Three esseys in gender and political economy

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Essays in Gender and Political Economy

Giulia Savio

Faculty of Economics

Università della Svizzera italiana

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Doctoral committee:

Prof. Patricia Funk, Università della Svizzera italiana (Lugano, CH), Supervisor

Prof. Fabrizio Mazzonna, Università della Svizzera italiana (Lugano, CH), Internal member

Prof. Ulf Zölitz, University of Zurich (Zurich, CH), External member

To the dearest friends, far away, I cannot see everytime I want

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Introduction

The thesis is composed by three papers. They all rely on different empirical strategies to investigate social dynamics among agents in the context of Gender Economics or Political Economy. The first chapter "Let the Voters Choose Women" (coauthored by A.Baltrunaite, A.Casarico, and P.Profeta, and published on the Journal of Public Economics in 2019) is related to the literature on gender divide in the political arena, and it speaks in favor of public policy intervention to narrow this gap. I document the effectiveness of two policy measures introduced in Italy to reduce gender gaps in politics, namely gender quotas in party lists, and Double Preference Voting conditioned on gender. Double Preference Voting (DPV) is an Italian novelty: this policy allows voters to express two preference votes - instead of only one - for candidates of different genders. Although DPV does not impose any coercive element nor on voters' or parties' decisions, this measure had a substantial impact on the share of women in municipal councils, which raised by 22 percentage points. Moreover, the policy generated a long-lasting change in electoral preferences. Even when the policy was not in place anymore, voters once targeted with DPV, are more likely to vote in favor of female politicians. Since both the two policies target municipalities with more than 5000 inhabitants, I mainly rely on a Sharp Regression Discontinuity Design as empirical strategy. The second chapter is called "Does Scarcity of Female Instructors Create Demand for Diversity among Students? Evidence from Observational and Experimental Data" (joint with P.Funk and N. Iriberri). In this paper, I investigate students' preferences for professors, considering the gender dimension. More specifically, I try to adress the following questions: Do students prefer to have professors of their own gender? Are these preferences stronger for female students? In which faculties are these preferences more pronounced? The motivation for this research arises from the fact that, particularly in STEM disciplines, female professors are a minority. If there is a same-sex preference of students for professors, scarcity of female teachers could be a deterrent for female students to get enrolled in STEM disciplines, usually dominated by a male audience. By exploiting teaching evaluations data of Università della Svizzera italiana, I find that female students (with respect to male students) evaluate better female professors than male professors. These results hold even with course fixed effects. This pattern is more pronounced in scientific faculties (like Informatics and Economics), than in humanistic ones (like Communication). In line with this evidence, in an experiment conducted on MTurk, I find that female students prefer to have a female professor (instead of a male one), if they are exposed to a male dominated pool of teachers. This preference is not detected, when female students are exposed to mix-gender pool of instructors. This paper has important policy implications. Hiring female professors in STEM would be, al least from the students' point of view, a pareto improvement, since it would leave male pupils indifferent, and female ones -which are a minority- better off. This study combines several methodologies: fixed effects regressions to analyze USI Student teaching evaluation, and a randomized field experiment conducted online. The third chapter is named "Can sport events foster electoral participation? Evidence from Switzerland". This study is related to the literature on the determinant of voter turnout. According to the standard rational choice theory, turnout decisions are taken by balancing individual costs and benefits associated to the act of voting. However, recent studies also show that human motivation, as well as social interactions may matter as well as drivers of turnout. In this paper, I try to shed light on this social channel by exploiting sport events as moments of social aggregation, in the context of swiss direct democracy. My goal is to verify whether turnout is higher in referenda anticipated by a hockey/football match, for cantons involved in that match. Results show that cantons experiencing a hockey match (in a window of one week) before the vote, exhibit higher turnout. My findings are corroborated by municipal level evidence. Within cantons involved in a sport match, municipalities closer to the sport stadium – and hosting a higher number of fans - are those experiencing higher voters' participation. Is this effect an emotional response to the outcome of the game? Alternatively, is the increase in turnout driven by the social interaction of peers both at the stadium and outside, fostered by the match? In order to distinguish between these two alternative mechanisms, I perform two empirical checks. First, I verify if my findings are sensitive to the result of the game. Second, I test for heterogeneous effects according to the size of the municipality. While the outcome of the game (victory, loss, parity) is not key, I find that the effect on turnout for hockey and football matches is stronger in smaller municipalities (those with less than 1000 inhabitants). This last finding is in line with previous empirical studies (Funk 2010), documenting how social pressure is stronger in small municipalities. In light of these findings, the paper is suggestive in showing how social events, like sport ones, can foster civic participation by increasing social interactions among citizens, particularly in small communities. This study relies mainly regressions with fixed effects, exploiting different units in cantonal and municipal analysis.

Chapter 1

Let the Voters choose women

1.1 Introduction

Gender gaps dominate the political arena. According to the Global Gender Gap Index (World Economic Forum, 2018), the world has closed only 22% of the gender gap in politics. In Europe, women represent 30% of politicians in legislative bodies and 29.5% in government cabinets (EIGE, 2018). In Italy, women represent approximately 35.7% of members of Parliament.

How to promote female political empowerment? This paper examines a new policy in Italy, which in municipal council elections introduces double preference voting conditioned on gender, whereby voters can express two preferences, instead of one, if they vote for candidates of different gender. In addition, the policy foresees gender quotas on candidate lists to municipal councils to guarantee a substantial presence of female candidates. The law targets all Italian municipalities with more than 5,000 residents, and we use it to implement a regression discontinuity design around this threshold. We first estimate that the policy introduced by Law 215/2012 leads to a 18 percentage point increase in the share of elected female politicians. We show that the result is robust to a number of specification changes and does not depend on pre-existing differences between municipalities below and above the 5,000 resident cut-off. To investigate the mechanisms behind the working of the policy, we hand-collect new data on candidate lists and preference votes, and find that the latter play an important role in promoting female political empowerment. We also analyze voters' behavior in casting preference votes in higher level elections to study the potential presence of spill-over effects of the policy. We find some evidence of more preference votes cast for women in regional elections. This suggests that even soft policy measures, such as double preference voting, may have effects beyond their direct target.

Female under-representation in politics may result from various obstacles in a multi-step ladder process of political recruitment (Norris and Lovenduski, 1995). First, women may not be willing to or may not be interested in competing for political seats, for instance due to time constraints associated with child care duties (e.g., Schlozman et al., 1994). Alternatively, lack of self-confidence or external encouragement (Fox and Lawless, 2004) or lower returns on the political market for women (Júlio and Tavares, 2017) may motivate their absence from politics. Second, parties, in their role of gatekeepers, may not put women forward as candidates (e.g., Kunovich and Paxton, 2005). Third, voters may be biased against female candidates and not cast votes for them (e.g., Schwindt-Bayer et al., 2010; Black and Erickson, 2004).

The promotion of female participation in politics is justified on the grounds of equity considerations (Stevens, 2007), since women represent 50% of the overall voting population. Moreover, female politicians appear less corrupt and show higher cooperation and team working skills (Epstein et al., 2005; Brollo and Troiano, 2016). Female participation in politics may also create role models for other women, who may decide to pursue a political career (Gilardi, 2015). In addition, a gender-balanced political body may impact public policy and the allocation of resources across different programs, as documented in, e.g., Chattopadhyay and Duflo (2004), Duflo and Topalova (2004), Beaman et al. (2010), Funk and Gathmann (2015).[1]

In this work we study the introduction of double preference voting, coupled with gender quotas, as a new tool to increase female presence in political institutions. The novelty of this policy measure is that it concentrates on voters' preferences, in addition to the more common gender quota requirement on candidate lists. Preference votes allow voters to select one candidate (or more) on a list in proportional representation systems and they were introduced in a number of countries[2] in past decades. Preference votes are argued to create a direct link between voters and candidates and raise accountability, due to a "threat" that politicians in top list positions are surpassed by candidates below them. In addition, parties may use preference votes cast for candidates in open list systems to test the popularity of politicians and then promote them to more powerful positions (Folke et al., 2016). However, preference votes appear to be highly ineffective, as voters continue to cast their preferences for candidates at the top of the list (Farrell, 2001; Gallagher and Mitchell, 2005). There is evidence of general voters' predisposition to vote for male over female candidates or viceversa, which is often context-specific (Sanbonmatsu, 2002; Black and Erickson, 2003; Schwindt-Bayer et al., 2010).[3] Conditioning double preference voting on gender

[1] Yet, this result varies considerably across different contexts. For examples, Clots-Figueras (2011), Gagliarducci and Paserman (2012), Ferreira and Gyourko (2014) and Bagues and Campa (2018) find that gender has no (or limited) effect on policies.

[2] Austria, Belgium, the Czech Republic, Denmark, Estonia, Lithuania, Norway, the Netherlands, Slovakia, and Sweden. Since 2013, in French subnational elections voters can elect two members of the opposite sex on a "binôme"or tandem ballot, whose names are arranged in alphabetical order. This new system of nomination of both female and male candidates ("binôme") guarantees the achievement of parity in departmental councils.

[3] The fact that women do not necessarily vote for other women is in line with evidence from other contexts outside

thus may be a promising way to raise effectiveness of preference votes in promoting female political representation. Up to date, there is no causal evidence on the effectiveness of policies targeting voters' preferences in achieving stronger female political empowerment.[4]

Gender quotas are the most common policy for tackling gender imbalances and are in place in a few countries, either at the national or the subnational level (Krook, 2009). They are often accompanied by additional measures to further support female political representation, such as zipping, i.e. a man and a woman alternate in the list of candidates, placement mandates (Schmidt, 2009; Schwindt-Bayer, 2009) or list-proportional representation systems (Tripp and Kang, 2008). However, their effectiveness is under scrutiny (see Dahlerup and Freidenvall, 2008 for a discussion). There is evidence from Italy showing that gender quotas on candidate lists increase the share of female municipal councilors (De Paola et al., 2010) and voters' turnout (De Paola et al., 2014); they also promote the election of younger politicians (Baltrunaite et al., 2015) and improve the quality of municipal councilors (Baltrunaite et al., 2014). The positive effect on quality is documented also for Sweden by Besley et al. (2017), who show that quotas do not stand at odds with meritocracy, as they raise male politicians' competence precisely where effects on female representation are the largest. However, Bagues and Esteve-Volart (2012) study the case of the Spanish senate and find that women remain "pawns" in the political game. Similarly, Bagues and Campa (2018) and Casas-Arce and Saiz (2015) show that women's access to political institutions can be challenged by the strategic positioning of female candidates on male-dominated party lists.

Our paper contributes to the existing literature and to the policy debate on how to promote the presence of women in politics. Against the background of mixed evidence on the effectiveness of gender quotas, our results show that paying attention to voters, and not only to parties, may have immediate and sizable effects on female political empowerment. In local councils elected in municipalities with double preference voting conditioned on gender, the women to men ratio rises to 40/60, as compared to a ratio below 30/70 in municipalities not subject to the policy.

The effectiveness of double preference voting can be explained by the presence of a limited voters' bias against female candidates. With single preference voting, voters are more likely to cast their (single) preference vote in favor of a male candidate. Thanks to the expanded set of voters' choices due to double preference voting, also female candidates have a chance of getting a preference vote, provided that voters are not fully biased against women. In fact, the higher

politics. For example, in academics, Bagues et al. (2017) find that the presence of women in selection committees does not lead to more female professors being promoted.

[4]In terms of descriptive analysis, Kunovich (2012) shows that in the Polish open-list system, preference votes cast by the electorate shift females higher up in the post-election ranking, compared with the original one proposed by the party, and that these shifts result in a higher number of elected women. Shair-Rosenfield and Hinojosa (2014) show evidence from Chile which is consistent with a negative gender (female) bias among parties, but not among voters.

number of women elected suggests that some female candidates are ranked close enough to the voters' favorite male candidates. In addition, the effectiveness of the new system may also be consistent with the presence of voters who, irrespectively of their gender preferences, derive extra utility from having more, rather than less, choice.

Our findings suggest that a simple change in the rules of the voting game may affect voters' behavior in the direction of more gender balanced political representation. They are also consistent with the idea that the underrepresentation of women in politics is not purely an artifact of intrinsic gender biases of voters, but it is at least in part institution-driven, and thus modifiable.

The paper is organized as follows: Section 1.2 presents the institutional setting and the details of Law 215/2012, Section 1.3 studies the impact of the policy on female politicians and Section 1.4 explores the mechanisms behind the effects of the reform and the potential spill-overs of the policy in higher level elections. Section 1.5 concludes.

1.2 The institutional framework and data

1.2.1 Law 215/2012

The sub-national levels of government in Italy include regions, provinces and municipalities. There are 20 regions,[5] 97 provinces and approximately 8,100 municipalities. Electoral rules are set independently at each level of government and Law 215/2012 applies at municipal level.

Italian municipalities vary in terms of geographic, demographic and economic indicators. The municipal administration manages the registry of births and deaths, the registry of deeds, and decides over the level and allocation of local expenditure to different goals, such as administration, education and social services. Expenditure is financed via own taxes and tariffs and via transfers from the central government. Municipalities are headed by a mayor, who is assisted by a legislative body –the municipal council (*Consiglio Comunale*)–, and an executive body –the executive committee (*Giunta Comunale*). Municipal elections take place every five years and municipal governments cannot affect their schedule.

The electoral rules at municipal level change at the 15,000 resident threshold. In order to keep the electoral institutions constant, and considering that the law we are interested in applies at the cut-off of 5,000 residents, we focus on municipalities with less than 15,000 residents. In these municipalities, a mayor is elected according to a single-ballot system.[6] The mayoral candidate who

[5]They are, in alphabetical order: Abruzzo, Basilicata, Calabria, Campania, Emilia-Romagna, Friuli-Venezia Giulia, Lazio, Liguria, Lombardia, Marche, Molise, Piemonte, Puglia, Sardegna, Sicilia, Toscana, Trentino-Alto Adige, Umbria, Valle d'Aosta, Veneto. 5 of these regions (Sicilia, Sardegna, Trentino-Alto Adige, Valle d'Aosta and Friuli-Venezia Giulia) have special autonomy (*Regioni a Statuto Speciale*).

[6]In municipalities above the 15,000 resident threshold the mayor is elected according to the run-off system.

gets the relative majority is appointed. Under this scheme, each candidate for the mayor position can be backed by one list only, with a substantial victory bonus: the list supporting the winner gets 2/3 of the seats in the municipal council, while the rest of the seats is assigned to the remaining lists according to a proportionality criterion. Candidate lists to municipal councils are formed by the local organization of a given party or by independently organized groups of citizens. The list consists of at most as many candidates as the number of seats in the council and at least as many candidates as 3/4 of the number of seats. The number of seats in municipal councils varies between 6 and 16, depending on the size of the resident population. The electoral system prescribes semi-open lists, whereby voters vote for a party and can also cast a preference vote for an individual candidate from their preferred list, by writing down a candidate name on the ballot. After the election, for each party, candidates are re-ranked according to the number of preference votes they receive. The number of seats each party wins determines the number of candidates who get elected according to this ranking.

Italian Law 215 was passed in 2012 with the aim of increasing women's presence on municipal councils. The measures prescribed by the law apply to municipalities with more than 5,000 residents. The law introduces double preference voting conditioned on gender, which gives the voters the following options: they vote for a list by crossing the related symbol, and may choose, among candidates of that list, one candidate of any gender for whom to express one preference vote, or two candidates of different gender for whom to express one preference vote each. Voters may also express no preference vote for any specific candidate. More specifically, for each party, the ballot displays two empty lines, rather than one, to write down up to two names of candidates of different gender.[7] To ensure the presence of candidates of both sexes, the law also establishes that neither gender can represent more than 2/3 of the total number of candidates on party lists for municipal councils. In practice, parties have to reserve at least 1/3 of the total number of positions for female candidates. In municipalities with resident population between 5,000 and 15,000, non-compliance is punished by removing the names of male candidates exceeding 2/3 of the total. The law was in force for the first time in the municipal elections in 2013.

[7]When a voter expresses a preference for a candidate, the candidate gains one preference vote. When a voter expresses preferences for two candidates, both candidates get one preference vote if they are of different gender; if the two candidates are of the same gender, only the candidate whose name is written in the first line gains one preference vote, and the other gets zero preference votes. When a voter does not express any preference, no preference votes are assigned to any candidate. Note that the vote expressed for a party holds independently of the expression of preference vote.

1.2.2 Data

We collect data on elected politicians, candidate lists and preference votes cast. More precisely, we gather publicly available data on elected politicians in the 4,599 Italian municipalities with less than 15,000 residents, which voted in 2013, 2014 and 2015,[8] and the corresponding previous elections. Since municipal elections take place every five years, the previous elections span the period 2008-2010.[9] For these municipalities we use information on the total number and identity of elected councilors, the number of female elected councilors, and the political orientation of the majority party. In addition, we collect information on the number of registered and effective voters, overall and by gender, as well as the number of invalid votes, for the elections taking place in 2013-2015, and the corresponding previous election.[10]

Table 1.1, Panel A shows that 3,628 municipalities are below the threshold of 5,000 residents and are not subject to the provisions of Law 215/2012; 971 municipalities are above this threshold and must therefore comply with the law. We refer to the former group of municipalities as control, and to the latter as treated. In terms of geographical distribution, both treated and control municipalities are spread all over the country. Table 1.1, Panel B shows the share of elected female councilors in treated and control municipalities, and provides descriptive evidence suggesting that the reform leads to a higher presence of female councilors in municipalities subject to it: in these municipalities, municipal councils are more gender balanced, with women representing between 39% (in 2013) and 42% (in 2015) of the total number of councilors, against corresponding values of 22% and 27% in municipalities which were not subject to the law.

We also gather information on a large number of observable municipal characteristics, which we use to test the validity of the regression discontinuity design in Section 3.1. From the 2011 Italian Census we collect information on gender and age composition and density of the resident population, shares of males and females with upper secondary education or higher, and share of employed males and females. We also use geographical indicators provided by the National Institute for Geophysics and Volcanology (*Istituto Nazionale di Geofisica e Vulcanologia*), such as the municipality geographical location in different macro areas of the country, surface area in square kilometers, gradient calculated as the difference between maximum and minimum altitude over the surface, degree of seismicity on a scale 0-4 and mountain area on a scale 1-3. We also

[8] Regions with special autonomy, with the exception of Sardinia, do not apply Law 215/2012. Therefore, we exclude municipalities in these regions (i.e., Sicily, Valle d'Aosta, Friuli-Venezia Giulia and Trentino-Alto Adige) from our sample.

[9] We note that municipal councils may terminate their mandate earlier due to factors such as the unexpected death of the mayor or the resignation of the majority of the councilors and therefore there are some municipalities that vote in intervals shorter than five years.

[10] The data are provided by the Ministry of Interior.

use the information provided by provincial Chambers of Commerce (*Infocamere*) to compute the number of limited liability firms in a given municipality, as well as indicators based on tax records and compiled by the Ministry of Economy and Finance on average municipal income and share of taxpayers.

In order to better understand how the policy works, we collect data on candidate lists. These data are difficult to obtain, as they are gathered only by local electoral offices and they are not published by the Ministry of Interior or made available on the Internet. To the best of our knowledge, this is the first time these data are systematically collected. We restrict our attention to municipalities which voted in 2013. We contact all electoral offices of these municipalities in order to request candidate lists presented by every party with the original (party-composed) candidate ordering and the number of preference votes each candidate on the lists obtained, for the 2013 election and for the previous one.[11]

Table 1.2 summarizes the sample coverage in terms of number of municipalities and party lists in the 2013, and in the previous election, happening for most of these municipalities in 2008.

[Tables 1.1 and 1.2 here]

To study the existence of broader effects of the policy, we complement the dataset with information on female candidates' performance in higher level regional elections. In particular, we collect preference votes cast in municipalities in our sample for candidates in regional elections. Regional elections are ruled by regional electoral laws, which vary across regions.[12] Some regional electoral laws prescribe double preference voting. We consider all regional elections taking place after the introduction of Law 215/2012, excluding regions which adopt double preference voting. The resulting sample consists of municipalities voting in regional elections in Basilicata (2013), Calabria and Piemonte (2014), Liguria, Marche, Puglia and Veneto (2015).[13] The sample consists of 1,930 municipalities, of which 1,582 are in the control and 348 in the treated group.[14]

[11]If there was no response, we searched for candidate lists published in local newspapers, or directly contacted members of the municipal council or local politicians. On several occasions, the lists could only be obtained by watching parties' electoral campaign video material. We have verified that there are no statistically significant differences in the observable characteristics between municipalities for which we were able to obtain candidate lists for the election with the policy and the previous one, and those for which we were not.

[12]The element in common is that members of the regional assemblies are elected according to a proportional system combined with a majority premium. 4/5 of the members of the assembly are elected from lists formed at the province level, with a proportionality rule, allowing for preference votes. All the municipalities in a given province face the same candidate list by party. The remaining seats are assigned according to the majoritarian system, with regional closed candidate lists.

[13]After the introduction of Law 215/2012, regional elections were also held in Abruzzo and Molise. However, it is not possible to include them in our analysis because the data on preference votes are not reported at municipal level for these two regions.

[14]To assess the absence of pre-existing differences, we also collect data on preference votes cast in municipalities

1.3 The impact of the policy on female politicians

In this section we investigate the effects of double preference voting conditioned on gender and gender quotas on the election of women to municipal councils.

1.3.1 Empirical strategy

We adopt a sharp regression discontinuity design in order to estimate the effect of Law 215/2012 on female presence in local politics. We exploit the fact that the measures included in the law, gender quotas and double preference voting conditioned on gender, only apply to municipalities with more than 5,000 residents. This results in a discontinuous variation in the institutional framework for municipalities of different size along a smoothly increasing forcing variable, namely, municipal population size. Our main regression equation is:

$$\begin{aligned} y_i = \alpha + \gamma_{01}\widetilde{x}_i + \gamma_{02}\widetilde{x}_i{}^2 + \cdots + \gamma_{0p}\widetilde{x}_i{}^p + \psi Treatment_i + \\ \gamma_{11}\widetilde{x}_i * Treatment_i + \gamma_{12}\widetilde{x}_i{}^2 * Treatment_i + \cdots + \\ \gamma_{1p}\widetilde{x}_i{}^p * Treatment_i + \varepsilon_i \end{aligned} \tag{1.1}$$

where y_i is the outcome variable of interest, e.g., the share of elected female councilors in municipality i; $\widetilde{x}_i$ is the resident population size in municipality i, centered at the 5,000 resident threshold; p is the order of the control polynomial function, with $p = 1, 2, 3, 4$; and $Treatment_i$ is an indicator for municipalities with more than 5,000 residents ("treated municipalities"). The coefficients on the polynomial terms γ are also indexed by 0 and 1 because we allow for different polynomial coefficients on the two sides of the cut-off. The main coefficient of interest is ψ, which estimates the local average treatment effect of the reform.

We rely on three sets of results:

1. We graphically investigate the existence of the discontinuity around the 5,000 resident cut-off. For this purpose, we plot local sample means of the dependent variable in small equidistant non-overlapping bins over the support of the resident population size $\widetilde{x}_i$, together with the quadratic polynomial fit for municipalities below and above the threshold, and the 95 per cent confidence interval.

2. We estimate Equation (1) using polynomials of different orders, ranging from 1 to 4, for the entire sample of municipalities (*parametric approach*).

in our sample for female candidates in lists in the previous regional election. For these elections, we have data for Basilicata, Liguria, Piemonte and Veneto, which all voted in 2010, because data on preference votes are not collected at municipal level in the other regions.

3. We implement local linear regressions using the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) (*non-parametric approach*).

While these different specifications serve the purpose of transparently showing the robustness of the results, we will focus on the estimates from local linear regressions when commenting on the magnitudes of the effects.

For the validity of the regression discontinuity, we first verify that there are no discontinuities at the 5,000 resident threshold in the distribution of demographic (male and female shares, children and elderly share, population density), geographical (a dummy indicator North for the geographical location, surface in squared Kilometers, gradient, degree of seismicity on a scale 0-4, mountain area on a scale 1-3), educational (share of males and femalse with upper secondary education or higher) and economic (shares of employed females and males, average income, share of taxpayers, and the number of firms) characteristics for municipalities voting in 2013-2015. The results of the graphical analysis in Figure 1.1 and those of the local linear regressions in Table 1.3 show that municipal characteristics vary continuously with municipal population size.

We then test the potential presence of sorting, i.e. the tendency of municipalities to strategically manipulate their population to fall on the preferred side of the cut-off. We implement a McCrary test (McCrary, 2008) and find no evidence of manipulation of the population size in the sample of Italian municipalities which voted in the period 2013-2015, as shown in Figure 1.2.

[Figures 1.1, 1.2 and Table 1.3 here]

1.3.2 Results

We examine the share of elected female councilors (i.e. the number of elected female councilors over the total number of councilors) around the 5,000 resident threshold.

Figure 1.3 shows a discontinuous jump in the share of elected female councilors in the municipalities above the cut-off, which were subject to the policy.[15]

[Figure 1.3 here]

We next estimate the magnitude of the change in the share of female councilors using the control polynomial (parametric) approach. Specifically, we use observations both close to and far from the cut-off point and estimate equation (1) with polynomials of orders 1 to 4 in the four Columns of Table 1.4, Panel A. Polynomials are allowed to differ on the two sides of the cut-off. The results

[15]The discontinuity in the share of female councilors is robust and evident in analogous figures with polynomial fits of orders 1, 3 and 4.

show that the estimated coefficient on the indicator *Treatment* is positive and remains statistically significant in all Columns.

[Table 1.4 here]

To test the existence of a discontinuity in the share of elected female councilors non-parametrically, we implement local linear regressions using a triangular kernel density estimator. In Table 1.4, Panel B, conventional estimates with conventional standard errors are presented in row 1. The results are consistent with the coefficients presented in Panel A. Moreover, the point estimate increases as we concentrate on observations closer to the 5,000 resident threshold. We also show biased-corrected estimates with conventional standard errors, and biased-corrected estimates with robust standard errors in rows 2 and 3 in Table 1.4, Panel B. The point estimate of the coefficient on the variable *Treatment* is 0.183 in these last specifications and implies that municipalities that voted under the provisions of Law 215/2012 elected municipal councils with 18 percentage points more women. This corresponds to two more women in municipal councils, which is a rather sizable effect. The increase in female elected politicians is confirmed when we conduct the analysis separately in the subsample of municipalities in the North, Centre and South of Italy, which are characterized by a marked divide in female empowerment, as shown in Table 1.A.1.

1.3.3 Robustness checks

In Table 1.5 we present robustness checks of non-parametric estimation. In particular, we investigate sensitivity of the estimated parameters to the choice of the bandwidths, as well as to the use of alternative placebo cut-offs in the municipality size. As before, the dependent variable is the share of female councilors over the total number of councilors in elections in 2013-2015 and we report conventional RD estimates with a conventional variance estimator, bias- corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator. In Panel A we consider cut-offs alternative to the 5,000 resident cut-off which determines whether the policy applies. Namely, we consider cut-offs of 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents. The results show that the only significant change in the share of elected female councilors is at the correct 5,000 resident cut-off, while there are no significant changes at alternative placebo cut-offs. In Panel B we consider other bandwidths alternative to the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017). In particular, we consider bandwidths of 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000 residents. Our estimate of the treatment effect is not sensitive to the use of these alternative bandwidths.

[Table 1.5 here]

As a placebo exercise, we assess whether there are pre-existing differences in the share of female politicians that could confound our estimates of the policy effect. We thus examine the potential discontinuity in the share of female councilors in the previous election. Table 1.6 and Figure 1.4 show that the share of female elected politicians does not exhibit any discontinuity at the cut-off in the previous election.[16]

[Table 1.6 and Figure 1.4 here]

We also deal with the threats to the interpretation of regression discontinuity design results, coming from "confounding policies" (Eggers et al., 2017). The only relevant confounding policy concerning local electoral outcomes is the legislation which imposes a variation in the salary of the mayor at the same cut-off of 5,000 residents.[17] However, we point out that our analysis focuses on municipal councilors, and not mayors, and compensation of municipal councilors is not regulated by the Italian law. Furthermore, the change in the mayor's salary at the 5,000 resident cut-off precedes the introduction of Law 215/2012 and it was present long before 2013-2015. As argued above, there are no discontinuities in the share of female councilors (or of female candidates, as will be shown in Section 4.1.) in the previous election, confirming that the observed effects are not driven by differences in the mayor's salary. Finally, we also show that the result on elected female politicians are robust to adopting a difference-in-discontinuities design. Following the specification adopted by Grembi et al. (2016), we estimate the following linear model:

$$y_{it} = \delta_0 + \delta_1 \widetilde{x}_i + Treatment_i(\gamma_0 + \gamma_1 \widetilde{x}_i) + After_t[\alpha_0 + \alpha_1 \widetilde{x}_i + Treatment_i(\beta_0 + \beta \widetilde{x}_i)] + \epsilon_{it} \quad (1.2)$$

where y_i is the outcome variable of interest, namely the share of elected female councilors in municipality i, $\widetilde{x}_i$ is the resident population size in municipality i, centered on the 5,000 resident threshold, $Treatment_i$ is an indicator for municipalities with more than 5,000 residents ("treated municipalities") and $After_t$ is an indicator equal to 1 for the election with the policy (i.e., in the 2013-2015 period) and 0 for the previous election. We further augment the regression specification with municipality fixed effects to account for time-constant observable and unobservable municipal-level characteristics. The main coefficient of interest is β_0, which estimates the local average

[16] In the Appendix, we also show that this zero result is not sensitive to the use of alternative cut-offs and bandwidths, as shown in Table 1.A.2.

[17] We note that in 2013 the rules of the Internal Stability Pact, regulating local public finances, vary at the 5,000 resident cut-off, while they are the same in 2014 and 2015. We find that the effects on the share of female councilors are present in 2013, 2014 and 2015, considered separately. This evidence points against the presence of confounding effects stemming from differences in the rules of the Internal Stability Pact. We also note that the size of the municipal council and, hence, the length of the candidate list, change at the cut-off in 2013, whereas they are the same in 2014 and 2015. We take this into account by defining our dependent variables in terms of shares, instead of absolute values. In addition, we point out that, also in this case, the results hold for each year analyzed in isolation.

treatment effect of the reform. The difference-in-discontinuities analysis hinges on the additional identifying assumption of the presence of parallel trends in the dependent variable prior to the reform. To test it, we complement our data with the information on the share of elected female councilors in municipal elections since 1997. Figure 1.5 plots the share of female councilors in treated and control municipalities in elections up to 2013 (which corresponds to election 0 on the horizontal axis). It shows that the share of elected female councilors develops in a parallel manner for the two groups of municipalities over the long period before the introduction of the double preference voting policy, validating the use of difference-in-discontinuities design.

Table 1.7 shows the results of the difference-in-discontinuities estimations. The coefficient of interest is positive, large and statistically significant, confirming that the effect of the reform on women's empowerment holds true even when controlling for the discontinuity in the mayor's salary.

