Temi di Discussione

(Working Papers)

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POLITICAL SELECTION IN THE SKILLED CITY

by Antonio Accetturo*

Abstract

This paper studies the impact of citizens' human capital on the characteristics of elected politicians in democratic elections for the post of mayor. By using a change in the rules for Italian mayoral elections and a difference-in-differences estimator, I find that cities endowed with a larger amount of human capital tend to elect mayors that have a higher educational attainment and that were previously employed in skill-intensive jobs. This result is quantitatively small but robust to omitted variables or selection issues.

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1.   Introduction[1]

This paper deals with the benefits of schooling. It aims to establish whether better-educated citizens make different political choices with respect to persons with a lower amount of human capital.[2]

The fact that a pool of better-educated individuals may exert a positive effect on political outcomes is well-known in the field of Political Science. As Almond and Verba (1989) point out, education is a crucial determinant of "civic culture" and, as regards participation in democratic politics, "the uneducated man […] is a different political actor from the man who achieved a higher level of education".

The linkage between schooling and political choices rests on the idea that education raises the cognitive skills of individuals and lowers the cost to them of acquiring information about policies and politicians. This may have two main consequences:

1. Political participation (turnout at elections) should be higher among educated individuals.

2. Political choices (in terms of preferred candidates) by educated citizens might differ from those by individuals with a lower amount of human capital.

A few papers analyse the first consequence (Dee, 2004; Milligan et al., 2004; Jaitman, 2013). The aim of this paper is to provide the first causal investigation of the second consequence. This is done by studying the political selection of Italian mayors over the

[2] In this paper, I use education and human capital interchangeably. Education is one of the most popular topics of analysis for economists and studies on its impact on individual wages and productivity date back more than four decades. However, its positive effects may spill over the individual sphere: economists have recently argued the existence of positive externalities due to the mere presence of a more educated pool of workers on aggregate productivity. These are defined as social returns to education or human capital externalities. Moretti (2004) extensively reviews this literature.

period 1987-1997 and assessing, in particular, whether municipalities endowed with higher human capital end up electing politicians with higher education or a more skill intensive job.

I concentrate on the second consequence for two main reasons. The first relates to the importance of political leadership. Electing talented individuals is of primary importance for the functioning of democratic systems and for the successful adoption of important economic reforms. As Besley (2005) points out "[in the past 200 years,] Almost every major episode of economic change […] has been associated with key personalities coming to power with a commitment to these changes."[3] The second reason relates to the relative strengths of the two consequences under different institutional settings. As Milligan et al. (2004) suggest, the effects of education on turnout are limited to the cases in which voting is particularly costly (i.e. pre-registration for the US). This implies that it is quite unlikely to find an effect of schooling on participation in areas in which costs are negligible as is the case, for example, of many European countries (where local administrations are in charge of voter registration). This may mean that, in these countries, the second consequence is comparatively stronger than the first one and, for this reason, it deserves a thorough empirical investigation.

The identification of a causal link between citizens' education to political selection is not an easy task. Human capital is not randomly distributed across cities: it tends to be higher in places with high productivity, good amenities, and better institutions. This implies that better administered cities are likely to attract a disproportionate number of more educated citizens, thus creating a severe problem of reverse causality (Glaeser and Gottlieb, 2009). Moreover, local politicians are usually selected from a pool of local candidate-citizens (Besley and Coate, 1997) thus implying a spurious correlation between the two variables.

In order to identify a causal parameter, I make use of a difference-in-differences (diff-in-diffs) framework that exploits a change in the electoral rules for mayors in 1993. In the pre-1993 context, mayors were elected by city councils; local elections were dominated by

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[3] Key (1956) remarks that "The nature of the workings of government depends ultimately on the men who run it. The men we elect to office and the circumstances we create that affect their work determine the nature of popular government. Let there be emphasis on those we elect to office."

city-level sections of national parties and voters had little command over the lists that these parties presented. As a result, voters' decisions were mainly *ideological* and their attention was directed more towards parties' political platforms rather than the characteristics of the candidates themselves. The 1993 reform established direct voting for mayors along with new powers. As a consequence, voters were more likely to evaluate the characteristics of the candidates more carefully, alongside making the usual assessment of their political platforms. Politicians' characteristics are measured by years of schooling, which signals for an individual's intrinsic abilities, and skill intensity of her previous profession that proxies "market skills" which can reasonably be correlated with political abilities (Caselli and Morelli, 2004). The core of my identification strategy rests on the fact that if better-educated individuals prefer candidates with a larger amount of human capital, the rise in the educational attainment of mayors should be stronger in cities that are more endowed in terms of human capital. The use of first-differencing helps to control for all other time-invariant characteristics and has clear methodological advantages with respect to the previous literature that relies, instead, on cross-sectional identifications.[4]

Results show that average education of voters positively impacts on the schooling and the skill intensity of previous profession of elected mayors. This result is confirmed in a number of robustness checks that take into account omitted variables bias or sorting of citizens. The estimated effect does not seem to be driven by homophily. The magnitude seems instead comparatively small: the interquantile range of the human capital distribution across cities accounts for 6% of the interquantile rise in the schooling of mayors due to the 1993 reform mentioned above.

This is the first paper that tries to uncover the causal link between citizens' educational level and political selection. Previous studies mostly concentrated on the relationship between schooling and the stability of democratic systems from both a theoretical (Bourguignon and Verdier, 2000; Glaeser et al., 2007) and an empirical point of view (Barro, 1999; Glaeser et al., 2004; Papaioannou and Siourounis, 2008). A much closer stream of research investigates, instead, the relationship between turnout at elections and

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[4] This kind of identification is widely used for the analysis of the heterogeneous effects of trade liberalizations on firm-level outcomes (see, for example, Verhoogen, 2008).

education. Dee (2004) finds that civic awareness is positively influenced by schooling. The result holds by using college proximity and exposure to child labour laws as instruments for school attendance. Milligan et al. (2004) use an instrumental variable strategy based on the changes in compulsory schooling laws and find a strong effect of education on voting in presidential elections over the period 1948-2000. They also find that educated adults are more likely to discuss politics and associate with political groups. The effect, however, is limited to the cases in which registration for voting lies with the citizen. When it rests on local government officials (as in the UK), they fail to find any effect. Jaitman (2013) analyses the relationship between turnout and education by exploiting, in a regression discontinuity framework, a feature of the Argentinean electoral law according to which voting is compulsory (and enforced by fines) for individuals aged below 70.[5]

This paper also relates to the "Political Selection" literature, that analyses the determinants of the attraction and retention of talented individuals into politics. To the best of my knowledge, there are no works linking citizens' education to the selection of politicians.[6]

The paper is organized as follows. Section 2 introduces the institutional framework. Section 3 describes the empirical design. Section 4 presents the data and some descriptive statistics. Section 5 shows the results. Section 6 concludes.

