Margin	0.16	0.0099	0.0026	0.063	0.0066
Turnout	-0.044	0.0027	-0.000092	0.021	0.046
Blank/Null	0.021	0.11	0.00055	0.012	0.089
Candidates' Votes	-0.065	0.00075	-0.00065	0.022	0

Each row in the table corresponds to one dependent variable. Columns (1) and (2) report the RD coefficient computed at the true cutoff (1000 inhabitants) and its p-value. Columns (3) and (4) report the mean and standard deviation of RD coefficients computed at 300 placebo cutoffs (for each population value in ranges 700-850 and 1150-1300). Column (5) indicates the fraction of placebo coefficients that have an absolute value greater than the absolute value of the true coefficient. All estimates (both the true coefficient and the placebo ones) are obtained from local linear regressions with a 150-inhabitants bandwidth. Regressions include municipality controls, and region, year and month fixed effects.

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Using Centralized Assignment to Evaluate Entrepreneurship and Life-Skills Training Programs in Argentina

Santiago Pérez Vincent and Diego Ubfal*

Abstract

We implemented a centralized assignment mechanism (random serial dictatorship) to fill seats for a set of training courses offered by the government of Buenos Aires in schools across the city. The mechanism generates a seat assignment that is random conditional on people's preferences over schools. We exploit the fact that we know these preferences and the assignment rule to analytically compute individuals' propensity scores (i.e., their probability of obtaining a seat). We then use propensity score stratification to evaluate the short-term impact of two of the courses (an entrepreneurship course and a life-skills course). This strategy leads to important sample size gains relative to full preference stratification, allowing us to fully exploit the random variation in treatment assignment. Using survey-based information collected three months after the courses, we find positive effects on course-related knowledge for both programs. Entrepreneurship training helps participants start a business from an initial idea, leading to increased business ownership and self-employment. Life-skills training leads to higher job-search rates. There are no significant effects on soft skills.

JEL codes: I28, J24, J68, L26

Keywords: entrepreneurship, job training, centralized assignment, random serial dictatorship, occupational choice, program evaluation.

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1 Introduction

Job and business training programs are a part of the policy portfolio of governments and non-profit organizations. These programs aim to help people join and stay in the labor force, and they are motivated by the notion that a fast-changing economy requires workers and businesses to adapt and acquire new skills frequently. In many settings, especially in large urban areas, candidates to these programs can apply for seats at different training centers, creating a matching problem similar to that faced by public school districts (Abdulkadiroğlu and Sönmez, 2003). To address this matching problem, school districts have increasingly used centralized assignment mechanisms based, for example, on the student-proposing deferred acceptance algorithm (Gale and Shapley, 1962) because of their efficiency and fairness properties.

The gains from introducing these centralized assignment mechanisms go beyond efficiency and fairness. When such mechanisms use lotteries to ration vacancies in oversubscribed programs, they generate quasi-experimental variation in assignment that can be used to credibly evaluate the impact of these programs (Abdulkadiroğlu, Angrist, Narita, and Pathak, 2017). However, even in the education literature, there are very few cases in which the theoretically recommended algorithm is implemented without modifications, mainly because of political constraints.

We successfully implemented a pure random serial dictatorship procedure (Abdulkadiroğlu and Sönmez, 1998) to assign vacancies in a novel setting. We partnered with the government of the city of Buenos Aires, Argentina, and implemented this centralized assignment mechanism to fill seats in schools providing subsidized training courses. We exploit that, conditional on people's preferences over schools, the random serial dictatorship procedure generates random variation in assignment to evaluate the short-term impact of the courses.

Since 2014, the city of Buenos Aires offers four levels of free courses that teach life and entrepreneurial and business tools, which are designed to meet the needs of people at different stages of their professional life. The courses are offered in different training centers or schools distributed across the city. Until 2016, the program assigned seats in each of the schools on a *first come, first served* basis through a common online application opened a few weeks before the beginning of the course. This enrollment process benefited those who, for any reason, were informed first about the opening of the application process, raising potential fairness concerns regarding the allocation of seats. Furthermore, given that people who registered promptly for the course were likely to be a selected subsample of the population and that the application process closed after all vacancies were filled, the *first come, first served* basis assignment hindered the evaluation of the program. To the extent that the drivers of quick registration (e.g., motivation, connectedness, and time availability) were correlated with the outcomes of interest, any comparison with other subpopulations (even those showing interest in the course) would have been subject to selection bias.

To overcome these difficulties, we followed the recent literature on matching and school

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choices, and we implemented a random serial dictatorship mechanism to fill seats for the training programs in the different schools (Abdulkadiroğlu and Sönmez, 1998, 2003). In the application process, enrollees were asked to indicate their preferences over schools, listing only those schools that they were willing to attend (i.e., options that were preferred to not doing the course). We then ordered enrollees randomly and assigned the first enrollee to her most preferred school, the second enrollee also to her most preferred school among those with available seats, and so on, until all seats were allocated or until we reached the last enrollee. This mechanism is Pareto efficient, strategy proof, can accommodate any preference ordering, and, crucial to fairness and the evaluation strategy, it guarantees that enrollees who share the same preferences have the same probability to enter the course (a property usually called equal treatment of equals).

We then exploit the random variation in assignment generated by the described mechanism to evaluate the short-term impact of the courses. The impact evaluation focuses on two of the courses, for which over-subscription allowed the existence of a control group. The first is a life-skills training course that aimed to help people build their own personal and professional projects and acquire a proactive mindset. It consists of seven three-hour lessons, in which a tutor discusses and proposes activities to exercise self-knowledge, communication, conflict resolution, leadership, and creativity. The second course is a more traditional entrepreneurship training course designed for people who want to validate business ideas and start their own business. It consists of seven three-hour lessons aimed at providing tools to develop and assess the sustainability and viability of business ideas (e.g., design thinking, business model canvas, and lean startup).

We use the properties of the assignment mechanism to separate the random variation in assignment from the variation in assignment generated by non-random preferences. In this mechanism, the probability of being treated (i.e., getting into the training program) depends on the preferences: participants who want to attend less-preferred schools are more likely to get a seat than those who want to attend only the most popular schools.[1] If these preferences are correlated with the outcomes of interest, a comparison of the treated and non-treated enrollees would suffer from selection bias. The equal treatment of equals property ensures that, conditional on preferences, the treatment assignment is random and independent of potential outcomes. Therefore, stratification on preferences allows us to retrieve the average treatment effect of the course. In our application, however, conditioning on preferences would considerably reduce degrees of freedom and eliminate many individuals from the sample. To avoid this problem, we use a propensity score stratification based on the recent results of Abdulkadiroğlu, Angrist, Narita, and Pathak (2017). As in their paper, propensity score stratification leads to important sample size gains relative to stratification on preferences: the sample size increases from 196 to 237 individuals (20.9%) in the life-skills training course and from 377 to 499 individuals (32.3%) in the entrepreneurship training course.

To compute the propensity score analytically without relying on simulations, we fol-

[1] In this context, the popularity of schools depends mainly on their location and time slot offered. Courses are standardized, and candidates do not have much information on how tutoring quality might vary across schools.

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low Che and Kojima (2010). The formula for the analytical propensity score clarifies how individuals with different orderings of preferences can end up having the same value of the propensity score and, therefore, why controlling for the propensity score instead of the raw preferences can help improve statistical power. This formula could also be useful to understand how statistical power can be maximized by manipulating the availability of seats across schools.

We evaluate the September 2016 edition of the courses.[2] Using survey-based information collected three months after the courses, we measure the short-term impact of the courses on four different sets of variables: knowledge, employment status, entrepreneurship, and socio-emotional skills. We find a positive impact on course-related knowledge for both programs, indicating that on average, participants assimilate a part of the theoretical content taught in class. We also observe effects on employment status and entrepreneurial activity, which is largely consistent with the objectives of the different courses. Entrepreneurship training helps transition from the idea to the start-up phase, leading to increased business ownership and self-employment. Life-skills training leads to higher job-search rates. We do not find significant effects on socio-emotional skills, such as locus of control, empathy, or self-efficacy.[3]

This study has two main contributions. First, it provides one of the first applications of propensity score analysis, proposed by Abdulkadiroğlu, Angrist, Narita, and Pathak (2017), to exploit the random variation generated by centralized assignment mechanisms with equal treatment of equals. The results show the significant sample size gains of such an approach in a totally unrelated context to that originally studied, supporting its relevance and generality. The study also contributes to this literature by providing a formulation of the propensity score for random serial dictatorship as a function of enrollees' preferences and school capacities (i.e., the primitives of the matching problem).

Second, this study contributes to the growing literature on program evaluation in Latin America (Escudero et al, 2017). The proliferation of labor and business training programs has led to an increase in the number of studies that measure the impact of such programs by using both experimental and non-experimental evidence. Card, Kluve, and Weber (2015) conduct a meta-analysis of the evaluations of job-training programs and other active labor market policies. They find a positive effect of these programs on the probability of employment in the medium run, an effect that is moderated by the recipients' gender and pre-treatment unemployment duration. McKenzie and Woodruff (2013) review the experimental evidence on business training programs in developing countries and conclude that most programs lead to some, although small, improvement in business practices, which makes identifying the effect on business outcomes difficult because of a lack of statistical power. They also find a modest increase in the survivorship of existing

[2]Courses are taught twice every year—in April and September. September 2016 was the first edition of the course that used the random serial dictatorship mechanism to assign seats. After this edition, the program utilized the mechanism and applied it to assign seats for all its editions.

[3]These results should be taken with caution because even when attrition was not differential across treatment arms, it was very large (more than 60 percent). This was mainly due to the lack of resources to implement a face-to-face survey and the low response rate for surveys sent by email. We are expecting to obtain administrative data on formal jobs and businesses created to complement the survey data.

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firms and a larger and more consistent effect on short-term business ownership among prospective owners. Despite these and other efforts to organize existing evidence, there is not much agreement in the literature and in the policy arena regarding the effectiveness of these initiatives. The difficulty of reaching a consensus is partly explained by the vast heterogeneity of programs offered (in terms of content, length, treated population, and context) and by usual limitations in the evaluation phase, such as the high attrition rates and small samples (McKenzie and Woodruff, 2013). In Latin America, the situation is more acute because despite some recent contributions (Attanasio, Kugler, and Meghir, 2011; Card et al., 2011; Karlan and Valdivia, 2011; Alzúa, Cruces, and Lopez, 2016), many programs are still not being evaluated (Gonzalez-Velosa, Ripani, and Rosas-Shady, 2012).[4]

Our study provides empirical evidence on the short-term effect of a large-scale subsidized training program, which is currently in place and has attracted more than 20,000 enrollees in the last three years. This study will be complemented with the evaluation of subsequent editions of the program and the use of administrative data.

The remainder of this paper is organized as follows. In section 2, we describe the training programs. In Section 3, we describe the centralized assignment mechanism and the empirical strategy. In section 4, we present the empirical results on the different sets of outcomes. In section 5, we provide some concluding remarks and discuss the next steps of the project.

2 Academia BA Emprende: Description of the Courses

In 2014, the municipal government of the city of Buenos Aires created Academia BA Emprende, a program that provides free training courses on life, entrepreneurial and business tools.

The main goal of the program is to teach tools to help people adapt to the needs of the current labor market, where jobs and opportunities "are not just found, but have to be created".[5] Academia BA Emprende offers 4 different courses, all consisting of 7 weekly meetings of 3 hours each (21 hours in total). Each of these courses is designed to meet the needs of people at a certain stage of their professional life, from those who are seeking to better understand their strengths and improve basic skills to those who want to expand already existing businesses.[6] In all cases, classes are led by tutors who have experience in entrepreneurship, and are responsible for transmitting the main theoretical

[4] In one of the closest studies to the current work, Alzúa, Cruces, and Lopez (2016) examine the effect of a youth training program in Córdoba, Argentina, finding some positive short-term effects on employment and income, which dissipate in the medium and long terms. The intervention included more than 100 hours of technical classroom training and 64 hours of life-skills training distributed over three months, plus an internship phase of up to four months.

[5] A description of Academia BA Emprende program can be found in the website of the municipal government: http://www.buenosaires.gob.ar/innovacion/emprendedores/capacitacion-e-incubadoras/academia-ba-emprende (checked: September 8th, 2017).

[6] Information on the courses is available on the official website of the Academia BA Emprende: http://academia.buenosaires.gob.ar/informacion (checked: September 8th, 2017).

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content of the course and encouraging exchange of ideas among participants. The program organizers give tutors detailed instructions on how to teach the courses' content and on how to guide interactions among attendees, and provide them with a set of case-studies and exercises to be discussed in class and as homework. In its four years of existence, Academia BA Emprende has attracted more than 20 thousand enrollees and provided training to 10 thousand people. Different to other programs in Latin America studied in recent years (see, for example, Alzúa, Cruces and Lopez (2016) and Karlan and Valdivia (2011)), Academia BA Emprende does not target any specific subpopulation, and therefore courses' participants are generally diverse in terms of age, gender, education, and other socio-demographic characteristics.

The first course is a life-skills training program, called "Professional Growth", designed for people seeking to improve their professional and/or personal life. The objective of the course is to help people build their own personal project and develop a proactive mindset. Meetings are focused on different skills: self-knowledge, communication, conflict resolution, leadership, and creative process. In each meeting, tutors provide basic concepts and information on the importance of these skills for professional and personal development, and conduct exercises along with participants to enhance these skills and help them discover their own strengths and limitations. The course is taught in eight different venues and nine shifts (one venue offers two different shifts), with an average of almost 60 seats each (530 seats in total). Venues are located in different parts of the city of Buenos Aires.

The second course, called "Ideation", is a more traditional entrepreneurship training program designed for people who want to start their own business. The goal of the course is to strengthen creativity and provide tools to assess the sustainability and viability of business ideas. Meetings focus on different techniques: Design Thinking, Canvas business models, and Lean StartUp, among others. Every meeting, the tutor exposes the basics of a given technique, and proposes exercises for participants to implement them in groups and individually. As homework, participants are asked to apply the different techniques to their own business ideas, and bring conclusions and questions to the following meeting. The course is taught in fourteen different venues and eighteen shifts (some venues offer more than one shift), with an average of almost 60 seats each (1059 seats in total). Like in the life-skills course, there are venues in different parts of the city of Buenos Aires.

In the September 2016 edition, the number of enrollees largely exceeded the number of seats available for these two courses, allowing for the existence of a control group and an evaluation of their impact. Academia BA Emprende offers two other courses, "StartUp Companies" and "Expansion". These two courses are smaller in size (865 seats available in total), as they are targeted to people who own a business and want to either consolidate it or expand it. They provide basic business training, and aim at improving business practices and, ultimately, enhancing sales, profits and stability of the firms. In the September 2016 edition, there was no over-subscription in these two other courses (and no randomized assignment, as all enrollees got a seat).

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3 Using Centralized Assignment for Program Evaluation

In the first five editions of Academia BA Emprende program, launched between April 2014 and April 2016, seats in each of the schools were assigned in a "first come, first served" basis through a common online application process opened a few weeks before the beginning of the courses. In those editions, 18 thousand people registered to the program, and 14 thousand got a seat.[7] Given that people who registered promptly for the course were likely to be a selected subsample of the population, the assignment mechanism hindered an evaluation of the program. To the extent that drivers of quick registration (motivation, time availability, etc.) were correlated to the outcomes of interest, any comparison with other subpopulation (even those showing interest in the course) would have been subject to selection bias.[8] Furthermore, the "first come, first served" basis benefited those who, for any reason, got informed first about the opening of the application process, raising potential fairness considerations regarding the allocation of seats.

To overcome these difficulties, for the September 2016 edition of the program, we implemented a random serial dictatorship mechanism to fill seats in different schools, following the literature on matching and school choices (Abdulkadiroğlu and Sönmez, 1998, 2003).[9] In the application form, enrollees were now asked to indicate their preferences over schools, listing only schools they were willing to attend (that is, options that were preferred to not doing the course). Then, once the application period ended, we ordered enrollees randomly and assigned the first enrollee to her most preferred school, the second enrollee to her most preferred school among those with available seats, and so on, until all seats were allocated (or we got to the last enrollee).

This random serial dictatorship mechanism has nice efficiency and fairness properties (Abdulkadiroğlu and Sönmez, 1998). It is Pareto efficient, as resulting seat allocations cannot be modified without harming someone or leaving everyone indifferent; it is strategy-proof, since an enrollee cannot improve her chances of entering the treatment (or getting a seat in a more preferred school) by misreporting her preferences; it can accommodate any set of preference orderings; and, crucial to the evaluation strategy, it guarantees that enrollees with equal preferences have the same probability of entering the course (a property usually called "equal treatment of equals").

Under this assignment mechanism, enrollees' probability of being treated (that is, getting into the training program) depends on their preferences: participants who want to attend less-demanded schools are more likely to get a seat than those who only want to attend the most popular schools. If these preferences are correlated to outcomes of in-

[7] Most of the people who enrolled in those editions got a seat because organizers were closing the application soon after all seats were allocated.

[8] An evaluation could have been done exploiting variation in access to the course coming from people submitting the application just before and just after all seats were assigned (see, for example, Pinotti, 2017). This strategy would have resulted in a small sample and a limited external validity.

[9] This centralized assignment mechanism is still being used by Academia BA Emprende program to allocate seats for the different courses.

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terest, an unconditional comparison of the treated and non-treated enrollees would again suffer from selection bias. The "equal treatment of equals" property helps to overcome this, as it ensures that, conditional on preferences, treatment assignment is random and independent of potential outcomes (Abdulkadiroğlu, Angrist, Narita and Pathak, 2017). Therefore, stratification on preferences allows to retrieve the average treatment effect of the course.