[Figure 1.5 and Table 1.7 here]

1.4 Mechanisms

In this section, we investigate the role of parties and the way they select candidates, as well as the role of voters and their preferences in determining the increase in the presence of female municipal councilors. Moreover, we analyze whether the policy induces broader effects related to voting behavior and political selection.

1.4.1 The working of the policy

Our purpose is to shed light on how the expanded set of voters' choices interacts with party selection of candidates in fostering female presence in local politics. To this end, we examine the gender composition of candidate lists, which are formed by parties, and preference votes cast for female candidates by the electorate. More specifically, we consider the share of female candidates on the electoral lists composed by political parties, the party-determined ranking of women on the candidate list, the preference votes cast for female candidates and the preference-vote-determined ranking of female candidates. We restrict our attention to the 2013 election, for which we use hand-collected data on these outcomes.

We run party-level regressions as in (1.1), where the subscript i is replaced by is and all variables are defined for party list s in municipality i.[18] In the interest of space, for this set of outcomes we report results of non-parametric estimations and graphical evidence.[19]

[18]Civic lists can also run for seats. They are also considered under the wording "party lists".

[19]The results of parametric estimations are in line with the evidence presented in the paper and are available upon request.

Since for municipalities with more than 5,000 residents the law requires that at least 1/3 of the candidates on each list are female, we start by investigating the behavior of the share of female candidates on party list s in municipality i around the 5,000 threshold. Non-parametric estimates for the share of female candidates are shown in Table 1.8, Column 1: there is no significant discontinuity at the threshold, indicating that parties do not set the gender composition of the lists differently across the cut-off.[20] This evidence is confirmed by the graphical analysis in Figure 1.6, Panel A. The pattern is also similar when we look at the election prior to the introduction of the policy. Table 1.9, Column 1 and Figure 1.7, Panel A, show that the share of female candidates does not exhibit any discontinuity at the cut-off in the previous election.[21]

[Table 1.8 and Figure 1.6 here]

[Table 1.9 and Figure 1.7 here]

The absence of a significant increase in the share of female candidates may appear in contrast with other contributions showing the effectiveness of gender quotas in promoting female presence in politics (for Italy, see De Paola et al., 2010; and Baltrunaite et al., 2014). One potential explanation of the lack of effectiveness of the quota may be the fact that the latter is non-binding, i.e., it imposes a requirement which is equal to or smaller than the existing share of female candidates. Yet, the data do not support this hypothesis, since the share of women on candidate lists stood below the 33% quota requirement imposed by the law in the election before the reform for most municipalities in our sample (Figure 1.7). Interestingly, our evidence reveals that over a five year period of time, there was an overall increase in the share of women candidates in all municipalities, not only those subject to the reform. This may reflect a general positive trend in female political participation. These contextual differences may help to reconcile the seemingly contrasting evidence on the effectiveness of gender quotas.

Although the share of female candidates does not change at the threshold, the likelihood of being elected may depend on the ranking of candidates, as politicians at the top of the list tend to obtain more preference votes and are therefore more likely to be elected (Farrell, 2001). Some studies (Bagues and Esteve-Volart, 2012; Casas-Arce and Saiz, 2015) show that, when constrained by gender quotas, parties manipulate the ranking of the candidates, placing women at the bottom,

[20] Table 1.A.3 shows that this zero result is not sensitive to the use of alternative cut-offs and bandwidths. We replicate this robustness check also for the other dependent variables in Table 1.8, Columns 2-4, in Tables 1.A.4, 1.A.5 and 1.A.6 respectively, which all confirm the robustness of the results.

[21] Table 1.A.7, Column 1 shows the difference-in-discontinuity estimation, which also confirm the result. We replicate this estimation also for the other dependent variables in Table 1.8, Columns 2-4, in Table 1.A.7, Columns 2-4, respectively. All of the results appear robust.

so that there is little change in the chances of being elected for male candidates, who usually form the existing party élite. On the contrary, Shair-Rosenfield (2012) shows that parties in India often place women on their lists higher than required by the law. Therefore, we investigate whether parties below and above the 5,000 resident threshold rank male and female candidates differently. If this is the case, the discontinuity we observe in the number of elected females at the cut-off may partially result from party decisions regarding the ranking of candidates.[22] We rely on Borda ranking which attributes a decreasing number of points to each candidate on the list, i.e. in a list with five candidates, the first one gets five points, the second one – four points, etc., and the last one – one point. We define a Borda score of female candidates as the sum of Borda points of female candidates over the total number of Borda points of all candidates on a given list. This measure exploits the information on the full ranking of candidates to detect systematic differences in candidates' placement, across lists of different length.

The results of the regression analysis in Table 1.8, Column 2 show that there is no change at the threshold.[23] Overall, parties do not appear to be strategic in deciding the ranking of female candidates under the new constraints imposed by the policy. Interestingly, this is the case also in the previous election, as shown in Table 1.9, Column 2, and Figure 1.7, Panel B. All the above evidence suggests that it is unlikely that the gender composition of candidate lists or differences in ranking can solely explain the large increase in the share of female councilors.

We then turn to analyzing preference votes to examine the role of double preference voting conditioned on gender in promoting female politicians. The regression results in Table 1.8, Column 3 show that the share of preference votes cast for female candidates on lists presented in municipalities subject to the policy increases by 15 percentage points. Figure 1.6 confirms that there is a visible positive discontinuity at the cut-off. These results provide evidence that the policy was effective in attracting more preference votes for women. This is further confirmed by the analysis of the preference votes in the previous election: Table 1.9, Column 3 and Figure 1.7, Panel C show no discontinuities at the 5,000 resident cut-off.

We further investigate how preference votes cast for female candidates affect women's presence on municipal councils. In the Italian semi-open lists system, the original party ranking of candidates is re-ordered according to preference votes cast by the electorate. This post-election ranking determines which candidates are elected and reflects the influence of the voters' decisions on the ultimate electoral outcome. To capture this influence, we calculate the Borda score of female can-

[22] We point out that 42% of the lists in our sample are ranked alphabetically and, therefore, are not very likely to exhibit a strategic placement of candidates by parties.

[23] We also consider an alternative measure of candidate placement based on the presence of at least one female candidate on the top two positions of the list. Once more, we do not find a discontinuity at the cut-off. The results are available upon request.

didates using the post-election ranking of all female candidates (elected and non-elected) and use it as a dependent variable in the analysis. Table 1.8, Column 4 and Figure 1.6, Panel D, show that there is a positive discontinuity in this measure at the cut-off. Similar to other outcomes, this is not an artifact of pre-existing differences in outcomes across the two groups of municipalities: Table 1.9, Column 4 and Figure 1.7, Panel D show no changes at the threshold in the election prior to the introduction of the policy. Overall, as parties do not compile candidate lists - either in terms of gender composition or ranking - differently across the threshold, the results in this section strongly support that preference votes elicited by the reform have an important role in promoting female presence on municipal councils.

1.4.2 Other voting outcomes

To strengthen our analysis of the mechanisms behind the effectiveness of the reform, in this section we investigate further outcomes which may influence the increase in the share of female councilors, such as voters' turnout, use of preference votes and the quality of councilors. Results are presented in Table 1.10 and Figure 1.8.

We first ask whether the reform changes voters' turnout –overall and by gender– in municipal elections, which we measure as the share of actual voters (i.e., those who turn out to vote in a given municipality) over eligible voters. Table 1.10, Columns 1 and 2 and Figure 1.8, Panels A and B show that there is no discontinuous change in overall voters' turnout and voters' turnout by gender.[24]

[Table 1.10 and Figure 1.8 here]

Next, we examine the use of preference votes measured as the ratio between the total number of preference votes cast for candidates of a given list and the number of actual voters for that list.[25] Results shown in Table 1.10, Column 3 and in Figure 1.8 Panel C indicate that preference votes are used more actively in treated municipalities. In particular, the figure shows that in municipalities below the threshold, roughly 7 out of 10 voters choose to express a preference. Under the assumption that voters' turnout in expressing preference votes does not change due to the reform, the full adoption of the double preference voting policy would imply 14 preference votes every 10 voters, whereas no adoption of the policy would imply no change in the number

[24]In addition, we find no evidence that voters are "confused" by this policy: the number of invalid ballots is not significantly different at the cut-off. Results are available upon request.

[25]We rely on this measure because electoral data do not register whether a voter has expressed 0, 1, or 2 preferences. We also point out that the number of actual voters - used as the denominator of this ratio - is continuous across the cut-off.

of preference votes per voter, i.e. 7 preference votes every 10 voters. In municipalities above the threshold, we observe roughly 9 preference votes every 10 voters. This suggests that preference votes are used more, though their potential is not fully exploited. Note that we find that there is no discontinuity at the cut-off in the number of votes cast for male candidates,[26] thus double preference voting does not subtract preference votes from them.

The increase in preference votes cast for women may come from a change in the selection of politicians, which increases the quality of candidates running for office. We cannot test this effect directly, because data on the personal characteristics of candidates are not collected. Hence, we study the quality of the elected councilors, as measured by the average years of education (Galasso and Nannicini, 2011; Baltrunaite et al., 2014).[27] The following possibilities can arise. If the quality of both male and female candidates increases, the higher number of preferences for female candidates at the threshold cannot be explained by changes in quality. If only the quality of female candidates increases, we should expect that better-quality women obtain more preference votes, independently of the double preference voting mechanism, and are hence elected. However, we do not find any significant discontinuity at the cut-off in the quality of elected female councilors, as shown in Table 1.10, Column 4 and Figure 1.8, Panel D.[28] Finally, if only the quality of male candidates increases, we should expect an increase in the number of votes cast for male candidates, which we do not observe, as argued above. Therefore, changes in the selection of politicians do not appear to be consistent with the observed patterns in the data.[29]

To further investigate effects related to the quality of elected councilors, in Table 1.11 we perform a difference-in-discontinuities estimation, using as outcomes the difference between male and female average years of education and the average years of education of men and women, separately. There is no evidence of significant changes, except for a marginally significant increase in educational attainment for men, which is however not confirmed in the local estimation.

To ensure that our results in Table 1.10 – overall and female voters' turnout, use of preference votes and quality of politicians – do not depend on pre-determined conditions, we consider the

[26]The results are available upon request.

[27]Note that not only the researcher but also the voters are not systematically provided with information on the level of education or job held by candidates to the municipal council.

[28]The robustness of the results in Table 1.10 to the use of alternative cut-offs and bandwidths is assessed in Tables 1.A.8 - 1.A.11. The overall evidence indicates that the results are not systematically sensitive to the choice of the bandwidth. Similarly, there are no spurious relationships between the municipal population and our outcome variables, with an exception of Table 1.A.10 revealing a drop, rather than an increase, in the use of preference votes at some cut-offs. Regardless, the effect documented at the actual 5,000 resident cut-off is of the opposite sign, substantially larger and much neater, as shown in Figure 1.8, Panel C.

[29]Rather than changes in the selection of politicians, the increase in preference votes can be linked to a change in the behavior of candidates who, in the presence of the policy, increase their effort in political campaigning. If this were the case, we would expect an increase of turnout and/or turnout by gender, which instead is not confirmed by the data (see Table 1.10, Columns 1 and 2).

four outcomes in the election prior to 2013 and show that no discontinuity arises both in the non-parametric estimation in Table 1.12 and in the graphical analysis in Figure 1.9.

We also look at the political orientation of the majority party elected in municipalities below and above the 5,000 resident threshold. Interestingly, in most municipalities that held elections in the period 2013-2015 (4,195 out of 4,599) civic lists obtained the majority of seats and the shares of municipalities with a civic list, left-wing, center-left and right-wing majority are smooth around the 5,000 resident threshold.[30]

Having established the key role of preference votes in promoting female political empowerment, we are now interested in understanding whether the positive effects of the reform on female political empowerment last beyond the first election in which voters were given the opportunity to express more than one preference vote. To this end, we consider elections where voters voted under the double preference voting system for a second time, when the implications of the policy are expected to be learnt by parties and voters. We collect data on the share of female councilors for municipalities which voted in 2013 and a second time afterwards, which for most municipalities happens in 2018. Figure 1.A.1 shows that the policy remains effective in promoting female political empowerment and its effects do not die out over time.

Finally, although the policy directly targets elections to the municipal council, one may expect gender salience originated by the policy to affect also other electoral outcomes within the local government. To test this possibility, we first focus on mayors, and check whether the policy results in higher chances of women being elected to the top executive position in municipalities subject to the law. We find no conclusive evidence in favor of the latter hypothesis; if anything – there is a slight decline in the number of female mayors in affected municipalities. Thus, in the short-term, the 2012 policy does not help women to gain easier access to executive positions in municipalities.

In summary, there is evidence that voters do make use of the expanded set of choices guaranteed by double preference voting and that the latter plays an important role in ensuring that more women are elected to municipal councils. Although with single preference voting voters are more likely to choose a male candidate as a single preference, double preference voting also gives female candidates a chance of getting preference votes, provided that voters are not fully biased against women (as in Shair-Rosenfield and Hinojosa, 2014). In fact, the higher number of women elected suggests that female candidates are ranked close enough to the voters' favorite men. Moreover, the effectiveness of the double preference voting policy may be explained by the presence of voters who, irrespectively of their gender preferences, derive extra utility from having more, rather than

[30] All results are available upon request.

less, choice.[31] Both interpretations suggest that the underrepresentation of women in politics may be determined, at least in part, by voting rules constraining voters' choices.

1.4.3 Spill-over effects on regional elections

Given the salient role of double preference voting documented in earlier sections, we next analyze whether this affirmative action measure introduced by the 2012 policy affects outcomes beyond its direct scope of application. This influence may result, for instance, from voters' behavioral reactions due to learning, mimicking or habit formation. In general, this analysis may contribute to the debate on the use of hard versus soft measures to achieve gender balance in political decision making (Dahlerup, 2006).

To study the potential presence of spill-over effects of the policy, we investigate voters' behavior in casting preference votes in higher level elections. We consider all regional elections taking place after the introduction of Law 215/2012, excluding regions which adopt double preference voting in the regional electoral law, as explained in Section 1.2. We define our dependent variable as the average number of preference votes cast in municipality i for female candidates on a given list in regional elections. This allows us to study voters' behavior in casting preference votes, in isolation from any effect of candidate supply.[32]

Figure 1.10, Panel A shows a positive discontinuity at the threshold in the average number of preference votes cast for female candidates in municipalities voting in regional elections in the period subsequent to the local election with the double preference voting policy. Such discontinuity is not present in the previous regional election in 2010 (Figure 1.10, Panel B). To quantify the effect, we perform difference-in-discontinuities analysis and report the results in Table 1.13. The coefficient is positive, large and statistically significant in Column 1, in which the analysis uses the entire sample of the data. It amounts to roughly three preference votes more, on average, cast for female candidates. The effect is sizable with respect to the sample mean of roughly 4 preference votes for an average candidate on a party list cast in a given municipality. We note, however, that there is no immediate link between a large effect in a given treated municipality and the electoral outcome at the regional level, as the latter is determined by candidates' success in all municipalities within the region. In Table 1.13, Column 2, we restrict the analysis to a narrower window around the cut-off. The effect is positive, yet smaller, and loses its statistical significance in this specification. We note, however, that this may at least partially be driven by the fact that a

[31]This explanation builds on the theory of expressive voting in explaining voting behavior, according to which voters enjoy benefits from the act of voting itself. These benefits may stem from the possibility to express one's opinion, confirm one's identity and follow moral norms (Hamlin and Jennings, 2018).

[32]In particular, this measure is unaffected by the presence or absence of gender quotas on regional candidate lists.

very stringent regression specification is used in a substantially reduced sample. Overall, since the policy is very recent, the documented effects may be interpreted as a lower bound in the presence of habit formation regarding women presence in politics.

[Figure 1.10 and Table 1.13 here]

1.5 Conclusions

This paper shows that the policy which introduces double preference voting conditioned on gender and guarantees a minimum presence of both genders on candidate lists has a large and robust impact on women's political representation in Italian municipalities. Specifically, our causally identified estimates suggest an increase of 18 percentage points in the share of female councilors. We provide evidence that the effect, to a large extent, comes from preference votes in favor of female candidates expressed by electorate in municipalities subject to the policy. In other words, if voters are given the option of casting a preference vote for one candidate of each gender, they do select female candidates more often.

The design of policies to promote women in politics has so far mostly focused on selection made by parties, prescribing, mainly, gender quotas on candidate lists. However, gender quotas are not always effective, and when they are, the increase in female representation is often of limited size. Our results show that a policy which targets voters, such as double preference voting, leads to stronger effects on female representation and brings the municipal council composition closer to gender equality. In addition, they suggest that even soft policy measures, imposing no obligation on parties or voters, but rather acting through the expansion of the set of choices available to the latter, may spill-over beyond their direct target. This result is particularly encouraging for the evaluation of the effectiveness of affirmative action measures, such as or similar to the one analyzed in this paper.

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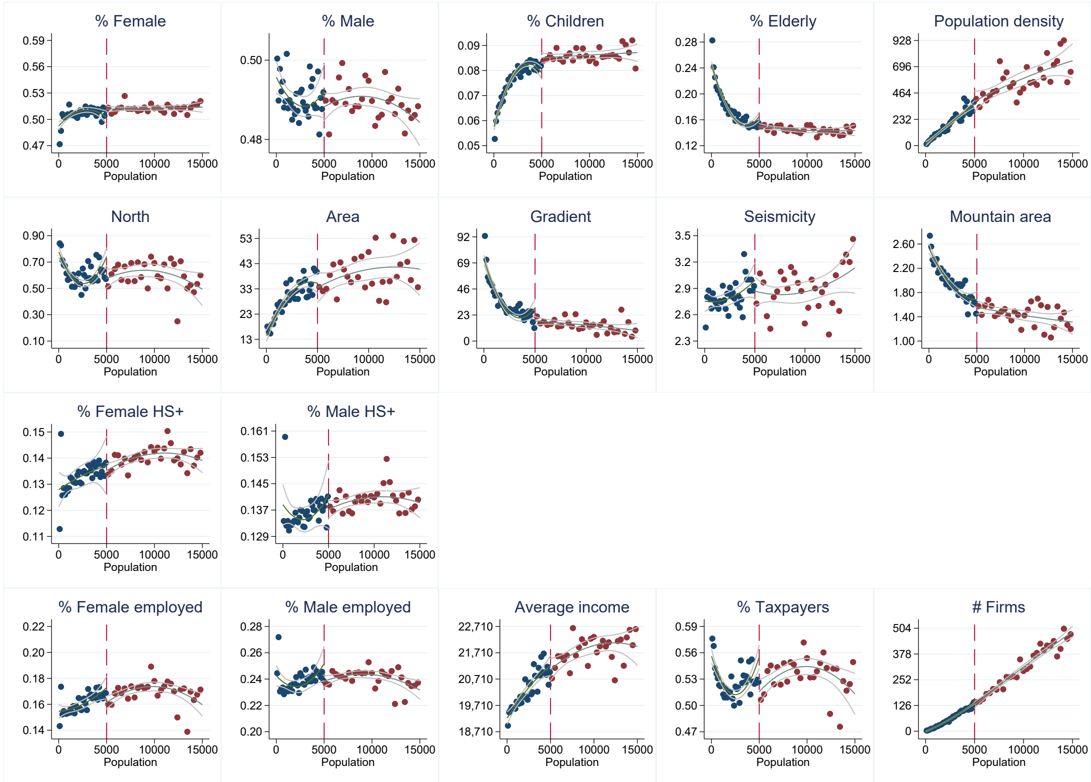

Figure 1.1: Balance check of covariates

Notes. The figure plots the binned averages of demographic, geographic, education and economic characteristics at municipal level against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. Row 1 reports female, male, children (0-9) and elderly (+70) shares of the population and the population density. Row 2 reports a dummy North for geographical location, area in squared Km, gradient, degree of seismicity on a scale 0-4, mountain area on a scale 1-3. Row 3 reports the share of female and male population with upper secondary education or higher. Row 4 reports the share of employed females and males, average income (total municipality income over the number of taxpayers), share of taxpayers, and the number of limited liability companies. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015.

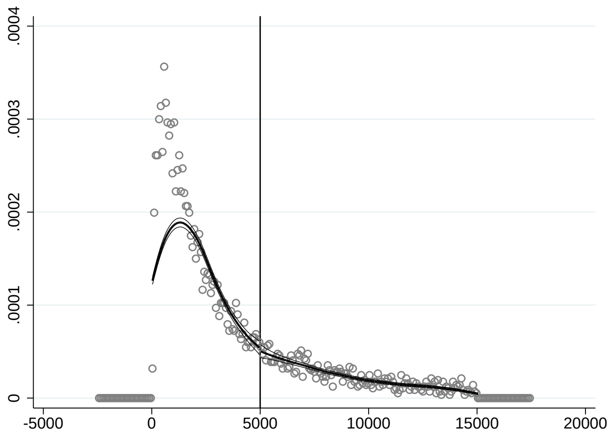

Figure 1.2: McCrary test

Notes. The figure plots the density of the municipal population. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015.

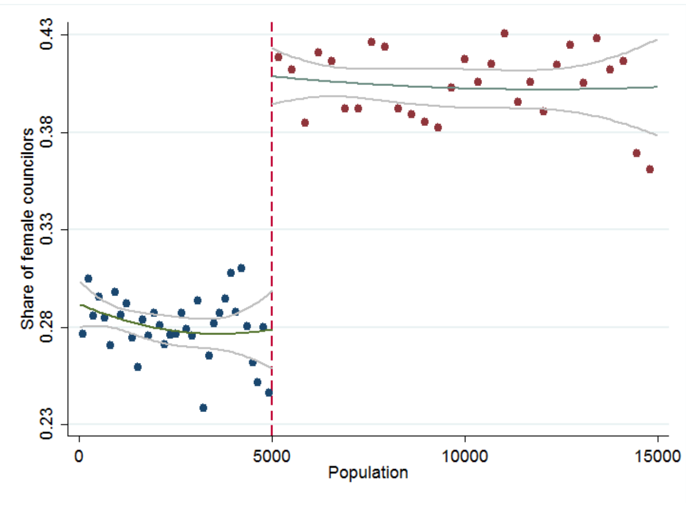

Figure 1.3: Female presence on municipal councils

Notes. The figure plots the binned averages of the share of female councilors against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015.

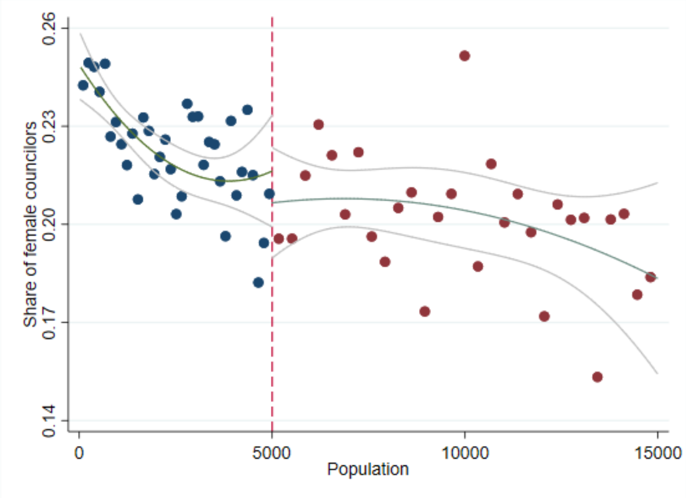

Figure 1.4: Female presence on municipal councils before the reform

Notes. The figure plots the binned averages of the share of female councilors in the election before the reform (2008-2010) against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015.

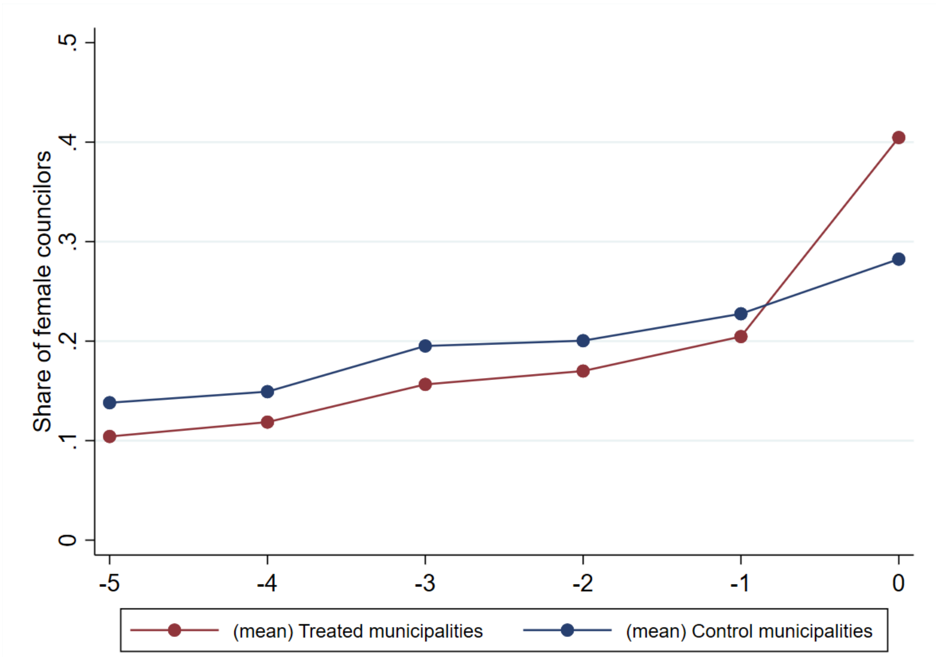

Figure 1.5: Female presence on municipal councils: parallel trends

Notes. The figure plots the share of female councilors in municipalities with more than 5,000 residents (treated) and municipalities with less than 5,000 residents (control). On the horizontal axis, which goes from -5 to 0, we report the outcomes of the elections prior to the 2013 election (coded as 0). The sample includes all the municipalities that held elections in 2013-2015. Only elections from 1997 onward are considered.

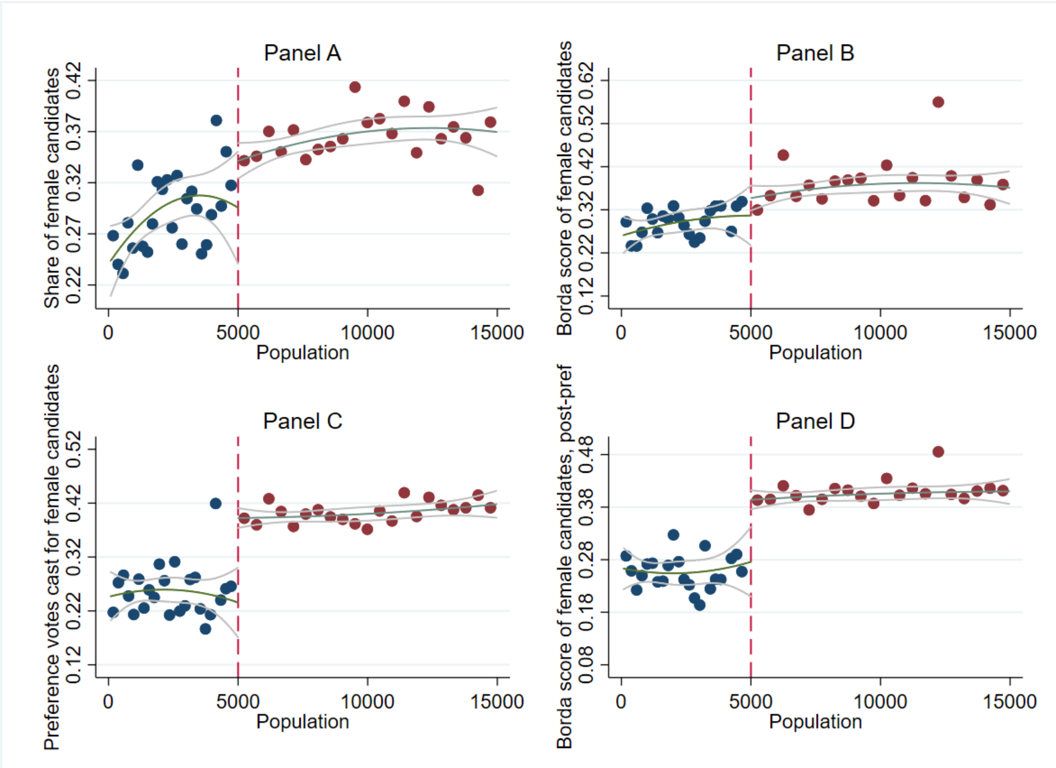

Figure 1.6: Working of the policy

Notes. The figure plots the binned averages of four outcomes against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. Panel A reports the share of female candidates over the total number of candidates on list s in municipality i; Panel B reports the Borda score of female candidates on list s in municipality i; Panel C reports the share of preference votes cast for female candidates on list s in municipality i; Panel D reports the post-election Borda score of female candidates on list s in municipality i. See the main text for details on the definition of the variables. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013.

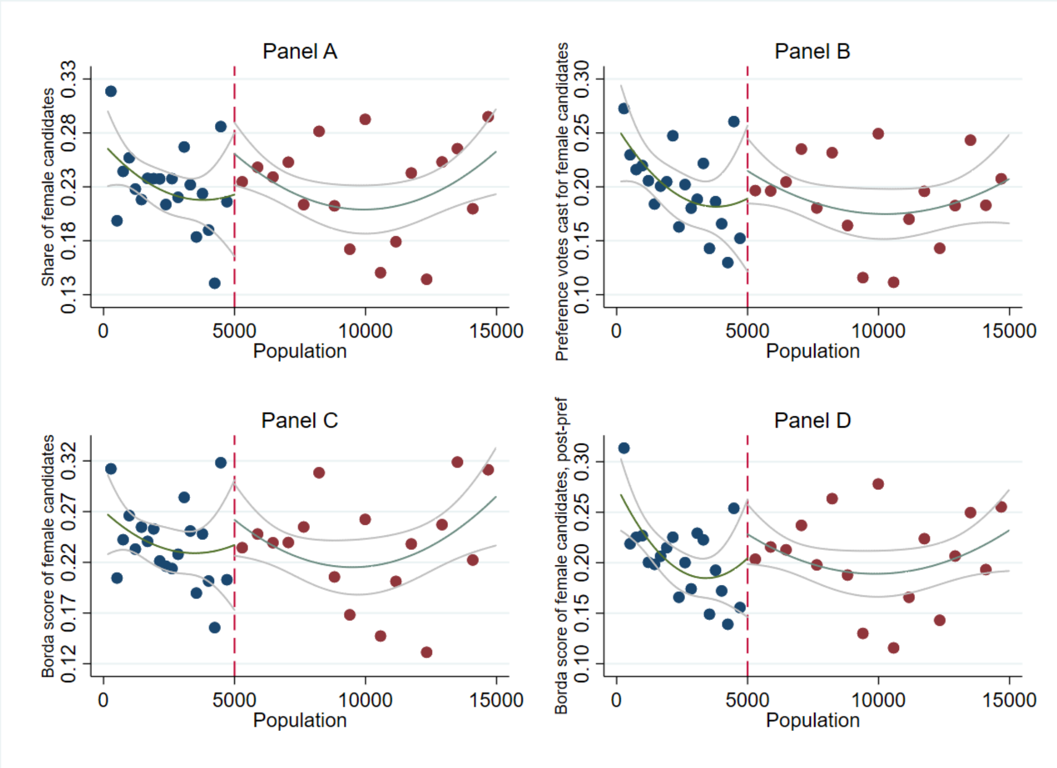

Figure 1.7: Working of the policy before the reform

Notes. The figure plots the binned averages of four outcomes against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. Outcomes are measured in the election before the reform, which in most cases is 2008. Panel A reports the share of female candidates over the total number of candidates on list s in municipality i; Panel B reports the Borda score of female candidates on list s in municipality i; Panel C reports the share of preference votes cast for female candidates on list s in municipality i; Panel D reports the post-election Borda score of female candidates on list s in municipality i. See the main text for details on the definition of the variables. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013.