2. How do (did) Italians elect their mayors?

Local governance in Italy is a 3-tier system: 20 regions, 110 provinces, and roughly 8,100 municipalities. Municipal administrations are in charge, among other things, of managing public utilities (local roads, water, and garbage collection), the provision of public elementary schooling, and care of children and the elderly. This is why Italian citizens are

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[5] A somewhat related stream of literature has focused on the 1993 reform in Italy and its political effects. A widely exploited feature is that cities with more than 15,000 inhabitants have run-offs while for smaller cities a first-past-the-post mechanism is at work (see Section 2). Bordignon et al. (2011) show the positive effects of the dual ballot rule on the number of political candidates. De Paola and Scoppa (2010) analyse, instead, the effects of the number of candidates on the quality of elected politicians. Barone and De Blasio (2013) show the positive effects of the dual ballot on turnout; they also find that a dual ballot rule improves the quality of the politicians selected for the executive body.

[6] In the robustness check section (5.2.2), where I list a set of possible confounding factors that are likely to influence my estimations, I will briefly review the "Political Selection" literature.

generally very interested in the composition and the performance of municipal administrations. These comprise an executive body, made up of the mayor and the executive committee, and a legislative body (the municipal council).

2.1 New rules

A major breakthrough in city-level governance was achieved by the 1993 electoral reform: new rules radically changed the political arena for the election of mayors, representing a drastic policy shift.[7]

Table 1 gives an overview of the main changes involved:

Mayors. – Before 1993, mayors were elected by municipal Councils, generally from among the available councilors. As a standard practice in Italian politics, political majorities in the municipal council were created after the elections; this implied that the choice of a mayor was based on bargaining among the local political party leaders following the results of the poll. According to a long-standing tradition, mayors were not the local party leaders.[8] After 1993, direct elections were established. Different rules applied according to the size of the city measured in terms of number of inhabitants. For municipalities with more than 15,000 inhabitants, there was a run-off between the two candidates with the most votes if no single candidate received 50 per cent of the ballot. For smaller cities a first-past-the-post system was in place. As a standard practice, in municipalities with fewer than 5,000 inhabitants candidates ran their own lists (called *Liste Civiche*) without any formal affiliation to political parties. This was due to the fact that in those cities, even before the reform, a majority premium was granted to the most voted party, thus generating an incentive to aggregate parties into coalition lists. The 1993 reform extended the mayors' powers: members of the executive committee were now directly appointed by the mayor, who could even recruit local experts without political affiliations.

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[7] Law 81/1993, *Elezione diretta del sindaco e del presidente della provincia* (Direct election of mayors and provincial presidents). Previously, elections had been regulated by Presidential Decree (DPR) 570/1960 and Law 663/1964.

[8] The system was actually mirroring that of central government, established by the 1948 Constitution, with the explicit aim of preventing the creation of a strong executive power - a clear reaction to the 1922-1943 fascist regime.

The municipal council. – Before 1993, the municipal council was at centre stage in local politics. It was the only directly elected body and could appoint or remove the mayor. All elections were held with a proportional system, except for cities with fewer than 5,000 inhabitants in which majoritarian elections were held. The 1993 reform marked a sharp reduction in the powers of the municipal councils as it could no longer appoint the mayor and the executive committee. Moreover, a motion of no confidence in the mayor automatically implied the dissolution of the council. Lastly, the 1993 reform extended the majoritarian system to all cities, irrespective of size.[9]

After the approval of the reform in March 1993, the new rules began to be implemented gradually according to the schedule for new elections envisaged at local level.

2.2 Overall assessment

Pre-1993 politics was characterized by the overwhelming influence of national political parties. It was nearly impossible to identify a possible mayor before the elections, since post-election bargaining was particularly delicate and no party could seriously commit to choosing a possible candidate. This was an intended outcome: the system was aimed at ensuring close party control over local governmental activities and – at the same time – at preventing the mayor from consolidating any degree of autonomous leadership. Strom (1990) reports that Italian politics was, at that time, characterized by a low level of "ex-ante identifiability" as it was nearly impossible, before elections, to predict the winning candidate for the head of the executive. As a result, the intermediation by political parties was overwhelming and voting was predominantly ideological as little room was left for the local politicians to improve their visibility.[10]

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[9] It is not easy to understand the kind of political selection that was working in the pre-reform system. The paper by Mattozzi and Merlo (2011), however, is able to give some guidance on how parties select their members and possible candidates for elections. They find that parties have weak incentives to select individuals with very high abilities since a relatively more homogeneous group (without superstars) is likely to maximize the collective effort of the party. In other words, selection by parties leads to a set of candidates whose characteristics are much closer to the median voter's characteristics.

[10] For similar scenarios, at local level, see also Agosta (1999), Parisi (1984), Pasquino (2001), and Vandelli (1997).

The 1993 reform was passed in a period of great transformation in Italian politics. In 1990, following years of political paralysis, massive government debt, extensive corruption, and the considerable influence of organized crime, there was a great demand for political, economic, and ethical changes. The reform was introduced to reinvigorate local governments and to simplify local administration. The target was to meet a broader demand for accountability and limit the overwhelming influence of political parties.

As for its stated goal, the reform is deemed a success; it managed to introduce a new (and more stable) system of representation. Voters had more influence and more incentives to carefully select their mayors. According to Baldini and Legnante (1998), the new system secured a more visible and independent position for the new mayors: "by being directly elected above and beyond the party wrangling in the municipal council and bureaucratic fetters, the mayor gains individual legitimation and visibility which are valuable instruments not so much for governing, but particularly for re-election."

A first, non causal, assessment of the effects of the reform is given in Figure 1. For each election year after 1993, I report the share of winning mayors running with their own party list without any formal affiliation to national parties (blue bar). For each year, I also report the same figure for their (pre-1993) predecessors (red bar).[11] It is apparent that the 1993 reform markedly reduced ideological voting, as mayors were now directly relying on their own platforms and personal characteristics.

This is reflected by a rise in the educational attainment of elected mayors: in Figure 2, I show the average increase in years of schooling of the elected mayor with respect to their predecessor for three cases. For the first case, the blue bars represent the increase for individuals elected under the pre-1993 electoral system; for the second case, the red bars show the increase from a pre-reform to a post-reform system; and for the third case, the green bars represent the rise in education of mayors elected under the post-1993 regime. The change in schooling for mayors elected under the same regime (both before and after, blue and green bars) does not display a clear increasing pattern, thus suggesting that, for those

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[11] Independent mayors are those that either are labelled as "Independent" in the Register of Local Administrators or were elected heading a *Lista Civica*. The sample includes cities larger than 5,000 since, all Mayors in smaller cities usually ran independently.

elections, incentives to select politicians remained stable. A stark difference emerges, instead, when we compare mayors elected under different electoral rules (red bars): in this case, incentives to choose a more educated politician rose and this is reflected by a steady increase in the average years of schooling of mayors.