In our application, however, conditioning on preferences would considerably reduce degrees of freedom and eliminate many individuals from the effective sample. In the September 2016 edition of Academia BA Emprende program, 806 people enrolled for the life-skills course and reported their preferences over the 9 different school/shifts available. There were 211 different preference orderings, and 140 of them were indicated by just one person. In the entrepreneurship course, 1649 people enrolled and indicated their preferences over 18 different school/shifts. Among these, there were 550 different preference orderings, 437 of which were reported by only one enrollee.[10] Full preference stratification uses variation in assignment *within* preference types, and, therefore, individuals who do not share their type with anyone else would "fall" from the sample.[11]

To deal with this issue, we follow recent results by Abdulkadiroğlu, Angrist, Narita and Pathak (2017) and use a propensity score stratification to estimate the average treatment effect of the courses. This stratification allows to fully exploit the random variation in assignment generated by the centralized mechanism, separating it from the non-random variation in assignment generated by different preferences. For a given vector of school capacities, the random serial dictatorship assignment mechanism maps each preference ordering to a probability of being treated (or propensity score). Since assignment is exogenous conditional on preferences (due to the equal treatment of equals property), assignment is also exogenous conditional on propensity scores (Rosenbaum and Rubin, 1983). Importantly, the propensity score is the coarsest function of preferences that warrants conditional independence of potential outcomes. This is crucial to increase the effective sample size: exploiting variation in assignment *within* large propensity score strata allows to use variation coming from, for example, individuals who do not share their preference type with anyone else but have the same propensity score as others.

Computing Propensity Scores for Random Serial Dictatorship

To use a propensity score stratification we need to obtain the mapping from preference types to propensity scores. Abdulkadiroğlu, Angrist, Narita and Pathak (2017) use a continuum economy approximation to derive an analytical formula of propensity scores in general deferred acceptance (DA) assignment mechanisms. For random serial dictatorship, their formulation shows that the propensity score of an individual is determined by the probability of accessing the school that is easiest to access among her listed ones. For example, in a setting with two schools, A and B, where it is easier to get a seat in school

[10] There were 26 other enrollees in the life-skills course and 59 in the entrepreneurship course for whom baseline information is not available. They are excluded from the analysis.

[11] More generally, under this specification, individuals with preference types that are either all in the control group or all in treatment group do not provide information for the estimation of treatment effects.

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A, individuals with preference orderings $\{A \succ B, B \succ A, A\}$ have all the same propensity score. If A is in their list, it does not matter if they are also willing to attend school B or not. The relative ease to get into different schools can be characterized by schools' cutoffs, that is, the (randomly assigned) position or rank of the enrollee who gets the last seat in the school.[12] Schools with lower cutoffs are harder to get in. While in a discrete economy a school's cutoff might depend on the lottery realization, in a continuum economy each school's cutoff is a deterministic function of the vectors of school capacities and preference orderings. Consider an economy with a unit mass continuum of individuals and lottery numbers drawn from a standard uniform distribution ($U \sim [0,1]$), the propensity score of a person with preference type θ ($p(\theta)$) is the probability of obtaining a rank lower than the highest cutoff among all schools in her list:

$$p(\theta) \;=\; P(U \leq c_\theta) \;=\; c_\theta, \quad \text{with} \;\; c_\theta \;\equiv\; \max_{s \in S_\theta}\{c_s\} \tag{1}$$

where S_θ is the set of schools listed by an enrollee with preference type θ, and c_s is the cutoff for school s. The fact that propensity scores do not depend on the number and on the relative order of schools listed (but just on the set of schools in the list) reveals the potential benefits of propensity score stratification relative to full preference stratification: while in a setting with N different schools, the number of possible preference orderings is in the order of N factorial, the number of propensity score values is at most N. Propensity score stratification allows for fewer and larger strata, leading to potentially important sample size gains. Consider the following example: in an economy with two schools (A and B) and 100 enrollees, preferences and school capacities are such that schools' cutoffs are $c_A = 70$ and $c_B = 50$. In this economy, it is easier to get a seat in school A than in school B: individuals randomly ranked up to 70th position can get a seat in school A, but only those ranked up to the 50th position can to get a seat in school B. Since all enrollees have the same probability of being assigned any rank, the propensity score (that is, the probability of obtaining a seat in *some* school) is 0.7 for those who are willing to go to school A and 0.5 for those who only want school B. The example shows how the number of conditioning sets falls: while, in this economy, there are four possible preference types ($\{A \succ B, B \succ A, A, B\}$), there are only two propensity score strata.

The critical step is to compute the cutoff for each school. These cutoffs can be computed by simulation: repeating the random assignment several times and obtaining the average rank of the enrollee who gets the last seat in each school. Propensity scores can then be approximated by:

$$\hat{p}(\theta) \;=\; \max_{s \in S_\theta}\{\hat{c}_s\} \tag{2}$$

$$\hat{c}_s \;\equiv\; \tfrac{1}{T}\Sigma_{t=1}^{T}\tfrac{r_{st}}{N} \tag{3}$$

$$r_{st} \;\equiv\; \begin{cases} r_{st}^{last}, & \text{if all seats in } s \text{ are allocated} \\ N, & \text{if some seat in } s \text{ is not allocated} \end{cases} \tag{4}$$

where T is the number of iterations, N is the total number of enrollees, and r_{st}^{last} is the rank of the enrollee obtaining the last seat at school s in iteration t.

[12] If a school is not filled, its cutoff is equal to the total number of enrollees (that is, the maximum rank across enrollees).

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To compute the propensity score without resorting to simulation, we build on results by Che and Kojima (2010). Consider an economy with a unit mass continuum of individuals. There is a mass of seats to be allocated among these people, distributed in a set of schools (S_1). Each school has a mass n_s of seats available. Individuals have strict preferences over elements in $\bar{S}_1 = S_1 \cup \{H\}$, where H stands for the alternative of staying at home and not taking the course. For each school s and set of schools $L \subseteq S_1$, $\pi_s(L) \in [0,1]$ is the fraction of individuals who prefer school s to all other schools in L and to staying at home. Individuals are assigned a random rank (r) from a standard uniform distribution ($U \sim [0,1]$). The fraction of individuals with rank lower than r is therefore $P(U \leq r) = r$. The intuition for obtaining the lowest cutoff (that is, the hardest school to get in) is simple: while all schools have seats available, all enrollees with low ranks get a seat in their most preferred school. This simple allocation rule finishes when there are no more seats available in some school. To compute the first cutoff (c_1), we obtain for each school $s \in S_1$ a value c_1^s such that the fraction of people who prefer s ($\pi_s(S_1)$) multiplied by the mass of enrollees with rank lower than c_1^s is equal to the mass of seats in s (n_s). The "hardest schools to get in" (k_1) are the ones with the lowest value c_1^s. Formally:

$$c_1^s = \{c \mid \pi_s(S_1) \cdot c = n_s\} \tag{5}$$

$$c_1 \equiv \min_{s \in S_1}\{c_1^s\} \tag{6}$$

$$k_1 \equiv \{s \in S_1 \mid c_1^s = c_1\} \tag{7}$$

If $c_1 > 1$, all enrollees are assigned to their first choice. Otherwise, once the first school(s) is full, enrollees are allocated among the set of schools with open seats. This procedure continues until all individuals are allocated to some school ($c_n > 1$) or there are no more seats available. To compute the following cutoffs we define the set of schools with open seats after the n-th cutoff: $S_{n+1} \equiv S_n \setminus k_n$. We also define the values $A_1^n \equiv 0$ and $c_0 \equiv 0$. The cutoffs and the sets of schools with open seats can then be obtained iteratively:

$$c_n = \min_{s \in S_n}\{c_n^s\} \quad \text{for } n \geq 2 \tag{8}$$

$$c_n^s = \{c \mid A_n^s + \pi_s(S_n) \cdot (c_n^s - c_{n-1}) = n_s\} \quad \text{for } n \geq 2 \tag{9}$$

$$A_n^s = A_{n-1}^s + \pi_s(S_{n-1}) \cdot (c_{n-1} - c_{n-2}) \quad \text{for } n \geq 2 \tag{10}$$

$$k_n = \{s \in S_n \mid c_n^s = c_n\} \quad \text{for } n \geq 2 \tag{11}$$

The above expressions show, for example, that to compute the second cutoff we need to consider that those who listed the hardest school as their first choice and didn't get in are now allocated to their second choice (hence, we need to compute $\pi_s(S_2)$), and that some school seats are allocated before the first cutoff ($\pi_s(S_1) \cdot c_1$).

In our discrete economy application, we approximate each of the values with the observed frequencies. Let $P_s(S_n)$ be the number of enrollees who prefer school s among schools in set S_n, and N_s the number of seats in school s. Cutoffs can be iteratively

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approximated:

$$\hat{c}_n = \min_{s \in S_n} \{ \frac{num_n}{denom_n} \} \tag{12}$$

$$num_n \equiv \begin{cases} N_s & \text{for } n = 1 \\ num_{n-1} + P_s(S_n) \cdot \hat{c}_{n-1} & \text{for } n \geq 2 \end{cases} \tag{13}$$

$$denom_n \equiv \begin{cases} P_s(S_1) & \text{for } n = 1 \\ denom_{n-1} + P_s(S_n) & \text{for } n \geq 2 \end{cases} \tag{14}$$

For example, to compute the first cutoff, we calculate for each school the ratio between the number of available seats and the number of people who chose that school as a first option. The school with the lowest ratio is the hardest school to get in. Its cutoff value is given by the value of this ratio. Once all cutoffs are computed, propensity scores can be retrieved from equation (2). Computing the propensity score analytically and relating them to fundamentals of the matching problem helps to better understand the determinants of propensity scores for individuals with different preference orderings. This allows, for example, to compute how, given a distribution of preferences, the vector of school capacities can be changed to boost statistical power, crucial for program evaluations.

Academia BA Emprende: Enrollees and Propensity Scores.

The above formulas allow to approximate cutoff values for schools, and propensity scores for enrollees. As reported above, 806 people enrolled for the September 2016 edition of Academia BA Emprende life-skills course, representing 211 different preference orderings. Given the distribution of preference orderings and school capacities, cutoff values for schools ranged from 0.38 (hardest school to get in) to 1 (an under-subscribed school). These cutoff values mapped into 9 different propensity score values, also ranging from 0.38 to 1. A total of 90 (11.1%) individuals included the under-subscribed school in their list and, therefore, have a propensity score equal to 1. These people fall from the sample both under full preference stratification and propensity score stratification since there is no variation in assignment to be exploited within them: they all get a seat. Relative to full preference stratification, using propensity scores leads to an increase in the sample in consideration from 576 to 716. In the empirical analysis, these numbers fall to 196 and 237, respectively, due to large survey attrition.

In the entrepreneurship course, there were 1649 enrollees and a total of 550 preference orderings over 18 different school/shifts. Cutoffs and propensity score values ranged from 0.125 to 1: 259 (15.7%) individuals listed an under-subscribed school and have a propensity score equal to 1. Propensity score stratification allows to increase the sample in consideration from 1041 to 1390 individuals (or from 377 to 499 people when accounting for attrition).

For each of the two courses, we compared the propensity score values obtained using cutoffs derived from expression (12) with the ones obtained from (i) simulating the random assignment 500 times and computing the fraction of times each enrollee gets a seat (rounding to the nearest hundredth and to the nearest thousandth), and (ii) obtaining

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each school's cutoff by simulation and then computing propensity scores (expression (2)). Different ways of computing propensity scores lead to almost the same results, as it can be seen by the correlations reported in table (1). However, the analytical formulation proposed by Abdulkadiroğlu, Angrist, Narita and Pathak (2017) smooths estimated scores and leads to fewer and larger strata. The further contribution of computing propensity scores using expression (12) is to relate scores to the fundamentals of the matching problem. This helps to better understand where their values come from and how they would be affected by changes in school capacities or enrollees' preferences.

4 Balance Tests and Follow-up Survey

To assess the success of propensity score stratification in eliminating selection bias, we check for conditional balance in a set of individual characteristics. Enrollees' personal and socio-demographic information was obtained from their responses to the online registration survey, which had to be completed during the application process before the assignment of seats. Information includes: gender, age, the way they found out about the course, educational attainment, employment status, income level, and previous assistance to similar training courses, among other things.

The follow-up survey was administered by email 3 months after the end of the courses. Enrollees were given 20 days to complete it, and reminders were sent every three days. The emails indicated that the information would be used to improve the design of municipal government programs, and that respondents would enter a lottery for 3 tablets (with a value of approximately 150 dollars each). The survey was answered by 237 life-skills course enrollees (33.1%) and by 499 entrepreneurship course enrollees (35.9%).[13] The median respondent took 11 minutes to answer the survey. The high attrition rates indicate that incentives to take the survey were not sufficient, and probably reveal some concerns on how information would be used (although every question stated that answers would be treated confidentially, trust in government institutions is not high).[14] Lack of program funding did not allow to provide greater incentives or to conduct face-to-face interviews.

Tables 2 (life-skills course) and 3 (entrepreneurship course) report the mean of each variable for the treatment and control groups (columns 1 and 2), their unconditional difference (column 3), and their difference (and its p-value) after partialling out fixed effects for propensity scores strata (columns 3 and 4). The information in these tables considers only those persons who responded to the follow-up survey.[15] Given that the assessment of the impact of the course is based on responses to this survey, we verify that respondents in the control and treatment groups are conditionally comparable, and that there are no

[13] These numbers consider only those who responded the survey completely (124 enrollees started the survey but never completed it), and do not exhibit patterns of automatic response -29 cases- (for example, answering always the first option of the question throughout an entire survey section) or inconsistencies -38 cases- (that is, giving contradictory answers to questions in the survey).

[14] In the latest Latinobarometro survey (2015, available online at www.latinobarometro.org (accessed March 6th, 2017)), around two thirds of Argentinean respondents indicated having none or low trust in government institutions.

[15] Tables with information on all enrollees are included in the appendix (tables A2 and A3)

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patterns of differential non-response across groups.[16]

Individual characteristics are generally balanced across treatment and control groups. In the life-skills course, there are some differences in recent income changes. In the entrepreneurship course, people in the control group are (conditionally) more likely to have a graduate degree and to be unwilling to report their income. These differences are not more than those expected to happen by chance. Still, in the regression analysis, we report estimates when controlling for these variables. In both courses, the p-values of the joint significance tests indicate that, when controlling for the propensity score strata of enrollees, personal characteristics are not informative on the result of the assignment mechanism.[17]

To check the existence of differential attrition between treatment and control individuals in a formal way, we regress an indicator variable of survey attrition on (a) enrollees' personal characteristics, (b) a treatment dummy, and (c) the interaction between personal characteristics and treatment status. The F-statistic of the interaction terms (c) indicates that there is no evidence of differential attrition in any of the two courses (see tables A5 and A6 reported in the Appendix).[18]

5 Empirical Strategy

We estimate the impact of *assignment* (intention-to-treat) and *attendance* to each of the two courses. To measure the intention-to-treat effect (ITT) we estimate the following regression model:

$$y_{ip} \quad = \sigma_p + \beta T_{ip} + \boldsymbol{\theta X_{ip}} + \varepsilon_{ip} \tag{15}$$

where y_{ip} is the post-course value of outcome y for enrollee i (with propensity score p); T_{ip} is a dummy variable indicating if enrollee i got a seat in the course; σ_p are propensity score fixed effects associated to outcome y; and $\boldsymbol{X_i}$ includes a number of individual baseline characteristics that help augment precision, and account for chance imbalances between the characteristics of the members of the treatment and control groups. The coefficient of interest is β, which gives the average impact of being assigned to the course on outcome y. The identification of a causal effect of assignment relies on the "equal treatment of

[16] We expect to obtain administrative data that would cover the whole sample in consideration. The data request is currently being processed by the Evaluation Unit of the municipal government of the City of Buenos Aires.

[17] In the life-skills course, the unconditional differences (table 2, column 2) show a very similar picture to that of the conditional comparison, as preference types (and propensity score strata) are not strongly related to specific individual characteristics. In the entrepreneurship course (table 3, column 2), the unconditional comparison shows some important differences between treatment and control groups (in occupation, income and previous entrepreneurial experience) that disappear once we compare within propensity score strata, validating the empirical strategy.

[18] The high attrition rates are nevertheless worrisome both for the *internal validity* (as there still might be differential attrition in terms of unobservables) and for the *external validity* (it is difficult to argue that respondents are representative of the whole sample) of the study. We have requested administrative data on labor careers of all enrollees to address these concerns. The data request is currently being processed by the Evaluation Unit of the municipal government of the City of Buenos Aires.

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equals" property of the assignment mechanism, which ensures that, conditional on preferences or propensity scores, treatment assignment is random and independent of potential outcomes.

To estimate the average effect of *attending* the courses we use an instrumental variables (IV) approach (Angrist, Imbens and Rubin, 1996). We use the *assignment* to the treatment to instrument attendance, exploiting that (i) assignment is random conditional on propensity score strata; (ii) assignment is highly correlated to attendance since enrollees who do not get a seat are not allowed to participate in the courses; and (iii) assignment is likely to affect outcomes of interest only through attendance to the course (exclusion restriction). This set of conditions allows us to credibly identify a causal effect of attending the courses for each propensity score stratum.[19] We use a regression model, and estimate an average treatment effect using information from all the different strata. Formally, for each of the courses and each of the outcomes of interest, we estimate the following two-stage linear model:

$$C_{ip} = \alpha_p + \gamma T_{ip} + \boldsymbol{\beta X_{ip}} + \epsilon_{ip} \tag{16}$$

$$y_{ip} = \omega_p + \delta C_{ip} + \boldsymbol{\Theta X_{ip}} + u_{ip} \tag{17}$$

where C_{ip} is a dummy variable indicating if enrollee i attended at least one class of the course; α_p and ω_p are propensity score fixed effects associated to attendance and outcome y, respectively; and the other variables have the same definition as in equation (15).

We consider those who participated in *at least one class* as having attended the course because each class provides specific content, is important on its own, and might have an effect on the outcomes of interest. The exclusion restriction is therefore more plausible under this specification than if we considered, for example, attendance based on having completed the course: for those who do not complete the course, obtaining a seat could still impact potential outcomes by inducing participation in some class.