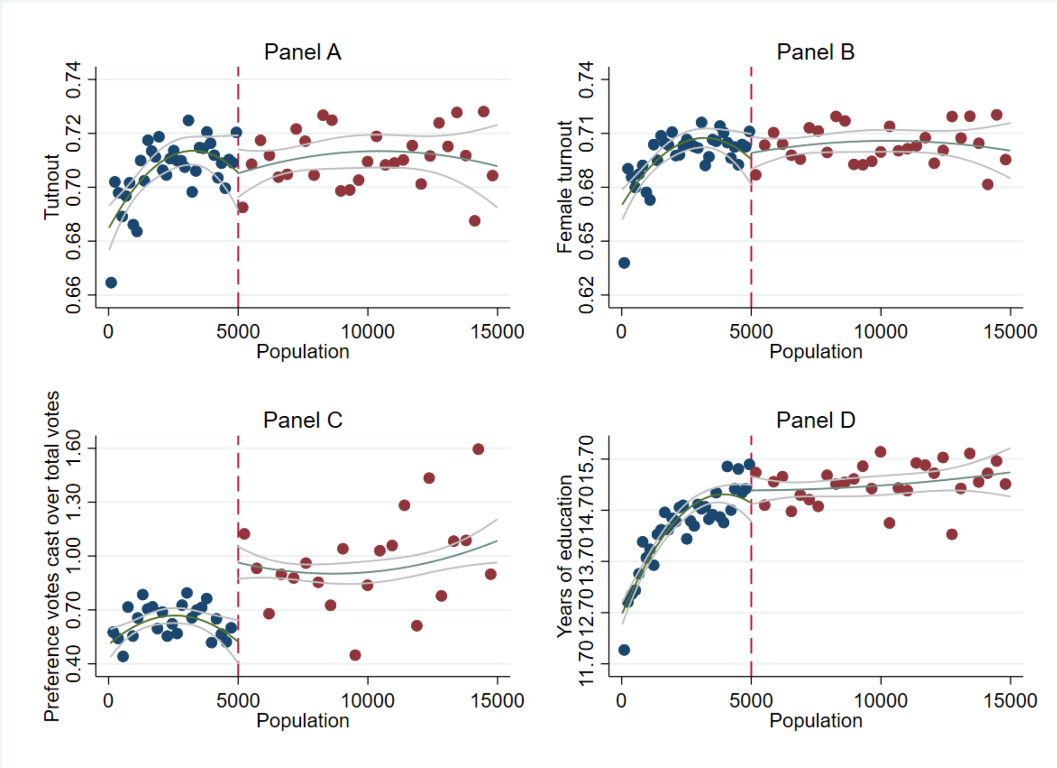

Figure 1.8: Other voting outcomes

Notes. The figure plots the binned averages of four outcomes against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. Panel A reports turnout, measured as the share of actual voters over eligible voters in municipality i; Panel B reports female turnout, measured as the share of actual female voters over eligible female voters in municipality i; Panel C reports the number of preference votes over the total number of actual voters for list s in municipality i; Panel D reports the average number of years of education of elected female councilors in municipality i. The sample includes all municipalities that held election in 2013-2015 in Panel A, B and D, and includes all municipalities that held election in 2013 for which preference votes were available in 2013 in Panel C.

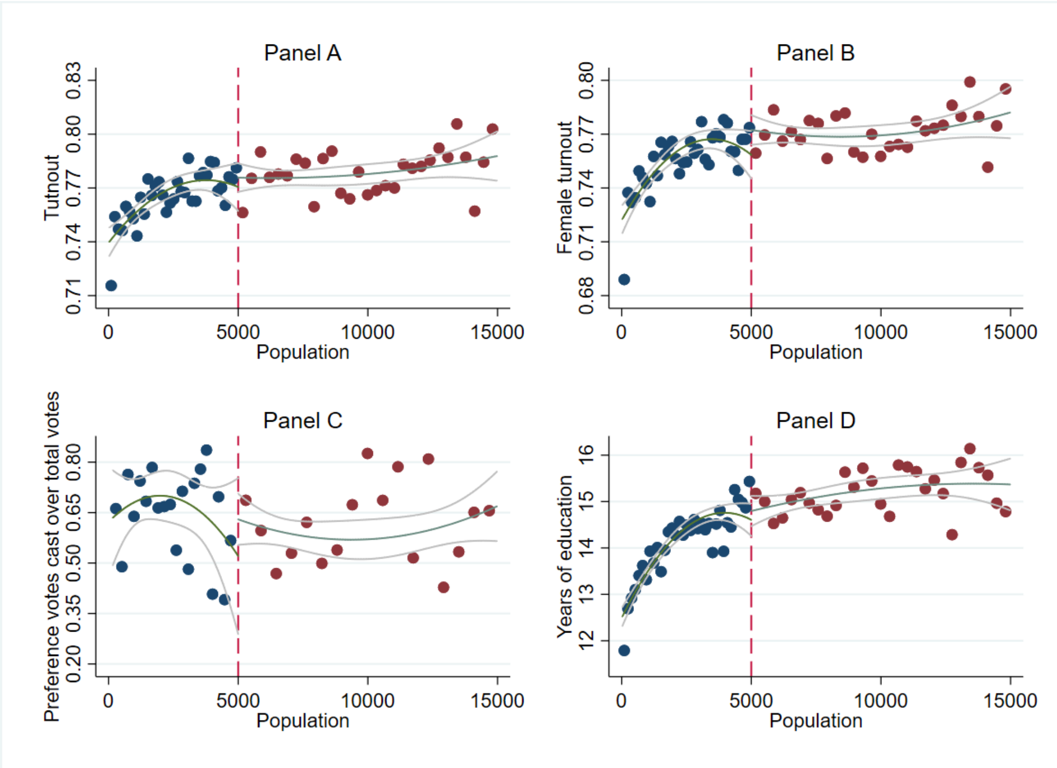

Figure 1.9: Other voting outcomes before the reform

Notes. The figure plots the binned averages of four outcomes against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. All outcomes are measured in the election before the reform, which is in 2008-2010. Panel A reports turnout, measured as the share of actual voters over eligible voters in municipality i; Panel B reports female turnout, measured as the share of actual female voters over eligible female voters in municipality i; Panel C reports the number of preference votes over the total number of actual voters for list s in municipality i; Panel D reports the average number of years of education of elected female councilors in municipality i. The sample includes all municipalities that held election in 2013-2015 in Panel A, B and D, and includes all municipalities that held election in 2013 for which preference votes were available in 2008 in Panel C.

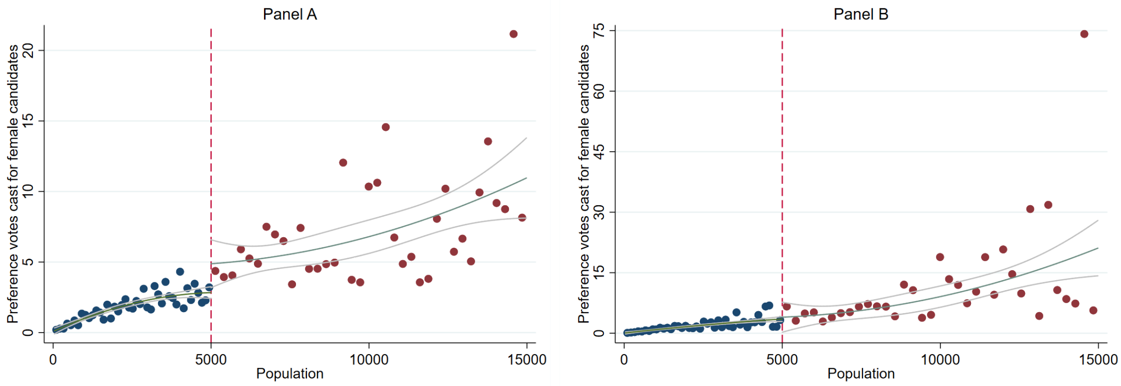

Figure 1.10: Preferences cast for female candidates in regional elections: before and after the reform

Notes. Panel A plots the binned averages of average preference votes cast in municipality i for female candidates on list s in regional elections in 2013-2015 against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence. The sample includes lists presented in municipalities with less than 15,000 residents that held elections in the period 2013-2015 and that are in regions which do not apply double preference voting at regional level. Panel B shows the analogous plot for elections before the introduction of the reform (the relevant regional election is 2010). The sample includes lists presented municipalities with less than 15,000 residents that held elections in the period 2013-2015 and that are in regions which do not apply double preference voting at regional level.

Table 1.1: Descriptive statistics: municipalities and elected councilors

Panel A: Geographical coverage			
No. of municipalities voting in 2013:	Control	Treated	Total
North	132	65	197
South and islands	153	63	216
Center	34	21	55
Total	319	149	468
No. of municipalities voting in 2014:	Control	Treated	Total
North	2023	493	2,516
South and islands	473	99	572
Center	392	117	509
Total	2,888	709	3,597
No. of municipalities voting in 2015:	Control	Treated	Total
North	94	32	126
South and islands	295	74	369
Center	32	7	39
Total	421	113	534
Panel B: Share of female councilors			
Municipalities voting in 2013:	Control	Treated	Total
	0.22	0.39	0.28
	(0.19)	(0.11)	(0.19)
Municipalities voting in 2014:	Control	Treated	Total
	0.29	0.40	0.31
	(0.14)	(0.10)	(0.14)
Municipalities voting in 2015:	Control	Treated	Total
	0.27	0.42	0.30
	(0.14)	(0.09)	(0.14)

Notes. The table reports descriptive statistics on voting municipalities and share of female councilors. Panel A reports the number of municipalities with less than 15,000 residents that held elections in 2013, 2014 and 2015, distinguishing between treated municipalities (those with at least 5,000 residents) and control municipalities (those with less than 5,000 residents), overall and separately for each geographical area. Panel B reports the means of the share of elected female councilors (with standard errors in parentheses) in municipalities with less than 15,000 residents that held elections in 2013, 2014 and 2015, distinguishing between treated and control group.

Table 1.2: Descriptive statistics: candidate lists

Panel A: 2013 election			
No. of municipalities:	Control	Treated	Total
voted	319	149	468
with all lists available	276	134	378
with preference votes available	255	126	381
with pre-election ranking available	213	116	329
No. of party lists:	659	475	1,134
with pre-election ranking available	560	444	1,004
with non-alphabetical ranking	302	277	579
Panel B: Previous election			
No. of municipalities:	Control	Treated	Total
voted	319	149	468
with all lists available	178	93	271
with preference votes available	178	93	271
with pre-election ranking available	178	93	271
No. of party lists	437	300	737
with pre-election ranking available	437	300	737
with non-alphabetical ranking	311	230	541

Notes. The table reports sample numerosity for the 2013 municipal election (Panel A) and for the previous one (Panel B), distinguishing between treated and control municipalities. Elections take place every 5 years, thus, in most cases, previous election took place in 2008. See the main text for details. Panel A and B report, for 2013 and the previous election, respectively, the number of municipalities that voted (for which we have data on all elected councilors), the number of municipalities with all party lists available, those with post-election preference votes available, and those with pre-election party-composed ranking of candidates available. It also reports the total number of party lists, the number of party lists with pre-election party-composed ranking of candidates available and, among them, those with non-alphabetical ranking of candidates.

Table 1.3: Balance checks of covariates

	Panel A: Demographic characteristics				
	% Female	% Male	% Children	% Elderly	Population density
Treatment	0.005	-0.004	0.002	-0.004	41.610
	(0.004)	(0.003)	(0.002)	(0.006)	(60.518)
Bias-corrected	0.006	-0.003	0.003	-0.006	53.125
	(0.004)	(0.003)	(0.002)	(0.006)	(60.518)
Treatment (bias-corrected, robust SE)	0.006	-0.003	0.003	-0.006	53.125
	(0.005)	(0.004)	(0.003)	(0.007)	(72.398)
Cut-off	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,249	2,142	1,744	1,256	1,565
Observations on the left	400	782	606	401	520
Observations on the right	242	388	323	243	294
	Panel B: Geographical characteristics				
	North	Area	Gradient	Seismicity	Mountain area
Treatment	-0.077	-6.851	4.450	-0.017	-0.032
	(0.072)	(6.607)	(4.133)	(0.170)	(0.112)
Bias-corrected	-0.090	-6.995	5.554	0.020	-0.033
	(0.072)	(6.607)	(4.133)	(0.170)	(0.112)
Treatment (bias-corrected, robust SE)	-0.090	-6.995	5.554	0.020	-0.033
	(0.088)	(8.290)	(4.770)	(0.200)	(0.135)
Cut-off	5,000	5,000	5,000	5,000	5,000
Bandwidth	2,047	1,721	1,435	1,798	2,264
Observations on the left	732	598	473	595	844
Observations on the right	370	320	268	316	404
	Panel C: Education characteristics				
	% Female HS+	% Male HS+			
Treatment	-0.003	-0.001			
	(0.003)	(0.003)			
Bias-corrected	-0.004	-0.002			
	(0.003)	(0.003)			
Treatment (bias-corrected, robust SE)	-0.004	-0.002			
	(0.003)	(0.003)			
Cut-off	5,000	5,000			
Bandwidth	1,364	1,574			
Observations on the left	441	520			
Observations on the right	257	294			
	Panel D: Economic characteristics				
	% Female employed	% Male employed	Average income	% Taxpayers	# Firms
Treatment	-0.009*	-0.007	-177.530	-0.023	-2.114
	(0.005)	(0.005)	(450.627)	(0.014)	(8.143)
Bias-corrected	-0.010**	-0.008	-224.842	-0.025*	-2.384
	(0.005)	(0.005)	(450.627)	(0.014)	(8.143)
Treatment (bias-corrected, robust SE)	-0.010*	-0.008	-224.842	-0.025	-2.384
	(0.006)	(0.006)	(548.946)	(0.017)	(9.878)
Cut-off	5,000	5,000	5,000	5,000	5,000
Bandwidth	2,021	1,617	1,711	1,980	1,655
Observations on the left	720	549	593	710	554
Observations on the right	365	299	316	358	301

Notes. The table shows the results of non-parametric estimation for demographic, geographic, education and economic characteristics at municipal level, as dependent variables. Panel A reports female, male, children (0-9) and elderly (70+) shares of the population and the population density. Panel B reports a dummy North for geographical location, area in squared Km, gradient, degree of seismicity on a scale 0-4, mountain area on a scale 1-3. Panel C reports the share of female and male population with upper secondary education or higher. Panel D reports the share of employed females and males, average income (total municipality income over the number of taxpayers), share of taxpayers, and the number of limited liability companies. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015, within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017). *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 1.4: Female presence on municipal councils

	Panel A: Parametric Approach			
Dependent variable:	Share of female councilors			
	(1)	(2)	(3)	(4)
Treatment	0.135***	0.130***	0.151***	0.162***
	(0.009)	(0.014)	(0.018)	(0.023)
Polynomial order	1	2	3	4
Observations	4,599	4,599	4,599	4,599
R-Squared	0.122	0.122	0.123	0.124

	Panel B: Non-parametric Approach
Dependent variable:	Share of female councilors
	(1)
Treatment	0.174***
	(0.021)
Bias-corrected	0.183***
	(0.021)
Treatment (bias-corrected, robust SE)	0.183***
	(0.024)
Bandwidth	1,132
Observations on the left	353
Observations on the right	219

Notes. The table shows the results of parametric and non-parametric estimations. The dependent variable is the share of female councilors over the total number of councilors. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. In Panel A, the sample includes all municipalities with less than 15,000 residents that held elections in the period 2013-2015. Columns 1-4 include polynomials of orders 1-4, respectively, in the resident population, centered on the 5,000 resident threshold. Polynomials are allowed to differ on the two sides of the cut-off. In Panel B, conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.5: Female presence on municipal councils: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Share of female councilors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.009	-0.009	-0.001	0.174***	-0.005	-0.016	-0.014
	(0.018)	(0.018)	(0.024)	(0.021)	(0.018)	(0.022)	(0.025)
Bias-corrected	0.009	-0.011	0.010	0.183***	-0.012	-0.013	-0.003
	(0.018)	(0.018)	(0.024)	(0.021)	(0.018)	(0.022)	(0.025)
Treatment (bias-corrected, robust SE)	0.009	-0.011	0.010	0.183***	-0.012	-0.013	-0.003
	(0.022)	(0.021)	(0.027)	(0.024)	(0.021)	(0.027)	(0.027)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	727	1,212	727	1,132	1,767	1,471	1,883
Observations on the left	709	801	299	353	436	251	276
Observations on the right	494	476	211	219	265	190	194
	Panel B: Alternative bandwidths						
Dependent variable:	Share of female councilors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.173***	0.165***	0.150***	0.145***	0.140***	0.137***	0.136***
	(0.022)	(0.019)	(0.016)	(0.014)	(0.013)	(0.012)	(0.011)
Bias-corrected	0.163***	0.179***	0.181***	0.167***	0.160***	0.154***	0.148***
	(0.022)	(0.019)	(0.016)	(0.014)	(0.013)	(0.012)	(0.011)
Treatment (bias-corrected, robust SE)	0.163***	0.179***	0.181***	0.167***	0.160***	0.154***	0.148***
	(0.030)	(0.026)	(0.023)	(0.021)	(0.019)	(0.018)	(0.017)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	300	495	718	983	1,338	1,798	2,392
Observations on the right	203	278	360	437	494	555	609

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the share of female councilors over the total number of councilors. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within each bandwidths. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.6: Female presence on municipal councils before the reform

	Panel A: Parametric approach			
Dependent variable:	Share of female councilors			
	(1)	(2)	(3)	(4)
Treatment	0.008	-0.010	0.006	0.003
	(0.008)	(0.012)	(0.017)	(0.021)
Polynomial order	1	2	3	4
Observations	4,599	4,599	4,599	4,599
R-Squared	0.012	0.013	0.014	0.014

	Panel B: Non-parametric approach
Dependent variable:	Share of female councilors
	(1)
Treatment	-0.009
	(0.016)
Bias-corrected	-0.011
	(0.016)
Treatment (bias-corrected, robust SE)	-0.011
	(0.019)
Bandwidth	2,037
Observations on the left	730
Observations on the right	368

Notes. The table shows the results of parametric and non-parametric estimations. The dependent variable is the share of female councilors over the total number of councilors in the election prior to 2013-2015. Elections take place every 5 years, thus, in most cases, previous election is in 2008-2010, see the main text for details. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. In Panel A, the sample includes all municipalities with less than 15,000 residents that held elections in the period 2013-2015. Columns 1-4 include polynomials of orders 1-4, respectively, in the resident population, centered on the 5,000 resident threshold. Polynomials are allowed to differ on the two sides of the cut-off. In Panel B, conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.7: Female presence on municipal councils: diff-in-disc

Dependent variable:	Share of female councilors	
	(1)	(2)
Treatment × After	0.127***	0.186***
	(0.010)	(0.028)
Local		X
Observations	9,198	890
R-Squared	0.327	0.504

Notes. The table shows the results of difference-in-discontinuities estimation. The dependent variable is the share of female councilors over the total number of councilors. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. *After* is an indicator variable for elections in 2013-2015. Only the coefficient of interest *Treatment***After* is reported. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 and, correspondingly, in 2008-2010. In Column 1 the sample includes all municipalities; in column 2 the sample includes municipalities within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.8: Working of the policy

	Non-parametric approach			
Dependent variable:	Female candidates	Borda score	Preference votes	Post-election Borda score
	(1)	(2)	(3)	(4)
Treatment	0.002	-0.069	0.158**	0.121**
	(0.054)	(0.087)	(0.062)	(0.052)
Bias-corrected	-0.011	-0.082	0.151**	0.124**
	(0.054)	(0.087)	(0.062)	(0.052)
Treatment (bias-corrected, robust SE)	-0.011	-0.082	0.151**	0.124*
	(0.065)	(0.102)	(0.074)	(0.064)
Bandwidth	1,278	1,294	1,199	1,456
Observations on the left	82	65	74	78
Observations on the right	104	89	95	97

Notes. The table shows the results of non-parametric estimation. The dependent variable is the share of female candidates over the total number of candidates on list s in municipality i in column 1; the Borda score of female candidates on list s in municipality i in column 2; the share of preference votes cast for female candidates on list s in municipality i in column 3; the post-election Borda score of female candidates on list s in municipality i in column 4. See the main text for details on the definition of the variables. *Treatment* is an indicator variables for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.9: Working of the policy before the reform

	Non-parametric Approach			
Dependent variable:	Female candidates	Borda score	Preference votes	Post-election Borda score
	(1)	(2)	(3)	(4)
Treatment	-0.066	-0.012	-0.009	-0.029
	(0.058)	(0.046)	(0.049)	(0.048)
Bias-corrected	-0.085	-0.021	-0.027	-0.047
	(0.058)	(0.046)	(0.049)	(0.048)
Treatment (bias-corrected, robust SE)	-0.085	-0.021	-0.027	-0.047
	(0.071)	(0.057)	(0.059)	(0.059)
Bandwidth	1,059	2,157	1,917	1,536
Observations on the left	38	128	109	77
Observations on the right	73	121	119	96

Notes. The table shows the results of non-parametric estimation. The dependent variable is the share of female candidates over the total number of candidates on list s in municipality i in column 1; the Borda score of female candidates on list s in municipality i in column 2; the share of preference votes cast for female candidates on list s in municipality i in column 3; the post-election Borda score of female candidates on list s in municipality i in column 4. See the main text for details on the definition of the variables. All outcome variables refer to the election prior to 2013, which is in most cases in 2008. *Treatment* is an indicator variables for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. The sample includes all lists presented in the previous election in municipalities with less than 15,000 residents that held elections in 2013 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.10: Other voting outcomes

	Non-parametric Approach			
Dependent variable:	Turnout	Female turnout	Use of preferences	Female education
	(1)	(2)	(3)	(4)
Treatment	-0.015	-0.011	0.858***	-0.130
	(0.011)	(0.010)	(0.157)	(0.287)
Bias-corrected	-0.018*	-0.014	0.854***	-0.151
	(0.011)	(0.010)	(0.157)	(0.287)
Treatment (bias-corrected, robust SE)	-0.018	-0.014	0.854***	-0.151
	(0.013)	(0.012)	(0.199)	(0.345)
Bandwidth	1,742	1,901	852	2,025
Observations on the left	605	679	62	663
Observations on the right	322	352	74	350

Notes. The table reports results of non-parametric estimation. The dependent variable is turnout, measured as the share of actual voters over eligible voters in municipality i in column 1; female turnout, measured as the share of actual female voters over eligible female voters in municipality i in column 2; the number of preference votes over the total number of actual voters for list s in municipality i in column 3; the average number of years of education of elected female councilors in municipality i in column 4. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. In column 1,2 and 4 the sample includes municipalities with less than 15,000 residents that held elections in 2013-2015, within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. In column 3, the sample includes municipalities with less than 15,000 residents that held elections in 2013 for which preference votes were available, within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.11: Quality of elected councilors: diff-in-disc

Dependent variable:	Years of education					
	Male-Female	Male-Female	Male	Male	Female	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × After	0.338	-0.430	0.212*	-0.058	-0.141	0.273
	(0.215)	(0.422)	(0.118)	(0.220)	(0.183)	(0.345)
Local		X		X		X
Observations	8,456	1,700	9,141	2,032	8,474	2,044
R-Squared	0.004	0.008	0.023	0.037	0.006	0.013

Notes. The table shows the results of difference-in-discontinuities estimation. The dependent variable is the difference between the average years of education of male and female councilors in columns 1-2; the average years of education of male councilors in columns 3-4, the average years of education of female councilors in columns 5-6. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. *After* is an indicator variable for elections in 2013-2015. Only the coefficient of interest *Treatment*After* is reported. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 and, correspondingly, in 2008-2010. The results are computed for the entire sample in column 1, 3 and 5, and for the sample of municipalities within the optimal bandwidth selected by one common MSE-optimal bandwidth selector around the cut-off of 5,000 residents in column 2, 4 and 6. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.12: Other voting outcomes before the reform

	Non-parametric Approach			
Dependent variable:	Turnout	Female turnout	Use of preferences	Female education
	(1)	(2)	(3)	(4)
Treatment	-0.014	-0.001	0.245	0.076
	(0.010)	(0.012)	(0.154)	(0.375)
Bias-corrected	-0.018*	-0.002	0.219	0.155
	(0.010)	(0.012)	(0.154)	(0.375)
Treatment (bias-corrected, robust SE)	-0.018	-0.002	0.219	0.155
	(0.012)	(0.015)	(0.184)	(0.448)
Bandwidth	1,702	2,032	997	1,586
Observations on the left	591	726	35	497
Observations on the right	313	366	73	278

Notes. The table reports results of non-parametric estimation. The dependent variable is turnout, measured as the share of actual voters over eligible voters in municipality i in column 1; female turnout, measured as the share of actual female voters over female eligible voters in municipality i in column 2; the number of preference votes over the total number of actual voters for list s in municipality i in column 3; the average number of years of education of elected female councilors in municipality i in column 4. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Outcome variables refer to the election prior to 2013-2015, which is in 2008-2010. In column 1,2 and 4 the sample includes municipalities with less than 15,000 residents that held elections in 2013-2015, within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. In column 3, the sample includes municipalities with less than 15,000 residents that held elections in 2013 for which preference votes for the previous elections were available, within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 1.13: Preference votes in regional elections: diff-in-disc

Dependent variable:	Average preference votes	
	(1)	(2)
Treatment × After	2.838***	1.719
	(0.788)	(1.086)
Local		X
Observations	47,474	15,937
R-Squared	0.076	0.072

Notes. The table shows the results of difference-in-discontinuities estimation. The dependent variable is the average number of preference votes cast in municipality i for female candidates on list s in regional elections. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. *After* is an indicator variable for regional elections after 2013. Only the coefficient of interest *Treatment*After* is reported. The sample includes treated and control municipalities in regions which held elections after 2013 and, correspondingly, in 2010, which do not apply double preference voting at regional level. The results are computed for the entire sample in column 1, and for the sample of municipalities within the optimal bandwidth around the cut-off of 5,000 residents in column 2. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Appendix

1.A Additional Material

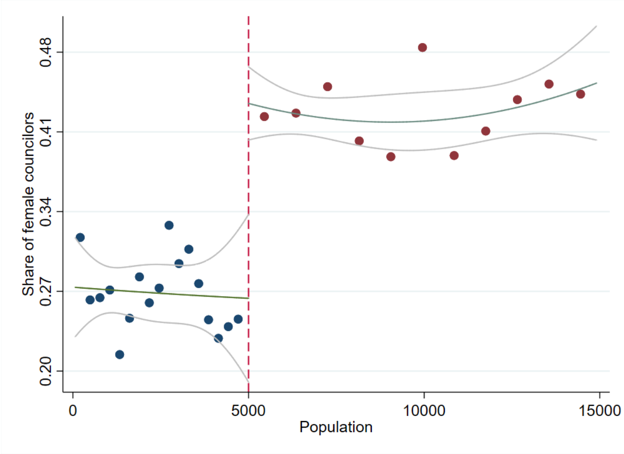

Figure 1.A.1: Female councilors

Notes. The figure plots the binned averages of the share of female councilors elected in the second election with double preference voting against the municipal population, together with the quadratic polynomial fit on both sides of the 5,000 resident cut-off and the 95% confidence intervals. Elections take place every 5 years, thus, in most cases, the second election with double preference voting is in 2018. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015.