3. Empirical design

A typical way to identify a causal effect in the human capital externalities literature is to use some institutional factors that determine (exogenous) differences in local human capital accumulation as an instrument for the current endowment of city-average schooling (Milligan et al., 2004; Acemoglu and Angrist, 1999). In this paper, I make use of an alternative strategy based on a diff-in-diffs framework.

More formally the baseline diff-in-diffs model is as follows:

$$y_{ct} = \alpha_c + \delta_1 TIME_t + \delta_2 School_c * POST_t + \delta_3 X_{ct} + \xi_{ct} \qquad (1)$$

where y_{ct} is either average schooling or a dummy equal to one for individuals with a skill intensive job for mayors elected in city c at time t. α_c is a city fixed effect, $TIME_t$ is a set dummy variables equal to one for all election years, $POST_t$ is a dummy equal to one for all elections that took place after 1993, $School_c$ is the treatment variable and indicates the educational attainment (human capital) at city level, and X_{ct} is a set of time-varying city-level controls.

As Bertrand et al. (2004) point out, standard errors of equation (1) may be systematically downward biased if serial autocorrelation is not seriously taken into account.[12] Bertrand et al. (2004) propose a number of solutions. The simplest one, which I use in this paper, is to collapse the information on two periods, before and after the reform, by taking averages of all variables and running a regression over two points in time.

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[12] Note that the use of the city-level fixed effect does not solve the problem since fixed effects do not take into account the autocorrelation of the $School_c * POST_t$ term.

Averages are calculated over the period 1987-92 for the pre-reform years and 1993-97 for the post-reform period.[13]

Equation (1) changes as follows:

$$\bar{y}_{ct} = \alpha_c + \beta_1 POST_t + \beta_2 School_c * POST_t + \beta_3 \bar{X}_{ct} + \varepsilon_{ct} \qquad t = 0,1 \qquad (2)$$

where $\bar{y}_{ct}$ and $\bar{X}_{ct}$ are the average of, respectively, the dependent variable and controls before and after the reform.

After taking the averages, I compute the first-differences of equation (2) and estimate this simple equation:

$$\Delta \bar{y}_c = \beta_1 + \beta_2 School_c + \beta_3 \Delta \bar{X}_{ct} + \varepsilon_{ct} \qquad (3)$$

Where $\Delta \bar{y}_c$ and $\Delta \bar{X}_{ct}$ are the changes over time of the averaged dependent and explanatory variables. The coefficient of interest is β_2 that aims to capture the effect of human capital on the rise in schooling (of in the skill intensity of the previous job) of elected mayors.

There are three main conditions under which β_2 can be deemed as a causal parameter:

- The treatment variable ($School_c$) is predetermined with respect to the reform (predetermined treatment).

- There are no other omitted variables that correlate with both the schooling level of the population and the change in the characteristics of elected mayors after 1993 (omitted variable bias).

- Changes in the dependent variable are not correlated with $School_c$ before the implementation of the reform (parallel trend assumption).

I deal with these issues in the robustness check section.

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[13] Five years is the maximum term of a local legislature.

4.   Data and Variables

The data come from two main sources: the Register of Local Administrators (RLA) and the 1991 and 1971 population censuses.

The RLA is managed by the Central Directorate of Electoral Services of the Italian Ministry of the Interior. The register provides is a picture of all elected officers in all Italian municipalities, provinces, and regions at 31 December of each year starting from 1985. For municipalities, "elected officers" include the mayor, deputy mayor, members of the Executive Committee, the chair and the vice-chair of the city council, and all councilors. For cities in which no mayor is currently in office, [14] the database also indicates the Commissioner appointed by the Ministry of the Interior before new elections. The database is quite detailed. It indicates year of election, party political affiliation, date and city of birth, educational attainment, and previous occupation.

The RLA provides the dependent variable of equation (3).

As for the characteristics of the mayor, I use two measures:

- *Education*. This proxies for individual intrinsic abilities. It is computed by taking the logs of the minimum years required to obtain the individual's highest degree. As usual in the estimates of Mincerian equations for Italy, I attribute: zero years for no formal education (uncompleted elementary school); 5 years for elementary school; 8 years for lower secondary (middle) school; 13 years for upper secondary (high) school; 18 years for a university degree. Alternatively I use the share of elected Mayors with at least a secondary school (high) diploma.

- *Skill intensity* of the Mayor's previous occupation. It proxies for "market skills," which, according to the literature dealing with political selection, can be highly correlated with political abilities (Caselli and Morelli, 2004). It is computed as the share of individuals with a high-skilled job before and after

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[14] This can occur for several reasons: resignation, death, forced dissolution of the city council due to suspected organized crime infiltration.

1993. Skilled workers are: university professors, journalists, physicians, pharmacists, judges, lawyers, engineers, chemists, biologists, architects, mathematicians, economists, statisticians, business consultants, and managers.[15]

The other main data sources are the Italian censuses of 1991 and 1971. They provide for the treatment variable, that is the educational level of the resident population (over the age of 6) at city level. 1991 data is used in the baseline regression, 1971 data is used as an instrument in a robustness check. In this case too, I calculate the average schooling of the population by using the lowest number of years necessary to obtain the higher degree.

Table 2 reports the descriptive statistics of all the variables used in the empirical analysis.

As Table 2 shows, schooling of mayors rose by 6.4%, the share of individuals previously working in skill-intensive professions increased by 2.7 percentage points, the share of high-school graduates grew by 6.6 percentage points after the reform. The average number of years of schooling of mayors before 1993 (13.3, not reported) only just corresponds to the achievement of a high school diploma and is more than twice the average number of years of schooling of the population in 1991 (6.5).

5.   Results

In this section, I show the results by assessing whether human capital externalities have an effect on the political selection at city level.

The core of my identification strategy is the diff-in-diffs estimation presented in Section 3. In Section 5.1 I show the baseline results on the average schooling of elected mayors. In Section 5.2 I make a number of robustness checks that take into account issues like omitted variable bias, sorting, and the parallel trend assumption. Further robustness is achieved by considering alternative time spans and making a placebo experiment. Finally, I show that the main result holds when using alternative measures.

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[15] For similar definitions see Brollo et al. (2013) and De Paola and Scoppa (2010).