The coefficient of interest is δ, which gives the average impact of attending the course on enrollees to whom the assignment to the treatment group induces to take the course, also known as *compliers* (Angrist, Imbens and Rubin, 1996). In our application, this *local average treatment effect* (LATE) is equal to the *average treatment on the treated* (TOT), since only those assigned to the treatment group can take the course.

Attendance to the Courses.

Attendance to the course has been steadily low in the first years of Academia BA Emprende program: on average, only 50% of those enrollees who got a seat actually showed up to the course. This number is lower than the 65% average participation rate in similar courses in developing countries reported by McKenzie and Woodruff (2013). The introduction of the centralized assignment mechanism did not change this pattern. In the

[19] Actually, we need to further assume (i) SUTVA (an enrollee's course attendance and outcomes of interest are not affected by other enrollees' assignment), and (ii) monotonicity (that is, obtaining a seat can only increase (and never decrease) potential attendance to the course) (Angrist, Imbens and Rubin, 1996).

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September 2016 edition of the program, only 47.3% of those admitted to the life-skills course attended at least one class (25.5% attended at least five classes, and 10.3% took the seven classes). Attendance was a bit higher in the entrepreneurship course: 55.8% attended at least one class, 29% attended at least five classes, and 11.3% went to every class. Still, in spite of the relatively low attendance rate, course assignment remains a strong instrument for attendance, as participation in the course is only permitted to those who obtain a seat.[20] To formally estimate the average effect of course assignment on course attendance, we estimate the first-stage equation (16). Results are reported in table A4, included in the appendix.

6 Evaluation Results

We measure the courses' short-term impact at three months after training on 4 different families of outcomes of interest: (a) course-related knowledge; (b) employment status; (c) entrepreneurial activity; and (d) socio-emotional skills. Results show that participants assimilate part of the content taught in the course. There are also effects on employment status and entrepreneurial activity largely consistent with the objectives of the different courses. On the contrary, we do not observe any significant effect on socio-emotional skills.

Course-Related Knowledge.

To understand if the courses help attendees to acquire specific knowledge that could be then used in their personal and professional lives, we built two different short questionnaires (one for each course), which included questions on the most important topics discussed in class.

In the life-skills course, the questionnaire consisted of 10 questions on the five main topics covered in the course: self-knowledge, communication, conflict resolution, leadership, and creativity. For each topic, respondents were given one or more sentences stating concepts or ideas discussed in class, and were asked to indicate their level of agreement or disagreement on a scale of 1 (completely disagree) to 7 (completely agree). Answers are considered *correct* if respondents partially agree (values strictly greater than 4) with concepts that are true according to the course's contents, or if they disagree (values strictly smaller than 4) with false concepts.[21] Results, reported in table 4 (Panel A), show that attendees incorporate part of the contents taught in the course: coefficients are positive for all topics, and statistically significant for communication, leadership, and creative process. Attending the course raises the average number of correct answers in 11 percentage points (column 6), from 61 percent to 72 percent, an effect that is statistically significant at the 1-percent level.[22] To understand the magnitude of this impact it is useful to com-

[20]The first stage F-statistics is greater than 200 in the life-skills course, and greater than 400 in the entrepreneurship course. Exact values are reported in table A4.

[21]Results do not change qualitatively (or in terms of statistical significance) when the intensity of responses is considered and original values (1 to 7) are used.

[22]The comparison of the treatment effect is done against the control complier mean (CCM), obtained as the difference between the treatment effect and the average outcome (in this case, average correct

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pare the percentages of correct answers against the percentage that would be obtained if choosing randomly -and in a uniform way- between the different answers. The percentage of random correct answers would be 42 percent (3 out of 7 numbers). Both those who attended the course and those in the control group have a better performance than that of a population that responds in a random manner, which may indicate that concepts taught in the course are also incorporated -though with less effectiveness- from alternative sources.

In the entrepreneurship course, the questionnaire included 7 questions covering the topics of Design Thinking, Canvas, Value Proposition, and Lean Start-Up. Some questions had the same format of the ones in the life-skills course questionnaire (where a concept discussed in the course was presented, and respondents were asked to indicate their degree of agreement or disagreement) and others were multiple choice (with only one correct answer). Table 4 (Panel B) shows that attendees of the course obtained a higher percentage of correct answers on all topics. Coefficients are positive and statistically significant for the different topics. Entrepreneurship training increases the average of correct answers in 22 percentage points, from 32 percent to 54 percent (column 5). In this case, opposed to the life-skills course, we observe that the average of the control group is very low (especially in the questions related to Canvas business models and Design Thinking), reflecting that concepts taught are unknown to the majority of respondents and are difficult to acquire from alternative sources (even after taking the course the percentage of correct answers is not high).

Results in both segments show that, on average, attendees incorporate -at least in the short term- some of concepts taught in class. This impact on course-related knowledge validates tutors' work, and it is a relatively important pre-condition to find further changes in attendees' attitudes and decisions.

Employment Status.

Academia BA Emprende program seeks to provide tools to help attendees adapt to the requirements of nowadays' labor market and allow them to "create" their own jobs. To assess the short term impact on employment status, we asked enrollees to indicate which of the following best described their occupational status: (a) Employee, (b) Self-Employed, (c) Unemployed (including housekeepers and those who never had a job). To further assess their attitude towards the labor market and their current income level, we asked them if they were looking to find or change their jobs (either in an active or passive way), and to indicate their monthly income (choosing between 5 different categories, from "I have no income" up to "more than $20.000").[23]

Table 5 reports the courses' impact on employment status. Life-skills training reduces employer-employee relationships, and increases job-search. The magnitudes of both effects are sizable: attending the course decreases the fraction of people in employer-employee relationships by 22 percentage points (with a control complier mean of 63 percent), and raises job-search by approximately one-third (22 percentage points vs. control complier

responses) of those who attended at least one class.

[23]The exchange rate at the moment of the follow-up survey was approximately $13 per dollar.

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mean of 0.63).[24] These effects are consistent with different explanations. Looking at heterogeneity in the impact according to initial employment status (Table A7 in the appendix), we observe a positive effect in unemployment concentrated among those respondents who were not employed before the course. The course seems to lengthen the unemployment spell, which might be driven, among other things, by participants deciding to postpone job-search during its duration. Among those initially employed, the course induces a shift from employee to self-employed, and increases job search. The impact of the course on employment status, especially among this last group, is in line with the objective of the course, which proposes a more proactive stance in the development of one's own professional career, with certain emphasis on an entrepreneurial spirit. However, the short-term increase in unemployment spell among those initially unemployed might raise some concerns that need to be addressed by assessing if such impact persists over time.

The entrepreneurship course increases the probability that respondents report to be self-employed, and leads to lower job-search rates. Impact on self-employment rates is sizable: participation in the course raises the incidence of self-employed workers by 15 percentage points (vs. a control complier mean of 30 percent). Entrepreneurship training motivate people to move to self-employment and we observe (non statistically significant) falls in the fraction of unemployed, homemakers, and workers in employer-employee relationships. The impact on occupational status seems to be in line with the goals of the training, which provides tools to validate business ideas and promotes entrepreneurship. The fall in the job-search rate is consistent both with individuals being more satisfied with their new employment status, as well as with them dedicating more time to their new occupation. The shift towards self-employment has no clear short-term impact on individuals' income. However, we cannot reject the existence of large effects: the confidence interval of the treatment effect includes values up to 15% of the control complier mean.

Entrepreneurship.

Fostering and sustaining entrepreneurial activity is one the main objectives of Academia BA Emprende program, especially in its three more advanced courses. To assess the impact of the life-skills course and the entrepreneurship course on entrepreneurial activity, we asked enrollees if they had a business of their own (and to indicate the stage of development) by choosing one of the following options: (a) Yes, I have a business of my own; (b) No, but I've already set in motion my business venture; (c) No, but I have an idea to start a business; or (d) I do not have a business venture. In a separate question, we also asked to indicate the approximate dollar value of their business' monthly sales (by choosing between 7 categories, from "I do not have a business venture" up to "more than US$5.000").[25]

Estimates of the courses' impact on entrepreneurial activity are reported in table 6.

[24]The inclusion of covariates is important for the precision (and size) of estimates: pre-course employment status captures a sizable portion of the variation in post-course status, which reduces estimates' standard errors.

[25]Program officials favored the use of income categories over exact numbers to reduce potential concerns about confidentiality and increase response rates.

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Life-skills training does not have a significant impact on entrepreneurship. Standard deviations are large, and the no-impact null hypothesis cannot be rejected. The absence of a significant impact is not surprising, since, as explained in the description of the different courses, in spite of promoting an entrepreneurial mindset, life-skills training does not provide specific tools or knowledge to set up a business venture.

Entrepreneurship training has an impact on business ownership and entrepreneurial activity. This finding aligns with the objectives of the course: among attendees, more people declare to own a business or to be setting it in motion (not statistically significant), and fewer people indicate having an idea to start one. These effects are all consistent with the course helping participants to start a business from an initial idea, one of its main goals. Magnitudes are sizable: attending the course reduces the fraction of people who declare just having an idea to start a business by 20 percentage points (vs. a control complier mean of 46 percent), while the fraction of business owners raises by 12 percentage points and of those who are setting up a business by 6 percentage points. The course has also a positive effect on business sales, which is partially driven by the creation of new businesses (we impute zeros to those respondents who do not own a business). As indicated above, sales data come from self-reported average monthly sales categories, which might provide a good approximation of sales volumes but are not precise. The positive effect on sales should therefore be taken cautiously.

Socio-Emotional Skills.

In recent years, there has been an increased focus on the importance of socio-emotional skills for labor market outcomes and entrepreneurship (Campos et al, 2017; Dening, 2017; Heckman et al, 2013). In response to these trends, Academia BA Emprende program seeks to help attendees of the different courses acquire a set of soft skills (from self-knowledge to leadership capabilities) to help them adapt and succeed in their personal and professional paths. Table 7 reports the courses impact on different socio-emotional skills. The follow-up survey included a series of questions to measure different skills among participants: locus of control, empathy, self-efficacy, and the ability to carry out projects, among others. The questions asked were obtained from psychometric tests designed to evaluate the different abilities and from the literature on *entrepreneurship* that studies the importance of these qualities on the entrepreneurial performance.[26]

We do not observe a significant impact in any of the socio-emotional skills evaluated in the survey for either of the two courses. Although it is not possible to discard that changes will arise in the medium run after participants process the contents of the courses, these results should increase awareness about the difficulty of changing these traits, often

[26]The questions were obtained from the following studies: Locus of Control: a selection of questions from the "intrapersonal" and "employment" components of Rotter's Locus of Control Scale (Rotter, 1966). The Spanish version was obtained from the work of Brenlla and Vázquez (2010). Empathy: a selection of questions from the "perspective taking" component of the Interpersonal Reactivity Index (IRI) developed by Davis (1980, 1983). The Spanish version was obtained from the work of Mestre Escrivá et al (2004). Project and Self-efficacy: questions were obtained from the work by Brenlla (2014), used in previous evaluations by Corporación Andina de Fomento (CAF).

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deeply rooted in people's personality. Changing these skills might require a different approach or an intensive training focused exclusively in changing specific personality traits (for example, Campos et al, 2017). This is specially important for the life-skills course, which has a particular emphasis on socio-emotional skills.

It is important to note that the way of measuring these skills, which must be done with short questionnaires used in survey contexts, can also decrease the precision of the measurements and hinder the detection of impacts. The fact that these socio-emotional skills are not measured before the start of the course does not contribute to the statistical power either. In addition to the variables reported in the tables, questions about personal well-being, creativity, trust in others, and attitude towards risk were included, and we did not observe statistically significant effects of the courses.

Accounting for Survey Attrition.

Even if there is no evidence of differential attrition and response rates are not statistically different between treatment and control groups, the high attrition rates are worrisome. To assess if our results might be affected by differential attrition, we re-estimate the regressions for all outcomes of interest using inverse probability of response as sample weights. We first use enrollees' baseline characteristics to estimate the probability of response, and then use the inverse of these probabilities as sample weights, attaching more importance to respondents who are similar to attriters. Results, reported in tables A8 to A11 included in the appendix, show that estimated impacts are not affected by the sample re-weighting.

7 Conclusion

This paper presents the short-term impact of a life-skills training course and an entrepreneurship training course offered by the municipal government of the city of Buenos Aires, Argentina. We introduced a random serial dictatorship procedure to fill seats in different schools. This mechanism uses a lottery to ration seats in oversubscribed schools and satisfies the "equal treatment of equals" property (that is, people who share the same preferences over schools have the same probability of getting a seat). These characteristics of the assignment mechanism imply that assignment is random conditional on preferences. We exploit this random variation in assignment using a propensity score stratification, as recently proposed by Abdulkadiroğlu, Angrist, Narita, and Pathak (2017). To compute propensity scores, we build on results by Che and Kojima (2010) and relate propensity score values to the fundamentals of the matching problem.

In terms of methodology, this study supports the relevance and generality of the results by Abdulkadiroğlu, Angrist, Narita, and Pathak (2017): in our application, propensity score stratification leads to important sample size gains relative to full preference stratification. Furthermore, our formulation of propensity scores as a function of enrollees' preferences and school capacities helps to better understand where propensity scores come from and how they would be affected by changes in those variables. This allows, for example, to compute how, given a distribution of preferences, the vector of school capacities

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could be changed to boost statistical power, a topic to be further developed.

We measured the impact of the courses on 4 different dimensions: (a) course-related knowledge; (b) employment status; (c) entrepreneurial activity; and (d) social-emotional skills. The results show that, on average, attendees incorporate content taught during the courses. This result is encouraging, since it validates the efficacy of the tutors and is a key condition for subsequent changes in attitude and behavior.

Entrepreneurship training helps participants start a business from an initial idea, thus leading to more business ownership and self-employment. These results are consistent with previous findings by McKenzie and Woodruff (2013) in developing countries, and by Fairlie, Karlan, and Zinman (2015) in United States. Medium and long run evaluations of the impact need to follow to understand if it is long-lasting, and to assess the effectiveness of the course in boosting entrepreneurial activity.

We find no effects of the training on socio-emotional skills, such as locus of control, empathy or self-efficacy. Given the growing importance of these skills in labor market outcomes and the emphasis given to them (especially in the life-skills course), these results should increase awareness about the difficulty of changing these traits, and should be taken into account when designing interventions targeting these traits. In terms of the evaluation, different measures of these skills and baseline levels should be collected to increase reliability.

The findings of this paper have two main empirical issues: high attrition, and the reliance on self-reported data (which might be influenced by experimenter demand effects). These problems, not uncommon in the literature (McKenzie and Woodruff, 2013), could be addressed by using administrative data on labor and entrepreneurship outcomes of all enrollees (already requested to the program officials). New editions of the Academia BA Emprende program were launched in 2017, and we expect to include them in subsequent versions of the paper.

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Table 1: Propensity Score Stratification and Sample Sizes

Life-Skills Training

			Correlation with:			
Method	**Strata**	**Sample**	Simul. (000)	Simul. (00)	AANP	AANP/CK
Full preferences	211	576				
Simulated (000)	105	635	1.0000			
Simulated (00)	33	708	0.9999	1.0000		
AANP	9	716	0.9987	0.9986	1.0000	
AANP + CK	9	716	0.9970	0.9971	0.9975	1.0000

Entrepreneuship Training

			Correlation with:			
Method	**Strata**	**Sample**	Simul. (000)	Simul. (00)	AANP	AANP/CK
Full preferences	550	1041				
Simulated (000)	203	1236	1.0000			
Simulated (00)	55	1378	0.9999	1.0000		
AANP	13	1390	0.9993	0.9993	1.0000	
AANP + CK	13	1390	0.9959	0.9958	0.9963	1.0000

+ Full preference: full preferences type stratification. Simulated (000): propensity score stratification, using scores obtained from simulating the assignment 500 times (rounded to the nearest 1000-th). Simulated (00): propensity score stratification, using scores obtained from simulating the assignment 500 times (rounded to the nearest 100-th). AANP: propensity score stratification, using scores based on Abdulkadiroglu et al (2017) and school cutoffs obtained from simulating the assignment 500 times. AANP + CK: propensity score stratification, using scores based on Abdulkadiroglu et al (2017) and school cutoffs obtained analytically using results based on Che and Kojima (2010).

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Table 2: Life-Skills Course: Covariate Balance (Respondents Follow-up Survey)

	(1) Treatment (Mean)	(2) Control (Mean)	(3) Unadj. Difference	(4) Adjusted Difference	(5) Ad. Diff. (p-value)
Age	36.6	36.7	-0.15	0.26	0.85
Female	0.77	0.76	0.0042	-0.019	0.74
Entrepreneur	0.29	0.33	-0.041	-0.046	0.46
Informed [Facebook]	0.49	0.52	-0.031	-0.041	0.55
Informed [Friend]	0.26	0.23	0.028	0.020	0.73
Lab. [Employee]	0.41	0.37	0.039	0.11*	0.078
Lab. [Self-employed]	0.29	0.30	-0.014	-0.052	0.40
Lab. [Unemployed]	0.30	0.33	-0.026	-0.056	0.35
Educ. [Post-Second.]	0.24	0.22	0.022	0.0063	0.91
Educ. [Undergrad.]	0.35	0.38	-0.025	-0.028	0.68
Educ. [Graduate]	0.078	0.064	0.014	0.020	0.57
Δ Inc. [$<$ 0]	0.21	0.34	-0.13**	-0.16***	0.0062
Δ Inc. [$<$ Inflation]	0.43	0.33	0.099	0.11*	0.083
Δ Inc. [$>$ Inflation]	0.26	0.23	0.028	0.038	0.50
Income	11106.4	10807.4	298.9	661.0	0.44
Income [No reply]	0.16	0.17	-0.010	-0.014	0.77
Training [1 course]	0.21	0.23	-0.018	-0.0083	0.88
Training [2+ courses]	0.10	0.16	-0.054	-0.040	0.39
Goal-Oriented [1-5]	2.63	2.65	-0.019	-0.017	0.90
Organized [Yes]	0.72	0.72	0.0032	0.026	0.67
Problem Solver [Yes]	0.70	0.68	0.016	0.022	0.72
N	128	109			
F (p-value)			0.97	0.84	

[a] Information refers to enrollees with propensity score strictly between 0 and 1 who answered the follow-up survey.