Table 1.A.1: Female presence on municipal councils: geographical areas

	Non-parametric Approach		
Dependent variable:	Share of female councilors		
	(1)	(2)	(3)
Treatment	0.137***	0.159***	0.215***
	(0.028)	(0.060)	(0.028)
Bias-corrected	0.147***	0.160***	0.211***
	(0.028)	(0.060)	(0.028)
Treatment (bias-corrected, robust SE)	0.147***	0.160**	0.211***
	(0.032)	(0.077)	(0.034)
Area	North	Center	South
Bandwidth	986	2,061	1,886
Observations on the left	187	110	152
Observations on the right	118	46	94

Notes. The table shows the results of non-parametric estimation. The dependent variable is the share of female councilors over the total number of councilors. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Columns 1, 2 and 3 show the results for municipalities in the North, Center and South, respectively. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around the cut-off of 5,000 residents. *p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.2: Female presence on municipal councils before the reform: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Share of female councilors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.013	0.015	0.006	-0.009	0.035	0.004	0.003
	(0.018)	(0.016)	(0.015)	(0.016)	(0.023)	(0.021)	(0.023)
Bias-corrected	0.017	0.017	0.011	-0.011	0.036	0.008	0.009
	(0.018)	(0.016)	(0.015)	(0.016)	(0.023)	(0.021)	(0.023)
Treatment (bias-corrected, robust SE)	0.017	0.017	0.011	-0.011	0.036	0.008	0.009
	(0.021)	(0.020)	(0.017)	(0.019)	(0.027)	(0.025)	(0.027)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	532	1,048	1,474	2,037	1,745	1,952	1,890
Observations on the left	491	671	667	730	429	351	277
Observations on the right	379	429	396	368	261	242	194

	Panel B: Alternative bandwidths						
Dependent variable:	Share of female councilors						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.001	-0.009	-0.009	-0.006	-0.006	-0.005	-0.005
	(0.023)	(0.019)	(0.016)	(0.014)	(0.013)	(0.012)	(0.011)
Bias-corrected	0.009	0.008	-0.007	-0.010	-0.009	-0.010	-0.009
	(0.023)	(0.019)	(0.016)	(0.014)	(0.013)	(0.012)	(0.011)
Treatment (bias-corrected, robust SE)	0.009	0.008	-0.007	-0.010	-0.009	-0.010	-0.009
	(0.034)	(0.028)	(0.024)	(0.021)	(0.020)	(0.018)	(0.017)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	300	495	718	983	1,338	1,798	2,392
Observations on the right	203	278	360	437	494	555	609

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the share of female councilors over the total number of councilors in the election prior to 2013-2015. Elections take place every 5 years, thus, in most cases, previous election is in 2008-2010. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes municipalities with less than 15,000 residents that held elections in the period 2013-2015 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.3: Share of female candidates in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Share of female candidates						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.008	-0.008	0.055	0.002	0.025	0.024	0.014
	(0.052)	(0.050)	(0.035)	(0.054)	(0.025)	(0.019)	(0.019)
Bias-corrected	-0.021	0.004	0.066*	-0.011	0.033	0.033*	0.018
	(0.052)	(0.050)	(0.035)	(0.054)	(0.025)	(0.019)	(0.019)
Treatment (bias-corrected, robust SE)	-0.021	0.004	0.066	-0.011	0.033	0.033	0.018
	(0.066)	(0.059)	(0.043)	(0.065)	(0.029)	(0.022)	(0.023)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	724	1,160	1,639	1,278	1,477	1,411	2,166
Observations on the left	133	159	165	82	109	119	177
Observations on the right	112	83	123	104	124	91	76
	Panel B: Alternative bandwidths						
Dependent variable:	Share of female candidates						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.027	-0.012	-0.004	0.011	0.025	0.034	0.035*
	(0.070)	(0.043)	(0.034)	(0.029)	(0.025)	(0.023)	(0.021)
Bias-corrected	0.057	0.053	-0.019	-0.032	-0.026	-0.010	0.006
	(0.070)	(0.043)	(0.034)	(0.029)	(0.025)	(0.023)	(0.021)
Treatment (bias-corrected, robust SE)	0.057	0.053	-0.019	-0.032	-0.026	-0.010	0.006
	(0.176)	(0.095)	(0.070)	(0.053)	(0.045)	(0.039)	(0.035)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	62	105	145	207	277	389	462
Observations on the right	87	112	173	211	251	268	280

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variables is the share of female candidates over the total number of candidates on list s in municipality i. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.4: Borda score of female candidates in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Borda score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.014	-0.015	-0.120*	-0.069	0.074***	0.028	0.013
	(0.053)	(0.063)	(0.070)	(0.087)	(0.029)	(0.039)	(0.034)
Bias-corrected	-0.020	-0.033	-0.141**	-0.082	0.086***	0.046	0.008
	(0.053)	(0.063)	(0.070)	(0.087)	(0.029)	(0.039)	(0.034)
Treatment (bias-corrected, robust SE)	-0.020	-0.033	-0.141*	-0.082	0.086***	0.046	0.008
	(0.067)	(0.075)	(0.082)	(0.102)	(0.034)	(0.045)	(0.039)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	913	887	1,163	1,294	2,201	1,628	2,151
Observations on the left	135	105	75	65	132	127	161
Observations on the right	114	60	59	89	159	93	69
	Panel B: Alternative bandwidths						
Dependent variable:	Borda score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.062	-0.048	-0.010	-0.003	0.007	0.021	0.028
	(0.108)	(0.069)	(0.048)	(0.040)	(0.035)	(0.031)	(0.029)
Bias-corrected	0.180*	-0.082	-0.086*	-0.045	-0.040	-0.041	-0.027
	(0.108)	(0.069)	(0.048)	(0.040)	(0.035)	(0.031)	(0.029)
Treatment (bias-corrected, robust SE)	0.180	-0.082	-0.086	-0.045	-0.040	-0.041	-0.027
	(0.332)	(0.153)	(0.113)	(0.082)	(0.066)	(0.055)	(0.049)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	54	80	116	173	233	315	379
Observations on the right	72	97	154	185	225	242	251

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the Borda score of female candidates on list s in municipality i. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.5: Share of preference votes for female candidates in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Preference votes						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.025	0.029	0.033	0.158**	0.050	-0.043*	0.033
	(0.045)	(0.054)	(0.044)	(0.062)	(0.033)	(0.022)	(0.027)
Bias-corrected	0.031	0.046	0.042	0.151**	0.061*	-0.044**	0.040
	(0.045)	(0.054)	(0.044)	(0.062)	(0.033)	(0.022)	(0.027)
Treatment (bias-corrected, robust SE)	0.031	0.046	0.042	0.151**	0.061	-0.044*	0.040
	(0.057)	(0.063)	(0.056)	(0.074)	(0.037)	(0.026)	(0.032)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	1,029	1,056	1,526	1,199	1,161	1,541	1,850
Observations on the left	184	144	145	74	87	134	156
Observations on the right	134	83	113	95	104	95	67
	Panel B: Alternative bandwidths						
Dependent variable:	Preference votes						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.176**	0.115***	0.119***	0.132***	0.140***	0.146***	0.146***
	(0.071)	(0.044)	(0.035)	(0.031)	(0.027)	(0.024)	(0.023)
Bias-corrected	0.105	0.218***	0.121***	0.101***	0.108***	0.118***	0.130***
	(0.071)	(0.044)	(0.035)	(0.031)	(0.027)	(0.024)	(0.023)
Treatment (bias-corrected, robust SE)	0.105	0.218**	0.121*	0.101*	0.108**	0.118***	0.130***
	(0.167)	(0.096)	(0.073)	(0.055)	(0.046)	(0.041)	(0.037)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	62	105	145	207	276	387	460
Observations on the right	87	112	173	211	251	268	280

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the share of preference votes cast for female candidates on list s in municipality i. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.6: Post-election Borda score of female candidates in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Post-election Borda score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.061	0.057	-0.001	0.121**	0.005	-0.056**	0.028
	(0.056)	(0.057)	(0.054)	(0.052)	(0.026)	(0.022)	(0.024)
Bias-corrected	0.074	0.067	-0.011	0.124**	0.011	-0.062***	0.033
	(0.056)	(0.057)	(0.054)	(0.052)	(0.026)	(0.022)	(0.024)
Treatment (bias-corrected, robust SE)	0.074	0.067	-0.011	0.124*	0.011	-0.062**	0.033
	(0.069)	(0.068)	(0.068)	(0.064)	(0.030)	(0.026)	(0.028)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	838	1,021	1,447	1,456	1,405	1,513	2,224
Observations on the left	120	120	112	78	88	116	166
Observations on the right	107	62	89	97	110	88	69
	Panel B: Alternative bandwidths						
Dependent variable:	Post-election Borda score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.147*	0.117**	0.114***	0.112***	0.114***	0.124***	0.126***
	(0.077)	(0.049)	(0.037)	(0.032)	(0.028)	(0.026)	(0.024)
Bias-corrected	0.106	0.168***	0.123***	0.122***	0.107***	0.095***	0.104***
	(0.077)	(0.049)	(0.037)	(0.032)	(0.028)	(0.026)	(0.024)
Treatment (bias-corrected, robust SE)	0.106	0.168	0.123	0.122**	0.107**	0.095**	0.104***
	(0.223)	(0.109)	(0.082)	(0.060)	(0.049)	(0.043)	(0.039)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	54	80	116	173	233	315	379
Observations on the right	72	97	154	185	225	242	251

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the post-election Borda score of female candidates on list s in municipality i *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.7: Working of the policy: diff-in-disc

		Panel A		
Dependent variable:	Female candidates	Borda score	Preference votes	Post-election Borda score
	(1)	(2)	(3)	(4)
Treatment × After	-0.008	0.026	0.103***	0.087***
	(0.026)	(0.031)	(0.030)	(0.029)
Local				
Observations	1,871	1,722	1,798	1,722
R-Squared	0.133	0.102	0.228	0.247
		Panel B		
Dependent variable:	Female candidates	Borda score	Preference votes	Post-election Borda score
	(1)	(2)	(3)	(4)
Treatment × After	-0.007	0.000	0.119**	0.088
	(0.050)	(0.061)	(0.058)	(0.055)
Local	X	X	X	X
Observations	550	502	549	502
R-Squared	0.171	0.128	0.334	0.346

Notes. The table shows the results of difference-in-discontinuities estimation. The dependent variable is the share of female candidates over the total number of candidates on list s in municipality i in column 1, the Borda score of female candidates on list s in municipality i in column 2, the share of preference votes cast for female candidates on list s in municipality i in column 3, the post-election Borda score of female candidates on list s in municipality i in column 4. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. *After* is an indicator variable for elections in 2013-2015. Only the coefficient of interest *Treatment*After* is reported. The sample includes all lists presented in municipalities with less than 15,000 residents that held elections in 2013 and, correspondingly, in 2008. Panel A reports results for the entire sample of municipalities; Panel B reports results for the sample of municipalities within the optimal bandwidth around the cut-off of 5,000 residents. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.8: Turnout in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Total turnout						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.013	0.011	-0.010	-0.015	-0.012	0.006	0.019
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Bias-corrected	-0.013	0.014	-0.011	-0.018*	-0.015	0.003	0.023*
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
Treatment (bias-corrected, robust SE)	-0.013	0.014	-0.011	-0.018	-0.015	0.003	0.023*
	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	595	908	1,219	1,742	1,735	2,167	1,806
Observations on the left	566	548	529	605	426	414	264
Observations on the right	411	385	342	322	261	269	190

	Panel B: Alternative bandwidths						
Dependent variable:	Total turnout						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.026*	-0.018	-0.013	-0.011	-0.010	-0.009	-0.010
	(0.014)	(0.011)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
Bias-corrected	-0.025*	-0.030***	-0.025**	-0.019**	-0.016**	-0.015**	-0.012*
	(0.014)	(0.011)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
Treatment (bias-corrected, robust SE)	-0.025	-0.030*	-0.025*	-0.019	-0.016	-0.015	-0.012
	(0.021)	(0.017)	(0.015)	(0.013)	(0.012)	(0.011)	(0.010)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	300	495	718	983	1,338	1,798	2,392
Observations on the right	203	278	360	437	494	555	609

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is turnout, measured as the share of actual voters over eligible voters in municipality i in the period 2013-2015. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.9: Female turnout in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Female turnout						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.014	0.007	-0.010	-0.011	-0.013	0.007	0.018
	(0.012)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.012)
Bias-corrected	-0.014	0.009	-0.011	-0.014	-0.016	0.005	0.021*
	(0.012)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.012)
Treatment (bias-corrected, robust SE)	-0.014	0.009	-0.011	-0.014	-0.016	0.005	0.021
	(0.014)	(0.014)	(0.013)	(0.012)	(0.013)	(0.013)	(0.014)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	579	976	1,219	1,901	1,781	2,099	1,826
Observations on the left	545	600	529	679	438	392	268
Observations on the right	402	406	342	352	266	261	191
	Panel B: Alternative bandwidths						
Dependent variable:	Female turnout						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.023*	-0.015	-0.011	-0.009	-0.008	-0.008	-0.008
	(0.014)	(0.011)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
Bias-corrected	-0.022	-0.027**	-0.022**	-0.017*	-0.014*	-0.012	-0.009
	(0.014)	(0.011)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
Treatment (bias-corrected, robust SE)	-0.022	-0.027	-0.022	-0.017	-0.014	-0.012	-0.009
	(0.021)	(0.017)	(0.015)	(0.013)	(0.012)	(0.011)	(0.010)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	300	495	718	983	1,338	1,798	2,392
Observations on the right	203	278	360	437	494	555	609

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is female turnout, measured as the share of actual female voters over eligible female voters in municipality i in the period 2013-2015. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 within each bandwidth. Standard errors clustered at municipal level in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 1.A.10: Use of preference votes in municipal elections: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Use of preferences						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.011	-0.101	-0.176	0.858***	-0.332***	-0.009	-0.277***
	(0.053)	(0.063)	(0.423)	(0.157)	(0.100)	(0.069)	(0.062)
Bias-corrected	0.042	-0.127**	-0.096	0.854***	-0.347***	-0.020	-0.288***
	(0.053)	(0.063)	(0.423)	(0.157)	(0.100)	(0.069)	(0.062)
Treatment (bias-corrected, robust SE)	0.042	-0.127*	-0.096	0.854***	-0.347***	-0.020	-0.288***
	(0.060)	(0.072)	(0.473)	(0.199)	(0.116)	(0.080)	(0.071)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	598	547	437	852	1,358	1,915	2,531
Observations on the left	122	69	38	62	96	168	212
Observations on the right	88	42	28	74	118	107	89
	Panel B: Alternative bandwidths						
Dependent variable:	Use of preferences						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.766***	0.732***	0.644***	0.573***	0.495***	0.454***	0.444***
	(0.144)	(0.090)	(0.069)	(0.061)	(0.055)	(0.050)	(0.047)
Bias-corrected	0.887***	0.793***	0.839***	0.855***	0.818***	0.716***	0.624***
	(0.144)	(0.090)	(0.069)	(0.061)	(0.055)	(0.050)	(0.047)
Treatment (bias-corrected, robust SE)	0.887**	0.793***	0.839***	0.855***	0.818***	0.716***	0.624***
	(0.359)	(0.195)	(0.146)	(0.109)	(0.091)	(0.079)	(0.073)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	62	105	145	207	277	389	462
Observations on the right	87	112	173	211	251	268	280

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the number of preference votes over the total number of actual voters for list s in municipality i. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes municipalities with less than 15,000 residents that held elections in 2013 for which preference votes were available within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes municipalities with less than 15,000 residents that held elections in 2013 for which preference votes were available within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Table 1.A.11: Years of education of female councilors: robustness checks

	Panel A: Alternative cut-offs						
Dependent variable:	Years of education						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-0.003	0.143	0.488	-0.130	0.062	-0.026	0.049
	(0.340)	(0.348)	(0.345)	(0.287)	(0.373)	(0.366)	(0.409)
Bias-corrected	0.023	0.211	0.487	-0.151	0.180	-0.027	-0.081
	(0.340)	(0.348)	(0.345)	(0.287)	(0.373)	(0.366)	(0.409)
Treatment (bias-corrected, robust SE)	0.023	0.211	0.487	-0.151	0.180	-0.027	-0.081
	(0.408)	(0.417)	(0.422)	(0.345)	(0.436)	(0.442)	(0.479)
Cut-off	2,000	3,000	4,000	5,000	6,000	7,000	8,000
Bandwidth	608	1,071	1,589	2,025	1,354	1,875	2,022
Observations on the left	509	616	664	663	293	323	279
Observations on the right	381	392	396	350	205	215	192
	Panel B: Alternative bandwidths						
Dependent variable:	Years of education						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	0.020	-0.159	-0.132	-0.098	-0.064	-0.043	-0.074
	(0.417)	(0.336)	(0.288)	(0.260)	(0.238)	(0.220)	(0.204)
Bias-corrected	0.123	0.109	-0.097	-0.135	-0.143	-0.115	-0.021
	(0.417)	(0.336)	(0.288)	(0.260)	(0.238)	(0.220)	(0.204)
Treatment (bias-corrected, robust SE)	0.123	0.109	-0.097	-0.135	-0.143	-0.115	-0.021
	(0.617)	(0.500)	(0.430)	(0.380)	(0.344)	(0.318)	(0.297)
Cut-off	5,000	5,000	5,000	5,000	5,000	5,000	5,000
Bandwidth	1,000	1,500	2,000	2,500	3,000	3,500	4,000
Observations on the left	280	457	656	890	1,213	1,616	2,149
Observations on the right	194	267	346	416	469	527	576

Notes. The table shows the robustness checks of non-parametric estimation. The dependent variable is the average number of years of education of elected female councilors in municipality i. *Treatment* is an indicator variable for municipalities with more than 5,000 residents. Only the coefficient of interest *Treatment* is reported. Conventional RD estimates with a conventional variance estimator, bias-corrected RD estimates with a conventional variance estimator, and bias-corrected RD estimates with a robust variance estimator are reported. Panel A reports results for placebo cut-offs, namely 2,000, 3,000, 4,000, 6,000, 7,000 and 8,000 residents, in addition to the correct 5,000 one reported in Column 4. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 within the optimal bandwidth selected by one common MSE-optimal bandwidth selector (Calonico et al., 2017) around each cut-off. Panel B reports results for alternative bandwidths, namely 1,000, 1,500, 2,000, 2,500, 3,000, 3,500 and 4,000. The sample includes municipalities with less than 15,000 residents that held elections in 2013-2015 within each bandwidth. Standard errors clustered at municipal level in parentheses. * p<0.1, ** p<0.05, *** p<0.01.

Chapter 2

Does Scarcity of Female Instructors Create Demand for Diversity among Students? Evidence from Observational and Experimental Data

2.1 Introduction

The scarcity of female economists has recently attracted considerable attention (Bayer and Rouse, 2016; Chari, Anusha and Paul Goldsmith-Pinkham, 2017). According to the most recent survey by the American Economic Association, 23.5 percent of tenured and tenure-track faculty in economics are women. As such, gender diversity in academia in economics is as poor as in the male-dominated tech industry, where 30 percent of the Silicon Valley workforce is female. Even worse, among full professors in economics, the share of females is often less than 15 percent (Lundberg and Stearns, 2019).

As forcefully argued by Bayer and Rouse (2016), the low share of females in the economic profession may have negative consequences for research. First, May, McGarvey and Whaples (2014) have shown that female and male economists hold different views on economic policies (see also the article "Women in Economics" in the Economist, 2018). As such, research conducted by mostly male economists may not be representative of a more gender-balanced researcher pool and may miss topics relevant for society as a whole. Second, experimental studies have shown that diverse teams tend to be more productive (Apesteguia, Azmat and Iriberri, 2012; Hoogendoorn, Oosterbeek, and van Praag, 2013). Having a more gender-unequal faculty may therefore negatively affect academic output.

In contrast to the literature that notes the potential negative effects of male-dominated faculties on economic research, little is known about potential negative effects on students. However, as a lack of diversity affects the type of research topics studied and taught to the students, this factor may directly channel into female and male students' interest in economics. Moreover, teaching styles may vary with instructor gender and affect student satisfaction of either, or both sexes. In sum, if students value diversity in the instructor pool, a low share of female instructors may make them more valuable in the students' eyes. This taste for female instructors could be driven by all students (general taste for diversity), or among certain subgroups of students in particular. Concerning the latter channel, research in social psychology suggests that an individual's distinctive trait in relation to other people in the environment is more salient if this trait is a numerical minority ("numerical distinctiveness theory", see McGuire and Padawer-Singer, 1976; McGuire and McGuire, 1981). As such, when female professors are scarce, gender may become particularly salient to female students, which may affect their preferences for female (as opposed to male) instructors.

To obtain empirical evidence on this last conjecture (gender-specific taste for diversity), we analyze around 27,000 teaching evaluations from three faculties (Communication, Economics, and Computer Science[1]) of the Università della Svizzera italiana. In all three faculties, women are under-represented in the instructor pool, but the degree of under-representation varies considerably across the faculties. While in Computer Science, only 17% of instructors are female, this share goes up to 23% in Economics, and even up to 33% in Communication Science. As such, the share of female instructors in Communication Science is double the size of Computer Science.

If scarcity makes female professors particularly valuable to female students, we may expect this fact to be reflected in course evaluations. Indeed, we find that female professors are evaluated relatively more positively by female students but only in male-dominated faculties.[2] This relative female preference for female professors in male-dominated faculties cannot be explained by cross-faculty differences in student response rates to the evaluation survey. In fact, response rates are well above 90 percent for all three faculties, as filling out the evaluations is necessary to have access to the course grades. This absence of survey response bias makes our setting unique. Moreover, student selection into the different faculties is unlikely to account for these differences, as surveys among the students reveal similar gender gaps on gender attitudes across the three faculties. This leaves us with a few plausible explanations for the observed correlations: First, our proposed scarcity channel (where the effect of professor scarcity may be amplified by student scarcity). Second, the fact that female professors may teach courses that are more appealing to female students; or they are simply able to motivate female students particularly in male-dominated environments. Third,

[1]Computer Science is called Informatics at this university.

[2]Specifically, we run regressions with course fixed effects and identify differences-in-differences in the evaluation of female versus male students for courses taught by female professors relative to courses taught by male professors.

the selection process of female professors may be different in male-dominated faculties, and forth, the quantitativeness of the subject may matter. In male-dominated faculties, subjects are more technical, and female professors may be particularly valued by female students, if for example, they explain in a more intuitive way.

Due to the difficulty of determining the precise mechanism with observational data, we directly test for the presence of a (potentially gender-specific) taste for instructor diversity in an experimental setting. With this aim, we design a deception-free, incentivized instructor-choice experiment on MTurk, where subjects have to select an instructor who will give them advice on how to solve a given task. The choice set consists of two instructors with comparable qualifications and experience but different gender. Before getting to this choice (which is our key outcome of interest), subjects are told that there is a pool of six instructors, all of which give advice. To test whether scarcity of females affects the choice of the additional instructor (male or female), we experimentally vary the "stock" of six instructors. In the balanced treatment, the subject is presented a stock of three female and three male instructors, whereas in the unbalanced treatment, the subject has a stock of six male instructors.

The main interest of the experiment is analyzing whether the choice of the additional instructor (male or female) depends on the gender balance of the existing instructor pool. To rule out that the order of presenting the two instructors (or details of their profile) affects the subjects' choice, we randomize subjects into permutations that vary according to task type, the order of presenting the two candidates, and the values of the two characteristics attached to the candidates (all details are provided in Section 3). To ensure that subjects take the experiment seriously, we use a variable remuneration that increases with the correct answers in the tasks (next to a fixed show-up fee).

Our main findings are the following. First, scarcity in the stock of female instructors positively affects the probability of having a female chosen as the additional instructor. On average, the female instructor is 11 percentage points more likely to be selected if the stock of instructors is gender-unbalanced. Second, female and male subjects react differently to scarcity in the instructor pool. If female instructors are scarce, female subjects are 12.3 percentage points more likely than male subjects to select the female instructor (this difference is highly statistically significant).[3] Moreover, in contrast to female subjects, male subjects do not react to scarcity of female instructors in a statistically significant way.

While the experimental setting mimics the case of underrepresentation of females (as present in STEM and economics), two plausible mechanisms can explain the results. First, female subjects

[3] When decomposing the tasks according to task type (mathematics versus English task), we find larger effects for mathematics, but the differences are not statistically different from the English task.

prefer female instructors when female instructors are scarce. Second, female subjects have a general preference for diversity (independent of whether scarcity refers to their own gender). To investigate the plausibility of the second channel, we compare instructor-choices of the gender-balanced treatment with a new unbalanced treatment, where all instructors are female. We find that females also value diversity when male instructors are scarce, but to a lesser extent than when female instructors are scarce. By contrast, men value diversity *only* if the scarcity is related to their own gender.

Our study contributes to three main strands of the literature. The first is on gender and hiring decisions. As studies relying on observational data are problematic because of unobserved quality differences between candidates, the most convincing studies exploit experimental variation. An early study by Steinpreis, Anders and Ritzke (1999) studied a hypothetical hiring decision among psychology faculty, where the gender of the candidate was experimentally varied. The main finding was that both male and female faculty were less favorable towards the female candidate. More recently, Williams and Ceci (2015a) conducted a similar hypothetical hiring experiment among faculty in biology, engineering, economics, and psychology. Surprisingly, the results show a consistent preference for females, with the exception of male economists, who were found to be gender-neutral. Our main contribution to these papers is to rigorously test for scarcity effects in a setting where the hiring decision is incentivized. Our results support the view that, especially in settings where women are scarce, female candidates have an edge.

Second, we find that on average, female subjects prefer female candidates, especially when female candidates are scarce. This result complements previous research on ingroup favoritism and outgroup bias (see Tajfel et al., 1971; Chen and Li, 2009; Chen and Chen, 2011; and Chen et al., 2014; and Coffman, Exley and Niederle, 2018). We add to this strand of literature a connection between the strength of this ingroup preference and the scarcity of the ingroup. As becomes apparent from our study, ingroup preferences may become amplified when the ingroup gets relatively smaller.

Third, our paper relates to literature that documents gender differences in student evaluations. While female students in Economics appear to be more critical than males when evaluating male professors, the same does not hold when evaluating female professors (Boring, A., 2017; Mengel, Sauermann, and Zölitz, 2019). Our study complements this literature by providing across-discipline evidence. While we replicate previous results with our sample, we also document that gender differences in instructor evaluations are completely absent in more gender-balanced faculties (Communication), whereas they are aggravated in even more unbalanced faculties (Computer Science).

In summary, our two pieces of evidence (experimental and observational) indicate that gender-related preferences emerge differently in different contexts. When females are scarce, they become more valuable, particularly among the subgroup of female decision makers. As such, increasing the share of females in male-dominated faculties (e.g., STEM disciplines) may increase student satisfaction and act as a pull-factor for future female students.

The remainder of this article is structured as follows. Section 2 presents the results based on observational data. Section 3 shows the evidence from experimental data, and Section 4 concludes.

2.2 Observational Evidence from Teaching Evaluations

2.2.1 Setting and Data

We obtained student evaluation data from the Università della Svizzera italiana (USI) for three different faculties (Communication, Economics, and Computer Science). As shown in Figure 2.1 and the summary statistics (Appendix Table 2.A.1), a stark variation exists in the presence of female professors across faculties: the share of female instructors is lowest in Computer Science (17 percent), followed by Economics (23 percent) and Communication (33 percent). We collected teaching evaluations for all courses taught by the three faculties for the consecutive academic years of 2015-2016 and 2016-2017.[4]

The academic year is organized into two semesters, where students take approximately 7 classes per semester. Teaching evaluations are done online after the students have taken the courses and completed the exams but before they know their actual grade. As filling out the teaching evaluations is necessary to access the grades, the response rate is close to 100% (see the variable "Dummy students not reporting Teaching Evaluations Score" in Appendix Table 2.A.1). The teaching evaluation questionnaire consists of 10 questions. We focus on the question that represents the summary evaluation of the course: "Please express your overall satisfaction with this course".

Our database contains 26,996 teaching evaluations from 1,910 different students for 847 different courses taught by 318 different instructors. As shown in Appendix 2.A.1, approximately one-half of the students are female, although the gender composition varies significantly across disciplines. The average student is 24 years old, between 50% and 66% are doing their Bachelor degree, while the rest are at the Masters level, and Italian and Swiss nationalities are roughly equally represented. Regarding the courses, more than one-half of the courses are compulsory. The proportion of quantitative courses varies substantially across disciplines: 90% are taught in Computer Science, 51% in Economics, and only 14% in Communication. The average class size is also smaller in Computer Science than in the other faculties, as is the overall number of students enrolled. Regarding the

[4]In the academic year 2017-2018, a new evaluation system was introduced, so the newer data were no longer comparable.

instructors, the large majority of instructors are lecturers, followed by full professors. We measure their research productivity through citations (received from the database "Publish or Perish"). Finally, with respect to course-student characteristics, only a minority of students is repeating a course and, as mentioned previously, a very small minority does not complete the teaching evaluation (5%). Students earn an average grade of 7.5 out of 10, where the highest grades are in Communication, followed by Economics and Computer Science. Overall, the students show a high satisfaction level, with an average of 7.2 out of 10, which does not differ across disciplines. In conclusion, the most significant differences across disciplines are observed in the gender composition of both, the faculty as well as the student body, and in the proportion of quantitative courses.

2.2.2 Regression Equation and Results

We test for gender gaps in evaluations with the following regression, which we estimate separately for each faculty:

$$TE_{spc} = \alpha + \beta F_s + \gamma F_p + \delta F_s \times F_p + \eta' T_s + \theta' X_{sc} + \psi' J_p + \rho_c + \epsilon_{scp} \tag{2.1}$$

The dependent variable TE_{spc} is the teaching evaluation score (ranging from 1 to 10) given by student s to professor p teaching course c. F_s is a dummy variable, taking a value of one for female students, F_p is a dummy variable taking a value of one for female professors, and $F_s \times F_p$ refers to the interaction between F_s and F_p. The vectors T_s, J_p and X_{sc} denote student, professor and student-course covariates (see Appendix Table 2.A.1 for an overview of the control variables). δ is the main coefficient of interest, as it measures the differences-in-differences in the evaluation of female versus male students for courses taught by female professors relative to courses taught by male professors. In other words, $\delta > 0$ would suggest that females have a relative preference for female professors (which we loosely call "same-sex preferences"). Moreover, if scarcity of female instructors is the main driver of same-sex preferences, we would expect δ to be higher for more male-dominated faculties (Economics and Computer Science).

The results are presented in Table 2.1. Note that we always include course fixed effects, meaning that we compare evaluations for the *same course*. Baseline estimates with course-year fixed effects are presented in columns 1, 4 and 7. In columns 2, 5, and 8, we add student fixed effects. Last, we add professor-course fixed effects that vary by year to account for the fact that some courses are co-taught (see columns 3, 6, and 9).

First, we note that male students tend to give lower evaluations to female than to male professors (estimated γ), with the largest differences observed in Computer Science. Female students, by contrast, show no differences in how they evaluate male and female professors on average ($\gamma+\delta$). The differences-in-differences (δ) in how female students (relative to males) evaluate female professors (compared to male professors) depend on the faculty. We find evidence of same-gender preferences in Economics and Computer Science but not in Communication. Note that although there are fewer students in Computer Science, the estimated same-gender preference (coefficient δ) is almost doubled compared to that observed in Economics. Estimates of δ suggest that female professors receive 0.2 points more when they are evaluated by female students (compared to when they are evaluated by male students, and relative to the gender gap in evaluating male professors) in Economics and receive almost 0.4 points more in Computer Science. The results are quite stable across specifications. The fact that female students give relatively more generous evaluations to female professors when the faculty is male-dominated may support the view that scarcity matters for student evaluations. While scarcity of female professors seems a very plausible mechanism, scarcity of female students may also play a role (possibly amplifying the effect of scarcity on the professor side).

One caveat could be that students self-select into different disciplines and might exhibit different attitudes towards gender. This characteristic would be troublesome for us if gender *gaps* in attitudes differ across faculties. To shed light on this issue, we designed and administered an 11 question survey for first-year undergraduate students. We interviewed students from three classes during the first semester of the academic year 2017/2018, one from each faculty. To increase sample size, we repeated the survey for freshmen in the academic year 2019/2020. Although the questionnaire was advertised as a survey on students' labor market attitudes and aspirations, five questions (out of eleven) were related to gender stereotypes. In these five questions, students were asked whether they agree or disagree with the following statements: "When jobs are scarce men should have more right to a job than women" (question "Jobs Scarce"), "Having a job is the best way to gain independence for a women" (question "Having a Job"), "When a mother works for pay, children suffer" (question "Mother Works"), "A university education is more important for a boy than for a girl" (question "University Education"), and finally "On the whole, men make better business executives than women" (question "Men better executives"). As shown in Appendix Table 2.A.2, statistically significant gender gaps only exist for two questions (question "Mother Works" and question "Men better executives"). However, these gender gaps are not systematically smaller in Communication, compared to Economics and Computer Science (which needed to be the case should different gender gaps in values explain the across-faculty differences in teaching evaluations). As such, we find little support for differences in gender-equality values driving same-sex preferences across disciplines.

To strengthen our suggestive evidence that instructor-scarcity shapes preferences for instructor-gender, we conduct an instructor-choice experiment on M-Turk. The experiment has the advantage that we can isolate the effect of variation in instructor-gender on subjects' hiring preferences (female versus male instructors), and keep constant other factors that could have confounded the results from observational data (e.g. student gender composition).