5.1 Baseline results

Estimates of equation (3) are displayed in Table 3. Column 1 reports the results for the full sample that includes all Italian cities. The coefficient of interest (β_2) is positive and significant with a point estimate of 0.077. This means that, for a municipality at the 75th percentile of the human capital distribution, the 1993 reform raised the schooling of elected mayors by 7 per cent while the effect for a municipality at the 25th percentile was 5.8 per cent. This means that the implied effect of the interquatile range (7-5.8=1.2 percentage points) accounts for roughly 6% of the interquantile range of the dependent variable (0.207). The implied effect is quite small, which is not surprising as the period under consideration was characterized by radical changes in Italian politics that may have affected other determinants of political selection. However, as we will see, the result is very stable and robust, thus confirming the existence of human capital externalities in the selection of politicians.[16]

As shown in Table 1, the 1993 reform envisaged different electoral mechanisms according to city size. We take into these issues into account in columns (2) and (3) by looking at two population thresholds, one regarding single/dual ballots and the other concerning party political affiliations.

The first threshold is set at 15,000. According to the new rules, in municipalities smaller than this, all elections are with a first-past-the post system, and the winner is the one who wins more votes. For larger cities, electoral rules envisage a second round if none of the candidates received 50% of the votes in the first round. As pointed out by De Paola and Scoppa (2010) and Barone and de Blasio (2013), this is likely to change the nature of political competition: a dual ballot, indeed, raises the overall schooling of candidates by triggering broader political representation and more competition among political parties.

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[16] The negative (but not significant) sign for the constant should not be misinterpreted. It indicates the potential effect of the reform for a municipality with the log of the average schooling of population equal to zero (i.e. with an average schooling of population of one year). By calculating the implied effect for the city at the 5th percentile of the human capital distribution (i.e. with the log of schooling of population equal to 1.63, 5.13 years), the implied effect is positive and equal to 4.8 per cent.

The second threshold is set at 5,000. As a standard practice even before the reform, candidates in cities smaller that size were not usually affiliated to national parties but ran their own party list (called *liste civiche*). This was due to the fact that electoral rules envisaged a majority premium for the most voted party and groups belonging to the same coalition had strong incentives to gather in the same list. This is a relevant feature under direct elections as it is likely to reduce the potential influence of national politics and raise the importance of individual characteristics.

Estimates are provided in columns (2) and (3) for, respectively, the 15,000 and 5,000 inhabitants threshold. The baseline result is confirmed: estimated value of β_2 is positive and significant and even slightly larger than the baseline one.[17]

5.2   Robustness

5.2.1   Predetermined treatment

The causal nature of the estimates of β_2 heavily relies on the predetermined nature of the treatment variable with respect to the policy change (see Section 3). In this study, the treatment variable (level of schooling of the population) is measured in 1991, two years before the variation of the electoral rules (1993). This should be enough as long as the 1993 reform could not be predicted in 1991 and the population did not start sorting according to education before 1993.

In order to reject this possibility, I instrument the 1991 average schooling of the population at its 1971 level. This is a good instrument assuming that the 1993 reform could not be predicted in 1971, which is a more than reasonable assumption.

The results are displayed in Table 4. First stage statistics are quite satisfactory: the F of excluded instruments is very high and well above the rule-of-thumb value of 10. The coefficient of the first stage is also positive and highly significant, thus indicating a good correlation between current and past values of educational levels at city level. The second stage results are also quite reassuring. The coefficient is positive, significant, and quite

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[17] The implied effects for both thresholds are quite similar to the baseline one as the interquantile ranges for the restricted samples are slightly smaller than for the full sample.

similar in magnitude with respect to the OLS values, with the single exception of the estimate for cities with fewer than 5,000 inhabitants, that is slightly larger.

5.2.2 Omitted variable bias

Causality also rests on the absence of omitted variables that are correlated with both the educational level of the population and the growth in the schooling of elected mayors. Political selection literature identifies a number of determinants of the political selection. Table 5 presents a brief review of the literature and also reports the variables that I use as controls.

A first concern relates the Besley and Coate (1997) candidate-citizen model, according to which candidates self-select from the pool of citizens.[18] This implies a possible spurious correlation between the education of the citizens and the characteristics of candidates. This feature is controlled by first-differencing and by using a predetermined treatment variable - see eq. (3).

The second issue relates to changes in economic conditions. Caselli and Morelli (2004) show that, when returns to education are particularly high in the private sector, politicians with a high human capital may decide to stay out of politics thus leaving room for less educated candidates.[19] Empirically, my main concern relates to the possible changes in returns to education in the period under consideration due, for example, to skill-biased technical change (Acemoglu, 1998; Autor et al., 2003). This feature may create an attenuation bias in the estimate of β_2 if those technologies were more likely to be adopted in cities with higher human capital (Accetturo et al., 2013). In order to capture this issue, I use

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[18] See also Osborne and Slivinski (1996).

[19] This effect is amplified when the observable characteristic is particularly noisy in signaling the true quality of an individual. Dal Bò et al. (2006) extended the Caselli-Morelli analysis to give a role to pressure groups.

the 1991 and 1996 economic censuses to control for changes in the labour demand for more educated workers: *Δln (employment – skilled).*[20]

The third possible confounder is related to the arrival of public funds and their distortionary effects in the selection of politicians (Brollo et al., 2013). Larger transfers may increase the probability for mismanagement and rent extraction, thus raising the relative returns for being in office for lower ability individuals. Eventually, this may tilt the political equilibrium by inducing a sustained entry for less educated candidates. From an empirical point of view, if the eligibility for transfers were stable over time, first-differencing would cope with this problem. However, this is not the case for Italy: the early 1990s were characterized by a thorough review of the public interventions in lagging areas (Braunerhjelm et al., 2000; Accetturo and de Blasio, 2012). In particular, in the 1980s and before 1992, aid to the underdeveloped South was based on direct transfers to households and basically all municipalities received them. After 1992, aid was reorganized according to the regulations envisaged by Law 488/1992 (Bronzini and De Blasio, 2006). The new rules established an explicit targeting of productive units (mainly industrial plants). This had two consequences: (i) only a subset of southern cities succeeded in keeping their transfers; (ii) for the first time some cities in the de-industrializing North were able to obtain public funds. In order to control for this change, I insert as an explanatory variable $\Delta Transfers_i = D_{i,pre-1992} - D_{i,post-1992}$, where $D_{i,pre-1992}$ and $D_{i,post-1992}$ are a dummy variable equal to one if city *i* received transfers, respectively, before and after 1992.[21]

The fourth issue relates to the change in the political conditions at city level. Galasso and Nannicini (2011) show that higher contestability generates better political selection.[22] As explained in Section 2, the 1993 electoral reform was introduced in a period of increasing contestability within the Italian political system, due, mainly, to both left- and right-wing new political parties having access to the political arena. If the increase in contestability

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[20] Highly skilled industries are selected by using the EU-KLEMS database and are defined as the industries that, over the period 1991-96, employed a higher share of individuals with a university degree than the Italian average. These industries are: Education (Nace rev. 1: M), Health (N), Financial intermediation (J), Renting and business activities (K), and Other public administration (L).