[b] Columns (5) and (6) report the coefficient and p-value from a regression of the corresponding variable on treatment status *including fixed effects for propensity score strata*.

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Table 3: Entrepreneurship Course: Covariate Balance (Respondents Follow-up Survey)

	(1) Treatment (Mean)	(2) Control (Mean)	(3) Unadj. Difference	(4) Adjusted Difference	(5) Ad. Diff. (p-value)
Age	37.2	36.5	0.70	0.70	0.50
Female	0.69	0.64	0.042	0.018	0.70
Entrepreneur	0.30	0.24	0.056	0.029	0.50
Informed [Facebook]	0.51	0.45	0.055	0.048	0.34
Informed [Friend]	0.25	0.27	-0.019	-0.0015	0.97
Lab. [Employee]	0.31	0.48	-0.17***	-0.0055	0.90
Lab. [Self-employed]	0.32	0.29	0.028	-0.057	0.20
Lab. [Unemployed]	0.37	0.23	0.14***	0.063	0.16
Educ. [Post-Second.]	0.22	0.25	-0.036	-0.031	0.46
Educ. [Undergrad.]	0.35	0.39	-0.034	-0.013	0.78
Educ. [Graduate]	0.053	0.10	-0.050**	-0.068**	0.028
Δ Inc. [$<$ 0]	0.29	0.23	0.052	-0.0076	0.87
Δ Inc. [$<$ Inflation]	0.39	0.44	-0.047	0.017	0.71
Δ Inc. [$>$ Inflation]	0.19	0.25	-0.060	-0.042	0.31
Income	10927.1	12236.3	-1309.2**	-292.1	0.64
Income [No reply]	0.17	0.11	0.061*	0.068*	0.083
Training [Yes]	0.27	0.26	0.010	-0.0064	0.89
Entrepreneur	0.47	0.43	0.042	0.043	0.40
Will to Start Firm	0.97	0.98	-0.0084	-0.016	0.34
Capital to Start	0.49	0.43	0.053	0.053	0.29
Motive [Income]	0.12	0.15	-0.026	-0.031	0.34
Motive [Own Boss]	0.59	0.60	-0.0051	0.0058	0.91
N	207	292			
F (p-value)			0.00	0.20	

[a] Information refers to enrollees with propensity score strictly between 0 and 1 who answered the follow-up survey.

[b] Columns (5) and (6) report the coefficient and p-value from a regression of the corresponding variable on treatment status *including fixed effects for propensity score strata.*

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Table 4: Impact on Knowledge

A. Life-Skills Course

	(1) Self-Knowledge	(2) Effective Comm.	(3) Conflict Resol.	(4) Leadership	(5) Creativity	(6) All
ITT	0.068	0.12**	0.059	0.064**	0.071**	0.072***
	(0.045)	(0.058)	(0.065)	(0.030)	(0.032)	(0.023)
TOT	0.11	0.19**	0.094	0.10**	0.11**	0.11***
	(0.071)	(0.092)	(0.10)	(0.048)	(0.049)	(0.036)
N	237	237	237	237	237	237
Control Mean	0.80	0.64	0.43	0.65	0.58	0.62
C. Complier Mean	0.85	0.61	0.44	0.64	0.57	0.61

B. Entrepreneurship Course

	(1) Design Thinking	(2) Canvas	(3) Value Proposition	(4) Lean StartUp	(5) All
ITT	0.095***	0.25***	0.14***	0.15***	0.16***
	(0.029)	(0.034)	(0.035)	(0.050)	(0.024)
TOT	0.13***	0.35***	0.19***	0.21***	0.22***
	(0.041)	(0.047)	(0.048)	(0.069)	(0.033)
N	499	499	499	499	499
Control Mean	0.24	0.15	0.59	0.52	0.35
C. Complier Mean	0.22	0.09	0.57	0.47	0.32

[a] *Intention-to-Treat* (ITT) coefficients indicate the average effect of being assigned to the course. The *treatment effect on the treated* (TOT) reports the average effect of the course for those who attended at least 1 class. TOT is estimated via instrumental variables, using the outcome of the random assignment as excluded instrument. All regressions include individual controls (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

[c] Dependent variables are the average of correct answers to questions on course-related knowledge. In the life-skills course, topics are: Self-knowledge (1 question), Communication (1), Conflict Resolution (1), Leadership (3), and Creativity (4). For the entrepreneurship course, topics are: Design Thinking (2 questions), Canvas (2), Value proposition (2), and Lean Start-up (1).

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table 5: Impact on Employment Status

A. Life-Skills Course

	(1) Employee	(2) Self-Employed	(3) Unemployed	(4) Income	(5) Job-Search
ITT	-0.14***	0.054	0.061	-727.7	0.14***
	(0.038)	(0.044)	(0.042)	(656.7)	(0.046)
TOT	-0.22***	0.086	0.096	-1154.6	0.22***
	(0.063)	(0.070)	(0.067)	(1049.0)	(0.076)
N	237	237	237	237	237
Control Mean	0.46	0.35	0.16	10596.33	0.76
C. Complier Mean	0.63	0.28	0.08	12612.93	0.63

B. Entrepreneurship Course

	(1) Employee	(2) Self-Employed	(3) Unemployed	(4) Income	(5) Job-Search
ITT	-0.064*	0.11***	-0.028	486.5	-0.084**
	(0.035)	(0.033)	(0.037)	(589.9)	(0.038)
TOT	-0.089*	0.15***	-0.039	673.9	-0.12**
	(0.048)	(0.046)	(0.051)	(816.7)	(0.052)
N	499	499	499	499	499
Control Mean	0.53	0.25	0.17	11780.82	0.85
C. Complier Mean	0.38	0.30	0.23	9442.75	0.86

[a] *Intention-to-Treat* (ITT) coefficients indicate the average effect of being assigned to the course. The *treatment effect on the treated* (TOT) reports the average effect of the course for those who attended at least 1 class. TOT is estimated via instrumental variables, using the outcome of the random assignment as excluded instrument. All regressions include individual controls (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

[c] Dependent variables in first three columns are obtained from the answers to question: Which of the following best describes your employment status? (a) Employee. (b) Self-employed. (c) Unemployed (including homemaker and those who never had a job). Dependent variable in the fifth column (Job-Search) indicates who reported to be looking for a job, either in a passive or active way (regardless of their employment status).

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table 6: Impact on Entrepreneurial Activity

A. Life-Skills Course

	(1) Own Firm [Yes]	(2) Own Firm [Starting]	(3) Own Firm [Idea]	(4) Own Firm [No]	(5) Sales
ITT	-0.018	-0.028	0.063	-0.016	1.40
	(0.048)	(0.044)	(0.059)	(0.050)	(88.6)
TOT	-0.029	-0.045	0.099	-0.026	2.22
	(0.076)	(0.070)	(0.092)	(0.079)	(140.6)
N	237	237	237	237	237
Control Mean	0.35	0.13	0.32	0.20	189.91
C. Complier Mean	0.31	0.16	0.34	0.18	143.61

B. Entrepreneurship Course

	(1) Own Firm [Yes]	(2) Own Firm [Starting]	(3) Own Firm [Idea]	(4) Own Firm [No]	(5) Sales
ITT	0.090**	0.044	-0.15***	0.012	137.5**
	(0.042)	(0.032)	(0.045)	(0.035)	(56.3)
TOT	0.12**	0.062	-0.20***	0.016	190.4**
	(0.058)	(0.045)	(0.062)	(0.048)	(78.3)
N	499	499	499	499	499
Control Mean	0.30	0.11	0.44	0.15	90.75
C. Complier Mean	0.33	0.11	0.46	0.11	73.56

[a] *Intention-to-Treat* (ITT) coefficients indicate the average effect of being assigned to the course. The *treatment effect on the treated* (TOT) reports the average effect of the course for those who attended at least 1 class. TOT is estimated via instrumental variables, using the outcome of the random assignment as excluded instrument. All regressions include individual controls (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

[c] Dependent variables in the first four columns are obtained from answers to question: Do you have a business of your own? (a) Yes. (b) No, but I've already set in motion my business venture. (c) No, but I have an idea to start a business. (d) No. Dependent variable in the fifth row is obtained from answers to question: What is the approximate sales level of your business? [Categories, in USD per month] A value of 0 is assigned to respondents who do not have a business or report that the venture does not have significant sales. In all other cases, the average value of the chosen category is assigned.

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table 7: Impact on Socio-Emotional Skills

A. Life-Skills Course

	(1) Locus of Control	(2) Empathy	(3) Goal-Orientation	(4) Self-Efficacy	(5) Index (All)
ITT	0.024	-0.042	0.013	-0.074	-0.020
	(0.078)	(0.083)	(0.11)	(0.12)	(0.073)
TOT	0.039	-0.066	0.020	-0.12	-0.031
	(0.12)	(0.13)	(0.18)	(0.19)	(0.12)
N	237	237	237	237	237
Control Mean	0.00	0.00	0.00	0.00	0.00
C. Complier Mean	-0.11	0.14	-0.01	0.03	0.01

B. Entrepreneurship Course

	(1) Locus of Control	(2) Empathy	(3) Goal-Orientation	(4) Self-Efficacy	(5) Index (All)
ITT	0.098*	0.089	0.032	0.048	0.067
	(0.056)	(0.058)	(0.081)	(0.093)	(0.052)
TOT	0.14*	0.12	0.044	0.066	0.093
	(0.078)	(0.080)	(0.11)	(0.13)	(0.073)
N	499	499	499	499	499
Control Mean	0.00	0.00	-0.00	-0.00	0.00
C. Complier Mean	-0.08	-0.03	-0.12	-0.18	-0.10

[a] *Intention-to-Treat* (ITT) coefficients indicate the average effect of being assigned to the course. The *treatment effect on the treated* (TOT) reports the average effect of the course for those who attended at least 1 class. TOT is estimated via instrumental variables, using the outcome of the random assignment as excluded instrument. All regressions include individual control (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ and * $p < 0.1$.

[c] Dependent variables are standardized averages (*z-score*) of the responses to different questions designed to measure the level of each socio-emotional skill. Locus of Control: 4 questions. Empathy: 5 questions. Personal Project: 3 questions. Self-efficacy: 3 questions. Index: average of the 4 previous z-scores.

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Appendix

Table A1: Schools and Number of Seats by Course

Life-Skills Course

Address	**Shifts**	**Seats**
Location 1	1	50
Location 2	1	50
Location 3	1	35
Location 4	1	50
Location 5	1	45
Location 6	1	80
Location 7	1	60
Location 8	2	160
Total	**9**	**530**

Entrepreneurship Course

Address	**Shifts**	**Seats**
Location 1	3	240
Location 2	1	60
Location 3	1	60
Location 4	1	50
Location 5	1	50
Location 6	1	100
Location 7	1	59
Location 8	2	100
Location 9	2	80
Location 10	1	50
Location 11	1	35
Location 12	1	45
Location 13	1	60
Location 14	1	80
Total	**18**	**1069**

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Table A2: Life-Skills Course: Covariate Balance

	(1) Treatment (Mean)	(2) Control (Mean)	(3) Unadj. Difference	(4) Adjusted Difference	(5) Ad. Diff. (p-value)
Age	34.6	34.8	-0.15	-0.13	0.87
Female	0.69	0.66	0.037	0.021	0.56
Entrepreneur	0.30	0.33	-0.027	-0.052	0.15
Informed [Facebook]	0.51	0.55	-0.046	-0.056	0.15
Informed [Friend]	0.24	0.24	0.0052	0.0047	0.89
Lab. [Employee]	0.36	0.38	-0.022	0.029	0.41
Lab. [Self-employed]	0.34	0.31	0.024	-0.0045	0.90
Lab. [Unemployed]	0.30	0.31	-0.0020	-0.024	0.48
Educ. [Post-Second.]	0.23	0.24	-0.0073	-0.027	0.40
Educ. [Undergrad.]	0.32	0.34	-0.027	-0.025	0.49
Educ. [Graduate]	0.077	0.085	-0.0079	-0.0042	0.85
Δ Inc. [$<$ 0]	0.24	0.26	-0.022	-0.042	0.21
Δ Inc. [$<$ Inflation]	0.38	0.37	0.015	0.039	0.30
Δ Inc. [$>$ Inflation]	0.26	0.24	0.016	0.016	0.64
Income	10412.8	10983.2	-570.4	-337.1	0.49
Income [No reply]	0.15	0.19	-0.042	-0.047	0.11
Training [1 course]	0.21	0.20	0.013	0.015	0.63
Training [2+ courses]	0.12	0.13	-0.015	-0.0031	0.90
Goal-Oriented [1-5]	2.69	2.72	-0.032	-0.049	0.53
Organized [Yes]	0.74	0.72	0.015	0.017	0.62
Problem Solver [Yes]	0.70	0.69	0.011	-0.011	0.75
N	400	316			
F (p-value)			0.72	0.59	

[a] Information refers to all enrollees with propensity score strictly between 0 and 1 who answered the follow-up survey.

[b] Columns (5) and (6) report the coefficient and p-value from a regression of the corresponding variable on treatment status *including fixed effects for propensity score strata*.

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Table A3: Entrepreneurship Course: Covariate Balance

	(1) Treatment (Mean)	(2) Control (Mean)	(3) Unadj. Difference	(4) Adjusted Difference	(5) Ad. Diff. (p-value)
Age	35.5	35.0	0.45	0.24	0.70
Female	0.64	0.62	0.012	-0.011	0.71
Entrepreneur	0.27	0.22	0.045*	0.024	0.34
Informed [Facebook]	0.48	0.45	0.028	0.0064	0.83
Informed [Friend]	0.28	0.30	-0.016	0.0082	0.76
Lab. [Employee]	0.35	0.47	-0.13***	0.014	0.60
Lab. [Self-employed]	0.34	0.28	0.065**	-0.00012	1.00
Lab. [Unemployed]	0.31	0.25	0.062**	-0.014	0.59
Educ. [Post-Second.]	0.21	0.23	-0.016	-0.027	0.28
Educ. [Undergrad.]	0.40	0.40	-0.0047	0.015	0.60
Educ. [Graduate]	0.085	0.093	-0.0078	-0.0084	0.64
Δ Inc. [$<$ 0]	0.25	0.21	0.039*	0.0070	0.79
Δ Inc. [$<$ Inflation]	0.41	0.45	-0.035	0.0031	0.92
Δ Inc. [$>$ Inflation]	0.22	0.25	-0.028	-0.010	0.69
Income	11277.9	12287.6	-1009.7***	-9.31	0.98
Income [No reply]	0.18	0.16	0.022	0.027	0.25
Training [Yes]	0.27	0.26	0.015	0.0078	0.77
Entrepreneur	0.46	0.43	0.029	0.014	0.63
Will to Start Firm	0.98	0.98	-0.0027	-0.0065	0.43
Capital to Start	0.50	0.47	0.035	0.055*	0.063
Motive [Income]	0.12	0.15	-0.027	-0.032	0.12
Motive [Own Boss]	0.62	0.61	0.0079	0.020	0.50
N	540	850			
F (p-value)			0.01	0.86	

[a] Information refers to all enrollees with propensity score strictly between 0 and 1.
[b] Columns (5) and (6) report the coefficient and p-value from a regression of the corresponding variable on treatment status *including fixed effects for propensity score strata.*

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Table A4: First Stage: Impact of Assignment on Attending at Least 1 Class

	Life-Skills		Entrepreneurship	
	(1) No Covars.	(2) Covars.	(3) No Covars.	(4) Covars.
Assignment	0.66***	0.63***	0.71***	0.72***
	(0.044)	(0.044)	(0.035)	(0.034)
N	237	237	499	499
F - 1st Stage	218.4	206.4	408.1	455.1
Indiv. Controls	No	Yes	No	Yes
P-Score FE	Yes	Yes	Yes	Yes

[a] Coefficients indicate the effect of course assignment on course attendance.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ y * $p < 0.1$.

[c] Dependent variables is a dummy variable indicating if the enrollee attended at least once to the course.

[d] Individual covariates in regression models reported in columns (2) and (4) include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

[e] Propensity Score FE: fixed effects for each propensity score value, which depends on the preferences on schools expressed in the baseline survey.

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Table A5: Life-Skills Course: Individual Characteristics and Attrition

	(1) Attrition		(2) Attrition	
Treat	0.025	(0.036)	0.134	(0.230)
Age × Treat			-0.001	(0.004)
Female × Treat			0.015	(0.077)
Entrepreneur × Treat			0.010	(0.095)
Informed [Facebook] × Treat			-0.025	(0.091)
Informed [Friend] × Treat			-0.052	(0.109)
Lab. [Self-employed] × Treat			0.031	(0.106)
Lab. [Unemployed] × Treat			0.000	(0.107)
Educ. [Post-Second.] × Treat			-0.075	(0.097)
Educ. [Undergrad.] × Treat			-0.058	(0.090)
Educ. [Graduate] × Treat			-0.090	(0.141)
Δ Inc. [< 0] × Treat			0.158	(0.134)
Δ Inc. [< Inflation] × Treat			-0.029	(0.136)
Δ Inc. [> Inflation] × Treat			0.034	(0.144)
Income × Treat			-0.000	(0.000)
Income [No reply] × Treat			-0.095	(0.100)
Training [1 course] × Treat			0.079	(0.093)
Training [2+ courses] × Treat			0.115	(0.121)
Goal-Oriented [1-5] × Treat			-0.015	(0.041)
Organized [Yes] × Treat			0.030	(0.088)
Problem Solver [Yes] × Treat			-0.013	(0.080)
N	716		716	
Mean Attrition (Control Group)	0.66		0.66	
Indiv. Controls	Yes		Yes	
Controls Interacted	No		Yes	
Prop. Score FE	Yes		Yes	
P-value for F-test (interactions)			0.95	

[a] The dependent variable in all columns is a dummy indicating if the person did not respond the follow-up survey. All regressions include personal information from the baseline survey and propensity score strata fixed effects (coefficients are omitted). Column (2) includes the interaction between these personal characteristics and treatment status. Heteroscedasticity robust standard errors reported between parentheses.