2.3 Field Experiment on MTurk

2.3.1 Design and Data

We design an incentivized and deception-free instructor-choice experiment on MTurk. Subjects choose an instructor to give them advice on how to solve a given task under time pressure. Subjects are randomized into two types of tasks: mathematical multiplications ("math task") or spelling certain English words correctly ("English task"). At the beginning of the experiment, students are given information on the payoff structure (payoff depends on task performance) and the type of task (math or English, without giving any further details). All subjects are informed that they will receive 1 dollar for their participation plus 40 cents for each correct answer. Most importantly for this experiment, subjects are told that six instructors (selected by us) will give them tips on how to solve the tasks and that they can choose *one additional instructor*. In the end, they will have to choose among one male and one female instructor with comparable qualifications and experience. The key feature of this experiment is that we experimentally vary the "stock" of six instructors. In the balanced treatment, the student has a stock of three female and three male instructors, whereas in the unbalanced treatment, the student has a stock of six male instructors. The only information given to the students is the instructor's name and the fact that he/she is a graduate student (see Figure 2.2 for a screenshot).[5] Concerning the additional instructor to choose, the student gets information on the name (Margaret or Richard), the GPA (3.5 or 3.6 out of 4), and the accumulated hours as a teaching assistant (29 or 31). The main interest of the experiment is analyzing whether the choice of Margaret (as opposed to Richard) as an additional instructor depends on the treatment.

We design 16 permutations, 8 for the math task and 8 for the English task. For each task type, 4 permutations have a balanced instructor pool and 4 permutations have an unbalanced instructor pool. These 4 permutations differ in the order of instructor presentation (Margaret first or second)

[5] In the balanced treatment, the subjects are told that they have six instructors "Jim", "Mary", "John", "Patricia", "Robert" and "Linda", all graduate students. In the unbalanced treatment, the subjects are told that they have six instructors "Jim", "Kevin", "John", "William", "Robert" and "David", all graduate students. The actual tips are obtained from real graduate students who were shown the task and were asked to describe the task in written form.

and characteristics (Margaret with a higher GPA but fewer accumulated hours as TA or Margaret with a lower GPA but more accumulated hours as TA). The goal was to obtain roughly 100 subjects for each permutation, leading to a total of 1,600 subjects. We managed to collect 1,955 observations. However, we removed all subjects who tried to run the experiment twice and those who appeared to be doing the experiment together with a second person.[6] This left us with a subject pool of 1,478: summary statistics are reported in Appendix Table 2.A.3. As shown in Panel A, randomization of CV-characteristics (GPA and hours of experience as TA) across the two candidates' profiles worked well, as the likelihood that Margaret comes first or that Margaret has a higher GPA is always approximately 50%. Most importantly, as evident from Panel B, all demographic covariates are balanced across treatments. In contrast to the teaching evaluation setting, the share of male and female subjects is stable across balanced and unbalanced instructor pools. In Panel C, we report subjects' behavior during the experiment. As expected, the main endogenous variable of interest (instructor choice) differs across treatments. Margaret is chosen more frequently when the treatment is "Unbalanced" (when female instructors are scarce). Regarding the duration of the task, the number of times instructor advice was sought, or performance in terms of correct answers, we do not see any differences across treatments.

While the summary statistics indicate that the subjects receive advice slightly more than 4 times on average, it is also interesting to look at *which* type of advice the subjects seek. In Appendix Figure 2.A.1, we document the percentage of subjects who click on a specific advice, starting with advice from the instructor to the farthest left of the instructor pool (Tip 1, referring to the advice from Jim), followed by advice from the second-leftmost instructor (Tip 2, referring to advice from Kevin in the unbalanced treatment and Mary in the balanced treatment), etc. The advice number 7 (Tip 7) is the advice from the instructor chosen by the subject (Margaret or Richard). As shown in Appendix Figure 2.A.1, a spike is observed for Tip 7 (for both male and female subjects), meaning that advice is most frequently sought from the subject-selected instructor.

Of 1,478 subjects, only 267 did not look at the advice of their chosen instructor. We present the main results for the 1,009 subjects who actually looked at the advice of their selected instructor. Arguably, these subjects took the instructor-choice decision most seriously, as they did (and likely planned to) look at the instructor's advice. However, we will also document the robustness to alternative data samples.

2.3.2 Regression Equations and Results

We run two regression equations:

[6] That is, two subjects running the experiment with the same IP address.

$$Margaret_i = \alpha + \beta Unbalanced_i + \eta Math_i + \theta MargFirst_i + \psi MargTA_i + \iota' X_i + \epsilon_i \quad (2.2)$$

$$Margaret_i = \alpha + \beta Unbalanced_i + \gamma Female_i \times Unbalanced_i + \eta Math_i + \theta MargFirst_i + \psi MargTA_i + \iota' X_i + \epsilon_i \quad (2.3)$$

The dependent variable $Margaret_i$ is a dummy equal to one if subject i chooses the female candidate (Margaret) over the male candidate (Richard). $Female_i$ is a dummy equal to one if the subject is female. $Unbalanced_i$ is a dummy equal to one if subject i is exposed to a pool of six male instructors. The variables $Math_i$, $MargFirst_i$ and $MargTA_i$ control for the experimental permutation: $Math_i$ is a dummy equal to one for the math task; $MargFirst_i$ is a dummy equal to one if - in the instructor choice step - the name of the female candidate (Margaret) comes before the name of the male candidate (Richard); and $MargTA_i$ is a dummy variable taking a value of one if the female candidate (Margaret) is more experienced as a teaching assistant than is the male candidate (Richard). Finally, X_i is a vector of all individual covariates listed in Appendix Table 2.A.3.

The main coefficient of interest in equation 2 is β. A positive β suggests that female instructors are more frequently selected when scarce. Equation 3 adds an interaction term $Female \times Unbalanced$, which enables testing for whether the potential effect of (female) scarcity on instructor choice is gender-specific.

Our main experimental results are shown in Table 2.2. Columns 1-2 show the results of regression equation 2. Clearly, being exposed to a pool of male instructors increases preferences for the female instructor. The probability of choosing Margaret increases by 11 percentage points if a subject is exposed to the gender-unbalanced instructor pool. Adding controls (column 2) hardly affects the estimated coefficient of the treatment "Unbalanced", as would be the case in successful randomization. Column 3 displays the results for regression equation 2, indicating that the stronger preference for female instructors in the treatment "Unbalanced" is *entirely* driven by female subjects. Males are *not* more likely to choose Margaret if the teacher pool is unbalanced (see the estimated β).[7]

These results suggests that females (but not males) value the diversity brought in by a female instructor if the pool of instructors is all male. Female students may expect a different type of advice from female instructors that would help them to answer the questions correctly and earn more money. In this instrumental view, females select the female instructor because they would like

[7] Note that we also estimated models with triple interaction terms to see whether effects differ between task type (English or math). Since the estimated coefficient before the triple interaction $Unbalanced \times Female \times Math$ is statistically insignificant, we report results for the two tasks combined.

to receive the advice. Alternatively, the decision could be entirely subconscious, where females select the female instructor (when the instructor pool is all male), even though they have no expectations in terms of the advice they would receive from the female instructor. To test for this possibility, we conduct a placebo analysis. We restrict the sample to those subjects who did not check any single advice (remember - before seeing the task!). These are likely subjects whose strategy is to earn the participation fee but have no intention of exerting any additional effort to answer the questions correctly. As shown in Table 2.3 columns 1 and 2, there is no effect of the treatment "Unbalanced" (nor the interaction of "Unbalanced" with subject gender) on the probability that Margaret is chosen. As such, in the sample of subjects who exert very little effort in the experiment, scarcity of females in the instructor pool does not affect the probability that the female instructor is chosen.

Most interesting to us are the subjects who exert at least some minimal effort to correctly answer the questions. We presented the results for subjects who looked at the advice of the chosen instructor in Table 2.2. As additional evidence, we now report the results for different subject samples, varying in the number of advice seen. As shown in Table 2.3, columns 3-8, gender-specific preferences for diversity get larger in samples where subjects ask for more advice and, arguably, take the task more seriously. Last, we focus on those subjects who could answer the last survey question: "How many women were in the initial instructor pool of six instructors?" Again, the effect is strong (in fact the strongest) among those subjects who appeared to pay close attention to the experiment.

In sum, we provided evidence that female subjects value female instructors when female instructors are scarce. While we were most interested in the setting lacking female diversity, we were curious whether females would also value diversity in a setting where males are scarce. We therefore ran an additional treatment, where the scarce group is male (i.e., the unbalanced treatment is all female).[8] As shown in Appendix Table 2.A.5, females also value diversity in this scenario but to a smaller extent (compare the estimated $(\beta + \gamma)$ with those in Table 2.2). By contrast, males value diversity *only* when male instructors are underrepresented.[9]

2.4 Conclusions

Female underrepresentation in science (especially STEM faculties) is a topic of heated debate. While numerous articles explore potential causes (e.g., stereotypes (Reuben, 2014), family and career incompatibilities (Goldin, 2014), or publishing hurdles (Card et al., 2019)), little is known about the consequences of a lack of academic diversity. Since diverse teams are often found to be more

[8]Summary statistics are presented in Appendix Table 2.A.4.

[9]The estimated β is large and highly significant in Appendix Table 2.A.5 but not in Table 2.2.

productive, a reasonable conjecture is that academic diversity would favor research output (Bayer and Rouse, 2016). What about teaching quality? We first document that the *perceived* teaching quality of the same course may differ across student gender. Importantly, gender differences in preferences are amplified when faculties are male-dominated. While gender gaps in Communication are negligible, female students are (relatively) more satisfied with female professors (vis-à -vis male professors) in male-dominated faculties such as Economics and Computer Science. Does that mean that if female professors are scarce, female students would prefer to hire more female professors? Our experimental evidence suggests that this might be the case. Female (but not male) subjects show a clear preference for female instructors if females are scarce in the instructor pool. Luckily for the few existing female students in STEM faculties, hiring preferences seem to become more female friendly as long as female candidates are equal to or better than male candidates (Williams and Ceci, 2015a, 2015b).

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Figure 2.1: Gender Ratio of Professors by Discipline

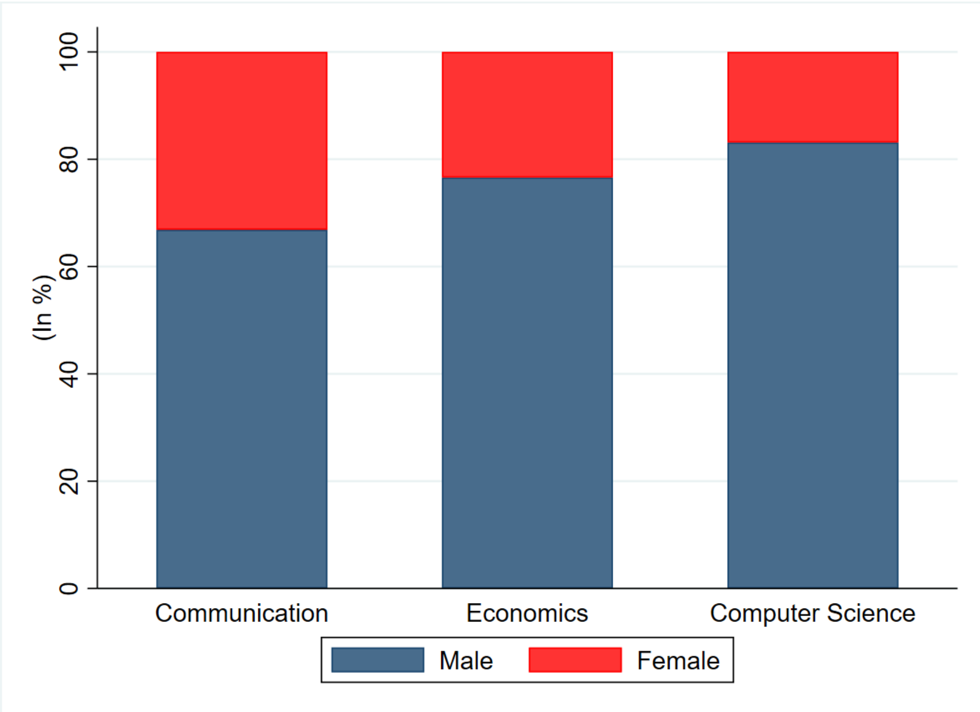
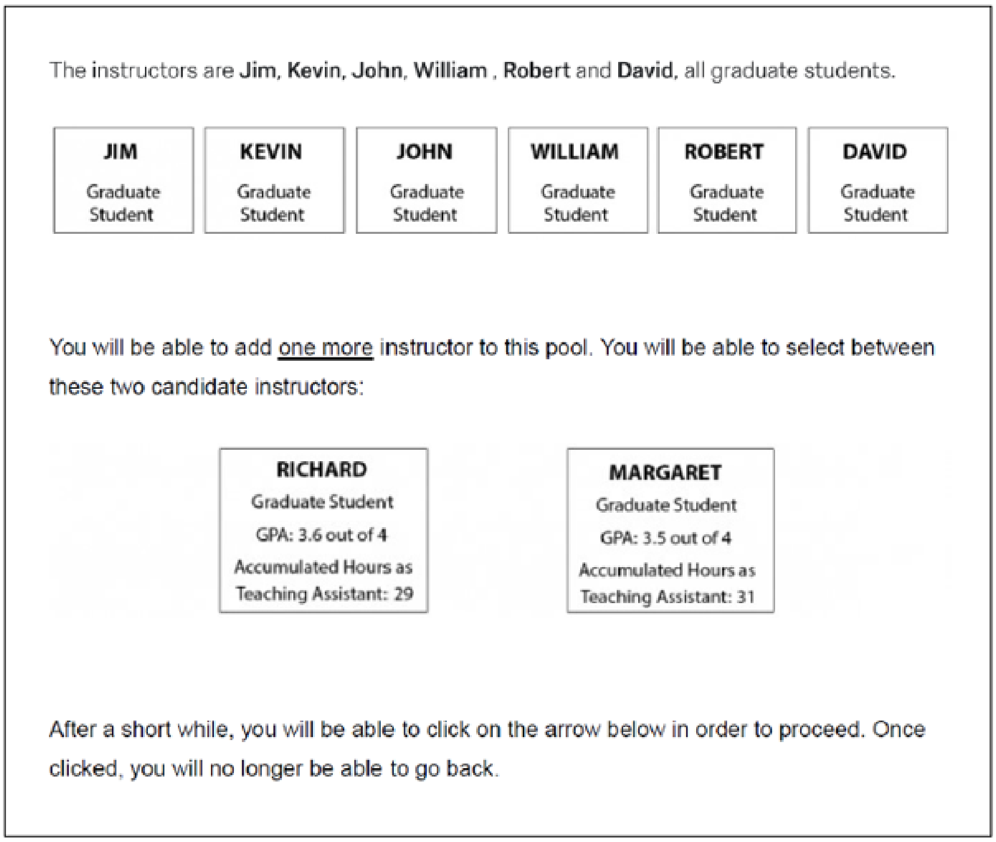


Figure 2.2: Experimental Treatments

Panel A: Treatment Unbalanced



Panel B: Treatment Balanced

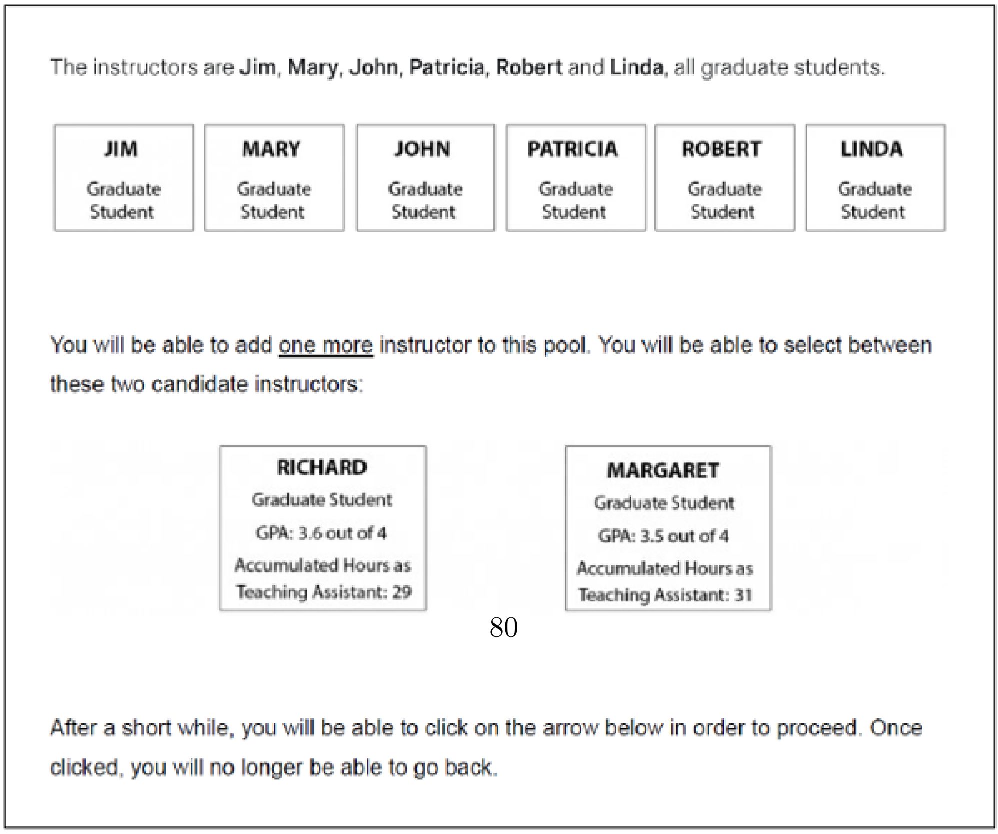


Table 2.1: Gender Gaps in Teaching Evaluations, by Discipline

Disciplines	Communication			Economics			Computer Science		
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female_s (β)	-0.046			-0.266***			0.234*		
	(0.055)			(0.048)			(0.132)		
Female_p (γ)	-0.180	-0.251*		-0.075	-0.065		-0.326***	-0.343***	
	(0.152)	(0.151)		(0.124)	(0.129)		(0.025)	(0.038)	
$\text{Female}_S \times \text{Female}_P$ (δ)	0.019	0.110	0.106	0.270**	0.225**	0.232***	0.414	0.444*	0.442*
	(0.089)	(0.082)	(0.073)	(0.111)	(0.096)	(0.085)	(0.257)	(0.250)	(0.256)
Constant	5.424***	5.346***	5.535***	7.081***	7.407***	7.564***	1.583	5.992***	6.527***
	(0.380)	(0.366)	(0.369)	(0.419)	(0.375)	(0.419)	(1.801)	(0.540)	(0.479)
$\gamma + \delta$	-0.161	-0.141		0.195	0.16		0.088	0.10	
	(0.163)	(0.165)		(0.127)	(1.333)		(0.236)	(0.235)	
Course-Year FE	YES	YES	NO	YES	YES	NO	YES	YES	NO
Student FE	NO	YES	YES	NO	YES	YES	NO	YES	YES
Professor-Course-Year FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Student-Course Control	YES	YES	YES	YES	YES	YES	YES	YES	YES
Student Control	YES	NO	NO	YES	NO	NO	YES	NO	NO
R-squared	0.221	0.472	0.480	0.187	0.490	0.494	0.381	0.568	0.569
N	10799	10820	10891	11250	11266	11266	2402	2410	2410

Notes. The dependent variable is the teaching evaluation score received by instructor i for course j. Evaluations in courses with less than six students are excluded from the analysis. Columns 1,4,7 include Course-Year fixed effects, Columns 2,5,8 include Course-Year fixed effects and Student fixed effects, and Columns 3,6,9 include Professor-Course-Year fixed effects and Student fixed effects. Standard errors, clustered at course-year level, are reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.2: Choice of Female Instructor when Female Instructors are Scarce

	(1)	(2)	(3)
Unbalanced (β)	0.116***	0.111***	0.049
	(0.029)	(0.029)	(0.041)
Female×Unbalanced (γ)			0.125**
			(0.053)
Math Task	0.006	0.009	0.006
	(0.025)	(0.024)	(0.024)
Margaret First	-0.055**	-0.059**	-0.059**
	(0.026)	(0.026)	(0.025)
Margaret TA	-0.022	-0.023	-0.021
	(0.038)	(0.039)	(0.038)
Female		0.050**	-0.014
		(0.023)	(0.036)
Age		0.004***	0.004***
		(0.001)	(0.001)
White		0.009	0.010
		(0.042)	(0.042)
College Degree		0.021	0.016
		(0.047)	(0.046)
Post-graduate Degree		0.013	0.005
		(0.051)	(0.050)
Constant	0.670***	0.494***	0.531***
	(0.038)	(0.069)	(0.067)
$\beta + \gamma$			0.174***
			(0.035)
R-squared	0.019	0.032	0.036
N	1009	1009	1009

Notes. The dependent variable is a dummy equal to one if Margaret is chosen. Treatment "Unbalanced" is a dummy equal to one if the subject is exposed to a pool of six male instructors, and zero if he/she is exposed to a gender balanced pool of instructors. All included subjects checked the advice by the chosen instructor. Robust standard errors are reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.3: Choice of Female Instructor when Female Instructors are Scarce: Robustness Checks

	Zero advices		One advice		Two advices		Three advices		Guessed right	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unbalanced	-0.039	0.040	0.081***	0.057	0.098***	0.058	0.090***	0.028	0.110***	0.039
	(0.082)	(0.096)	(0.023)	(0.039)	(0.024)	(0.041)	(0.024)	(0.041)	(0.038)	(0.055)
Female×Unbalanced		-0.174		0.050		0.085		0.127**		0.128*
		(0.146)		(0.059)		(0.060)		(0.060)		(0.075)
Math Treatment	-0.048	-0.049	0.016	0.015	0.010	0.007	0.005	0.002	0.010	0.007
	(0.083)	(0.083)	(0.022)	(0.022)	(0.025)	(0.025)	(0.024)	(0.024)	(0.032)	(0.032)
Margaret First	0.062	0.061	-0.050**	-0.050**	-0.049*	-0.050*	-0.058**	-0.058**	-0.023	-0.026
	(0.074)	(0.074)	(0.022)	(0.022)	(0.026)	(0.025)	(0.028)	(0.028)	(0.037)	(0.037)
Margaret TA	0.002	0.016	-0.056*	-0.056*	-0.063*	-0.061*	-0.057*	-0.055	-0.083**	-0.079*
	(0.076)	(0.076)	(0.033)	(0.032)	(0.032)	(0.032)	(0.033)	(0.033)	(0.041)	(0.041)
Female	0.065	0.160**	0.067***	0.042	0.057**	0.016	0.057**	-0.005	0.025	-0.029
	(0.079)	(0.071)	(0.023)	(0.034)	(0.023)	(0.037)	(0.024)	(0.041)	(0.038)	(0.039)
Age	0.003	0.002	0.003***	0.003***	0.002**	0.002**	0.002*	0.002*	0.004***	0.004***
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
White	-0.089	-0.081	0.033	0.033	0.042	0.042	0.029	0.030	0.075*	0.073
	(0.100)	(0.099)	(0.042)	(0.042)	(0.048)	(0.048)	(0.043)	(0.043)	(0.044)	(0.045)
College Degree	0.113	0.096	0.051	0.049	0.053	0.050	0.034	0.028	-0.014	-0.022
	(0.140)	(0.136)	(0.050)	(0.050)	(0.050)	(0.049)	(0.050)	(0.049)	(0.066)	(0.064)
Post-graduate Degree	0.060	0.037	0.033	0.031	0.046	0.042	0.036	0.029	0.024	0.010
	(0.153)	(0.146)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.068)	(0.065)
Constant	0.474***	0.458***	0.459***	0.472***	0.480***	0.505***	0.523***	0.562***	0.482***	0.524***
	(0.148)	(0.151)	(0.071)	(0.071)	(0.071)	(0.067)	(0.073)	(0.071)	(0.092)	(0.085)
R-squared	0.030	0.037	0.029	0.030	0.028	0.030	0.025	0.030	0.042	0.047
N	202	202	1276	1276	1077	1077	1005	1005	645	645

Notes. The dependent variable is a dummy equal to one if Margaret is chosen. Treatment "Unbalanced" is a dummy equal to one if the subject is exposed to a pool of six male instructors, and zero if he/she is exposed to a gender balanced pool of instructors. In columns from 1 to 10, we report results of MTurk experiments for different samples of subjets, namely those who did not check any advice (1-2), those who checked at least one advice (3-4), those who checked at least two advices (5-6), those who checked at least 3 advices (7-8), and those who guessed corretly how many female instructors were in the pool (9-10). Robust standard errors are reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

2.A Additional Material

2.A.1 Description of MTurk Experiment

The experiment is structured in seven steps, which are listed below. In every step, subjects are shown a screen window. In the first four steps, subjects are free to choose when to move forward by clicking on the arrow in the lower right corner of the screen. Once the subjects click on the arrow, they move to the next step and cannot go back. We made this rule clear by warning subjects with this sentence at the bottom of the screen window in step 1 to step 4: "After a short while, you will be able to click on the arrow below in order to proceed. Once clicked, you will no longer be able to go back."

Step 1. All the subjects are given the following information:[10]

- They will have to solve simple math/language tasks (10 questions) under time pressure.
- They will be paid based on performance (40 cents for each correct answer).
- They will all receive $1 for their participation.
- Before the test, they can read tips on how to solve the tasks written by different instructors.

Step 2. Two different lists of 6 instructors are shown to subjects. They are not given any information other than the instructors' first names and qualification as "graduate student" (see Figure 2.2, panel A and B, upper part).

- Treatment subjects are exposed to a pool of 6 male instructors.
- Control subjects are exposed to a pool of 3 female and 3 male instructors.

[10] Subjects randomly assigned to the math task visualized precisely the following message: "Thank you for your participation in this study. You will receive 1 dollar for your participation, that is, if you complete the study. We estimate it will not take more than 15-20 minutes. We will ask you to perform a MATH task and we will pay you according to how well you do the task. In particular, we will ask you 10 questions with limited time to respond, and we will pay you 40 cents per correct answer. If you answer correctly all the 10 questions you will receive 4 dollars in addition to the 1 dollar for your participation. Before you do the task, you will be able to read explanations on the task, and you will receive tips on how to get the correct answer for the MATH questions quickly. You will have 10 seconds to answer each question. In the next screen you will find the pool of instructors, all of whom will explain the task and give you tips on how to solve the task correctly under limited time. After a short while, you will able to click on the arrow below in order to proceed. Once clicked, you will no longer be able to go back.". Subjects randomly assigned to the english task visualized the same message, with the only difference that the word MATH was replaced by the word ENGLISH.

Step 3. Subjects are asked to choose one additional instructor; they can choose between one female and one male candidate (see Figure 2.2, panel A and B, lower part).

- The two candidates are Margaret (female candidate) and Richard (male candidate).
- The two candidates have the same educational background: they are both enrolled in a PhD.
- Subjects are given some additional information about the two candidates: GPA and hours of experience as TA.

Step 4. Subjects may read as many tips as they want. They do not have any time limit in this stage.

Step 5. Whenever they feel ready, subjects can proceed with the exercise solving part. They have 10 seconds for each question.

- If subjects are randomized into the math task, they have to solve 10 multiplications of the number 11 with a two or more digit number.
- If subjects are randomized into the language task, they have to spell 10 English words correctly.

Step 6. Subjects are asked to give some personal information (age, gender, education).

Step 7. At the end, subjects are asked to answer the question "In the pool of six instructors how many women were there?". Options were in a range from zero to three.

Figure 2.A.1: Percentage of Subjects Choosing Each Advice

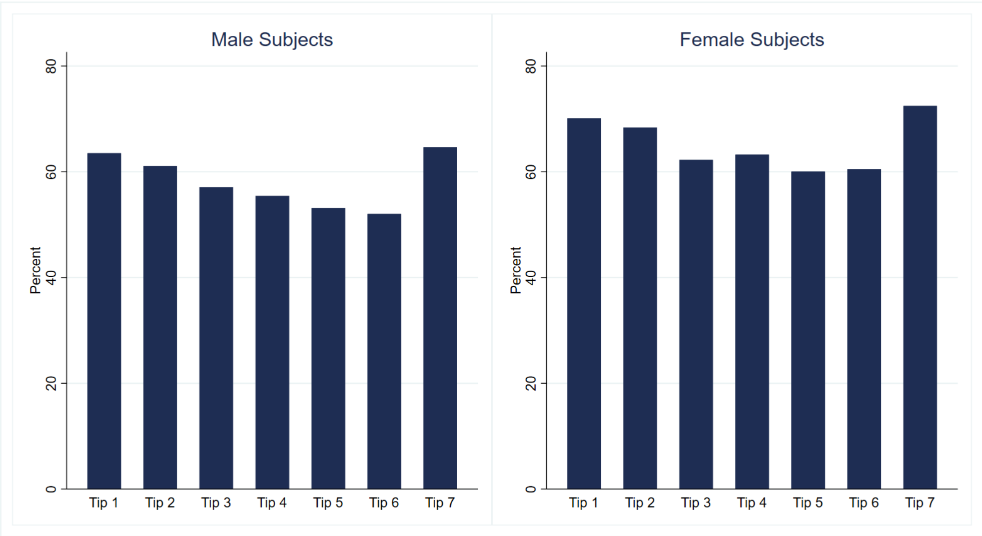


Table 2.A.1: Descriptive Statistics of Teaching Evaluations

	Comm.	Econ.	Comp. Sc.	Δ(E,CO) *P*-value	Δ(E,CS) *P*-value
Column	(1)	(2)	(3)	(4)	(5)
Panel A: Students Characteristics					
No. of Students	770	922	218	-	-
Dummy Female Student	0.69	0.43	0.12	0.00	0.00
Dummy Swiss Students	0.44	0.34	0.35	0.00	0.66
Dummy Italian Students	0.41	0.49	0.30	0.00	0.00
Dummy Other Nationalities	0.15	0.17	0.34	0.25	0.00
Dummy Bachelor Students	0.59	0.48	0.66	0.00	0.00
Student Age	24.56	23.89	24.41	0.00	0.03
Panel B: Course Characteristics					
No. of Courses	430	420	191	-	-
Dummy Compulsory Courses	0.60	0.45	0.71	0.00	0.00
Dummy Quantitative Courses	0.14	0.51	0.90	0.00	0.00
Class Size	34.30	39.36	24.61	0.05	0.00
Panel C: Instructor Characteristics					
No. of Instructors	181	171	89	-	-
Dummy Female Instructors	0.33	0.23	0.17	0.04	0.22
Dummy Full Professors	0.28	0.32	0.36	0.42	0.54
Dummy Associate Professors	0.13	0.17	0.18	0.33	0.84
Dummy Assistant Professors	0.07	0.10	0.10	0.26	0.96
Dummy Lecturers	0.52	0.40	0.34	0.02	0.30
Publish or Perish Citations	87.23	131.68	1225.48	0.11	0.00
Panel D: Student-Course Characteristics					
No. of Teaching evaluations (TE)	11,768	12,435	2,793	-	-
Dummy Students repeating courses	0.02	0.04	0.06	0.00	0.00
Dummy Students not reporting TE-Score	0.06	0.07	0.04	0.00	0.00
Student Grade	7.92	7.51	7.51	0.00	0.84
TE-Score: Overall satisfaction with the course	7.21	7.22	7.28	0.82	0.42

Notes. Table reports summary statistics related to students (Panel A), courses offered (Panel B), professors (Panel C), and students-course characteristics (Panel D) for the academic years 2015 to 2017. In each panel, we report sample numerosity in the first row. For each variable, we report the mean of the variable by faculty (Columns 1-3). In Column 4 we report the P-value of the difference between the mean values of Economics and Communication. In Column 5 we reports the P-value of the difference between the mean values of Economics and Computer Science.