[21] Data on transfers under Law 488/92 were kindly provided by the Ministry of Economic Development.

[22] This is confirmed in a subsequent paper (Galasso and Nannicini, 2012) for different electoral rules.

were equally spread across all municipalities, its effect would simply be absorbed by the constant. If, instead, it were more pronounced in more educated cities, estimates would be upwardly biased. In order to tackle this issue, I insert a measure of political contestability taken from general election results (*ΔContestability*).[23] I construct a variable that is the difference in the share of votes between the winning and the second party at city level in the general elections of 1987 and 1992 (for the years before the change) and 1994 and 1996 (for the years after). This variable is subsequently first-differenced. For all the available years, I took the votes in the proportional elections.[24]

Finally, I also insert a dummy variable equal to one for the Special Status Regions (SSRs), i.e. the five autonomous regions (Sicily, Sardinia, Valle d'Aosta, Trentino-Alto Adige, and Friuli-Venezia Giulia) that established slightly different rules for local elections.[25]

Results are displayed in Table 6 and confirm the baseline estimates of Table 3. The measure of contestability is never statistically significant, thus suggesting that changes in contestability equally affected all municipalities. Moreover the controls for high-skilled sectors and transfers are not significant.

Are these controls enough to reject an omitted variable bias? In order to answer this question, I assess the relative importance of unobservable omitted variables by analysing the possible variation in the coefficient of interest with the inclusion of observable explanatory variables. If these controls substantially attenuate the estimates, it is possible that inclusion of more controls would drive the estimated effect to zero. Conversely, if the inclusion of

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[23] Data are taken from the Istituto Carlo Cattaneo (ICC) database on national Italian elections from 1861 to 2008. ICC is a private research organization aimed at promoting studies on political issues in Italy. ICC runs a huge database on the results of political elections at municipal level.

[24] This is a relevant issue for the 1994 and 1996 general elections. For those years, a mixed electoral system was at work. Some of the members of Parliament were elected in majoritarian constituencies, while the remaining part of Parliament was elected under a proportional system. All voters were given two voting papers, one for the majoritarian vote and the other for the proportional vote. In the calculation of this measure, I only take the votes for the proportional seats. All elections before 1994, instead, were conducted under a purely proportional system.

[25] Local rules mostly involved the way in which the municipal councils were elected and the appointment rules for the Executive Committee (see Baldini and Legnante, 1998).

controls does not change the point estimate in a significant way, I can more confidently claim the causal interpretation of the parameter.

Following Altonji et al. (2005) and its extension to the continuous case made by Bellows and Miguel (2009), I measure the relative strength that omitted variables should have vis-à-vis the observed controls to completely drive the coefficient to zero. This is calculated as the ratio between the coefficient of interest with controls (for example, 0.071 for the full sample case) and the coefficient without controls (0.079, this is the coefficient on the same estimation sample, as reported at the foot of the table) and the coefficient with controls (0.071). The results for these calculations are reported in the last row of the table. If the set of observed controls is representative of all possible controls, then a large ratio suggests that it is implausible that omitted variable bias can explain away the entire effect. In the full sample case, unobserved omitted variables should be 8.88 times stronger than observed controls to drive the coefficient to zero, which seems highly unlikely.[26]

5.2.3   Parallel trend assumption

A third threat to the causal interpretation of β_2 rests on the fact that an increase in the schooling of mayors may have started even before the 1993 reform for more educated cities, thus violating the parallel trend assumption.

In order to address this issue I split the sample in two. The first subperiod relates to the pre-treatment period (1987-1992), the second to the years 1993-1997. For both subperiods, I run eq. (1) by using each point in time as a (placebo) treatment year. Due to the correlation between city fixed effects and $School_c * POST_t$, regressions cannot be run for the years 1987 and 1993 for, respectively, the 1987-1992 and 1993-1997 periods.

Figure 3 reports the coefficient and the 95% confidence intervals for δ_2. For comparative reasons I also report the 1993 estimate, calculated over the entire period 1987-1997. If δ_2 were positive and significant before or after 1993, the parallel trend assumption

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[26] Altonji et al. (2005) consider 3.55 a fairly safe ratio for their estimates.

would be violated thus invalidating my identification strategy. Results show that this is not the case as the only significant coefficient is that related to 1993.

5.2.4   Alternative time span and (placebo) effects on councilors

I also further explore the robustness of the results by changing the time span of analysis. In Table 7, I restrict the analysis to the period 1990-96; I use the 1990-92 years as pre-reform and 1993-96 as post-reform. Sample size is now reduced as some cities did not elect a mayor both before and after 1993 in this restricted period. The baseline results still hold: the estimated coefficients are now slightly larger but confidence intervals largely overlap with the baseline result.

Table 8 presents a placebo experiment on the average schooling of councilors. As explained in Section 3,[27] the 1993 reform affected the way mayors were elected but they dealt only marginally with city councils. In particular, rules did not change at all for cities with fewer than 5,000 inhabitants (Table 1) while, for larger municipalities, elections passed from a proportional to a majoritarian system. The coefficient of interest is the one in column (3) (i.e. for councils unaffected by the reform) and is small and far from standard significance levels. This shows that political selection did not change significantly more in cities with a high level of human capital with respect to the others. This result is also confirmed for larger cities - see columns (1) and (2).

5.2.5 Alternative measures

In Table 9, I report the baseline estimates for other measures commonly used in the empirical literature on political selection: the share of mayors with a high-skilled job and the share of mayors with at least a secondary education. Results show that even by using the alternative definition, the baseline result is confirmed.

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[27] See also Bordignon et al. (2011).

5.3   An interpretational issue: selection or homophily?

Results so far undoubtedly point to the fact that cities endowed with more human capital end up in electing more educated politicians. A possible concern in the interpretation of the results rests with the use of schooling of mayor as dependent variable. Educated citizens may not prefer more educated politicians because their expected abilities as administrators, but just because they are closer in terms of preferences and characteristics. This means that the causal effect uncovered by this paper is just linked to homophily rather than selection.

If homophily is at work, I should observe a reversion to population mean of elected mayors after the reform. In other words, once allowed to directly choose their preferred candidate, citizens should tend to elect politicians that are closer (in terms of educational attainment) to population average.

A simple way to cope with this issue is to assess the existence of heterogeneous effects across cities. In principle, under homophily, cities with a lower endowment of human capital should observe a decrease in the average schooling of elected mayors; this is due to the fact that, given the fuzzy nature of political selection before the reform, mayors in those municipalities were likely to be much more educated than local population. In more skilled cities, instead, where mayors were likely to have an educational level comparable with the citizens' one, the increase should be close to zero.[28]

In order to detect heterogeneity I discretise the schooling level of population into four quartiles and run equation (3).