[b] F-statistics (interactions) is used to test the hypothesis of non-differential attrition between treatment and control groups.

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Table A6: Entrepreneurship Course: Individual Characteristics and Attrition

	(1) Attrition		(2) Attrition	
Treat	-0.029	(0.029)	-0.036	(0.249)
Age × Treat			-0.001	(0.003)
Female × Treat			-0.042	(0.058)
Entrepreneur × Treat			-0.073	(0.069)
Informed [Facebook] × Treat			-0.046	(0.068)
Informed [Friend] × Treat			-0.016	(0.073)
Lab. [Self-employed] × Treat			0.046	(0.073)
Lab. [Unemployed] × Treat			-0.110	(0.076)
Educ. [Post-Second.] × Treat			0.132*	(0.079)
Educ. [Undergrad.] × Treat			0.140**	(0.068)
Educ. [Graduate] × Treat			0.271***	(0.103)
Δ Inc. [< 0] × Treat			0.020	(0.107)
Δ Inc. [< Inflation] × Treat			0.021	(0.105)
Δ Inc. [> Inflation] × Treat			0.068	(0.112)
Income × Treat			-0.000	(0.000)
Income [No reply] × Treat			-0.105	(0.070)
Training [Yes] × Treat			0.024	(0.061)
Entrepreneur × Treat			-0.026	(0.057)
Will to Start Firm × Treat			0.029	(0.208)
Capital to Start × Treat			-0.041	(0.056)
Motive [Income] × Treat			0.087	(0.094)
Motive [Own Boss] × Treat			0.061	(0.065)
N	1390		1390	
Mean Attrition (Control Group)	0.66		0.66	
Indiv. Controls	Yes		Yes	
Controls Interacted	No		Yes	
Prop. Score FE	Yes		Yes	
P-value for F-test (interactions)			0.44	

[a] The dependent variable in all columns is a dummy indicating if the person did not respond the follow-up survey. All regressions include personal information from the baseline survey and propensity score strata fixed effects (coefficients are omitted). Column (2) includes the interaction between these personal characteristics and treatment status. Heteroscedasticity robust standard errors reported between parentheses.

[b] F-statistics (interactions) is used to test the hypothesis of non-differential attrition between treatment and control groups.

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Table A7: Impact on Employment Status (Life-Skills Course)

A. Employed at Baseline Survey

	(1) Employee	(2) Self-Employed	(3) Unemployed	(4) Income	(5) Job-Search
ITT	-0.15***	0.11**	0.022	-799.1	0.19***
	(0.042)	(0.045)	(0.032)	(637.5)	(0.060)
TOT	-0.22***	0.17**	0.034	-1200.0	0.28***
	(0.065)	(0.070)	(0.047)	(963.2)	(0.097)
N	162	162	162	162	162
Control Mean	0.59	0.36	0.05	14041.10	0.67
C. Complier Mean	0.72	0.24	0.05	14771.43	0.54

B. Unemployed at Baseline Survey

	(1) Employee	(2) Self-Employed	(3) Unemployed	(4) Income	(5) Job-Search
ITT	-0.14	-0.081	0.22*	-1373.0	0.020
	(0.096)	(0.091)	(0.12)	(982.4)	(0.052)
TOT	-0.27	-0.15	0.41*	-2584.1	0.038
	(0.20)	(0.17)	(0.24)	(1964.4)	(0.097)
N	75	75	75	75	75
Control Mean	0.19	0.33	0.36	3611.11	0.94
C. Complier Mean	0.46	0.39	0.07	7703.18	0.91

[a] *Intention-to-Treat* (ITT) coefficients indicate the average effect of being assigned to the course. The *treatment effect on the treated* (TOT) reports the average effect of the course for those who attended at least 1 class. TOT is estimated via instrumental variables, using the outcome of the random assignment as excluded instrument. All regressions include individual controls (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ y * $p < 0.1$.

[c] Dependent variables in first three columns are obtained from the answers to question: Which of the following best describes your employment status? (a) Unemployed. (b) Homemaker. (c) Employee. (d) Self-employed. (e) Never had a job. Dependent variable in fifth row (Job-Search) indicates who reported to be looking for a job, either in a passive or active way (regardless of their employment status).

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table A8: Impact on Knowledge (Attrition Weights)

A. Life-Skills Course

	(1) Self-Knowledge	(2) Effective Comm.	(3) Conflict Resol.	(4) Leadership	(5) Creativity	(6) All
TOT (Baseline)	0.11	0.19**	0.094	0.10**	0.11**	0.11***
	(0.071)	(0.092)	(0.10)	(0.048)	(0.049)	(0.036)
TOT (Att. Weights)	0.11*	0.22**	0.030	0.12**	0.098**	0.11***
	(0.064)	(0.088)	(0.10)	(0.049)	(0.049)	(0.035)
N	237	237	237	237	237	237
Control Mean	0.80	0.64	0.43	0.65	0.58	0.62
C. Complier Mean	0.84	0.58	0.51	0.62	0.58	0.61

B. Entrepreneurship Course

	(1) Design Thinking	(2) Canvas	(3) Value Proposition	(4) Lean StartUp	(5) All
TOT (Baseline)	0.13***	0.35***	0.19***	0.21***	0.22***
	(0.041)	(0.047)	(0.048)	(0.069)	(0.033)
TOT (Att. Weights)	0.14***	0.34***	0.16***	0.20***	0.21***
	(0.040)	(0.048)	(0.049)	(0.068)	(0.034)
N	499	499	499	499	499
Control Mean	0.24	0.15	0.59	0.52	0.35
C. Complier Mean	0.21	0.10	0.59	0.48	0.33

[a] TOT (Baseline) coefficients report average effects under baseline specification. TOT (Attrition Weights) report estimates from regressions using inverse probability of response as sample weights. All regressions include individual control (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ y * $p < 0.1$.

[c] Dependent variables are the average of correct answers to questions on course-related knowledge. In the life-skills course, topics are: Self-knowledge (1 question), Communication (1), Conflict Resolution (1), Leadership (3), and Creativity (4). For the entrepreneurship course, topics are: Design Thinking (2 questions), Canvas (2), Value proposition (2), and Lean Start-up (1).

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table A9: Impact on Employment Status (Attrition Weights)

A. Life-Skills Course

	(1) Employee	(2) Self-Employed	(3) Unemployed	(4) Income	(5) Job-Search
TOT (Baseline)	-0.22***	0.086	0.096	-1154.6	0.22***
	(0.063)	(0.070)	(0.067)	(1049.0)	(0.076)
TOT (Att. Weights)	-0.19***	0.067	0.11*	-928.5	0.19***
	(0.063)	(0.062)	(0.063)	(1005.0)	(0.075)
N	237	237	237	237	237
Control Mean	0.46	0.35	0.16	10596.33	0.76
C. Complier Mean	0.61	0.30	0.07	12386.78	0.66

B. Entrepreneurship Course

	(1) Employee	(2) Self-Employed	(3) Unemployed	(4) Income	(5) Job-Search
TOT (Baseline)	-0.089*	0.15***	-0.039	673.9	-0.12**
	(0.048)	(0.046)	(0.051)	(816.7)	(0.052)
TOT (Att. Weights)	-0.10**	0.14***	-0.034	697.6	-0.094*
	(0.048)	(0.043)	(0.049)	(832.5)	(0.050)
N	499	499	499	499	499
Control Mean	0.53	0.25	0.17	11780.82	0.85
C. Complier Mean	0.39	0.31	0.23	9419.11	0.83

[a] TOT (Baseline) coefficients report average effects under baseline specification. TOT (Attrition Weights) report estimates from regressions using inverse probability of response as sample weights. All regressions include individual control (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ y * $p < 0.1$.

[c] Dependent variables in first three columns are obtained from the answers to question: Which of the following best describes your employment status? (a) Unemployed. (b) Homemaker. (c) Employee. (d) Self-employed. (e) Never had a job. Dependent variable in fifth row (Job-Search) indicates who reported to be looking for a job, either in a passive or active way (regardless of their employment status).

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table A10: Impact on Entrepreneurial Activity (Attrition Weights)

A. Life-Skills Course

	(1) Own Firm [Yes]	(2) Own Firm [Starting]	(3) Own Firm [Idea]	(4) Own Firm [No]	(5) Sales
TOT (Baseline)	-0.029	-0.045	0.099	-0.026	2.22
	(0.076)	(0.070)	(0.092)	(0.079)	(140.6)
TOT (Att. Weights)	-0.013	-0.011	0.057	-0.033	-79.8
	(0.073)	(0.069)	(0.092)	(0.071)	(147.5)
N	237	237	237	237	237
Control Mean	0.35	0.13	0.32	0.20	189.91
C. Complier Mean	0.30	0.13	0.38	0.19	225.66

B. Entrepreneurship Course

	(1) Own Firm [Yes]	(2) Own Firm [Starting]	(3) Own Firm [Idea]	(4) Own Firm [No]	(5) Sales
TOT (Baseline)	0.12**	0.062	-0.20***	0.016	190.4**
	(0.058)	(0.045)	(0.062)	(0.048)	(78.3)
TOT (Att. Weights)	0.14**	0.056	-0.23***	0.029	253.1***
	(0.058)	(0.042)	(0.061)	(0.047)	(96.4)
N	499	499	499	499	499
Control Mean	0.30	0.11	0.44	0.15	90.75
C. Complier Mean	0.31	0.11	0.48	0.10	10.93

[a] TOT (Baseline) coefficients report average effects under baseline specification. TOT (Attrition Weights) report estimates from regressions using inverse probability of response as sample weights. All regressions include individual control (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ y * $p < 0.1$.

[c] Dependent variables in the first four columns are obtained from answers to question: Do you have a business of your own? (a) Yes. (b) No, but I've already set in motion my business venture. (c) No, but I have an idea to start a business. (d) No. Dependent variable in the fifth row is obtained from answers to question: What is the approximate sales level of your business? [Categories, in USD per month] A value of 0 is assigned to respondents who do not have a business or report that the venture does not have significant sales. In all other cases, the average value of the chosen category is assigned.

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Table A11: Impact on Socio-Emotional Skills (Attrition Weights)

A. Life-Skills Course

	(1) Locus of Control	(2) Empathy	(3) Goal-Oriented	(4) Self-Efficacy	(5) Index (All)
TOT (Baseline)	0.039	-0.066	0.020	-0.12	-0.031
	(0.12)	(0.13)	(0.18)	(0.19)	(0.12)
TOT (Att. Weights)	-0.021	-0.13	-0.047	-0.17	-0.091
	(0.12)	(0.13)	(0.16)	(0.16)	(0.10)
N	237	237	237	237	237
Control Mean	0.00	0.00	0.00	0.00	0.00
C. Complier Mean	-0.05	0.20	0.06	0.08	0.07

B. Entrepreneurship Course

	(1) Locus of Control	(2) Empathy	(3) Goal-Oriented	(4) Self-Efficacy	(5) Index (All)
TOT (Baseline)	0.14*	0.12	0.044	0.066	0.093
	(0.078)	(0.080)	(0.11)	(0.13)	(0.073)
TOT (Att. Weights)	0.10	0.099	-0.0019	0.011	0.053
	(0.077)	(0.076)	(0.10)	(0.12)	(0.068)
N	499	499	499	499	499
Control Mean	0.00	0.00	-0.00	-0.00	0.00
C. Complier Mean	-0.05	-0.00	-0.08	-0.13	-0.06

[a] TOT (Baseline) coefficients report average effects under baseline specification. TOT (Attrition Weights) report estimates from regressions using inverse probability of response as sample weights. All regressions include individual control (see note [d]) and propensity score fixed effects.

[b] Robust standard errors reported between parentheses. Stars denote statistical significance: *** $p < 0.01$, ** $p < 0.05$ y * $p < 0.1$.

[c] Dependent variables are standardized averages (*z-score*) of the responses to different questions designed to measure the level of each socio-emotional skill. Locus of Control: 4 questions. Empathy: 5 questions. Personal Project: 3 questions. Self-efficacy: 3 questions. Index: average of the 4 previous z-scores.

[d] Individual covariates in all regressions include: (i) demographic information (age and gender), (ii) employment status, (iii) educational level, (iv) level and recent evolution of income, (v) socio-emotional skills (ability to define objectives, organize and propose solutions). Information is self-reported and obtained from the baseline survey, completed prior to the drawing and allocation of course seats.

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Paying Politicians: A Semi-Structural Approach

Santiago Pérez Vincent*

Abstract

I examine how the wage paid to politicians affects citizens' decision to participate in politics and their performance in office. I estimate the political agency model by Aruoba, Drazen and Vlaicu (2015) using data on mayors' performance for two samples of Italian municipalities to each side of an arbitrary population threshold determining a jump in mayors' remuneration (Gagliarducci and Nannicini, 2013). I rely on the structure of the model to identify unobservable characteristics of the political environment, such as the quality of politicians and the proportion of office-motivated and intrinsic-motivated candidates. The comparability of the two samples and the exogenous nature of the variation in mayors' wages allow me to credibly attribute the differences in parameters' estimates to the difference in wages. Results show that higher wages lead to a pool of more able but also less intrinsically motivated politicians. Overall, higher wages lead to better average performance. The effect is mainly driven by the increase in candidates' skills, but it is also favored by an enhanced role of elections, which discipline the office-motivated incumbents.

JEL codes: M52, D72, H70
Keywords: political selection, political agency, wages, structural estimation.

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1 Introduction

How can a society improve its politicians' performance? From a political economics perspective, culture and institutions (that is, the shared preferences and beliefs, and the rules shaping people's behavior) are key determinants of a society's capability to both select and discipline its politicians. In this paper, I focus on a crucial aspect of institutions: politicians' remuneration. The paper aims at contributing to the understanding on how the wage paid to politicians affects citizens' decision to participate in politics and their behavior in office.

The main innovation of the paper is to use a semi-structural approach to address the relationship between politicians' remuneration and their performance, building on recent advances in the political economics literature (Aruoba, Drazen and Vlaicu, 2015). Relative to the existing empirical literature on the topic, this approach serves to better quantify and disentangle the different mechanisms at play, providing a stronger link to the theoretical literature and clearer policy implications.

I estimate the political agency model by Aruoba, Drazen and Vlaicu (2015) using data on mayors' performance for two samples of Italian municipalities to each side of an arbitrary population threshold determining a jump in mayors' remuneration (Gagliarducci and Nannicini, 2013). Aruoba, Drazen and Vlaicu (2015) build a stochastic political agency model with imperfect information, which allows to identify and estimate some basic characteristics of the electoral and political environment using information on electoral results and politicians' performance. The structure of their model identifies the parameters behind the quality of politicians, the proportion of office-motivated and intrinsic-motivated candidates, and the competitiveness of the electoral races. The estimated parameters can be then used to simulate counterfactual scenarios, and quantify the selection effect and the discipline effect of elections on politicians' performance. I rely on their proposed structure to estimate the model parameters on two comparable samples of Italian municipalities with different wages, exploiting a change in mayors' wages determined by the Italian law at an arbitrary population threshold.[1] This institutional setting provides arguably exogenous variation in wages (Gagliarducci and Nannicini, 2013) that allows me to credibly attribute the differences in parameters' estimates to the change in wages. Wages are assumed to affect the behavior and performance of politicians only through their effect on the quality and motivations of the candidates' pool and the competitiveness of the election (i.e. parameters of the model). The paper follows a semi-structural approach in the following sense: parameters in each separate sample are estimated using the structural identification provided by the model by Aruoba, Drazen and Vlaicu (2015), and the natural experiment generated by the Italian law (Gagliarducci and Nannicini, 2013) allows to give a causal interpretation to the differences in estimates across the two samples.

Results show that higher wages lead to a pool of more able but also fewer intrinsic-

[1] The Italian Law determines that mayors' wages depend on the number of inhabitants in the municipality, following a non-decreasing step function. The law determines 9 population thresholds for which the mayor wage increases. As Gagliarducci and Nannicini (2013), I focus on the 5000 inhabitants threshold and estimate the model for those municipalities just above and below.

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motivated politicians. Overall, a higher remuneration leads to better average performance. The effect is mainly driven by the change in candidates' skills, but it is also favored by an enhanced role of elections, which discipline the office-motivated incumbents. These results show how the structural estimation of a political economics model can help to better disentangle the different mechanisms through which wages affect political performance. Results are insightful, but rely on some assumptions regarding politicians' and voters' behavior that find only weak support on the data. These limitations highlight one of the main challenges to be addressed in future research: the need to collect more information and to obtain more and better measures of politicians' performance.

The structure of the paper is as follows: Section II presents a brief review of the main theoretical and empirical contributions in this literature. In the following section, I describe the theoretical model proposed by Aruoba, Drazen and Vlaicu (2015). Section IV discusses the institutional setting in Italian municipalities and the data set used. In Section V, I present the results of the estimation. Finally, I conclude, and briefly comment on limitations of the current project and lines for future research.

2 Related Literature

How do wages paid to politicians affect their performance? The political economics literature dealing with this question has usually distinguished two different aspects of it: (i) how the pool of candidates is affected by the politicians' remuneration (the so-called *selection* or, more precisely, *ex-ante selection* channel); and (ii) how wages affect the behavior of politicians once they are in office (*discipline* channel) and the possibility of voters to screen out bad incumbents (*ex-post selection* channel).