Table 2.A.2: Descriptive Statistics of Gender-Related Questions in the Survey

	All	Male	Female	N_M	N_F	$\Delta(M,F)$ P-value
	(1)	(2)	(3)	(4)	(5)	(6)
University education						
Communication	0.039	0.059	0.031	34	95	0.48
Economics	0.074	0.094	0.048	85	63	0.289
Computer Science	0.036	0.044	0	45	10	0.50
Having a job						
Communication	0.152	0.227	0.128	22	70	0.26
Economics	0.272	0.327	0.211	58	52	0.176
Computer Science	0.382	0.392	0.333	28	6	0.79
Mother works						
Communication	0.07	0.176	0.03	34	95	0.00
Economics	0.102	0.095	0.111	84	63	0.75
Computer Science	0.31	0.35	0.1	45	10	0.11
Men better executives						
Communication	0.165	0.42	0.07	33	94	0.00
Economics	0.24	0.36	0.08	85	63	0.00
Computer Science	0.145	0.177	0	45	10	0.15
Jobs scarce						
Communication	0.033	0.034	0.034	29	89	0.98
Economics	0.074	0.082	0.066	61	60	0.75
Computer Science	0	0	0	39	10	-

Notes: The table shows gender differences in attitudes toward gender stereotypes within the faculties of Communication, Economics and Computer Science. We focus on the following survey questions: "A university education is more important for a boy than for a girl" , "Having a job is the best way to gain independence for a women", "When a mother works for pay, children suffer", "On the whole, men make better business executives than women", and "When jobs are scarce men should have more right to a job than women". We report the share of students enrolled in a given faculty who agree with the statement, in total (column 1) and by gender (columns 2-3), the number of students filling out the survey by gender (columns 4-5), and the P-value of a two-sample t-test for differences between female and male answers (column 6).

Table 2.A.3: Summary Statistics of MTurk Experiment: Balanced versus Unbalanced (Female Scarce)

Group	Balanced			Unbalanced			(B-U)
	No.Obs	Mean	Std.Dev	No. Obs	Mean	Std.Dev	P-value
Panel A: Permutation variables							
Math Task	743	0.47	0.50	735	0.47	0.50	0.886
Margaret First	743	0.49	0.50	735	0.50	0.50	0.756
Margaret TA	743	0.49	0.50	735	0.52	0.50	0.404
Panel B: Sociodemographic variables							
Female	743	0.45	0.50	735	0.48	0.50	0.18
Age	743	35.78	11.32	735	36.36	11.41	0.33
White	743	0.77	0.42	735	0.76	0.43	0.76
College degree	743	0.60	0.49	735	0.61	0.49	0.59
Post-graduate degree	743	0.30	0.46	735	0.31	0.46	0.75
Panel C: Subjects' performance							
Margaret chosen	743	0.63	0.48	735	0.69	0.46	0.013
Duration	743	819.90	352.43	735	840.87	502.71	0.353
No. of advices	743	4.35	2.70	735	4.25	2.77	0.472
No. of correct answers	743	7.03	3.48	735	6.97	3.36	0.732

Notes. The group "Balanced" includes all subjects exposed to a gender balanced pool of instructors, while the group "Unbalanced" includes all subjects exposed to a pool of six male instructors. For each variable of interest, we report the number of observations, mean and standard deviation. The last column reports P-values of a t-test between variables in control and treatment group.

Table 2.A.4: Summary Statistics of MTurk Experiment: Balanced versus Unbalanced (Male Scarce)

Group	Balanced			Unbalanced			(B-U)
	No.Obs	Mean	Std.Dev	No. Obs	Mean	Std.Dev	P-value
Panel A: Permutation variables							
Math Task	743	0.47	0.50	699	0.47	0.50	0.76
Margaret First	743	0.49	0.50	699	0.50	0.50	0.83
Margaret TA	743	0.49	0.50	699	0.50	0.50	0.80
Panel B: Sociodemographic variables							
Female	743	0.45	0.50	699	0.42	0.49	0.29
Age	743	35.78	11.32	699	34.67	10.32	0.051
White	743	0.77	0.42	699	0.75	0.44	0.28
College degree	743	0.60	0.49	699	0.70	0.46	0.00
Post-graduate degree	743	0.30	0.46	699	0.20	0.40	0.00
Panel C: Subjects' performance							
Richard chosen	743	0.37	0.48	699	0.52	0.50	0.00
Duration	743	819.90	352.43	699	827.60	354.50	0.68
No. of advices	743	4.35	2.70	699	4.69	2.58	0.01
No. of correct answers	743	7.03	3.47	699	7.20	3.23	0.34

Notes. The group “Balanced” includes all subjects exposed to a gender balanced pool of instructors, while the group “Unbalanced” includes all subjects exposed to a pool of six female instructors. For each variable of interest, we report the number of observations, mean and standard deviation. The last column reports P-values of a t-test between variables in control and treatment group.

Table 2.A.5: Choice of Male Instructor when Male Instructors are Scarce

	(1)	(2)	(3)
Unbalanced (β)	0.136***	0.131***	0.161***
	(0.035)	(0.034)	(0.041)
Female×Unbalanced (γ)			-0.064
			(0.053)
Math Task	-0.005	-0.012	-0.012
	(0.028)	(0.028)	(0.028)
Margaret First	0.087**	0.089***	0.087***
	(0.033)	(0.033)	(0.032)
Margaret TA	-0.016	-0.013	-0.014
	(0.038)	(0.039)	(0.039)
Female		-0.020	0.012
		(0.028)	(0.036)
Age		-0.002	-0.002
		(0.001)	(0.001)
White		0.027	0.025
		(0.033)	(0.033)
College Degree		0.082*	0.085*
		(0.046)	(0.046)
Post-graduate Degree		0.026	0.028
		(0.046)	(0.046)
Constant	0.334***	0.338***	0.320***
	(0.044)	(0.069)	(0.070)
$\beta + \gamma$			0.096**
			(0.045)
R-squared	0.029	0.037	0.038
N	994	994	994

Notes. The dependent variable is a dummy equal to one if Richard is chosen. Unbalanced is a dummy equal to one if the subject is exposed to a pool of six female instructors, and zero if he/she is exposed to a gender balanced pool of instructors. All included subjects checked the advice by the chosen instructor. Robust standard errors are reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Chapter 3

Can sport events foster electoral participation? Evidence from Switzerland

3.1 Introduction

Sport events are one of the most common occasions for social interactions, since they gather peers together both at the stadium and outside. In the first place, they offer an almost unique situation in which a multitude can experience a vast spectrum of shared emotions, including extreme ones, like euphoria or rage. Secondly, they may be a chance, especially in small towns, for single individuals to approach their community, and cement their group identity. Yet, it is not known to which extent these social events may influence collective decisions, like political ones. A legitimate question is whether these episodes can boost voter participation in elections. In this paper, I investigate this question in the Swiss context of direct democracy, comparing turnout in referenda anticipated by popular sport events - hockey and football matches - with referenda that are not. I assembled a cantonal level dataset which includes information on voter participation to all the federal propositions from 1950 to 2018, as well as information on all the hockey/football matches played in a week before each vote. I find that referenda anticipated by hockey matches (in the week before the vote) exhibit an increase in turnout of 2 pp. By exploiting a policy change, namely the introduction of postal voting, I find that the effect of sport on turnout is significant in the years before the introduction of postal voting, and smaller in size (although not in a significance way) in the years after the introduction of postal voting. This is reasonable and in line with my expectations. Indeed, only before the introduction of postal voting, people can all be potentially exposed to the sport events. After the introduction of postal voting, only a small fraction of them is voting on Sunday at the polls, while the majority is voting by post, not necessarily in the referendum day but before as well.

Therefore, once vote by post is allowed, there are smaller chances that people saw the match before the voting. When I replicate the analysis for football, I don't find any positive effect of the matches on turnout. In addition, I rely on new geo-localized data documenting, for a subsample of cantons, the municipalities of residence of hockey and football teams supporters. I use these data to justify the hypothesis that the effect of sport events should be stronger in municipalities that are closer to the stadium, where more fans are located. My analysis with municipal turnout provide evidence in line with this hypothesis for hockey matches. However, I don't find that football matches are driving an increase in turnout. These analysis, however, only capture an average effect, and it could mask important heterogeneity, for example related to the type of the match, and to the size of the city. Other then limiting the analysis to the aggregated pool of municipalities, next I focus on particular subsamples, to draw some intuition on the mechanism behind this increase in turnout. In this spirit, I analyze two alternative channels that could explain the relationship between turnout and sport events, namely an emotional reaction to sportive achievements/failures, and an increase of social pressure fostered by sport meetings. Despite their volatility, emotions have an impact also on long run choices. According to Loewenstein (2000), there are three main classes of behaviours which can be strongly affected by visceral factors, namely bargaining behaviours, intertemporal choices, and decision making under risk and uncertainty. With respect to the setting analyzed in my paper, experiencing a winning may boost people confidence and optimism, and let them think they can make the difference with their vote. On the other side, bad mood due to a sport failure, can lead eligible voters to underestimate their chance to be pivotal. If emotions play a role, I would expect victories to amplify the positive effect of sport events on local turnout, while defeats to attenuate it. However, this hypothesis is not confirmed by my data: none of the outcome is driving alone the effect on turnout. On the contrary, evidence is more in line with another explanation, namely an increased social pressure due to sport meetings. As the pioneer experiment by Gerber et al. (2008) demonstrated and many other studies confirmed, social pressure can stimulate turnout (Funk, 2010; Gerber et al., 2010; Panagopoulos, 2010; Davenport, 2010; Mann, 2010). Moreover, some scholars speculated on the interaction between social pressure and community size. The hypothesis of a negative relationship directly follows from the theory of collective action (Olson, 1986), which suggest that individual incentives to social contributions decrease with the size of the group. Indeed, several empirical studies documented negative correlation between civic participation and group size, although uncertainty remains on the exact channels through which social pressure operates in small communities[1]. Following this theoretical framework, I exploit variation in the size of Swiss

[1]Among others, Oliver (2000) considers as key the differences in inhabitants social relations and psychological orientation, across cities and small towns. In a different perspective, Funk (2010) gives more relevance to the higher prospects -in small municipalities - for public surveillance of civic involvement.

municipalities, finding that the positive effect of matches on turnout is stronger in towns with less that 1000 inhabitants.

This paper relies on three strands of literature. First, I add to the literature on sport events and their effects on people's behaviour. Several studies documented a causal link between sport games and violence. Among them, the influential paper by Card and Dahl (2011) shows that unexpected losses of professional football teams in US are associated to an increase of 10% in domestic violence. Financial behaviour can also be affected by games outcome, trough sudden changes in investor mood (Edman et al., 2007). Regarding political decisions, the seminal paper of Healy et al. (2010) documents that football supporters are more likely to vote against incumbent politicians, when elections take place right after a defeat of the local team. However, they do not find any effect of the match on electoral participation. To my knowledge, mine is the first study to document an effect of sport events on turnout.

Second, I relate to previous work on the role of shared emotions in building group identity, and civic involvement. Based on a field experiment, Clingingsmith et al. (2010) document that participation to Hajj pilgrimage to Mecca increases observance of Islamic religion, but it also strengthens beliefs in harmony among different religious groups, and in gender equality. More recently, Depetris-Chauvin et al. (2020) find that experiencing football national team victories in sub-Saharan Africa increase group identity, at the same time fostering trust across different ethnic groups, and reducing civil conflicts. Finally, Madestam et Yanagizawa-Drott (2012) suggest that exogenous participation in the Fourth of July US celebrations in childhood, change political ideology toward Republican party and increase turnout later on in life. I do not find, however, that in the Swiss context of hockey and football matches emotions are key in explaining the documented increase in voter turnout.

Finally, I contribute to the empirical research on the determinants of voting participation. The standard rational choice theory considers turnout primarily as a function of cost and benefits associated to the act of voting (Aldrich, 1993; Blais, 2000; Blais, 2006). Consistently with these models, several studies confirmed that a reduction in the costs associated to voting (for example, shorter polls distance, elections over the weekends, introduction of postal voting, good weather) increase voters' participation (Gronke et al., 2007; Franklink, 2004; Dyck and Gimpel, 2005; Gomez et al., 2007; Fujiwara et al., 2017). However, recent strand of literature suggests that human motivation may play a role as well. In this vain, Della Vigna et al. (2017) show that social image is an important driver of turnout, i.e. people may vote to tell the others they did so. Funk (2010) also shed light on the importance of social concerns in voting behaviour. The study documents how the introduction of postal voting in Switzerland differentially affects larger municipalities compared to smaller ones. Theoretically, on the one hand postal voting generated a decrease in voting costs for all the municipalities. On the other hand, it made harder to verify if someone voted or not. Consequently, in smaller municipalities, where social pressure to vote is generally very high, it is possible that - with the introduction of postal voting - the second effect offset the first, driving turnout down. Indeed, the study reports a significant reduction of citizens attendance in federal elections driven by towns with less than 1000 inhabitants, where citizens interactions are more tight, and social monitoring easier to implement. Building on this literature, my paper suggests that social pressure can also be fed by sport events, which can indeed contribute to people involvement in local communities. The paper is organized as follows. Section 2 provides an overview of the empirical setting, documenting the Swiss voting system, and the hockey and football tournament regulation. Section 3 describes the cantonal and municipal level datasets. Section 4 presents the estimated equations and results. Section 5 discusses the potential mechanism behind the relationship between turnout and sport events. Finally, Section 6 concludes and discusses future developments of the study.

3.2 Institutional Setting

3.2.1 Swiss Referenda and Popular Initiatives

Switzerland is one of the few examples of direct democracy in the world. All the citizens aged 18 and above are called to decide over important political and social issues multiple times per year, at all levels of government (municipal, cantonal, and federal level). In this paper, I focus on federal propositions, since these votes involve all the cantons. Moreover, at this stage of the study, I am not including elections for identification reasons: they only occur every four years (always in the same month), while popular votes are more frequent and happen several times per year (spread across all the seasons). At the federal level, swiss citizens have three instruments to exercise their direct decision power over policy issues, namely popular initiative, optional referendum, and mandatory referendum. Popular Initiatives are launched to propose any change regarding the Swiss Constitution, and require the collection of 100,000 signatures within 18 months. Through the optional referendum, instead, citizens can put under scrutiny any bill approved by the Federal Government, by collecting 50,000 signatures within 100 days since the publication of the law. Finally, mandatory referenda aim at modifying international treaties, or approving constitutional amendments voted by the Parliament. Before all these votes, all eligible voters receive by post the official electoral ballot, and also an informative brochure about the object of the proposition, edited by the Swiss Federal Chancellery. The brochure includes details on the financial aspects of the issue, copy of the parliamentary discussion, debates of the interest groups, and the suggestion of the Federal Council whether to accept or reject the proposal. In the case of a mandatory referendum, a proposal is accepted if the majority of the population and the majority of cantons[2] are voting in favour. Instead, for an optional referendum, the popular majority is sufficient for the law to pass. Over the last decades, Swiss citizens voted in referenda and popular initiatives around four times per year, with an average turnout of 40%.

3.2.2 Hockey and Football Matches

Switzerland has a long tradition of Ice Hockey. As reported by the Federal Council, in the country there are more than 1,160 teams, playing around 14,000 matches each year. At a professional level, the first top tier is the the National League, followed by the the Swiss League. The National League, formerly called National League A, is particularly popular among sport supporters, and its games are characterized by high attendance rates. The Federal Council defines it as the "Premier-ice hockey division". The hockey season is concentrated in winter months, starting in September and closing in March. Since 1950, 13 cantons have been playing in the League, namely Appenzell Aussenrhoden, Bern, Basel Stadt, Friburg, Geneva, Graubunden, St.Gallen, Neuchatel, Ticino, Valais, Vaud, and Zug. The popularity of hockey varies across cantons. This is reflected by the differential number of cantonal teams playing in the National League since 1950 up to today. Over the last 50 years, the Cantons more represented in the league were Graubunden (with four teams), Zurich (with four teams), and Bern (with three teams). Following, Neuchatel, Ticino, Vaud and Valais count two teams each, and all the others have only one team[3]. Canton Bern ranks first for the number of National league championships won (20 titles), followed by Graubunden (19 titles) and Zurich (13 titles)[4]. The rules of the championships changed several times over the years. In the most recent setting, the hockey season consists of three stages: regular season, play-off and play-out. Each of the twelve teams takes part in the regular season, facing all other teams for a total of 50 matches. Play-off and Play-out follow the regular season. The best eight teams of the regular season, access the play-off. Each team faces another one[5] in a series of matches according to the best of seven formula: the team that wins four matches progresses to the next round, while the other is eliminated. This elimination formula proceeds until only two teams are left to play for the title in the final. Simultaneously, the bottom four teams of the regular season participate in the

[2]In the counting for the majority of the cantons, the results in the former half-cantons of Obwald, Nidwald, Basel-Stadt, Basel-Landschaft, Appenzell Ausserrhoden and Appenzell Innerrhoden count as only half a vote.

[3]This statistics aggregate the total number of teams, who played across different years, by canton.

[4]Data are updated until winter 2019.

[5]The first in the ranking with the last one, the second with the 7th, the third with the 6th and the 4th with the fifth.

play out: each team faces all the others twice, for a total of six matches, summing up this new score to the one accumulated in the regular season. Finally the worst two face each other in a best of seven competition. The losing one plays against the champion of the Swiss League[6], for the access to the next season of the National League. The best eight ones then play the playoffs - i.e. the best-of-seven games, and those in the last four places play the playoffs. Finally, the looser of the play-out has to face the champion of the playoff in a best of seven games for his qualification in the next season. Equally popular, football counts over 1400 clubs in Switzerland, and the matches of his top tier league, known as the Super League, were watched by a total of 2 million spectators in the season 2018/2019. Football Super League, compared to the hockey National League, enumerates a higher number of cantons who have played across the years (Aargau, Bern, Basel Stadt, Friburg, Geneva, Jura, Luzern, Neuchatel, Schaffhausen, Solothurn, St.Gallen, Ticino, Valais, Vaud, Zug and Zurich). Also the number of teams per canton is higher in football: Bern has five teams, followed by Geneva, Ticino, and Zurich (which have four), Argau, Neuchaetel and Vaud (with three), Basel Stadt, Friburg, Lucern and St.Gallen (with two). The remaining five cantons have one team.[7] The most successful cantons are Zurich and Basel, who won 24 and 20 titles. The season normally lasts one year and goes from July until May next year, usually with a break in January which split the season in two parts. Each team has to play with any other team once away and once at home, once in the first part of the season (first round) and once in the second (second round). The rank of the teams is made based on the following score system: a winning gives 3 points, a draw just one point, and a loss zero points. The first team wins the cup, while the last one is downgraded to the inferior tier, namely Challenge League.

3.3 Data

3.3.1 Cantonal level data

The cantonal level database combines data on the federal referenda with information on sport events anticipating the vote in a window of a week. Information on referenda, provided by the Swiss Federal Statistical Office, includes eligible voters, turnout, share of votes in favor of (and share of votes against) the proposal. The dataset includes 481 popular votes, taking place in 198 different referendum days (on Sunday) from 1950 to 2018. In case of multiple issue votes, turnout is collapsed at the referendum day-canton level, taking the average cantonal turnout of the different issues scrutinized in the same date. This choice is motivated by the fact that voter turnout of referenda scheduled on the same date tend to be very correlated. Concerning the sport events,

[6]The Swiss League is the second professional league in Switzerland for professional Hockey

[7]This statistics aggregate the total number of teams, who played across different years, by canton.

for each canton-referendum date observation, I report the number of football and hockey matches played by the canton team/teams in a window of seven days before the referendum. Data were downloaded by open access webpages www.diretta.it and www.hockeyarchives.com for hockey and www.diretta.it for football. I generate a dummy $TreatmentHockey$ ($TreatmentFootball$) equal to one, if the referendum in a given canton is anticipated by at least one hockey (football) match in the seven days before the vote, and zero otherwise.[8] Figures 3.1 and 3.2 show the variables $TreatmentHockey$ and $TreatmentFootball$ collapsed at canton-year level, over the last seventy years. Finally I merged the data with canton-year varying demographic characteristics, provided by different units of the Swiss Federal Office of Statistics. Most variables, especially for the early years, were available by decades, so I interpolate them linearly and with a cubic form, to fill the missing values. Covariates include the share of people in different age categories, the share of people living in urban areas, the share of high school diplomas for people aged 15-19, the share of urban population,and the share of unemployed population. Table 3.1 summarizes the sample, which covers the time span 1950 -2018.

[Table 3.1 here]

Turnout is 45% on average; however, given the high frequency of Swiss referenda, participation tends to fluctuate a lot depending on the saliency of the issue under scrutiny. Within cantons, 48% of the times the majority of the poulation approved a vote.[9] Regarding the sport games, only 12% of the referenda are anticipated by a hockey match, while 30% of them are anticipated by a football match. This higher frequency of football matches in the sample is due to both the longer Football season, and the higher number of teams per canton playing in the football league, compared to the hockey league. Finally, $PostalVoting$ is coded as a dummy 0 for the years before the introduction of postal voting, and 1 for the years after. The timing of the introduction of postal voting varies across cantons. The first canton to introduce postal voting was Basel-Land in 1978, followed by many others during the 80's. Some late "introducers" adopted the new system only after 1994, when the federal Swiss law was changed to require the remaining cantons to set up Postal Voting. Since 2006, all cantons offer this option (see Table 3.A.1 in the Appendix for more information about the introduction of postal voting.)

[8] I exclude matches taking place the same day of the referendum, i.e. if a match is played the same Sunday of the referendum, I consider the vote as unaffected by the match.

[9] This statistics is not reported in summary statistics, since table 3.1 refers only to collapsed referendum-day data.

3.3.2 Municipal level data

To precisely estimate the impact of sport events, I had to complement the cantonal analysis with a municipal level one, in order to verify if the effect of matches is particularly stronger in municipalities hosting a higher number of sport fans residents. There are no registries of fans publicly available, so I asked all hockey and football Swiss clubs to have access to their private data. Only four clubs for hockey (HC Lausanne, HC Friburg, HC Zurich, and HC Kloten) and four clubs for football (FC Bern, FC Luzern, FC St.Gallen, and FC Neuchatel), sent me the anonymized list of all their fans who subscribed to the yearly abonnement, with their postal code of residence. For this subsample of cantons, I assembled a unique municipal level database containing referendum municipal turnout[10], and the number of fans who are resident in each municipality. Figures 3.3 and 3.4 display the concentration of fans in Swiss municipalities with different color intensity. For hockey, all the fans are concentrated within the canton of the team, while for football there is a little bit more dispersion, i.e. there are fans who are located in different cantons, other than the team one (Ex: Football Club Bern, Football Club Luzern). Consistently across sports and cantons, the dots are darker the closer is the municipality to the city where the sport stadium is located, suggesting a higher presence of fans in the team headquarter. Based on this evidence, I include in the local analysis the entire population of municipalities (in cantons who ever played any of the two sports) by proxying the actual concentration of fans with the distance of the town from the sport stadium (See table 3.A.2 in the Appendix for information on the location of Hockey/Football Stadium). Basically, I group municipalities within each canton in different subsamples, based on their distance from the Hockey/Football Arena. I then generate a variable treated municipalities, which is coded 1 if the municipality belongs to the bend around the stadium and the referendum is anticipated by a match, and zero if the municipality is outside of the bend, precisely on the cantonal border. Additional details of the construction of this last dataset are reported in Section 5.

3.4 Results

3.4.1 Cantonal analysis

The first goal of my analysis is to verify if sport events have any impact on whether people vote, comparing turnout in cantons that had a match before the vote, with cantons who did not have a match before the vote, controlling for unobserved differences that are canton specific. Table 3.2 reports my results. In column 1 and 2, Canton Fixed effects allow me to compare the same canton

[10] Municipal turnout was provided by the Swiss federal statistical office

in referenda anticipated by a match with referenda that are not. Referendum fixed effects absorb the variation that comes from different votes. In the first two columns, I also control for canton-year demographic variables. In column 3, I rely on canton-year fixed effects, namely I compare - within the same year and canton - referenda anticipated by a match with referenda that are not. [11] In this last specification, I exploit the different frequency of matches over the year before referenda, hence I may capture canton specific seasonality. However, sport and football occur in very different seasons, so treated (and control) referenda are identified by votes happening in different months for the two sports, as pictured in figures 3.5 and 3.6. In conclusion, if a similar patter emerges from both sports in the canton-year specification, it should not be driven by seasonality. Note that I can only precisely estimate the effect of sports events before the introduction of postal voting. After the introduction of postal voting, the intention to treat effect cannot be precisely estimated, since people may have voted before the match. Therefore I control for a postal voting dummy, and I interact it with all the other variables included in the model. This choice is motivated by the fact that postal voting, through its impact on the cost of voting (Luechinger et al. (2015)), may have affected the composition of the electorate; it seems then reasonable to allow for a differential impact of all the demographic controls before and after the introduction of postal voting.

The estimated regression equation in the cantonal analysis is:

$$y_{cdy} = \alpha + \beta * Treatment_{cdy} + \gamma * PostalVoting_{cdy} + \delta * Treatment_{cdy} * PostalVoting_{cdy} + X_{cdy} + \varepsilon_{cdy} \tag{3.1}$$

where $Turnout_{cdy}$ is the number effective voters over the number of eligible voters in canton c, in referendum in day d, in year y. $Treatment$ is a dummy 1 if there was at least 1 hockey (football) match in the seven days before the vote, and zero otherwise.[12] $PostalVoting$ is a dummy 1 if the canton c in referendum day d has already introduced postal voting, 0 otherwise. The vector X_{cdy} includes all the year-canton demographic covariates.

The results of the cantonal analysis are displayed in Table 3.2. Treatment Hockey, capturing the effect of hockey before the introduction of postal voting, is always positively correlated with Turnout. Precisely, Turnout is 2 pp higher in referenda anticipated by a match compared to referenda which are not. Results are statistically significant in column 1 and 2, and barely significant in the most stringent specification of canton-year fixed effects, in column 3 (P-value 0.105). About the timing of the effect, if anything, I would have expected an equal or slightly lower coefficient for Treatment Hockey after postal voting for two reasons. First, with postal voting, exposure

[11] The underlying assumption is that the timing of the matches - played in a window of seven day before the referendum - is exogenous with respect to referendum itself. In other words, I want to avoid that the timing of the match is correlated to some omitted factors that may influence turnout in referenda.

[12] I regress separately Turnout on TreatmentHockey and on TreatmentFootball.

to treatment (exposure to matches) is less likely to happen before the vote. Second, with the introduction of postal voting, social pressure is for sure less relevant and social events as sport games may not be anymore a pull factor to increase turnout.[13] $TreatmentHockeyXPostalVoting$ is negative, but not statistically different from zero, hence I cannot reject the hypothesis of equality of coefficients before and after the introduction of postal voting. For football, there is no significant impact of sports on turnout in none of the specifications.

[Table 3.2 here]

3.4.2 Municipal analysis

This evidence, although suggestive of a positive correlation between sport events and turnout, calls for a deeper investigation which exploits municipal level data and fans location. The rest of the analysis focuses on the years before the introduction of postal voting to estimate the coefficients of interest more precisely. In the municipal analysis, I want to investigate whether the effect of sport events, within the same canton, is stronger in municipalities where a higher number of fans is located.

In order to test this hypothesis, I build an additional dataset in which I proxy the fans distribution within cantons using the municipal distance from the stadium. As shown in the figures 3.7 and 3.8, the number of fans decreases with distance of the commune from the stadium. For Hockey, there is usually a drastic drop in the supporter concentration after 30-40 km, while for football after 20-25 km. Based on this evidence, I assume that if there is a match, and this has an effect on turnout, the effect should be bigger in municipalities within those bands of kilometers, and should decrease the farther we move away from the stadium, and we approach the cantonal borders. Therefore, in the upcoming analysis, I take communes at the cantonal /state boundaries as control group, i.e. I assume - in an extreme hypothesis - that very peripheral municipalities are not affected at all by the matches. Then, I group municipalities in 10 bands, based on their location. The first group includes all the municipalities within 5 km from the stadium, the second all the municipalities within 10 km, and so on, till the last bandwidth of 50 km. For each group, I run a separate regression, comparing municipalities in treated referenda (those anticipated by a match of the canton team) located within the band, with municipalities at the boundaries of the canton.

Each of the regressions performed take the following form:

[13] This last consideration is based on the hypothesis that social pressure is the main mechanism behind this increase in turnout. In the rest of the paper, I also consider some alternative channels as potential drivers of the results.

$$Turnout_{mbcdy} = \alpha + \beta * TreatedMunicipality_{mbcdy} + \varepsilon_{mcdy} \quad \text{with} \quad b \in (5km, 50km) \qquad (3.2)$$

Tables 3.3 and 3.4 document the impact of the sports on turnout including all the municipalities in all cantons playing before postal voting. I run separate estimates for different bandwidths, documented above each column. I display results from three specifications, namely i) canton fixed effects, and Referendum-day fixed effects (Panel A), ii) canton-year fixed effects (Panel B), canton - day fixed effects (Panel C). For Hockey, Table 3.3 shows a positive effect in all the specification, and, as expected, coefficients are decreasing from smaller to larger bandwidths.[14] For Football, there are no significant effects (see Table 3.4).

[Tables 3.3 and 3.4 here]

3.5 Mechanism

In this section, I try to test for two different channels that could explain the positive effect of sport events on turnout, namely an emotional reaction to specific outcomes of the games, and an increase in social capital or social pressure. One the one hand, winnings could generate a sense of patriotism and closeness to the community, which may translate into an higher participation in civic life, and, as an expression of this, in political referenda. On the other hand, it is also possible the increase in turnout is simply driven by the fact of being together at the event, irrespectively of the outcome of the competition. These meetings could be an occasion to exchange information not only on sport, but on political and civic life as well, fostering spillover of knowledge. Also, social pressure to vote in referenda could increase due to these meetings happening few days before the election.