Results, displayed in Table 10, report, for each quartile, both the coefficient and a measure of political mismatch for each group.[29]

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[28] Alternatively, in highly educated cities, elected mayors before 1993 could be less educated than population average. In this case, the increase in this group should be positive and significant.

[29] Political mismatch is computed as $100 * \left(School_{mayor,before,c} / School_c - 1 \right)$, that is the distance (in percentage points) between the years of schooling of mayors elected before 1993 and the educational attainment of the population.

Let us first focus on the first quartile. Before 1993 this groups was electing mayors with a schooling level 158%-159% higher than local population (see the mismatch indicator). Under homophily, the growth in schooling for that group should be negative, as citizens should prefer a mayor that is closer in terms of educational attainment. This is not confirmed by data: the dummy for the first quartile is positive and significant in all specification, thus implying that even low-skilled/heavily mismatched cities did not observe a deterioration in the schooling of elected politicians after the reform.

The analysis of the fourth quartile points to the same direction. Cities in that group were comparatively less mismatched than others as mayors elected before 1993 had, on average, only 72-81 per cent of schooling more than population. Under homophily, this implies that those citizens had fewer incentives to change the educational level of the elected mayor and the coefficient for that quartile should be lower than those of the other groups. This is not supported by results, as the coefficient for the fourth quartile is larger in a statistically significant way than those of the other groups.[30]

6.   Concluding remarks

The fact that more educated citizens behave differently from uneducated individuals in terms of political choices has sustained a long-lasting debate in the field of Political Science and Political Economy. Empirical evidence on this, however, is quite scant. This paper has filled this gap by analysing a new area over which education may exert its effects: the selection of politicians.

In this study I found that cities with a greater endowment of human capital tend to elect mayors with a higher educational attainment and that were previously employed in skill-intensive jobs. For this purpose, I exploited the 1993 electoral reform of the system of electing Italian mayors that generated stronger incentives to evaluate the mayor's characteristics in a diff-in-diffs framework. The estimated effect is quantitatively small, but robust to omitted variable and citizen sorting biases; it does not seem to be driven by

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[30] These results are confirmed by a local polynomial estimations (available upon request).

homophily. Overall, this finding has far-reaching consequences for economic policies and, in particular, for the educational policies by governments.

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Figure 1

SHARE OF INDEPENDENT MAYORS IN CITIES
WITH >5,000 INHABITANTS
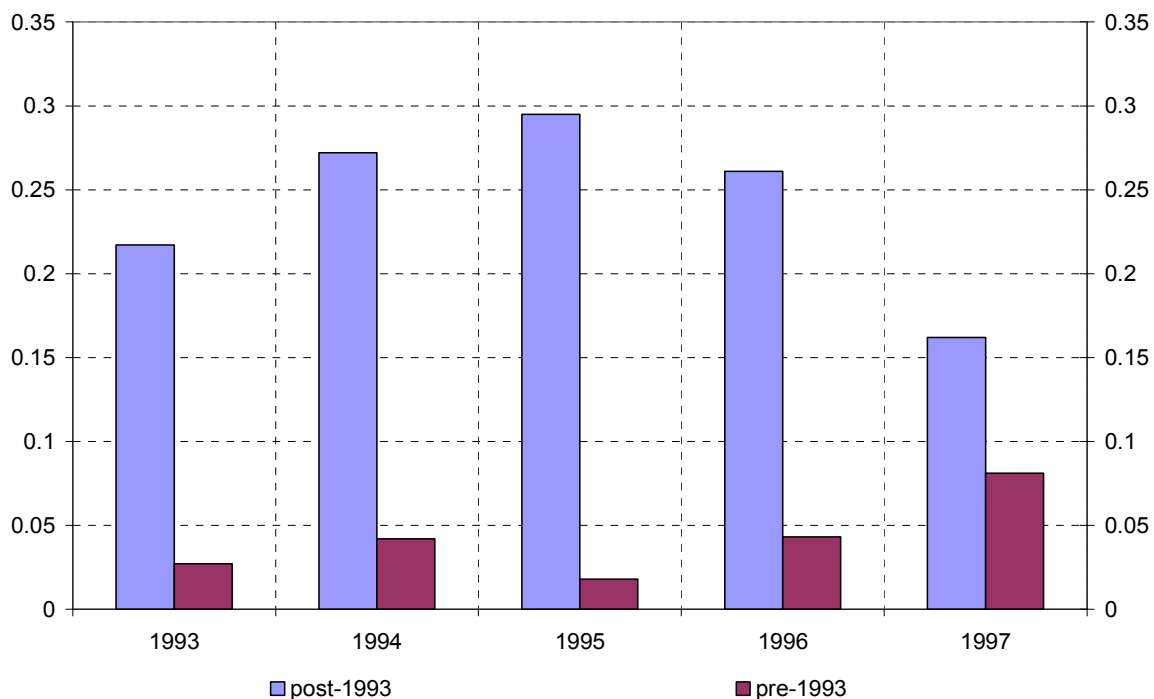
Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators).
Blue bars indicate the share of "Independent" mayors in each year. The red bars show the same share for his/her (pre-1993) predecessor. "Independent" mayors are those whose main supporting party is a *Lista Civica* (a civic list). The sample only considers cities with more than 5,000 inhabitants.

Figure 2

SCHOOLING OF MAYORS AND ELECTORAL REFORM

■ Growth rate of schooling in the pre-1993 electoral system

■ Growth rate of schooling at the switch of the electoral system

■ Growth rate of schooling within the post-1993 electoral system

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators).
Growth rate of the schooling of the elected mayor in each election year with respect to his/her predecessor. Blue bars indicate the change in schooling for elections in the pre-1993 system. Red bars indicate the change in schooling from a pre-1993 to a post-1993 mayor. Green bars indicate the growth rate in schooling for elections in the post-1993 system.