From a theoretical perspective, one family of models addresses each of these two aspects: On the one hand, *citizen-candidate* models present alternative mechanisms that explain what drives different types of individuals into politics (Persson and Tabellini, 2000; Caselli and Morelli, 2004; among others). These models examine the choice of individuals who decide whether to participate as a candidate in elections for public office or to stay working in the private sector by comparing the relative value of both options. They do not consider the incentive problems that arise once the politician is in office (by assuming, for example, no possibility of reelection) and just focus on how the equilibrium pool of candidates responds to changes in the different exogenous variables in the model. On the other hand, *political agency* models analyze the joint behavior of incumbent politicians and voters, and how it is affected by different informational structures and institutional settings (Persson and Tabellini, 2000). This type of models assume an exogenous pool of politicians and focus on how the optimal effort of the incumbent and the capability of voters to screen politicians depend on the exogenous variables and on the alternative assumptions.

Besley (2004) and Brollo, Nannicini, Perotti and Tabellini (2013) present models combining both an agency and a citizen-candidate setting. Besley (2004) addresses specifically the question on politicians wages, and provides an overview of the main mechanisms relat-

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ing the remuneration paid to politicians and their behavior. In the model, a representative voter opts between reelecting the incumbent politician or replacing her with a (random) member of an endogenous pool of candidates. Citizens in the economy differ in both their (unobservable) honesty and their private sector wage, and decide whether to run as candidates or keep their private jobs. Incumbents, who are subject to a two term-limit, can either respond to voters demands or behave dishonestly. The voter seeks to reelect honest politicians and get rid of corrupt ones.

How does politicians remuneration affect the equilibrium choices of the different agents? Ceteris paribus, a higher wage increases the value of holding office and might induce a dishonest incumbent to behave responsively in order to be reelected. The positive discipline effect on average politicians' performance can be partially offset by voters' reduced chances to distinguish between different types of politicians: if both good and bad politicians behave well when reelection is possible, voters are unable to separate the wheat from the chaff (ex-post selection). Reelected bad politicians will then misbehave when term limits are binding and reelection is not possible. The equilibrium choices of each type of incumbent affect their reelection probabilities, and they jointly impact on the value of pursuing public office, altering the endogenous pool of candidates (ex-ante selection).

In the baseline model presented by Besley (2004), an increase in politicians remuneration leads to improved performance, driven by both better discipline (that exceeds the cost of reduced ex-post selection) and a more honest pool of politicians. However, these results hinge on some specific assumptions. As discussed by Besley (2004) himself, both the effect on discipline and ex-ante selection could be negative in slightly different and also plausible settings. In particular, the direction of the ex-ante selection effect is highly dependent on the parametric assumptions in the model. An increase in the remuneration of politicians raises the value of office and attracts more people into politics, both honest and dishonest. Hence, the joint distribution of private sector wages and honesty is a key determinant of the direction and the size of the effect of a wage increase in the quality of the pool of politicians, and there are not *a priori* unequivocal assumptions on this distribution. The dimensions of politicians' quality (i.e. which characteristics define a good politician), and how these characteristics correlate with marketable skills are crucial for ex-ante selection.

Caselli and Morelli (2001, 2004) and Messner and Polborn (2004) present two citizen-candidate models and discuss alternative reasons why higher wages might not necessarily lead to better quality candidates. In Caselli and Morelli's (2001) model, citizens' types have two dimensions: honesty and competence. The main tension in the model is that, while voters prefer competent and honest politicians, low quality citizens (i.e. those dishonest and/or incompetent) are the ones who have a comparative advantage in pursuing elective office.[2] While very low (high) remunerations induce only low (high) quality citizens to be elected, in the intermediate cases, voters have to choose between honest but

[2]Competence is assumed to be perfectly correlated to market skills, while honesty is orthogonal to them. The comparative advantage of low quality citizens is due to: (a) incompetent citizens having a lower opportunity cost of becoming a politician; and (b) dishonest ones extracting rents from corruption, and thus obtaining a higher reward from being in office.

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incompetent or competent but dishonest candidates. In these situations, local increases in wages can induce a better balance between these two groups, implying that as one desirable quality increases the other falls.[3]

Messner and Polborn (2004) include an additional mechanism: *free-riding*. In their model, when deciding whether to run or not, citizens not only compare the relative rewards in each occupation, but also weigh the potential improvement in the quality of the public service provided if they are elected instead of another candidate. In their setting, citizens differ only in their competence, which is common knowledge and positively correlated to their (random) outside option. Again, at very high (low) wages only the highest (lowest) competence citizens are elected; but now, in the intermediate cases, an increase in the politician remuneration might induce a fall in the quality of the pool of candidates. This potentially negative effect is the result of two opposing forces: on the one hand, a higher remuneration increases the reward from office and the probability that some citizen will run as candidate; on the other hand, the increased probability of having *someone* running as candidate might induce high quality citizens to free ride.

The existence of different theories with opposite predictions underscores the importance of empirical efforts to address this question, a task that faces several challenges. First and foremost, the key identification issue comes from the potential endogeneity of wages. Di Tella and Fisman (2004), for example, show evidence for the United States consistent with governors being rewarded with higher salaries for better performance. Second, the empirical analysis requires a reliable measure of performance of the elected politicians. Finally, the existence of different mechanisms complicates the interpretation of the results and the policy implications that can be obtained from them.

In recent years, a number of empirical studies have circumvented some of these issues and exploited quasi-experimental settings to address the question on the relationship between remuneration and the selection and discipline of politicians. Ferraz and Finan (2011) exploit discontinuous jumps introduced by the Brazilian law in the (maximum) wages of local legislators across municipalities, and find that higher wages increase their productivity (as measured by the number of bills submitted and approved), and the provision of some public services. They also find a small effect on the education of the candidates and in the competition for office: higher wages lead to more and better educated candidates. Their data set and research design do not allow them to fully disentangle the ex-ante selection and the discipline effect on performance; however, they show that, even after controlling for the observable characteristics of the politicians, the effect of wages on performance remains significant, an indication of enhanced discipline.

Fisman, Harmon, Kamenica and Munk (2015) analyze the effect of salaries on the performance of the members of the European Parliament. They use a panel data approach, exploiting the introduction of a law that equalized the wages of the representatives of

[3]Caselli and Morelli (2001, 2004) focus in the case when the rewards from office are endogenous, and depend on the quality of the pool of politicians (current and past). They show that both *multiple equilibria* and *path dependence* in politicians' quality might arise (i.e. countries with the exact same exogenous characteristics might be stuck in a "bad politicians" or a "good politicians" equilibrium).

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all countries, which were originally different. Their analysis shows no significant effect of wages on the effort exerted by elected politicians, as measured by their participation in parliamentary activities and their shirking behavior.[4] Higher wages augment incumbents' willingness to stay in office, leading to longer average tenures, and increase the number of parties that field candidates for the European Parliament. On the selection front, Fisman *et al* (2014) find that higher wages reduce the average educational quality of incumbents.

Finally, Gagliarducci and Nannicini (2013) use a similar approach as the one used by Ferraz and Finan (2011) to analyze the effect of wages on politicians personal characteristics and on a set of performance measures using information on Italian municipalities. They use a regression discontinuity approach, exploiting a feature of Italian legislation: mayors' wages follow a scale that depends on the number of inhabitants in the municipality. They focus on small cities (around 5000 inhabitants) and find that, on average, higher wages attract better educated politicians. They also show that better paid politicians run smaller government and increase municipal administrative efficiency. In order to disentangle both mechanisms, they focus on the sub-sample of reelected mayors and compute the difference in the performance between their first and second term in office. They find that the performance of reelected incumbents is lower when the term-limit is binding, and consider this fall as the (negative of the) discipline effect. They finally show that this discipline effect is not different between the two sides of the population threshold, and conclude that higher wages do not lead to greater discipline.

The empirical evidence shows no clear pattern on the effect of wages on performance, and, in particular, on the relative importance of the different mechanisms. This lack of unanimity might be in part a consequence of the empirical challenges listed above, which complicate the execution of empirical studies and the interpretation of their results. In this state of things, the vast theoretical literature on the topic is extremely useful. The structural estimation of a model that combines both the endogenous entry of politicians and the agency problem should provide a clearer link between theory and evidence. The simulation of counterfactual scenarios could help to disentangle the different effects at play and to obtain precise policy implications.

This paper aims at giving a first contribution in this direction, and to point out limitations and lines of future research that need to be pursued to obtain further answers to these questions. I estimate the agency model by Aruoba, Drazen and Vlaicu (2015) on two samples of Italian municipalities to each side of an arbitrary population threshold determining a jump in mayors' remuneration (Gagliarducci and Nannicini, 2013). I use the structure of the model to disentangle the discipline and the ex-post selection effects of elections in each of the samples. The exogenous nature of the variation in wages allows me to credibly associate the difference in estimates across samples to the difference in wages. In this exercise, the mechanisms relating wages with the quality of the pool of politicians, their behavior and their accountability are not explicitly modeled. Thus, the exercise only allows to observe if wages have impacted on the pool of politicians and on their accountability, but does not allow to simulate counterfactual scenarios with alterna-

[4]Fisman *et al* (2014) construct a measure of shirking given by the number of days a member of the parliament signed the attendance register but did not participate in any of the votes held that day.

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tive wage schedules.

3 A Political Accountability Model (Aruoba et al, 2015)[5]

Aruoba, Drazen and Vlaicu (2015) build and estimate a political agency model with the aim of quantifying the discipline and the (ex-post) selection effects of elections.[6] In this section, I present their model. I first describe its main elements, the equilibrium concept, and the solution and estimation procedures. Finally, I briefly explain the counterfactual scenario built to measure and disentangle the different effects.

The model describes the joint behavior of an infinitely-lived representative voter and a sequence of incumbent mayors, who are subject to a two-term limit. In each period when the term limit is not binding, the voter decides whether to reelect the incumbent or not. If the incumbent is not reelected or the term limit is binding, a new incumbent is chosen randomly from an exogenous pool of candidates. There are two types of politicians: good and bad, who differ in their disutility of effort. Good politicians do not dislike doing high effort. Bad politicians do. The voter does not observe neither the mayor's type nor her effort, but just her performance. Performance is random, but depends positively on effort. Elections help the voter to discipline and screen the mayors.

The timing of the model is as follows: (i) the incumbent mayor decides her level of effort; (ii) a performance realization is drawn from the probability distribution that corresponds to the chosen effort level; (iii) the voter observes the performance and updates the probability of the incumbent being of good or bad type; (iv) the voter observes a popularity shock that affects the relative attractiveness of the incumbent and her opponents; (v) taking into account the updated probability of the incumbent's type and the popularity shock, the voter decides whether to reelect or not the incumbent; (vi) if the incumbent is not reelected, a new one is chosen randomly from the pool of candidates; if she is reelected, she chooses her optimal level of effort, and, in the following period, a random candidate takes office. Whenever a new incumbent takes office, the cycle starts again.

The voter and the incumbent both know the parameters of the model, and the decision processes of the different agents, which I describe in the following subsections.

Mayor's problem.

Every period, the incumbent mayor decides whether to exert high effort ($e = H$) or low effort ($e = L$). Her effort choice influences her performance (y), which can be understood as the efficiency in managing the municipality, or, more broadly, how effectively she

[5] This section follows sections 3.1-3.5, 4.1-4.2 and 5.2 of the article by Aruoba, Drazen and Vlaicu (2015).

[6] Aruoba, Drazen and Vlaicu (2015) estimate the model using information on U.S governors for 1982-2012. They find that elections significantly increase incumbents' performance, mainly through the discipline channel. Throughout this section, I present the model referring the incumbent as a "mayor" and not as a "governor" (as it was in the original article).

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responds to citizens' demands. In particular, performance follows the rule:

$$y_i|e_i = H \sim N(Y_H, \sigma_y^2) \quad (1)$$
$$y_i|e_i = L \sim N(Y_L, \sigma_y^2) \quad (2)$$

for term $i = 1, 2$, where $Y_H > Y_L$.

Exerting high effort leads, on average, to a better performance, which is independent of the incumbent's type or the term she is in. The mayor knows that the voter takes her performance into consideration when deciding to reelect her, and, thus, anticipates that her reelection probability depends on the effort choice: ρ_H (ρ_L) is the probability of being reelected when effort is high (low).

Politicians can be either good ($\theta = G$) or bad ($\theta = B$). The politicians' type is not observed by the voter, who nevertheless knows that there is an exogenous fraction π of good politicians in the pool of candidates. Good and bad politicians obtain an exogenous rent r from being in office.

Exerting low effort has no utility cost for each of both types ($c(L, B) = c(L, B) = 0$); but good and bad politicians differ in the cost of exerting high effort. While good politicians bear no utility loss from high effort ($c(H, G) = 0$), bad politicians suffer a random cost c, which is expressed as a fraction of the exogenous rents of office (r) and is assumed to be drawn from a uniform distribution on the interval $[0, 1]$. The cost c is observed by the incumbent (before choosing her effort level) but not by the voters.

The incumbent's problem is then:

$$\max_{e_1, e_2} \; [1 - c(e_1; \theta)]r + [\mathbf{1}_H \rho_H + (1 - \mathbf{1}_H)\rho_L][1 - c(e_2; \theta)]r \quad (3)$$

where e_i is effort in term i and $\mathbf{1}_H$ is an indicator function that equals 1 if effort is high, and equals zero otherwise. In solving this decision problem, the incumbent takes the reelection probabilities (ρ_H, ρ_L) as given. The decision problem (implicitly) assumes that incumbents always run for reelection.

A strategy for the incumbent is an effort choice for each term, and each possible level of c. The optimal strategy is the one that solves the maximization problem above (3).

Good politicians always find optimal to exert high effort. In the first term, high effort increases the probability of reelection and bears no additional utility cost.[7] In the second term, they are indifferent between high or low effort, but they lean to the voters' side and exert high effort. The optimal strategy for bad type politicians is less trivial. In the second term, when there is no possibility of reelection, they exert low effort. In the first term, they exert high effort if the cost (c) is smaller than the expected benefit of doing so, given by the increase in the reelection chances. The optimal strategy is then to choose high effort if and only if $c < \rho_H - \rho_L$.

[7] In equilibrium, $\rho_H > \rho_L$.

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It is important to note that, *conditional on the model parameters*, r (the exogenous rents of office) does not affect the optimal strategy of the mayor. Nevertheless, as it follows from the previous review of the theoretical and empirical literature, the expected value of office (that is affected by the value of r) might impact on the quality of the pool of candidates, the proportion of office-motivated vs. intrinsic-motivated ones, and the competitiveness of the electoral races (captured by the model parameters). The impact of wages on behavior and performance therefore is not direct, but mediated by the characteristics of the political environment captured by the parameter values. In this paper, no structure is imposed on the relationship between wages and the model parameters.[8]

In short, in equilibrium, good politicians always exert high effort, and bad politicians only do so in the first term if the cost (c) is sufficiently low. Hence, under the distributional assumption for c, the equilibrium probability of a low type incumbent exerting high effort in the first term is:

$$\delta \equiv P(e_1 = H|\theta = L) = \rho_H - \rho_L \tag{4}$$

Voter's problem.

In every period that the term limit is not binding, the representative voter chooses whether to reelect the incumbent or not. If the incumbent is reelected, she takes office for a second and last term, and then is replaced by a random pick from the pool of candidates. If she is not reelected, a random candidate takes office directly in the following period. Whenever a new incumbent takes office, the game starts again.

Voters have all the same preferences and information, and can be modeled as a single representative voter. The representative voter is infinitely lived and seeks to maximize her discounted expected utility. The voter instantaneous utility is linear in the mayor's performance.

The model includes a popularity shock $\varepsilon \sim N(\mu, \sigma_\varepsilon)$, which is observed by the voter before the reelection decision but not by the incumbent, and that is added to the voters' utility if the incumbent is reelected. In this way, a positive mean of the popularity shock ($\mu > 0$) implies an incumbency advantage. The popularity shock adds stochasticity to the reelection rule and intends to capture every other factor that influences voters' decision apart from the incumbent's performance.

The representative voter's lifetime utility, after observing the incumbent's first term performance and the popularity shock, is:

$$W(y_1, \varepsilon) = y_1 + \beta \max_{R \in \{0,1\}} \mathbb{E}\left\{R\left[y_2 + \varepsilon + \beta W(y_1', \varepsilon')\right] + (1 - R)\, W(y_1', \varepsilon') | y_1, \varepsilon\right\} \tag{5}$$

where β is the voter's discount rate between electoral terms, and R is the decision to reelect ($R = 1$) or not ($R = 0$).

[8]Building and estimating a fully-fledged model that explicitly incorporates the different channels through which wages impact on politicians' performance is part of the future research agenda.

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The voter's problem can be rewritten as follows:

$$W(y_1, \varepsilon) = y_1 + \beta \max_{R \in \{0,1\}} R\left[\mathbb{E}\left[y_2|y_1\right] + \varepsilon + \beta\mathbb{V}\right] + (1-R)\,\mathbb{V} \tag{6}$$

where $\mathbb{V}$ denotes $\mathbb{E}\left[W(y_1', \varepsilon')\right]$, which is constant because all the stochastic variables are not persistent. The voter takes the probability δ as given when solving the decision problem.