To test for the emotional reaction channel, I analyze whether the effect of sport on turnout is driven by any specific outcome of the match. If this channel plays a role, I would expect citizens to vote more if a victory is experienced, as discussed in Section 1. I regress turnout on the share of won, lost and even games (over the total number of matches), having as a reference group municipalities at the boundaries. Moreover, I control for the share of matches played at home. Regression equation is the following:

$$Turnout_{mbcdy} = \alpha + \beta * ShareWinner_{mbcdy} + \gamma * ShareLost_{mbcdy} + \delta * ShareParity_{mbcdy} + \sigma * ShareHome_{mbcdy} + \varepsilon_{mcdy} \quad \text{with} \quad b \in (5km, 50km) \qquad (3.3)$$

[14] I also performed a placebo test, namely I compare municipalities within the bandwidths with those at the boundaries for referenda that are not anticipated by any match. I find that municipalities close to the stadium vote more than those close to the bandwidth when there is no match, but this difference is larger if there is a match.

Voter participation seems not to be driven by an emotional reaction to specific outcomes of neither hockey or football games. Note that, indeed, in table 3.5 a positive increase in turnout is driven by all type of games (lost, won or even). For football, Panel B display positive coefficients both for won and lost games, but results are not stable across the other two specifications.

[Tables 3.5 and 3.6 here]

Finally, Tables 3.7 and 3.8 investigate whether the impact of the sport events on turnout is particularly strong in smaller municipalities, as the social pressure hypothesis would predict. For each specification, I run the estimates separately for the samples of municipalities below and above 1000 inhabitants. Note that, consistently across the two sports, the coefficients are statistically significant and positive mainly in the upper panels (Less than 1000 inh.), while their explanatory power and their magnitude tend to decrease in the lower panel (more than 1000 inh.). As alternative thresholds, I also split the sample in municipalities with more than 1500 and 2000 inhabitants. Consistently, coefficients for $TreatedMunicipality$ are larger and significant for the smaller towns (See tables 3.A.3 and 3.A.4 in the Appendix). In a more rigorous way, I also regress turnout on the dummies *Less than* 1000/1500/2000 *inh.* (each dummy equal to one for municipalities with less than 1000 inhabitants, 1500 inhabitants, and 2000 habitants), interacted with the variable $TreatedMunicipality$. While for hockey I cannot reject equalities of coefficients, for football, I can reject that coefficient of $TreatedMunicipality$ is the same for the two size groups of communes, since the effect of football matches is significantly higher for those municipalities with less - compared to more - than 1000/1500/2000 inhabitants.

[Tables 3.7 and 3.8 here]

3.6 Discussion and Conclusions

In this paper, I investigate whether sport events taking place before referenda can increase voter turnout, by bringing together peers of the same community. Both cantonal and municipal analysis confirm that hockey matches are associated to higher turnout. Although I cannot exactly document how the games determined the increase in electoral participation, preliminary evidence is suggestive in indicating social pressure as a possible channel. Indeed, I document a positive effect of sport events on turnout in municipalities with less than 1000 inhabitants. For football, the effect is significantly higher than in larger municipalities, consistently with the social pressure theory. These findings should be read in light of the more general discussion of electoral participation in Switzerland. Turnout in Swiss legislative elections has been significantly decreasing over time, and it is sharply lower, if compared to other developed countries in the world. In the last national elections, Switzerland exhibited the lowest turnout after Romenia among all european countries (figure 3.9). On the one hand, this can be due to the consociational model of democracy adopted by Switzerland, where all parties are asked to govern in a coalition, according to the spirit of cooperation. Moreover, ballot measures like referenda and popular initiatives represent more effective tools Swiss citizens have to affect political decisions. Hence, these direct democracy features may relax the personal motivation of voters to express support to parties. In referenda and popular initiatives, indeed, reduction in electoral participation over time is less evident. Turnout seems to fluctuate a lot, depending on the salience of the issue under scrutiny. However, as documented by the Swiss Federal Administration (figure 3.10), also average turnout for federal propositions was constantly declining from the early years of 1900 to the 80's, remaining stable till the years 2000. Only since then, there seem to be a slight increase in participation. Therefore, especially in a context in which electoral involvement is going down, it is important to investigate what drives turnout up, even in absence of specific policies targeting discouraged voters. It is not straightforward, however, to translate my findings into normative considerations. Whether or not the documented increase in turnout, driven by sport events, was beneficial for society, depends on the characteristics of the new voters, namely on their motivation, and on their level of knowledge. It is possible, though, that social gathering also let to exchange of information on the referendum political content. If so, the occurring of sport events, by generating positive spillovers of knowledge, and subsequently an increase in political participation of informed voters, may turn as beneficial for society. Further research is needed to investigate this relevant aspect.

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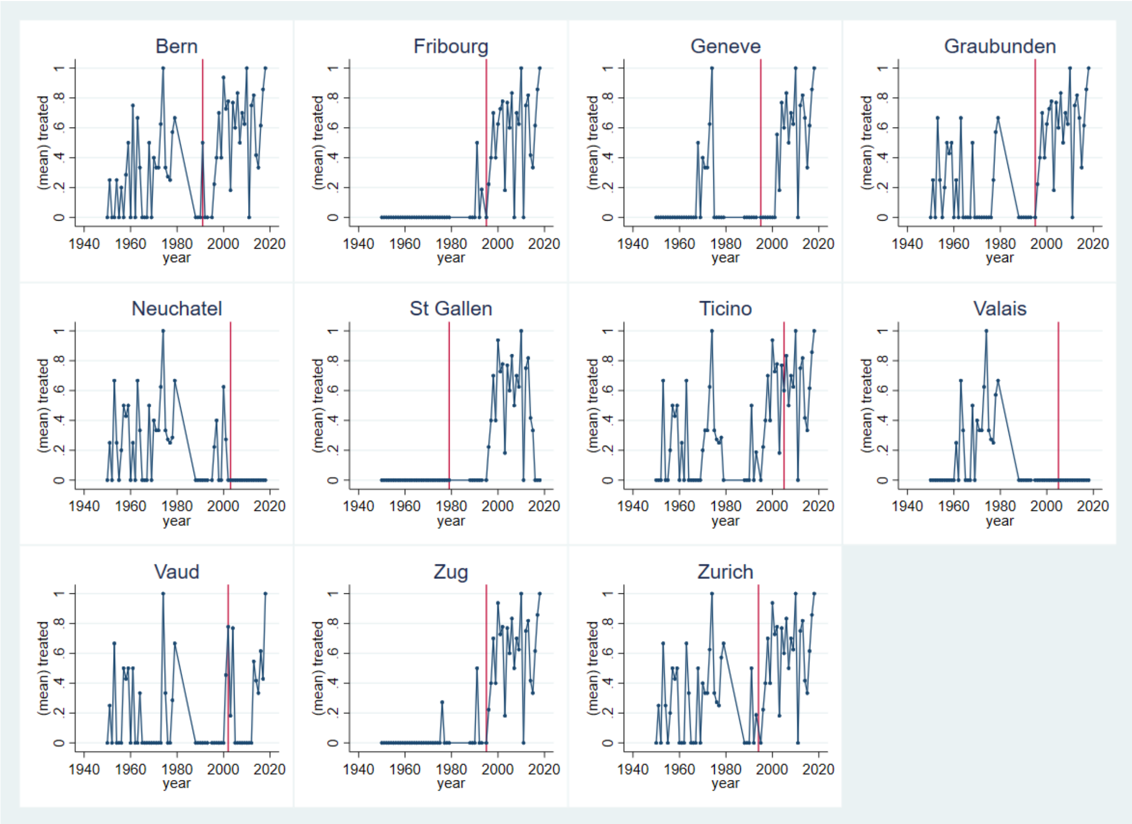
Figure 3.1: Treatment Hockey

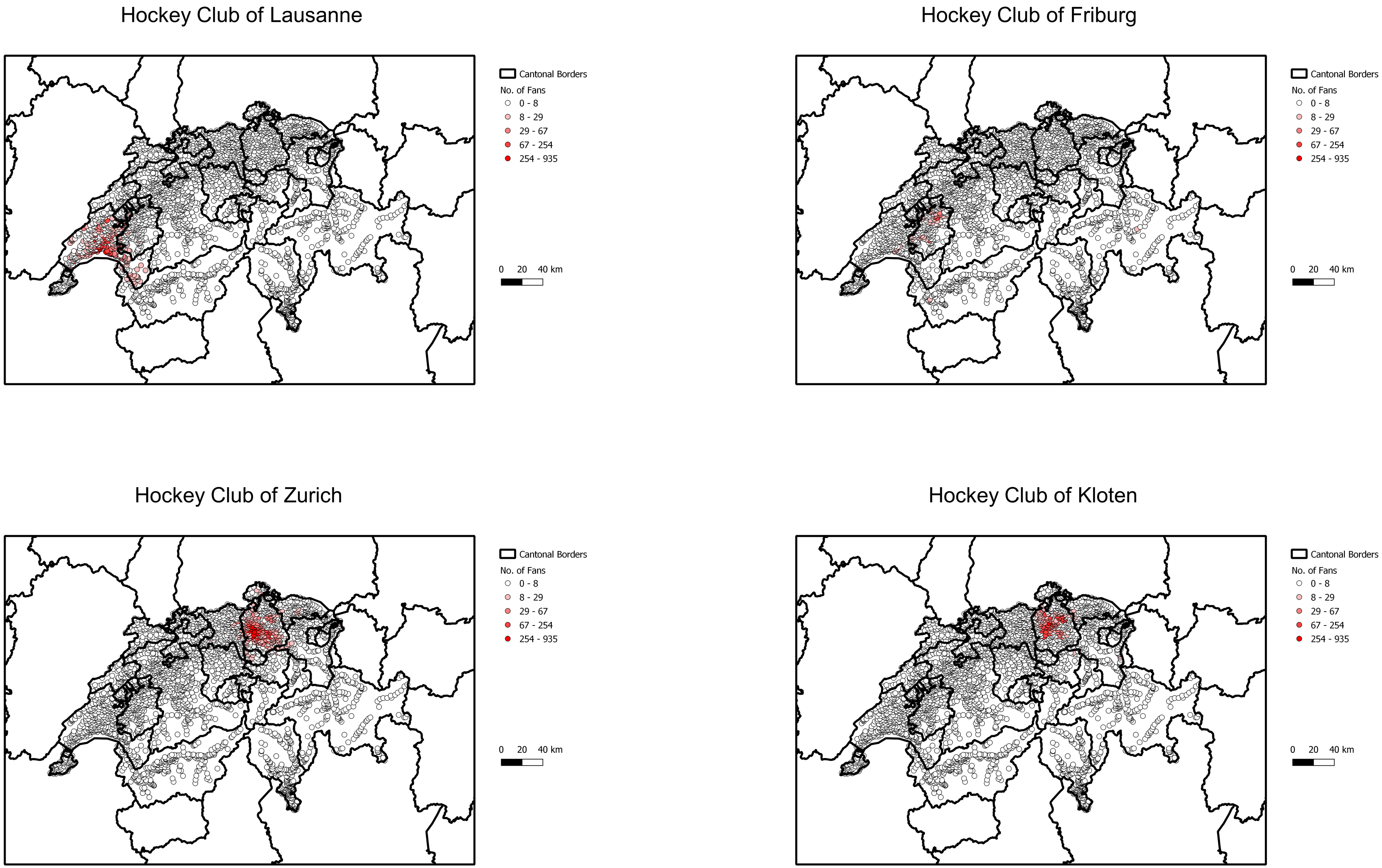


Notes. The figure shows the mean of Hockey Treatment, by year and canton. The variable Hockey Binary Treatment takes value one if a referendum is anticipated -within a week- by at least a match played by the canton team, and zero otherwise. The red line indicates the introduction of Postal Voting.

Figure 3.2: Treatment Football

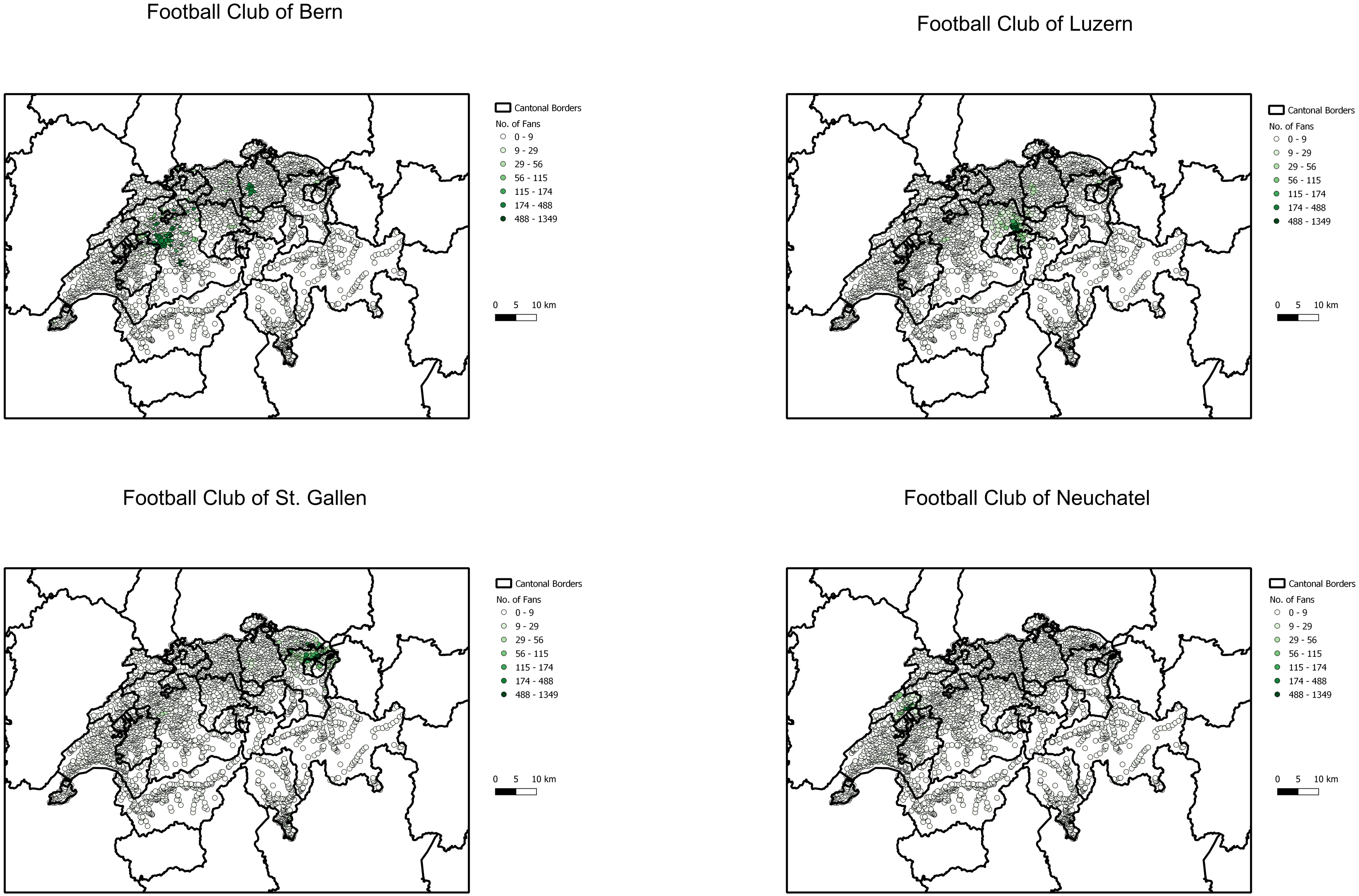
Notes. The figure shows the mean of Football Treatment, by year and canton. The variable Hockey Binary Treatment takes value one if a referendum is anticipated - within a week - by at least a match played by the canton team, and zero otherwise. The red line indicates the introduction of Postal Voting.

Figure 3.3: No. of Hockey Fans in Swiss Municipalities



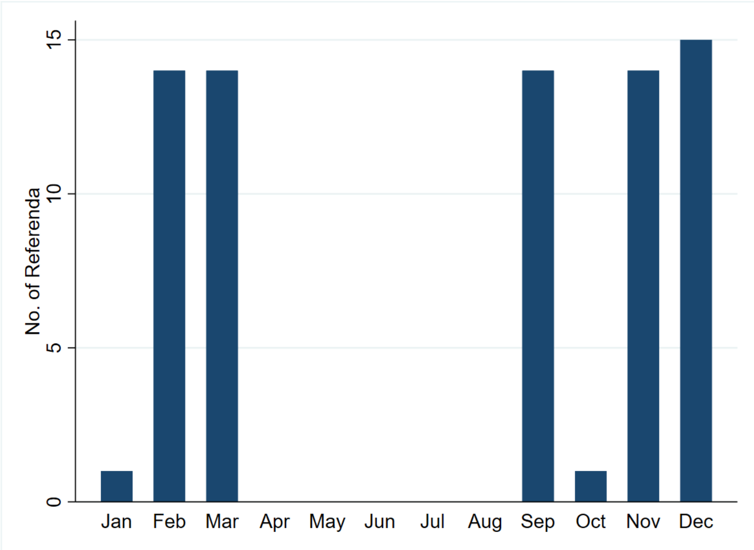
Notes. The figure shows the concentration of fans in Swiss Municipalities for four Hockey teams: Hockey Club of Lausanne (top-left), Hockey Club of Fribourg (top-right), Hockey Club of Zurich (bottom-left) and Hockey Club of Kloten (bottom-right). Each circle in the map indicates a municipality. Different colors signal different number of fans, as indicated in the legend.

Figure 3.4: No. of Football Fans in Swiss Municipalities



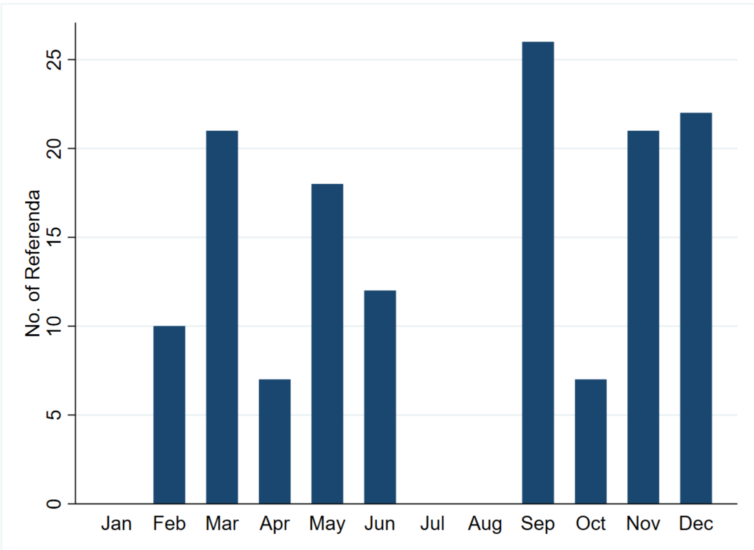
Notes. The figure shows the concentration of fans in Swiss Municipalities for four Football teams: Football Club of Bern (top-left), Football Club of Luzern (top-right), Football Club of StGallen (bottom-left) and Football Club of Neuchatel (bottom-right). Each circle in the map indicates a municipality. Different colors signal different number of fans, as indicated in the legend.

Figure 3.5: No. of referenda anticipated by hockey matches



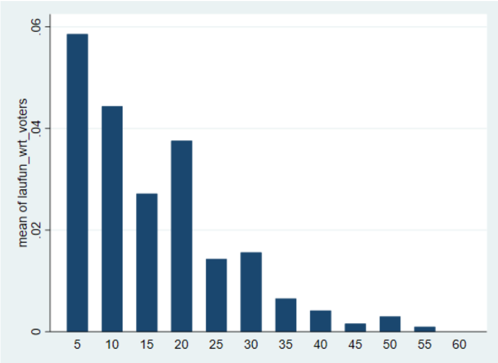
Notes. The figure shows, for each month of the year, the cumulative number of federal referenda anticipated -within a week- by at least one hockey match. The data refer to the time span between 1950 and 2018.

Figure 3.6: No. of referendum anticipated by football matches

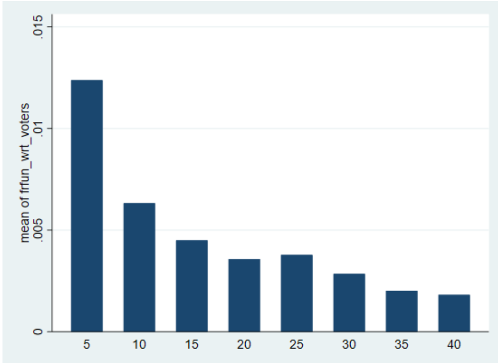


Notes. The figure shows, for each month of the year, the cumulative number of federal referenda anticipated -within a week- by at least one football match. The data refer to the time span between 1950 and 2018.

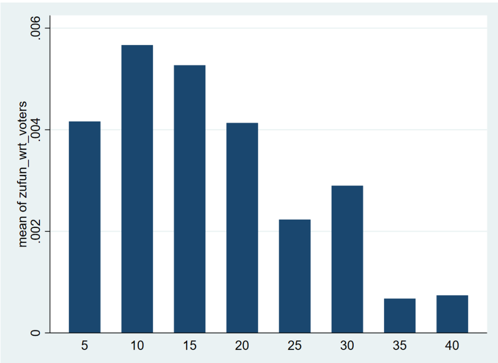
Figure 3.7: Share of Fans by Distance from Hockey Arena



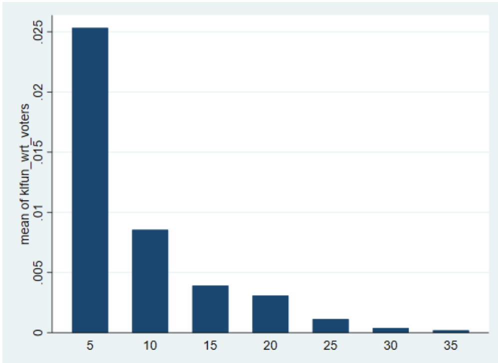
Hockey Club of Lausanne



Hockey Club of Friburg



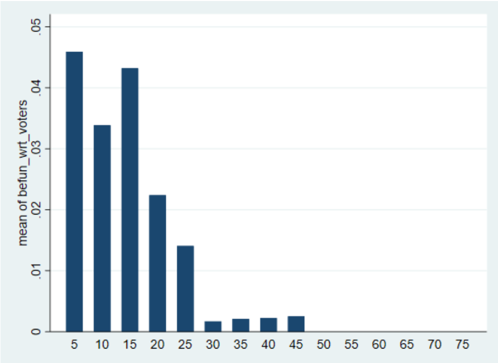
Hockey Club of Zurich



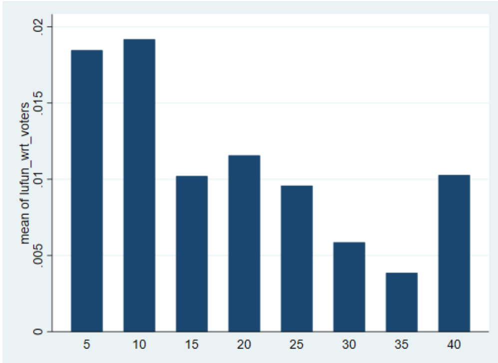
Hockey Club of Kloten

Notes. The figure shows the share of Fans over Eligible Voters, for municipalities located within a bandwidth of some kilometers from the Hockey arena. The bandwidth ranges from 5 to 60 km. Data on fans locations are available only for teams from cantons Vaud (Hockey Club of Luasanne), Fribourg (Hockey Club of Fribourg), and Zurich (Hockey Club of Zurich and Hockey Club of Kloten).

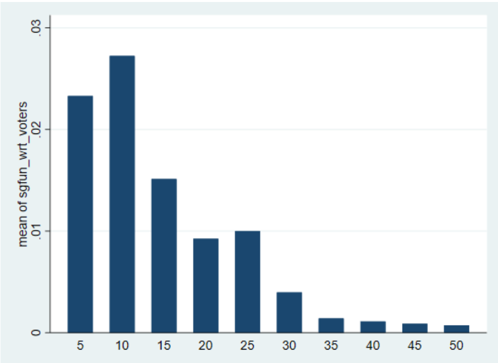
Figure 3.8: Share of Fans by Distance from Football Arena



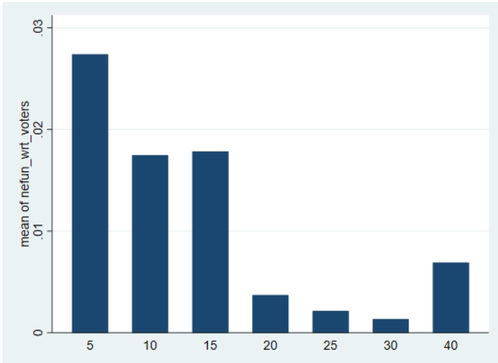
Football Club of Bern



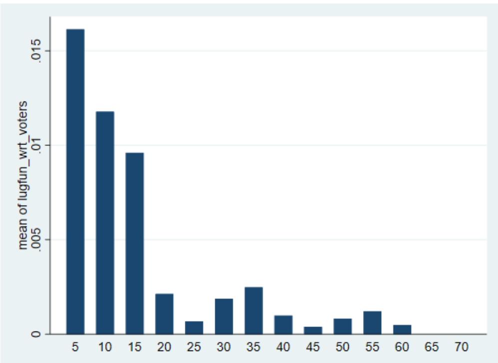
Football Club of Luzern



Football Club of St.Gallen



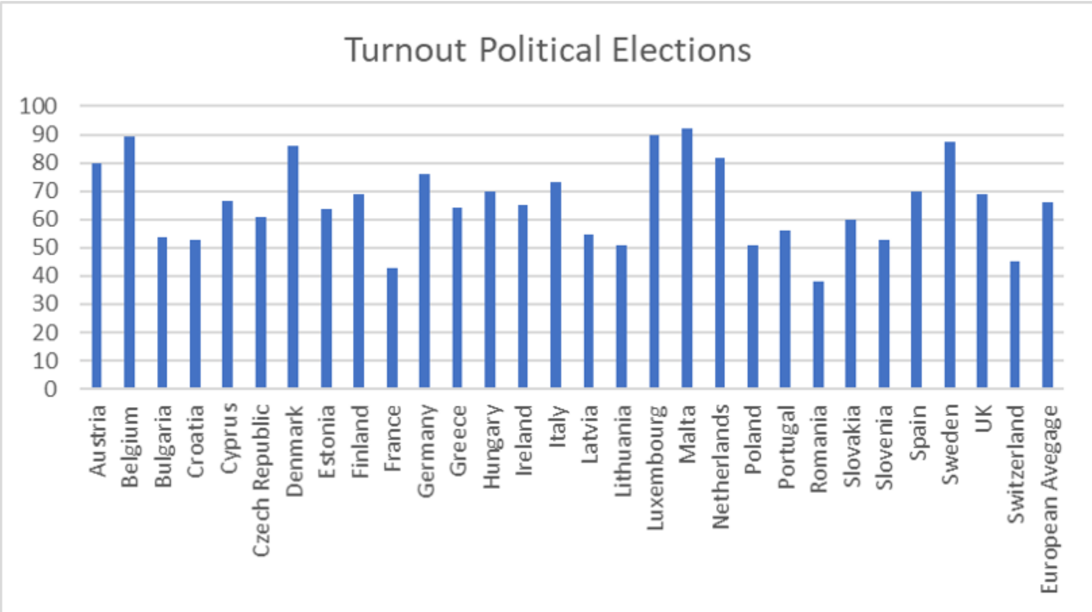
Football Club of Neuchatel



Footbal Club of Lugano

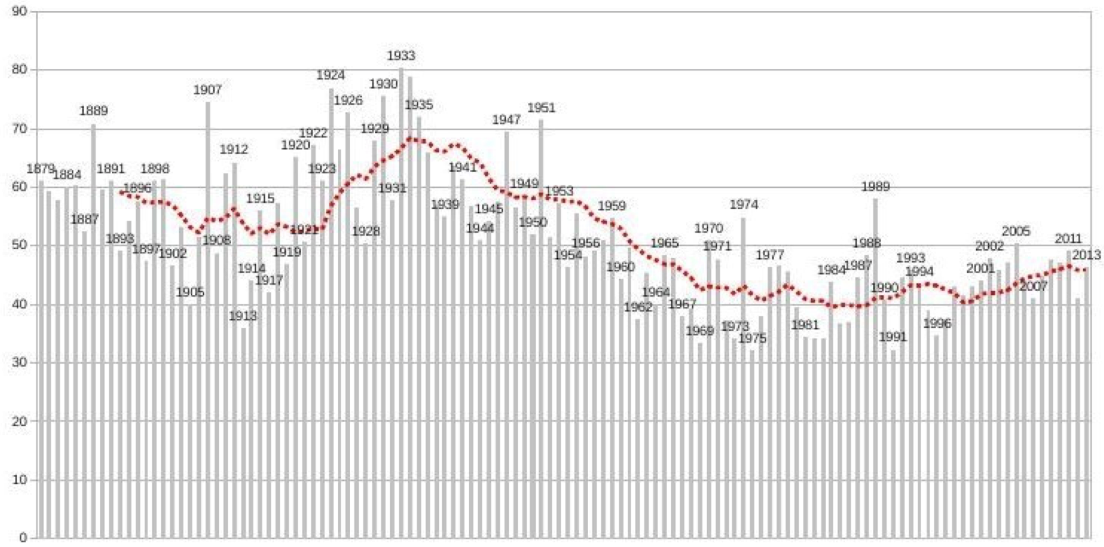
Notes. The figure shows the share of Fans over Eligible Voters, for municipalities located within a bandwidth of some kilometers from the Football arena. The bandwidth ranges from 5 to 60 km. Data on fans locations are available only for teams from cantons Bern, Luzern, St.Gallen, Neuchatel and Ticino.

Figure 3.9: Turnout in political national election in European countries



Notes. The figure shows turnout in last political elections, for each country. The Last two bars display Swiss Voter participation in the 2019 National Council Elections, and the average turnout of all European countries (excluding Switzerland).