Figure 3

COMMON TREND ASSUMPTION
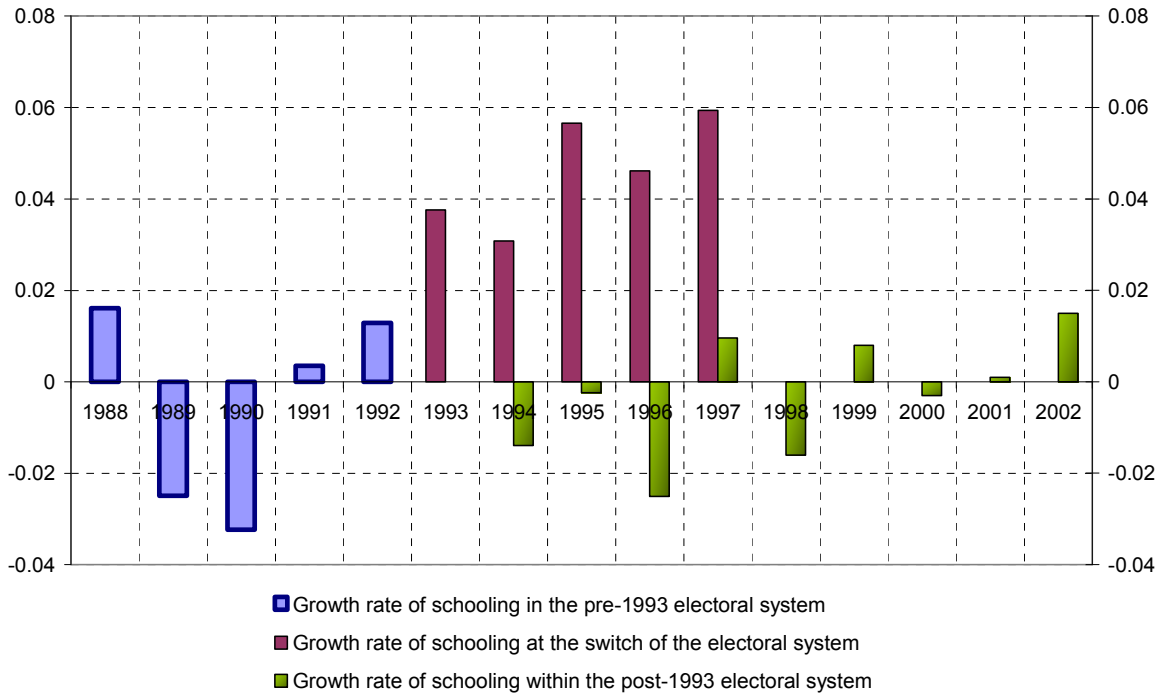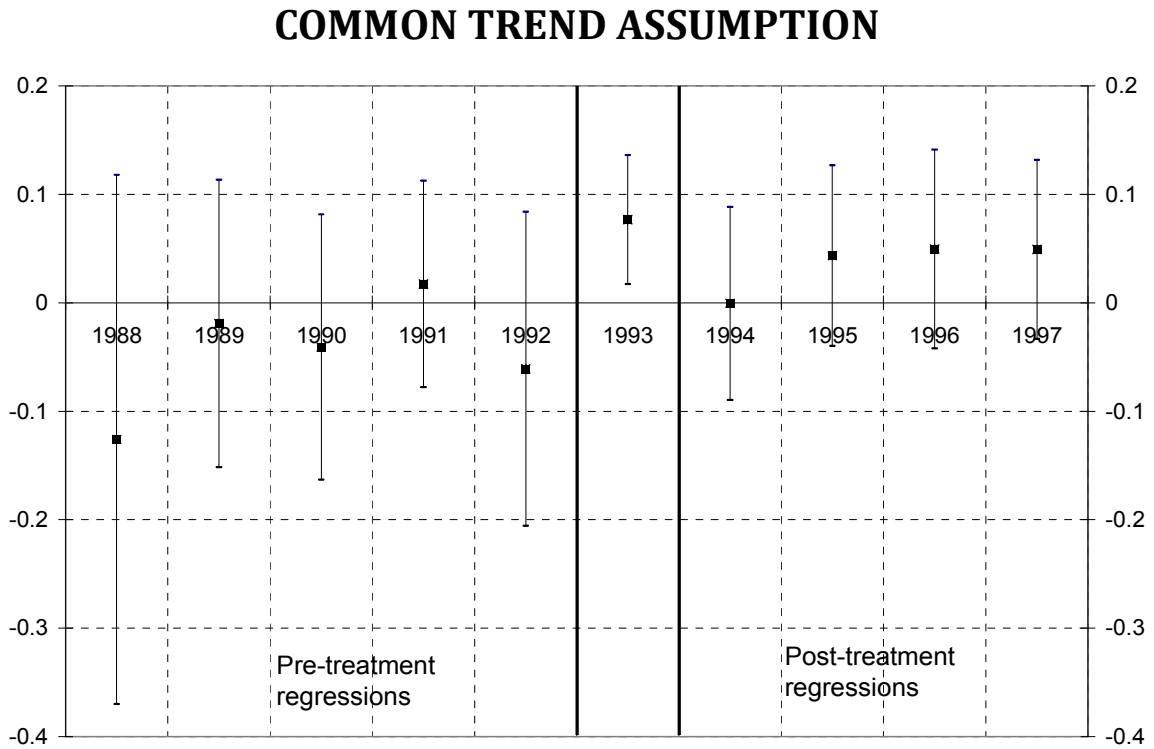

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators) and on the 1991 census.

OLS estimates, see eq. (1). Dependent variable: log of the mayor's schooling. Robust 95% c.i. reported. For the years 1988-92 and 1994-97, all regressions are run, respectively, over the period 1987-92 and 1993-1997. For these estimates, each point in time represents a (placebo) treatment year. 1987 and 1993 estimates are omitted due to the perfect collinearity between the $School_c * POST_t$ term and city-level fixed effects. The 1993 estimate is run over the period 1987-1997 and is reported for comparative reasons. All regressions include city and year-of-election fixed effects.

Table 1

THE 1993 REFORM

	Before 1993	After 1993
Mayor:		
pop<5,000	*Elected by the council among council members*	*Direct Election (first-past-the-post)*
5,000<pop<15,000	*Elected by the council among council members*	*Direct Election (first-past-the-post)*
pop>15,000	*Elected by the council among council members*	*Direct Election (run-off)*
Municipal Council:		
pop<5,000	*Majoritarian System*	*Majoritarian System*
5,000<pop<15,000	*Proportional System*	*Majoritarian System*
pop>15,000	*Proportional System*	*Majoritarian System*

Source: DPR 570/60 and Law 663/64 for the pre-1993 system; Law 81/93 for the post-1993 system.

Table 2

DESCRIPTIVE STATISTICS

	No. Obs.	Mean	Standard Deviation
$\Delta \bar{y}_c$: ln(schooling)	7,332	0.064	0.324
$\Delta \bar{y}_c$: profession	7,332	0.027	0.505
$\Delta \bar{y}_c$: high school	7,332	0.066	0.375
ln(Schooling) – population (1971)	7,332	1.366	0.228
ln(Schooling) – population (1991)	7,332	1.857	0.123
Δln (employment – skilled)	7,332	0.000	0.545
ΔContestability	7,212	-0.089	0.149
ΔTransfers	7,332	0.005	0.532
$\Delta \bar{y}_c$: ln(schooling) (Municipal Council)	7,291	0.068	0.146

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), the 1971, 1991, and 1996 censuses, and ICC.