The constant $\mathbb{V}$ can be expressed as:

$$\begin{aligned}\mathbb{V} \;=\; & [\pi + (1-\pi)\delta] \iint W(y_1', \varepsilon')\phi\left(\frac{y_1' - Y_H}{\sigma_y}\right)\phi\left(\frac{\varepsilon' - \mu}{\sigma_\varepsilon}\right) dy_1' d\varepsilon' \\ & + (1-\pi)(1-\delta) \iint W(y_1', \varepsilon')\phi\left(\frac{y_1' - Y_L}{\sigma_y}\right)\phi\left(\frac{\varepsilon' - \mu}{\sigma_\varepsilon}\right) dy_1' d\varepsilon'\end{aligned} \tag{7}$$

To solve her decision problem, the representative voter observes the first term performance and updates the probability of the incumbent being of good type. The Bayesian posterior probability ($\hat{\pi}(y_1)$) is:

$$\hat{\pi}(y_1) \equiv \frac{\pi\phi\left(\frac{y_1' - Y_H}{\sigma_y}\right)}{[\pi + (1-\pi)\delta]\,\phi\left(\frac{y_1' - Y_H}{\sigma_y}\right) + (1-\pi)(1-\delta)\phi\left(\frac{y_1' - Y_L}{\sigma_y}\right)} \tag{8}$$

The conditional expectation of the incumbent's second term performance is then:

$$\mathbb{E}\left[y_2|y_1\right] = \hat{\pi}(y_1)Y_H + \left[1 - \hat{\pi}(y_1)\right]Y_L \tag{9}$$

A strategy for the representative voter is a choice to reelect or not to reelect for each possible combination of first term performance and popularity shock (y_1, ε). In the optimal strategy, the incumbent is reelected if and only if:

$$\hat{\pi}(y_1)Y_H + \left[1 - \hat{\pi}(y_1)\right]Y_L + \varepsilon + \beta\mathbb{V} \;>\; \mathbb{V} \tag{10}$$

The voter reelects the incumbent if either y_1 or ε are sufficiently high. In particular, for a given value of the first term performance, the popularity shock that makes the voter indifferent between reelection and picking a new candidate is:

$$\hat{\varepsilon}(y_1) \;\equiv\; (1-\beta)\mathbb{V} - \hat{\pi}(y_1)Y_H - \left[1 - \hat{\pi}(y_1)\right]Y_L \tag{11}$$

Hence, the optimal reelection rule for a given y_1, is:

$$R(y_1, \varepsilon) = \begin{cases} 0 & \text{if}\, \varepsilon \leq \hat{\varepsilon}(y_1) \\ 1 & \text{if}\, \varepsilon > \hat{\varepsilon}(y_1) \end{cases} \tag{12}$$

The reelection probability, conditional on the first term performance, implied by the optimal reelection rule is:

$$\psi(y_1) \equiv P(R = 1|y_1) \;=\; P\left[\varepsilon > \hat{\varepsilon}(y_1)\right] \;=\; 1 - \Phi\left(\frac{\hat{\varepsilon}(y_1) - \mu}{\sigma_\varepsilon}\right) \tag{13}$$

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Hence, in equilibrium, the incumbent's reelection probability conditional on her effort is:

$$\rho_H = \int \psi(y_1)\phi\left(\frac{y_1 - Y_H}{\sigma_y}\right) dy_1 \tag{14}$$

$$\rho_L = \int \psi(y_1)\phi\left(\frac{y_1 - Y_L}{\sigma_y}\right) dy_1 \tag{15}$$

Equilibrium.

As explained above, in equilibrium, good politicians always exert high effort, and bad politicians exert low effort when the term limit is binding. Knowing this, the equilibrium of the model consists of (a) a strategy for bad incumbents for their first term in office; (b) a strategy (reelection rule) for the representative voter; and (c) a system of beliefs, such that: (i) they solve the maximization problem of the incumbent and the voter, respectively; (ii) they are consistent with each other; and (iii) and they are consistent with the voter's Bayesian update of beliefs.

Formally, Aruoba, Drazen and Vlaicu (2015) state the following definition:

Definition. The outcome of a Perfect Bayesian Equilibrium of the game between a mayor and the voter is a collection of scalars $(\rho_L, \rho_H, \delta, \mathbb{V})$ where:

1. Given δ, the voters choices lead to ρ_L, ρ_H and $\mathbb{V}$.
2. Given ρ_L, ρ_H and $\mathbb{V}$, a bad mayors' choice of e_1 leads to δ.

Solution.

As it follows from the formal definition of the equilibrium, for given parameter values, to solve the model it is sufficient to find the values for $\rho_L, \rho_H, \delta, \mathbb{V}$. In fact, for given values of δ and $\mathbb{V}$, it is possible to evaluate the equilibrium mappings: $\hat{\pi}(y_1)$ (equation 8), $\hat{\varepsilon}(y_1)$ (equation 11), $W(y_1, \varepsilon)$ (equation 6), $R(y_1, \varepsilon)$ (equation 12) and $\psi(y_1)$ (equation 13). Then, once these functions are evaluated, the values for ρ_L and ρ_H can be obtained from equations (14) and (15), respectively. The only two additional conditions that need to be satisfied are equations (7) and (4). Hence, finding the equilibrium of the model amounts to solving the following non-linear two-by-two system of equations, where the two unknowns are $\mathbb{V}$ and δ:

$$\begin{aligned}
\mathbb{V} &= [\pi + (1-\pi)\delta] \iint W(y_1', \varepsilon')\phi\left(\frac{y_1' - Y_H}{\sigma_y}\right)\phi\left(\frac{\varepsilon' - \mu}{\sigma_\varepsilon}\right) dy_1' d\varepsilon' \\
&\quad + (1-\pi)(1-\delta) \iint W(y_1', \varepsilon')\phi\left(\frac{y_1' - Y_L}{\sigma_y}\right)\phi\left(\frac{\varepsilon' - \mu}{\sigma_\varepsilon}\right) dy_1' d\varepsilon' \\
\delta &= \rho_H - \rho_L
\end{aligned}$$

where $W(y_1', \varepsilon')$ is given by equation (6), and ρ_H and ρ_L are given by (14) and (15), respectively.

The equilibrium is found by solving numerically the system above. I evaluate the system at a grid of values for $(\mathbb{V}, \delta)$ and pick the combination that minimizes the sum of

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squared differences between the two sides of the equations as the initial condition for a standard solver algorithm.[9]

Estimation.

There are seven parameters to estimate: π, Y_H, Y_L, σ_y, μ, σ_e and β. The discount factor β is fixed at 0.85, which represents roughly a 3 percent annual discounting over a five-year term. The rest of the parameters are estimated by maximum likelihood. The data set consists of a measure of performance for each mayor for each term in office (either one or two), and a reelection variable (R) that takes value 1 if the mayor was reelected and value 0 if not. The density of a mayor who won reelection with performance y_1 and y_2 is:

$$\begin{aligned} p_W(y_1, y_2) \equiv\ & \pi\phi\left(\frac{y_1 - Y_H}{\sigma_y}\right)\psi(y_1)\phi\left(\frac{y_2 - Y_H}{\sigma_y}\right) \\ & + (1-\pi)\delta\phi\left(\frac{y_1 - Y_H}{\sigma_y}\right)\psi(y_1)\phi\left(\frac{y_2 - Y_L}{\sigma_y}\right) \\ & + (1-\pi)(1-\delta)\phi\left(\frac{y_1 - Y_L}{\sigma_y}\right)\psi(y_1)\phi\left(\frac{y_2 - Y_L}{\sigma_y}\right) \end{aligned}$$

The density of a mayor who lost reelection with a performance y_1 is:

$$\begin{aligned} p_L(y_1) \equiv\ & \pi\phi\left(\frac{y_1 - Y_H}{\sigma_y}\right)[1 - \psi(y_1)] \\ & + (1-\pi)\delta\phi\left(\frac{y_1 - Y_H}{\sigma_y}\right)[1 - \psi(y_1)] \\ & + (1-\pi)(1-\delta)\phi\left(\frac{y_1 - Y_L}{\sigma_y}\right)[1 - \psi(y_1)] \end{aligned}$$

The contribution to the likelihood of each observation (mayor) is:

$$L_k \equiv R_k \log\left[p_W(y_{1k}, y_{2k})\right] + (1 - R_k)\log\left[p_L(y_{1k})\right]$$

The log-likelihood is the sum of the above expression over all the observations of the sample. The maximum likelihood estimates are found numerically using a standard optimization algorithm. I run the algorithm from different initial conditions to assess its convergence properties and to reduce the chances of finding a local, and not global, maximum.[10]

[9] I implement this algorithm in Matlab. Codes are available upon request.

[10] I implement this algorithm in Matlab. Codes are available upon request. The optimization routine is time-consuming, since every evaluation of the likelihood function requires to numerically solve the model to find the equilibrium for the given set of parameter values. The standard deviations are obtained by computing (numerically) the negative of the inverse of the Hessian of the log-likelihood function at the optimum, and taking the square root of the elements in its main diagonal.

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Computing the Discipline and Selection Effects of Elections.

To quantify the discipline and the selection effects of elections, Aruoba, Drazen and Vlaicu (2015) use the estimated values of the parameters to evaluate a counterfactual scenario in which mayors can only serve one term. The discipline effect and the selection effect are then computed by comparing the results in the benchmark model and in the one-term limit counterfactual.

The solution to the one-term limit model is straightforward. The voter has no choice in this setting: every period a new incumbent is chosen randomly from the exogenous pool of candidates. Given that there are no reelection chances, bad politicians always exert low effort. Good politicians are indifferent between low and high effort, but again choose to exert high effort. Hence, in equilibrium, the average fraction of incumbents exerting high effort is given by the parameter π, and the average performance in each period is equal to $\pi Y_H + (1-\pi)Y_L$.

In the two-term limit setting, elections help the voter to discipline and screen the mayors. The discipline effect of elections comes from those bad politicians who exert high effort in the first term in order to increase their reelection chances. The discipline effect is calculated in two different ways: First, as the difference in the proportion of candidates that exert high effort in their first term in office:

$$\underbrace{\pi + (1-\pi)\delta}_{\substack{\text{\% of high effort in first term}\\ \text{(two-term limit model)}}} - \underbrace{\pi}_{\substack{\text{\% of high effort}\\ \text{(one-term limit model)}}} = \underbrace{(1-\pi)\delta}_{\text{discipline effect \#1}}$$

Second, as the difference in the incumbents' average first term performance:

$$\underbrace{[\pi + (1-\pi)\delta]\, Y_H + (1-\pi)(1-\delta)Y_L}_{\substack{\text{average performance in first term}\\ \text{(two-term limit model)}}} - \underbrace{\pi Y_H + (1-\pi)Y_L}_{\substack{\text{average performance}\\ \text{(one-term limit model)}}} = \underbrace{(1-\pi)\delta\,[Y_H - Y_L]}_{\text{discipline effect \#2}}$$

The selection effect comes from the fact that more good mayors are reelected than bad mayors. Aruoba *et al* (2015) propose also two measures for the selection effect. First, the difference between the proportion of good politicians among those who are reelected and the exogenous fraction of good candidates:

$$\underbrace{\frac{\pi\rho_H}{[\pi + (1-\pi)\delta]\,\rho_H + (1-\pi)(1-\delta)\rho_L}}_{\substack{\text{\% of high effort in second term}\\ \text{(two-term limit model)}}} - \underbrace{\pi}_{\substack{\text{\% of high effort}\\ \text{(one-term limit model)}}} = \text{selection effect \#1}$$

Second, as the difference between the average second term performance of reelected mayors in the benchmark model and the average performance in the one-term limit sce-

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nario:

$$\underbrace{\frac{\pi\rho_H Y_H + [(1-\pi)\delta\rho_H + (1-\pi)(1-\delta)\rho_L]\, Y_L}{[\pi + (1-\pi)\delta]\,\rho_H + (1-\pi)(1-\delta)\rho_L}}_{\substack{\text{average performance in second term}\\ \text{(two-term limit model)}}} - \underbrace{\pi Y_H + (1-\pi)Y_L}_{\substack{\text{average performance}\\ \text{(one-term limit model)}}} = \text{sel. effect}\,\#2$$

In the results section below, I present the different effects as a percentage change with respect to the one-term limit model, and not as an absolute difference as here.

The validity of the counterfactual evaluations and of the estimated effects relies on a crucial assumption: the parameters in the model must be structural in the sense of being unaltered by the counterfactual policy change. This assumption is common to every counterfactual simulation, but it is not at all trivial in the exercise conducted here. In particular, it is not obvious that a change in the term limit would leave the pool of candidates unaffected. The theoretical models discussed above argue that the pool of candidates depends, among other variables, on the value of public office, which could be affected by a change in the term limit. In the sections below, I follow Aruoba, Drazen and Vlaicu (2015) and proceed under the assumption that the model parameters are structural.

4 Italian Institutional Framework and Data

I estimate the above model using data on Italian municipalities for the period 1993-2000, first used by Gagliarducci and Nannicini (2013). In this section, I explain the Italian municipalities' institutional setting and describe the data set.

Italian Municipalities' Institutional Framework.

In Italy there are around $8,000$ municipalities. The municipal government is composed by a mayor (Sindaco), an executive committee (Giunta) chosen by the mayor, and an elected council (Consiglio Comunale). Until 1993, municipal governments had a proportional parliamentary system: citizens elected the council members, who then appointed a mayor. Since 1993, mayors are elected directly by the citizens and are subject to a two term limit.[11] The institutional framework matches in these dimensions that of the theoretical model.

The remuneration of the mayor is determined by the number of inhabitants in the city (as measured by the last population census available), following a non-decreasing step function with nine jumps. Wages are adjusted every year to account for price inflation. In addition to wages, there are other policies and regulations that change with the population size of the municipality: the number and the remuneration of the members of the executive committee and of the council; the electoral rule; and the possibility of running a health care district or host a municipal hospital, among others. Table A1, obtained from

[11] Before 1993, mayors were not subject to term limits. Those mayors who were in office when the new law was introduced were allowed to run for two additional terms.

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Gagliarducci and Nannicini (2013), shows the different policies in place in the year 2000 and their respective population thresholds.

Only two thresholds determine a jump exclusively on the politicians' remuneration: $5,000$ and $50,000$. Following Gagliarducci and Nannicini (2013) choice, I focus on the $5,000$ inhabitants threshold, for which the sample size is relatively large (which is not the case for the $50,000$ inhabitants threshold). In addition to these data availability issues, the choice of relatively small municipalities seems particularly fit to address the effect of wages on the selection and behavior of politicians, given that the other material and immaterial rewards associated to public office appear *a priori* to be less important than in larger municipalities, and regional or national positions. In the $5,000$ inhabitants thresholds, the mayor's monthly gross wage jumps a significant 28.5%, from €2169 to €2789.

There are two additional clarifications about the institutional framework that are important. First, as shown in Table A1 in appendix, the wages of both the mayors and the executive committee change at the $5,000$, and thus it is not possible to clearly disentangle the effect of both policy changes. Nevertheless, the change in the remuneration of the committee members is small in absolute value and, as Gagliarducci and Nannicini (2013) indicate, it is safe to assume that most of the effect comes from the change in the mayor's wage. Second, the Italian law indicates that under specific conditions the municipal council can allow a 15-percent wage increase for the mayor. Gagliarducci and Nannicini (2013) conducted a telephone interview survey to 36 mayors in municipalities between $4,900$ and $5,100$ in office on May 1st, 2009, and found that only 2 out of 36 respondents had been granted this increase. In the estimation of the model, I assume that the municipalities on each side of the threshold are equal among them. The existence of different wage levels within each of the two samples would then generate problems. I assume, based on the results of the survey by Gagliarducci and Nannicini (2013), that these wage increases are rare enough not to generate major biases.[12]

Data on Local Elections and Mayors' Performance.

The model is estimated in two different samples, one to each side of the $5,000$ inhabitants threshold. The unit of observation is a mayor, who might have been in office for one or two terms. The only variable needed to estimate the model is the measure of performance (y). In the original article, Aruoba *et al* (2015) use the "job approval ratings" as proxy for performance, collected from surveys of voters. They measure performance as the fraction of respondents who consider the mayor as either good or excellent, and average across the different waves of the survey in each term. This variable is not available for Italian mayors.

[12]There is a third important additional aspect of the institutional setting. The Italian law allows elected mayors to keep their jobs. Independent workers can accumulate earnings from both activities without restrictions, while dependent workers have to either ask for an absence leave or accept a 50-percent cut in their mayor's wage. In the same telephone survey mentioned above, Gagliarducci and Nannicini (2013) asked mayors about their current job status. The fraction of mayors with other jobs was 53 percent and 54 percent in each side of the threshold, respectively. The self-reported number of hours worked per week was 38 for full-time mayors, and 28 for part-time mayors. As noted by Gagliarducci and Nannicini (2013), the evidence indicates that being a mayor carries an important opportunity cost.

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The measure of performance I use is the speed of payment, defined as "the ratio between the outlays actually paid and the outlays committed in the municipal budget within the year" (Gagliarducci and Nannicini, 2013). The timing of payments is under control of the municipal government, and, thus, the variable is intended to proxy the efficiency of the mayor's administration. In the model, performance enters directly in the utility function of the voter. An important assumption for the validity of the replication exercise is that the speed of payment is in fact a good proxy for administrative efficiency, which affects citizens' welfare. The speed of payment is computed annually by the Ministry of Interior (Ministero dell'Interno). The measure used is the average over the years of the mayor's term, without considering the transition years (in which elections take place).

I use a bandwidth of 300 inhabitants, and estimate the model for those mayors elected during the period 1993-2000 in municipalities with 4700 to 5000, and 5000 to 5300 inhabitants.[13] I exclude women mayors due to the following reason: In the theoretical model, mayors are assumed to always run for reelection, but, in the data, we observe that many of them decide not to seek a second term in office. This is an important issue since mayors who decide to drop out voluntarily will be considered as losers in the estimation of the model, and might bias the results. Only if all mayors who have dropped out would have lost the elections if they had decided to run, there would be no problem. Assuming this is far-fetched. In the current data set, we cannot identify which mayors had dropped out and who had run for elections, but we know that the rate of dropping out is significantly higher among women. Hence, I exclude women mayors from the sample to ameliorate this misspecification bias.[14]

The "low-wage sample" (< 5000) consists of 103 observations, of which 56 are reelected mayors (54% reelection rate). The "high-wage sample" (> 5000) includes 122 mayors, 62 of them who have been reelected (51% reelection rate). The municipalities in the different samples are similar in terms of their geographical characteristics (with the obvious exception of population size).