Figure 3.10: Average turnout in referenda and popular initiatives, by year



Notes. The figure shows, for each year from 1879 to 2013, the average turnout of Swiss referenda, by year. Referendum votes took place in most but not all years. The gray bar represent average turnout, computed taking the mean of turnout of all ballot votes taking place on a specific date. The dotted red line is the 10-year moving average in percent. Source: www.c2d.ch and wwww.admin.ch

Table 3.1: Descriptive statistics

Variable	Mean	SD.	Min	Max	Obs.
Turnout	0.45	0.14	0.10	0.89	5065
Treatment Hockey	0.12	0.32	0.00	1.00	4472
Hockey Matches	2.73	1.61	1.00	10.00	521
Treatment Football	0.22	0.41	0.00	1.00	5148
Football Matches	1.48	0.77	1.00	6.00	1057
Postal Voting	0.37	0.48	0.00	1.00	5148
Panel A: cubic interpolation					
Share of people between 0 and 19 years old (in %)	0.27	0.06	0.15	0.42	5148
Share of people between 20 and 39 years old (in %)	0.28	0.03	0.22	0.35	5148
Share of people between 40 and 64 years old (in %)	0.31	0.04	0.23	0.39	5148
Share of people above 65 years old (in %)	0.14	0.04	0.03	0.23	5148
Share Urban Population (in %)	0.37	0.26	0.00	1.00	5148
Share High Education (in %)	0.06	0.08	0.00	0.35	5148
Share of unemployed population (in %)	0.01	0.02	0.00	0.17	5148
Panel B: linear interpolation					
Share of people between 0 and 19 years old (in %)	0.27	0.06	0.15	0.42	5148
Share of people between 20 and 39 years old (in %)	0.28	0.02	0.22	0.35	5148
Share of people between 40 and 64 years old (in %)	0.31	0.04	0.23	0.39	5148
Share of people above 65 years old (in %)	0.14	0.04	0.06	0.23	5148
Share Urban Population (in %)	0.37	0.25	0.00	1.00	5148
Share High Education (in %)	0.06	0.08	0.00	0.35	5148
Share of unemployed population (in %)	0.01	0.02	0.00	0.17	5148

Notes. The table reports summary statistics of the analysis at referendum-cantonal level. Data include turnout of all the federal referenda from 1950 to 2018, for cantons having a team playing hockey (13/26) or football (10/26), and cantons who did not have a team. Turnout is measured as the ratio of number of ballots and eligible voters. Treatment Hockey (Football) is a dummy taking value one if the federal referendum is anticipated -within a week- by at least one hockey (football) match of the canton team/s, and zero otherwise. No.Matches Hockey (Football) is the exact number of matches taking place during the week before a federal referendum. Postal Voting is a dummy one if the canton already introduced postal voting, and zero otherwise. Cantonal-year covariates are infant mortality (number of dead infants before reaching the age of 1 out of 1000 births), share of employed people out of the population (these people work 6hr/week or more), share of people in different age categories, share of people in urban areas, and share of high school diplomas for people aged 15-19.

Table 3.2: Cantonal Analysis: Binary Treatment for Hockey/Football matches

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Hockey	0.0287***	0.0287**	0.0225			
	(0.0100)	(0.0105)	(0.0134)			
Treatment Football				-0.0161	-0.0149	0.0026
				(0.0115)	(0.0110)	(0.0055)
TreatmentHockey X PostalVoting	-0.0155	-0.0175	-0.0187			
	(0.0262)	(0.0254)	(0.0136)			
TreatmentFootball X PostalVoting				0.0085	0.0059	-0.0124
				(0.0214)	(0.0208)	(0.0080)
Postal Voting	0.4956**	0.5700***		0.3667*	0.4271**	
	(0.2096)	(0.2013)		(0.1997)	(0.1945)	
Share of people between 40 and 64 years old (in %)	0.7501			0.6863		
	(0.5391)			(0.5644)		
Share of people above 65 years old (in %)	0.4389			0.2947		
	(0.4645)			(0.4487)		
Share Urban Population (in %)	0.0099			0.0085		
	(0.0577)			(0.0531)		
Share High Education (in %)	1.0659*			1.1882**		
	(0.5959)			(0.5506)		
Share of unemployed population (in %)	2.9390***			2.2667***		
	(0.6770)			(0.6219)		
Share of people between 40 and 64 years old (in %)		1.0103*			0.8949	
		(0.5217)			(0.5558)	
Share of people above 65 years old (in %)		0.4872			0.3485	
		(0.4670)			(0.4539)	
Share Urban Population (in %)		0.0149			0.0129	
		(0.0558)			(0.0510)	
Share High Education (in %)		1.2885**			1.3887**	
		(0.6215)			(0.5811)	
Share of unemployed population (in %)		2.8370***			2.1212***	
		(0.7113)			(0.6537)	
Constant	0.3802**	0.3039**	0.4586***	0.3855**	0.3234**	0.4617***
	(0.1514)	(0.1472)	(0.0013)	(0.1576)	(0.1560)	(0.0012)
R-squared	0.717	0.722	0.917	0.716	0.719	0.913
N	4389	4389	4389	5065	5065	5065
Referendum-Day FE	YES	YES	YES	YES	YES	YES
Canton FE	YES	YES	NO	YES	YES	NO
Canton X Year FE	NO	NO	YES	NO	NO	YES
Cubic Ipolation	YES	NO	NO	YES	NO	NO
Linear Ipolation	NO	YES	NO	NO	YES	NO

Notes. Data include turnout of all the federal referenda from 1950 to 2018 for all the Swiss cantons. Turnout is the ratio of ballots over eligible voters in a canton. Main independent variables are Treatment Hockey (Treatment Football), a dummy one if the referendum is anticipated -within a week- by at least one hockey (football) match played by the canton team/s (and zero otherwise), Postal Voting, which is a dummy one if the canton already introduced postal voting (and zero if not), and the interaction between these two terms, i.e. TreatmentHockey X Postal Voting (TreatmentFootball X Postal Voting). In each column, I report the specifications used in the regression. Errors are clustered at cantonal level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.3: Local Analysis for Hockey: comparison across municipalities

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality	0.022***	0.018***	0.021***	0.020***	0.018***	0.016***	0.016***	0.016***	0.016***	0.016***
	(0.009)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	17,137	17,626	17,967	18,416	18,908	19,206	19,303	19,368	19,387	19,395
R-Squared	0.660	0.656	0.653	0.651	0.648	0.646	0.646	0.646	0.646	0.646
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality	0.031***	0.022***	0.021***	0.018***	0.015***	0.013***	0.012***	0.012***	0.012**	0.012**
	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	17,137	17,626	17,967	18,416	18,908	19,206	19,303	19,368	19,387	19,395
R-Squared	0.706	0.703	0.702	0.701	0.699	0.698	0.698	0.698	0.698	0.698
Panel C: Canton X Referendum-Day FE										
Treated Municipality	0.024***	0.016**	0.018***	0.017***	0.012**	0.011**	0.010**	0.010**	0.010**	0.010**
	(0.009)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	17,137	17,626	17,967	18,416	18,908	19,206	19,303	19,368	19,387	19,395
R-Squared	0.782	0.780	0.779	0.778	0.776	0.774	0.773	0.773	0.773	0.773

Notes. Data include municipal turnout of all the federal referenda before the of postal voting (1988-2005) for all the Swiss cantons. Municipal turnout is the ratio of ballots and eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the hockey arena. The control group is made by municipalities at the cantonal boundaries. The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: Local Analysis for Football: comparison across municipalities

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality	-0.004	-0.003	0.001	0.002	0.003	0.004	0.003	0.003	0.002	0.001
	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	34,518	38,671	42,645	47,106	50,948	53,829	55,276	56,771	57,453	57,822
R-Squared	0.596	0.597	0.600	0.602	0.604	0.601	0.600	0.593	0.592	0.591
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality	-0.004	-0.002	0.002	0.004	0.004	0.005	0.004	0.004	0.003	0.002
	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	34,518	38,671	42,645	47,106	50,948	53,829	55,276	56,771	57,453	57,822
R-Squared	0.646	0.647	0.648	0.650	0.652	0.650	0.650	0.645	0.644	0.643
Panel C: Canton X Referendum-Day FE										
Treated Municipality	-0.005	-0.003	0.001	0.003	0.003	0.005	0.004	0.003	0.002	0.001
	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	34,518	38,671	42,645	47,106	50,948	53,829	55,276	56,771	57,453	57,822
R-Squared	0.722	0.723	0.722	0.722	0.723	0.720	0.720	0.715	0.715	0.714

Notes. Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005) for all the Swiss cantons. Municipal turnout is the ratio of ballots and eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the hockey arena. The control group is made by municipalities at the cantonal boundaries. The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.5: Local Analysis for Hockey: Outcome of the match

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality Win	0.062***	0.062***	0.066***	0.049***	0.032***	0.028***	0.027***	0.020**	0.022**	0.021**
	(0.012)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
Treated Municipality Lost	0.046***	0.044***	0.053***	0.045***	0.033***	0.033***	0.033***	0.030***	0.030***	0.030***
	(0.012)	(0.009)	(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)
Treated Municipality Parity	0.109***	0.132***	0.080**	0.056**	0.065***	0.055**	0.062***	0.052**	0.053**	0.057**
	(0.031)	(0.034)	(0.035)	(0.029)	(0.024)	(0.022)	(0.023)	(0.021)	(0.023)	(0.023)
Observations	17,137	17,626	17,967	18,416	18,908	19,206	19,303	19,368	19,387	19,395
R-Squared	0.661	0.657	0.654	0.652	0.648	0.646	0.646	0.646	0.646	0.647
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality Win	0.060***	0.057***	0.056***	0.041***	0.027***	0.023***	0.022***	0.018**	0.019**	0.018**
	(0.012)	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)
Treated Municipality Lost	0.055***	0.047***	0.044***	0.035***	0.024***	0.023***	0.022***	0.020***	0.020***	0.019**
	(0.011)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Treated Municipality Parity	0.046	0.092**	0.074**	0.059**	0.063***	0.052**	0.060***	0.051**	0.051**	0.055**
	(0.047)	(0.038)	(0.031)	(0.027)	(0.022)	(0.022)	(0.022)	(0.020)	(0.022)	(0.022)
Observations	17,137	17,626	17,967	18,416	18,908	19,206	19,303	19,368	19,387	19,395
R-Squared	0.706	0.704	0.703	0.702	0.700	0.698	0.698	0.698	0.698	0.698
Panel C: Canton X Referendum-Day FE										
Treated Municipality Win	0.038***	0.038***	0.038***	0.026***	0.014	0.011	0.011	0.008	0.008	0.007
	(0.012)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
Treated Municipality Lost	0.035***	0.037***	0.042***	0.034***	0.022***	0.020***	0.020***	0.019***	0.019**	0.019**
	(0.011)	(0.009)	(0.008)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
Treated Municipality Parity	0.084***	0.112***	0.090***	0.060**	0.056**	0.041*	0.051**	0.037	0.038	0.042*
	(0.019)	(0.027)	(0.025)	(0.028)	(0.024)	(0.024)	(0.024)	(0.023)	(0.025)	(0.025)
Observations	17,137	17,626	17,967	18,416	18,908	19,206	19,303	19,368	19,387	19,395
R-Squared	0.782	0.780	0.779	0.778	0.776	0.774	0.774	0.773	0.773	0.774

*Notes.*Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005). Municipal turnout is the ratio of ballots and eligible voters in a municipality. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the Football arena. The control group is made by municipalities at the cantonal boundaries. Independent variables are the share of matches won, lost, and even, and the share of matches played at home(not reported in the table). The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.6: Local Analysis for Football: Outcome of the match

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality Win	-0.006	0.001	0.007	0.009**	0.007*	0.008**	0.005	0.004	0.003	0.002
	(0.008)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treated Municipality Lost	-0.005	0.001	0.003	0.006	0.005	0.005	0.005	0.004	0.004	0.003
	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treated Municipality Parity	-0.004	0.001	0.004	0.005	0.006	0.008*	0.007*	0.006*	0.006	0.005
	(0.008)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	34,518	38,671	42,645	47,106	50,948	53,829	55,276	56,771	57,453	57,822
R-Squared	0.596	0.597	0.600	0.602	0.604	0.601	0.600	0.594	0.592	0.591
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality Win	-0.005	0.001	0.008*	0.011***	0.011***	0.012***	0.010**	0.009**	0.008**	0.007*
	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treated Municipality Lost	-0.001	0.004	0.007*	0.009**	0.008**	0.007*	0.005	0.003	0.003	0.001
	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treated Municipality Parity	-0.009	-0.003	0.001	0.002	0.003	0.005	0.005	0.004	0.004	0.003
	(0.008)	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	34,518	38,671	42,645	47,106	50,948	53,829	55,276	56,771	57,453	57,822
R-Squared	0.646	0.647	0.648	0.650	0.652	0.650	0.650	0.645	0.644	0.644
Panel C: Canton X Referendum-Day FE										
Treated Municipality Win	-0.017**	-0.009*	-0.002	0.000	-0.001	-0.000	-0.002	-0.003	-0.003	-0.004
	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treated Municipality Lost	-0.007	0.001	0.004	0.006	0.004	0.003	0.003	0.003	0.003	0.002
	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Treated Municipality Parity	-0.012	-0.007	-0.003	-0.003	-0.004	-0.003	-0.003	-0.003	-0.003	-0.004
	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	34,518	38,671	42,645	47,106	50,948	53,829	55,276	56,771	57,453	57,822
R-Squared	0.722	0.723	0.722	0.723	0.723	0.720	0.720	0.715	0.715	0.714

Notes. Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005) for all the Swiss cantons. Municipal turnout is the ratio of ballots and eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the hockey arena. The control group is made by municipalities at the cantonal boundaries. Independent variables are the share of matches won, lost, and even, and the share of matches played at home(not reported in the table). The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.7: Local Analysis for Hockey: less/more than 1000 inhabitants

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality ($\leq$ 1000)	0.032**	0.020*	0.030***	0.030***	0.026***	0.022***	0.021***	0.021***	0.021***	0.020***
	(0.014)	(0.011)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	7,211	7,440	7,632	7,867	8,168	8,374	8,445	8,492	8,499	8,504
R-Squared	0.627	0.624	0.622	0.619	0.615	0.613	0.613	0.613	0.613	0.613
Treated Municipality (> 1000)	0.022**	0.020***	0.016**	0.012*	0.008	0.007	0.007	0.007	0.007	0.006
	(0.010)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	9,926	10,186	10,335	10,549	10,740	10,832	10,858	10,876	10,888	10,891
R-Squared	0.715	0.710	0.708	0.707	0.705	0.704	0.704	0.705	0.705	0.705
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality ($\leq$ 1000)	0.045***	0.025**	0.027***	0.024***	0.019**	0.015*	0.014*	0.014*	0.014*	0.014*
	(0.015)	(0.012)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	7,211	7,440	7,632	7,867	8,168	8,374	8,445	8,492	8,499	8,504
R-Squared	0.665	0.663	0.663	0.660	0.658	0.656	0.656	0.655	0.656	0.656
Treated Municipality (> 1000)	0.028***	0.023***	0.017***	0.014**	0.010	0.009	0.009	0.008	0.008	0.008
	(0.010)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	9,926	10,186	10,335	10,549	10,740	10,832	10,858	10,876	10,888	10,891
R-Squared	0.770	0.768	0.767	0.767	0.766	0.766	0.766	0.766	0.766	0.766
Panel C: Canton X Referendum-Day FE										
Treated Municipality ($\leq$ 1000)	0.039**	0.018	0.024***	0.022***	0.017**	0.013	0.012	0.012	0.012	0.012
	(0.015)	(0.012)	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Observations	7,211	7,440	7,632	7,867	8,168	8,374	8,445	8,492	8,499	8,504
R-Squared	0.733	0.731	0.730	0.728	0.725	0.722	0.722	0.722	0.722	0.722
Treated Municipality (> 1000)	0.020**	0.016**	0.014**	0.011*	0.007	0.006	0.006	0.005	0.005	0.005
	(0.010)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	9,926	10,186	10,335	10,549	10,740	10,832	10,858	10,876	10,888	10,891
R-Squared	0.857	0.856	0.856	0.855	0.854	0.853	0.853	0.853	0.853	0.853

Notes. Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005). Municipal turnout is the ratio of ballots and eligible voters in a municipality. Estimates are runned separately in two different samples of municipalities, namely those with less than 1000 eligible voters, and those with more than 1000 eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the hockey arena. The control group is made by municipalities at the cantonal boundaries. The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.8: Local Analysis for Football: less/more than 1000 inhabitants

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality ($\leq$ 1000)	0.010	0.007	0.009	0.008	0.008	0.009	0.007	0.006	0.005	0.004
	(0.015)	(0.008)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	14,459	15,819	17,913	20,218	22,399	24,459	25,398	26,466	26,859	27,063
R-Squared	0.562	0.567	0.577	0.578	0.586	0.586	0.586	0.578	0.574	0.572
Treated Municipality($>$ 1000)	0.002	0.002	0.001	0.002	-0.001	-0.000	-0.001	-0.001	-0.002	-0.002
	(0.006)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	20,059	22,852	24,732	26,888	28,549	29,370	29,878	30,305	30,594	30,759
R-Squared	0.662	0.662	0.665	0.668	0.668	0.668	0.666	0.662	0.662	0.661
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality ($\leq$ 1000)	0.010	0.008	0.012*	0.010	0.011*	0.011*	0.009	0.008	0.007	0.005
	(0.015)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	14,459	15,819	17,913	20,218	22,399	24,459	25,398	26,466	26,859	27,063
R-Squared	0.612	0.616	0.624	0.624	0.631	0.633	0.634	0.627	0.625	0.623
Treated Municipality ($>$ 1000)	0.003	0.003	0.001	0.002	-0.000	-0.000	-0.001	-0.002	-0.002	-0.003
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	20,059	22,852	24,732	26,888	28,549	29,370	29,878	30,305	30,594	30,759
R-Squared	0.717	0.716	0.718	0.721	0.722	0.722	0.721	0.718	0.718	0.718
Panel C: Canton X Referendum-Day FE										
Treated Municipality ($\leq$ 1000)	0.010	0.007	0.011*	0.009	0.010*	0.010*	0.008	0.007	0.006	0.005
	(0.016)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	14,459	15,819	17,913	20,218	22,399	24,459	25,398	26,466	26,859	27,063
R-Squared	0.682	0.686	0.690	0.688	0.694	0.694	0.695	0.689	0.688	0.687
Treated Municipality ($>$ 1000)	0.001	0.003	0.001	0.002	-0.001	-0.001	-0.002	-0.002	-0.002	-0.003
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	20,059	22,852	24,732	26,888	28,549	29,370	29,878	30,305	30,594	30,759
R-Squared	0.802	0.800	0.801	0.803	0.803	0.802	0.802	0.799	0.799	0.799

*Notes.*Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005). Municipal turnout is the ratio of ballots and eligible voters in a municipality. Estimates are runned separately for two different samples of municipalities, namely those with less than 1000 eligible voters, and those with more than 1000 eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the Football arena. The control group is made by municipalities at the cantonal boundaries. The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix

3.A Additional Material

Table 3.A.1: Introduction of Postal Voting

Canton	Year
Basel - Land (BL)	1978
St. Gallen (SG)	1979
Appenzell Innerrhoden (AI)	1979
Solothum (SO)	1980
Thurgau (TG)	1985
Appenzell Aussenrhoden (AR)	1988
Bern (BE)	1991
Aargau (AG)	1993
Zurich (ZH)	1994
Luzern (LU)	1994
Nidwalden (NW)	1994
Basel - Stadt (BS)	1995
Zug (BS)	1995
Freiburg (FR)	1995
Schaffhausen (SH)	1995
Glarus (GL)	1995
Uri (UR)	1995
Graubuenden (GR)	1995
Obwalden (OW)	1995
Geneve (GE)	1995
Jura (JU)	1999
Schwyz (SZ)	2000
Wadt (2002)	2002
Neuenburg (NE)	2003
Tessin (TI)	2005
Wallis (VS)	2005

Table 3.A.2: Hockey stadium in Switzerland since 1950

Canton	Cities with a stadium
Panel A: Hockey	
Basel - Land (BL)	Basel
St. Gallen (SG)	St. Gallen
Appenzell Aus. (AR)	Herisau
Bern (BE)	Bern, Biel, Langnau
Zurich (ZH)	Kloten, Zurich
Zug (BS)	Zug
Freiburg (FR)	Freiburg
Graubuenden (GR)	Chur, Davos
Geneve (GE)	Geneve
Tessin (TI)	Lugano, Quinto
Vaud (VD)	Lausanne
Neuchatel (NE)	Chaux de Fonds
Panel B: Football	
Bern (BE)	Bern, Thun
Aargau (AG)	Suhr, Fislisbach, Wettingen
Basel (BS)	Basel
Zurich (ZH)	Winterthur, Zurich
Luzern (LU)	Luzern, Kriens
Zug (BS)	Zug
Freiburg (FR)	Freiburg
Schaffhausen (SH)	Schaffausen
Geneve (GE)	Thonex, Lancy, Carouge
Jura (JU)	Delemont
Tessin (TI)	Bellinzona, Chiasso, Locarno, Lugano
Neuchatel (NE)	Chaux, Neuchatel
Wallis (VS)	Sion
Vaud (VD)	Lausanne, Vevey, Yverdon

Table 3.A.3: Local Analysis for Hockey, by municipality size (less/more than 1500/2000 inhabitants)

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality ($<$ 1500 inh.)	0.036***	0.023**	0.031***	0.030***	0.027***	0.024***	0.023***	0.023***	0.023***	0.022***
	(0.012)	(0.009)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	9,435	9,791	10,015	10,270	10,604	10,853	10,939	10,996	11,007	11,013
R-Squared	0.638	0.634	0.631	0.629	0.625	0.623	0.623	0.623	0.623	0.623
Treated Municipality ($>$ 1500 inh.)	0.016	0.015*	0.009	0.005	0.000	0.000	-0.000	-0.001	-0.001	-0.001
	(0.011)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	7,702	7,835	7,952	8,146	8,304	8,353	8,364	8,372	8,380	8,382
R-Squared	0.729	0.726	0.725	0.724	0.723	0.722	0.722	0.723	0.723	0.723
Treated Municipality ($<$ 2000 inh.)	0.029***	0.020**	0.028***	0.026***	0.024***	0.021***	0.021***	0.020***	0.020***	0.020***
	(0.011)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	11,224	11,593	11,828	12,126	12,471	12,721	12,809	12,867	12,878	12,885
R-Squared	0.644	0.641	0.638	0.636	0.632	0.630	0.630	0.630	0.631	0.631
Treated Municipality ($>$ 2000 inh.)	0.016	0.017*	0.011	0.010	0.005	0.005	0.005	0.004	0.004	0.004
	(0.012)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	5,913	6,033	6,139	6,290	6,437	6,485	6,494	6,501	6,509	6,510
R-Squared	0.739	0.735	0.734	0.733	0.731	0.731	0.731	0.731	0.731	0.731
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality ($<$ 1500 inh.)	0.048***	0.029***	0.029***	0.026***	0.021***	0.017***	0.017**	0.016**	0.016**	0.016**
	(0.012)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	9,435	9,791	10,015	10,270	10,604	10,853	10,939	10,996	11,007	11,013
R-Squared	0.679	0.677	0.676	0.675	0.672	0.670	0.670	0.670	0.670	0.670
Treated Municipality ($>$ 1500 inh.)	0.021*	0.018**	0.011	0.008	0.003	0.003	0.003	0.002	0.002	0.002
	(0.011)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	7,702	7,835	7,952	8,146	8,304	8,353	8,364	8,372	8,380	8,382
R-Squared	0.784	0.782	0.782	0.782	0.781	0.780	0.780	0.780	0.781	0.781
Treated Municipality ($<$ 2000 inh.)	0.040***	0.026***	0.026***	0.022***	0.019***	0.015**	0.014**	0.014**	0.014**	0.014**
	(0.011)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	11,224	11,593	11,828	12,126	12,471	12,721	12,809	12,867	12,878	12,885
R-Squared	0.688	0.686	0.685	0.684	0.681	0.679	0.679	0.679	0.679	0.680
Treated Municipality ($>$ 2000 inh.)	0.019	0.018*	0.013*	0.013*	0.008	0.008	0.007	0.007	0.007	0.006
	(0.013)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	5,913	6,033	6,139	6,290	6,437	6,485	6,494	6,501	6,509	6,510
R-Squared	0.794	0.791	0.791	0.791	0.790	0.789	0.789	0.789	0.789	0.790
Panel C: Canton X Referendum-Day FE										
Treated Municipality ($<$ 1500 inh.)	0.043***	0.021**	0.025***	0.023***	0.018***	0.014**	0.014**	0.014**	0.013**	0.013**
	(0.012)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	9,435	9,791	10,015	10,270	10,604	10,853	10,939	10,996	11,007	11,013
R-Squared	0.752	0.749	0.748	0.747	0.744	0.742	0.741	0.741	0.741	0.741
Treated Municipality ($>$ 1500 inh.)	0.013	0.013	0.011	0.007	0.002	0.002	0.001	0.001	0.001	0.001
	(0.011)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	7,702	7,835	7,952	8,146	8,304	8,353	8,364	8,372	8,380	8,382
R-Squared	0.870	0.869	0.869	0.868	0.867	0.867	0.867	0.867	0.867	0.867
Treated Municipality ($<$ 2000 inh.)	0.032***	0.017**	0.021***	0.018***	0.014**	0.011*	0.011*	0.011*	0.010*	0.010*
	(0.011)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	11,224	11,593	11,828	12,126	12,471	12,721	12,809	12,867	12,878	12,885
R-Squared	0.763	0.761	0.760	0.759	0.756	0.754	0.753	0.753	0.753	0.753
Treated Municipality ($>$ 2000 inh.)	0.014	0.016	0.014*	0.014*	0.008	0.008	0.007	0.007	0.006	0.006
	(0.013)	(0.010)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Observations	5,913	6,033	6,139	6,290	6,437	6,485	6,494	6,501	6,509	6,510
R-Squared	0.877	0.875	0.876	0.875	0.874	0.873	0.873	0.873	0.873	0.873

Notes. Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005). Municipal turnout is the ratio of ballots and eligible voters in a municipality. Estimates are runned separately in four different samples of municipalities, namely those with less than 1500/2000 eligible voters, and those with more than 1500/2000 eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the hockey arena. The control group is made by municipalities at the cantonal boundaries. The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.A.4: Local Analysis for Football, by municipality size (less/more than 1500/2000 inhabitants

Columns	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bandwidth (Km.)	5	10	15	20	25	30	35	40	45	50
Panel A: Canton FE, Referendum-Day FE										
Treated Municipality (< 1500 inh.)	0.021*	0.012*	0.012**	0.009*	0.009*	0.010**	0.008	0.007	0.006	0.004
	(0.011)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	18,844	20,728	23,178	26,008	28,566	30,947	32,059	33,322	33,812	34,072
R-Squared	0.573	0.578	0.585	0.586	0.592	0.591	0.591	0.582	0.580	0.578
Treated Municipality (> 1500 inh.)	-0.001	0.002	0.001	0.002	-0.001	-0.000	-0.001	-0.001	-0.001	-0.002
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	15,674	17,943	19,467	21,098	22,382	22,882	23,217	23,449	23,641	23,750
R-Squared	0.682	0.682	0.685	0.689	0.689	0.689	0.686	0.684	0.683	0.683
Treated Municipality (< 2000 inh.)	0.020**	0.013**	0.011**	0.008	0.008*	0.009**	0.007*	0.007	0.006	0.005
	(0.009)	(0.006)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	22,287	24,446	27,312	30,525	33,377	35,899	37,142	38,453	38,946	39,237
R-Squared	0.581	0.584	0.590	0.589	0.594	0.594	0.593	0.586	0.584	0.582
Treated Municipality (> 2000 inh.)	0.000	0.003	0.002	0.003	-0.000	-0.001	-0.001	-0.002	-0.002	-0.002
	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	12,231	14,225	15,333	16,581	17,571	17,930	18,134	18,318	18,507	18,585
R-Squared	0.690	0.694	0.697	0.702	0.703	0.703	0.701	0.698	0.698	0.697
Panel B: Canton X Year FE, Referendum-Day FE										
Treated Municipality (< 1500)	0.021 *	0.013**	0.014**	0.011**	0.012**	0.012**	0.010**	0.009*	0.008	0.006
	(0.011)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	18,844	20,728	23,178	26,008	28,566	30,947	32,059	33,322	33,812	34,072
R-Squared	0.623	0.627	0.632	0.633	0.639	0.640	0.641	0.633	0.632	0.630
Treated Municipality (> 1500)	0.000	0.003	0.001	0.002	-0.001	-0.001	-0.002	-0.002	-0.002	-0.003
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	15,674	17,943	19,467	21,098	22,382	22,882	23,217	23,449	23,641	23,750
R-Squared	0.737	0.737	0.738	0.742	0.742	0.742	0.741	0.739	0.739	0.739
Treated Municipality (< 2000 inh.)	0.020**	0.014**	0.012**	0.010**	0.011**	0.011**	0.009**	0.008*	0.007*	0.006
	(0.010)	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	22,287	24,446	27,312	30,525	33,377	35,899	37,142	38,453	38,946	39,237
R-Squared	0.632	0.635	0.639	0.638	0.644	0.645	0.646	0.639	0.638	0.637
Treated Municipality (> 2000 inh.)	0.002	0.004	0.002	0.004	-0.000	-0.001	-0.001	-0.002	-0.002	-0.003
	(0.007)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	12,231	14,225	15,333	16,581	17,571	17,930	18,134	18,318	18,507	18,585
R-Squared	0.745	0.747	0.749	0.753	0.754	0.754	0.753	0.751	0.750	0.750
Panel C: Canton X Referendum-Day FE										
Treated Municipality (< 1500 inh.)	0.023*	0.012*	0.014**	0.011**	0.011**	0.012**	0.009*	0.008*	0.007	0.006
	(0.012)	(0.007)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Observations	18,844	20,728	23,178	26,008	28,566	30,947	32,059	33,322	33,812	34,072
R-Squared	0.695	0.700	0.703	0.702	0.706	0.706	0.707	0.700	0.700	0.698
Treated Municipality (> 1500 inh.)	-0.002	0.002	0.000	0.001	-0.002	-0.002	-0.002	-0.003	-0.003	-0.003
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	15,674	17,943	19,467	21,098	22,382	22,882	23,217	23,449	23,641	23,750
R-Squared	0.823	0.821	0.821	0.823	0.822	0.821	0.821	0.819	0.819	0.818
Treated Municipality (< 2000 inh.)	0.021**	0.013**	0.012**	0.009*	0.010**	0.011**	0.009**	0.008*	0.007	0.006
	(0.010)	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	22,287	24,446	27,312	30,525	33,377	35,899	37,142	38,453	38,946	39,237
R-Squared	0.708	0.711	0.714	0.711	0.715	0.714	0.715	0.709	0.708	0.707
Treated Municipality (> 2000 inh.)	-0.000	0.002	0.001	0.003	-0.001	-0.002	-0.002	-0.003	-0.003	-0.004
	(0.007)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	12,231	14,225	15,333	16,581	17,571	17,930	18,134	18,318	18,507	18,585
R-Squared	0.830	0.828	0.827	0.829	0.829	0.829	0.828	0.827	0.826	0.826

Notes. Data include municipal turnout of all the federal referenda before the introduction of postal voting (1988-2005). Municipal turnout is the ratio of ballots and eligible voters in a municipality. Estimates are runned separately in four different samples of municipalities, namely those with less than 1500/2000 eligible voters, and those with more than 1500/2000 eligible voters. In each panel, the specification used in the regression is reported. In each column, I consider a different treatment group, i.e. municipalities in a canton playing within a week before the referendum, and that are located in a bandwidth of some kilometers from the hockey arena. The control group is made by municipalities at the cantonal boundaries. The length of the bandwidth, reported at the top of the table, varies from a minimum of 5 km to a maximum of 50 Km. Data include all the federal referenda (and all the cantons) before the introduction of postal voting (1988-2005). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.