Table 3

BASELINE RESULTS

Dependent variable: ln(schooling) – Mayor	Full sample	Pop<15,000	Pop<5,000
β_2	0.077**	0.086**	0.085**
	(0.029)	(0.032)	(0.037)
Constant	-0.078	-0.095	-0.094
	(0.054)	(0.059)	(0.002)
R^2	0.000	0.001	0.001
No. Obs.	7,332	6,741	5,312

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), and the 1991 census.
OLS estimates, see eq. (3). Dependent variable: growth rate of the mayor's schooling. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4

PREDETERMINED TREATMENT: IV ESTIMATES

Dependent variable: ln (schooling) – Mayor	Full sample	Pop<15,000	Pop<5,000
β_2	0.073*	0.088**	0.125**
	(0.039)	(0.044)	(0.051)
Constant	-0.071	-0.098	-0.169*
	(0.073)	(0.080)	(0.093)
R^2	0.038	0.036	0.032
No. Obs.	7,332	6,741	5,312
F-first stage	9,144	7,904	5,759
Marginal effect (instrument)	0.412***	0.397***	0.385***
	(0.004)	(0.004)	(0.005)

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), and the 1991 and 1971 censuses. IV estimates, see eq. (3). Dependent variable: growth rate of the mayor's schooling. Robust standard errors in parenthesis. Log schooling of population in 1991 instrumented with log schooling of population in 1971. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5

ALTERNATIVE EXPLANATIONS:
WHAT DOES THE POLITICAL SELECTION LITERATURE SAY

Paper:	Economic/Political mechanism	Control variables in the regression
Besley-Coate (1997)	Candidates self-select from the citizens pool	First Differencing and predetermined treatment variable
Caselli-Morelli (2004); Dal Bò et al. (2006)	Economic conditions (for talented individuals) influence the entry for good politicians	Δln (employment – skilled)
Brollo et al. (2013)	Transfers induce negative political selection (due to rent extraction)	ΔTransfers
Galasso-Nannicini (2011 and 2012); De Paola and Scoppa (2010)	Higher contestability increases political selection	ΔContestability

Source: Author's own literature review.

Table 6

CONTROL FOR CONTESTABILITY, TRANSFERS, ECONOMIC CONDITIONS, AND SPECIAL STATUS REGION

Dependent variable: ln(schooling) – Mayor	Full sample	Pop<15,000	Pop<5,000
β_2 (A)	0.071**	0.077**	0.081**
	(0.032)	(0.035)	(0.041)
Δln (employment – skilled)	0.007	0.008	0.007
	(0.008)	(0.008)	(0.008)
ΔContestability	0.033	0.031	0.017
	(0.026)	(0.027)	(0.030)
ΔTransfers	-0.003	-0.002	-0.002
	(0.007)	(0.008)	(0.009)
SSR	0.000	0.000	0.006
	(0.011)	(0.012)	(0.014)
Constant	-0.065	-0.077	-0.089
	(0.060)	(0.065)	(0.076)
R^2	0.001	0.002	0.001
No. Obs.	7,212	6,624	5,200
Result of baseline estimates on the same estimation sample			
β_2 (B)	0.079**	0.089**	0.089**
	(0.029)	(0.032)	(0.037)
(A)/[(B)-(A)]	8.88	6.42	11.57

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), the 1991 and 1996 censuses, and ICC dataset.
OLS estimates, see eq. (3). Dependent variable: growth rate of the mayor's schooling. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7

TIME SPAN: 1990-96

Dependent variable: ln(schooling) – Mayor	Full sample	Pop<15,000	Pop<5,000
β_2	0.094**	0.101**	0.097**
	(0.032)	(0.036)	(0.041)
Constant	-0.121**	-0.136**	-0.130**
	(0.060)	(0.066)	(0.076)
R^2	0.001	0.001	0.001
No. Obs.	6,438	5,895	4,618

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), and the 1991 census.
OLS estimates, see eq. (3). Dependent variable: growth rate of the mayor's schooling. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8

PLACEBO: EFFECTS ON COUCILLORS

Dependent variable: ln(schooling)–Councilors	Full sample	Pop<15,000	Pop<5,000
β_2	-0.038**	0.000	0.022
	(0.012)	(0.015)	(0.017)
Constant	0.141***	0.078**	0.048
	(0.026)	(0.028)	(0.032)
R^2	0.003	0.002	0.001
No. Obs.	7,291	6,700	5,272

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), and the 1991 census.
OLS estimates, see eq. (3). Dependent variable: growth rate of the schooling of councilors. All regressions include a dummy for Special Status Regions. Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 9

ALTERNATIVE MEASURES

	Full sample	Pop<15,000	Pop<5,000
Share of high-skilled jobs			
β_2	0.158**	0.144**	0.118**
	(0.048)	(0.052)	(0.059)
Constant	-0.266**	-0.243**	-0.200*
	(0.090)	(0.096)	(0.108)
R^2 – within	0.001	0.001	0.001
No. Obs.	7,332	6,741	5,313
Share of secondary education			
β_2	0.091**	0.108**	0.112**
	(0.032)	(0.036)	(0.042)
Constant	-0.104*	-0.135**	-0.138*
	(0.061)	(0.067)	(0.077)
R^2	0.001	0.001	0.001
No. Obs.	7,332	6,741	5,313

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), and the 1991 census.
OLS estimates, see eq. (3). Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 10

SELECTION OR HOMOPHILY?

Dependent variable: ln(schooling) – Mayor	Full sample	Pop<15,000	Pop<5,000
Constant (1st quartile)	0.050***	0.048***	0.044***
	(0.007)	(0.007)	(0.008)
Dummy 2nd quartile	0.018*	0.018	0.022*
	(0.010)	(0.011)	(0.012)
Dummy 3rd quartile	0.013	0.015	0.020
	(0.011)	(0.011)	(0.013)
Dummy 4th quartile	0.026**	0.031**	0.032**
	(0.010)	(0.011)	(0.014)
Mismatch (1st quartile)	*159%*	*159%*	*158%*
Mismatch (2nd quartile)	*110%*	*109%*	*104%*
Mismatch (3rd quartile)	*91%*	*90%*	*85%*
Mismatch (4th quartile)	*81%*	*77%*	*72%*
R^2	0.001	0.001	0.001
No. Obs.	7,332	6,741	5,313

Source: Author's calculations based on data in the *Anagrafe Amministratori Locali* (Register of local administrators), and the 1991 census. Mismatch indicator is calculated as $100*\left(School_{mayor,before,c}/School_c-1\right)$. OLS estimates, see eq. (3). Robust standard errors in parenthesis. * significant at 10%, ** significant at 5%, *** significant at 1%.

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