The choice of the bandwidth seeks to balance two needs: working with a relatively large sample on both sides of the threshold, and keeping municipalities relatively homogeneous in terms of size across both samples. Nannicini and Gagliarducci (2013) show that the speed of payment is in general related to population size. This is not an issue for their identification strategy, since they use a regression discontinuity design and explicitly control for population size. I use a relatively small bandwidth (the difference in population size between the smallest and the biggest city in the samples is less than 13 percent) and assume that, within the chosen range, population does not significantly affect administrative efficiency. The linear regressions of first and second term performances on population size show no clear effect of population on the speed of payment within each

[13] I repeated the estimation using a bandwidth of 250 inhabitants, and results are qualitatively unchanged.

[14] The correction is small since women are very few in the sample: 14 overall (8 in the low-wage sample, and 6 in the high-wage sample). I performed the estimation for the complete sample (including women) and all the results are qualitatively unchanged.

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of the samples (the coefficient is only significantly different from zero for the second term performance in the high-wage sample, with a p-value around 5%).

In the model, municipalities are identical in all relevant dimensions and mayors differ only in their type. These are obviously strong assumptions. In this sense, the main issue is the difference between municipalities in North and the South, which present many long-dated and highly documented differences. In particular, a regression of speed of payment on a North dummy (taking a 1 for municipalities in the Northern regions of Italy and a 0 otherwise) shows that the difference between the two groups is significant, even though it does not explain much of the observed variation (R-squared smaller than 0.1). Another important doubt is whether there are observable characteristics of the mayors that are related to their performance in office, and could inform voters decision. Table 2 shows the linear regressions of first and second term performance (speed of payment) on some mayor's personal characteristics. The joint explanatory potential of the variables is low. Nevertheless, there are some statistically significant patterns: mayors who are unemployed when running for their first term in office perform worse than those employed, particularly in independent or white collars jobs. The number of years of schooling also appears to be negatively correlated to first-term performance. None of the included personal characteristics seem to explain cross-mayor variation in the second-term performance.

5 Estimation Results

I now present the results of the estimation of the agency model proposed by Aruoba, Drazen and Vlaicu (2015) on the two different samples of Italian municipalities. Table 3 shows the estimated values of the six structural parameters in the model (as noted before, the parameter β is fixed at 0.85).

In the low-wage sample, the proportion of good candidates (π) is very high (94%).[15] Good politicians bear no utility loss from exerting high effort and, thus, are not affected by reelection motives: they behave responsively in both their first and second term. This behavior might respond to politicians being motivated by other material or immaterial rewards. If, for example, public service motivation is a first order consideration for politicians, reelection incentives should not affect their behavior. There are other potential explanations for this result: on the one hand, politicians could be motivated by some after-public-office rewards accrued by good performers, such as social recognition, a political career, or professional opportunities in the private sector.[16] In the high-wage sample, the fraction of good candidates is smaller than for low-wage municipalities. This result

[15]The fact that this proportion is so high (and close to 1) might be problematic since it could reflect a poor fit of the model, and it is an obstacle to the identification of the model parameters. In the extreme case where all mayors are of the same type, they all exert the same effort in both terms and therefore it would impossible to identify whether they are all of the good or of the bad type (that is, whether the observed performance comes from low effort or high effort). In our case, it is somewhat reassuring of our interpretation (i.e. most candidates are of the good type and exert high effort) that in the high-sample (where there is more variability of types) the estimated means of the performance with high and low effort are close to the estimates in the low sample.

[16]The political career motivation, although potentially important, does not seem to be extremely relevant in this context. Using an extended version of the database provided by Gagliarducci and Nannicini

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hints that higher wages crowd out other motivations, and leads to a pool of politicians that is, on average, more affected by reelection considerations.

The increase in wages not only leads to less public-service-motivated candidates, but to more able ones. In the high-wage sample, the mean of the performance distributions for high effort (Y_H) and low effort (Y_L) are higher than those in the low-wage sample. In other words, for a given level of effort, politicians in the high-wage sample are, on average, better at managing the municipality, and deliver a higher speed of payment.

It is important to note that the difference between the two samples is statistically weak. For each of the six structural parameters estimated, the two-standard-deviations confidence intervals overlap, indicating that the differences between the samples pointed out in the previous paragraphs might be due to sample variation, and not to differences in the underlying structural parameters. This consideration applies also for the following discussion on the effects of wages on the ex-post selection and the discipline effects.

In both samples, the parameters of the distribution of the popularity shock $(\mu, \sigma_\varepsilon)$ show that there is a small incumbency advantage ($\mu > 0$) and a relatively large variance, which leads to the popularity shock being very important in the reelection decision. The results indicate that the measure of performance is not capturing important factors driving the voters' decision. The interpretation of this result hinges on how much we rely on the chosen measure as an appropriate proxy of performance. If the speed of payment is a good measure of the mayor's performance, the results show that Italian voters in small municipalities are not too responsive to local administrative efficiency, but are focused on other factors, possibly national or regional politics, or ideological considerations. One of the main challenges to be addressed in future research is to dig deeper into this question by constructing different measures of performance and assessing the voters' responsiveness to such measures.

Table 4 shows the equilibrium values of the endogenous variables for the two sets of parameter values. The equilibrium probability of being reelected is higher when high effort (ρ_H) is exerted than when low effort (ρ_L) is done, but the difference between the two is higher in the low-wage sample. In the low-wage sample, the difference between $Y_H - Y_L$ is large compared to the variance of the distributions (σ_y), and the performance realization is highly informative about the incumbent's effort level.

The first block of Table 5 shows some relevant statistics of the benchmark model for both sets of parameter values: (a) the percentage of incumbents exerting high effort in the first and the second term; (b) the mayor's average performance in their first and second term; and (c) the voter's life-time welfare (i.e. the present value of the incumbent's performance, without considering the utility derived from the popularity shock). The results show that higher wages lead to a lower fraction of incumbent's exerting high effort in each

(2013) on Italian politicians, I find that the great majority of the mayors in small municipalities in the last decades have not pursued a political career at the regional or national level; and, more importantly, that only a small fraction of the regional and national politicians start their political career as mayors or council members in small municipalities.

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of both terms, but to a higher average performance, consequence of the enhanced ability of the politicians. The second block ("One-term limit counterfactual") presents the results for the model with no reelection: the percentage of incumbents exerting high effort in each period, their average performance, and the voter's life-time welfare.

The last block of table 5 ("Measures of interest") shows the discipline and the selection effect (measured in the two ways previously described) and the overall change in the life-time welfare induced by the possibility of reelection. Elections increase welfare by allowing voters to discipline and screen their mayors. The increase in welfare due to elections is larger in the high-wage sample, driven by both greater discipline and selection effects.

On the discipline side, in the low-wage sample, the proportion of first-termers who exert high effort increases 3.07% relative to the one-term limit scenario. The average performance in the first term increases 0.81%. The effect is greater in the high-wage sample: the fraction of mayor's exerting high effort raises 8.10% and the average performance increases by 1.03%.

Voter's welfare is also benefited by the voters' possibility to screen out bad politicians. In the low-wage sample, the proportion of incumbents exerting high effort in their second term in office is 2.53% greater than in the one-term limit counterfactual, and leads to a 0.67% increase in the average performance. The selection effect is also greater in the high-wage sample, where the proportion of good politicians among those who are reelected is 8.94% higher than the fraction of good citizens in the pool of candidates (the average performance of second-termers is 1.14% higher than when no reelection is possible).

Why is the welfare gain from elections greater in the high-wage sample? Higher wages lead to a pool of politicians that is more balanced in terms of types. In the trivial cases of candidates being all of the same type ($\pi = 0$ or $\pi = 1$), elections provide no welfare gain to the voters. It is only when both bad and good candidates coexist that elections are relevant. In the low-wage sample, almost every candidate is of the good type, and, thus, voters have little room to discipline and select incumbents. In the high-wage sample, the proportion of those reelection-motivated (bad) politicians is relatively large and, thus, it becomes more important to have an institutional setting that provides incumbents with incentives to exert high effort, and gives the voters the possibility to screen out those politicians who will likely misbehave if reelected.

The greater effect of elections in the high-wage sample due to a more balanced pool of politicians is in part mitigated by the smaller difference between the performance of those incumbents who exert high effort and those who do not. In this sense, it is immediate to note that if effort did not affect performance ($Y_H = Y_L$), elections would not provide any welfare gain. Higher wages lead to a pool of more able politicians, as measured by the means of the performance distributions, given by Y_H and Y_L. The increase in Y_L is greater than that in Y_H, and (given a similar σ_y in the two samples) this reduces both the voters' possibility to screen out those incumbents who exert low effort and their gain from doing so. This can be seen from the equilibrium reelection probabilities: while in the low-wage

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sample only 4 percent of those who exert low effort are reelected, this fraction increases to 24 percent in the high-wage sample (as shown in Table 4).

The last piece of results relates to the paper by Gagliarducci and Nannicini (2013). In their paper, briefly discussed in Section II, they find that higher wages lead to better performance (in particular, higher speed of payments), mainly through the composition channel (*ex-ante selection* + *ex-post selection*). As explained above, they focus on the sub-sample of reelected mayors and, by comparing their relative performance in the first and second term in office, argue that wages do not lead to greater discipline. The exercise proposed in this field paper tries to improve upon the one performed by Gagliarducci and Nannicini (2013) by providing a closer link to the theoretical literature. The structural estimation is more transparent about the assumptions needed to disentangle the different effects and about the precise definition of each of them. The overall result is the same: higher wages lead to better performance, but here we observe that both the ex-ante selection channel and elections (partly through the discipline channel) explain the increase. The structural estimation of the model makes the interaction between the different channels explicit: it is mostly the change in the pool of candidates that enhances the role of elections on performance.

The above exercise does not allow to fully disentangle and quantify the different effects, since it is not possible to evaluate counterfactual scenarios with different wage levels and term limits that would serve to that purpose. Nevertheless, we can obtain some indicative information: in the one-term limit scenario, where elections play no role, higher wages lead to a voter's welfare increase of 0.6% (3.2), which can only be explained by the change in the pool of candidates. In the two-term scenario the increase is higher (0.9%) due to an enhanced role of elections. Both the discipline and the ex-post selection channels, which lead, respectively, to greater increases in the average first and second term performance, explain this improvement.

6 Conclusions

This paper uses a semi-structural approach to estimate the effect of wages on politicians' performance, and to disentangle and quantify the discipline and selection channels behind such effect. It relies on the model proposed by Aruoba, Drazen and Vlaicu (2015), and exploits the exogenous variation in Italian mayors' wages generated by the Italian institutional framework (as in Gagliarducci and Nannicini, 2013).

The estimation results show that, in the Italian municipalities under analysis, higher wages crowd out other motivations, and lead to a pool of politicians that is, on average, more affected by reelection considerations. The increase in wages not only leads to less intrinsically-motivated candidates, but to more able ones: for a given level of effort, politicians in the high-wage sample deliver, on average, a higher speed of payment. Overall, higher wages lead to better administrative efficiency by attracting more able candidates. Elections are important for this improvement by allowing voters to discipline and screen out some of the reelection-motivated politicians.

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The analysis done in this paper, although insightful, has limitations that should be taken into consideration when interpreting the above results, and that could be used as guide for future research addressing these questions. First and most importantly, it is crucial to obtain better measures of politicians' performance. This would not only be a contribution in itself, but it would be also important to better accommodate the model's assumptions. The current measure (*speed of payments*) has interesting features (mainly, it is under the direct control of the mayor and there is arguably not much voters' disagreement on it), but its relationship with voters' welfare is somewhat weak. In addressing this limitation, there are two alternative routes that might be worth exploring: (a) Income and expenditure statements of every municipality in Italy are available on an annual basis, and can be used to construct indexes on the state of local finances (some of them readily available in the information set provided by the Ministero dell'Interno).[17] (b) Judicial sentences involving public officials are passed by different courts (Procure Generale and Regionali, Consiglio di Stato, Tribunali Amministrativi Regionali), which keep public records, in some cases available online. The systematic analysis of the documents on the judicial decisions might allow to build a new data set on the number and characteristics of the corruption and mismanagement cases involving Italian mayors and council members, a potentially relevant measure of performance.

Second, in the above analysis, the mechanisms relating wages to politicians' quality, behavior and performance are not explicitly modeled. The analysis does not allow to simulate counterfactual scenarios with alternative wage schedules. Building a model that explicitly considers incumbents' remunerations and allows to identify structural parameters behind the relationship between remuneration and performance would be the appropriate way to address this limitation. This paper raises some key issues when pursuing this task. In particular, it is important to further analyze (a) if the observable characteristics of candidates provide information to the voters on their quality as policy makers (i.e. whether types in the model are observable or not), and (b) if these features are measurable and available to the econometrician. Furthermore, the model should contemplate some heterogeneity across municipalities: for example, municipalities' educational or employment level (which could account for differences in the pool of candidates).

[17] Periodic reports produced by Corte dei Conti, the government agency in charge of analyzing regional and municipal financial statements, provide an orientation on how to evaluate the administrative efficiency of the municipalities and build alternative measures.

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Table 1: Geographical Characteristics of Municipalities

	Low-wage Sample	High-wage Sample	Difference	SD Diff.
Population (1991)	4845.2	5156.5	-311.3***	(12.040)
North (1:Yes, 0:No)	0.466	0.492	-0.026	(0.067)
Extension (Km^2)	42.97	42.05	0.917	(5.480)
Altitude ($mts.$)	254.5	236.26	18.24	(29.744)
N	103	122	225	

Table 2: Incumbents' Characteristics and Performance

	Low-wage Sample		High-wage Sample	
	First-term Performance (Y_1)	Second-term Performance (Y_2)	First-term Performance (Y_1)	Second-term Performance (Y_2)
Age	0.112	0.174	0.00590	0.0698
	(0.0699)	(0.108)	(0.0748)	(0.120)
School (Yrs.)	-0.544	-0.328	-0.174	0.656
	(0.258)**	(0.305)	(0.288)	(0.529)
White Collar	0.954	-0.0754	-0.464	-2.366
	(1.504)	(1.831)	(1.502)	(2.533)
Blue Collar	-0.542	3.684	-0.637	2.651
	(2.013)	(2.759)	(1.977)	(3.219)
Unemployed	-4.720	1.682	-4.266	2.924
	(2.292)**	(3.445)	(2.208)*	(3.879)
N	121	61	102	55
R^2	0.084	0.177	0.040	0.041

Constant term omitted. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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Table 3: Parameter Estimates

Structural Parameters	π	Y_L	Y_H	σ_y	μ	σ_ϵ
Low-wage Sample	0.944 (0.032)	57.5 (5.11)	79.4 (0.54)	5.22 (0.40)	2.3 (1.56)	7.89 (1.58)
High-wage Sample	0.788 (0.088)	68.7 (2.78)	81.4 (0.72)	4.54 (0.55)	1.26 (1.63)	9.56 (3.30)

Note: standard deviations in parentheses.

Table 4: Equilibrium Values

Equilibrium Values	δ	ρ_L	ρ_H	$\mathbb{V}$
Low-wage Sample	0.52	0.04	0.56	531.7
High-wage Sample	0.30	0.24	0.54	534.1

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Table 5: Properties of Estimated Model

	Low-wage Sample	**High-wage Sample**
BENCHMARK MODEL		
High Effort (Term 1)	97.3%	85.2%
High Effort (Term 2)	96.8%	85.9%
Average Performance (Term 1)	78.81	79.49
Average Performance (Term 2)	78.70	79.57
PV $\{Y_t\}$	525.2	530.1
ONE-TERM LIMIT COUNTERFACTUAL		
High Effort	94.4%	78.8%
Average Performance	78.19	78.67
PV $\{Y_t\}$	521.3	524.5
MEASURES OF INTEREST		
ΔPV $\{Y_t\}$	0.76	1.07
Discipline #1 (% Change)	3.07	8.10
Discipline #2 (% Change)	0.81	1.03
Selection #1 (% Change)	2.53	8.94
Selection #2 (% Change)	0.67	1.14

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Appendix

Table A1: Legislative Thresholds for Italian Municipalities. (Gagliarducci and Nannicini, 2013)

Population	Wage Mayor	Wage Comm.	Fee Council	Comm. Size	Council Size	Electoral Rule	Neighbor. Council	Hospital/ Health
Below 1,000	1,291	15%	18	4	12	single	no	no/no
$1,000 - 3,000$	1,446	20%	18	4	12	single	no	no/no
$3,000 - 5,000$	2,169	20%	18	4	16	single	no	no/no
$5,000 - 10,000$	2,789	50%	18	4	16	single	no	no/no
$10,000 - 15,000$	3,099	55%	22	6	20	single	no	no/no
$15,000 - 20,000$	3,099	55%	22	6	20	runoff	no	no/no
$20,000 - 30,000$	3,099	55%	22	6	20	runoff	no	yes/no
$30,000 - 50,000$	3,460	55%	36	6	30	runoff	allowed	yes/no
$50,000 - 60,000$	4,132	75%	36	6	30	runoff	allowed	yes/no
$60,000 - 100,000$	4,132	75%	36	6	30	runoff	allowed	yes/yes
$100,000 - 250,000$	5,010	75%	36	10	40	runoff	yes	yes/yes
$250,000 - 500,000$	5,784	75%	36	12	46	runoff	yes	yes/yes
Above 500,000	7,798	75%	36	14-16	50-60	runoff	yes	yes/yes

Notes: *Population* is the number of resident inhabitants as measured by the last available Census. *Wage Mayor* and *Wage Comm.* refer to the monthly gross wage of the mayor and the members of the executive committee, respectively; the latter is expressed as a percentage of the former, which refers to 2000 and is measured in Euros. *Fee Council* is the reimbursement per session paid to council members and is measured in Euros. The wage thresholds at 1,000 and 10,000 were introduced in 2000; all of the others date back to 1960. *Comm. Size* is the maximum allowed number of executives appointed by the mayor. *Council Size* is the number of seats in the City Council. All of the size thresholds were set in 1960. Since 1993, Electoral Rule can be either single round (with 60% premium) or runoff (with 66% premium) plurality voting. *Neighborhood Councils* are bodies that represent different neighborhoods within the city and are provided with independent budgets. *Hospital/Health* captures whether the municipality is allowed to have a hospital or a health-care district, respectively.

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